The Double-Edged Sword of The 2020 European Short-Selling Bans*

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Abstract

In this paper, we present a theoretical framework to study the effects of short-selling bans on markets and we test its predictions using cross-sectional variation in the European 2020 short-selling bans. The model's novelty is in the way that institutional ownership affects the conditions under which bans help avert a sharp decline in prices. Empirically we find, consistent with the model, that tail risk was reduced in countries that implemented short-selling bans, and that this effect was more pronounced in stocks with low institutional ownership. However, bans were detrimental for liquidity and failed to support the average level of prices.

Keywords: short selling, ban, liquidity, price discovery, covid.

JEL Classification: G01, G12, G14, G18.

1 Introduction

> *"Some European countries have introduced short selling bans ... The FCA has not introduced such a ban. Most European National Competent Authorities have not introduced such bans. Nor has the United States or any other major financial market ... [T]here is no evidence that short selling has been the driver of recent market falls."*
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> — Financial Conduct Authority, Statement on UK markets, 23 March 2020.

The controversial effects of short-selling bans on stock market liquidity and price discovery have been the subject of a longstanding debate among regulators around the world.[1] Disagreement on the efficacy of short-selling restrictions to promote stability in volatile markets has reemerged during the Covid-19 outbreak and is well reflected in a statement released by the Financial Conduct Authority, the UK national watchdog, on March 23, 2020: "*A great many investment and risk management strategies rely on the ability to take 'long' and 'short' positions. These benefit a wide range of ordinary investors ... The loss of these benefits would need to be carefully balanced before determining that any intervention to prevent short selling was appropriate.*"

In this paper, we study the potential costs and benefits of short-selling bans. To guide our empirical work, we first build a stylized theory model that endogenizes the decision of regulators to impose short-selling bans, and then derive theoretical implications of such restrictions for prices and liquidity. Empirically, we verify the model's predictions by exploiting the differences between European countries that imposed short-selling bans and countries that did not between March and May 2020.

Our model extends the seminal work of Diamond and Verrecchia (1987) by introducing a Regulator whose goal is to avert a sharp decline in prices. We assume that she is uncertain about the liquidity needs of Noise traders and that she optimally decides whether to prohibit short-selling activity, based on various market characteristics. We show that a necessary condition for a Regulator to take action is that fundamentals are weak or that the uncertainty

[1] See, for example, 'Regulators across Europe clash over bans on short selling', *Financial Times*, 31 March 2020.

about the liquidity needs of Noise traders is large.[2] However, the need for an action does not necessarily imply that a short-selling ban will be optimal; in fact, the effectiveness of the ban depends on the ratio of informed to noise traders who own the stock. In particular, the probability of a sharp decline in prices depends on the likelihood of a low bid price and on the probability of a sell order (conditional on that low bid). While the latter always decreases when short-selling is not allowed, the former may decrease or increase, depending on the fraction of informed traders who own the stock. Intuitively, the smaller the fraction of informed traders who own the stock, the lower the adverse selection facing market makers and the less likely that a sell order is submitted by an informed investor.

Using institutional ownership as a proxy for the fraction of informed traders who own the stock, we develop and test five hypotheses leading to the following results. First, we find that institutional ownership is lower in countries that imposed short-selling bans. Second, in line with the majority of the previous literature (e.g., Beber and Pagano, 2013), we find that short-selling bans during the Covid-19 pandemic were associated with a deterioration of liquidity by approximately 12-13 basis points, as measured by bid-ask spreads. Third, we show that short-selling bans help support the left tail of stock returns. As measured by median or mean values, however, stocks subject to short-selling bans tend to underperform stocks not affected by such restrictions, in line with the findings of the prior literature (e.g., Beber and Pagano, 2013). Fourth, when short-selling is prohibited, stocks with higher institutional ownership experience greater deterioration of liquidity. Indicatively, bid-ask spreads of stocks with low institutional ownership increase by 8 basis points on average as a result of the bans, whereas bid-ask spreads of stocks with high institutional ownership increase by 20-25 basis points. Fifth, when short-selling bans are in place, stocks with lower institutional ownership benefit more in terms of limiting extreme negative outcomes (i.e., left tail support). We estimate that once short-selling bans are in place, the fifth percentile of stock returns would be approximately 1 percent higher if institutional ownership were 10 percent lower.

The key policy implication of our findings is that regulators should carefully consider the

[2]Indeed, this uncertainty is likely to have played a great role in the recent decision of various European countries to implement short-sellin bans. For example, an ESMA opinion issued on 18th of March concerning the French short-selling bans states: *"AMF reports to have observed examples of disinformation, rumours and false news... these rumours may affect listed companies and may damage the confidence of investors on an efficient market"*.

costs and benefits of introducing short-selling bans while also taking into consideration the appropriate market characteristics. More specifically, we believe that regulators could consider the implementation of short-selling bans, not to the whole stock market, but to parts of it. In practice, considering the regulatory frictions of implementing different short selling constraints in various individual stocks, one solution would be to impose the bans to all the stocks belonging to particular industries. The criteria to impose the ban would then have to include, among other things (e.g. price levels of the corresponding stocks, the costs of a potential further decline in these prices or the uncertainty about the liquidity needs of the Noise traders), the average level of institutional ownership in that particular industry; the lower this is, the more effective would the regulation be in deterring a huge decline in prices. Our empirical results, moreover, corroborate the evidence reported earlier by Beber and Pagano (2013) and more recently by Enriques and Pagano (2020), who caution against the use of short-selling bans due to their harmful effects on liquidity and their failure to support average price levels.

The power to temporarily restrict short sales of financial instruments in European trading venues is granted to national authorities by Article 23 of the Short Selling Regulation. Under the provisions of this article, a national competent authority shall prohibit short selling in the case of a significant fall in the price of a financial instrument in a single trading day.[3] Effective on March 18, 2020, Austria, Belgium, France, Greece, Italy, and Spain exercised their right under Article 23 of the European Short Selling Regulation and decided to introduce a temporary ban on taking or increasing net short positions with respect to all shares admitted to their trading venues. Initially, the bans were introduced for a period of one month. On April 15th, however, in a coordinated fashion, all six countries notified the European Securities and Markets Authority (ESMA) of their intention to extend the ban of short sales for one more month. ESMA issued positive opinions on the proposed measures, and the bans remained in place until May 18, 2020. The scope of the bans applied to any natural or legal person, regardless of where they are located, and covered all stocks traded in cash and derivatives markets, including American Depository Receipts. Bearish intraday operations

[3] A significant fall refers to a price drop larger than 10% for liquids shares, larger than 20% for illiquid shares whose price is higher than 0.50 euro, and larger than 40% for illiquid shares whose price is below 0.50 euro. See here the announcement of the ban of short selling by the French national competent authority, Autorité des Marchés Financiers (AMF).

were also in scope. The prohibitions did not apply to market-making activities or trading in index-related instruments. More details are provided in Section 3.

Our paper is related to three streams of the literature. First, it is related to the empirical work on the effects of short-selling bans. For example, Beber and Pagano (2013) investigate the impact of short-selling bans around the world during the 2007-09 financial crisis and conclude that short-selling bans are detrimental for liquidity, slow price discovery, and fail to support prices except for US financial stocks. Beber, Fabbri, Pagano, and Simonelli (2017) find that bans increase the probability of default and volatility. Boehmer, Jones, and Zhang (2013) study the response of liquidity to the short-selling ban imposed from September 18, 2008, to October 8, 2008, in the United States. In particular, they exploit the difference between financial stocks that were targeted by the ban and those that were not. Similarly, Marsh and Payne (2012) study the effects of short-selling bans in the UK stock market in 2008 and document a deterioration of liquidity on affected stocks. Moreover, they claim that bans may have managed to "*arrest sharp declines in stock prices*" according to their results. Finally, Battalio and Schultz (2011) investigate the impact of 2008 bans on the option market in the US and document a dramatic increase in the bid-ask spreads of affected options. Our paper contributes to this literature by examining short-selling bans during a global public health crisis that witnessed an unprecedented economic and financial meltdown.

Moreover, we contribute to a growing theoretical literature that evaluates the effects of short-selling bans. Miller (1977) predicts that short-selling bans lead to overpricing, Diamond and Verrecchia (1987) build on Glosten and Milgrom (1985) and show that this is not true in a rational expectations framework and stocks are not systematically overpriced when short sales are prohibited but liquidity and price discovery are compromised. Hong and Stein (2003) build a heterogeneous agent model and find that short-selling bans may aggravate price declines, while Bai, Chang, and Wang (2006) point out that short-selling constraints can increase uncertainty about the asset in a model with risk-averse investors, and thus also lead to a decline in prices. On the other hand, Brunnermeier and Oehmke (2014) show how short selling impacts the fundamentals of firms rather than just the price discovery process. They argue that financial institutions may be vulnerable to predatory short selling, which may lead to a bank-run equilibrium, and their model provides a potential justification for temporary restrictions on short selling. Finally, Dixon (2021) endogenizes the incentives for information acquisition when short selling is costly and shows that a ban increases adverse selection on the sell side and reduces it on the buy side. We add to this literature by endogenizing the decision of financial markets authorities (Regulator) to implement a ban on short sales. Moreover, we focus on the implications for the distribution (especially the left tail) of prices of affected stocks.

Our work, finally, also contributes to the broader empirical literature on short selling (e.g., Boehmer, Huszar, and Jordan, 2010; Reed, 2013). In particular, Saffi and Sigurdsson (2011) show that stocks subject to short-selling constraints, as measured by low lending supply, have lower price efficiency and relaxing those constraints does not lead to instability in the form of a higher probability of left tail returns. Jones and Lamont (2002) document evidence consistent with the overpricing hypothesis when short sale constraints bind, but Diether, Lee, and Werner (2009) find that short sellers correctly predict negative future returns. Finally, Bris, Goetzmann, and Zhu (2007) find that short-selling constraints reduce price efficiency and are associated with less negative skewness.

The remainder of this paper is organized as follows. Section 2 introduces the model and its empirical implications, Section 3 describes the data, and Section 4 reports the empirical results. We provide our concluding remarks in Section 5. A separate Appendix contains additional technical details and empirical results.

2 Model

2.1 Setting

We extend the Diamond and Verrecchia (1987) (henceforth DV) model by introducing a Regulator who can act by banning short sales in order to avert a sharp decline in prices. We use a static model with one risky asset with payoff $V \in \{0, 1\}$ and probability of the high payoff denoted by $p = P(V = 1)$. There is a continuum of investors who come to the market sequentially and submit an order to buy or sell one unit of the risky asset or they simply do not trade.[4] The prices are set by competitive risk-neutral Market Makers who set bid-ask prices (equal to their expectation of V conditional on their information set). There are two types of investors, who want to trade with probability g for information or liquidity reasons. Informed traders (I), with mass α, may be fully informed about the payoff V, while Noise traders (N), with mass $1 - \alpha$, act for liquidity reasons and sell or short the asset with probability η_s. Finally, investor $i \in \{I, N\}$ owns the stock with probability h_i[5].

We introduce two deviations from the DV model. First, the economy includes a Regulator (R) who want to avert a disastrous decline in price, and can act by imposing a ban on short sales. In specifying her beliefs, we assume that while η_s (the probability of a sell order by a Noise trader) is known to the Market Maker when he is trading, the Regulator only knows the distribution of η_s, denoted by $f(\eta_s)$, at the time she has to act.[6] Second, in contrast to DV, we assume that h_N, h_I may not be equal. We view this as a very realistic and relevant assumption for what we study. In fact, we will see that the relationship between these two quantities will be very important in the decision making process of the Regulator.

[4]The fact that the demand for the asset is bounded is important to get an equilibrium.

[5]Equivalently, we can think of h_i as the fraction of investors of type i who own the stock.

[6]This assumption ensures that the distribution of prices is non-trivial. Moreover, it implies that the probability of a very low price realization (which we also interpret as a "sharp decrease in price") does not always fall when a ban is implemented, because although the probability of a sell order decreases with the imposition of bans, the probability that the Bid price is low enough may rise.

Summary of notation:

$$\alpha = P(Informed)$$
$$p = P(V = 1)$$
$$g = P(\text{Investor } i \text{ wants to trade})$$
$$\eta_s = P(\text{Noise trader sells})$$
$$h_i = P(\text{Investor } i \text{ owns the stock})$$
$$q = Price$$

Regulator: As already mentioned, the objective of the Regulator is to avoid a sharp fall in price. She wants to ensure that $P(q < c) < x$, where x is a confidence level (say 5%) and c corresponds to a sufficiently low threshold that if the price of the stock were to fall below would pose a risk for financial stability, as perceived by the Regulator.[7] Price may fall below c if two things happen: an investor submits a sell order and the Market Maker sets a bid price below c. The likelihood of the former depends positively on the probability, η_s, that a Noise trader sells. The latter is more likely to happen when η_s is small enough, so that the Market Maker perceives any sell order more likely to contain information. The Regulator chooses whether to impose a short-selling ban depending on its effect on this joint probability.

2.2 Unconstrained Short Selling

We first study the model assuming that all investors can freely short the asset. As in the Diamond and Verrecchia (1987) model, the bid and ask prices are equal to the expectation of V, conditional on the corresponding action of the investor. Since the Market Maker is risk-neutral he submits a bid equal to the expectation of V conditional on that bid being hit. Thus,

[7]For example, Brunnermeier and Oehmke (2014) find that a sharp decrease in the price of financial institutions combined with leverage constraints may lead to a bank-run equilibrium. We assume that c is lower than the maximum value that the Bid can take (which is achieved when $\eta_s = 1$), i.e. $c < \frac{(1-\alpha)p}{1-\alpha p}$. This also ensures that $c < p = E[V]$ so that the price cannot fall sharply when there is simply "No Trade".

$$
\begin{aligned}
Bid = \mathbb{E}\left[V|Sell\right] &= P\left[V=1|Sell\right]\times 1 + \underbrace{P\left[V=0|Sell\right]\times 0}_{=0} \\
&= P\left[V=1|Sell\right] \\
&= \frac{P(Sell|V=1)P(V=1)}{P(Sell)}
\end{aligned} \tag{1}
$$

When $V=1$, any sell order would have to come from Noise traders, hence the conditional probability of receiving a sell order in that case is given by

$$P(Sell|V=1) = g(1-\alpha)\eta_s \tag{2}$$

and the unconditional probability of receiving a sell order is

$$P(Sell) = g\alpha(1-p) + g(1-\alpha)\eta_s \tag{3}$$

Plugging in 2 and 3 to equation 1 above, we get

$$Bid = \frac{(1-\alpha)\eta_s p}{\alpha(1-p)+(1-\alpha)\eta_s} \tag{4}$$

Similarly, the Market Maker submits an ask price equal to the expectation of V conditional on that ask being lifted. Thus,

$$Ask = \mathbb{E}\left[V|Buy\right] = \frac{(\alpha+(1-\alpha)(1-\eta_s))p}{\alpha p+(1-\alpha)(1-\eta_s)} \tag{5}$$

The parameter η_s, which denotes the probability of a sell order by a Noise trader, is known to the Market Maker but not to the Regulator. The Regulator only knows the distribution of $f(\eta_s)$ of η_s. The following lemma, gives us the probability of a sharp decline in prices (in the absence of any short-selling constraints), as perceived by the Regulator.

Lemma 1. *In the case where all agents are unconstrained, the probability that the price, q,* *of the asset falls below a lelel c is:*

$$P(q < c) = \int_0^K ((1-p)g\alpha + (1-\alpha)g\eta) f(\eta) d\eta \tag{6}$$

where $K = \frac{c\alpha(1-p)}{(1-\alpha)(p-c)}$. *In particular, if* $\eta_s \sim U[0,1]$ *then*

$$P(q < c) = (1-p)\alpha g K + \frac{1-\alpha}{2} g K^2$$

Proof. Note that $Bid < c$ iff $\eta_s < \frac{c\alpha(1-p)}{(1-\alpha)(p-c)} = K$. Therefore, if c is small enough (so that a lower price can only be attained when there is a sell order),[8] we have:

$$\begin{aligned} P(q < c) &= \mathbb{E}[\mathbb{E}[\mathbb{1}_{q<c} \mid \eta_s]] \\ &= \mathbb{E}[P(sell \mid \eta_s)\mathbb{1}_{Bid<c}] \\ &= \mathbb{E}\left[P(sell \mid \eta_s) \mid \eta_s < K\right] P(\eta_s < K) \end{aligned} \tag{7}$$

where $\mathbb{1}$ is the indicator function. Noting that $P(sell \mid \eta_s) = (1-p)g\alpha + (1-\alpha)g\eta_s$, the above can be rewritten as:

$$P(q < c) = P(\eta_s < K) \cdot ((1-p)g\alpha + (1-\alpha)g\mathbb{E}[\eta_s \mid \eta_s < K])$$

which concludes the proof. □

The price is more likely to be low when Noise traders are unlikely to sell (which would make any potential negative information of I very easily revealed) or when the expectation of a sell order (conditional on a low bid) is high. In Lemma 4 in the Appendix, we specify a general family of distributions $f_u(\eta)$, parametrized by u and we show that, keeping K constant (and assuming that c is sufficiently small), $P(q < c)$ increases as the variance of η_s increases. Thus, the Regulator is more likely to consider imposing bans when uncertainty about the liquidity needs of the Noise traders is larger.

[8]In particular, the assumption is that $c < p = E[V|\text{No Trade}]$. Otherwise the price could be below c even when there is "no-trade", and this would complicate the final outcome, as explained in more detail when we study the median price.

From the above result, we can then obtain a necessary condition for the implementation of bans:

Result 1. *There exists a threshold ζ such that the Regulator imposes short-selling bans only if:*

$$\frac{c\alpha(1-p)}{(1-\alpha)(p-c)} > \zeta.$$

Proof. We know that the regulator acts only if $P(q < c) > x$, where x is her pre-specified level of confidence. Equivalently, from equation (6): $\int_0^K ((1-p)g\alpha + (1-\alpha)g\eta) f(\eta)d\eta > \zeta$. Since the left-hand side is increasing in K, we obtain the above necessary condition. □

In particular, the Regulator may only impose bans at a period when the probability of a high payoff is low, when many informed traders exist in the economy or when the threshold c (determining the desired support of the left tail of prices) increases. While the above conditions, which are likely to happen during a crisis, give us ssome necessary conditions for the Regulator to act, the actual enforcement of bans will also depend on the impact of the new rules, and the subsequent distribution of the price. Hence, in the following section we examine the asset pricing implications assuming that short-selling bans have been imposed.

2.3 Imposing Short-Selling Bans

When short selling is allowed, the Market Maker cannot distinguish a sell order from a short sale. However, when short selling is prohibited, then investor can only sell if she already owns the stock. Thus, when the Market Maker submits a bid under short selling restrictions, the probability of a sell order entering his calculation of the bid price will be adjusted by the fraction of informed investors who own the stock. Crucially, we assume that the fraction of Informed traders who own the stock (h_I) may be different from the fraction of Noise traders who own the stock (h_N), as this affects the adverse selection facing the Market Maker. Concretely, the new bid price, denoted by SSBid, is given by:

$$SSBid = \frac{P(Sell|V=1)P(V=1)}{P(Sell)} = \frac{(1-\alpha)h_N\eta_s p}{\alpha h_I(1-p) + (1-\alpha)h_N\eta_s} \tag{8}$$

Thus, the relationship between Bid and SSBid depends on the fraction h_I/h_N.[9] We now examine how likely it is that prices fall sharply in this economy (assuming again that c is sufficiently low so that it is lower than the maximum Bid price and prices can fall below that threshold only through a sell order).[10] Following the logic of lemma 1 and noting that $SSBid < c \iff \eta_s < \frac{h_I c\alpha(1-p)}{h_N(1-\alpha)(p-c)} = \frac{h_I}{h_N}K$ we get the following lemma:

Lemma 2. *When short-selling bans are in place, the probability that the price $\tilde{q}$ falls below the threshold c is equal to:*

$$P(\tilde{q} < c) = \int_0^{\frac{h_I}{h_N}K} ((1-p)\alpha g h_I + (1-\alpha) g h_N \eta) f(\eta) d\eta$$

where $K = \frac{c\alpha(1-p)}{(1-\alpha)(p-c)}$. In particular, if $\eta_s \sim U[0,1]$ then:

$$\begin{aligned} P(\tilde{q} < c) &= \frac{h_I^2}{h_N} P(q < c) && (9)\\ &= \left(h_I \mathbb{E}\left[P(sell \mid \eta_s, No\ bans) \mid \eta_s < K\right]\right)\left(\frac{h_I}{h_N} P(Bid < c)\right) && (10)\end{aligned}$$

We can decompose h_I^2/h_N in two multiplicative parts, h_I and h_I/h_N. The probability of a sharp decrease in price is affected by the (conditional) likelihood of a sell order, which decreases by a factor of h_I, and by the probability that the Bid price is low, which depends on the asymmetric information in the market, and changes by a factor of $\frac{h_I}{h_N}$. Hence, we can get an additional condition for a short-selling ban to be implemented:

Result 2. *If $\eta_s \sim U[0,1]$, the Regulator successfully manages to decrease the probability that price falls below the prespecified level c only if*

$$h_I^2 < h_N. \qquad (11)$$

As we can see, the more the Noise traders who own the asset and the fewer the Informed

[9] For the Ask price, we have $Ask = SSAsk = \frac{(\alpha+(1-\alpha)(1-\eta_s))p}{\alpha p+(1-\alpha)(1-\eta_s)}$ since the bans do not limit anyone from buying the stock.

[10] In particular, $c < \min\{\frac{(1-\alpha)h_N p}{\alpha h_I(1-p)+(1-\alpha)h_N}, \frac{(1-g)p}{1-g+g\alpha(1-p)(1-h_I)}\}$, where the first term is the maximum SSBid price and the last term is the minimum possible "no-trade price"; indeed $E[V|\text{No Trade, Bans}] = \frac{((1-g)+g(1-\alpha)\eta_s(1-h_N))p}{1-g+g(1-\alpha)\eta_s(1-h_N)+g\alpha(1-p)(1-h_I)}$

investors, the more successful is the ban. The regulator may achieve his goal by reducing the probability there will be a sell order, or by reducing the likelihood that the bid price will be smaller than c. In times where an irrational exuberance has led many Noise traders to own the asset, and has driven informed investors away, the intervention of the Regulator will be more warranted and more successful. If, however, the proportion of Informed traders who own the stock is large relative to the corresponding fraction of Noise traders, then imposing bans may not even support prices.[11]

The above results capture the effect of short-selling bans on the left tail of prices. We believe that this is of paramount importance to the Regulators' decision to impose any bans. However, it would be also interesting to note what is the effect of the regulation on various measures of the central tendency of prices. More specifically, assuming for simplicity that the probabilities of a buy and of a sell order are both less than 1/2 (e.g. if $g < 1/2$), we have:

Lemma 3. *When short-selling bans are implemented, the mean price remains unchanged, while the median price decreases.*

Proof. As explained also in DV, because of the law of iterated expectations and the fact that the Marker Maker is risk-neutral, we have:

$$E[\tilde{q}] = E[E[V|\tilde{\mathcal{F}}]] = E[V] = E[E[V|\mathcal{F}]] = E[q],$$

where $\mathcal{F}$, $\tilde{\mathcal{F}}$, denote the information sets of the Market Maker without and with short-selling bans, respectively. As for the median price, which we denote by $\mu_{1/2}$, note that in either case this median coincides with the expected payoff given a no-trade "action". Indeed, when there are no bans $E[V|\text{No trade}] = p$ (and $Bid < p < Ask$). Hence: $P(q \leq p) = 1 - P(Buy) \geq 1/2$ and $P(q \geq p) = 1 - P(Sell) \geq 1/2$. Therefore $\mu_{1/2}(q) = p$. Similarly, when short-selling bans are implemented, the median price is attained when there is no-trade (since the probabilities of a buy or of a sell order are still less than 1/2). But the no-trade price is always less than p, independently of the values of η_s: $E[V|\text{No Trade}, \eta_s] = \frac{((1-g)+g(1-\alpha)\eta_s(1-h_N))p}{1-g+g((1-\alpha)\eta_s(1-h_N)+\alpha(1-p)(1-h_I))} < p$. Therefore $\mu_{1/2}(\tilde{q}) < p = \mu_{1/2}(q)$. □

[11]Previous empirical literature has hinted towards a mixed result concerning whether bans succeed in supporting stock prices. We explore this issue further in the next Section.

To better understand the effect of bans, depending on the market parameters, we run a simulation to find the distribution of prices with or without bans. We choose the following baseline parameters: $a = 0.5, p = 0.5, \eta = 0.5, g = 0.9, h_N = 0.2$,[12] and we assume, as in DV, that in the case of a No-trade event, price is equal to $E[V|\text{No trade}]$. We then compare two cases. The first one, shown in Panel A of Figure A1a of the appendix, is with h_I relatively low (equal to $h_N = 0.2$), so that $h_I^2 < h_N$. According to Result 2, in this case the Regulator is successful in reducing the probability of an extreme left-tail event. Indeed the figure shows that when bans are imposed, the weight on very small realizations of the price decreases, and instead the likelihood of below average (but not extreme) prices increases, as the no-trade event is now "negative news". On the other hand, if h_I is high ($h_I = 0.5$), so that $h_I^2 > h_N$, as can be seen in Panel B of Figure A1b of the appendix, the bans do not help the regulator support the price of the asset; left tail events in fact are now more likely after bans. Why is this so? Even though, the likelihood of a sell order decreases, the bid price after the ban also declines. This is because when there are many informed traders who own the asset, the sell order becomes more informative about the payoff, and market makers adjust their valuation towards the low payoff (i.e., zero). Therefore, in that case, bans are ineffective.

2.4 Testable Hypotheses

Based on our baseline model, we now form a number of testable hypotheses. In this way, we will be able to study three questions: when are bans imposed, what is their effect on prices, and what happens to liquidity?

First of all, since the parameters h_I, h_N are important in our model, we should try to find their empirical proxy. In particular, let m be the fraction of the risky stock owned by informed traders (while $1 - m$ is the corresponding fraction owned by Noise traders), which can be later proxied by the institutional ownership of a stock. Then, using Bayes' rule, and assuming that the fraction of informed investors (i.e., α) in the whole economy is fixed, we obtain:

$$\frac{h_I}{h_N} \propto \frac{m}{1-m} \tag{12}$$

[12] The specific values of these parameters are not important for the qualitative implications of our simulation.

and,

$$\frac{h_I^2}{h_N} \propto \frac{m^2}{1-m} \tag{13}$$

According to our model, the Regulator only imposes bans when h_I^2/h_N is less than 1. Thus, in markets with a large number of sophisticated stock owners, the Regulator may stay away from imposing any short-selling restrictions.

Hypothesis 1. *Short-selling bans are more likely to be imposed by regulators in markets with low institutional ownership.*[13]

Assuming that short-selling activity is prohibited, the overall effect of the bans is to change the conditional distribution of the payoff from the perspective of the Market Maker, which in turn affects the bid-ask spreads and the distribution of prices. In particular, the bid price changes because the composition of the pool of potential sellers changes when short-selling restrictions are in place. This change is dictated by h_I/h_N, which captures the relative likelihood that an informed investor owns the stock. When this is higher than 1, it is relatively more likely that a sell order is initiated by an informed trader, and thus the Market Maker submits a lower bid. Since liquidity can be measured by the bid-ask spread and bans do not affect the ask price ($Ask = SSAsk$), the model is consistent with the following hypothesis:[14]

Hypothesis 2. *Under certain conditions ($h_I > h_N$), short-selling bans lead to a deterioration in liquidity.*

The model also predicts that as long as $h_I^2/h_N < 1$, the "low price" realization becomes more unlikely (since the probability of a sell order is smaller), and this effect dominates any change in the bid price. As a result, a ban leads to a thinner left tail, which can be also interpreted as an increasing q-*th* percentile of the distribution (where q is a small enough number, such as 1, 5 or 10). On the other hand, according to Lemma 3 the *expectation* of the price remains the same when bans are in place, while the *median* price decreases (consistent with the findings

[13] According to Boehmer and Kelley (2009), the higher the level of institutional ownership the more efficient is the price of a stock in the sense that it follows a random walk; thus, our hypothesis implies that short-selling bans are more likely to be implemented in more inefficient markets.

[14] This hypothesis is also discussed in Dixon (2021), while in DV the focus is on the dynamics of the bid-ask spread. We choose to also include this hypothesis here to facilitate the discussion of our empirical results in the next section.

of Beber and Pagano (2013)), because under the "ban-regime", any no-trade action is more likely to reflect negative news.

From the above discussion, we can derive the following hypothesis concerning the effect of bans on the distribution of prices.

Hypothesis 3. *Under certain conditions ($h_I^2 < h_N$), short-selling bans support the left tail of returns. Moreover, the median return decreases relative to the unconstrained case, while the mean remains the same.*

Apart from the above predictions about the first order effect of bans, our model can give us cross-sectional predictions about the effect of institutional ownership, which we use as a proxy for the fraction of informed traders owning the stock, on liquidity and on the change in the left tail of returns. Specifically, we get the following set of additional hypotheses:

Hypothesis 4. *When short-selling bans are implemented, the higher the institutional ownership of a stock, the larger the increase in bid-ask spreads.*

Hypothesis 5. *When short-selling bans are implemented, the higher the institutional ownership of a stock, the lower the support in the left tail of returns.*

In other words, stocks with a larger number of sophisticated owners would be more likely to have very low returns and wider bid-ask spreads after the introduction of a short-selling ban. This is because adverse selection worsens relative to the unconstrained case, as the fraction of informed sellers is relatively larger. In other words, when short selling is allowed, then a sell order may arise from either an informed or a noise trader and Market Makers adjust their expectations of the payoff, depends on the relative masses of I to N in the whole population. In contrast, when short-selling is not allowed, the pool of potential sellers changes, and includes only those who already own the stock. Therefore, the fraction of informed traders who own the stock becomes relevant and the higher this is, the more the market makers think that a sell order contains information, thus adjusting the bid downwards, and leading to a thickening of the left tail of returns.

Overall, it is important to notice that it can very well be the case that a Regulator manages to avert a huge drop in price (if $h_I^2/h_N < 1$) while causing deterioration of liquidity (if

$h_I/h_N > 1$). However, it is also possible that a non-optimal imposition of short-selling bans can have a negative effect on both the left tail of returns and on liquidity. We leave the study of this trade-off of Regulators for future work.

2.5 Discussion of the model

Our model may be highly stylized but it offers a number of predictions that we can easily test in the data, so from that perspective it's a useful model. However, due to its simplicity, it also has a number of caveats that are worth discussing further.

First of all, the model is static; hence, it is only able to capture the short-term effects of short-selling bans. As such, our model cannot capture different measures of price informativeness, such as the speed of price adjustment to fundamentals, and may miss some important implications, such as for the autocorrelation of returns. Moreover, while we measure liquidity conditions as the bid-ask spread at time 0, the ban may have a non-trivial effect on the dynamics of spreads over time. Similarly, it is important to note that we only consider a market with a single risky asset. Thus, our cross-sectional predictions do not take into account the interactions between the returns of different assets and the changes in investor portfolios. We leave such extensions for future work, where one can also study the effects of lifting the bans, distinguish between short-term and longer term effects, and make further inferences concerning the differential effect of bans on various stocks.

Finally, we assume exogenously that the objective of the Regulator is to avert a sharp decline in prices, that is to ensure that $P(q < c)$ is small enough. Although this is consistent with the goal of regulators to ensure financial stability and maintain market confidence, in practice, regulators may also consider the effects of short-selling bans on liquidity and price informativeness. Thus, in future versions of the model this trade-off could be incorporated in the decision-making process of regulators. An additional assumption we have implicitly made is that the model parameters remain unchanged by the introduction of the ban. This is a simplifying assumption, but it could have important implications if some of these parameters change endogenously. For example, the decision to impose a ban could change the incentive of investors to acquire information for a stock and could, thus, affect the parameters h_I, h_N

of the model (Dixon, 2021). Finally, in the model, there can be instances where no trade takes place; in these cases we assume that the price of the asset is equal to the updated expectation of the payoff from the perspective of the market makers. However, in the data, we only observe transaction prices; hence, in order to avoid a censored-sample bias problem[15] we exclude from our empirical analysis all micro and nano-cap stocks, which may be less actively traded.

[15]See Section 5.3 of DV for a more detailed analysis of this issue.

3 Data

Our dataset comprises daily observations collected from different sources between January 1, 2018, and June 12, 2020, for 17 European countries, i.e., Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Norway, Poland, Portugal, Spain, Sweden, Switzerland, and the United Kingdom. Data for bid and ask prices, total return indices, market capitalizations, and trading volumes for 3,655 stocks are gathered from Datastream. We also collect data of institutional ownership from Bloomberg, as of December 31, 2019.[16] Our initial sample contains 2,927,375 stock-day observations. After dropping micro and nano-cap stocks (i.e., stocks with a market capitalization below $300 million) and removing observations with negative bid-ask spreads, we end up with a sample of 1,153,018 stock-day observations corresponding to 1,925 stocks. We further winsorize the data by eliminating the observations corresponding to the top 1% of the bid-ask spread, as in Beber and Pagano (2013). Table 1 summarizes the key features of this dataset and shows that the countries with the largest number of stocks and observations are the United Kingdom, France, and Germany.

TABLE 1 ABOUT HERE

Finally, we collect the timeline of the national lockdown measures introduced to prevent a further spread of the Covid-19 by scraping Wikipedia's page on National responses to the COVID-19 pandemic. Both inception and lifting dates of the short-selling bans enacted in Austria, Belgium, France, Italy, and Spain, moreover, are obtained by searching for opinion documents on the ESMA's website coupled with the decisions issued by the national authorities. Figure A2 of the appendix displays the dates for the short-selling bans and lockdown measures in each country. All countries in our sample except for Sweden imposed a national lockdown while six countries (out of the 17 in our sample) imposed a ban on new short

[16] In exercises involving institutional ownership, we drop 281 securities with institutional ownership larger than 100%. These are potentially erroneous observations, stemming from reporting lags or double-counting due to short selling.

sales. Effective on March 18, 2020, Austria, Belgium, France[17], Greece, Italy, and Spain[18] exercised their right under Article 23 of the European Short Selling Regulation and decided to introduce a temporary ban on taking or increasing net short positions with respect to all shares admitted to their trading venues. Initially, the bans were introduced for a period of one month.[19] On April 15th, however, in a coordinated fashion, all six countries notified the European Securities and Markets Authority (ESMA) of their intention to extend the ban of short sales for one more month. ESMA issued positive opinions on the proposed measures, and the bans remained in place until May 18, 2020. The scope of the bans applied to any natural or legal person, regardless of where they are located, and covered all stocks traded in cash and derivatives markets, including American Depository Receipts. Bearish intraday operations were also in scope. The prohibitions did not apply to market-making activities or trading in index-related instruments.[20]

[17]France's financial markets regulator, the Autorité des Marchés Financiers (AMF), initially banned short selling in 92 specified equities for a one-day period, beginning on March 16, 2020 and ending on March 17, 2020. This applied to many of France's blue-chip stocks. On March 17, 2020, the AMF announced a ban on short selling of all shares admitted to French trading venues, starting on March 18, 2020, in line with the other five European countries that introduced similar restrictions of short selling.

[18]Spain introduced a ban on short selling of shares admitted to Spanish trading venues one day earlier, on March 17, 2020.

[19]Suggested duration of short-selling bans varied slightly by country, but eventually all countries decided to lift restrictions on short selling on May 18, 2020.

[20]Exceptions also included convertible bond arbitrage with a delta-neutral structure, and short positions hedged by a purchase that is equivalent in terms of subscription rights. Index-related instruments in which restricted shares represented more than a given country-specific threshold were also exempt. Initially, the thresholds were 20% for Belgium, Greece, and Italy, and 50% for France and Spain. From April 15 onward, a uniform threshold of 50% was adopted for all countries.

4 Main Results

Guided by the testable predictions of our model, this section describes our preliminary empirical evidence on the effects of short-selling bans on market liquidity and stock prices.

Figure 1 about here

In our model, regulators impose bans only if they think that restricting short-selling activity will avert a sharp drop in prices. According to Hypothesis 1, this is more likely to happen if the fraction of sophisticated traders owning the stock is low. In Figure 1, we take this prediction to the data by plotting the average institutional ownership by country. We observe that countries that imposed short-selling bans are on the lower end of institutional ownership compared to that countries that did not impose any restrictions.[21] We view Figure 1 as suggestive evidence in support of Hypothesis 1, maintaining that institutional ownership is an important factor in the decision-making process of regulators when considering to impose restrictions on short-selling activity.

4.1 Market Liquidity

We study the effect of short-selling restrictions on stock market liquidity using bid-ask spreads, following the seminal paper of Beber and Pagano (2013). While other measures of market liquidity could be used, Goyenko, Holden, and Trzcinka (2009) show that liquidity measures based on bid-ask prices are closely related to actual transaction costs. To assess the impact of the ban, we calculate the average bid-ask spread over a window that covers 30 calendar days before and 30 calendar days after the introduction of short-selling bans.

Table 2 about here

[21] There are three exceptions to this general observation: Switzerland, Germany, and Denmark. We have reviewed the quality and sources of the institutional ownership data in these three countries, and it seems comparable to that of the rest of the countries; therefore, the idiosyncrasies related to the data gathering in these countries do not appear to be obvious explanations for these exceptions.

Table 2 provides descriptive statistics of the bid-ask spreads observed across all 17 countries in our sample. The first column refers to the period prior to the introduction of short-selling bans (February 17, 2020 to March 17, 2020), the second column focuses on the ban period (March 18, 2020 to April 15, 2020). The ratio of the two in the last column reveals a substantial widening of bid-ask spreads during the ban period in all countries. A careful examination of column (3) of table 2 reveals that bid-ask spreads widened more and, on average, doubled in Austria, Belgium, France, Greece, Italy, Spain, i.e., the countries that imposed short-selling bans.

Although liquidity was lower during the ban than in the preceding period, we cannot conclude that the imposition of short-selling bans *caused* the rise in bid-ask spreads. There is evidence that liquidity started to decrease several weeks before the imposition of bans. To visually inspect the sensitivity of our results to the specific choice of the start and end date of the observation windows, we examine daily bid-ask spreads between January 2018 and June 2020.

Figure 2 about here

Figure 2 plots the median bid-ask spread for two groups of countries, namely the countries that imposed short-selling bans (in red) and the countries that did not (in green). The bid-ask spreads started to widen much earlier than the introduction of the bans. This implies that the bans neither caused, nor were associated with the collapse in liquidity. An examination of Figure 2 shows that although illiquidity peaked shortly after the ban, bid-ask spreads started decreasing shortly after and were at roughly the same average level at the end as at the beginning of the ban. Overall, Figure 2 suggests that bid-ask spreads began to widen in February when infections started rising in Europe, and short-selling bans were only imposed on March 18, 2020. On the contrary, in the sample studied by Beber and Pagano (2013) short-selling bans were imposed almost immediately after the collapse of Lehman Brothers on September 15, 2008. We view the lag in regulators' actions in 2020 as potentially beneficial for our analysis since we are interested in measuring the differential effect of short-selling bans on liquidity and prices. The lag between the onset of the crisis and the enactment of short-selling bans implies (in our view) a smaller degree of contamination of the ban period with endogenous outcomes.

Table 3 about here

Table 3 presents the results of a difference-in-differences regression estimating the differential effect of short-selling bans on liquidity. Specification (1) shows that average bid-ask spreads increased 24 basis points during the short-selling ban period across all stocks in the sample. Moreover, stocks in countries that imposed short-selling bans tend to have larger bid-ask spreads ($\approx$ 11 basis points) compared to stocks in other countries. However, the difference-in-differences specification allows us to estimate the differential effect of short-selling bans on liquidity while controlling for those unconditional differences in the levels of bid-ask spreads as well as the general increase of bid-ask spreads during the short-selling ban period. We estimate that average bid-ask spreads in countries that imposed short-selling bans widened by an additional 12 basis points compared to countries with no short-selling restrictions, and the result is statistically significant at the 1% level. Specification (2) introduces stock level fixed effects, while specification (3) adds day fixed effects. Stock-level fixed effects control for time-invariant unobserved heterogeneity such as the number of market makers, analyst coverage, capitalization, size of public float, and country characteristics such as insider trading regulation and enforcement. Time fixed effects account for the commonality in liquidity or returns, which is especially important at a time of a global shock such as the Covid-19 pandemic. Specifications (2) and (3) confirm the result of the baseline specification (1) in terms of statistical significance and point estimate. Overall our results are consistent with those of Beber and Pagano (2013) based on the global financial crisis.[22] The evidence suggests that short-selling bans during the Covid-19 pandemic were associated with a deterioration of liquidity by approximately 12-13 basis points, as measured by bid-ask spreads. This is consistent with Hypothesis 2, assuming that the fraction of sophisticated traders who own

[22] Beber and Pagano (2013) estimate the impact of short-selling bans on bid-ask spreads to be around 198 basis points (for covered bans) but in jurisdictions with a short sale disclosure regime the authors estimate the effect to be 65 basis points lower. Still there is a large discrepancy between our quantitative estimates (12-13 basis points) and the net effect estimated by Beber and Pagano (2013) (133 basis points). We believe these differences to be sample-specific. We also note that we exclude from our analysis micro and nano-cap stocks that could potentially exacerbate the effect. As a result, bid-ask spreads in the pre-ban period are significantly smaller in our sample and across all countries, compared to the figures reported by Beber and Pagano (2013). Furthermore, in 2008 several countries imposed bans on naked short selling, which is no longer allowed in European markets since 2012. Thus, it would be reasonable to expect the effect of short-selling bans to be smaller in 2020 compared to 2008, given the disclosure regime that is in place and the permanent ban on naked short selling.

any specific stock is larger than that of the liquidity traders.

Figure 2 about here

One of the caveats that we need to bear in mind when interpreting the difference-in-differences results is that they depend on a range of statistical assumptions, including the parallel trends assumption. The parallel trend assumption is critical and requires that in the absence of treatment, the difference between the treatment and control group is constant over time. There is no statistical test for this assumption, but a visual inspection can be used. Figure 2 shows that while bid-ask spreads for the two groups of countries co-move before the enactment of short-selling bans, they increased more sharply after the enactment of short-selling bans in the countries that imposed such restrictions.

Table 4 about here

Next, we test the prediction of Hypothesis 4 according to which the negative effect of short-selling bans on liquidity is larger on stocks with higher institutional ownership. That is, when short-selling is prohibited, stocks with higher institutional ownership will experience greater deterioration in liquidity, manifested in larger bid-ask spreads. This is because the adverse selection facing Market Makers will be greater when more informed investors own a stock and can thus submit a sell order despite the short-selling bans. To test this hypothesis, we split our sample in stocks with low/high institutional ownership, and we estimate the difference-in-differences regressions of Table 3 in these two subsamples. For the set of stocks with low institutional ownership we choose the bottom tercile (i.e., institutional ownership $\leq 40\%$), and for the set of stocks with high institutional ownership we choose the top tercile (i.e., institutional ownership $\geq 70\%$). The results, presented in Table 4, are indeed in line with the prediction of our model. Bid-ask spreads of stocks with low institutional ownership increase by an additional 8 basis points on average as a result of the short-selling bans, whereas bid-ask spreads of stocks with high institutional ownership increase by 20-25 basis points. Moreover, the impact of short-selling bans on the liquidity of stocks with low institutional ownership is not statistically significant at any of the conventional levels, whereas the estimated impact of 20-25 basis points on stocks with high institutional ownership is statistically significant at the 1% level across all specifications. We conclude that short-selling bans have a negative impact on liquidity, especially for stocks with high institutional ownership.

A reasonable question that may arise in the context of our results is whether our conclusions would be different if we examined other measures of liquidity. We use the bid-ask spreads as we believe they appropriately capture the adverse selection component of liquidity, which is analyzed in the theory section of our paper. Other measures of liquidity as well as measures of financial market stability could also be important to study, but are beyond the scope of our paper. Anecdotal evidence from conversations with market practitioners at institutional asset managers and brokers in London confirm that volumes, volatility and bid-ask spreads behaved very similarly before, during, and after the short-selling ban of 2020. Volumes showed a trough, volatility peaked before the imposition of the bans and the normalization in both measures followed a pattern very similar to that documented in our study of bid-ask spreads.

4.2 Stock Prices

In this section, we examine whether short-selling bans were effective in supporting prices.

Figure 3 about here

Figure 3 shows the cumulative average return for stocks in countries that imposed bans (in red) and countries that did not (in green). First, the majority of the decline occurred before the imposition of the ban. Second, the decline continued for several days after the imposition of the ban. Third, stock prices did recover during the ban, but even more so after the ban. Fourth, although stock prices increased during the ban, they did not increase more in countries that imposed the ban. To enhance the robustness of our findings, we also present results in a matched sample of stocks. Concretely, we match each stock subject to short-selling bans to a stock that's closest in terms of market capitalization that belongs to the same industry (according to the ICB classification code) and was not subject to any short-selling restrictions during the sample period. Figure 4 plots the mean and median cumulative returns for the two groups of stocks in the matched sample. Overall, this preliminary evidence suggests that short-selling bans do not support the average level of prices. This is consistent with previous empirical findings (e.g., Beber and Pagano, 2013), as well as with Hypothesis 3, maintaining that the median stock return is lower under short-selling bans while the mean stays the same.

Figure 4 about here

It is important to note that this evidence is not conclusive on whether the ban had a beneficial effect in terms of supporting financial stability. The reason for that is the lack of counterfactual evidence and the endogeneity of the decision to impose the ban, as we noted above. The counterfactual evidence refers to the hypothetical performance of stock markets in countries that imposed the ban if these countries had not imposed the ban. The performance of stock markets subject to bans may have been worse if the bans had not been imposed. This endogeneity concern can only be conclusively resolved in the hypothetical case of a natural experiment that randomly assigns bans across countries and then studies their impact. Though such an experiment is in theory possible, it is unlikely to be attempted by policymakers.

Furthermore, our theoretical model suggests that even though short-selling bans may not be effective in supporting the average level of prices, they may as well be effective in shifting the distribution of prices in a way that the left tail gets supported and sharp decreases in price are avoided. Of course, this is particularly important during a financial crisis when a precipitous fall in prices may raise concerns about financial stability. For example, Brunnermeier and Oehmke (2014) show that when a financial institution is sufficiently close to its leverage constraints, a sharp fall in its stock price may trigger a run on the bank. Naturally, regulators may be inclined to impose temporary short-selling bans to prevent that from happening and avert a more generalized market panic.

Having established both theoretically and empirically that short-selling bans do not support the mean level of prices (and may even have a negative effect on the median level of prices), we wish to test whether short-selling bans had any discernible effect on the left tail of the distribution of prices, as suggested by Hypothesis 3. As preliminary evidence, we measure skewness in each market to see the relation between short-selling bans and skewness of returns.[23] In Figure A3 of the appendix we plot the average historical skewness in countries that have and countries that have not implemented a ban. Indeed, we observe that the skewness of returns fell sharply across all stocks before the short-selling bans were introduced, but it recovered faster in the countries that implemented short-selling bans. Notwithstanding our earlier caveat about endogeneity, the true distribution of stock returns at any given point in time is, of course, unobservable. Therefore, in order to pin down some basic statistics pertaining to the distribution of stock returns, we resort to temporal characteristics measured over small windows around the imposition of short-selling bans. For each stock in the sample, we measure the mean, median, volatility, 10th percentile, 5th percentile, and 1st percentile based on the time series of stock returns in two 30-day windows: the *pre-ban period* (February 16, 2020 – March 16, 2020) and the *ban period* (March 17, 2020 – April 15, 2020). Thus, for each one of those measures we have a balanced panel containing two observations per stock, i.e., one in the pre-ban period and one in the ban period.

Table 5 about here

With a cross-section of 1,922 stocks, we have enough observations to estimate a classic difference-in-differences regression to assess the effect of short-selling bans on the distribution of stock returns. Panel A of Table 5 presents the results. To account for unobserved heterogeneity across stocks we use stock fixed effects, and the variable of interest is the dummy *Ban* which is equal to one for the observations of stocks subject to short-selling bans during the ban period. The coefficient corresponding to *Ban* represents the differential effect measured in stocks that were subject to short-selling bans over stocks that were not. We observe that the mean and the median of stocks subject to bans were 7 basis points lower, whereas

[23] Although evidence about the effects of bans on skewness in previous literature has not been conclusive, it is worth noting that, Bris, Goetzmann, and Zhu (2007) do indeed find that short selling restrictions are associated with an increase in the skewness of returns.

the 10th, 5th, and 1st percentiles were 0.7%, 1.3%, and 2.1% higher, respectively. This evidence suggest that short-selling bans support the left tail of the distribution of prices, at the expense of marginally lower mean/median - and poorer liquidity. Moreover, our results are statistically significant at the 1% level and economically large (e.g., the 1st percentile of stock returns is approximately 2% higher during the ban period in stocks subject to short-selling bans compared to stocks with unconstrained short-selling). In Panel B of Table 5, we estimate the regression in a matched sample of stocks,[24] and obtain results of the same magnitude and statistical significance. As a further robustness check, we repeat the exercise in Table 6 using a window of 60 days (instead of 30), and confirm that our earlier findings remain virtually unchanged.

Table 6 about here

Finally, we test the cross-sectional predictions of our model in Table 7. According to Hypothesis 5, the effectiveness of short-selling bans in limiting extreme negative outcomes is inversely proportional to the institutional ownership of the affected stock. Institutional ownership is used as a proxy for the fraction of informed traders who own the stock. If short-selling is not allowed, then the fraction of informed traders owning the stock affects the distribution of prices in two ways. One one hand, the lower this is, the less sale orders will be submitted (these will be hidden under a veil of "no order" events), as potential investors with negative information will be prohibited from submitting a short-sale order. On the other hand, this fraction determines the adverse selection in the market. When this fraction is low relative to the corresponding fraction of noise traders owning the stock, market makers are more likely to perceive a sell order as if it were initiated by a noise trader; thus, they would be less aggressive in revising their expectation of fundamentals and would submit a relatively high bid when they are faced with a sell order. To test this prediction, we run a series of cross-sectional regressions in the subset of stocks that were subject to short-selling bans. The dependent variables are the same as in the previous exercise (i.e., mean, median, volatility,

[24]Each stock subject to short-selling bans is matched to a stock that's closest in terms of market capitalization, belongs in the same industry (according to the ICB classification code), and is not subject to any short-selling constraints during the sample period.

10th percentile, 5th percentile, 1st percentile), estimated at the stock level based on the time series of returns during the ban period. We regress each of those measures, capturing the distribution of stock returns, against institutional ownership and a number of controls (i.e., market capitalization, dollar volume, Amihud illiquidity) while controlling for country and industry fixed effects. As predicted by the model, the results presented in Table 7 confirm that short-selling bans are more effective in stocks with lower institutional ownership. Indicatively, we estimate that the 5th percentile of stock returns would be approximately 1% higher if institutional ownership were 10% lower.

As a robustness check, we also run a placebo test in Table 8. We estimate the cross-sectional regressions of Table 7 in the full sample of stocks, but the dependent variables are now computed during the *pre-ban period* (January 16, 2020 – March 16, 2020). In this sample, short-selling restrictions are not in place, so we can check whether the effect of institutional ownership on the left tail is significant even in the absence of bans. Indeed, we can see that there is no statistically significant relationship between institutional ownership and the distribution of stock returns in the pre-ban period, thus supporting our Hypothesis 5 that the relationship we documented earlier can be attributed to the imposition of short-selling bans.

TABLE 7 AND TABLE 8 ABOUT HERE

Overall, our empirical findings support the view that short-selling bans can, under certain conditions, reduce the likelihood of a sharp price decline, but this comes at a cost of a deterioration in the median level of prices and in the liquidity of the market. It has been often claimed that the regulators' reasoning when imposing short-selling bans is to restore financial stability and market confidence.[25] Based on our results and on the trade-off we document here, it can be concluded that the imposition of bans in times of crisis should

[25]For example, Robert Ophele, the Charman of the French Financial Market Authority, stated in an interview in Bloomberg (18 May 2020): *"The European regulation is very clear: this restriction [the short-selling ban] is possible in case of adverse developments which constitute a serious threat to financial stability or market confidence. This restriction should be temporary, and taken in order to prevent the disorderly decline in the price of financial instruments..."*

depend on the degree in which supporting the left tail of returns (e.g. to avoid potential self-reinforcing downward price spirals) matters for the aforementioned goals.

5 Conclusion

Since the seminal work of Beber and Pagano (2013), it is generally accepted that short-selling bans have a detrimental effect on market liquidity and fail to support prices. Yet regulators in six European countries (i.e., Austria, Belgium, France, Greece, Italy, and Spain) decided to impose a two-month ban on new short sales (in March 2020) in response to the financial crisis caused by the Covid-19 outbreak. In this paper, we build a theoretical model endogenizing the regulator's decision to impose a ban on short sales, and derive testable predictions for liquidity and prices, which we then verify empirically.

Our model extends Diamond and Verrecchia (1987) by introducing a Regulator whose goal is to avert a sharp decline in prices, and we show that the effectiveness of short-selling bans depends on the relative ratio of informed to noise traders who own the stock. We identify institutional ownership as a useful proxy for this model parameter, and we exploit cross-sectional variation in the European 2020 short-selling bans to test the model's predictions. Consistent with the model, we find that tail risk was reduced in countries that implemented short-selling bans, and that this effect was more pronounced in stocks with low institutional ownership. However, we corroborate the evidence of the prior literature that bans were detrimental for liquidity and failed to support the average level of prices.

Our findings are relevant for regulators considering the costs and benefits of imposing short-selling bans.

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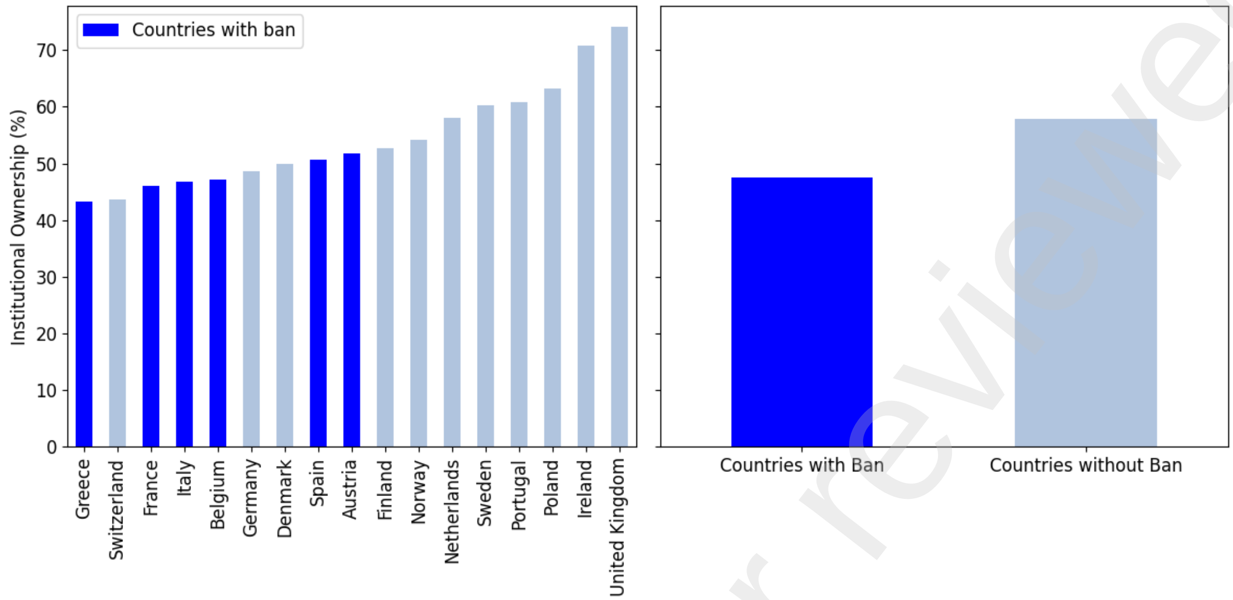


Figure 1. Institutional Ownership by Country

This figure shows the average institutional ownership in countries that banned short sales (Austria, Belgium, France, Greece, Italy, and Spain) and countries in which short-selling is allowed (Denmark, Finland, Germany, Ireland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and the United Kingdom). The chart on the left computes a simple average of institutional ownership across all stocks in each country while the chart on the right computes a market cap weighted average.

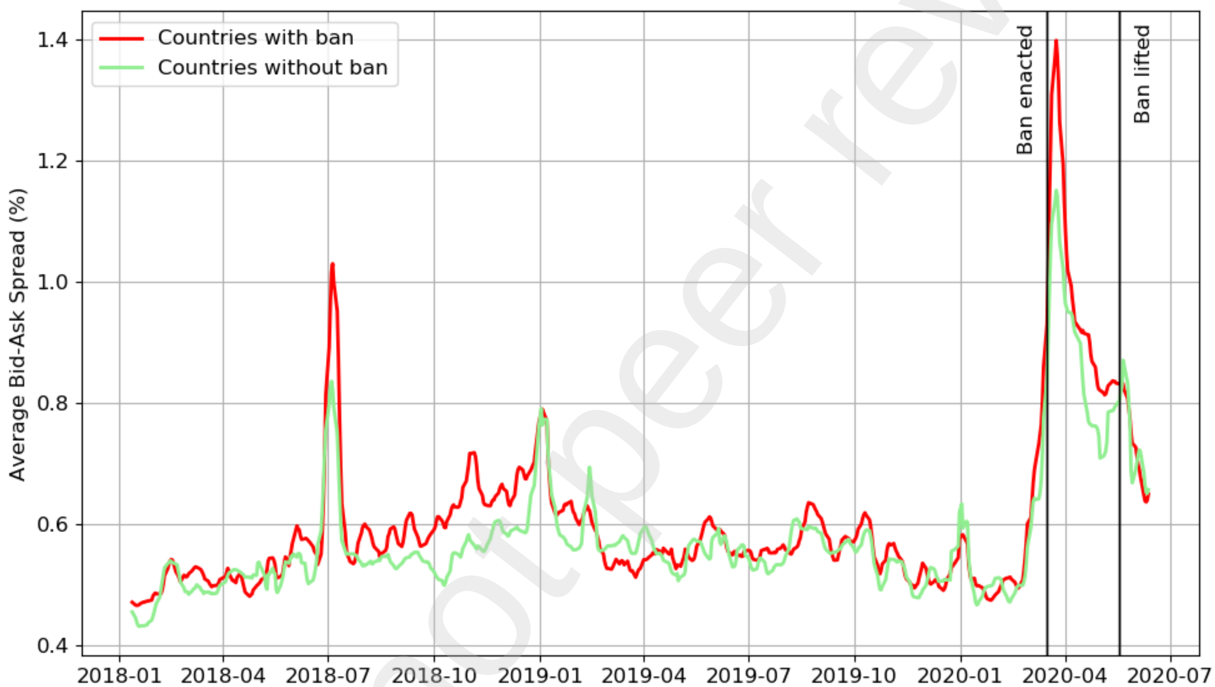


Figure 2. Average Bid-Ask Spread and Short-Selling Bans

This figure shows the average bid-ask spread in countries that banned short sales (Austria, Belgium, France, Greece, Italy, and Spain) and countries in which short-selling is allowed (Denmark, Finland, Germany, Ireland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and the United Kingdom).

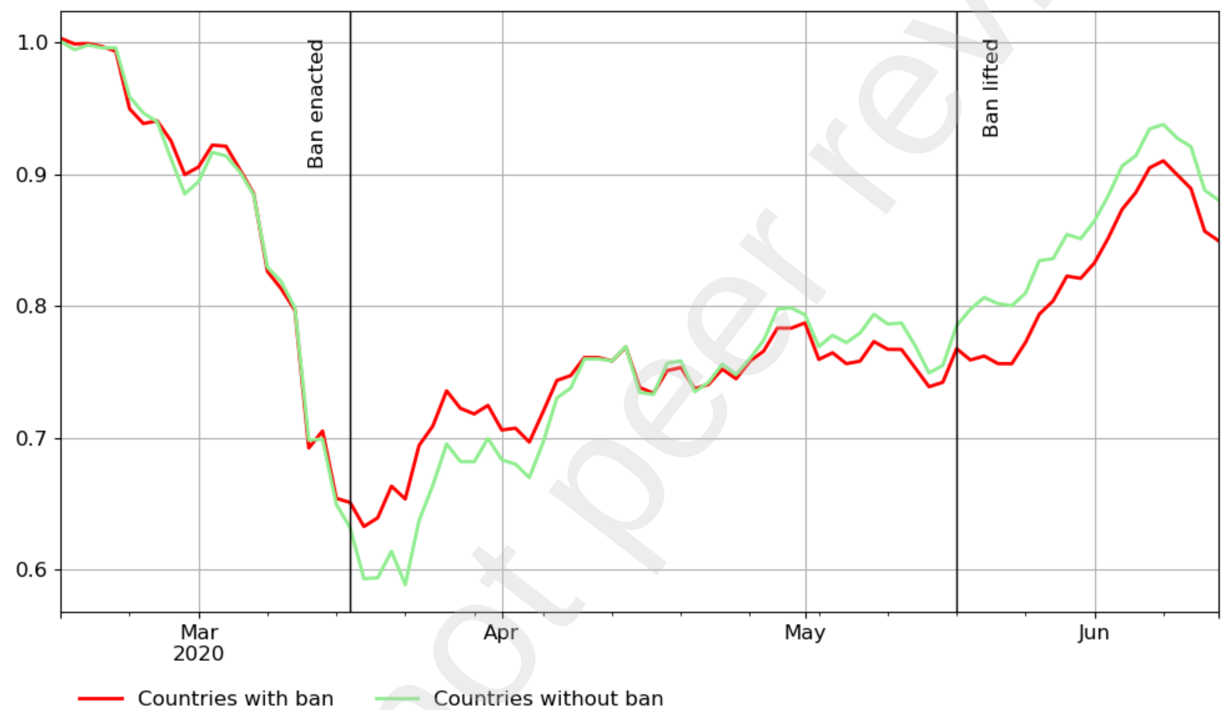


Figure 3. Cumulative returns and Short-Selling Bans (Full Sample)

This figure shows the cumulative average return in countries that banned short sales (Austria, Belgium, France, Greece, Italy, and Spain) and countries in which short-selling is allowed (Denmark, Finland, Germany, Ireland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and the United Kingdom).

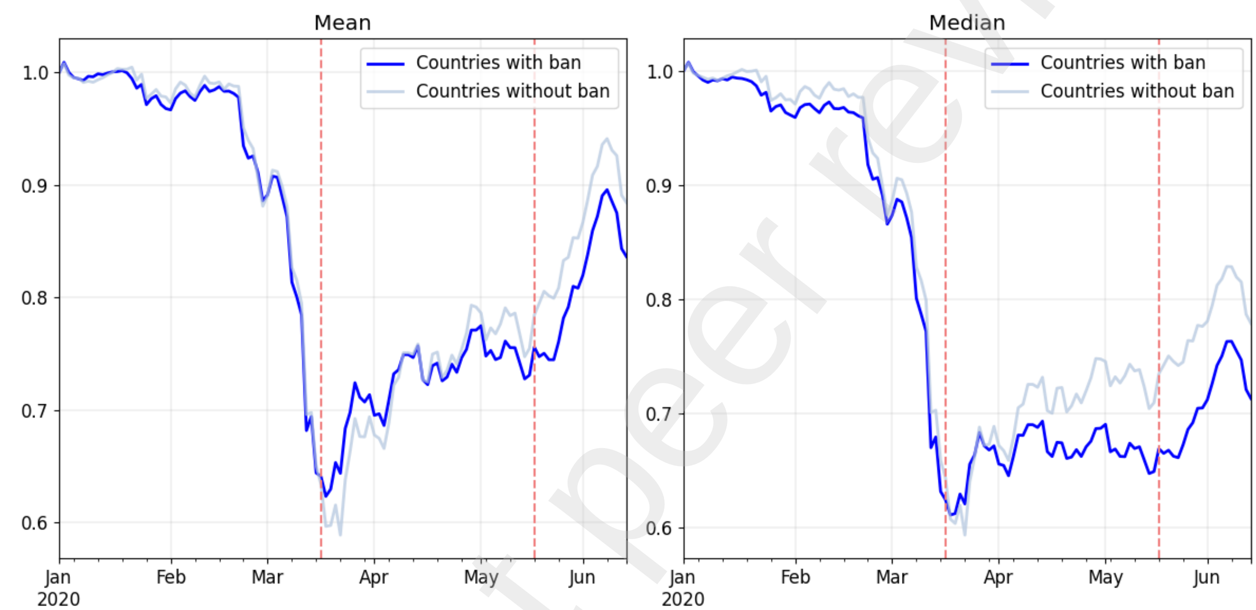


Figure 4. Cumulative returns and Short-Selling Bans (Matched Sample)

This figure shows cumulative average and median stock returns in a matched sample of securities from countries that banned short sales (Austria, Belgium, France, Greece, Italy, and Spain) and countries that did not (Denmark, Finland, Germany, Ireland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and the United Kingdom). The red vertical lines represent the beginning and the end of the short-selling ban period.

Table 1. Trading Data: A Summary

This table describes the composition of the trading data collected from Datastream. The sample ranges at the daily frequency between January 1, 2018, and June 12, 2020.

Country	Day/Stock Observations	N Days	N Stocks	Small Cap	Mid Cap	Large Cap
Austria	19,568	611	33	14	17	2
Belgium	42,317	623	69	44	18	7
Denmark	31,022	605	52	24	17	11
Finland	32,304	612	55	33	15	7
France	137,611	623	225	114	60	51
Germany	127,023	615	210	108	61	41
Greece	14,934	604	28	21	7	
Ireland	11,981	622	20	13	4	3
Italy	70,470	618	118	65	39	14
Netherlands	37,349	623	62	26	17	19
Norway	49,478	609	86	62	18	6
Poland	33,742	607	59	41	16	2
Portugal	9,342	623	15	8	4	3
Spain	50,810	624	83	39	28	16
Sweden	107,407	613	181	122	46	13
Switzerland	107,056	609	181	105	48	28
United Kingdom	270,604	619	448	268	134	46
Total	1,153,018		1,925	1,107	549	269

Table 2. Descriptive Statistics of Bid-Ask Spreads

This table provides median values for the bid-ask spread by country before and during the ban of short sales enacted in France, Italy, Spain, Belgium, Austria, and Greece. A window of 30 calendar days around the ban enactment date (March 17, 2020) is used to compute medians. The superscripts *, **, and *** in column (2) indicate that the median bid-ask spread during the ban is significantly different from the median before the ban at the 10%, 5%, and 1% level, respectively, based on a Wilcoxon test for differences between medians.

Country	Before (1)	During (2)	Ratio (3)
Austria	0.3859	0.6890***	1.7858
Belgium	0.3578	0.7083***	1.9796
Denmark	0.1940	0.3095***	1.5952
Finland	0.1912	0.3267***	1.7085
France	0.2580	0.5525***	2.1416
Germany	0.2740	0.4556***	1.6629
Greece	0.6515	0.8523***	1.3082
Ireland	0.8203	1.1351***	1.3837
Italy	0.1823	0.3947***	2.1648
Netherlands	0.0973	0.1654***	1.6998
Norway	0.3028	0.4811***	1.5886
Poland	0.4803	0.5025	1.0462
Portugal	0.1705	0.2187***	1.2829
Spain	0.1238	0.2491***	2.0124
Sweden	0.2235	0.3798***	1.6996
Switzerland	0.1695	0.2371***	1.3989
United Kingdom	0.1439	0.2116***	1.4709

Table 3. Short-Selling Bans and Bid-Ask Spreads

The dependent variable is the percentage bid-ask spreads quoted at the market close. The sample period is February 17 to May 17, 2020 - corresponding to a window of approximately 30 calendar days around the short-selling ban inception date (March 17, 2020). Column (1) corresponds to a classic diff-in-diff regression: *Ban country* is a dummy variable that is equal to one for countries that banned short sales (Austria, Belgium, France, Italy, and Spain) and zero otherwise, *Ban enactment* is a dummy variable that equals one for the period March 17–April 15 and zero otherwise, and *Ban* is the interaction term between *Ban country* and *Ban enactment*. The coefficient of interest is the one corresponding to *Ban*, measuring the differential effect of short-sale bans on bid-ask spreads between countries with bans and countries without bans. Column (2) introduces stock fixed effects, thereby eliminating the need for the dummy variable *Ban country* (which would be colinear), and column (3) adds day fixed effects which eliminates the need for the calendar dummy *Ban enactment*. The regressions are estimated by OLS on daily data with robust standard errors clustered at the stock level. The numbers reported in parentheses are t-statistics.

	(1)	(2)	(3)
Ban country	0.1059**		
	(2.1872)		
Ban enactment	0.2385***	0.2383***	
	(14.248)	(14.236)	
Ban	0.1172***	0.1307***	0.1308***
	(4.0816)	(4.5900)	(4.5315)
Constant	0.6192***	0.6478***	0.7666***
	(24.278)	(95.209)	(185.41)
N	79,624	79,624	79,624
Stock Fixed Effects	No	Yes	Yes
Day Fixed Effects	No	No	Yes

Table 4. Institutional Ownership and the Effect of Short-Selling Bans on Liquidity

This table presents difference-in-differences regressions estimating the effect of short-selling bans on liquidity in two subsamples: the bottom tercile of stocks with low institutional ownership ($\leq 40\%$) and the top tercile of stocks with high institutional ownership ($\geq 70\%$). The dependent variable is the percentage bid-ask spreads quoted at the market close. The sample period is February 17 to May 17, 2020 - corresponding to a window of approximately 30 calendar days around the short-selling ban inception date (March 17, 2020). Column (1) corresponds to a classic diff-in-diff regression: *Ban country* is a dummy variable that is equal to one for countries that banned short sales (Austria, Belgium, France, Italy, and Spain) and zero otherwise, *Ban enactment* is a dummy variable that equals one for the period March 17–April 15 and zero otherwise, and *Ban* is the interaction term between *Ban country* and *Ban enactment*. The coefficient of interest is the one corresponding to *Ban*, measuring the differential effect of short-sale bans on bid-ask spreads between countries with bans and countries without bans. Column (2) introduces stock fixed effects, thereby eliminating the need for the dummy variable *Ban country* (which would be collinear), and column (3) adds day fixed effects which eliminates the need for the calendar dummy *Ban enactment*. The regressions are estimated by OLS on daily data with robust standard errors clustered at the stock level. The numbers reported in parentheses are t-statistics.

	Low Institutional Ownership			High Institutional Ownership		
	(1)	(2)	(3)	(4)	(5)	(6)
Ban country	0.0570			0.1819		
	(0.64)			(1.45)		
Ban enactment	0.2947***	0.2967***		0.2119***	0.2147***	
	(7.86)	(7.75)		(7.40)	(7.49)	
Ban	0.0831	0.0831	0.0845	0.2098***	0.2699***	0.2699***
	(1.56)	(1.55)	(1.58)	(2.94)	(3.85)	(3.84)
Constant	0.8038***	0.8272***	0.9747***	0.6267***	0.6642***	0.7711***
	(12.95)	(61.17)	(85.41)	(11.19)	(49.70)	(86.61)
N	20,987	20,987	20,987	19,723	19,723	19,723
Stock Fixed Effects	No	Yes	Yes	No	Yes	Yes
Day Fixed Effects	No	No	Yes	No	No	Yes

Table 5. Short-Selling Bans and the Distribution of Stock Returns (30d window)

This table presents the results of several difference-in-differences regressions relating the distribution of stock returns to short-selling bans. The dependent variables represent summary statistics of stock returns computed at the stock level in two (approximately) 30-day windows around the enactment of short-selling bans, i.e., the *pre-ban* period (February 16, 2020 – March 16, 2020) and the *ban period* (March 17, 2020 – April 15, 2020). More specifically, we use the following quantities as dependent variables: *mean*, *median*, *volatility*, *10th percentile*, *5th percentile*, *1st percentile* of stock returns. For the computation of percentiles we use linear interpolation when the desired quantile lies between two data points. *Ban enactment* is a dummy variable that equals one for the observations measured during the ban period (March 17, 2020 – April 15, 2020) and zero otherwise, and *Ban* is a dummy variable that equals one for observations during the ban period in countries that had active short-selling bans. The coefficient of interest is the one corresponding to *Ban*, measuring the differential effect of short-sale bans between countries with bans and countries without bans. Panel A reports results on the full sample, whereas in Panel B we match stocks based on market capitalization and ICB industry classification. All specifications include stock fixed effects, and standard errors are clustered at the stock level. The numbers reported in parentheses are t-statistics.

Panel A: Full Sample						
	(1) Mean	(2) Median	(3) Vol	(4) 10th pctl	(5) 5th pctl	(6) 1st pctl
Ban enactment	0.0263***	0.0160***	0.2377***	0.0072***	0.0154***	0.0303***
	(61.69)	(46.5)	(22.26)	(9.19)	(13.81)	(20.02)
Ban	-0.0007	-0.0022***	-0.1982***	0.0152***	0.0191***	0.0246***
	(-1.03)	(-3.75)	(-11.95)	(10.91)	(10.6)	(9.5)
Constant	-0.0197***	-0.0125***	0.7014***	-0.0647***	-0.0902***	-0.1313***
	(-114.92)	(-88.41)	(166.49)	(-198.64)	(-202.29)	(-212.58)
N	3,844	3,844	3,844	3,844	3,844	3,844
R^2	0.56	0.43	0.51	0.52	0.54	0.52
Panel B: Matched Sample						
	Mean	Median	Vol	10th pctl	5th pctl	1st pctl
Ban enactment	0.0252***	0.0153***	0.2342***	0.0055***	0.0170***	0.0293***
	(31.17)	(22.66)	(11.63)	(3.24)	(7.08)	(8.92)
Ban	0.0003	-0.0015*	-0.1949***	0.0170***	0.0176***	0.0257***
	(0.33)	(-1.81)	(-8.16)	(8.26)	(6.32)	(6.58)
Constant	-0.0194***	-0.0122***	0.6978***	-0.0643***	-0.0906***	-0.1303***
	(-78.76)	(-58.27)	(116.92)	(-125.11)	(-130.20)	(-133.29)
N	2,212	2,212	2,212	2,212	2,212	2,212
R^2	0.62	0.48	0.53	0.51	0.57	0.53

Table 6. Short-Selling Bans and the Distribution of Stock Returns (60d window)

This table presents the results of several difference-in-differences regressions relating the distribution of stock returns to short-selling bans. The dependent variables represent summary statistics of stock returns computed at the stock level in two (approximately) 60-day windows around the enactment of short-selling bans, i.e., the *pre-ban* period (January 16, 2020 – May 16, 2020) and the *ban period* (March 17, 2020 – May 17, 2020). More specifically, we use the following quantities as dependent variables: *mean*, *median*, *volatility*, *10th percentile*, *5th percentile*, *1st percentile* of stock returns. For the computation of percentiles we use linear interpolation when the desired quantile lies between two data points. *Ban enactment* is a dummy variable that equals one for the observations measured during the ban period (March 17, 2020 – May 17, 2020) and zero otherwise, and *Ban* is a dummy variable that equals one for observations during the ban period in countries that had active short-selling bans. The coefficient of interest is the one corresponding to *Ban*, measuring the differential effect of short-sale bans between countries with bans and countries without bans. Panel A reports results on the full sample, whereas in Panel B we match stocks based on market capitalization and ICB industry classification. All specifications include stock fixed effects, and standard errors are clustered at the stock level. The numbers reported in parentheses are t-statistics.

Panel A: Full Sample						
	(1) Mean	(2) Median	(3) Vol	(4) 10th pctl	(5) 5th pctl	(6) 1st pctl
Ban enactment	0.0145***	0.0069***	0.1948***	-0.0031***	0.0022***	0.0244***
	(60.91)	(38.27)	(26.12)	(-6.34)	(3.05)	(18.85)
Ban	-0.0007*	-0.0007**	-0.1305***	0.0072***	0.0133***	0.0211***
	(-1.86)	(-2.28)	(-11.29)	(7.98)	(10.66)	(9.37)
Constant	-0.0098***	-0.0046***	0.5585***	-0.0429***	-0.0650***	-0.1210***
	(-102.68)	(-61.66)	(189.96)	(-207.41)	(-218.24)	(-227.32)
N	3,844	3,844	3,844	3,844	3,844	3,844
R^2	0.56	0.38	0.58	0.55	0.56	0.56
Panel B: Matched Sample						
	Mean	Median	Vol	10th pctl	5th pctl	1st pctl
Ban enactment	0.0143***	0.0069***	0.1978***	-0.0040***	0.0015	0.0242***
	(31.64)	(19.18)	(14.04)	(-4.06)	(0.99)	(8.41)
Ban	-0.0005	-0.0007	-0.1339***	0.0082***	0.0140***	0.0215***
	(-0.91)	(-1.60)	(-8.03)	(6.54)	(7.54)	(6.26)
Constant	-0.0098***	-0.0045***	0.5494***	-0.0419***	-0.0644***	-0.1200***
	(-71.10)	(-40.52)	(131.82)	(-134.48)	(-138.35)	(-140.03)
N	2,212	2,212	2,212	2,212	2,212	2,212
R^2	0.63	0.44	0.60	0.53	0.55	0.54

Table 7. Cross-sectional Regressions: Price Support and Institutional Ownership

This table presents cross-sectional regressions of price support measures on institutional ownership, while controlling for a number of stock characteristics. The regressions are estimated in the cross-section of stocks with active short-selling bans, and the dependent variables are computed at the stock level based on the distribution of returns during the ban period (March 17, 2020 – May 17, 2020). More specifically, we use the following quantities as dependent variables: *mean*, *median*, *volatility*, *10th percentile*, *5th percentile*, *1st percentile* of stock returns. For the computation of percentiles we use linear interpolation when the desired quantile lies between two data points. All dependent variables are expressed in percentage terms. *Institutional Ownership* represents the fraction of shares outstanding (in %) owned by institutions (e.g., pension funds, mutual funds, insurance companies) and is sourced from Bloomberg as of December 31, 2019. All other controls are as of March 17, 2020, the beginning of the short-selling ban period. *Market Capitalization* is the market value of a firm in billions USD; *Dollar Volume* is the average trading volume in millions USD; and *Amihud ILLIQ* is the Amihud (2002) illiquidity measure constructed as the absolute return divided by the dollar volume (scaled by 10^6). The coefficient of interest is the one corresponding to *Institutional Ownership*, measuring the differential effect of institutional ownership (a proxy for informed trading) on price support measures. All specifications include country and industry fixed effects, and standard errors are clustered at the stock level. The numbers reported in parentheses are t-statistics.

	(1) Mean	(2) Median	(3) Vol	(4) 10th pctl	(5) 5th pctl	(6) 1st pctl
Institutional Ownership	0.0005	0.0005	0.0956**	-0.0062**	-0.0103***	-0.0123*
	(0.77)	(0.74)	(2.34)	(-2.40)	(-2.99)	(-1.95)
Market Cap	0.0013	0.0008	-0.2902***	0.0195***	0.0243***	0.0348**
	(1.11)	(0.47)	(-2.99)	(2.90)	(2.64)	(2.57)
Dollar Volume	-0.0003	0.0000	0.0586***	-0.0050***	-0.0059***	-0.0069**
	(-1.40)	(0.14)	(2.90)	(-3.29)	(-3.08)	(-2.41)
Amihud ILLIQ	-0.0045***	-0.0024***	-0.1766*	0.0066	-0.0013	0.0112
	(-4.18)	(-3.47)	(-1.75)	(0.75)	(-0.10)	(0.89)
Constant	0.3715***	0.1381***	57.4708***	-3.5020***	-4.5423***	-7.0613***
	(10.86)	(3.78)	(27.77)	(-27.52)	(-26.32)	(-20.32)
N	518	518	518	518	518	518
R^2	0.11	0.06	0.07	0.08	0.07	0.05

Table 8. Cross-sectional Regressions (Placebo)

This table presents a placebo test for the cross-sectional regressions estimated in Table 7. The regressions are estimated in the cross-section of all stocks, but the dependent variables are computed during the *pre-ban period* (January 16, 2020 – March 16, 2020) as a placebo test. More specifically, we use the following quantities as dependent variables: *mean*, *median*, *volatility*, *10th percentile*, *5th percentile*, *1st percentile* of stock returns. For the computation of percentiles we use linear interpolation when the desired quantile lies between two data points. All dependent variables are expressed in percentage terms. *Institutional Ownership* represents the fraction of shares outstanding (in %) owned by institutions (e.g., pension funds, mutual funds, insurance companies) and is sourced from Bloomberg as of December 31, 2019. All other controls are as of January 16, 2020, the beginning of the pre-ban period. *Market Capitalization* is the market value of a firm in billions USD; *Dollar Volume* is the average trading volume in millions USD; and *Amihud ILLIQ* is the Amihud (2002) illiquidity measure constructed as the absolute return divided by the dollar volume (scaled by 10^6). The coefficient of interest is the one corresponding to *Institutional Ownership*, measuring the differential effect of institutional ownership (a proxy for informed trading) on price support measures. All specifications include country and industry fixed effects, and standard errors are clustered at the stock level. The numbers reported in parentheses are t-statistics.

	(1) Mean	(2) Median	(3) Vol	(4) 10th pctl	(5) 5th pctl	(6) 1st pctl
Institutional Ownership	-0.0010	-0.0003	-0.0214	0.0030	-0.0009	0.0013
	(-1.01)	(-0.29)	(-0.92)	(1.12)	(-0.23)	(0.28)
Market Cap	0.0010	0.0026***	-0.0600***	0.0060**	0.0081***	0.0023
	(1.19)	(4.13)	(-3.27)	(2.55)	(2.63)	(0.57)
Dollar Volume	0.0000	0.0000	-0.0010**	0.0002***	0.0001	0.0002*
	(1.34)	(0.20)	(-2.24)	(3.06)	(0.74)	(1.71)
Amihud ILLIQ	0.0999***	0.0674***	-1.2292*	0.1797**	0.2400**	0.3718***
	(2.77)	(2.68)	(-1.91)	(2.04)	(1.98)	(3.41)
Constant	-1.9005***	-1.2479***	71.2631***	-6.6572***	-8.8562***	-13.1171***
	(-32.83)	(-24.08)	(51.66)	(-40.38)	(-39.86)	(-47.40)
N	1,501	1,501	1,501	1,501	1,501	1,501
R^2	0.20	0.12	0.24	0.22	0.23	0.20

A Technical Appendix

General distribution $f_u(\eta)$: Let us consider the following family of symmetric distributions in $[0,1]$, parametrized by $u \in [0,2]$:

$$f_u(\eta) = \begin{cases} u - 4(u-1)\eta & \eta \leq \frac{1}{2} \\ 4 - 3u + 4(u-1)\eta & \eta > \frac{1}{2} \end{cases}$$

For example, for $u = 1$, we obtain the $U[0,1]$ distribution. But more generally, this is a tractable family[26] of distributions in $[0,1]$, indexed by u, that can be (second-order) stochastically ordered. Since these distributions are symmetric with $E[f_u(\eta)] = 1/2$, it is easy to show that $f_{u_1}(\eta) \succeq f_{u_2}(\eta)$ iff $u_1 < u_2$: if we consider the ratio $\frac{f_{u_1}(\eta)}{f_{u_2}(\eta)}$, this is increasing in $[0, \frac{1}{2}]$ and decreasing in $[\frac{1}{2}, 1]$. It, thus, follows by Ramos, Ollero, and Sordo (2000) that the two distributions are second-order stochastically ordered. Then using Lemma 1, and assuming that K is sufficiently small, we get:

Lemma 4. *In the unconstrained economy when c is sufficiently small ($c < \frac{(1-a)p}{(1-a)+4a(1-p)}$), the likelihood of a very low price, $P(q < c)$, is increasing in the perceived variance of η_s.*

Proof. Using Lemma 1, we know that

$$P(q < c) = \int_0^K ((1-p)g\alpha + (1-\alpha)g\eta)\, f_u(\eta) d\eta$$

We now have that $c < \frac{(1-a)p}{(1-a)+4a(1-p)} \implies K < \frac{1}{4}$ and hence we can write

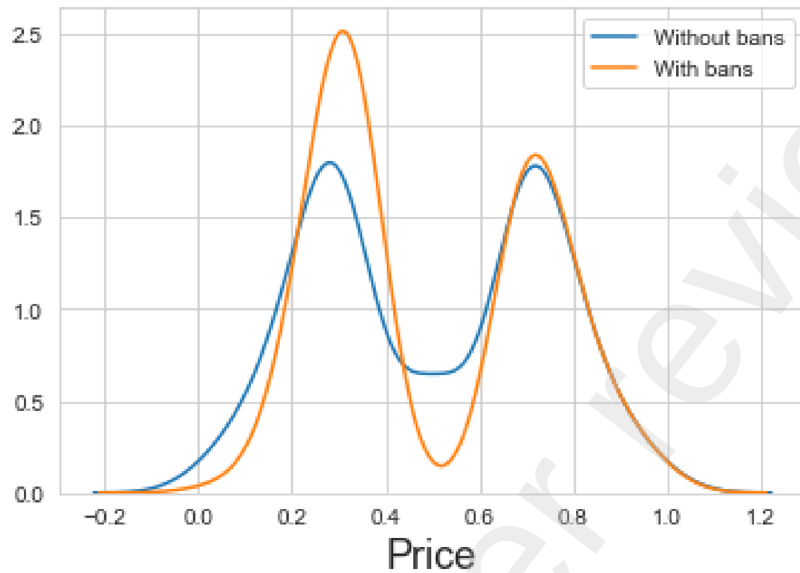
$$P(q < c) = \int_0^K ((1-p)g\alpha + (1-\alpha)g\eta)\,(u - 4(u-1)\eta) d\eta$$

We can easily see that the above expression is increasing in u. But also, because of the stochastic dominance result shown above for the specified families of distributions $f_u(\eta)$, we get that $var[\eta(u)]$ is also increasing in u. Thus, the more uncertain the Regulator is about

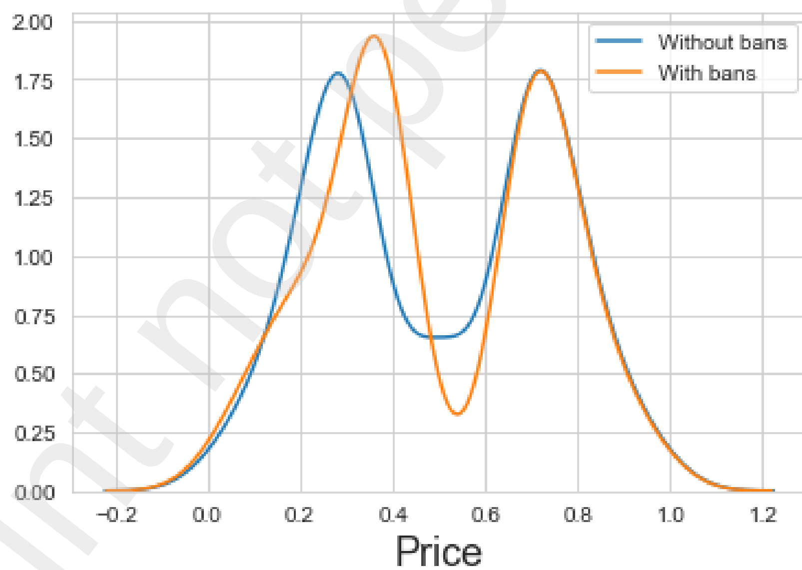
[26] We choose this family of distributions, in comparison to other such families (e.g. Beta(a,a) distributions), so that we can compute the $E[\eta_s | \eta_s < K]$ in closed form.

the sentiment of the Noise traders, the higher the left tail of prices and hence the higher the likelihood of bans getting imposed. □

B Figures



(a) Distribution of Prices when $h_I^2/h_N = 0.2$ (low)



(b) Distribution of Prices when $h_I^2/h_N = 1.25$ (high)

Figure A1. Distribution of Prices

In both plots $a = 0.5, p = 0.5, g = 0.9, \eta = 0.5, h_N = 0.2$. Top: $h_I = 0.2$, so that h_I^2/h_N is low; bans reduce the probability of a left tail event. Bottom: $h_I = 0.5$, so that h_I^2/h_N is high; bans increase the probability of a left tail event.

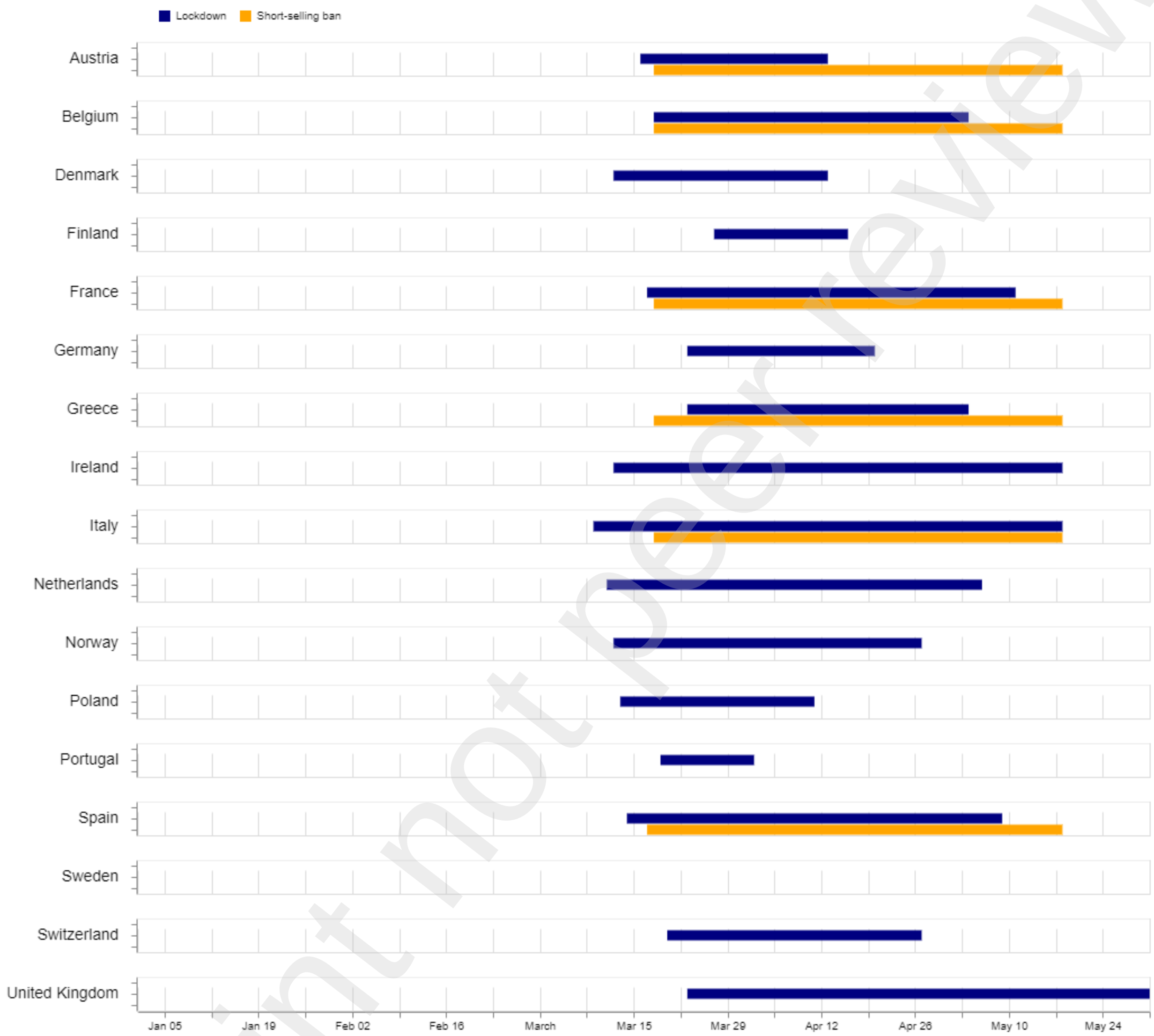


Figure A2. Short-Selling bans and Lockdown Measures in Europe

This figure displays the inception and lifting of short-selling bans in Austria, Belgium, France, Italy, and Spain, as well as the lockdown periods across all 17 countries in our data set.

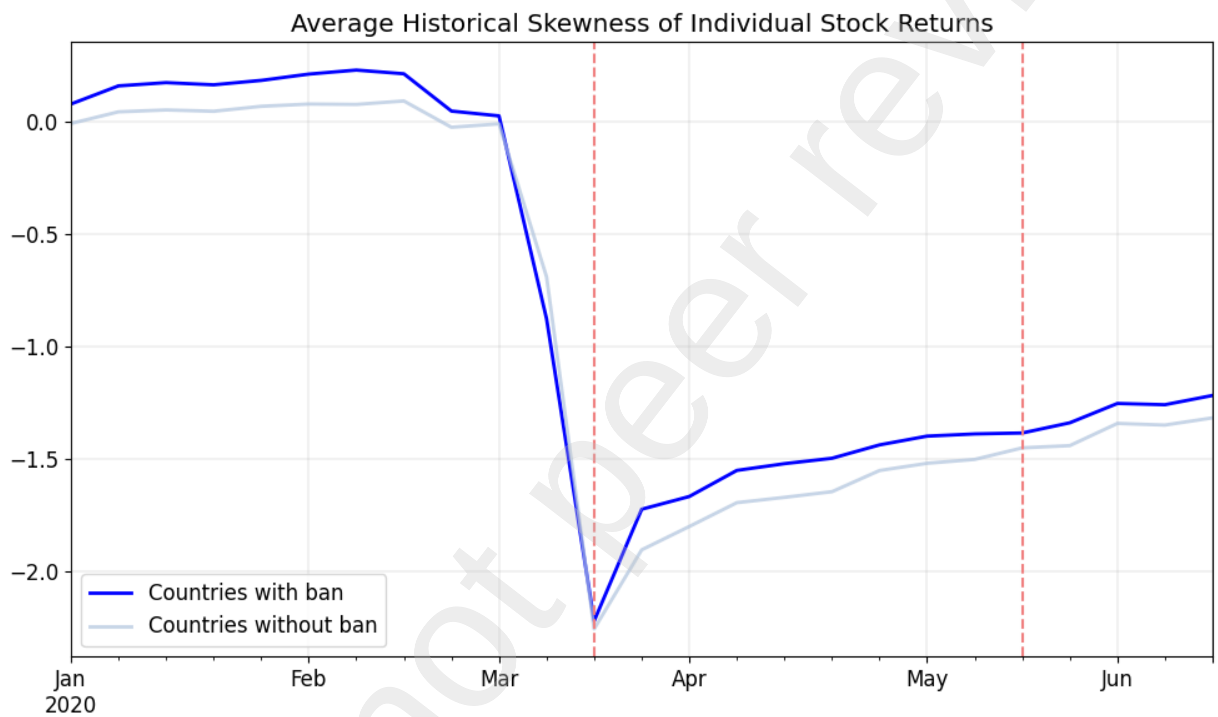


Figure A3. Average Historical Skewness of Stock Returns

This figure shows the average historical skewness in countries that banned short sales (Austria, Belgium, France, Greece, Italy, and Spain) and countries that did not (Denmark, Finland, Germany, Ireland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and the United Kingdom).