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

Pasquale Della Corte
Imperial College London
& CEPR

Robert Kosowski
Imperial College London
& CEPR

Dimitris Papadimitriou
King's College London

Nikolaos P. Rapanos
Imperial College London

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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 lies in the way the ratio of informed to noise traders who own the stock influences the effectiveness of short-selling bans. Empirically we use institutional ownership as a proxy for the ratio of informed to noise traders who own the stock and 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."*
>
> — Financial Conduct Authority, Statement on UK markets, 23 March 2020.

The effects of short-selling bans on stock market liquidity and price discovery during periods of financial distress have been part of a long-standing debate among regulators and academics around the world (e.g., Beber and Pagano, 2013; ESMA, 2022). Disagreement on the effectiveness of these temporary restrictions in restoring market quality 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.*"[1]

In this paper, we contribute to this debate by providing new insights on the potential costs and benefits of short-selling bans. To guide our analysis, we first build a stylized theory model that endogenizes the decision of a regulator to impose bans to short-selling, and then derive the theoretical implications of such a decision for both asset prices and liquidity. Hence, we empirically verify the predictions of our model by exploiting the differences between European countries that imposed short-selling bans and European countries that abstain from such restrictions between March and May 2020 using a difference-in-differences exercise.

The power to temporarily restrict the 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.[2]

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

[2] A significant fall refers to a price drop larger than 10% for liquids shares, larger than 20% for illiquid

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 were located, and covered all stocks traded in cash and derivatives markets, including American Depository Receipts, while bearish intraday operations were also in scope. Finally, the prohibitions did not apply to market-making activities or trading in index-related instruments (more details are provided in Section 3).

Our theoretical model builds on and extends the seminal work of Diamond and Verrecchia (1987) by introducing a *regulator* whose goal is to avert a sharp decline in asset prices. The regulator is uncertain about the liquidity needs of *noise traders* and decides whether to prohibit short-selling activity on various market characteristics. We show that a necessary condition for her to take action is that fundamentals are weak or that the liquidity needs of *noise traders* are very volatile. Indeed, this volatility is likely to have played a great role in the recent decision of various European countries to implement short-selling bans. For example, an ESMA opinion issued on March 2020 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"*. However, the need for an intervention does not necessarily imply that imposing a short-selling ban is the solution. 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

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).

traders who own the stock. Intuitively, the smaller the fraction of informed traders who own the stock, the lower the adverse selection that market makers are facing and the less likely that a sell order is submitted by an informed investor. Consequently, the market makers will set a bid price that will not reflect a very negative view about the future payoff, and thus this price will not be very low.

Using data for 17 European stock markets, we test five hypotheses implied from our model. The first three hypotheses compare countries with and without short-selling bans whereas the last two hypotheses focus on cross-sectional predictions within countries that prohibited short-selling activity. Overall, they lead to the following results. First, we use institutional ownership as a proxy for the fraction of informed traders who own the stock and show that institutional ownership is lower in countries that imposed short-selling bans. Second, in line with the existing 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 up to 13 basis points, as measured by average 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 unperformed stocks not affected by such restrictions, in line with Beber and Pagano (2013). Fourth, we uncover that 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. Finally, stocks with lower institutional ownership benefit more in terms of limiting extreme negative outcomes (i.e., left tail support), as measured by the maximum drawdown.

The key insight from our work that is relevant for regulators considering the imposition of temporary short-selling bans is that the effectiveness of such measures in preventing large declines in stock prices is related to the fraction of informed to noise traders who own the stock. We uncover this channel with a theoretical model that extends Diamond and Verrecchia (1987) and we identify *institutional ownership* as a proxy for the relative composition of *informedness* among stock holders. In particular, our findings suggest that short-selling bans could be effective in markets with low institutional ownership, whereas the same restrictions might bear no benefit in markets with a (relatively) high degree of informed stock holders.

Moreover, we corroborate the findings of earlier literature (e.g., Beber and Pagano, 2013; Enriques and Pagano, 2020) about the detrimental effect of short-selling bans on liquidity. Thus, we provide a framework to think about the trade-offs facing the regulators as well as some quantitative estimates on the impact of short-selling bans based on the most recent experience in Europe.

Our paper is related to three strands 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; by exploiting the difference between financial stocks that were targeted by the ban and those that were not, they find that liquidity worsened in all but the smallest stocks. Similarly, Marsh and Payne (2012) study the effect of the short-selling ban in the UK stock market in 2008 and document a deterioration of liquidity on affected stocks and conclude that bans were detrimental to the quality of the market; they also recognise, however, that "if the goal of the FSA was to arrest sharp declines in financials' stock prices" then their results may signify that the policy of banning short sales was successful. 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, and by focusing on their potential benefits in terms of supporting the left tail of returns.

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, while 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 and jointly studying the conditions for such an intervention and its implications; in particular, we focus on the effect of bans on the liquidity and the distribution -especially the left tail- of prices of affected stocks.

Our work, finally, 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

In this section, we first extend the model of Diamond and Verrecchia (1987) by introducing a *regulator* who can impose a short-selling ban to avert a sharp decline in prices. Then, we find conditions under which it is optimal for the *regulator* to impose such restrictions to short sales.

2.1 Setting

We consider a static model with a single stock in the spirit of Diamond and Verrecchia (1987). The value of the risky asset is denoted by V and follows a Bernoulli distribution that takes the liquidating value of one with probability p or the liquidating value of zero with probability $1-p$. While the liquidating value is paid in the future, we abstract from any discounting for simplicity. This model comprises an infinite number of risk-neutral traders who sequentially enter the market and want to trade with probability g. They can also decide not to engage in trading with probability $1-g$, a case in which no trade is observed. Active traders initiate a buy order or a sell order of a unit of the risky asset. When a sell order is submitted by a trader who does not own the asset, we have a short sale. Since no market participants can distinguish sell orders from short sales, the set of observed actions include buy orders, sell-or-short orders, and no trade.

There are two types of traders in our setting: *informed* traders and *noise* traders. The first group of traders have a probability mass of α, privately know the true liquidating value of the stock, and trade only for information reasons. An *informed* trader that wants to trade submits a buy order when V is high and a sell (or short) order when V is low. The second group of investors have a probability mass of $1-\alpha$, infer the value of the risky asset using publicly available information, and trade only for liquidity reasons exogenous to our model. A *noise* trader with a desire to trade submits a buy order with probability $1-\eta_s$ and a sell-or-short order with probability η_s. Each group of traders, moreover, owns the stock with a given probability. We use h_I to indicate the probability that an *informed* trader owns a share of the risky asset prior to submitting a sell-or-short order, and h_N to denote the corresponding quantity for a *noise* trader. h_I and h_N can also be interpreted as the fraction of *informed* and *noise* traders that own the stock, respectively. When a trader faces restrictions to short sales and owns no stock, we will observe no trade.

In addition to *informed* and *noise* traders, our model is also populated by competitive risk-neutral *market makers* facing no inventory costs or constraints, so that the expected profit from each trade is zero. The *market maker* has no access to private information, but he observes all trades as they take place, and knows the probability of a sell order from a *noise* trader. He sets bid and ask prices such that profits and losses from transacting with *noise* and *informed* traders, respectively, offset each other. Ultimately, the *market maker* will post a bid (ask) price that equals his expectation of V conditional on public information and the fact that the transaction is a sell-or-short (buy) order. Finally, the demand for the risky asset is assumed to be bounded as otherwise the *informed* traders would be willing to trade an infinite amount of the risky asset.

The first difference between our model and that of Diamond and Verrecchia (1987) is that our economy also includes a *regulator*, whose main objective is to avert a disastrous decline in price by ensuring that

$$P(q < c) < x, \tag{1}$$

where q is the equilibrium price of the risky asset from her perspective, c is a sufficiently low price threshold, x is a confidence level, and $P(q < c)$ denotes the probability that q falls below c. For example, if $q = €1,000$, $c = €600$, and $x = 5\%$, the goal of the *regulator* is to avert a stock price decline below €600 with a confidence interval of 5%. When q falls below c, the *regulator* must decide whether or not to impose a ban on the short sales of the risky asset as a sharp decline in price represents a potential threat for financial stability. For instance, Brunnermeier and Oehmke (2014) find that a sharp decrease in the share price of financial institutions combined with leverage constraints may lead to a bank-run in equilibrium. Similarly, an ESMA opinion issued on March 2020 concerning the French short-selling bans states: *"the AMF considers that the growth of short positions betting on negative news (be they real or ill-based)... could destabilize markets in a way that could be self-reinforcing, with downward price spirals"*.

At the time of her decision (that is, before any investors enters the market), the *regulator* knows the probability distribution of a sell order from a *noise* trader, which we denote as $f(\eta_s)$. This assumption allows us to examine how uncertainty about the liquidity needs or the sentiment of Noise traders, captured by η_s, impacts the policy decision of the *regulator*; indeed, this uncertainty implies that the bid price is a random variable and the probability of a sharp decline in price is never zero from her perspective. Other than that, the regulator knows all the other market parameters and could be considered a sophisticated uninformed agent.

To preview our results, the equilibrium price can fall below the price threshold when the *market maker* sets a bid price that is below such threshold and simultaneously a trader submits a sell/short order. We show that the joint likelihood of these events depends on the probability of a sell order from a *noise* trader. Specifically, when η_s is high, the bid price is unlikely to fall below the price threshold as the *market maker* anticipates that any sell/short order is uninformative about the liquidating value of the risky asset. Vice versa, when η_s is low, any sell order may carry valuable information about the liquidating value of the risky asset, thus increasing the probability of having a bid price falling below the price threshold. Hence, the *regulator* will decide whether to impose a short-selling ban depending on the effect of η_n on this joint probability.

TABLE 1 ABOUT HERE

The second difference compared to Diamond and Verrecchia (1987) is that h_N and h_I are not necessarily identical, thus relaxing one of their assumptions. In doing so, we show that the *market maker* will set a different bid price depending on whether short-selling activity is allowed or restricted. Also, the relationship between h_N and h_I will affect the policy decision of the *regulator* on whether or not to introduce a short-selling ban. We summarize our notation in Table 1 before presenting our theoretical predictions in the next section.

2.2 Unconstrained Short Selling

We first study the model when all traders can freely short the asset. Akin to Diamond and Verrecchia (1987), a *market maker* sets the bid price at which he is willing to buy a share of the risky asset equals to his expectation of V conditional on public information and the fact the transaction is a sell-or-short order, i.e., $Bid = \mathbb{E}\left[V|Sell\right] = \sum_v P\left(V = v|Sell\right) \times v$. Since V takes either the value of one or zero, we obtain $Bid = P\left(V = 1|Sell\right)$ and using the Bayes' rule

$$Bid = \frac{P(Sell|V = 1) \times P(V = 1)}{P(Sell)}. \tag{2}$$

When the liquidating value of the risky asset is high, only a *noise* trader would submit a sell-or-short order. As a result, the conditional probability of a sell-or-short order is given by

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

while its unconditional probability equals

$$P(Sell) = g(1 - \alpha)\eta_s + g\alpha(1 - p), \tag{4}$$

with $g\alpha(1-p)$ accounting for the probability that an *informed* trader submits a sell-or-short order. The latter only happens when the liquidating value of the risky asset is low. By plugging Equations (3) and (4) into Equation (2) and simplifying, we obtain the bid price when all traders can freely short the asset

$$Bid = \frac{(1 - \alpha)\eta_s p}{(1 - \alpha)\eta_s + \alpha(1 - p)}. \tag{5}$$

Similarly, the ask price at which a *market maker* is willing to sell a unit of the risky asset equals his expectation of V conditional on public information and the fact the transaction is a buy order, i.e., $Ask = \mathbb{E}\left[V|Buy\right]$. Using the Bayes' rule, it then follows that

$$Ask = \frac{P(Buy|V = 1) \times P(V = 1)}{P(Buy)}. \tag{6}$$

Since any traders can submit a buy order when the liquidating value of the risky asset is high, the conditional probability of a buy order is given by

$$P(Buy|V = 1) = g(1 - \alpha)(1 - \eta_s) + g\alpha \tag{7}$$

whereas its unconditional probability is equal to

$$P(Buy) = g(1 - \alpha)(1 - \eta_s) + g\alpha p. \tag{8}$$

By substituting Equations (7) and (8) into Equation (6), we obtain the ask price as

$$Ask = \frac{(1 - \alpha)(1 - \eta_s)p + \alpha p}{(1 - \alpha)(1 - \eta_s) + \alpha p}. \tag{9}$$

When no trade occurs, finally, the *market maker* cannot update his beliefs and sets the stock price equal to the conditional expectation of V given public information. As the liquidating value of the risky asset is one with probability p and zero with probability $1 - p$, the *No Trade* price is set as $\mathbb{E}\,[V|No\ Trade] = p$, which is independent of η_s.

In this scenario, the *regulator* has to decide whether or not to introduce a ban on short sales. Since the prices posted by the *market maker* are random from her perspective, the equilibrium price q follows a compound probability distribution that depends on $f(\eta_s)$ as well as $\mathbb{E}\,[V|Sell]$, $\mathbb{E}\,[V|Buy]$, and $\mathbb{E}\,[V|No\ Trade]$ with probabilities $P(Buy|\eta_s)$, $P(Sell|\eta_s)$ and $P(No\ Trade|\eta_s)$, respectively. Put differently, conditional on the unknown parameter η_s, the *regulator* knows that q follows a three point probability distribution. To derive the probability of a sharp decline in prices in the absence of any short-selling restriction, we further assume that the price threshold c is less than the maximum bid price and less than the minimum no-trade price, i.e., $c < \frac{(1-\alpha)p}{1-\alpha p}$. We then have

Lemma 1. *When all agents are unconstrained, the regulator's perceived probability that the stock price q falls below a price threshold c is given by:*

$$P(q < c) = \int_0^K \left((1 - p)g\alpha + (1 - \alpha)g\eta\right) f(\eta)d\eta \tag{10}$$

where $K = \frac{c\alpha(1-p)}{(1-\alpha)(p-c)}$. If we assume that $\eta_s \sim U[0,1]$, we then obtain

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

Proof. If c is small enough, we have

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

where $Bid < c$ iff $\eta_s < K$, and $\mathbb{1}$ is an indicator function that takes the value of one (and zero otherwise) if $q < c$. Noting that $P(Sell|\eta_s) = (1-p)g\alpha + (1-\alpha)g\eta_s$, we than have

$$P(q < c) = ((1-p)g\alpha + (1-\alpha)g\mathbb{E}[\eta_s \mid \eta_s < K]) \cdot P(\eta_s < K) \tag{11}$$

which concludes the proof. □

According to Lemma 1, a sharp decline in prices is more likely (i.e., $P(q < c) \uparrow$) when *noise* traders are unlikely to sell (i.e., $P(\eta_s < K) \uparrow$) and the expected probability of a sell order is likely to come from *informed* traders (i.e., $\mathbb{E}\left[P(Sell|\eta_s) \mid \eta_s < K\right] \uparrow$), thus easily revealing any negative information they possess. Differently, when the probability of a sell order from *noise* traders is high, it is unlikely for *market makers* to set a low bid price as any sell order is unlikely to reflect valuable information about V. By using a uniform distribution for η_s, we then assume that the *regulator* has an uninformative prior while adding tractability to our results. In Lemma 5 in the Appendix, we specify a general family of distributions and show that $P(q < c)$ increases as the variance of η_s increases, while keeping K constant. Thus, the *regulator* is more likely to consider imposing bans when uncertainty about the liquidity needs of the Noise traders is larger.

We can then obtain a necessary condition for the implementation of a short-selling ban:

Result 1. *There exists a threshold ζ such that the regulator imposes a ban on short sales* *only if:*

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

Proof. The *regulator* only acts when $\int_0^K ((1-p)g\alpha + (1-\alpha)g\eta)\, f(\eta)d\eta > x$ according to Equation (11). Since the left-hand side is increasing in K, we obtain the above necessary condition. □

In particular, the *regulator* may only impose a ban when the probability of a high payoff is low (i.e., $p \downarrow$), the probability mass of *informed* traders is high ($\alpha \uparrow$) or when the desired support of the left tail of prices (captured by the threshold c) increases. These conditions are more likely to arise during a crisis and can be thought of as being also driven by macroeconomic variables or by the *country-specific risk*; hence in the empirical part of our paper we also control for the credit default swap (CDS) spreads of each country. It is important to note here that our assumption that the Regulator only cares about averting a sharp decline in prices means that any action he takes, will not take into account the effect on the *informational efficiency* of the market; in particular, the Regulator would potentially harm efficiency when imposing bans while the fundamentals were weak (captured by a low p).

While the above conditions give us the potential trigger for the Regulator's intervention, 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 the Regulator decides to impose short-selling. In this way, we are able to endogenously determine the conditions under which a regulator would optimally choose to ban short-selling activity.

2.3 Imposing Restrictions on Short Selling

When short selling is allowed, the *market maker* in unable to distinguish a sell order from a short sale. When short selling is prohibited, however, only investors that own the stock can sell it and *market makers* form bid prices while taking such information into consideration. Importantly, the fraction of *informed* traders who own the stock (h_I) differs from the fraction of *noise* traders who own the stock (h_N), thus affecting the adverse selection faced by *market* *makers.* Here, we study the effect of short-selling constraints on the bid-ask spread and obtain the following lemma:

Lemma 2. *When short-selling bans are in place, the bid-ask spread increases, relative to the unconstrained case, if and only if* $\frac{h_I}{h_N} > 1$.

According to Lemma 2, under short-selling bans, any sell orders is more likely to come from an informed trader when $h_I > h_N$. Under these circumstances, *market makers* face higher adverse selection and thus set a lower bid price that will harm liquidity. To see why this happens, we first derive bid and ask prices when short selling is restricted and then draw a comparison with bid and ask prices formed when short selling is allowed.

The bid price set by *market makers* under short-selling bans is given by

$$\widetilde{Bid} = \frac{(1-\alpha)h_N\eta_s p}{\alpha h_I(1-p) + (1-\alpha)h_N\eta_s} \tag{12}$$

whereas the corresponding ask price is equal to

$$\widetilde{Ask} = \frac{(\alpha + (1-\alpha)(1-\eta_s))p}{\alpha p + (1-\alpha)(1-\eta_s)}. \tag{13}$$

By comparing Equations (5) and (12), we observe that the difference between $\widetilde{Bid}$ and Bid depends on the ratio between *informed* and *noise* traders owning the stock, and $\widetilde{Bid} < Bid$ when $h_I/h_N > 1$. From Equations (9) and (13), we instead learn that $\widetilde{Ask}$ and Ask remain identical as short-selling bans do not affect the buying activity of any investors. It then follows that the bid-ask spread, a commonly used measure of liquidity, widens under short-selling bans

$$\underbrace{\frac{\widetilde{Ask} - \widetilde{Bid}}{\widetilde{Bid}}}_{\text{short selling is prohibited}} > \underbrace{\frac{Ask - Bid}{Bid}}_{\text{short selling is allowed}} \tag{14}$$

when more *informed* than *noise* traders holding the stock populate the economy, or $h_I > h_N$.

When no trade occurs under short-selling bans, *market makers* will condition on η_s to determine their $\widetilde{No\ Trade}$ price. In particular, when η_s is low, *market makers* may attribute absence of trade to constrained *informed* traders that are unable to short the stock, thus updating downward their expectations of V. When η_s is high, in contrast, *market makers* may associate lack of trading activity to constrained *noise* traders that cannot short the stock, thus adjusting upwards their expectation on V. As a result, under short-selling bans, the $\widetilde{No\ Trade}$ price is set as $E[V|No\ Trade, \eta_s] = \frac{((1-g)+g(1-\alpha)\eta_s(1-h_N))p}{1-g+g((1-\alpha)\eta_s(1-h_N)+\alpha(1-p)(1-h_I))}$, which is increasing in η_s and always less than p. To understand this formula note that, when $V = 1$, there will be no trade either when an investor does not want to trade or when a *noise* trader wants to sell but does not own the stock; thus, $P(No\ Trade|V = 1) = (1 - g) + g(1 - \alpha)\eta_s(1 - h_N)$. Similarly, unconditionally, there will be no trade either when an investor does not want to trade or when a constrained *noise* trader or a constrained *informed* trader want to sell; hence, $P(No\ Trade) = (1 - g) + g((1 - \alpha)\eta_s(1 - h_N) + \alpha(1 - p)(1 - h_I))$.

Figure 1 about here

In Figure 1, we provide a graphical illustration of the relationship between η_s and the prices – *Bid*, *Ask*, and *No Trade* – set by *market makers*. We use simulated data based on $\alpha = 0.5$, $p = 0.5$, $g = 0.9$, and $h_N = 0.3$. Panel A considers the benchmark scenario where all traders can freely short the risky asset, and shows that *Bid* and *Ask* are both increasing with η_s. *No Trade*, in contrast, is independent of η_s since *market makers* cannot update their beliefs and rely solely on public information to determine the conditional expectation of V, which equals 0.5 in our simulation. In the remaining panels, instead, we cover the alternative scenario where the *regulator* imposes a ban on short sales while using different levels of h_I/h_N. Specifically, Panel B sets $h_I = 0.6$ such that $h_I/h_N > 1$ and displays a wider bid-ask spread than the benchmark case, driven by a lower *Bid* and an identical *Ask*. Also, *No Trade* is now an increasing function of η_s since a higher (lower) η_s is associated with fewer (more) constrained *informed* investors such that *market makers* adjust upward (downward) their expectations of V. In Panel C, we set $h_I/h_N = 1$ as in Diamond and Verrecchia (1987) and show that the bid-ask spread remain identical to the benchmark case. In Panel D, finally, we set $h_I = 0.2$ such that $h_I/h_N < 1$ and report a tighter bid-ask spread than the benchmark case due to a higher *Bid*. This happens as sell orders are more likely to originate from a *noise* trader, thus reducing the adverse selection faced by *makers*.

We now examine the probability of sharp decline in price when the *regulator* imposes bans on short sales.[3] Following the logic of lemma 1 we first find the region of η_s that makes a bid price sufficiently low ($< c$): we note that $\widetilde{Bid} < c$ if and only if $\eta_s < \frac{h_I c\alpha(1-p)}{h_N(1-\alpha)(p-c)} = \frac{h_I}{h_N}K$. We then get the following lemma:

Lemma 3. *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:

$$P(\tilde{q} < c) = \frac{h_I^2}{h_N} P(q < c) \tag{15}$$

Proof. The proof of the first part of the Lemma, is the same as that of Lemma 1, with $\frac{h_I}{h_N}K$ playing the role of K. As for the case of the uniform distribution we have:

$$\begin{aligned}
P(\tilde{q} < c) &= \left((1-p) g h_I \alpha + (1-\alpha) g h_N \mathbb{E}[\eta_s \mid \eta_s < \frac{h_I}{h_N}K]\right) \cdot P(\eta_s < \frac{h_I}{h_N}K) \\
&= h_I \left((1-p) g \alpha + (1-\alpha) g \frac{\mathbb{E}[\eta_s \mid \eta_s < \frac{h_I}{h_N}K]}{h_I/h_N}\right) \cdot \frac{h_I}{h_N} \frac{P(\eta_s < \frac{h_I}{h_N}K)}{h_I/h_N} \\
&= h_I \left((1-p) g \alpha + (1-\alpha) g \mathbb{E}[\eta_s \mid \eta_s < K]\right) \cdot \frac{h_I}{h_N} P(\eta_s < K) \\
&= \frac{h_I^2}{h_N} P(q < c)
\end{aligned}$$

where to get from the second to the third line we used the linearity of the cumulative distribution function of the uniform distribution, while to get to the final line we used equation (11). □

FIGURE 2 ABOUT HERE

Figure 2 graphically depicts the relationship between the statistic K (which is an increasing

[3] We assume that $c < \min\left\{\frac{(1-\alpha)h_N p}{\alpha h_I(1-p)+(1-\alpha)h_N}, \frac{(1-g)p}{1-g+g\alpha(1-p)(1-h_I)}\right\}$ where the first term is the maximum $\widetilde{Bid}$ and the second term is the minimum possible $\widetilde{No\ Trade}$.

function of c) and the probability that the prices falls below the threshold c, in various cases, assuming that $\alpha = 0.5, p = 0.5, g = 0.9, h_N = 0.3$. The black line corresponds to the baseline case when there is no short selling ban, while the blue and red lines correspond to the constrained case with $h_I^2/h_N = 0.13, 1.2$ respectively. What we can see is that the introduction of a short selling ban leads to the drop of the probability of a sharp decline in price, when h_I^2/h_N is low. Figure 2 also illustrates Result 1, in that it shows that, in the unconstrained case, there is a threshold ζ so that when $K > \zeta$ the probability of a sharp decline in price surpasses the confidence level x, and may thus trigger an intervention by the Regulators.

The resulting h_I^2/h_N can be decomposed 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, which decreases or increases by a factor of $\frac{h_I}{h_N}$. Although the exact form of equation (15), which conveniently gives us the percentage change in the $P(price < c)$ depends on the particular assumption of uniform distribution of η_s, more generally the above proof shows that for a variety of distributions $f(\eta_s)$ it will still be true that $P(\tilde{q} < c)$ is increasing in h_I and in $\frac{h_I}{h_N}$. Indeed if, for example, η_s has a $Beta(a, 1)$ distribution[4] (for any $a > 0$) then both $\frac{\mathbb{E}[\eta_s | \eta_s < \frac{h_I}{h_N} K]}{h_I/h_N}$ and $\frac{P(\eta_s < \frac{h_I}{h_N} K)}{h_I/h_N}$ are non-decreasing in $\frac{h_I}{h_N}$. We thus believe that the empirical predictions that we will derive using our results are likely to hold despite the fact that the actual distribution of η_s is unknown.

Focusing for the rest of this chapter on the case of uniform distribution, we can now get a necessary and sufficient condition for the short-selling ban to be successful:

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 if and only if*

$$h_I^2 < h_N. \tag{16}$$

As we can see, the more the Noise traders who own the asset and the fewer the Informed investors, the more successful is the ban. The regulator may achieve his goal by reducing the

[4]The probability density function $g(x)$ of a $Beta(a, 1)$ distribution is proportional to x^{a-1}.

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.[5]

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 4. *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 $\mu_{1/2}$, we note that in either case this median coincides with the price attained when there is no-trade (i.e. the expected payoff given a "no-trade action"). Indeed, when there are no bans $E[V|\text{No trade}] = p$. Since $Bid < p < Ask$, we can verify that p is satisfying the conditions for being the median price: $P(q \leq p) = 1 - P(Buy) \geq 1/2$ 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 $\mu_{1/2}(\tilde{q}) < p = \mu_{1/2}(q)$. $\square$

[5] 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 and we estimate a smooth density of prices with or without bans. Without loss of generality, we choose the following baseline parameters: $a = 0.5, p = 0.5, \eta = 0.5, g = 0.9, h_N = 0.3$, 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 3, is with h_I relatively low (equal to $h_N = 0.3$), 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 ($h_I = 0.6$), so that $h_I^2 > h_N$, as can be seen in Panel B of Figure 3, 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. Since h_I is the fraction of informed traders that own the stock, we can consider a type of conjugate probability, m, to be the fraction of the risky stock owned by informed traders (while $1 - m$ is the corresponding fraction owned by Noise traders). If we assume that informed traders in the market are mostly financial institutions, then this fraction m can intuitively reflect the institutional ownership of a stock. Indeed, past literature (e.g. Bai, Philippon, and Savov (2016) and Dávila and Parlatore (2022)) has found that higher institutional ownership is associated with higher levels of price informativeness;

this is consistent with the notion that institutional investors can be considered as *informed* traders. Using Bayes' rule, and assuming that the fraction of informed investors (i.e., α) in the whole economy is fixed, we then obtain $\frac{h_I}{h_N} = \frac{m}{1-m}\frac{1-\alpha}{\alpha}$. Thus:

$$\frac{h_I}{h_N} \propto \frac{m}{1-m} \tag{17}$$

and,

$$\frac{h_I^2}{h_N} \propto \frac{m^2}{1-m} \tag{18}$$

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.*

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. 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 ($Ask = \widetilde{Ask}$), the model is consistent with the following hypothesis:

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. On the other hand, according to Lemma 4 the *expectation* of the price remains the same when bans are in place, while the *median* price decreases (consistent with the findings of Beber and Pagano (2013)), because under the "ban-regime", any no-trade action is more likely to reflect negative news. 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.

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. When short selling is allowed, a sell order may arise from any informed or noise trader in the economy and, hence, Market Makers adjust their expectations of the payoff, depending 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; the higher this fraction 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 potential implications of bans on return dynamics. 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.

Second, we exogenously assume 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. Third, we have implicitly made the assumption 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[6] we exclude from our empirical analysis all micro and nano-cap stocks, which may be less actively traded.

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.[7] 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,

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

[7]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.

as in Beber and Pagano (2013). Table 2 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 2 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 A1 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 sales. 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.[8] 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. Overall, while the proposed duration of short-selling bans varied slightly by country, all countries eventually decided to lift restrictions on short selling on the same date. 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

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

in index-related instruments. 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 4 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 4, 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.[9] We view Figure 4 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

[9] 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.

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 3 ABOUT HERE

Table 3 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 3 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 5 ABOUT HERE

Figure 5 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). Figure 5 shows 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. Visually,

Figure 5 illustrates 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

More formally, we present the results of a difference-in-differences regression estimating the differential effect of short-selling bans on liquidity in Table 4. 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.[10] We find that

[10] 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

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

Table 5 and Figure 5 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 4 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 5, 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.

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.

4.2 Stock Prices

In this section, we examine whether short-selling bans were effective in supporting prices. Figure 6 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 7 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 6 and Figure 7 about here

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, Figure A2 of the appendix shows the average historical skewness in countries that have and countries that have not implemented a ban.[11] 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, and the maximum drawdown (i.e., the maximum cumulative decline from a peak to a trough) 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 6 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. Table 6 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

[11] 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 daily mean returns of stocks subject to bans were a negligible 7 basis points lower, while the median returns were 22 basis points lower (and statistically significant); in contrast, the maximum drawdown was 335 basis points higher (and also significant at the 1% level). Note here that an increase in the maximum drawdown can be interpreted as a support for the left tail of returns, and the magnitude of the coefficient suggests that our results are also economically large. This exercise suggests that short-selling bans support the left tail of the distribution of prices, at the expense of marginally lower mean/median - and poorer liquidity. In addition, we note that during the ban period, the average annualized volatility increases by about 24%, but short-selling bans seem to (almost mechanically) dampen volatility by approximately 20%. In Table 7, we estimate the regression in a matched sample of stocks,[12] and obtain results of the similar magnitude and statistical significance.

Finally, we want to test the prediction of Hypothesis 5, according to which 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. o test this prediction, as before, we split our sample into two groups of stocks -the ones with low (bottom tercile) and the ones with high (top tercile) institutional ownership. We, then, estimate the difference-in-differences regressions of Table 6 in these two subsamples, controlling for stock and time fixed effects. As predicted by the model, the results presented in Table 8 confirm that short-selling bans are more effective in supporting

[12]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.

the left tail of stocks with lower institutional ownership. More specifically, we estimate that when bans are implemented the maximum drawdown for low institutional ownership stocks increases by 4.1%, while for high institutional ownership stocks it increases only by 2

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.[13] 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 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.

4.3 Robustness Tests

In this section, we test the robustness of our results to alternative specifications and methodological choices.

First, we recognize that stock fixed effect and day fixed effects fail to control for unobserved time-varying stock characteristics. If the decision to impose short-selling bans is correlated with such stock characteristics, then our results may incorrectly attribute the effects of those characteristics to the imposition of short-selling bans. In particular, Beber and Pagano (2013) in their seminal paper suggest that country CDS spreads may be correlated with regulators' decisions to impose short-selling bans. If this is indeed the case, then we may be capturing a relationship between bid-ask spreads and (country-specific) default premium rather than a relationship between bid-ask spreads and short-selling bans. It is, therefore,

[13]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..."*

of vital importance to show that our results remain intact after controlling for country CDS spreads.

Table 9 and Table 10 about here

Using CDS spreads from Datastream, we repeat the exercises of Table 4 and Table **??** while controlling for the level of CDS spreads. In Table 9, we estimate the effect of short-selling bans on bid-ask spreads. While the coefficient on CDS spreads turns out to be statistically significant at the 5% level, we observe that our results remain qualitatively unchanged with the coefficient on the interaction term (Ban = Ban Country $\times$ Ban Enactment) remaining statistically significant at the 1% level and only marginally smaller. We now estimate the short-selling bans cause bid-ask spreads to widen by approximately 11 basis points (compared to 13 basis points in our original specification).

Similarly, in Table 10, we estimate the effect of short-selling bans on the distribution of stock returns, and we obtain virtually unchanged results. This makes sense because if our results on liquidity suffered from an endogeneity issue (i.e., bid-ask spreads widen not because of short-selling bans *per se* but because short-selling bans happen to be introduced in countries with higher credit risk), then one should expect our results on the distribution of stock returns to be biased in the opposite direction, i.e., towards not finding support of the left tail of the distribution of stock returns. Thus, we confirm that our main results are intact after controlling for CDS spreads.

Table 11 about here

Second, in Table 11 we repeat the exercise about the impact of short-selling bans on the distribution of stock returns using a window of 60 days (rather than 30 days that we used in Table **??**). We obtain qualitatively and quantitatively similar results, thus, illustrating that our choice of a 30-day window around the imposition of short-selling bans is innocuous.

Putting all of this together, we are confident that our results are robust and indeed capture the mechanism we had in mind when designing the exercises presented in the earlier sections of the paper.

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 thus relevant for regulators considering the costs and benefits of imposing short-selling bans.

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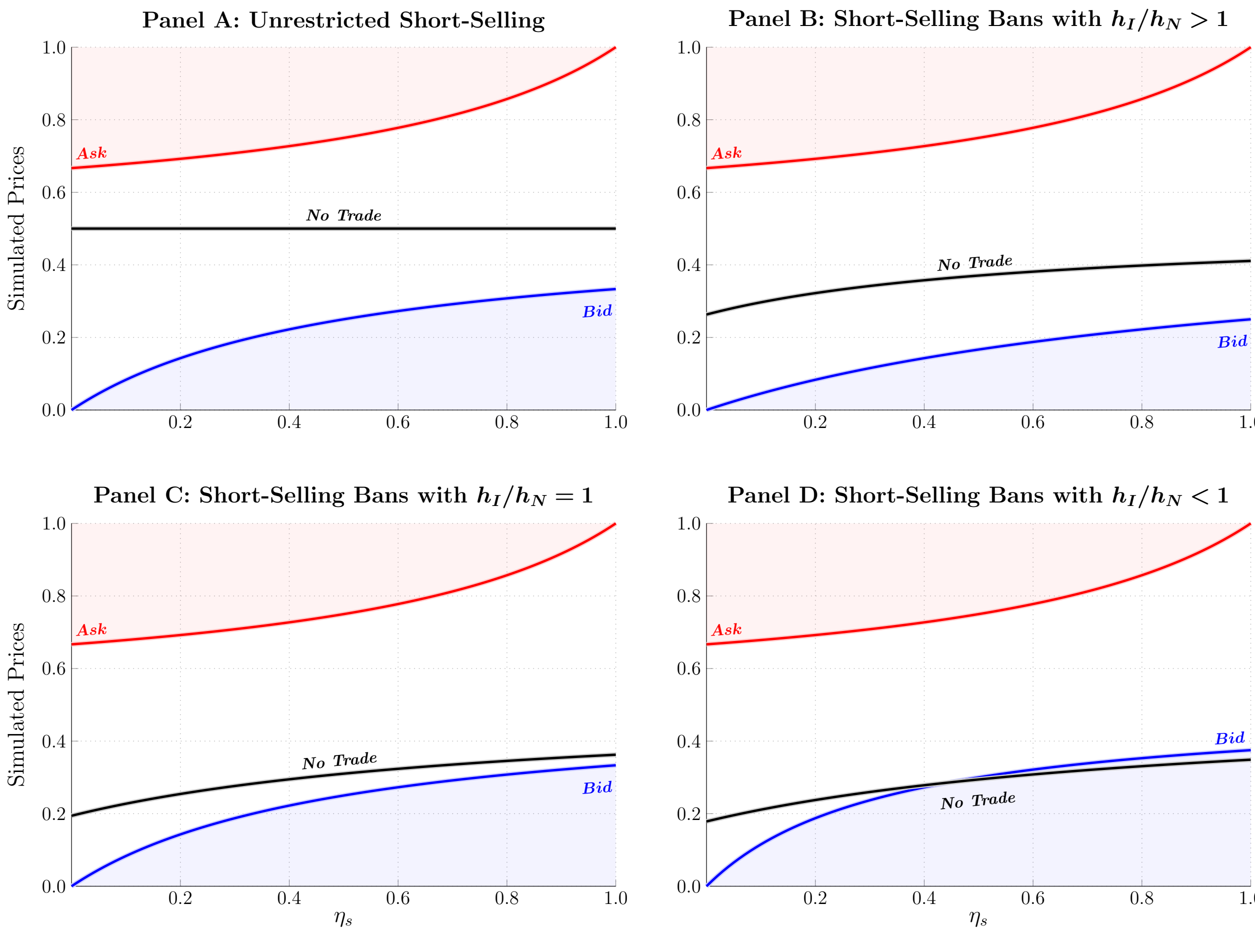

Figure 1. Simulated Bid-Ask Spreads

This figure displays the relationship between η_s and the *Bid*, *Ask*, and *No Trade* prices. Panel A illustrates the case of unrestricted short-selling bans, while Panels B,C and D depict the case when short-selling bans are in place, for h_I/h_N greater than 1, equal to 1, and smaller than 1 respectively.The simulation assumes $\alpha = 0.5$, $p = 0.5$, $g = 0.9$, and $h_N = 0.3$.

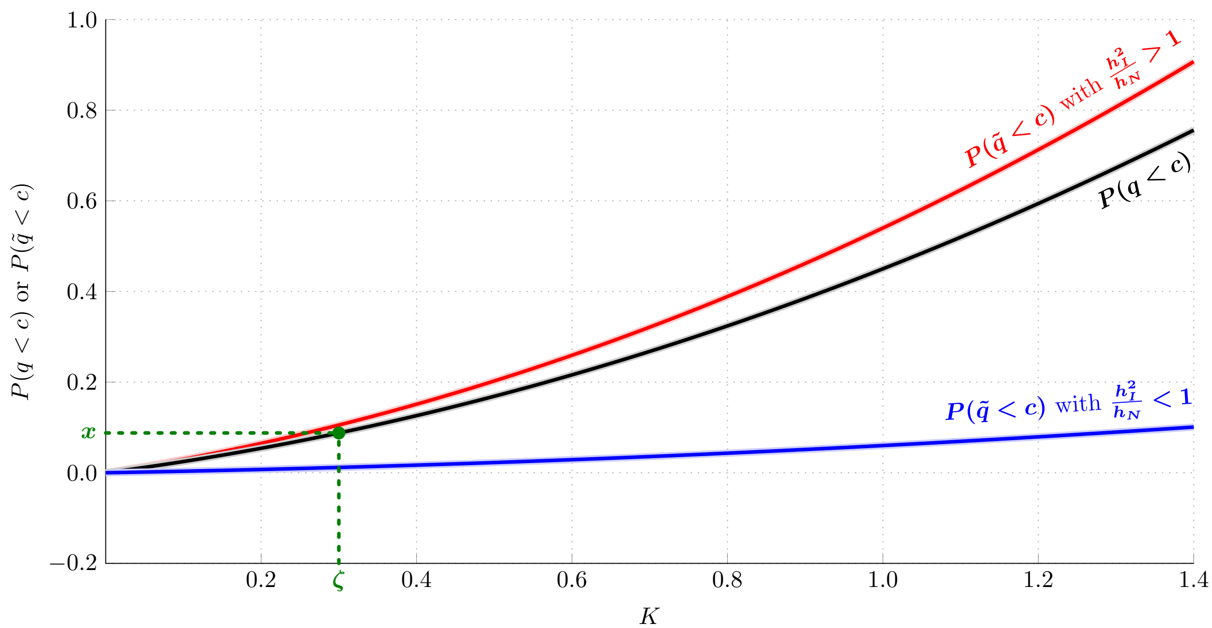

Figure 2. Simulated Bid-Ask Spreads

This figure displays the relationship between the statistic K and the probability that the prices falls below the threshold c, in various cases. The black line corresponds to the baseline case when there is no short selling ban, while the blue and red lines correspond to an economy with short-selling bans where $h_I^2/h_N = 0.13, 1.2$ respectively. The simulation assumes that $\alpha = 0.5, p = 0.5, g = 0.9, h_N = 0.3$

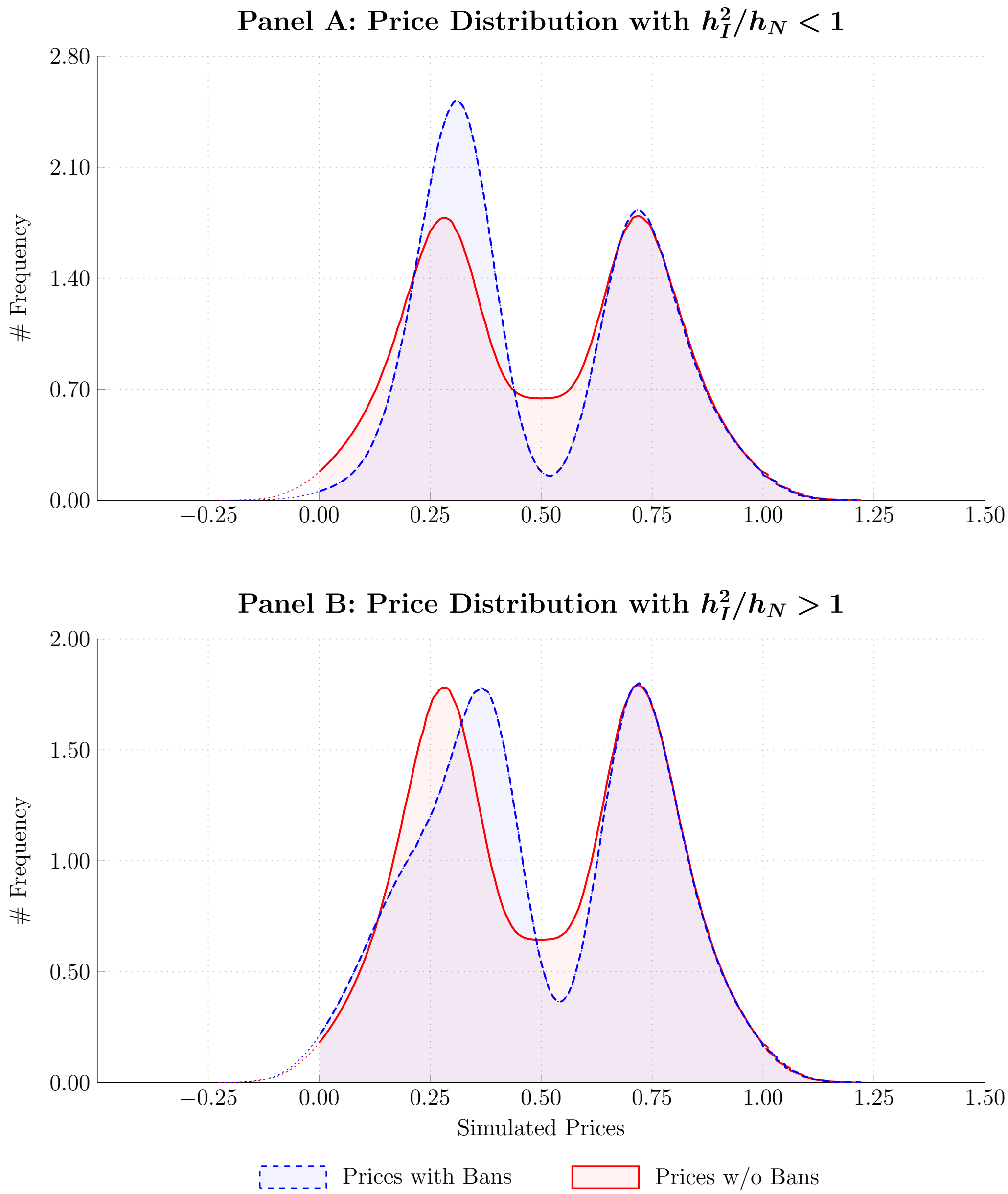

Figure 3. Distribution of Simulated Prices

This figure displays the distribution of simulated stock prices for $h_I^2/h_N < 1$ and $h_I^2/h_N > 1$, respectively. We simulate 200 prices using $a = 0.5$, $p = 0.5$, $g = 0.9$, $\eta = 0.5$, and $h_N = 0.2$. In Panel A, moreover, we set $h_I = 0.3$ so that $h_I^2/h_N = 0.3 < 1$, and short-selling bans reduce the probability of a left tail event. In Panel B, in contrast, we employ $h_I = 0.6$ so that $h_I^2/h_N = 1.2 > 1$, and short-selling bans increase the probability of a left tail event.

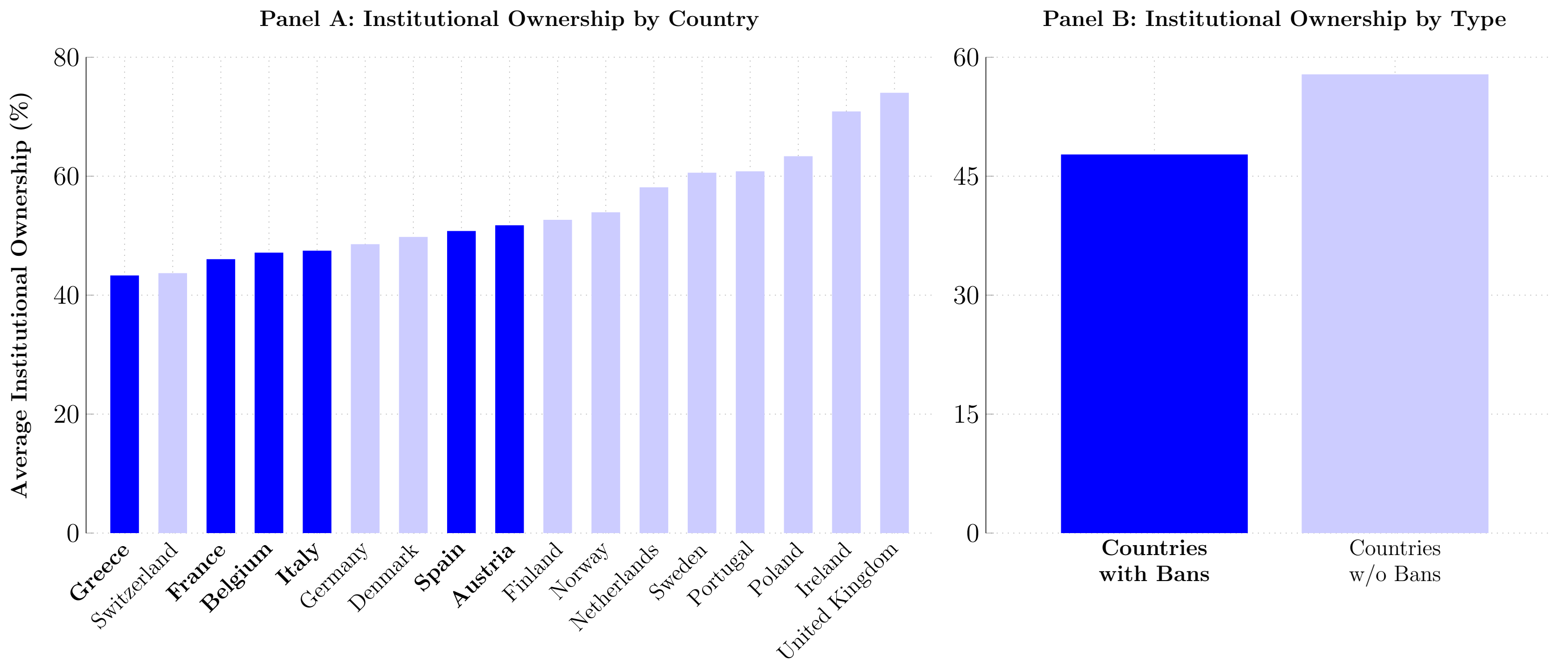

Figure 4. Average Institutional Ownership

This figure shows the average institutional ownership as of December 2019 for countries that restricted short sales (Austria, Belgium, France, Greece, Italy, and Spain) and countries that allow short sales (Denmark, Finland, Germany, Ireland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and the United Kingdom) during the Covid-19 pandemic. Panel A presents the average institutional ownership of all stocks in each country whereas Panel B displays the cross-country average, weighted by market capitalization, between countries with short-selling bans and countries without short-selling bans.

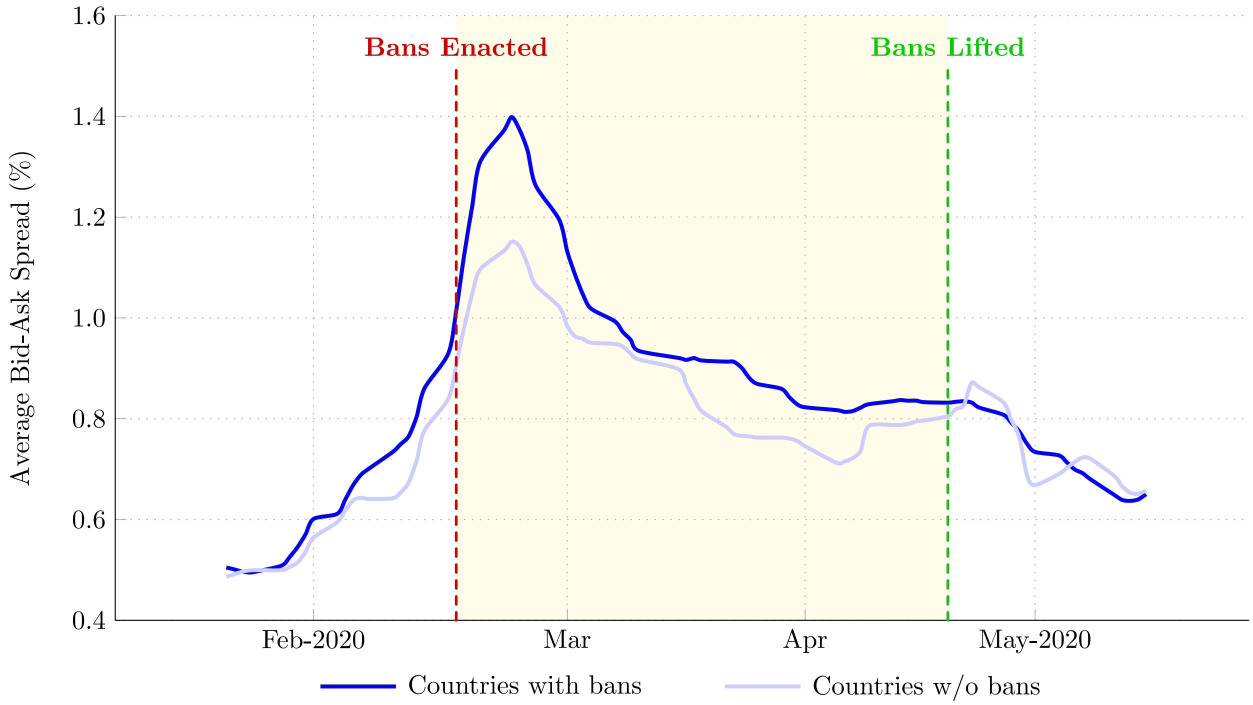

Figure 5. Bid-Ask Spreads and Short-Selling Bans

This figure shows the average percentage bid-ask spread of stocks in countries that introduced restrictions to short sales (Austria, Belgium, France, Greece, Italy, and Spain) and countries that allowed short sales (Denmark, Finland, Germany, Ireland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and the United Kingdom). The sample runs daily between February 2020 and June 2020. Data are collected from .....

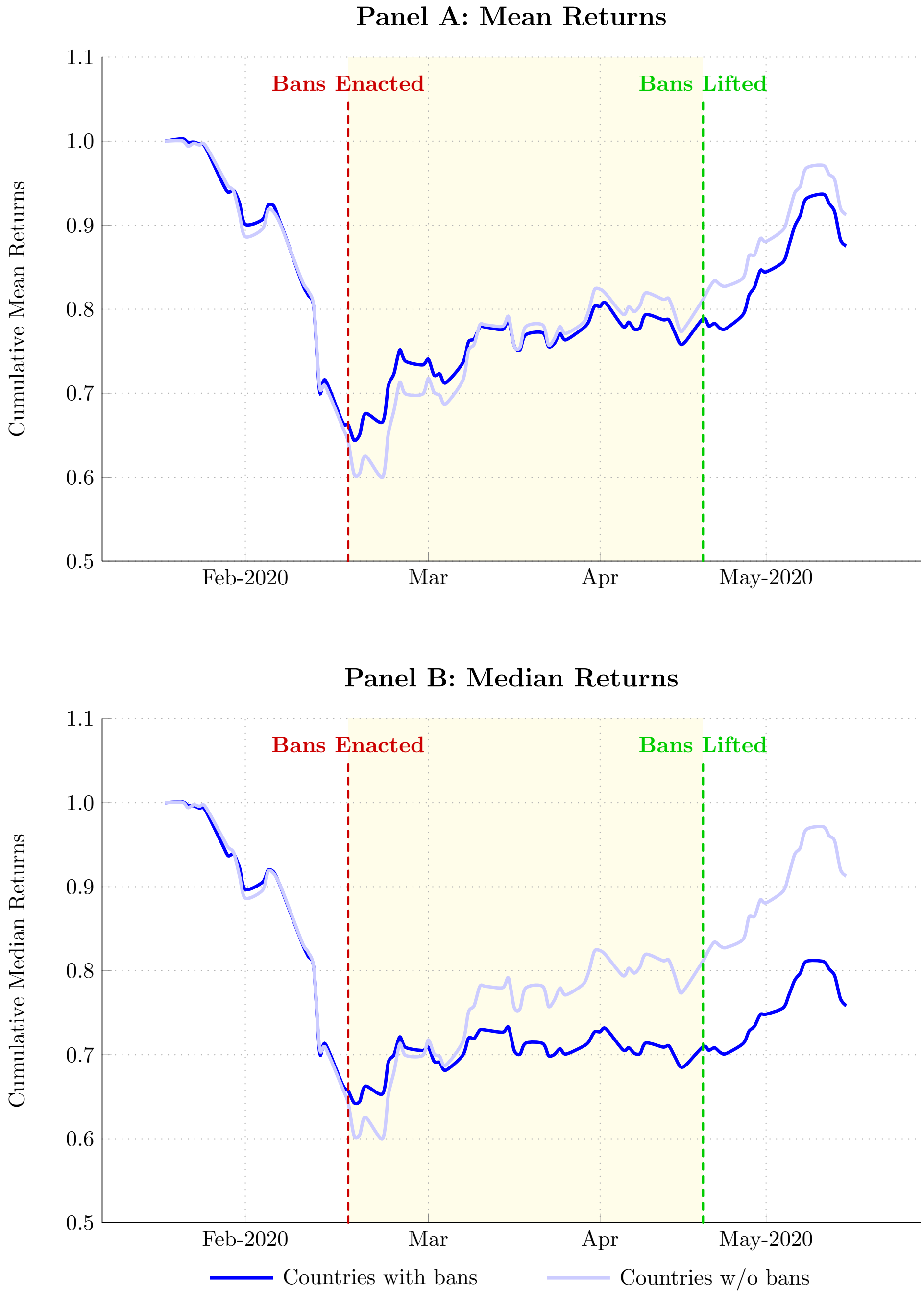

Figure 6. Cumulative returns and short-selling bans

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).

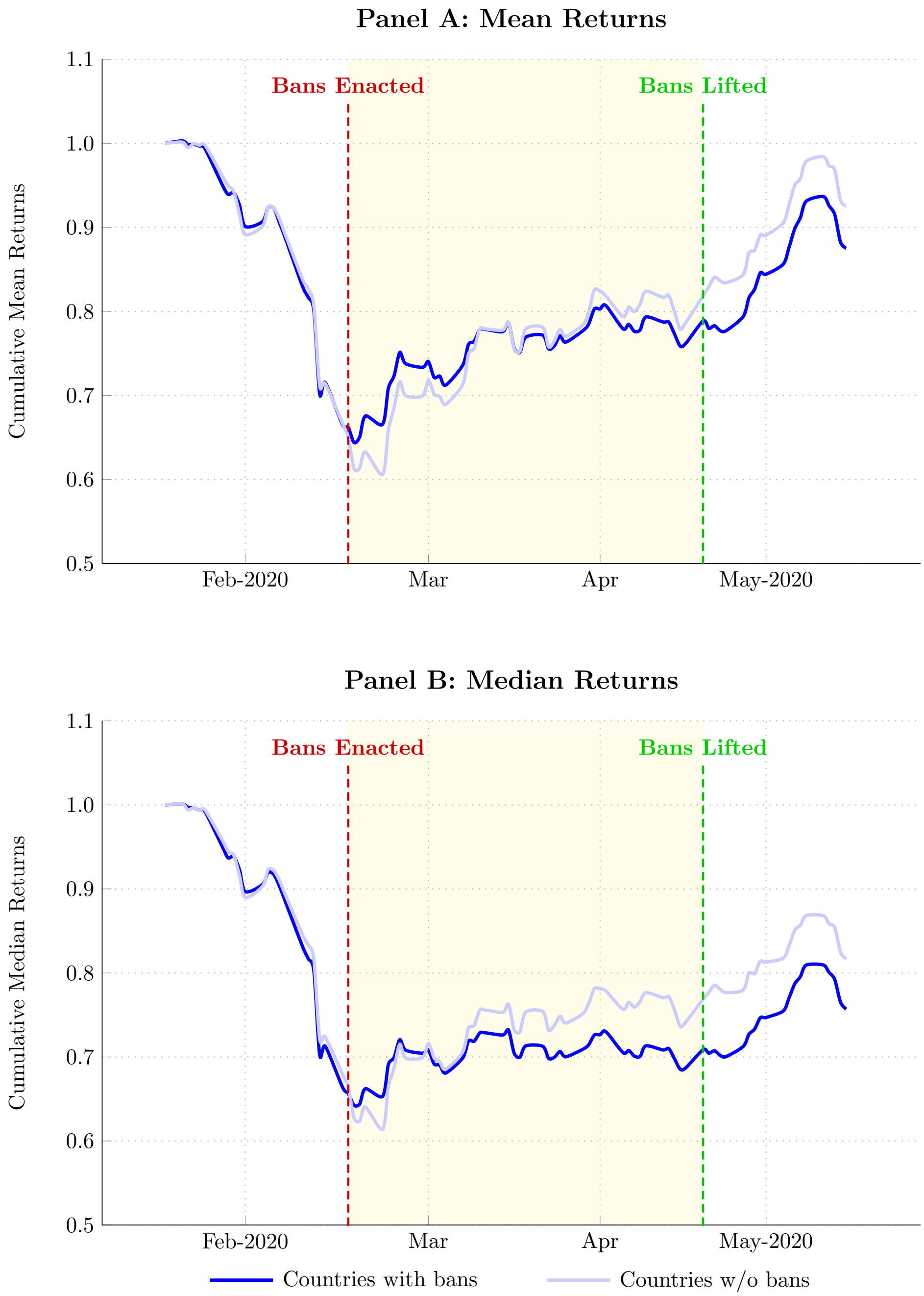

Figure 7. Cumulative returns and short-selling bans: Matched

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.

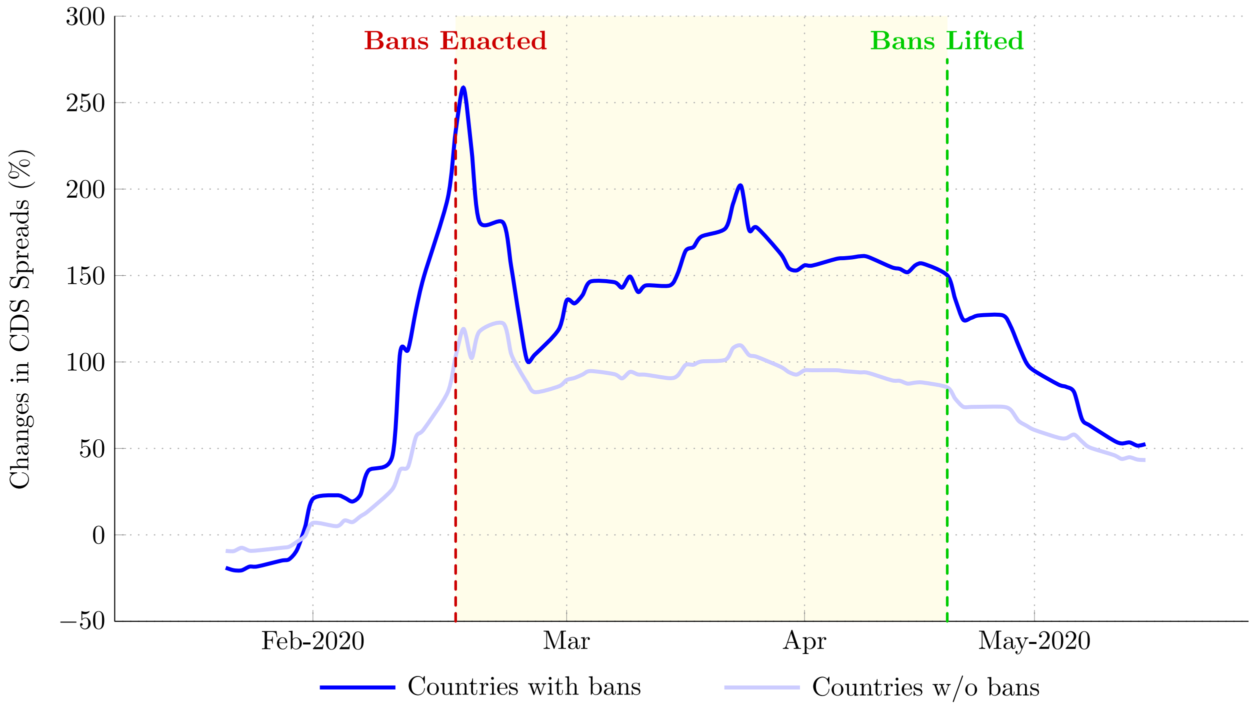

Figure 8. CDS Spreads and Short-Selling Bans

This figure shows the average percentage change of Sovereign CDS spreads in countries that introduced restrictions to short sales (Austria, Belgium, France, Greece, Italy, and Spain) and countries that allowed short sales (Denmark, Finland, Germany, Ireland, Netherlands, Norway, Poland, Portugal, Sweden, Switzerland, and the United Kingdom). For each country, we first calculate the percentage change relative to the average value recorded in December 2019, and then average across all countries with bans or countries without bans. The sample runs daily between February 2020 and June 2020. Data are collected from Datastream.

Table 1. Summary of the Notation

This table summarize the model's notation used throughout this paper and the online appendix.

Variable	**Definition**
V	Liquidating value of the stock, either zero or one.
p	Probability that V is equal to one.
$1-p$	Probability that V is equal to zero.
g	Probability that a trader wants to trade for either information or liquidity reasons. He will submit -if allowed- a buy order or a sell-or-short order of a single stock.
$1-g$	Probability that a trader does not actively participate in the market, so no trade is observed.
a	Probability that a given trader is *informed* or fraction of *informed* traders among those who actively participate in the market.
$1-a$	Probability that a given trader is *uninformed* or fraction of *uninformed* traders among those who actively participate in the market.
h_I	Probability that an *informed* trader already owns the stock.
h_N	Probability that an *uninformed* trader already owns the stock.
η_s	Probability that a *uninformed* trader submits a sell-or-short order, which is also known to the *market maker*.
$f(\eta_s)$	Probability distribution of η_s, which is known by the regulator.
q	Equilibrium price of the stock determined as the expectation of V conditional on the information set of the *market makers*.
c	A sufficiently low threshold for the stock price set by the *regulator*.
x	Confidence level set by the *regulator*.
$P(q < c)$	Probability that q falls below c, as perceived by the *regulator*.

Table 2. Data Description

This table presents a description of the stock market data by country. The sample runs daily between January 1, 2018, and June 12, 2020. We exclude micro and nano caps from our sample. Short-selling bans were introduced in Austria, Belgium, France, Greece, Italy, and Spain (highlighted in gray) on March 17, 2020. Data are collected from *Datastream*.

				Market Capitalization		
Country	#Days × Stocks	#Days	#Stock	#Small	#Mid	#Large
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	624	1,925	1,107	549	269

Table 3. Descriptive Statistics: Bid-Ask Spreads

This table presents median values of stock market percentage bid-ask spreads by country. A window of 30 calendar days around the ban enactment date on March 17, 2020 is used to compute medians 'Before Ban' and 'During Ban', respectively. The ban of short sales was enacted in Austria, Belgium, France, Greece, Italy, and Spain (highlighted in gray). The superscripts *, **, and *** indicate that the median bid-ask spread 'During Ban' is statistically different from the median bid-ask spread 'Before Ban' at the 10%, 5%, and 1% level, respectively, based on a Wilcoxon test. The sample includes the percentage bid-ask spreads quoted at the market close of small, mid, and large cap stocks and runs daily between February 16, 2020 and April 16, 2020. We exclude micro and nano caps from our sample. Data are collected from *Datastream*.

Country	Before Ban	During Ban	Ratio
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
Average	0.2957	0.6890***	1.6429

Table 4. Bid-Ask Spreads and Short-Selling Bans

This table presents difference-in-differences estimates associated with the introduction of short-selling bans in European stock markets on March 17, 2020 (see Table 2 for more details). The dependent variable is the percentage bid-ask spread, quoted on each trading day at the market close, for stocks traded in European venues. *Ban country* is a dummy variable that equals one (zero) for the treated (control) group of European countries. *Ban enactment* is a dummy variable that equals one (zero) for a post-treatment (pre-treatment) period of one month that goes from March 17, 2020 to April 16, 2020 (February 16, 2020 to March 16, 2020). Specifications are complemented with stock and time (calendar date) fixed effects by *fe*. Standard errors (in parentheses) are clustered at the stock and time (calendar date) level. *, **, ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. The sample includes small, mid, and large cap stocks (micro and nano caps are excluded), and runs daily between February 16, 2020 and April 16, 2020. Data are collected from *Datastream*.

	(1)	(2)	(3)
Ban Country × Ban Enactment	0.1174***	0.1308***	0.1309***
	(0.0385)	(0.0428)	(0.0430)
Ban Enactment	0.2386***	0.2384***	
	(0.0447)	(0.0461)	
Ban Country	0.1058**		
	(0.0507)		
Constant	0.6189***	0.6475***	0.7664***
	(0.0435)	(0.0418)	(0.0059)
R^2	0.0113	0.5626	0.5749
# Observations	79,607	79,606	79,606
Stock fe		✓	✓
Time fe			✓

Table 5. Bid-Ask Spreads, Short-Selling Bans, and Institutional Ownership

This table presents difference-in-differences estimates associated with the introduction of short-selling bans in European stock markets on March 17, 2020 (see Table 2 for more details). Panel A includes stocks with low institutional ownership (bottom tercile) whereas Panel B comprises stocks with high institutional ownership (top tercile). The dependent variable is the percentage bid-ask spread, quoted on each trading day at the market close, for stocks traded in European venues. *Ban country* is a dummy variable that equals one (zero) for the treated (control) group of European countries. *Ban enactment* is a dummy variable that equals one (zero) for a post-treatment (pre-treatment) period of one month that goes from March 17, 2020 to April 16, 2020 (February 16, 2020 to March 16, 2020). Specifications are complemented with stock and time (calendar date) fixed effects by *fe*. Standard errors (in parentheses) are clustered at the stock and time (calendar date) level. *, **, ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. The sample includes small, mid, and large cap stocks (micro and nano caps are excluded), and runs daily between February 16, 2020 and April 16, 2020. Data are collected from *Datastream* and *Bloomberg*.

	Panel A: Low Institutional Ownership			Panel B: High Institutional Ownership		
	(1)	(2)	(3)	(4)	(5)	(6)
Ban Country × Ban Enactment	0.0831	0.0831	0.0845	0.2098***	0.2699***	0.2699***
	(0.0510)	(0.0614)	(0.0620)	(0.0677)	(0.0807)	(0.0813)
Ban Enactment	0.2947***	0.2967***		0.2119***	0.2147***	
	(0.0713)	(0.0764)		(0.0450)	(0.0508)	
Ban Country	0.0570			0.1819		
	(0.0889)			(0.1243)		
Constant	0.8038***	0.8272***	0.9747***	0.6267***	0.6642***	0.7711***
	(0.0814)	(0.0658)	(0.0121)	(0.0655)	(0.0417)	(0.0099)
R^2	0.0106	0.4868	0.5094	0.0133	0.6303	0.6399
# Observations	20,987	20,987	20,987	19,723	19,723	19,723
Stock fe		✓	✓		✓	✓
Time fe			✓			✓

Table 6. Short-Selling Bans and Stock Return Distribution

This table presents difference-in-differences estimates associated with the introduction of short-selling bans in European stock markets on March 17, 2020 (see Table 2 for more details). The dependent variables are the daily percentage return statistics and the maximum drawdown of stocks traded in European venues. The statistics are computed using one-month windows around the enactment of short-selling bans, i.e., from February 16, 2020 to March 16, 2020 for the pre-treatment period, and from March 17, 2020 to April 16, 2020 for the post-treatment period. *Ban country* is a dummy variable that equals one (zero) for the treated (control) group of European countries. *Ban enactment* is a dummy variable that equals one (zero) for the post-treatment (pre-treatment) period. All specifications include stock fixed effects, and standard errors (in parentheses) are clustered at the stock level. *, **, ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. The sample includes small, mid, and large cap stocks (micro and nano caps are excluded), and runs daily between February 16, 2020 and April 16, 2020. Data are collected from *Datastream* and *Bloomberg*.

	Mean	Median	Volatility	Max drawdown
Ban Country × Ban Enactment	-0.0007	-0.0022***	-0.1982***	0.0276***
	(-1.03)	(-3.75)	(-11.95)	(4.91)
Ban Enactment	0.0263***	0.0160***	0.2377***	0.2146***
	(61.69)	(46.50)	(22.26)	(67.90)
Constant	-0.0197***	-0.0125***	0.7014***	-0.3615***
	(-114.92)	(-88.41)	(166.49)	(-276.18)
R^2	0.561	0.434	0.513	0.724
# Observations	3,844	3,844	3,844	3,844

Table 7. Short-Selling Bans and Stock Return Distribution: Matched

This table presents difference-in-differences estimates associated with the introduction of short-selling bans in European stock markets on March 17, 2020 (see Table 2 for more details). The dependent variables are the daily percentage return statistics and the maximum drawdown of stocks traded in European venues. The statistics are computed using one-month windows around the enactment of short-selling bans, i.e., from February 16, 2020 to March 16, 2020 for the pre-treatment period, and from March 17, 2020 to April 16, 2020 for the post-treatment period. The specification includes stock fixed effects. *Ban country* is a dummy variable that equals one (zero) for the treated (control) group of European countries. Stocks traded in the treated and control group of countries are matched on the basis of market capitalization and industry classification benchmark. *Ban enactment* is a dummy variable that equals one (zero) for the post-treatment (pre-treatment) period. Stocks are matched based on Standard errors (in parentheses) are clustered at the stock level. *, **, ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. The sample includes small, mid, and large cap stocks (micro and nano caps are excluded), and runs daily between February 16, 2020 and April 16, 2020. Data are collected from *Datastream* and *Bloomberg*.

	Mean	Median	Volatility	Max drawdown
Ban Country × Ban Enactment	0.0003	-0.0015*	-0.1949***	0.0335***
	(0.33)	(-1.81)	(-8.16)	(4.09)
Ban Enactment	0.0252***	0.0153***	0.2342***	0.2091***
	(31.17)	(22.66)	(11.63)	(31.06)
Constant	-0.0194***	-0.0122***	0.6978***	-0.3583****
	(-78.76)	(-58.27)	(116.92)	(-174.96)
R^2	0.622	0.479	0.531	0.741
# Observations	3,844	3,844	3,844	3,844

Table 8. Short-Selling Bans, Institutional Ownership and Maximum Drawdown

This table presents difference-in-differences estimates associated with the introduction of short-selling bans in European stock markets on March 17, 2020 (see Table 2 for more details). Panel A includes stocks with low institutional ownership (bottom tercile) whereas Panel B comprises stocks with high institutional ownership (top tercile). The dependent variables are the daily percentage return statistics and the maximum drawdown of stocks traded in European venues. The statistics are computed using one-month windows around the enactment of short-selling bans, i.e., from February 16, 2020 to March 16, 2020 for the pre-treatment period, and from March 17, 2020 to April 16, 2020 for the post-treatment period. The specification includes stock fixed effects. *Ban country* is a dummy variable that equals one (zero) for the treated (control) group of European countries. *Ban enactment* is a dummy variable that equals one (zero) for a post-treatment (pre-treatment) period of one month that goes from March 17, 2020 to April 16, 2020 (February 16, 2020 to March 16, 2020). Specifications are complemented with stock and time (calendar date) fixed effects by *fe*. Standard errors (in parentheses) are clustered at the stock and time (calendar date) level. *, **, ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. The sample includes small, mid, and large cap stocks (micro and nano caps are excluded), and runs daily between February 16, 2020 and April 16, 2020. Data are collected from *Datastream* and *Bloomberg*.

	Low Institutional Ownership	High Institutional Ownership
	Maximum Drawdown	Maximum Drawdown
Ban Country × Ban Enactment	0.0411***	0.0205*
	(4.21)	(1.82)
Ban Enactment	0.1996***	0.2141***
	(29.63)	(35.32)
Constant	-0.3327***	-0.3603***
	(-135.60)	(-140.86)
# Observations	1036	1038
R^2	0.713	0.706

Table 9. Short-Selling Bans and Bid-Ask Spreads (controlling for CDS spreads)

The dependent variable is the percentage bid-ask spreads quoted at the market close. The sample period is February 16 to April 16, 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 (zero) for a post-treatment (pre-treatment) period of one month that goes from March 17, 2020 to April 16, 2020 (February 16, 2020 to March 16, 2020). *CDS spread* is the daily CDS spread of the country in which a stock is traded. Column (2) introduces stock fixed effects, thereby eliminating the need for the dummy variable *Ban country* (which would be colinear), column (3) adds day fixed effects which eliminates the need for the calendar dummy *Ban enactment*, while column (4) adds the country-level CDS spreads as a control variable. 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)	(4)
Ban country	0.1058**			
	(2.18)			
Ban enactment	0.2386***	0.2384***		
	(14.25)	(14.23)		
Ban Country × Ban Enactment	0.1174***	0.1308***	0.1309***	0.1134***
	(4.08)	(4.59)	(4.59)	(3.56)
Constant	0.6189***	0.6475***	0.7664***	0.7382***
	(24.26)	(95.11)	(187.48)	(54.86)
CDS spread				0.0010**
				(2.03)
N	79,607	79,606	79,606	79,606
Stock FE	No	Yes	Yes	Yes
Day FE	No	No	Yes	Yes

Table 10. Short-Selling Bans and the Distribution of Stock Returns (controlling for CDS spreads)

This table presents difference-in-differences estimates associated with the introduction of short-selling bans in European stock markets on March 17, 2020 (see Table 2 for more details). The dependent variables are the daily percentage return statistics and the maximum drawdown of stocks traded in European venues. The statistics are computed using one-month windows around the enactment of short-selling bans, i.e., from February 16, 2020 to March 16, 2020 for the pre-treatment period, and from March 17, 2020 to April 16, 2020 for the post-treatment period. *Ban country* is a dummy variable that equals one (zero) for the treated (control) group of European countries. *Ban enactment* is a dummy variable that equals one (zero) for the post-treatment (pre-treatment) period. *CDS spread* is the daily CDS spread of the country in which a stock is traded. All specifications include stock fixed effects, and standard errors (in parentheses) are clustered at the stock level. *, **, ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. The sample includes small, mid, and large cap stocks (micro and nano caps are excluded), and runs daily between February 16, 2020 and April 16, 2020. Data are collected from *Datastream* and *Bloomberg*.

	Mean	Median	Volatility	Max drawdown
Ban Country × Ban Enactment	-0.0030***	-0.2403***	-0.0031***	0.0165**
	(-3.21)	(-10.08)	(-3.91)	(2.28)
Ban Enactment	0.0250***	0.2140***	0.0155***	0.2084***
	(49.14)	(19.97)	(36.35)	(52.98)
CDS spread	0.0001***	0.0025***	0.0001*	0.0006**
	(3.92)	(3.09)	(1.85)	(2.43)
Constant	-0.0229***	0.6427***	-0.0137***	-0.3769***
	(-26.71)	(31.11)	(-19.73)	(-57.45)
R^2	0.564	0.514	0.434	0.725
# Observations	3,844	3,844	3,844	3,844

Table 11. Short-Selling Bans and the Distribution of Stock Returns (60d window)

This table presents the results of several difference-in-differences regressions associated with the introduction of short-selling bans in European stock markets on March 17, 2020 (see Table 2 for more details). The dependent variables are the daily percentage return statistics and the maximum drawdown of stocks traded in European venues. The statistics are computed using 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). *Ban country* is a dummy variable that equals one (zero) for the treated (control) group of European countries. *Ban enactment* is a dummy variable that equals one (zero) for the post-treatment (pre-treatment) period. 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 (in parentheses) are clustered at the stock level. *, **, ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. The sample includes small, mid, and large cap stocks (micro and nano caps are excluded), and runs daily between February 16, 2020 and April 16, 2020. Data are collected from *Datastream* and *Bloomberg*.

Panel A: Full Sample				
	(1) Mean	(2) Median	(3) Vol	(4) Maximum drawdown
Ban Country × Ban Enactment	-0.0007*	-0.0007**	-0.1305***	0.0198***
	(-1.86)	(-2.28)	(-11.29)	(3.57)
Ban Enactment	0.0145***	0.0069***	0.1948***	0.2176***
	(60.91)	(38.27)	(26.12)	(68.93)
Constant	-0.0098***	-0.0046***	0.5585***	-0.3876***
	(-102.68)	(-61.66)	(189.96)	(-298.23)
# Observations	3,844	3,844	3,844	3,844
R^2	0.563	0.380	0.584	0.747
Panel B: Matched Sample				
	Mean	Median	Vol	Maximum drawdown
Ban Country × Ban Enactment	-0.0005	-0.0007	-0.1339***	0.0217***
	(-0.91)	(-1.60)	(-8.03)	(2.63)
Ban Enactment	0.0143***	0.0069***	0.1978***	0.2159***
	(31.64)	(19.18)	(14.04)	(31.33)
Constant	-0.0098***	-0.0045***	0.5494***	-0.3841***
	(-71.10)	(-40.52)	(131.82)	(-185.76)
# Observations	2,212	2,212	2,212	2,212
R^2	0.627	0.437	0.596	0.754

Internet Appendix to

"The Double-Edged Sword of the 2020 European Short-Selling Bans"

(not for publication)

Abstract

In the Internet Appendix, we present additional technical details and results not included in the main body of the paper.

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[14] 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 5. *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

[14] 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. $\square$

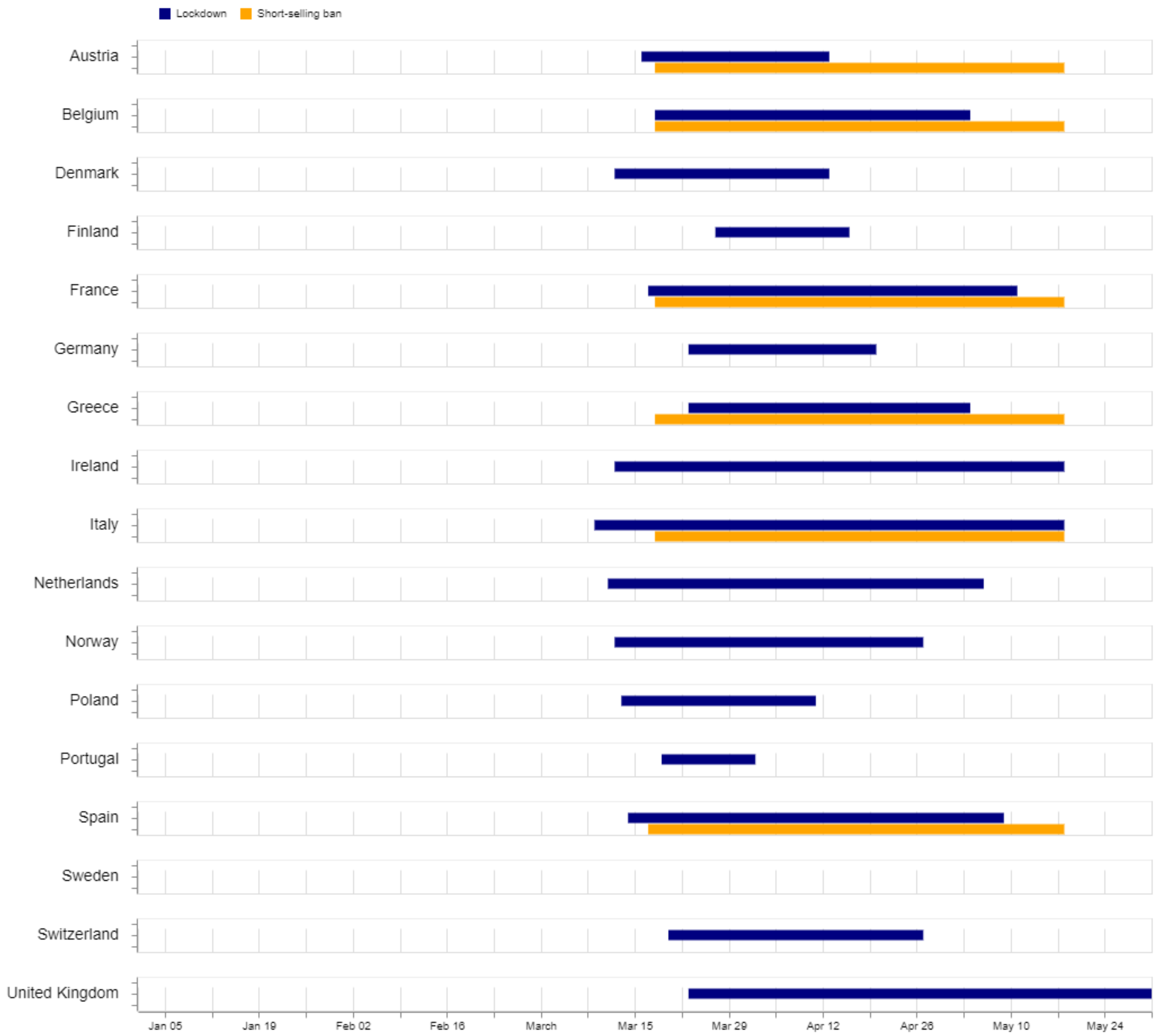

Figure A1. Short-Selling bans and Lockdown Measures in Europe

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.

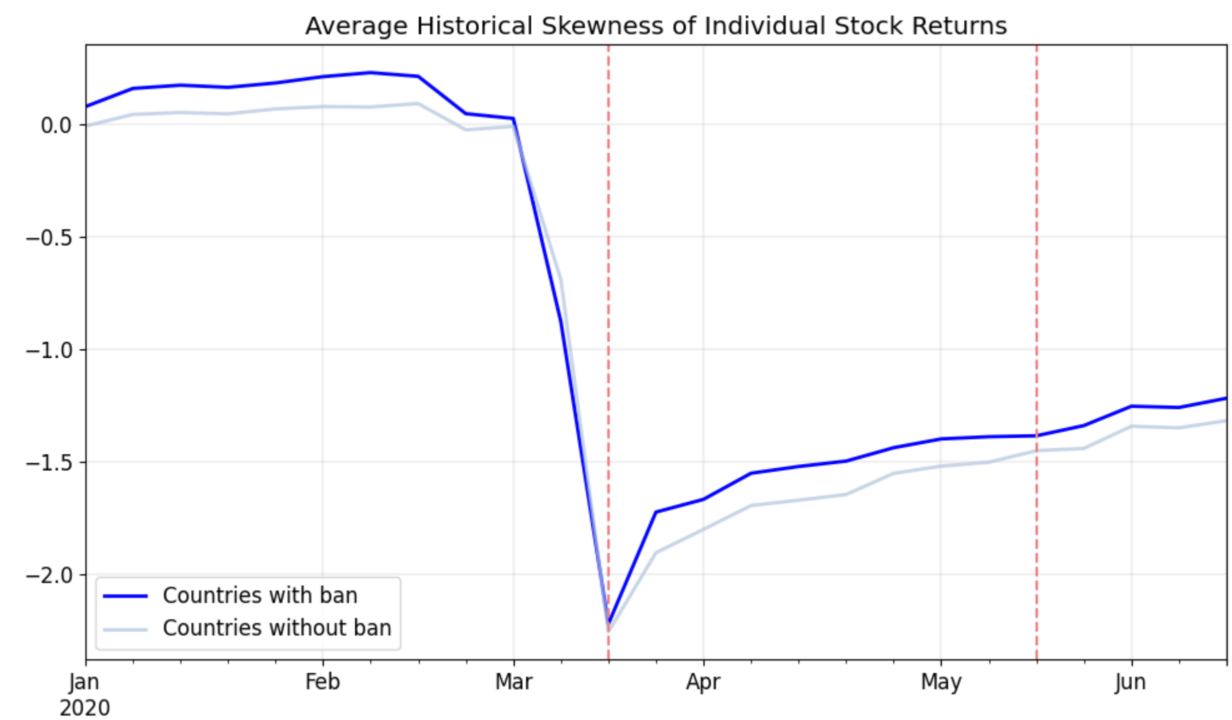

Figure A2. Average Historical Skewness of Stock Returns

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).