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Short Sale Bans May Improve Market Quality During Crises: New Evidence from the 2020 Covid Crash

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Abstract

In theory, banning short selling stabilizes stock prices but undermines pricing efficiency and has ambiguous impacts on market liquidity. Empirical studies find mixed and conflicting results. This paper leverages cross-country policy variation during the 2020 Covid crisis to assess differential impacts of bans on stock liquidity, prices, and volatility. Results suggest that bans improved liquidity and stabilized prices for illiquid stocks but temporarily diminished liquidity for highly liquid stocks.The findings support theories in which short sale bans may improve liquidity by selectively filtering out informed—potentially predatory—traders. Thus, policies that target the most illiquid stocks may deliver better overall market quality than uniform short sale bans imposed on all stocks.

1 Introduction

In times of instability, short selling is often blamed for exacerbating, or in some cases even instigating, downturns, and as a result, regulators have often

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turned to banning short sales in an effort to stabilize markets and prop up prices. Several countries implemented short sale bans in response to the 2009-09 financial crisis, the 2011-12 European debt crisis, and most recently in 2020 as the spread of the Covid-19 pandemic led to sharp declines in stock prices. Although both proponents and detractors tend to agree that these bans will restrict trading activity and inflate prices, they diverge on whether this is a desired effect or an unnecessary distortion, and whether bans will improve or harm liquidity overall. The haphazard and inconsistent responses of the various regulatory authorities undoubtedly blunts the impact compared to what a coordinated policy might achieve but does provide us with a convenient natural experiment, comparing markets in countries that implemented bans to similar ones that did not.

Studies on the effects of past bans, such as in Beber and Pagano (2013)[4], Boehmer, Jones, and Zhang (2011)[8], and Beber et. all (2020)[3] conclude that these bans largely failed to support prices and reduced liquidity overall. Studies by Siciliano and Ventoruzzo (2020)[18], Lopez and Pastor (2020)[16], Bessler and Vendrasco (2021,2022)[5][6], and Le Moign and Spolaore (2022)[13] show similar effects for the 2020 European ban as well. These results are consistent with the perspective that short selling improves market efficiency and that restricting this activity can only harm liquidity. Indeed as shown in Lenkey (2021)[14], short selling restrictions can reduce efficiency even if non-binding and non-prohibitive. However, another strand in the literature explores the notion that short sellers tend to be informed traders whose presence causes market participants to widen spreads. Short sale bans may therefore disproportionately remove informed traders from the marketplace and thus increase liquidity in affected stocks (Diamond and Verrecchia (1987)[10] and Appel and Fohlin (2010)[1]). In extreme cases, short sales can be construed as the type of predatory trading behavior outlined in Brunnermeier and Pedersen (2005)[9]. Indeed, Boehmer et. al. (2020)[7] shows that short sellers do hold information with significant predictive value, and Goldstein and Gumbel (2008)[12] show that selling can profitably manipulate markets in a way that buying cannot. As shown in Liu (2015)[15], short selling of bank stocks can lead to runs which then creates a feedback loop that can drastically increase the likelihood of a collapse. Further evidence from Tian, Yan, and Zheng (2021)[19], building upon the asset bubble model of Scheinkman and Xiong (2003)[17], shows that short sale bans have a temporary price effect that gradually dissipates, converging to zero by expiration.

This paper focuses on the short sale bans implemented by six European countries in March of 2020 in response to the Covid-19 pandemic. By February 2020 it had become clear that the virus was spreading rapidly across the globe. Beginning on February 24th, world markets saw sharp declines that continued into late March, precipitating fears of a prolonged recession. In response, regulatory authorities in six European countries implemented short sale bans of varying duration and scope. Beginning on March 13th when Italy and Spain imposed a one-day ban on short selling for targeted stocks, followed on the 17th by similar one-day bans in Belgium and France while Spain imposed a long term ban due to last until April 17th. On the 18th, Austria, Belgium, France, Greece, and Italy all followed suit and imposed long term bans due to expire between April 16th and June 18th. Notably, all except for Austria had also banned short selling in 2011 as well. Through coordination under the European Securities and Markets Authority, the different timetables initially set by each country were brought into alignment with all of the bans expiring on May 18th.[1] By this time, stock markets had largely recovered and thus the restrictions were deemed unnecessary. While these bans were rather limited in scope and duration, and measuring their impact also requires accounting for the differences in the countries' fiscal responses as well as the toll of the virus itself, this event still provides a valuable data point to assess the impact of short sale bans on liquidity and market quality.

2 Motivation

The foundation for this paper lies in the result of Diamond and Verrecchia (1987)[10], which introduces short selling to the model of Glosten and Milgrom (1985)[11], in which spreads are generated by informational asymmetries between traders and dealers. Notably, the result in the modified model deviates from the conventional wisdom that short sale bans make trading more difficult and reduce liquidity by showing that, since short sellers are more likely to be informed traders and thus will be disproportionately excluded from trading by a ban, a ban will narrow spreads and improve liquidity overall.

In the baseline Glosten and Milgrom (1985)[11] model, there is an asset whose underlying value $\tilde{V}$ is either high V^H or low V^L with equal probability. The market consists of a population of traders of whom a proportion $\alpha \in [0, 1]$

[1]The timing of these bans is summarized in Le Moign and Spolaore (2022)[13].

are informed insiders who receive a perfect signal of $\tilde{V}$ and the remaining $1-\alpha$ are uninformed liquidity traders with no private information. The sell side consists of multiple dealers in Bertrand competition with one another. Dealers are assumed to be risk neutral and face no inventory concerns, and in each period receive a single buy or sell order and set their bid and ask prices equal to conditional expected values. Due to the presence of both informed and uninformed traders, orders are imperfectly informative and thus for an insider it is profitable to buy if he receives the high signal and sell if he receives the low signal. Driven by exogenous liquidity concerns rather than profit seeking, uninformed traders are assumed to buy and sell with equal probability regardless of the true value. Hence the presence of asymmetric information implies that the dealer's conditional expectation upon observing a buy order is higher than upon observing a sell order, leading to the bid-ask spread $s = \alpha(V^H - V^L)$

In [10] this model is modified to account for short selling by assuming that a proportion h of traders already held the asset within their portfolios, and hence a trader who wishes to sell would with probability $1-h$ need to sell short. By default short selling is restricted but not prohibited ($c = 1$), and only informed traders would be willing to do so. Thus sell orders overall will be more informative than buy orders, dragging down the bid price. However, with a short sale ban ($c = 0$) in place, both types of trader will only be able to sell if they hold the stock. Thus sell orders will no longer come disproportionately from the informed, which raises the bid price and restores the spread from the baseline model. Therefore, rather than treating the ban as disrupting the standard functions of the market, the Diamond and Verrecchia (1987)[10] model treats short selling as the disruption, and the ban as a remedy that restores the market to its normal condition.

3 Data

The innovation of our paper in comparison to previous studies is that, rather than evaluating whether the ban was uniformly beneficial or harmful, we consider the possibility that it could improve or reduce liquidity depending on a priori characteristics of the stock. Given the link between short selling and informed trading, we postulate that stocks with higher relative spreads will be those with a greater proportion of informed short sellers, and hence a ban will serve to limit informed or predatory trading and improve liquidity.

Conversely, stocks with lower relative spreads have comparatively fewer informed traders, and the effect of a ban would primarily be to increase order processing costs.

Our sample is drawn from Bloomberg and consists of all stocks listed on Western European exchanges with a market capitalization of at least 100 million euro as of January 1st, 2020, for a total of 771 in the banned countries and 1,961 in the non-banned countries.[2] We collected daily price, quotes, volume, and volatility for a 12 month period centered around the two month ban, from October 2019 to September 2020, inclusive.

To study the heterogeneous impacts of the ban, we further divide the stocks into three groups based on their average relative spreads before the crisis, that is, from Oct. 1, 2019 to Feb. 1, 2020. Stocks whose average spreads are below the 25th percentile comprise the low-spread group, those between the 25th and 75th percentiles the mid-spread group, and those above the 75th percentile the high-spread group. If, as anticipated, the higher spread stocks see proportionally more activity from informed traders, then imposing the ban should reduce their spreads as short sellers are driven away, while for lower spread stocks, the effect would only be to increase order processing costs and thus increase spreads. Tables 1 and 2 summarize the sample.

[2]We initially have 789 stocks in the banned group and 1,993 stocks in the unbanned group. We dropped 24 stocks whose relative spreads are missing for the whole sample period. Further, we dropped the top 0.5% and bottom 0.5% (in terms of average relative spread) of stocks to make sure our results are not driven by outliers. This leads to the number of stocks listed in Table 1.

Table 1: Summary of Stocks

Banned					Unbanned				
Exchange Code	High	Mid	Low	Total	Exchange Code	High	Mid	Low	Total
AV	10	30	7	47	CY	2	1	0	3
BB	16	49	22	87	DC	4	39	29	72
FP	55	143	106	304	FH	6	53	23	82
GA	8	32	0	40	GR	129	206	8	343
HM	3	1	0	4	ID	3	11	5	19
IM	21	94	49	164	IR	3	15	0	18
SM	41	43	41	125	LI	7	5	1	13
					LN	268	272	230	770
					LX	7	0	0	7
					MV	13	1	0	14
					NO	25	73	27	125
					PL	6	5	10	21
					PZ	3	0	0	3
					SS	17	176	75	268
					SW	36	117	50	203
Total	154	392	225	771		529	974	458	1,961

Table 2: Summary Statistics

		Banned (771 stocks)		Unbanned (1,961 stocks)	
	Variable	Mean	Stdev	Mean	Stdev
Before Ban (121 days)	Return (%)	-0.237	2.480	-0.213	2.550
	Volatility	28.00	17.83	31.79	20.59
	Volume (log)	4.580	1.416	4.912	1.196
	Zero Volume Days (%)	9.928	23.11	7.860	17.85
	Relative Spread (%)	0.855	1.458	1.151	1.780
During Ban (43 days)	Return (%)	0.306	3.499	0.397	4.251
	Volatility	70.66	33.40	81.35	40.20
	Volume (log)	4.609	1.423	5.054	1.235
	Zero Volume Days (%)	13.32	21.81	10.30	16.20
	Relative Spread (%)	1.319	2.247	1.783	2.812
After Ban (98 days)	Return (%)	0.067	2.247	0.155	2.399
	Volatility	36.52	19.96	41.58	27.44
	Volume (log)	4.523	1.462	4.906	1.226
	Zero Volume Days (%)	7.333	22.88	6.364	16.16
	Relative Spread (%)	1.034	1.694	1.426	2.218

4 Empirical Methods

To measure the effect of the ban, we use a difference-in-difference model by comparing various measures of liquidity, as well as prices and volatility, for stocks affected by the short sale ban with those of comparable stocks that were not affected by the ban. The empirical specification is

$$Y_{it} = \alpha_i + \tau \times Ban_{it} + \beta\, \mathbf{X}_{it} + \theta_t + \epsilon_{it}. \tag{1}$$

The subscript i represents stock, and t represents date. For Y_{it}, we consider the relative spread, Amihud illiquidity measure, and number of zero volume trading days as measures of liquidity, as well as the level and volatility of the stock price in order to estimate the effect of the ban in supporting prices and mitigating market volatility. In the models of liquidity, we include a ban indicator variable Ban_{it}; controls $\mathbf{X}_{it}$ for log close price, log trading volume, and log price volatility; as well as stock-level fixed effect α_i and the daily time fixed effect θ_t. In the models of price and volatility, we include only the ban indicator, the stock-level fixed effect α_i, and the daily time fixed effect θ_t.

In addition to analyzing the full sample, we estimate the heterogeneous treatment effects by

$$Y_{it} = \alpha_i + \sum_j \tau_j \times Group_{ij} \times Ban_{it} + \beta\,\mathbf{X}_{it} + \sum_j Group_{ij} \times \theta_{jt} + \epsilon_{it}. \quad (2)$$

Here j indicates the group by relative spread. $Group_{ij}$ is a dummy indicator, and hence τ_j gives the group-specific treatment effect of the ban. We further add group-specific daily time fixed effects given by $Group_{ij} \times \theta_{jt}$.

We run our regressions over three separate time periods, in order to distinguish between the different effects of imposing and lifting the ban. The full sample runs from October 1st, 2019 to September 30th, 2020. To isolate the effect of imposing the ban, we look at a subsample from the beginning of the sample to just before the ban was lifted, running from October 1st, 2019 to May 17th, 2020. Similarly to study the impact of lifting the ban only, we take another subsample from just after the ban was imposed to the end of the sample, running from March 18th to September 30th, 2020.

As a robustness check, we further estimate the impact of the ban using the synthetic difference-in-differences (SDID) method by Arkhangelsky et al. (2021)[2]. The SDID method can be viewed as an extension to the two-way fixed effects model used in Equation (1) (but without the controls $\mathbf{X}_{it}$). It first chooses the unit weights $\hat{\omega}^{sdid}$ that match the pretreatment trends of unbanned stocks with those of the banned stocks in the parallel sense and the time weights $\hat{\lambda}^{sdid}$ that that balance pre-exposure time periods with postexposure ones. Then, it estimates the parameters via the weighted DID regression:

$$\left(\hat{\tau}^{sdid},\, \hat{\alpha},\, \hat{\beta}\right) = \underset{\tau,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \alpha_i - \theta_t - \tau \times Ban_{it}\right)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\}. \quad (3)$$

By assigning different weights to different control units, the SDID method directly addresses the parallel trends assumption and thus provides more robust estimates. To better isolate the impact of the ban, we focus on a shorter time window around the ban, which runs from January 1st to July 1st, 2020. Using the full sample generates similar patterns. We directly compare the DID and SDID estimations in the appendix.

5 Results

We first analyze the liquidity effects and then the stock price and volatility effects, the latter of which are the more likely intended concerns of regulators in enacting short sale bans.

5.1 Liquidity

The relative spread for stock i at date t is given by

$$Spread_{it} = \frac{Ask_{it} - Bid_{it}}{Mid_{it}}. \tag{4}$$

Ask_{it} and Bid_{it} are the quoted ask and bid prices respectively, and $Mid_{it} = \frac{Ask_{it}+Bid_{it}}{2}$ the midpoint. As shown in Table 3, the ban produced a mild but significant reduction in spreads overall, and we can infer that banning short sales led to an improvement in liquidity overall. This holds whether we look at the full sample, or look specifically at the impact of imposing the ban or lifting the ban, in which case the negative coefficient indicates that ending the ban increased spreads.

Table 3: Impact of Short Sale Ban on Relative Spread

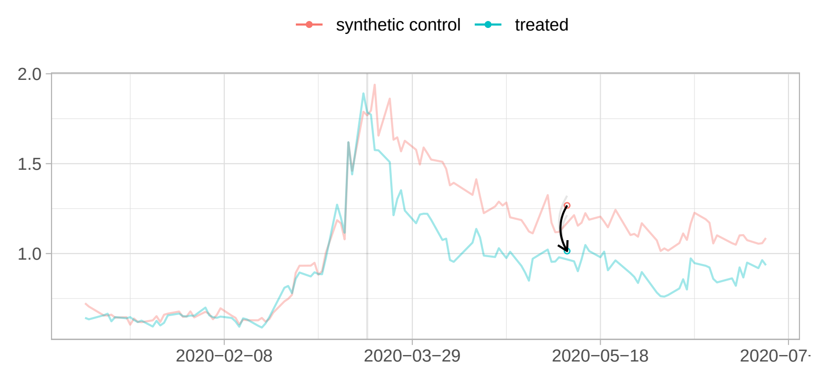
	(1) Full	(2) Full	(3) Imposing	(4) Imposing	(5) Lifting	(6) Lifting
Ban	-0.140***		-0.202***		-0.080***	
	(0.032)		(0.037)		(0.030)	
High×Ban		-0.442***		-0.666***		-0.214**
		(0.112)		(0.133)		(0.104)
Mid×Ban		0.065**		0.087***		0.032
		(0.028)		(0.031)		(0.028)
Low×Ban		0.010		-0.008		0.039***
		(0.012)		(0.014)		(0.012)
Observations	668,278	668,278	412,142	412,142	362,031	362,031
Adjusted R^2	0.691	0.706	0.673	0.696	0.735	0.741

Note: Robust standard errors clustered at the stock and the time level are shown in the parentheses. We include stock-level fixed effects, daily time fixed effects, and other control variables, including log close price, log trading volume, and log price volatility. Dropping the control variables does not change our main results.
*** p<0.01, ** p<0.05, * p<0.1.

The sub-group results are more illuminating, indicating that the ban significantly reduced spreads for the high-spread group, while it increased spreads for the mid and low-spread groups, albeit to a lesser degree. This conforms to our prediction that the ban would improve the liquidity of high-spread stocks, which are likely to have a higher proportion of informed traders and thus short sellers that increase adverse selection costs. On the other hand, with low-spread stocks, adverse selection is already less severe, and a ban would primarily increase order processing costs.

These results are more easily visualized by looking at the average spreads of the control and treatment groups (Figure 1). We see that while the onset of the pandemic greatly increased spreads for both groups, spreads on the stocks subject to the short sale ban dropped rapidly, while those of unbanned stocks remained well above their pre-pandemic levels, and that effect continued past the end of the ban.

Figure 1: Relative Spreads (All Stocks)



Dividing the stocks by spread group further highlights this disparity. Figure 2 shows that high-spread stocks in the control group had significantly higher spreads than correspondingly high-spread stocks in the treatment group from the imposition of the ban until after its lifting. On the other hand, Figure 3 shows no gap between mid-spread stocks in the two groups, and Figure 4 shows that for low-spread stocks, the treatment group only experienced a mild increase in spreads compared to the control group, and that effect did not persist past the end of the ban.

Figure 2: Relative Spreads (High-Spread Group)

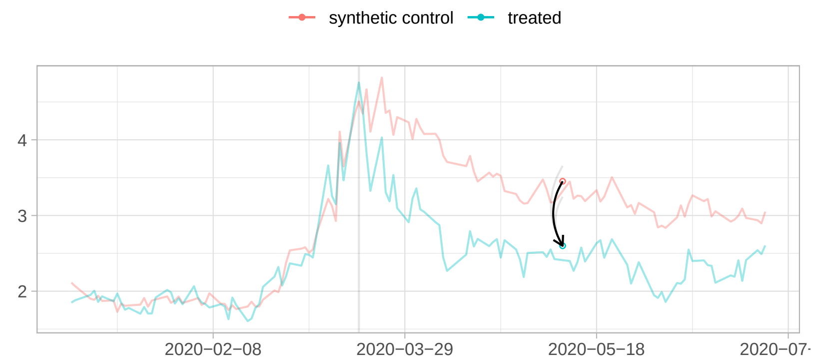


Figure 3: Relative Spreads (Mid-Spread Group)

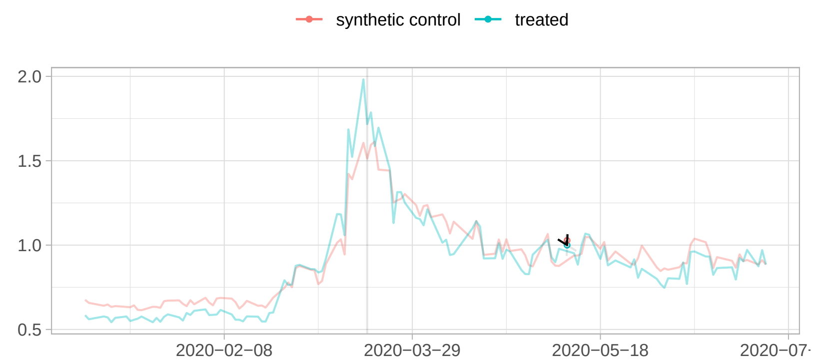
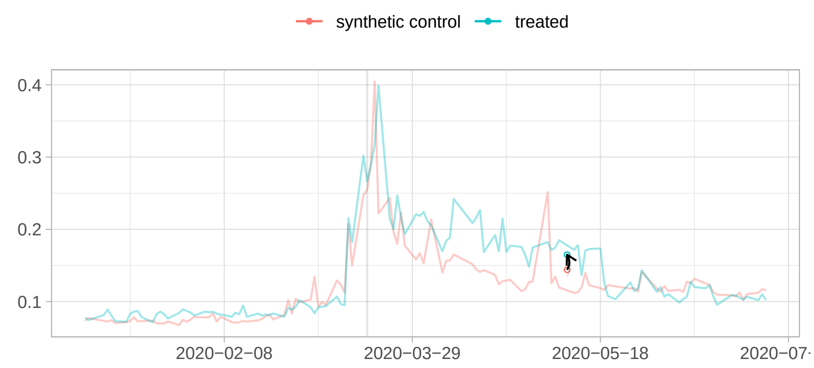


Figure 4: Relative Spreads (Low-Spread Group)



Our second liquidity metric, the Amihud illiquidity measure, is defined as the average daily absolute value of return weighted by dollar volume.

$$ILLIQ_{it} = \frac{1}{T} \sum_{s=t-T}^{t} \frac{|R_{is}|}{P_{is} V_{is}}. \tag{5}$$

Thus $IILIQ_{it}$ is a measure of the price impact of order flow on a given stock. We compute Amihud at the weekly level, with $T = 5$, in order to preserve enough observations in the time series dimension to detect effects of the ban. As shown in Table 4, the ban had an insignificant effect on the Amihud measure, viewed over all stocks. However, the results by group broadly align with those on relative spread, with the coefficient estimate being significantly negative for high-spread stocks, insignificant for mid-spread stocks, and significantly positive for low-spread stocks. This result further supports the interpretation that the ban improved liquidity for already illiquid stocks but had the opposite effect on stocks that would otherwise be the most liquid.

Table 4: Impact of Short Sale Ban on Amihud

	(1) Full	(2) Full	(3) Imposing	(4) Imposing	(5) Lifting	(6) Lifting
Ban	0.009		0.046		-0.024	
	(0.042)		(0.041)		(0.043)	
High×Ban		-0.127**		-0.083		-0.176***
		(0.056)		(0.058)		(0.058)
Mid×Ban		-0.003		0.029		-0.033
		(0.036)		(0.035)		(0.039)
Low×Ban		0.111*		0.147**		0.085
		(0.058)		(0.056)		(0.057)
Observations	138,117	138,117	83,399	83,399	75,611	75,611
Adjusted R^2	0.165	0.173	0.206	0.215	0.161	0.169

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

One final liquidity variable we consider is the percentage of zero-volume trading days, defined as the proportion of days within the last $T = 10$ that a stock did not trade.

$$ZeroVol_{it} = \frac{1}{T} \sum_{s=t-T}^{t} \mathbf{1}\{V_{is} = 0\}. \tag{6}$$

The $ZeroVol_{it}$ measure indicates a severe lack of liquidity for a stock. As seen in Table 5, the ban significantly increased zero-trading days overall. Again, the effect varies by group, with an insignificant effect on high-spread stocks but a significant increase in zero-trading days among mid- and low-spread stocks. This indicates that the ban did have the effect of stifling trading on liquid stocks, without correspondingly stimulating activity for illiquid stocks.

Table 5: Impact of Short Sale Ban on Zero-Volume Trading Days

	(1) Full	(2) Full	(3) Imposing	(4) Imposing	(5) Lifting	(6) Lifting
Ban	0.012***		0.006*		0.020***	
	(0.003)		(0.003)		(0.004)	
High×Ban		0.005		0.002		0.009
		(0.007)		(0.008)		(0.008)
Mid×Ban		0.013***		0.008***		0.019***
		(0.003)		(0.003)		(0.004)
Low×Ban		0.012***		0.004		0.020***
		(0.004)		(0.004)		(0.005)
Observations	648,987	648,987	391,377	391,377	364,061	364,061
Adjusted R^2	0.885	0.888	0.900	0.904	0.888	0.890

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

5.2 Stock Prices and Volatility

While most academic studies focus primarily on the effects of short sale bans on spreads and other liquidity measures, regulators first and foremost state the goal of preventing major price declines and resulting spikes in price volatility. Thus, we next study the effect of the ban on stock prices, taking the log closing price $\log P_{it}$ as the outcome variable.

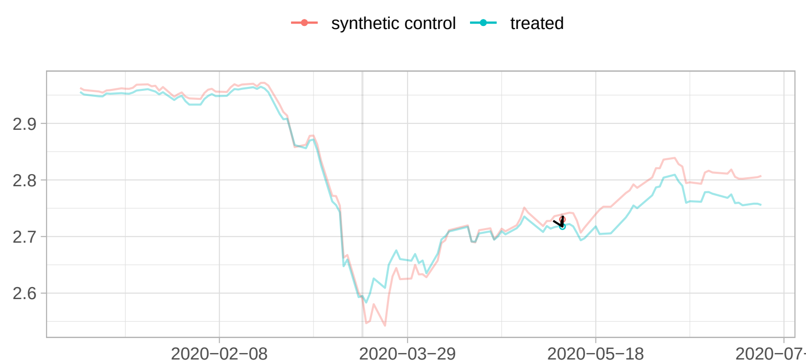
Table 6 shows that, taken over the full time period, the price effect of the ban was significantly positive, both for the full sample of stocks and for each individual group. Notably, while the effect of lifting the ban is significant for all groups, indicating that the ban did not permanently distort prices past its own expiration, the effect of imposing the ban was significant only for the high-spread group. As these are the least liquid stocks and thus the most vulnerable to predatory short selling, this result indicates that the ban effectively supported the prices of the stocks most likely to be targeted by short sellers without severely distorting those of more liquid, less vulnerable, stocks.

Table 6: Impact of Short Sale Ban on Stock Prices

	(1) Full	(2) Full	(3) Imposing	(4) Imposing	(5) Lifting	(6) Lifting
Ban	0.044***		0.019*		0.077***	
	(0.008)		(0.010)		(0.008)	
High×Ban		0.074***		0.084***		0.083***
		(0.013)		(0.019)		(0.013)
Mid×Ban		0.043***		0.004		0.091***
		(0.010)		(0.013)		(0.010)
Low×Ban		0.036***		0.017		0.058***
		(0.011)		(0.018)		(0.013)
Observations	671,888	671,888	414,278	414,278	364,061	364,061
Adjusted R^2	0.990	0.990	0.994	0.994	0.996	0.996

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

Figure 5: Stock Prices (All Stocks)



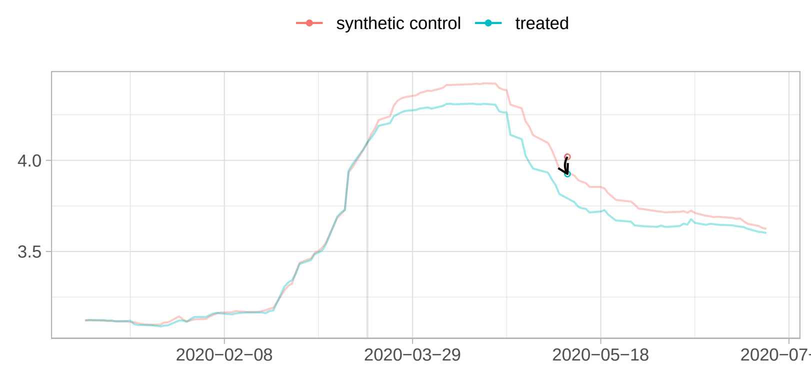
Lastly, we analyze the impact on stock price volatility. When we use the logarithm of the 30-day moving volatility as our measure, we find that the short sale ban led to a significant reduction in volatility overall. The results vary, however, depending on the level of pre-ban illiquidity (Table 7), with the most pronounced effect appearing for high-spread stocks. This pattern aligns with the notion that restrictions will selectively drive out informed traders, and therefore, for illiquid stocks, the ban should mitigate the effects of informed, and potentially predatory, trading. For the more liquid stocks, the results are more sensitive to model specification, with smaller and less consistently significant coefficient estimates. We provide direct comparisons of the TWFE and SDID results in the appendix.

Table 7: Impact of Short Sale Ban on Volatility

	(1) Full	(2) Full	(3) Imposing	(4) Imposing	(5) Lifting	(6) Lifting
Ban	-0.031**		-0.037**		-0.051***	
	(0.013)		(0.016)		(0.014)	
High×Ban		-0.224***		-0.285***		-0.176***
		(0.044)		(0.050)		(0.048)
Mid×Ban		-0.025		-0.045**		-0.034*
		(0.015)		(0.019)		(0.017)
Low×Ban		0.057***		0.099***		-0.016
		(0.018)		(0.023)		(0.017)
Observations	671,888	671,888	414,278	414,278	364,061	364,061
Adjusted R^2	0.784	0.788	0.823	0.828	0.832	0.834

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

Figure 6: Log Volatility (All Stocks)



Using the difference between each stock's daily high and low prices as a proxy for intra-day volatility,

$$HML_{it} = \log\left(\frac{High_{it} - Low_{it}}{Mid_{it}} + 1\right),$$

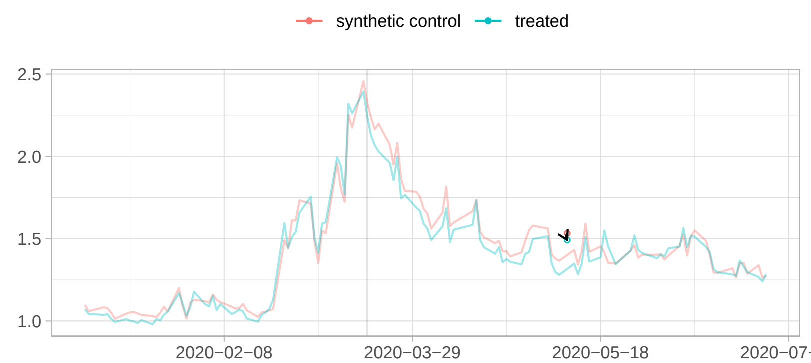
produces broadly similar results as those for the moving volatility measure (Table 8).

Table 8: Impact of Short Sale Ban on Intraday Volatility

	(1) Full	(2) Full	(3) Imposing	(4) Imposing	(5) Lifting	(6) Lifting
Ban	-0.024***		-0.015*		-0.043***	
	(0.007)		(0.008)		(0.007)	
High×Ban		-0.100***		-0.113***		-0.089***
		(0.018)		(0.020)		(0.019)
Mid×Ban		-0.050***		-0.049***		-0.064***
		(0.008)		(0.010)		(0.009)
Low×Ban		0.027***		0.054***		-0.011
		(0.010)		(0.012)		(0.010)
Observations	669,275	669,275	412,942	412,942	362,250	362,250
Adjusted R^2	0.646	0.650	0.677	0.682	0.644	0.647

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

Figure 7: Intraday Volatility (All Stocks)



Overall, volatility falls in the full sample and in the high-spread group, but increases for the low-spread group. These findings lend further weight to the notion that a ban can be a useful tool in mitigating volatility during crisis periods, particularly for those stocks most vulnerable to predatory trading.

6 Conclusion

Our analysis shows that, by a variety of disparate measures, the short sale bans imposed by several market regulatory authorities during the Covid-related crisis of 2020 significantly improved liquidity for the most illiquid stocks, while it worsened it somewhat for stocks that were very liquid. These results align with the assumption that stocks with higher spreads will have a higher proportion of informed traders, who would be disproportionately affected by short selling restrictions at a time of market crisis. Interestingly, the comparative reduction in relative spreads persists even after the ban was lifted, suggesting that short sellers fled these stocks and did not return afterwards. By contrast, more liquid stocks would likely have seen less informed trading activity, and thus the only effect of the ban would be to increase order processing costs, and this effect disappeared with the ending of the ban. Furthermore, looking at the stock prices and volatility, our results demonstrate that the temporary ban succeeded in supporting price levels and dampening volatility.

These results have interesting policy implications, since they demonstrate that, contrary to the assertions of much of the existing literature, short sale bans can be effective. Their effects are most pronounced for the least liquid stocks in the sample, where alleviating the adverse selection issues posed by informed short sellers far outweighs the additional order processing costs that the ban might impose. By targeting the ban on these stocks rather than imposing them market-wide, these benefits could be captured without imposing additional costs on the rest of the market.

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A Comparing TWFE and SDID Estimates

In this appendix, we provide direct comparisons of the TWFE and SDID model parameter estimates. Though the models are theoretically similar in many respects, we need to make several adjustments to ensure meaningful, apples-to-apples comparisons. First, we focus on the shorter period from January 2nd to May 17th, 2020. Since the SDID model assumes the treatment is permanent, we drop the observations after May 18th, 2020 when the ban was lifted. We also drop the observations in 2019, when markets were relatively stable, in order to reduce the computational cost. Shortening the pre-test period does not qualitatively alter the results. Likewise, because the SDID model cannot handle missing values, we need to drop from the TWFE model all stocks with excessive missing outcome variables (about 1% of the total sample) and use backward filling for stocks with a small number of missing values to keep the data set identical to that used in the SDID model.

Since the clustered standard error method does not apply to the SDID model, we report bootstrap standard errors for both models. Finally, to keep the estimates fully comparable, we do not include any control variables. These adjustments naturally cause the estimates of the new TWFE model to differ somewhat from those reported in Section 5. Nonetheless, we find most estimates are quantitatively similar (Table 9 versus tables in Section 5.) The main difference appears in the effects on the rolling volatility measure, in which case, as we noted previously, the results vary depending on model specification. Clearly, the volatility results for the high- and mid-spread groups remain strong, regardless of model.

Table 9: Comparing TWFE and SDID Estimates

Outcome Variable	Model	(1) All	(2) High	(3) Mid	(4) Low
Relative Spread	TWFE	-0.243***	-0.889***	0.029*	0.034***
		(0.035)	(0.119)	(0.019)	(0.007)
	SDID	-0.281***	-0.927***	-0.034	0.043***
		(0.033)	(0.129)	(0.027)	(0.007)
Log Relative Spread	TWFE	-0.022***	-0.136***	0.018***	0.026***
		(0.007)	(0.022)	(0.007)	(0.005)
	SDID	-0.033***	-0.145***	0.001	0.031***
		(0.007)	(0.025)	(0.008)	(0.004)
Amihud	TWFE	0.165***	-0.166	0.122***	0.534***
		(0.046)	(0.229)	(0.050)	(0.037)
	SDID	0.151***	-0.103	0.088*	0.533***
		(0.050)	(0.281)	(0.054)	(0.044)
Log Amihud	TWFE	0.084***	-0.043	0.050***	0.202***
		(0.012)	(0.048)	(0.015)	(0.014)
	SDID	0.076***	-0.019	0.032***	0.190***
		(0.012)	(0.051)	(0.016)	(0.017)
Zero-Volume Trading Day	TWFE	0.009***	-0.007	0.014***	0.011***
		(0.002)	(0.009)	(0.001)	(0.001)
	SDID	0.005***	0.003	0.006***	0.004***
		(0.002)	(0.008)	(0.002)	(0.000)
Log Stock Price	TWFE	0.033***	0.102***	0.022***	0.026*
		(0.008)	(0.015)	(0.011)	(0.017)
	SDID	0.006*	0.000	-0.005	0.015**
		(0.004)	(0.011)	(0.006)	(0.009)
Log Volatility	TWFE	-0.043***	-0.296***	-0.054***	0.073***
		(0.016)	(0.057)	(0.019)	(0.024)
	SDID	-0.112***	-0.202***	-0.116***	-0.062***
		(0.010)	(0.036)	(0.011)	(0.016)
Log Intra-day Volatility	TWFE	-0.064***	-0.129***	-0.098***	-0.020*
		(0.009)	(0.026)	(0.012)	(0.015)
	SDID	-0.078***	-0.087***	-0.113***	-0.039***
		(0.009)	(0.023)	(0.011)	(0.013)

*** p<0.01, ** p<0.05, * p<0.1. The bootstrap standard errors are reported in parenthesis.

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