Journal of Empirical Finance 78 (2024) 101532

Contents lists available at ScienceDirect

Journal of Empirical Finance

journal homepage: www.elsevier.com/locate/jempfin

The 2008 short-selling ban's impact on tail risk

Jonas Bartl [a], Denefa Bostandzic [b], Felix Irresberger [c], Gregor Weiß [a,*], Ruomei Yang [d]

[a] *Universität Leipzig, Germany*
[b] *Universität Witten/Herdecke, Germany*
[c] *Durham University, United Kingdom*
[d] *University of Liverpool, United Kingdom*

ARTICLE INFO

JEL classification:
G14
G18

Keywords:
Short-selling ban
Tail risk
Financial stability
Financial crisis

ABSTRACT

We examine how the 2008 U.S. short-selling ban on the stocks of financial institutions impacted their equity tail risk. Using propensity score matching and difference-in-difference regressions, we show that the ban was not effective in restoring financial stability as measured by the stocks' dynamic Marginal Expected Shortfall. In contrast, especially large institutions, those who were most vulnerable to market downturns in the preban period, as well as those equities with associated put option contracts, experienced sharp increases in their exposure to market downturns during the ban period, contrary to regulators' intentions.

1. Introduction

There has been a sustained debate on the role that short-sellers play in the functioning of fair and orderly markets. While for the most part, they are considered to contribute to efficient stock pricing and liquidity provision, during the Great Financial Crisis most exchange regulators restricted short-selling when stock prices dropped sharply. So too did the SEC in September 2008, banning most short sales for over 700 financial stocks, in order to "prevent substantial disruption in the securities markets".[1] Several studies (see, e.g., Boehmer et al., 2013; Beber and Pagano, 2013) have by now shown that short-selling bans during the financial crisis coincided with the collateral damage of a severe liquidity dryup in affected markets. The experience made during the crisis even led regulators to outright question the suitability of short-selling bans in general for preventing market crashes. However, while research has already addressed the unintended (detrimental) side-effects of short-selling bans, to the best of our knowledge no work has so far looked at the intended effects of these bans on stocks' tail risk. We fill this clear gap in the literature by studying the impact of short-selling bans on financial institutions' tail risk. To measure a stock's propensity to crash together with the market, we rely on the MES measure of Acharya et al. (2017) who define the MES of a stock as its negative mean return, conditional on the stock market experiencing its 5% lowest return days. In order to estimate a firm's daily MES, we follow Brownlees and Engle (2011), who propose a dynamic approach accounting for time-varying correlation and volatility.

We examine how the short-selling ban for financial institutions that was implemented in the US during the financial crisis affected stock markets. To establish causality, we run daily panel regressions with stock-level and time fixed effects, comparing banned stocks to similar nonbanned stocks. After matching each stock subject to the ban to a nonbanned stock that resembles it most based on firm

* Correspondence to: Universität Leipzig, Wirtschaftswissenschaftliche Fakultät, Chair in Sustainable Finance and Banking, Grimmaische Str. 12, 04109 Leipzig, Germany.

E-mail addresses: jonas.bartl@online.de (J. Bartl), denefa.bostandzic@uni-wh.de (D. Bostandzic), felix.irresberger@durham.ac.uk (F. Irresberger), weiss@wifa.uni-leipzig.de (G. Weiß), Ruomei.Yang@liverpool.ac.uk (R. Yang).

[1] SEC's Emergency Order, Release No. 34-58592, published September 18, 2008.

https://doi.org/10.1016/j.jempfin.2024.101532
Received 5 December 2023; Received in revised form 13 June 2024; Accepted 10 August 2024
Available online 20 August 2024
0927-5398/

size, leverage, and the market-to-book-ratio, we estimate daily panel regressions of the stocks' MES on DID interaction terms and analyze the question whether the bans causally and significantly reduced the stocks' propensity to crash together with the market.

As our main result, we find that the ban was not effective in reducing the crash risk of affected stocks. We find that the ban did not reduce the affected firms' exposure to market downturns as measured by MES. On the contrary, the MES of stocks subject to the ban increased during the ban period by up to 0.65% which corresponds to a rise of approximately 17% compared to the mean MES before the ban. This change is driven by the largest firms in our sample, while firms with total assets below the sample median can moderately profit from reductions in their exposure to market tail risk. Furthermore, we find that only those firms who had a low exposure to market downturns in the preban period experienced a reduction in their equity tail risk exposure, whereas the firms with the highest MES before the treatment period had to endure an additional increase caused by the short-selling ban. This supports our view that short-selling ban significantly impaired the resilience and stability of the banned stocks. One of the reasons some of the banned stocks in our sample were able to experience such tail risk exposure may have been that investors can use option trading to circumvent the ban on short-selling activities and thus, may put downward pressure on the underlying equities. In additional tests, we divide our sample into those stocks that have associated option contracts versus those that do not, and find that those stocks with optionability have a high relative increase in MES during the ban period. Their no-option counterparts, on the other hand, have reduced MES values, indicating that the ban reduced tail risk exposure as intended, but only for stocks without the option to circumvent the short-selling ban.

To rule out the possibility that our MES results are merely driven by price reversals that are the consequences of overvalued, short-selling constraint stocks with subsequent underperformance (cf. Miller, 1977; Jones and Lamont, 2002; Boehme et al., 2006), we add regression analyses using simple stock returns as our dependent variable. However, we do not find the overvaluation and price reversals to be the case, as none of the estimations show that stock returns significantly differ for banned versus nonbanned stocks during the ban period.

When zooming in on only the final few days of the ban period, and the weeks after the ban was lifted, we do find some limited evidence that the lift of the ban had the intended effect of reducing equity tail risk exposure, as some of the banned firms in our sample experience a slight relative increase in their MES following the lift of the ban (relative to the final week of the ban period). This is consistent with the notion that the ban might have worked for some stocks towards the end of the ban period, but then increased sharply relative to control group stocks following the lift of the ban. Nevertheless, such finding does not generalize to the full ban period and not to all stocks in our sample.

Our paper is related to several influential studies on the effects of short-selling bans, with several of these looking in particular at the (usually unintended) effects of short-selling bans on stock markets during the last financial crisis (see, e.g., Battalio and Schultz, 2011; Marsh and Payne, 2012; Boehmer et al., 2013; Beber and Pagano, 2013). In summary, these works find that short-selling bans reduce price discovery speed and trading volumes, increase volatility, and damage liquidity while not affecting stock prices. Thereby rational expectations models are supported that state that market participants anticipate short-selling bans (see, e.g., Diamond and Verrecchia 1987). In contrast, only Lecce et al. (2012) and Autore et al. (2011) find empirical evidence that supports the overpricing hypothesis as formulated by Miller (1977), implying a jump in the banned stocks' prices at the beginning of the ban and negative abnormal returns after the lift of the ban.

Closer to our analysis is the work by Beber et al. (2021), who examine, inter alia, whether shorting bans helped the most vulnerable banks during the European sovereign debt crisis. More precisely, they study the effects of short-selling bans on the distressed banks' stock returns, return volatilities, and default probabilities. They define vulnerable banks as entities with leverage, SRISK, the negative of the tier-1 capital ratio, or the negative of the stable funding ratio above the sample median.[2] They find that the short-selling bans led to drastic increases in the default probabilities of the most vulnerable banks. Our work, in comparison, is less concerned with the *systemic* risk of financial institutions (which requires difficult assumptions on the indirect linkages between a bank's stock, short-selling bans, leverage, capital ratio, and ultimately its default probability), but more with the outcome variable regulators are directly aiming at with their intervention: the crash propensity of financial stocks together with the market.

Furthermore, Félix et al. (2016) examine short-selling bans' effects on systemic risk, too, by taking an options market perspective. Analyzing implied volatilities and risk neutral densities during the European sovereign debt crisis 2011, they find a trade-off between systemic risk and jump risk. On the one hand, the ban significantly reduced financial contagion risk for the banned stocks, as implied by average conditional co-crash probabilities, while on the other hand it increased their jump risk which the authors interpret as an indicator for market failure.

Building on the empirical literature discussed above, we hypothesize that short-selling bans should increase rather than decrease equity tail risk exposure by reducing the liquidity of the banned stocks while leaving stock prices unaffected. However, theoretical work suggests that under certain conditions short-selling can cause drastic price declines without a fundamental underlying basis thus justifying short-selling bans. For example, Brunnermeier and Oehmke (2014) state that in turbulent times, weaker capitalized financial institutions may become the target of "predatory" short-selling, which they define as the attempt to induce downward spirals in a self-fulfilling manner for fundamentally healthy firms. Caused by aggressive shorting, a financial institution may be confronted with capital withdraws forcing it to liquidate long-term assets at fire-sale prices in order to comply with its leverage constraints.

Describing a different mechanism, Liu (2015) reaches a similar conclusion that short-selling, in fact, can cause sharp price declines and thereby increase tail risk without a fundamental justification. The author argues that, especially for less liquid stocks,

[2] SRISK refers to the systemic risk measure proposed by Acharya et al. (2012), Brownlees and Engle (2017) and is defined as "the expected capital shortfall of a financial entity conditional on a prolonged market decline" (Acharya et al., 2012; Brownlees and Engle, 2017).

short-selling activity can reduce price informativeness which increases uncertainty about the firm's fundamentals, giving short-term creditors an incentive to run the bank due to their concave payoff. By imposing negative externalities to other banks, the fall of one bank (stock) may cause serious troubles even to healthy banks, initiating further downward spirals, and thereby directly increasing tail risk.

Accordingly, both models suggest that a ban on shorting could reduce tail risk, especially during tumults in the financial markets. Surprisingly, to the best of our knowledge, up to this date there exists no study that looks directly at a shorting ban's influence on equity tail risk, even though limiting the latter should be a regulator's main justification for issuing a ban on short-sales. In fact, the SEC pointed these concerns out itself by arguing in it's press releases that short-selling could lead to sudden declines in stock prices, which then in turn could raise questions about the financial condition of the underlying institution, causing a crises of confidence "with potentially broad market consequences". We try to fill this gap in the literature by explicitly studying the relation between shorting bans and equity tail risk empirically.

The rest of the paper is structured as follows: in Section 2, we describe our data and discuss our dependent and independent variables of interest. In Section 3, we present and discuss our empirical analysis. Section 4 concludes.

2. Data and variables

2.1. Sample construction

We start our sample construction by retrieving equities data from CRSP with SIC codes affected by the SEC ban (Release No. 34-58592/September 18, 2008).[3] We retain all primary listings (A-shares), i.e., remove B, C, E, L class shares. For each ticker symbol, we manually check double occurrences (due to name changes) and combine price time series for those with the same PERMNO code. We keep the name and ticker symbol that belongs to the longer time period covered in our sample period. If there are two equities with the same PERMNO code but different ticker symbols (and company names), we manually check whether this is due to a ticker symbol (company name) change and provide data for the whole sample period. We assign the ticker symbol that corresponds to the equity listed on the SEC short-sale ban list in the first instance, and if the equity is not on that list, we assign both the ticker symbol and name that is given for the majority of the sample period. To evaluate the effect of the event on equity tail risk, we require price data to be available around the event date, i.e., at least one observations before and after the 17th September 2008. The final sample with available equity data includes 934 companies. The SEC ban list includes 795 unique equity ticker symbols, which we match to our sample of financial firm equities. Other equities in our initial sample are used as control sample. Our final restriction is that financial accounting data on total assets, leverage, and market-to-book ratios are available at the end of 2007, which leaves us with 629 of the 934 equities (549 banned; 82 control). For additional tests, we cross-check the availability of option data in June 2008 to June 2009 in OptionMetrics. We find listed put options for 247 underlying equities (ticker symbols); 174 correspond to treated and 73 to control group equities.

2.2. Marginal expected shortfall

To examine the effect of the short-selling ban on the tail risk of financial companies, we compute for each firm the daily Marginal Expected Shortfall (MES) as proposed by Brownlees and Engle (2011). MES builds on Acharya et al. (2017) and is defined as a bank's negative mean net equity conditional on the stock market experiencing its 5% lowest return days. As market index, we choose the value-weighted CRSP index (comprising NYSE/NASDAQ/AMEX/ARCA) stocks. The choice of the MES as a measure of tail risk is straightforward: regulators imposed the ban aiming to take away pressure from financial stocks during the turmoils of the financial crisis. The MES captures exactly a stock's exposure to distress in stock markets. In addition, the MES is theoretically well-founded and has been discussed extensively.[4]

We follow Brownlees and Engle (2011) and model firm log returns $R_{j,t}$ and market log returns $R_{M,t}$ as daily bivariate process denoted by

$$\begin{aligned} R_{M,t} &= \sigma_{M,t}\epsilon_{M,t} \\ R_{j,t} &= \sigma_{j,t}\rho_{j,t}\epsilon_{M,t} + \sigma_{j,t}\sqrt{1-(\rho_{j,t})^2}\epsilon_{j,t} \\ (\epsilon_{M,t},\epsilon_{j,t}) &\sim H, \end{aligned}$$

where $\sigma_{i,t}$ is the conditional volatility of the market return ($i = M$) or firm j's return ($i = j$), $\rho_{j,t}$ is the conditional market/firm correlation and $\left(\epsilon_{M,t},\epsilon_{j,t}\right)$ are i.i.d. innovations with $\mathbb{E}\left(\epsilon_{i,t}\right) = 0$, $Var\left(\epsilon_{i,t}\right) = 1$ and $i = \{j, M\}$ and zero covariance. The MES for a systemic event S is given by

$$\begin{aligned} MES_{j,t} &= -\mathbb{E}_{t-1}\left(R_{j,t} \mid R_{M,t} < S\right) \\ &= -\sigma_{j,t}\mathbb{E}_{t-1}\left(\rho_{j,t}\epsilon_{M,t} + \sqrt{1-(\rho_{j,t})^2}\epsilon_{j,t} \mid \epsilon_{M,t} < S/\sigma_{M,t}\right) \end{aligned}$$

[3] SIC codes: 6000, 6011, 6020-22, 6025, 6030, 6035-36, 6111, 6140, 6144, 6200, 6210-11, 6231, 6282, 6305, 6310-11, 6320-21, 6324, 6330-31, 6350-51, 6360-61, 6712, and 6719. For the most part, these codes include banks, insurers, real estate and other financial services companies.

[4] For a comprehensive discussion, see, e.g., Benoit et al. (2013, 2017).

$$= -\left[\sigma_{j,t}\rho_{j,t}\mathbb{E}_{t-1}\left(\epsilon_{M,t} \mid \epsilon_{M,t} < S/\sigma_{M,t}\right) + \sigma_{j,t}\sqrt{1-(\rho_{j,t})^2}\mathbb{E}_{t-1}\left(\epsilon_{j,t} \mid \epsilon_{M,t} < S/\sigma_{M,t}\right)\right],$$

and the probability of a systemic event, i.e., where market returns are below a threshold S, is denoted by $\mathbb{P}_{t-1}(R_{M,t} \le S) = P_{t-1}(\epsilon_{M,t} \le S/\sigma_{M,t})$. That is, MES is combination of conditional volatilities and correlations as well as tail expectations of the innovations.

To estimate MES, we follow Brownlees and Engle (2011, 2017) and first model variance dynamics using TGARCH (Rabemananjara and Zakoian, 1993), to account for an asymmetric response in volatility to negative returns (leverage effects), and then employ a standard (symmetric) Dynamic Conditional Correlation (DCC) (Engle, 2002) specification for time-varying correlations between firm and market log returns. The MES computation is then completed by nonparametrically estimating the tail expectations of innovations.

First, variance dynamics are specified as follows:

$$\begin{aligned}\sigma^2_{M,t} &= \omega_M + \alpha_M R^2_{M,t-1} + \beta_M \sigma^2_{M,t-1} + \gamma_M R^2_{M,t-1}\mathbb{1}\{R_{M,t-1} < 0\}\\ \sigma^2_{j,t} &= \omega_j + \alpha_j R^2_{j,t-1} + \beta_j \sigma^2_{j,t-1} + \gamma_j R^2_{j,t-1}\mathbb{1}\{R_{j,t-1} < 0\},\end{aligned}$$

where $\mathbb{1}\{R_{i,t-1} < 0\}, i = \{j, M\}$ is an indicator function to add volatility in response to negative return days (leverage effects); $\omega_i, \alpha_i, \beta_i, \gamma_i$ are scalar parameters.

Second, in the bivariate symmetric DCC model, correlations ρ_t of returns are expressed via pseudo-correlations

$$Q_t = \begin{bmatrix} Q_{jj,t} & Q_{jM,t} \\ Q_{jM,t} & Q_{MM,t} \end{bmatrix},$$

which are modeled in the following way (cf. Engle, 2002; Supper et al., 2020):

$$\rho_t = \frac{Q_{jM,t}}{\sqrt{Q_{jj,t}Q_{MM,t}}} \quad , \quad Q_t = (1-\psi-\phi)\Omega + \psi\epsilon^*_{t-1}{\epsilon^*_{t-1}}' + \phi Q_{t-1},$$

where Ω is a positive definite two by two constant correlation matrix and ψ, ϕ are scalars with $\psi > 0, \phi > 0, \psi + \phi < 1$ such that Q_t is positive definite; $\epsilon^*_t = (\epsilon^*_{j,t}, \epsilon^*_{M,t})'$, with $\epsilon^*_{i,t} = \epsilon_{i,t}\sqrt{Q_{ii,t}}$, are rescaled standardized returns (after fitting TGARCH models) (cf. Aielli, 2013). The joint distribution H of innovations is left unspecified in Brownlees and Engle (2011) as the tail expectations in MES are estimated nonparametrically, but for maximum likelihood estimation of parameters in the DCC model, we assume that H is a bivariate normal distribution.[5]

We obtain conditional volatility estimates $\hat{\sigma}_{i,t}$ and shocks $\hat{\epsilon}_{i,t}$, which are then used to estimate the tail expectations $\mathbb{E}_{t-1}\left(\epsilon_{M,t} \mid \epsilon_{M,t} < S/\sigma_{M,t}\right)$ and $\mathbb{E}_{t-1}\left(\epsilon_{j,t} \mid \epsilon_{M,t} < S/\sigma_{M,t}\right)$, by taking averages of the innovations $\hat{\epsilon}_{i,t}$ when $\hat{\epsilon}_{M,t} < S/\hat{\sigma}_{M,t}$. As in Brownlees and Engle (2011), we use a nonparametric kernel density function approach to improve estimation of the tail expectations.[6]

2.3. Control variables

In our analysis, we employ several control variables that reflect a firm's idiosyncratic features relevant to equity tail risk exposure. First, we consider a firm's size as the natural logarithm of its total assets. On the one hand, following Diamond (1984), large financial institutions may gain competitive advantages by possibly achieving a monopolistic position and thereby increasing their profits. Moreover, large institutions may also be able to diversify more efficiently. Both may lead to higher capital buffers and reduce a firm's vulnerability to macroeconomic or liquidity shocks, hence reduce their equity risk exposure (see Boyd et al., 2004; Matutes and Vives, 2000).[7] On the other hand, Beck et al. (2006) argue that big banks are more interconnected and characterized by a higher organizational complexity than their smaller competitors which may lead to a higher equity tail risk exposure. Furthermore, if an institution is considered as too big to fail, its management may be prone to excessive risk taking (see Stern and Feldman, 2004). Hovakimian et al. (2012) confirm this positive relation empirically for US banks during the Great Financial Crisis. Therefore, we expect the sign of the total assets to be unrestricted in our regressions.

As a second control variable, we take into account an institution's leverage, defined as the fraction of total assets plus the market values of assets minus common equity and the market values of assets (see Acharya et al., 2017). Fahlenbach et al. (2012) and Beltratti and Stulz (2012) find that highly leveraged banks performed worse during the Great Financial Crisis so that they may be more vulnerable to market downturns. Furthermore, Fahlenbach et al. (2012), Hovakimian et al. (2012), and Brunnermeier et al. (2020) find that banks with higher leverage contribute more to systemic risk. Also, Acharya et al. (2017) emphasize that the MES should be examined in conjunction with leverage, because the latter determines how grave the consequences are that result

---

[5] As a robustness check, we consider alternative specifications for variances and conditional correlations and the joint distribution assumed to estimate parameters of the DCC model. First, we model univariate innovations using skewed t distributions to account for fat tails in market and individual stock returns, respectively (see, e.g., Lucas et al., 2014). Second, we then assume H to be a symmetric multivariate t distribution for the estimation of DCC parameters via maximum likelihood. Third, for the correlation dynamics, we consider as alternative specification an asymmetric DCC model (cf. Cappiello et al., 2006) to capture asymmetric responses in conditional correlations (e.g., leverage effects). Overall, these alternative specifications to estimate the components for the MES calculation do not alter our main findings later on.

[6] Let $K_h(t) = \int_{-\infty}^{t/h} k(u)\,du$, with Gaussian kernel function $k(u) = \frac{1}{\sqrt{2\pi}}\exp(-\frac{1}{2}u^2)$ and positive bandwidth h. Then $\hat{\mathbb{E}}_h(\epsilon_{M,t}|\epsilon_{M,T} < \kappa) = \frac{1}{n\hat{p}_h}\sum_{i=1}^n \epsilon_{M,t}K_h(\epsilon_{M,T} - \kappa)$ and $\hat{\mathbb{E}}_h(\epsilon_{j,t}|\epsilon_{M,T} < \kappa) = \frac{1}{n\hat{p}_h}\sum_{i=1}^n \epsilon_{j,t}K_h(\epsilon_{M,t} - \kappa)$ with $\hat{p}_h = \frac{1}{n}\sum_{i=1}^n K_h(\epsilon_{M,t} - \kappa)$.

[7] Contrasting this argument, Demsetz and Strahan (1997) find that large banks are better diversified but also pursue riskier so that this advantage does not translate into risk reductions.

Table 1
Summary statistics.

Variable	Mean	Median	SD	Min	Max	5%-Quantile	95%-Quantile
Banned							
MES	0.0590	0.0520	0.0942	−0.2684	0.4604	−0.0729	0.2159
Total assets	35,742,050	1,370,683	225,731,200	1,485	3,643,585,000	193,000	69,857,000
Leverage	15.0644	9.2459	30.5072	1.0118	751.6813	1.9251	41.9003
MTBR	1.2207	1.0401	2.1933	−123.3097	49.1619	0.3019	2.5228
Control							
MES	0.0730	0.0572	0.0709	−0.2644	0.4604	−0.0079	0.2027
Total assets	20,446,280	1,999,118	68,892,660	17,779	563,077,000	99,010	65,893,430
Leverage	9.4732	3.5501	21.6493	1.0000	206.3031	1.0762	36.2038
MTBR	1.6670	1.1827	1.8452	−4.8352	12.8781	0.4337	5.1496

This table presents summary statistics for banned and nonbanned (control) stocks for a time window of 200 days before and 200 days after the ban lift. *MES* is the daily Marginal Expected Shortfall as introduced by Brownlees and Engle (2011), *MTBR* is the market-to-book ratio of a firm's equity. Total assets are given in thousands of US dollars. *SD* refers to the standard deviation, *Min* and *Max* indicate the minimum and maximum, respectively.

from the expected capital losses which the MES captures. Then again, DeAngelo and Stulz (2015) present a model stating that under idealized conditions leverage does not increase equity tail risk because banks can construct perfectly safe assets and liability structures. Furthermore, Grossman and Hart (1982) state that a higher leverage may decrease the firm managers' risk taking because it imposes a threat of bankruptcy which is associated with negative personal outcomes for the managers themselves (e.g., salary losses, reputation risks etc.). Berger and Di Patti (2006) confirm this empirically. As the overall impact of leverage on equity (tail) risk remains unclear, we do not expect a specified sign for this variable prior to our regression analysis.

Finally, we also control for a firm's market-to-book-ratio (MTBR). Again, literature shows two possible channels of impact: On the one hand, a higher charter value may reduce risk-taking because the bank "has something to lose" (Demsetz et al., 1996) and the charter value cannot be sold in the case of a bankruptcy (Keeley, 1990). On the other hand a higher MTBR may be the result of excessive risk-taking aiming to form a "glamor bank". Consequently Brunnermeier et al. (2020) find that banks with high MTBR contribute more to systemic risk. To minimize the effect of outliers on our results, all financial accounting variables are winsorized at the 1% and the 99% quantile.

We provide univariate statistics in Table 1, which emphasize meaningful differences between banned and nonbanned stocks. While the banned stocks have a mean dynamic MES of 5.9%, the average MES of the control stocks is only 7.3%. Thus, on average, banned stocks were less exposed to tail events in the market than nonbanned stocks. Moreover, firms affected by the ban were on average more than 1.5 times larger, but had a lower median size, indicating that the banned sample contains a few large firms as outliers. Banned stocks also have higher leverage statistics, but overall lower MTBRs, indicating lower charter value than stocks in the nonbanned sample. As a first step towards our regression analysis, we want to first explore the envelopment of the MES during the investigation period. Looking at the upper panel of Fig. 1, we see that the daily mean MES values in both groups follow a parallel trend, with the banned stocks showing an average value that is around 2%–3% lower than that of control group stocks during the preban period. Around the enactment of the ban, marked by the vertical line (a), both groups experience an increase in their MES. However, the increase in MES is steeper for banned stocks so that averages are roughly at the same levels during the ban period. Towards the end of the ban period, and subsequently in the postban period, MES values diverge again, with nonbanned stocks having higher MES values than banned stocks, and overall having higher equity tail risk than before the short-selling ban. The lower panel plots only the months of September and October of the two time series of mean MES values, allowing us to investigate the surroundings of the ban more in detail. First, we observe that after Lehman Brothers' bankruptcy on September 15th 2008, marked by the vertical line (c), the mean MES in both groups increases sharply. Furthermore, after the enactment of the ban the difference of the mean MES values between both groups grows considerably. At a first glance, this suggests that regulators achieved the opposite effect of what they actually aimed at, that is, compared to the control group the stocks affected by the ban got under even more downward pressure. According to our graphical inference, it seems that the turmoils in the financial markets caused by the Lehman Brothers bankruptcy triggered, on average, higher MES values for all financial institutions' stocks. It further appears that the ban did not succeed in reducing the banned stocks MES values throughout the entire ban period. To assess the statistical and economic relevance of these findings, we proceed with our difference-in-differences analysis that can shed light on the differential increases in MES of banned versus nonbanned stocks during the ban period.

3. Empirical results

To examine the effect of the short-selling ban on a firm's exposure to instability in financial markets, we estimate difference-in-differences (DID) regressions of the following type:

$$MES_{it} = \alpha_i + \beta_t + \gamma_1 \cdot D_i^{Banned} + \gamma_2 \cdot D_t^{BanPeriod} + \gamma_3 \cdot D_i^{Banned} \cdot D_t^{BanPeriod} + \gamma_4 \cdot F_{it} + \varepsilon_{it}, \tag{1}$$

where MES_{it} is the MES of financial institution i on day t. In all regressions, we include stock-level fixed effects α_i and day-fixed effects β_t that absorb unobserved heterogeneity. D_i^{Banned} is a dummy that equals one if a stock is on the SEC short-sale ban list and zero otherwise. The dummy $D_t^{BanPeriod}$ is equal to one on all days during the ban period (September 19th to October 7th 2008) and

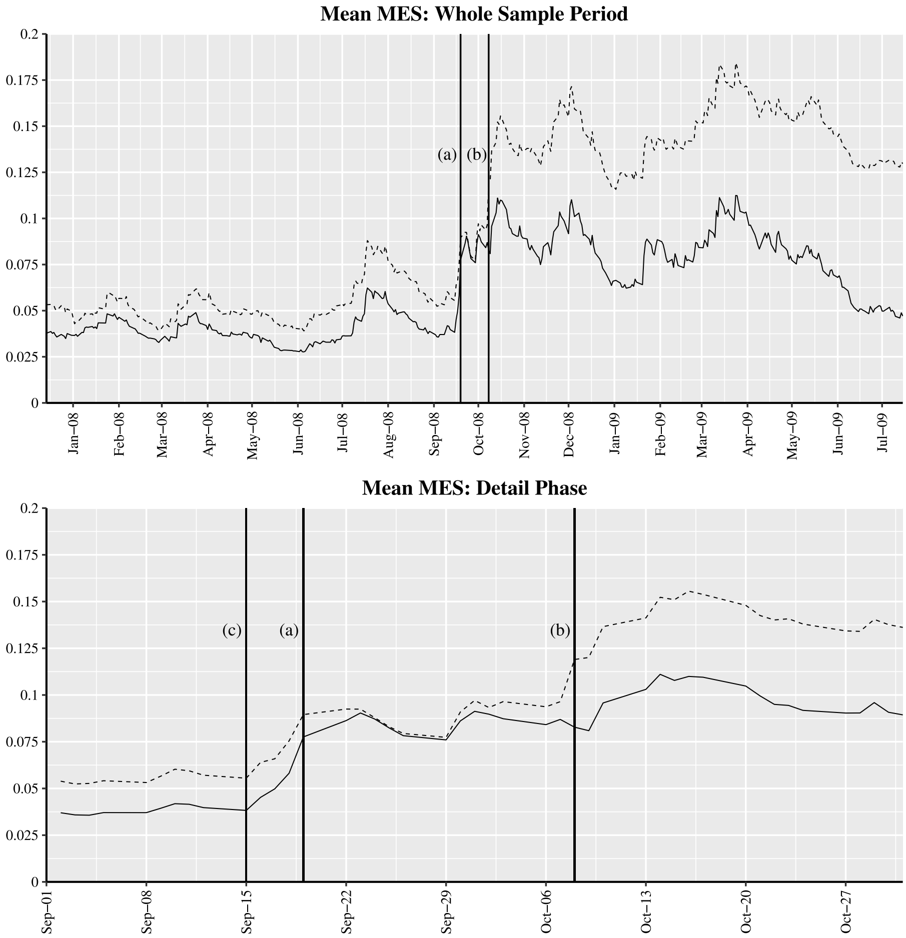

Fig. 1. Daily mean MES values (matched sample).
The bold and dashed line represent means of daily MES values for banned and nonbanned stocks, respectively. The vertical lines (a) and (b) mark the enactment and the lift of the short-selling ban. The upper panel plots the whole investigation period from 200 days before the ban until 200 days after its lift, the lower panel shows only the months September and October 2008. In the lower panel, (c) marks the bankruptcy of Lehman Brothers on September 15th 2008.

zero otherwise. Our main variable of interest is the coefficient γ_3 of the DID term $D_i^{Banned} \cdot D_t^{BanPeriod}$, which captures the effect of the ban on the affected stocks. Finally, F_{it} is a vector of the control variables and ε_{it} represents the error term.

We then perform a propensity score matching to find pairs of banned and nonbanned stocks that are similar on firm characteristics. We use total assets, leverage and the market to book ratio at the end of 2007 as our matching variables to compute the propensity of being affected by the ban (p-score). We then choose with replacement for each stock subject to the ban the nonbanned stock that minimizes the difference in the p-scores between both (nearest neighbor matching). Table 2 characterizes the quality of the matching procedure. For the distance metric and each matching variable we report the mean and median after the matching. Looking at the mean and median of the distance metric we observe that it is well balanced between both groups. This is confirmed by the p-values, which reject the hypothesis of differences in propensities being equal to zero. The mean difference in total assets is around $12.5 billion while the median difference is only slightly more than $19 million indicating that matching on this variable is not perfect due to some outliers in the banned stock sample, but the differences are not statistically different from zero at conventional significance levels. In contrast, leverage shows statistically significant differences even after our matching procedure. Considering the significant differences in leverage statistics between both groups, as documented by mean, median and percentiles documented in Table 1, this is not surprising. Market-to-book ratios are much closer to being distinguishable, with our t-tests confirming the hypothesis that means of banned and its matched control group firm do not differ in MTBRs. Overall, the two groups appear to be well matched during the preban period, but to further control for any remaining differences in firm size or leverage, we later include all three as control variables in our regressions.

Based on this matching we construct three distinct samples: our initial panel with all stocks, a balanced panel which contains all stocks subject to the ban and their matched control stock (with duplication), and a matched panel formed by the balanced sample but dropping duplicate control stocks (that is, stocks that have been matched to several stocks subject to the ban). By doing so, we aim to strengthen our causal inference and to eliminate the risk that our results are model-driven.

Table 2
Matching summary.

	Distance		Total assets		Leverage		MTBR	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Banned	0.8793	0.8899	35,024,280	1,228,492	8.4593	7.9072	1.4725	1.2418
Control	0.8793	0.8912	22,512,270	934,009	8.1278	7.3490	1.3810	1.1655
Difference	0.0000	0.0001	−12,512,010	−19,225	−0.3315	−0.2044	−0.0914	−0.0691
p-value (*t*-test)	0.9785		0.2151		0.0364		0.3429	
p-value (Wilcoxon)	0.7580		0.1048		0.0075		0.0029	

This table presents results of our matching procedure. We perform a propensity score matching following the nearest neighbor technique with replacement. For each stock subject to the ban with available data as of end of 2007, we select with replacement the nonbanned stock that has the smallest distance in propensity scores with respect to the matching variables *Total assets, Leverage*, and *Market-to-Book Ratio (MTBR)*. For each variable, we present means and medians, as well as *p*-values for the tests on differences in means and medians between banned and control stocks tested with t-tests and Wilcoxon tests. Total assets are reported in thousands of US dollars.

Table 3
Mean MES differences.

	ΔMES
Panel A: Ban period vs. preban period	
Banned stocks: Ban period - preban period	0.0415***
Control stocks: Ban period - preban period	0.0347***
DID: Banned stocks - control stocks	0.0068***
Matching Estimator ATT	0.0045***
Panel B: Postban period vs. ban period	
Banned stocks: Postban period - ban period	0.0149***
Control stocks: Postban period - ban period	0.0076***
DID: Banned stocks - control stocks	−0.0073**
Matching Estimator ATT	0.0011
Panel C: Postban period vs. preban period	
Banned stocks: Postban period - preban period	0.0564***
Control stocks: Postban period - preban period	0.0423***
DID: Banned stocks - control stocks	0.0141***
Matching Estimator ATT	0.0056***

This table presents the estimates of the change in the mean MES surrounding the ban. *Panel A* compares the ban period to the preban period by subtracting the preban period's mean MES from the ban period's mean MES. Respectively, *Panel B* and *Panel C* compare the postban period to the ban period and the postban period to the preban period. The first and the second row in each panel contain the differences in the mean MES between the two considered periods for the banned stocks and the control group stocks respectively. We test for differences in means using the Welch two-sample t-test. The third row in each panel contains the unmatched difference-in-differences estimates comparing the banned stocks to the control group stocks. The fourth row in each panel contains the estimates for the average treatment effect on the treated (ATT), based on the nearest neighbor matching as described in Table 2. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

Before turning to our regressions, we compare the mean daily MES of the ban period to the period before the enactment of the ban, as shown in Panel A of Table 3. The first row reports the banned stocks' mean daily MES during the ban period minus the preban period mean daily MES. The second rows show the difference of non-banned stocks, respectively. Third rows show the unmatched difference-in-differences, and fourth rows in each panel report the average treatment effect of the ban, based on the propensity matching described above. As shown in the first two rows, during the ban period, banned and control stocks' MES increases by 4.15% and 3.47%, respectively. This corresponds to a stark increase for both the banned and nonbanned stocks, which have MES values around 3.75% and 5.5% just before the ban, respectively. This effect may either reflect that the ban was enacted during turmoils in the markets that can be traced e.g. to the Lehman Brothers' bankruptcy a few days earlier, as suggested by Fig. 1, or that the ban itself actually caused disruptions affecting the whole market. Similarly, Panel B of Table 3 compares the postban period to the ban period. The negative average treatment effects indicates that, after the lift of the ban, affected stocks experience a moderate recovery in their MES, relative to the control group, which experienced an increase in MES in the postban period. This suggests that the ban, in fact, increased equity tail risk exposure of the banned stocks. However, the average treatment effect is not statistically significant in this case. Finally, Panel C compares the postban period to the preban period and shows that overall, the mean MES levels were higher after the ban than before. Even though the banned stocks could profit from the lift of the ban, their MES was increased by 0.56% due to the ban, as indicated by the average treatment effect.

Table 4
Main results.

	Initial panel (1)	Balanced panel (2)	Matched panel (3)
Banned × ban period	0.0065***	0.0021***	0.0036**
	(0.000)	(0.004)	(0.011)
Log(assets)	−0.0183***	−0.0400***	−0.0193***
	(0.000)	(0.000)	(0.000)
MTBR	0.00002	−0.0002***	0.0000158
	(0.695)	(0.000)	(0.778)
Leverage	0.0001***	0.0001***	0.0001***
	(0.000)	(0.000)	(0.000)
Stock-level fixed effects	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes
Observations	264,973	455,701	257,991
Adjusted R^2	0.747	0.744	0.747

This table presents results from our main regressions showing how the daily MES changes during the ban period for banned versus nonbanned stocks. We estimate daily difference-in-differences regressions with stock-level and day fixed effects. Column (1) presents results form our initial sample, including all stocks. In the *Balanced Panel*, column (2), each sample stock subject to the ban is matched with replacement to a nonbanned, control stock. The *Matched Panel* contains the same stocks as the balanced panel but drops duplicate control units (column (3)). *MTBR* is the market-to-book ratio of a firm's equity. The numbers reported in parentheses below the coefficient estimates are p-values. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

3.1. Baseline regressions

Table 4 presents the results of the baseline regressions specified by Eq. (1). The DID estimator *Banned x ban period* indicates that the stocks subject to the ban experience an increase in their daily MES by 0.21% to 0.65%, depending on the specification used. This effect is both statistically and economically significant, as it reflects a relative increase of up to 11% as compared to banned firm's mean MES over the whole investigation period, or an increase of approximately 17% when compared to MES values just before the ban. Firm size is negatively related to MES in all of the three regressions. The market-to-book ratio is negatively associated with MES using the balanced sample, but this relation is not statistically significant in the initial and matched panel. Unsurprisingly, higher leverage correlates positively to MES, suggesting that riskier capital structures are associated with more exposure of a financial institution's equity to extreme movements in the stock market.

Overall, this evidence suggests that the ban failed in taking pressure from financial stocks. In contrast, it shows that the stocks subject to the ban experience further increases in their MES, i.e., have higher equity tail risk exposure. As shown by Fig. 1, MES values increase over time for both treatment and control group, and our baseline regression indicates that banned stocks experienced an even steeper increase, which was then released when the ban was lifted. That is, during the ban period, treated stocks had an even more elevated MES as they would have had in absence of the short-selling ban.

3.2. Regressions with subsamples

Taking our analysis one step further, we now want to explore if the ban affected all stocks in a similar manner or if its impact differs for certain groups. Therefore, we construct four subsamples by dividing all banned stocks by their total assets as of the end of 2007 and adding in each quartile the matched control firms. We then rerun our regressions, using the balanced panel where each banned stock is matched with a similar nonbanned stock, and present results in Interestingly, for banned firms with total assets below the median (columns 1 and 2) the ban reduces the MES, whereas the largest firms experience a drastic relative increase in equity tail risk. Furthermore, the influence of size on equity tail risk does not vary with firm size, but MTBR is associated with an increase in MES for the smallest firms in our sample, but reduces MES for above median sized firms' equities. That is, higher charter value is important in reducing equity tail risk for the largest financial firms, and we can only confirm results on MTBR and systemic equity risk similar to Brunnermeier et al. (2020) for the smallest firms in our sample.

Next, we are interested in whether the ban helped particularly vulnerable firms in reducing their equity tail risk, as intended by the regulators. To examine this, we compute the average daily MES values for all stocks subject to the ban during the first half of the year 2008 and then divide our initial sample according to those mean MES values adding the respective matched control firms in each quartile. Again, we repeat the regressions as characterized by Eq. (1). Table 6 shows that those firms with a high MES in the preban period experience a further sharp increase in their MES that was triggered by the ban. However, for the first two quartiles of pre-ban MES values, i.e., those stocks with prior lower MES, the DID estimate for *Banned x ban period* has a negative sign and is statistically significantly different from zero. As shown in column (4), the most vulnerable firms with higher pre-ban MES experience an increase of their MES by 3.2%, which is both statistically and economically relevant and higher than our baseline estimates for the full sample. This, again, is the exact opposite effect of what regulators aimed to achieve with the restriction of short-selling.

In summary, banned stocks' exposure to financial instability increases significantly more than the exposure of the control stocks, affecting most firms with higher MES values in the past and very large firms.

Table 5
Regressions with size subsamples.

	Size Q1 (smallest) (1)	Size Q2 (2)	Size Q3 (3)	Size Q4 (largest) (4)
Banned × ban period	−0.0205***	−0.0061***	0.0006	0.0353***
	(0.000)	(0.000)	(0.655)	(0.000)
Log(assets)	−0.0224***	−0.0393***	−0.0526***	−0.0181***
	(0.000)	(0.000)	(0.000)	(0.000)
MTBR	0.0001**	0.00004	−0.0005***	−0.0102***
	(0.011)	(0.625)	(0.000)	(0.000)
Leverage	0.0002***	−0.00002***	0.0001***	0.0002***
	(0.000)	(0.002)	(0.000)	(0.000)
Stock-level fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes
Observations	114,040	113,447	114,809	112,015
Adjusted R^2	0.729	0.738	0.721	0.750

This table shows regression results using MES as dependent variable and splitting the full sample by firm size quartiles. Size quartiles are constructed by partitioning the banned stocks according to their total assets as of the end of 2007. Each sample stock subject to the ban is matched with replacement to a similar nonbanned stock ("balanced panel"). We carry out daily difference-in-differences regressions with stock-level and day fixed effects. *Ban period* is a dummy that equals one on all days during the ban period and zero otherwise. *Banned × ban period* is a difference-in-differences indicator variable that equals one if a stock is affected by the ban and the observation falls within the ban period. *MTBR* is the market-to-book ratio of a firm's equity. The numbers reported in parentheses below the coefficient estimates are p-values. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

Table 6
Regressions with MES subsamples.

	MES Q1 (lowest) (1)	MES Q2 (2)	MES Q3 (3)	MES Q4 (highest) (4)
Banned × ban period	−0.0244***	−0.0023**	0.0090***	0.0319***
	(0.000)	(0.028)	(0.000)	(0.000)
Log(assets)	−0.0663***	−0.0303***	−0.0383***	−0.0299***
	(0.000)	(0.000)	(0.000)	(0.000)
MTBR	0.0004***	−0.0039***	−0.0002***	−0.0051***
	(0.000)	(0.000)	(0.001)	(0.000)
Leverage	−0.00001*	0.0001***	0.0001***	0.0003***
	(0.100)	(0.000)	(0.000)	(0.000)
Stock-level fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes
Observations	123,916	114,695	114,356	102,734
Adjusted R^2	0.717	0.653	0.662	0.740

This table shows regression results using MES as dependent variable and splitting the full sample by firm MES quartiles. MES quartiles are constructed by partitioning the banned stocks according to their mean MES values during the first half of 2008. Each sample stock subject to the ban is matched with replacement to a similar nonbanned stock ("balanced panel"). We carry out daily difference-in-differences regressions with stock-level and day fixed effects. *Ban period* is a dummy that equals one on all days during the ban period and zero otherwise. *Banned × ban period* is a difference-in-differences indicator variable that equals one if a stock is affected by the ban and the observation falls within the ban period. *MTBR* is the market-to-book ratio of a firm's equity. The numbers reported in parentheses below the coefficient estimates are p-values. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

3.3. Short-selling ban and options

Some of the banned stocks in our study might have faced significant tail risk exposure due to the potential use of put option trading by investors as substitute for short-selling the stock. The availability of put options, in theory, allows investors to bypass restrictions on short-selling and consequently exert downward pressure on the stocks (cf. Figlewski and Webb, 1993; Danielsen and Sorescu, 2001). However, Grundy et al. (2012) argue that put options were not necessarily a perfect substitute for reduced short-selling of banned stocks, e.g., because of lower market liquidity (Kolasinski et al., 2013) and higher costs associated with short-selling stocks (Atmaz and Basak, 2019), resulting in lower demand for short-selling via put options by investors.

To test whether the ban had differential effects on the MES of stocks with existing options versus those without, we offer Table 7 where we report our baseline DID regressions using subsamples of stocks with and without existing option contracts. In regressions using the sample of stocks with existing options, we find that the treatment effect is positive and statistically significantly different from zero at the 1% level using either initial, balanced, or matched panel data. Banned stocks experienced a relative increase in MES versus nonbanned stocks, in line with out main findings, thus supporting the hypothesis that stocks where investors may employ put options to profit from decreasing stock prices experienced further downward pressure on tail risk. In regressions using the sample of stocks without options associated to them, we find that the short-selling ban had the opposite effect, with negative coefficient estimates of the matching estimators, i.e., a relative reduction in MES for banned stocks.

Table 7
Regressions with option subsamples.

	Initial panel		Balanced panel		Matched panel	
	Option	No option	Option	No option	Option	No option
	(1)	(2)	(3)	(4)	(5)	(6)
Banned × ban period	0.0312***	−0.0015	0.0112***	−0.0016**	0.0282***	−0.0043**
	(0.000)	(0.341)	(0.000)	(0.033)	(0.000)	(0.018)
Log(assets)	0.0021	−0.0226***	−0.0291***	−0.0245***	0.0005	−0.0225***
	(0.148)	(0.000)	(0.000)	(0.000)	(0.754)	(0.000)
MTBR	−0.0003***	0.0003***	−0.0020***	0.0004***	−0.0004***	0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Leverage	0.0002***	0.00004***	0.00003***	0.0001***	0.0001***	0.00003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Stock-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	80,396	184,577	121,750	333,951	76,624	181,367
Adjusted R^2	0.754	0.732	0.720	0.743	0.753	0.731

This table presents results from regressions showing how the daily MES changes during the ban period for banned versus nonbanned stocks with and without associated option contracts. We estimate daily difference-in-differences regressions with stock-level and day fixed effects. The *Initial Panel* includes all stocks. In the *Balanced Panel*, each sample stock subject to the ban is matched with replacement to a nonbanned, control stock. The *Matched Panel* contains the same stocks as the balanced panel but drops duplicate control units (that is, control firms that were matched to several banned firms appear only once, cf. Table 2 for details regarding the matching). Each of the three samples is split into stocks with available options data for the sample period, provided by *Optionmetrics*, or whether no option exists for the underlying stock. *MTBR* is the market-to-book ratio of a firm's equity. The numbers reported in parentheses below the coefficient estimates are p-values. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

Table 8
Regressions with stock returns.

Sample:	Balanced panel		
	Full	Option	No option
Banned × ban period	0.0009	−0.0006	0.0016
	(0.417)	(0.803)	(0.208)
Log(assets)	−0.0029***	−0.0025**	−0.0035***
	(0.002)	(0.039)	(0.009)
MTBR	−0.0002**	−0.0002***	−0.0002
	(0.021)	(0.008)	(0.159)
Leverage	0.0001***	0.00003***	0.0001***
	(0.000)	(0.003)	(0.000)
Stock-level fixed effects	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes
Observations	455,701	121,750	333,951
Adjusted R^2	0.155	0.411	0.101

This table presents results from regressions using stock returns as dependent variable. We estimate daily difference-in-differences regressions with stock-level and day fixed effects using the full sample (balanced panel), as well as subsamples split by whether a stock has associated options data for the sample period, provided by *Optionmetrics*, or whether no option exists for the underlying stock. *MTBR* is the market-to-book ratio of a firm's equity. The numbers reported in parentheses below the coefficient estimates are p-values. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

That is, our main results are driven by those stocks with existing (put) options, where the ban did not have the intended effect, but rather increased banned stocks tail risk. However, the short-sale ban was effective for those stocks without alternative ways to engage in short-selling activities.

3.4. Short-selling constraints and stock performance

Since our measure of equity tail risk, MES, considers average returns on days of extreme market downturns, it does not capture stocks' performance during normal days. For example, it could be that our main results are due to price corrections that occur following an overvaluation of treated stocks during the short-sale ban period. Short-selling restrictions make it more expensive to gain positive exposure to stock prices decreases and thus, can be associated with overpricing of affected stocks and lead to subsequent lower returns (Jones and Lamont, 2002; Boehme et al., 2006).[8] This is because in face of short-sale constraints, bearish investors find it harder to express their negative views (cf. Miller, 1977; Atmaz and Basak, 2018; Atmaz et al., 2024).

The short-selling ban in 2008 can be used as a laboratory to test whether banned stocks experience such underperformance and whether our findings may be explained by simple price reversals that mechanically drive MES and thus, would not allow us to

[8] Asquith et al. (2005) also find that short-sale constrains (not necessarily bans) lead to stocks' underperformance.

interpret our MES results as banned stocks having higher equity tail risk exposure during the ban. We use daily stock returns as dependent variable to test whether average stock performance was significantly different for banned versus nonbanned stocks during the ban period. Panel regressions as in (1) are performed for the full sample and subsamples of stocks with and without associated options, respectively, and regression results are given in Table 8. In all three regressions, we find that the short-selling ban did not affect average stock returns of banned versus nonbanned stocks. That is, none of the coefficient estimates of the DID dummy variable are significantly different from zero at conventional levels. This is counter to the argument by Miller (1977) and others that short-selling restrictions like the 2008 ban inflate stock values due to a lack of (demand for) short positions and lead to lower subsequent returns. Note that this is also not the case for our subsamples of treated stocks without associated (put) options, that would suffer even more from such overvaluation and lower performance. Furthermore, this finding rules out that our tail risk results are merely driven by price corrections that are the consequence of overvalued short-selling constrained stocks. As an additional test on stock performance, we consider an individual stock's Value-at-Risk (at the 5% threshold, as in MES calculations), estimated parametrically as $VaR_{j,t} = \sigma_{j,t}\mathbf{z}_{5\%}$, where $\sigma_{j,t}$ are conditional volatilities obtained from fitting TGARCH models to individual stock j's returns, $\mathbf{z}_{5\%}$ is the 5% quantile of the standard normal distribution, and assuming zero mean.[9] DID regression results are shown in Table 9 and indicate that banned stocks experienced a significant increase in individual tail risk (lower Value-at-Risk) in almost all the specifications. That is, since individual tail risk is related to our equity tail risk exposure measure MES (cf. Benoit et al., 2013), we can conclude that both sensitivity to extreme market downturns and the banned stocks' tail risk are elevated during the ban period.

3.5. Robustness: Alternative matching strategy

One of the drawbacks of our matching strategy is that banned stocks belonging to a particular type of financial institution, such as bank stocks, may be matched with stocks from other categories, such as insurers or other financial services providers with different business models.[10] To alleviate some of the concerns that the matched stocks are too different in terms of business model, we already match based on size (total assets), funding structure (leverage), and charter value (MTBR). As a robustness test, we now additionally restrict the set of matching firms by allowing banned stocks to only be matched with stocks of firms of the same type. For this purpose, we classify our sample of treated and matched control stocks according to their SIC code and cluster them into four broad categories: banks, insurers, financial services, and "other". To rule out that contamination due to cross-category matching is driving our results, we perform analyses using a matching strategy that pairs within-category (i.e., banks-banks, insurers-insurers, etc.). Summary statistics on our matching procedure and regressions results of our main specifications using the alternative matching strategy to construct a balanced sample are shown in Table 10. Restricting our matching stock pairs to have the same industry cluster (e.g., bank-bank pairs) slightly impairs our matching quality, indicated by the significant t-test on differences in means of our distance metric. This is likely driven by the imperfect matching of stocks within the same industry cluster based on size, where differences in means are significantly different from zero. When we re-run our regressions, however, we find similar results to our main findings, i.e., MES of banned stocks is relatively elevated and this result is stronger for stocks with associated option contracts. Thus, even though our alternative matching strategy is more restrictive, with imperfect matching in the statistical sense, our main findings are qualitatively the same.

3.6. End of the ban

We want to deepen our understanding of the events surrounding the lift of the ban. Our main regressions show that banned stocks experienced, on average, a sharper increase in MES during the ban period, relative to control stocks and relative to the period before the ban. However, when zooming into the days before the ban is lifted, we observe that this DID effect is partially set off by the few days before the lift of the ban. From the enlarged plot in Fig. 1, we can see that MES values of control group stocks were slightly increasing in the days before the lift of the ban, whereas banned stocks' MES remained relatively stable. We now therefore explore whether the lift of the ban had an additional effect on the affected stocks versus their matched counterparts to infer whether removal of the short-sale ban had a noticeable effect for financial market stability.

To conduct statistical inference, we follow Boehmer et al. (2013) and run regressions that include only the data from day [−5] to [+14] relative to the lift of the ban on day [0]. We compare the last days of the ban period [−5, −1] to the days [0, 4], [5, 9] and [10, 14] by estimating daily DID regressions that are specified by the following equation:

$$\begin{aligned} MES_{it} = \;& \alpha_i + \beta_1 \cdot D_i^{Banned} + \beta_2 \cdot D_t^{Day[0,4]} + \beta_3 \cdot D_t^{Day[5,9]} + \beta_4 \cdot D_t^{Day[10,14]} \\ & + \beta_5 \cdot D_i^{Banned} \cdot D_t^{Day[0,4]} + \beta_6 \cdot D_i^{Banned} \cdot D_t^{Day[5,9]} + \beta_7 \cdot D_i^{Banned} \cdot D_t^{Day[10,14]} \\ & + \beta_8 \cdot F_{it} + \varepsilon_{it}, \end{aligned} \tag{2}$$

where MES_{it} is the MES of financial institution i on day t. In all regressions, we include stock-level fixed effects α_i that absorb unobserved heterogeneity over time. D_i^{Banned} is a dummy that equals one if and only if a stock is on the ban list. The dummy

[9] As an alternative, we calculate VaR using a rolling window of return data and a filtered historical simulation approach and obtain similar regression results.

[10] In the cases where banks or other categories of treated equities are matched to equities in another category, we may argue that many financial stocks were treated similarly with respect to shareholder valuation and thus, equity tail risk exposure (MES), although many issues during the financial crises primarily stemmed from the banking sector (see, e.g., Irresberger et al., 2017, for evidence that insurer stocks were unreasonably associated with the financial crisis although they were not materially exposed or contributed to risks in the financial system at the time).

Table 9
Regressions With value-at-risk.

Dependent variable: Value-at-Risk	Initial panel		
Sample:	Full	Option	No option
Banned × ban period	−0.0081***	−0.0211***	−0.0038***
	(0.000)	(0.000)	(0.007)
Log(assets)	0.0104***	0.00186**	0.0179***
	(0.000)	(0.024)	(0.000)
MTBR	0.0002***	0.0002***	0.0003***
	(0.000)	(0.000)	(0.000)
Leverage	−0.0003***	−0.0003***	−0.0003***
	(0.000)	(0.000)	(0.000)
Stock-level fixed effects	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes
Observations	263,328	79,996	183,332
Adjusted R^2	0.640	0.715	0.619

Dependent variable: Value-at-Risk	Balanced panel		
Sample:	Full	Option	No option
Banned × ban period	−0.0030***	−0.0247***	0.0047***
	(0.000)	(0.000)	(0.000)
Log(assets)	0.0214***	0.0115***	0.0262***
	(0.140)	(0.000)	(0.000)
MTBR	−0.0001	−0.0010***	0.0003***
	(0.000)	(0.099)	(0.000)
Leverage	−0.0002***	−0.0003***	−0.0002***
	(0.000)	(0.000)	(0.000)
Stock-level fixed effects	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes
Observations	455,701	121,750	333,951
Adjusted R^2	0.586	0.582	0.598

Dependent variable: Value-at-Risk	Matched panel		
Sample:	Full	Option	No option
Banned × ban period	−0.00639***	−0.0186***	−0.00192
	(0.000)	(0.000)	(0.207)
Log(assets)	0.00922***	0.00163*	0.0154***
	(0.000)	(0.052)	(0.000)
MTBR	0.0002***	0.0002***	0.0003***
	(0.000)	(0.000)	(0.000)
Leverage	−0.0002***	−0.0003***	−0.0002***
	(0.000)	(0.000)	(0.000)
Stock-level fixed effects	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes
Observations	256,384	76,234	180,150
Adjusted R^2	0.653	0.736	0.630

This table presents results from regressions using a stock's Value-at-Risk (VaR) as dependent variable. Daily VaR is estimated parametrically as $VaR_{j,t} = \sigma_{j,t}\mathbf{z}_{5\%}$, where $\sigma_{j,t}$ are conditional volatilities obtained from fitting TGARCH models to individual stock j's returns, $\mathbf{z}_{5\%}$ is the 5% quantile of the standard normal distribution, and assuming zero mean. We estimate daily difference-in-differences regressions with stock-level and day fixed effects. The *Initial Panel* includes all stocks. In the *Balanced Panel*, each sample stock subject to the ban is matched with replacement to a nonbanned, control stock. The *Matched Panel* contains the same stocks as the balanced panel but drops duplicate control units. For each panel, the full sample is also split by whether stocks have associated options data available in *Optionmetrics* or whether no option exists for the underlying stock. Numbers reported in parentheses below coefficient estimates are p-values. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

$D_t^{Day[0,4]}$ is equal to one on all days during the first week after the lift of the ban (event days 0 to 4) and zero otherwise, with $D_t^{Day[5,9]}$ and $D_t^{Day[10,14]}$ defined similarly for the second and third week after the lift of the ban (event days 5 to 9 and 10 to 14 respectively). Being our main subject of interest, the coefficients β_{6-8} capture how the MES of banned stocks changes during the first, second and third postban week since $D_i^{Banned} \cdot D_t^{Day[0,4]}$, $D_i^{Banned} \cdot D_t^{Day[5,9]}$ and $D_i^{Banned} \cdot D_t^{Day[10,14]}$ are dummies that equal one if and only if stock i is subject to the ban and the observation day t lies within the time window of day [0, 4], day [5, 9] or day [10, 14] respectively, after the lift of the ban. Finally, F_{it} is the same vector of the control variables as before.

To begin with, we estimate the model described above with data from our size subsamples. Table 11 shows that for most stocks in our sample, except for the smallest ones, exposure to financial instability increases right after the lift of the ban, with the variable $Day[0,4]$ showing positive and statistically significant coefficients throughout most specifications. This effect remains present in the

Table 10
Robustness: Main results using alternative matching strategy.

Panel A: Alternative matching summary

	Distance		Total Assets		Leverage		MTBR	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Ban	0.9124	0.9279	35,146.50	1,228.49	8.4826	7.9274	1.4676	1.2399
Control	0.9105	0.9311	13,559.78	1,258.87	8.8983	7.8242	1.4221	1.1529
Difference	−0.0019	0.0000	−21,586.72	129.22	0.4157	−0.2736	−0.0455	−0.0341
p-value (t-test)	0.0055		0.0246		0.3293		0.7013	
p-value (Wilcoxon)	0.3720		0.3810		0.3753		0.0059	

Panel B: Regression results

Dependent variable: MES	Full	Option	No option
Banned × ban period	0.0036***	0.0181***	−0.0011
	(0.000)	(0.000)	(0.187)
Control variables	Yes	Yes	Yes
Stock-level fixed effects	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes
Observations	454,686	131,333	323,353
Adjusted R^2	0.733	0.729	0.718

This table shows regression results of our main specifications using an alternative matching strategy based on industry category to construct a balanced sample of banned and nonbanned stocks. Based on SIC codes, we assign each stock to one of four industry categories: (1) banking, (2) insurance, (3) financial services, and (4) others. For each stock subject to the ban (with available data end of 2007), we select, with replacement, the nonbanned stock that has the smallest distance in propensity scores. Our matching variables are *Total assets, Leverage*, and *Market-to-Book Ratio (MTBR)* and matching is only allowed within the same industry category. Panel A shows statistics summarizing our alternative matching procedure: for each variable, we present means and medians, as well as p-values for the differences in means and medians between banned and control stocks. Panel B shows regression results using MES as dependent variable. We conduct our regressions for the full sample (balanced panel) and for subsamples of stocks with and without available option data. The numbers reported in parentheses below the coefficient estimates are p-values. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

second and third postban week, with slightly larger coefficient in some of the regressions. Turning to the DID estimators, we can observe that all banned stocks, except the larger ones in the fourth size quartile, experience a further relative increase in their equity tail risk. Note that this specification estimates the relative increase from the five days before the lift of the ban, where MES values of control group stocks surged versus banned stocks already (cf. Fig. 1), to the end of the postban period. That is, observing MES in a short time window around the removal of the short-sale ban reveals that the ban had some of the intended effect of suppressing tail risk for banned stocks, but only in the final few days of the ban period. Immediately after the ban is lifted, we observe a relative increase of the previously banned stocks, relative to the few days before the ban removal. This provides us with some evidence that the short-sale ban had the intended effect of improving financial stability, but it only yielded such improvement many days after it was implemented, not for the majority of the ban period, so that its removal resulted in a surge in MES for banned stocks. The exception to this finding can be seen in column (4), where banned stocks in the larger size category experience no additional increase in MES values the first week after the ban, a slight decrease in the second week, and a stark reversal of MES increases in the third week, leading to overall reduction in MES relative to the days before the ban-lift for large banned stocks. This is in line with our previous findings in Table 5, where the largest stocks experience a very stark relative increase in MES during the ban period, indicating that the short-sale ban did not affect them in the intended manner. Next, we perform the same regressions at the end of the ban with our MES subsamples, as described in Section 3.2. In Table 12, we observe that the coefficients for the interaction term *Banned x day [0, 4]* are mostly positive, but not statistically significantly different from zero for all four subsamples and weeks after the ban removal. Firms in the lower MES quartile, i.e., those with low exposure prior to the short-sale ban in September 2008, have a relative surge in the first and third week after the lift of the ban as compared to the final few days of the ban period. Firms in the third MES quartile do not see any change to their MES from before the ban lift to three weeks after. However, the most exposed firms (fourth MES quartile) have the highest increase in MES in the two weeks following the ban lift, relative to their matched counterpart and the final five days of the ban period.

In summary, removing the ban slightly increased most firms' exposure to equity tail risk, except for the largest ones, as compared to where they were five days before the lift. That is, when focusing on a narrow window of the final five days of the ban period, where MES values of banned and control group stocks started diverging after being close to each other for most of the ban period (cf. Fig. 1), we find some evidence that the ban might have worked as intended, as equity tail risk exposure rises sharply for banned stocks when the short-sale ban is taken away. Taken together, our results suggest that the ban did not have the intended effect on stocks, as banned stocks experienced a relative and sharp increase in MES values during the ban period, but lifting the ban has resulted in banned stocks experiencing a further relative increase in equity tail risk exposure when compared to the final few days of the ban, suggesting that the ban might have worked only for a small time window before it was lifted.

4. Conclusion

In this study, we examine how the U.S. short-selling ban in 2008 on selected financial institutions' stock affected their equity tail risk exposure to extreme market downturns. After matching each stock subject to the ban to a nonbanned stock that resembles

Table 11
Regressions at the end of the ban: Size subsamples.

	Size Q1 (smallest) (1)	Size Q2 (2)	Size Q3 (3)	Size Q4 (largest) (4)
Day [0, 4]	0.0017	0.0031**	0.0162***	0.0231***
	(0.231)	(0.018)	(0.000)	(0.000)
Day [5, 9]	0.0009	0.0107***	0.0298***	0.0366***
	(0.501)	(0.000)	(0.000)	(0.000)
Day [10, 14]	−0.0036***	0.0035***	0.0148***	0.0236***
	(0.009)	(0.008)	(0.000)	(0.000)
Banned × day [0, 4]	0.0051**	0.0076***	0.0038*	0.0027
	(0.015)	(0.001)	(0.099)	(0.356)
Banned × day [5, 9]	0.0101***	0.0066***	0.0078***	0.0061**
	(0.000)	(0.001)	(0.001)	(0.026)
Banned × day [10, 14]	0.0140***	0.0035*	0.0070***	−0.0102***
	(0.000)	(0.081)	(0.001)	(0.000)
MTBR	2.142***	−0.0324***	0.149***	0.415***
	(0.000)	(0.000)	(0.000)	(0.000)
Leverage	0.0631***	−0.0112***	0.0079***	0.0162***
	(0.000)	(0.000)	(0.000)	(0.000)
Stock-level fixed effects	Yes	Yes	Yes	Yes
Observations	5,560	5,540	5,620	5,440
Adjusted R^2	0.934	0.929	0.902	0.899

This table shows daily MES regression results around the end of the ban for banned versus nonbanned stocks in different firm size quartiles. We carry out daily difference-in-differences regressions with stock-level fixed effects using only data from day −5 to day +14 relative to the lift of the ban on day 0. *Day [0, 4]* equals one if the observation is in the first five days after the lift of the ban, *Day [5, 9]* and *Day [10, 14]* indicate the other relevant time windows, respectively. *Banned × day* is a difference-in-differences dummy that equals one if the stock is affected by the ban and if the observation falls in the relevant postban time window. The numbers reported in parentheses below the coefficient estimates are p-values. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

Table 12
Regressions at the end of the ban: MES subsamples.

	MES Q1 (lowest) (1)	MES Q2 (2)	MES Q3 (3)	MES Q4 (highest) (4)
Day [0, 4]	−0.0020*	0.0089***	0.0260***	0.0120***
	(0.095)	(0.000)	(0.000)	(0.000)
Day [5, 9]	0.0005	0.0155***	0.0402***	0.0234***
	(0.682)	(0.000)	(0.000)	(0.000)
Day [10, 14]	−0.0042***	0.0058***	0.0238***	0.0143***
	(0.001)	(0.000)	(0.000)	(0.000)
Banned × day [0, 4]	0.0041**	0.0030	−0.0011	0.0145***
	(0.030)	(0.100)	(0.695)	(0.000)
Banned × day [5, 9]	0.0021	0.0061***	0.0043	0.0206***
	(0.236)	(0.000)	(0.105)	(0.000)
Banned × day [10, 14]	0.0079***	0.0039**	−0.0002	0.0030
	(0.000)	(0.022)	(0.951)	(0.328)
MTBR	−0.0477***	−0.0259	−0.0035	−0.1690***
	(0.000)	(0.999)	(1.000)	(0.000)
Leverage	−0.0164***	0.0006	−0.0014	−0.0429***
	(0.000)	(1.000)	(1.000)	(0.000)
Stock-level fixed effects	Yes	Yes	Yes	Yes
Observations	6,080	5,620	5,540	5,000
Adjusted R^2	0.939	0.899	0.828	0.913

This table shows daily MES regression results around the end of the ban for banned versus nonbanned stocks by firm MES quartiles. MES quartiles are constructed by partitioning the banned stocks according to their mean MES values during the first half of 2008. We carry out daily difference-in-differences regressions with stock-level fixed effects using only data from day −5 to day +14 relative to the lift of the ban on day 0. *Day [0, 4]* equals one if the observation is in the first five days after the lift of the ban, *Day [5, 9]* and *Day [10, 14]* indicate the other relevant time windows, respectively. *Banned x day* is a difference-in-differences dummy that equals one if the stock is affected by the ban and if the observation falls in the relevant postban time window. The numbers reported in parentheses below the coefficient estimates are p-values. *, **, and *** indicate statistical significance levels of 10%, 5%, and 1%, respectively.

it most, based on firm size, leverage, and the market-to-book-ratio, we estimate daily panel regressions and find that the ban was not effective in reducing tail risk exposure, as measured by the dynamic Marginal Expected Shortfall. Banned stocks experienced a sharp relative increase in their MES and reached similar levels of equity tail risk exposure as their matched counterpart during the ban period. Especially large firms, stocks with existing options, and those stocks most vulnerable to market downturns in the months prior to the ban experience sharp increases in their exposure to market downturns during the ban period.

We find very limited evidence that the short-selling ban had the intended effect of alleviating increase equity tail risk exposure for chosen stocks. Only for a narrow window of a few days around the lift of the ban, but not for the full sample, do we find some evidence that the ban might have worked towards the end of the ban period, but was subsequently lifted, resulting in even higher MES values for banned stocks, compared to what they would have experienced had the lift been left in place.

Given the SEC's intention to prevent "artificial price movements based on unfounded rumors regarding the stability of financial institutions [...] and [a] disruption in the functioning of the securities markets that could threaten fair and orderly markets" (cf. SEC's Emergency Order, Release No. 34-58592, published September 18, 2008.), we conclude that regulators failed in reaching this aim. In contrast, overall exposure of banned stocks to market downturns worsened during the ban. Our paper thus contributes to the debate on how short-selling restrictions impact security markets.

CRediT authorship contribution statement

Jonas Bartl: Writing – original draft, Investigation, Formal analysis, Conceptualization. **Denefa Bostandzic:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis. **Felix Irresberger:** Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **Gregor Weiß:** Writing – review & editing, Writing – original draft, Supervision, Methodology, Investigation, Conceptualization. **Ruomei Yang:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis.

References

Acharya, V.V., Engle, R., Richardson, M., 2012. Capital shortfall: A new approach to ranking and regulating systemic risks. Amer. Econ. Rev. 102 (3), 59–64.
Acharya, V.V., Pedersen, L.H., Philippon, T., Richardson, M., 2017. Measuring systemic risk. Rev. Financ. Stud. 30, 2–47.
Aielli, G.P., 2013. Dynamic conditional correlations: On properties and estimation. J. Bus. Econom. Statist. 31 (3), 282–299.
Asquith, P., Pathak, P.A., Ritter, J.R., 2005. Short interest, institutional ownership, and stock returns. J. Financ. Econ. 78 (2), 243–276.
Atmaz, A., Basak, S., 2018. Belief dispersion in the stock market. J. Finance 73 (3), 1225–1279.
Atmaz, A., Basak, S., 2019. Option prices and costly short-selling. J. Financ. Econ. 134 (1), 1–28.
Atmaz, A., Basak, S., Ruan, F., 2024. Dynamic equilibrium with costly short-selling and lending market. Rev. Financ. Stud. 37 (2), 444–506.
Autore, D.M., Billingsley, R.S., Kovacs, T., 2011. The 2008 short sale ban: Liquidity, dispersion of opinion, and the cross-section of returns of US financial stocks. J. Bank. Financ. 35 (9), 2252–2266.
Battalio, R., Schultz, P., 2011. Regulatory uncertainty and market liquidity: The 2008 short sale ban's impact on equity option markets. J. Finance 66 (6), 2013–2053.
Beber, A., Fabbri, D., Pagano, M., 2021. Short-selling bans and bank stability. Rev. Corp. Finance Stud. 10 (1), 158–187.
Beber, A., Pagano, M., 2013. Short-selling bans around the world: Evidence from the 2007-09 crisis. J. Finance 68 (1), 343–381.
Beck, T., Demirgüç-Kunt, A., Levine, R., 2006. Bank concentration, competition, and crises: First results. J. Bank. Financ. 30 (5), 1581–1603.
Beltratti, A., Stulz, R.M., 2012. The credit crisis around the globe: Why did some banks perform better? J. Financ. Econ. 105 (1), 1–17.
Benoit, S., Colletaz, G., Hurlin, C., 2013. A theoretical and empirical comparison of systemic risk measures. Working Paper.
Benoit, S., Colliard, J.-E., Hurlin, C., Pérignon, C., 2017. Where the risks Lie: A survey on systemic risk. Rev. Finance 109–152.
Berger, A.N., Di Patti, E.B., 2006. Capital structure and firm performance: A new approach to testing agency theory and an spplication to the banking industry. J. Bank. Financ. 30 (4), 1065–1102.
Boehme, R.D., Danielsen, B.R., Sorescu, S.M., 2006. Short-sale constraints, differences of opinion, and overvaluation. J. Financ. Econ. 41 (2), 455–487.
Boehmer, E., Jones, C.M., Zhang, X., 2013. Shackling short sellers: The 2008 shorting ban. Rev. Financ. Stud. 26 (6), 1363–1400.
Boyd, J.H., Nicoló, G.D., Smith, B.D., 2004. Crises in competitive versus monopolistic banking systems. J. Money Credit Bank. 36 (3), 487–506.
Brownlees, C.T., Engle, R.F., 2011. Volatility, correlation and tails for systemic risk measurement. Working Paper.
Brownlees, C., Engle, R.F., 2017. SRISK: A conditional capital shortfall measure of systemic risk. Rev. Financ. Stud. 30 (1), 48–79.
Brunnermeier, M.K., Dong, G.N., Palia, D., 2020. Banks' non-interest income and systemic risk. Rev. Corp. Finance Stud. 9 (2), 229–255.
Brunnermeier, M.K., Oehmke, M., 2014. Predatory short selling. Rev. Finance 18 (6), 2153–2195.
Cappiello, L., Engle, R.F., Sheppard, K., 2006. Asymmetric dynamics in the correlations of global equity and bond returns. J. Financ. Econom. 4 (4), 537–572.
Danielsen, B.R., Sorescu, S.M., 2001. Why do option introductions depress stock prices? A study of diminishing short sale constraints. J. Financ. Quant. Anal. 36 (4), 451–484.
DeAngelo, H., Stulz, R.M., 2015. Liquid-claim production, risk management, and bank capital structure: Why high leverage is optimal for banks. J. Financ. Econ. 116 (2), 219–236.
Demsetz, R.S., Saidenberg, M.R., Strahan, P.E., 1996. Banks with something to lose: The disciplinary role of franchise value. Econ. Policy Rev. 2 (2), 1–14.
Demsetz, R.S., Strahan, P.E., 1997. Diversification, size, and risk at bank holding companies. J. Money Credit Bank. 29 (3), 300–313.
Diamond, D.W., 1984. Financial intermediation and delegated monitoring. Rev. Econ. Stud. 51 (3), 393–414.
Diamond, D.W., Verrecchia, R.E., 1987. Constraints on short-selling and asset price adjustment to private information. J. Financ. Econ. 18 (2), 277–311.
Engle, R., 2002. Dynamic conditional correlation. J. Bus. Econom. Statist. 20 (3), 339–350.
Fahlenbach, R., Prilmeier, R., Stulz, R.M., 2012. This time is the same: Using bank performance in 1998 to explain bank performance during the recent financial crisis. J. Finance 67 (6), 2139–2185.
Félix, L., Kräussl, R., Stork, P., 2016. The 2011 European short sale ban: A cure or a curse? J. Financial Stab. 25, 115–131.
Figlewski, S., Webb, G.O., 1993. Options, short sales, and market completeness. J. Finance 48 (2), 761–777.
Grossman, S.J., Hart, O.D., 1982. Corporate financial structure and managerial incentives. In: McCall, J.J. (Ed.), The Economics of Information and Uncertainty. University of Chicago Press, pp. 107–140.
Grundy, B.D., Lim, B., Verwijmeren, P., 2012. Do option markets undo restrictions on short sales? Evidence from the 2008 short-sale ban. J. Financ. Econ. 106 (2), 219–236.
Hovakimian, A., Kane, E., Laeven, L., 2012. Variation in systemic risk at US banks during 1974–2010. NBER Working Paper No. 18043.
Irresberger, F., König, F., Weiss, G., 2017. Crisis sentiment in the U.S. insurance sector. J. Risk Insurance 84 (4), 1295–1330.
Jones, C.M., Lamont, O.A., 2002. Short-sale constraints and stock returns. J. Financ. Econ. 66, 207–239.
Keeley, M.C., 1990. Deposit insurance, risk, and market power in banking. Am. Econ. Rev. 80 (5), 1183–1200.
Kolasinski, A.C., Reed, A., Thornock, J.R., 2013. Can short restrictions actually increase informed short selling? Financial Manag. 42 (1), 155–181.
Lecce, S., Lepone, A., McKenzie, M.D., Segara, R., 2012. The impact of naked short selling on the securities lending and equity market. J. Financial Mark. 15 (1), 81–107.

Liu, X., 2015. Short-selling attacks and creditor runs. Manage. Sci. 61 (4), 814–830.
Lucas, A., Schwaab, B., Zhang, X., 2014. Conditional Euro Area sovereign default risk. J. Bus. Econom. Statist. 32 (2), 271–284.
Marsh, I.W., Payne, R.G., 2012. Banning short sales and market quality: The U.K.'s experience. J. Bank. Financ. 36 (7), 1975–1986.
Matutes, C., Vives, X., 2000. Imperfect competition, risk taking, and regulation in banking. Eur. Econ. Rev. 44 (1), 1–34.
Miller, E.M., 1977. Risk, uncertainty, and divergence of opinion. J. Finance 32 (4), 1151–1168.
Rabemananjara, R., Zakoian, J.M., 1993. Threshold arch models and asymmetries in volatility. J. Appl. Econometrics 8 (1), 31–49.
Stern, G.H., Feldman, R.J., 2004. Too big to fail: The hazards of bank bailouts, first ed. Brookings Institution Press, Washington, D.C..
Supper, H., Irresberger, F., Weiss, G., 2020. A comparison of tail dependence estimators. European J. Oper. Res. 284, 728–742.