Short-Selling Bans and Bank Stability

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In both the subprime crisis and the eurozone crisis, regulators imposed bans on short sales mainly aimed at preventing stock price turbulence from destabilizing financial institutions. Contrary to the regulators' intentions, financial institutions whose stocks were banned experienced greater increases in the probability of default and volatility than unbanned ones. Increases were larger for more vulnerable financial institutions. To take into account the endogeneity of short sales bans, we match banned financial institutions with unbanned ones with similar sizes and levels of riskiness and instrument the 2011 ban decisions with regulators' propensity to impose a ban in the 2008 crisis. (*JEL* G01, G12, G14, G18)

> Unbridled short selling is contributing to the recent sudden price declines in the securities of financial institutions unrelated to true price valuation.
> —Security Exchange Commission, News Release, 2008-211

> On 18 September 2008 we introduced temporary short selling measures in relation to stocks in UK financial sector companies on an emergency

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basis … it was apparent that sharp share price declines in individual banks were likely to lead to pressure on their funding and thus create a self-fulfilling loop.

—Financial Service Authority, DP 09/1, p. 3

Most stock exchange regulators around the world reacted to the financial crisis of 2007–2009 by banning or restricting short sales. As illustrated by the two quotes in the epigraph, these interventions were presented as measures to curb unwarranted price drops that could destabilize financial institutions, and particularly banks. More recently, European regulators offered the same motivation for the short-selling bans imposed during the 2011–2012 eurozone sovereign debt crisis.[1] Hence, in both financial crises, short-selling bans appear to have been prompted by concerns about the stability of financial institutions, and primarily by the solvency of banks: regulators felt that these bans could protect them from being pushed closer to insolvency by speculative pressures on their stock prices. Indeed, in most countries, short-selling bans targeted primarily financial institutions.

In this paper, we investigate whether the short-selling bans imposed by regulators during those two financial crises succeeded in improving the perceived solvency of financial institutions and reducing the volatility of their stock prices. We also study whether the effects of the bans were stronger for banks that were most vulnerable in terms of solvency and liquidity mismatch. Finally, we seek to determine whether short-selling bans tended to support stock prices, consistently with a stabilizing impact on indicators of solvency and volatility.

We find that, contrary to the regulators' intentions, financial institutions whose stocks were banned experienced greater increases in the probability of default and volatility than unbanned ones, and these increases were larger for more vulnerable financial institutions. Moreover, short-selling bans did not appear to support the stock prices of financial institutions whose shares were banned.

In our analysis, we take into account that short-selling bans are not imposed randomly, but in situations of high stock price volatility and financial distress, so a mere correlation between short-selling bans and instability of financial institutions cannot be interpreted as a causal relationship. Specifically, to take the endogeneity of short-sale bans into account, we use two approaches.

[1] For example, in 2012, the Spanish regulator (CNMV) motivated its decision to maintain its 2011 ban by citing "uncertainties with respect to the Spanish financial system that may affect financial stability" and arguing that "failure to ban short sales would heighten uncertainty." It accordingly considered the ban "to be absolutely necessary to ensure the stability of the Spanish financial system and capital markets." See the CNMV document at www.cnmv.es/loultimo/prorroga%201%20nov_en.pdf.

First, we use matching techniques to overcome the sample selection arising from the fact that short-selling bans may specifically target larger and more vulnerable institutions. Second, to take into account that short sales bans are themselves triggered by extreme stock return volatility, we instrument the decision to enact the ban in the European debt crisis of 2011–2012 with a measure of the propensity of security regulators to impose short-sales bans, based on their choices in response to the systemic risk of financial institutions during the 2008 crisis. The rationale for this instrument is that this measure of the security regulators' policy rules, being based on their observed behavior three years before, can be seen as exogenous to indicators of the stability of financial institutions in 2011. The results show that short-sale bans are destabilizing for the financial institutions whose share are banned, even after controlling for endogeneity issues.

The focus of this paper differs from that of previous research on short-selling bans, which extensively investigated their effects on stock returns, liquidity, and price discovery (Battalio and Schultz 2011; Battalio, Mehran, and Schultz 2011; Beber and Pagano 2013; Boehmer, Jones, and Zhang 2013; Crane, Crotty, Michenaud, and Naranjo 2019; Marsh and Payne 2012), rather than their effects on financial stability. The only exceptions are the studies by Félix et al. (2016) and Arce and Mayordomo (2016), both of whom focus on the 2011 ban: the first finds that the ban increased the option-implied jump risk levels of financial stocks with listed options in the Belgian, French, Italian and Spanish markets, while the second shows that the ban moderated the solvency risk of Spanish banking institutions. Our study differs from these for its wider coverage, being based on data for two crises, several countries and various stability measures, as well as for its attention to endogeneity concerns.

Our work also can be seen as a test of predictions offered by the models of Brunnermeier and Oehmke (2014) and Liu (2015). In these frameworks, preventing short sales of a financial stock can avert a price fall induced by strategic short sellers, which would result in a self-fulfilling decline in the stock's value. Their argument is that short sales may result in a deterioration of funding conditions, because a declining share price may make it more difficult to raise new equity or debt capital; or it might make depositors' expectations converge on a bank-run equilibrium, with potential further repercussions on stock prices. The ban is seen as a way to break this perverse feedback loop, and, hence, is envisioned as a measure that can stabilize the fundamental value of the bank and thus its share price. Hence, these models view short-selling bans as affecting the fundamentals of stock prices, rather than just the price discovery process (for given fundamentals) as in previous literature (Diamond and Verrecchia 1987; Hong and Stein 2003; Miller 1977).

In Brunnermeier and Oehmke (2014) the mechanism that links stock price decline with bank insolvency is the likelihood that the bank will violate a leverage constraint, which limits the amount of funding that short-term creditors and uninsured depositors are willing to provide. When these constraints are violated or nearly violated, predatory short sellers that temporarily depress the share price can force the bank to dispose of long-term assets in order to pay creditors and prevent a run on the bank. In some circumstances, predatory short sellers can force the complete liquidation of assets, even though in their absence the bank could have complied fully with the leverage constraint.

In Liu (2015), instead, short-selling attacks can damage a bank by amplifying stock volatility, heightening uncertainty and increasing information asymmetry about the fundamentals. Since creditors base their evaluation of the bank's fundamental value on the share price, they become increasingly unsure about this value as share prices grow more volatile. With greater uncertainty, creditors are less willing to roll over their short-term loans, and if enough creditors call their loans back, a bank run happens, which triggers failure.

Both of these theories imply that institutions with sounder capital structures or stronger fundamentals should be less susceptible to unwarranted short sales and so less likely to fail. Moreover, given that both models posit short-term creditors as crucial agents, maturity and liquidity mismatching between assets and liabilities are likely to be a critical determinant of vulnerability. Mismatching is common to all financial institutions, but it varies significantly by type.

Thus, these theories deliver two hypotheses on the effect of short-sales bans that we can test exploiting the cross-sectional heterogeneity of firms' balance sheets at the industry and institution level. The first prediction is that the bans should significantly reduce the probability of default and stabilize the stock prices of banks compared to other financial institutions, banks being far more highly leveraged and more exposed to the risks of maturity mismatching and liquidity shocks.

A second prediction of Brunnermeier and Oehmke (2014) and Liu (2015) is that the effect of short sellers' actions on banks depends crucially on the vulnerability of the target banks: short selling should increase default probability, heighten volatility and depress stock prices more significantly in banks that are closer to the regulatory minimum capital ratio or feature greater liquidity mismatch between assets and liabilities. By the same token, a short-selling ban should benefit such fragile banks more than solid ones, and therefore should bolster their stock returns more strongly, lower their return volatility more substantially and prompt a sharper recovery in their perceived solvency.

As already mentioned, our empirical findings are inconsistent with both of these predictions: the evidence suggests that, if anything, short-selling bans are destabilizing, as they trigger further declines in the stock prices and perceived solvency of financial institutions. While the well-documented negative impact of bans on market liquidity may suffice to explain their depressing effect on stock prices, one must appeal to additional mechanisms to rationalize their detrimental effects on the solvency of financial institutions. One such mechanism might be that short-selling bans weaken the discipline imposed by markets on bank managers' risk-taking, by silencing the most skeptical investors. This is consistent with evidence that increases in the cost of short-selling reduce investors' ability to monitor managers and detect fraud (Fang, Huang, and Karpoff 2016; Massa, Zhang, and Zhang 2015).

1. The Data

We identify the effect of short-selling bans on banks' stability and stock prices by exploiting the cross-sectional variability between banks, other financial institutions and nonfinancial corporations during the two recent waves of short sale restrictions, namely, the bans enacted during the credit crisis of 2008–2009 and the European sovereign debt crisis of 2011–2012. This empirical framework is well suited for identification, in that banks, other financial institutions and nonfinancial companies were affected differently by the two crises and by short-selling bans. In 2008–2009, the United States, Canada, the United Kingdom, Switzerland, and Ireland imposed short-selling bans before most other countries; in the 2011–2012 sovereign crisis, short-selling bans were put on bank stocks in several (but not all) eurozone countries; and other countries have not enacted bans in either period. As a result, in each crisis we have a sizeable control sample of companies not subject to short-selling bans.

Our data cover 15,983 stocks in the first crisis (2008–2009) and 17,586 in the second crisis (2011–2012) for 25 countries: 17 European countries (13 eurozone and 4 noneurozone countries),[2] the United States, Australia, Canada, Japan, Hong Kong, Israel, New Zealand, and South Korea; hence, all the main developed countries. The data span the period from June 2008 to April 2012: to prevent confounding factors from clouding the potential effects of short-selling bans, we do not consider subsequent data. Our data are drawn from different sources: stock returns from Datastream, financial institutions' 5-year credit default

[2] The eurozone countries in the sample are Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, and Spain. The noneurozone European countries are Norway, Sweden, Switzerland, and the United Kingdom.

swap (CDS) quotes from Bloomberg and Datastream, and balance-sheet data from Bloomberg and SNL Financials.

We winsorize stock return data by eliminating the top and bottom 1% of the observations as well as zero returns (which presumably correspond to stale prices), so that the final sample for our return regressions comprises 13,473 stocks in the first crisis and 16,424 in the second one. These screens eliminate virtually all the observations that would be dropped using the protocols used by Hou, Karolyi, and Kho (2011) for stock returns drawn from Datastream.[3]

Estimates of firm-level probability of default (PD) over a 3-month horizon are calculated by the Risk Management Institute (RMI) at the National University of Singapore. These conditional PDs are estimated by the forward intensity model developed by Duan, Sun, and Wang (2012). Their reduced-form model permits firm-by-firm forecasts over a range of time horizons and is an extension of the hazard-rate approach in Duffie et al. (2007) and Lando and Nielsen (2010), but allows to estimate the PD over multiple periods using only data known at the time of the prediction, thus overcoming the difficulty of specifying and estimating the time dynamics for covariates. In the model used by the RMI, the input variables are the domestic stock index return and interest rate for all the firms in a given country, plus a set of ten firm-specific variables that are transformations of measures of six firm characteristics (volatility-adjusted leverage, liquidity, profitability, relative size, market misvaluation/future growth opportunities, and idiosyncratic volatility). The forward intensity approach actually coincides with the model by Duffie, Saita and Wang (2007) when the application is limited to the one-month-ahead prediction, and on U.S. data it performs similarly on short horizons, with 90% accuracy.

As for the volatility of stock returns, we rely on two different measures. The first is the square root of the 20-day moving average of squared stock returns, which we compute for all the stocks in our sample, including nonfinancial firms. The approach of measuring volatility using moving averages of daily squared returns corresponds to an Integrated-GARCH filter with zero intercept. Andersen et al. (2003) provide a general framework for volatility modeling, where they show that these simple GARCH filters appear to track the low-frequency variation adequately, matching the broad temporal movement in volatilities (for a related empirical study, see also Andersen and Bollerslev 1998). Our second measure of volatility is the square root of the daily variance of stock

[3] Since the protocol proposed by Hou, Karolyi, and Kho 2011 is designed for monthly data, we apply it to the monthly returns of the stocks in our data set and find that the protocol would lead to dropping a very small additional number of observations compared to the screen described in the text, namely, 0.03% (27 observations) of the sample in the first crisis and 0.02% (27 observations) in the second crisis.

returns estimated by the NYU Volatility Laboratory (V-Lab) using a GJR-GARCH(1,1) model as in Glosten, Jagannathan, and Runkle (1993). This volatility measure is available only for financial institutions.

The measures of financial institutions' leverage and banks' systemic risk are also provided by the NYU V-Lab. The leverage of financial institutions is defined as market value of equity plus the difference between the book value of assets and the book value of equity, all divided by the market value of equity. The systemic risk measure (labeled SRISK by NYU VLab) is an estimate of the capital shortfall (relative to the prudential capital ratio of 8%) that banks are expected to incur in the event that the broad stock market index falls by 40% over 6 months, based on Brownlees and Engle (2012) and Acharya, Engle, and Richardson (2012). Though produced from publicly available information, this estimate is conceptually similar to those obtained via stress tests by U.S. and European regulators, and takes account of the correlation between the value of the single bank's assets and that of the financial sector aggregate in a crisis. A bank's SRISK is a function of its initial leverage and an estimate of its "downside beta," that is, the sensitivity of the bank's equity value to large declines in the broad stock market index. We standardize this variable by the corresponding company's stock market capitalization, to compute the systemic risk per unit of asset: this normalization ensures that the results are not driven by the size of individual banks. Furthermore, following Acharya et al. (2012), we replace negative observations on this measure of systemic risk intensity by truncating the variable at zero, since negative equity shortfalls do not contribute to systemic risk. More than half of the observations on this variable are negative, which implies that systemic risk is concentrated in a minority of banks.

Finally, the dates when short sales bans were enacted and lifted and the characteristics of short-selling regimes are taken from the websites of national regulatory bodies and of the European Securities and Markets Authority (ESMA). For each country, we determine whether a short-selling ban was enacted and when, which stocks it applied to, and what restrictions it imposed. In particular, we distinguish between "naked" and "covered" bans: the former forbid only transactions in which the seller does not borrow the stock to deliver it to the buyer within the standard settlement period, whereas the latter also forbid covered short sales, that is, those in which the seller does borrow the stock.[4]

[Insert Table 1 - Panel A]

Table 1 describes our data set, separately for the two financial crises: panel A refers to the bans enacted in 2008, and panel B to those enacted

[4] Grünewald et al. (2010) describes the different types of short-selling restrictions and discusses the possible rationale behind each.

in 2011. In 2008, regulators often imposed both naked and covered bans, in several cases subsequently lifting the latter but retaining the former. We show the dates of imposition and revocation and the scope of the first ban imposed in each country, be it naked or covered. In 2011, all the new bans were covered bans, so the right panel shows the inception and lifting dates and the scope of covered bans only. In many of these countries the naked bans imposed in the previous financial crisis were still in force through 2011. The bans for which the table indicates an inception date but no lifting date were still in effect at the end of our sample period, April 30, 2012.

[Insert Table 1 - Panel B]

From the table, one can clearly observe great heterogeneity in the geographical area, timing, type, and scope of the bans in the two crises. First, in the 2008–2009 subprime crisis short-selling bans were much more widespread than in the 2010–2011 eurozone debt crisis. Moreover, in the former case regulators in the United States, Australia, Canada, Switzerland, and the United Kingdom imposed more stringent (i.e., covered) bans and moved faster than most other regulators, whereas in the latter only a handful of eurozone countries (Belgium, Greece, France, Italy, and Spain) and South Korea imposed covered bans. This accords with the fact that the subprime crisis had its epicenter in the United States and was more global in nature and impact than the eurozone debt crisis. Finally, some countries (Finland, Hong Kong, Israel, New Zealand and Sweden) imposed no ban in either crisis. The scope of the bans also varied from country to country and between episodes. In 2008, short sales were banned for all stocks in Greece, Italy, Spain, Australia, Japan, and South Korea, whereas they were banned only for financials (or a subset of financials) in other countries that imposed a ban; in 2011, the bans applied to all stocks in Greece, Italy, and South Korea, and to a subset of financials only in Belgium, France, and Spain.[5] This heterogeneity of geography, timing, and scope, combined with the availability of data for both 2008 and 2011 waves, allows for sufficient experimental variation and gives us a large group of nonbanned stocks to be used as a control group in each ban episode.

[Insert Table 2]

Table 2 shows descriptive statistics for banks, broken down by geographic area (the United States and the eurozone) and by period (June to December 2008 and May to November 2011), respectively. Specifically, the table reports the daily median values of stock returns; the volatility measure estimated from the GJR-GARCH(1,1) model; the three-month default probability obtained as in Duan, Sun, and Wang

[5] More precisely, Italy modified the scope of the bans in both crises, initially applying it to financials only and then extending it to all stocks (see the footnotes to Table 1, panel A).

(2012); leverage, defined as the sum of book value of debt and market value of equity over market value of equity; standardized SRISK, that is, capital shortfall for a given financial institution as a fraction of its stock market capitalization, whenever SRISK is positive; the Tier 1 ratio as a measure of regulatory capital, and the stable funding ratio, defined as the ratio of customers' deposits plus equity to long-term assets, to capture maturity mismatch between liabilities and assets; and finally, the CDS spread for the banks for which it is available.

In the entire sample, the overall median daily stock return was zero in both crises, and the median bank had similar leverage in both subperiods, even though it had more regulatory capital (as measured by the Tier 1 ratio) and less maturity mismatch between assets and liabilities during the second crisis. Regarding risk-related measures, the median bank's stock return variance and PD were higher in 2008 than in 2011, while the opposite applies to the median CDS premium and systemic risk (standardized SRISK).

2. The Results

Our objective is to assess the impact of short-selling bans on the stability of financial institutions in the two financial crises of 2008 and 2010–2011. We start by estimating simple panel regressions whose dependent variables are, alternatively, the probability of default, the CDS premium, the volatility and the level of stock returns, while the explanatory variables include dummies for the short-selling bans, stock-level fixed effects and, in stock return regressions, the market return of the corresponding country. We estimate these regressions on daily data, first for all stocks, then for financials only, and finally for banks only. All regressions are estimated separately for the two financial crises.

Next, to address problems of sample selection, we construct a matched sample of "banned" and exempt financial institutions. The matching, which is implemented via the coarsened matching algorithm proposed by Iacus, King, and Porro (2011), seeks to identify banks with similar characteristics in size (as measured by market capitalization) and insolvency risk (as measured by leverage and the regulatory capital ratio). We estimate a second set of panel regressions on the matched sample, again controlling for stock-level fixed effects.

Finally, to consider the potential endogeneity of the ban's enactment, we estimate instrumental variable (IV) regressions. The decision to enact a short-sales ban in the second crisis period is instrumented with the propensity of national security regulators to ban short sales of financial institutions' shares in response to their systemic risk during the first crisis. The idea behind this instrument is that the propensity of a given regulator to impose a ban is determined not only by the level of systemic risk featured by the financial institutions that it supervises but also by its aversion to systemic risk, so that the ban is triggered by a different level of systemic risk for different regulators. This measure of a regulator's propensity to ban short sales in response to a financial institution's systemic risk in the 2008 crisis is arguably a valid instrument for the 2011 short-sales ban decision by the same regulator.

2.1 Baseline estimates

Our first set of estimates address the question of whether short-selling bans reduce the probability of default of financial institutions, and of banks in particular, based on the estimates of panel regressions in which the respective dependent variables are the PD and the CDS premium. Each regression includes stock-level fixed effects, and two dichotomous variables that capture the presence of short-selling bans and their stringency: those forbidding only naked short sales (*Naked ban*), and those that also forbid covered short sales (*Covered ban*). The *Naked ban* variable equals 1 when only naked short sales are forbidden, *Covered ban* equals 1 when covered short sales are also forbidden. Therefore, the effect of *Naked ban* is measured by the observations for which the ban does not extend to covered short sales. The estimation is conducted separately for the first and second crises, allowing potentially different values in the two cases: columns 1–3 report the estimates from June to December 2008, and columns 4–6 report those from May to November 2011. For each subperiod three regressions are reported — for all stocks (columns 1 and 4), financial stocks only (columns 2 and 5), and bank stocks only (columns 3 and 6).

Table 3 shows that in the first crisis, the PD over a 3-month horizon increased for all stocks when subject to naked or covered bans (column 1), for financials under either type of ban (column 2), and for bank stocks under naked, but not covered, bans (column 3). In the second crisis, PD increased significantly for all stocks subject to covered bans (column 4), especially financials (column 5) and even more so bank stocks (column 6): comparing the coefficient in column 6 with that in column 4 indicates that the increase in PD associated with the 2011 ban is eight times greater for banks than for "banned" stocks in general. This is an interesting finding: that is, while regulators have imposed bans in order to stabilize banks, these appear to have featured a larger increase in solvency risk than other companies with the enactment of naked short-selling bans in the first crisis and of covered bans in the second. The magnitude of the coefficients indicates that these effects are also economically significant: compared to the sample medians of banks shown in Table 2, the PD of banks doubled in coincidence with the naked bans of 2008, and more than doubled concomitantly with the covered bans of 2011.

[Insert Table 3]

A similar qualitative pattern of results emerges from the panel estimates of Table 4, where the dependent variable is the CDS premium. Although the number of observations is much smaller than in Table 3, being limited by CDS data availability, the estimates indicate that the bans were also associated with significantly greater CDS premiums for all stocks in both crises. Moreover, CDS premiums increased significantly more for financials than for other stocks in both crises, as can be seen by comparing the estimates shown in columns 2 and 5 with the corresponding estimates in columns 1 and 4. As for the PDs, the economic magnitude of the estimated coefficients is large: benchmarking them against the corresponding sample medians in Table 2, the CDS premiums of banks increased by 56% and 45%, respectively, in response to the 2008 naked and covered bans, and by 92% in response to the covered bans of 2011, based on the estimates in columns 3 and 6.[6]

[Insert Table 4]

An equally consistent picture emerges also from the estimates of the volatility regressions in Table 5, which refer to the measure estimated from the GJR-GARCH(1,1) model for financial institutions only. Also, in this case, the coefficients of the short-selling ban variables are positive and statistically different from zero at the 1% significance level, both in the first crisis and in the second. Moreover, in this case, naked bans in 2008 coincide with a doubling of the volatility of bank stocks relative to their median value, and covered bans in 2011 with a 267% increase in their volatility. Table A1 in the appendix shows that very similar results are obtained when using the simpler volatility measure based on squared daily returns: this table, besides providing a robustness check of the estimates of Table 5 for financials and banks, shows that short-selling bans were associated with an increase in volatility also for nonfinancial stocks. Félix et al. (2016) document that also option-based implied volatility measures increased in coincidence with the 2011 short-selling bans on eurozone stocks featuring option markets.

[Insert Table 5]

In summary, all the baseline regressions indicate that short-selling bans are associated with significant increases in risk measures. Moreover, the naked ban in the first crisis and the covered ban in the second were associated with a larger increase in the perceived insolvency risk of banks compared to other firms. This overall pattern is mirrored in

[6] As an example, the impact of the 2008 ban is obtained by dividing the coefficient in column 3 of Table 4 (0.0049) by the median CDS spread in the first column of Table 2 (0.0105).

the response of stock prices to the bans, shown in Table 6: the bans were associated with an overall decline in stock returns, and the decline was larger for bank stocks than for other stocks in coincidence with naked bans in the first crisis and with covered ones in the second. This evidence appears inconsistent with the thesis by Brunnermeier and Oehmke (2014) that short-selling bans can support bank shares by deterring predatory trading and by Liu (2015) that they should reduce their price volatility. It is also inconsistent with Miller 1977, who argued that in general short-selling bans should support share prices by suppressing the trades of the most pessimistic investors.

[Insert Table 6]

A natural question is whether the increase of PDs and stock price volatility in response to short-selling bans are just reflections of the bans negative impact on price discovery and market liquidity, which already have been extensively documented by other studies, such as Battalio and Schultz (2011), Beber and Pagano (2013), and Boehmer, Jones, and Zhang (2013), or whether they point to an additional direct effect of bans on stock fundamentals, particularly for financials, though opposite in sign to the predictions of Brunnermeier and Oehmke (2014) and Liu (2015). In principle, by suppressing valuable negative information in the price discovery process, short-selling bans may increase the uncertainty of investors and reduce stock market liquidity, resulting in a drop of equilibrium stock prices. In turn, the lower stock prices may increase the market leverage of the corresponding firms and thus increase their PDs and price volatility; the latter may also increase because of the greater bid-ask bounce associated with wider bid-ask spreads. This line of reasoning may also explain why the response of volatility and PDs was greater for financials, and banks in particular: the suppression of negative information may have created more uncertainty regarding the value of financials, which were at the center of the crisis.

To investigate whether this interpretation of the results is warranted, in columns 1 and 2 of Table 7 we expand the specification of the PD regressions for financials by controlling for the contemporaneous return of the corresponding stock: the estimates shown in columns 1 and 2, which refer to the first and the second crisis, respectively, show that the coefficients of the ban dummies are almost identical to those of the comparable regressions in columns 2 and 5 of Table 3, even though the coefficients of stock returns are strongly significant and negative, in accordance with intuition. Similar results are obtained controlling for lagged stock returns (up to one week) rather than contemporaneous ones: these results are not reported for brevity. The fact that the estimated coefficients of the ban dummies are almost unaffected in this expanded specification indicates that the increase of PDs in response to short-selling bans is not just a mechanical implication of the drop in stock prices via changes in leverage. In other words, short-selling bans appear to convey bad news about the perceived solvency of financial institutions, over and above the impact that they have on stock returns. This may be the case, for instance, because short-selling bans weaken the discipline imposed by markets on bank managers risk-taking, consistently with evidence that increases in the cost of short-selling reduce investors ability to monitor managers (Fang, Huang, and Karpoff 2016; Massa, Zhang, and Zhang 2015).

[Insert Table 7]

Columns 3 and 4 of Table 7 present a similar robustness check for the volatility regressions, by including not only the corresponding stocks return but also its illiquidity (measured by the contemporaneous value of the relative bid-ask spread) as additional controls. Illiquidity turns out to be positively and significantly correlated with stock return volatility, possibly reflecting the impact of the bid-ask bounce. However, the estimated coefficients of the ban variables are still precisely estimated and similar to the baseline estimates in columns 2 and 4 of Table 5. Also, in this case, similar results are obtained by controlling for lagged values of the stock return and illiquidity (again, not reported for brevity).

As a further robustness check, in Table A2 of the appendix the specifications of Table A1 are reestimated using the volatility of weekly returns rather than that of daily returns as dependent variable, so as to reduce even further the possible role of the bid-ask bounce as a determinant of stock price volatility: the ban coefficients are still positive and significant. Hence, the response of volatility to short-selling bans is not just mechanically driven by the response of prices and illiquidity documented in previous studies.

2.2 Estimates obtained from matched samples

A possible objection to the results in Section 2.1 is that the stocks subject to short-selling bans differ from those that were exempt. In particular, bans may be targeted mainly to the financial institutions that are the most fragile owing to their greater leverage or maturity mismatch, rather than to randomly selected ones. Indeed, policy makers should have the incentive to apply bans in this selective fashion if they hold the belief that bans can stabilize financial institutions, as witnessed by the quotes in the epigraph of this paper. If so, the results reported above are vitiated by sample selection bias.

To address this selection concern, we match the observations for each financial institution whose stock was subject to a ban with those for another financial institution with similar characteristics in size and riskiness, but not subjected to a ban. For each financial institution subject to a short-selling ban, we identify nonbanned stocks within the same category (banks, insurance companies, financial service companies, real estate firms) whose issuers are closest to it in (a) market capitalization, (b) core tier 1 capital ratio, and (c) leverage.

The matching is implemented via the coarsened matching (CEM) algorithm proposed by Iacus, King, and Porro (2011), which proceeds in three steps. First, the data are temporarily coarsened by defining bin intervals, called “strata, according to the three above-listed variables chosen as matching criteria.[7] Second, we carry out an exact matching on the coarsened data by retaining all the strata with at least one treated and one control observation (i.e., a banned and a nonbanned financial institution) and discarding the others. Third, we only use the retained observations in the estimation, weighting them by the size of the corresponding “stratum size. Hence, this method allows for more than a single control observation to be matched to a single treated observation, and vice versa, but corrects the potential imbalance of observations using these weights.

The matching algorithm is the same for the two crises, but the matching is done separately for each, since the institutions characteristics could have changed in the meantime. We measure the average characteristics of treated and control financial institutions in June, July and August 2008 for the first wave, and in April, May and June of 2011 for the second wave. Table 8 illustrates the results of the matching algorithm separately for the two crises. In the first crisis (top panel), the algorithm results in a sample of 1,034 treated and 935 control financial institutions, starting from two subsamples of 1,419 treated and 999 control observations. In the second crisis (bottom panel), it results in a sample of 165 treated and 1,617 control financial institutions, starting from 194 treated and 2,465 control observations, reflecting the much more limited scope of the covered ban in the second crisis. The quality of the matching is highlighted by the improvement in the similarity of the three chosen characteristics for the treated and control groups in both crises: banned financial institutions are significantly more levered and larger than nonbanned ones in both crises, and feature significantly lower regulatory capital in the second crisis; but after the matching, the two subsamples are not significantly different in any of these three dimensions.

[Insert Table 8]

Table 9 shows the results from estimating the effects of the bans on the PD, volatility and stock returns (i.e., the specifications of Tables 3, 5, and 6) on the sample of financial institutions resulting from our matching procedure. Owing to the relatively small size of the sample, we now use a single ban variable, equal to 1 whenever a short-selling ban (whether naked or covered) was enacted and 0 otherwise. In the

[7] The number and width of bins are chosen by applying Sturges rule (1926).

2011 crisis, as noted above, this variable coincides with the covered ban dummy. Columns 1–3 present the estimates for the 2008 crisis in regressions where the dependent variables are PD, volatility and stock return, respectively; columns 4-6 show the corresponding estimates for the 2011 crisis. In the PD and volatility regressions of columns 1-2 and 4-5, we also control for the stocks own return, as in Table 7, in order to focus on the effect of short-selling bans that does not arise mechanically from their effects on the stock price. In these matched sample regressions too, short-selling bans are associated with significantly greater volatility, higher probability of default and lower stock returns, in both crises. The magnitudes of the coefficients are very close to the estimates for the full sample of financial institutions in columns 2 and 5 of Tables 3, 5, and 6, respectively. This indicates that the baseline estimates reported in those tables are not significantly affected by selection bias.

[Insert Table 9]

We use our matched sample also to test a prediction specific to the Brunnermeier and Oehmke (2014) model, exploiting cross-sectional differences in the fragility of financial institutions. Recall that in this model short-selling bans should stabilize particularly the most vulnerable financial institutions. Hence, we reestimate the regressions in Table 9 with the addition of an interaction between the ban dummy and a dummy for financial vulnerability, which is equal to 1 for the institutions with greater than median vulnerability and 0 for the others. This interaction variable allows the coefficient of the short-selling ban to take a different sign for more vulnerable institutions. We measure vulnerability alternatively by one of the following four variables (measured as of May to June 2008 for the first crisis, and April to May 2011 for the second): (a) leverage, (b) systemic risk (SRISK), (c) the (negative of the) Tier 1 capital ratio (T1), and (d) the (negative of the) "stable funding ratio," to capture maturity mismatch between liabilities and assets. Of course, since the last two indicators apply only to banks, the regressions involving them are estimated only for banking stocks.

Table 10 reports the estimates separately for default probability (panel A) and return volatility (panel B). In each panel, vulnerability is measured with leverage in columns 1 and 2, systemic risk in columns 3 and 4, the T1 capital ratio in columns 5 and 6, and the stable funding ratio in columns 7 and 8. Each column refers to one of the two crises.

[Insert Table 10]

The results indicate that short-selling bans were associated with even greater probability of default and stock return volatility for more vulnerable financial institutions than for others. In particular, in the PD regressions in panel A, the coefficients of the interaction with all the vulnerability indicators are positive and significantly different from zero for both crises, implying that after the introduction of the bans the probability of default rose significantly more for the banks with above-median leverage and systemic risk, below-median Tier 1 capital ratios and above-median maturity mismatch between assets and liabilities. The impact of short-selling bans on the PD of the more vulnerable institutions is larger than the corresponding impact for stronger institutions. For instance, focusing on the estimates for the 2011 crisis (shown in even columns), the impact of the bans on the PD was 2.5 times larger for the more highly leveraged banks, 3.5 times larger for those with more systemic risk, 2.75 for those with less regulatory capital, and 9 times larger for those with greater maturity mismatch between assets and liabilities.[8] Panel B of the table shows qualitatively similar, but quantitatively smaller results for volatility: in both crises, the ban was associated with a larger increase in the volatility of stock returns for more fragile and unstable financial institutions, especially during the eurozone debt crisis. Hence, we do not find evidence in either crisis to support the hypothesis that bans on short sales reinforce less-capitalized banks or more fragile financial institutions in general.

2.3 Instrumental variables estimates

While the matching method described in Section 2.2 addresses the possible selection bias arising from the regulators' choice of the banned stocks, it does not address the possible endogeneity arising from the regulator's decision to impose a ban. If regulators impose short-selling bans when financial companies are particularly distressed, and feature abnormally high return volatility or steep price declines, the correlation between short-selling bans and bank instability documented so far cannot be interpreted as a causal relationship. Indeed, the causality could run the other way, from the rise in volatility, the increase in default risk or the drop in stock prices to the bans. To address this concern, we estimate an instrumental variables (IV) regression for the stocks of financial institutions in the second crisis, where the first stage is a linear probability model determining the likelihood of a ban and the second stage models the ban's effects on volatility, probability of default and stock returns.

The presence of two distinct waves of short-selling bans in our data, each triggered by a specific crisis, enables us to attack this identification problem by using the data generated by the first crisis to infer the propensity of regulators to impose a short-selling ban in the second crisis. Specifically, we denote by $srisk_{jc}^{*}$ the threshold level of systemic risk of stock j above which the regulator of country c chose to impose

[8] As an example, the effect of the ban for institutions with above-median leverage is 0.0005, that is, the sum of the two coefficients in column 2 of Table 10. Dividing this figure by the coefficient for institutions with below-median leverage, that is, 0.0002, yields 2.5.

the first short-selling ban (whether naked or covered) on stock j during the first crisis, and infer the policy rule that accordingly it should have followed in the second crisis by the following indicator function:

$$ban\_rule_{jct} = \begin{cases} 1 \text{ if } srisk_{jct} \geq srisk^*_{jc}, \\ 0 \text{ otherwise.} \end{cases} \tag{1}$$

The variable defined by (1) is supposed to capture the propensity of regulator c to impose a short-selling ban on stock j during the second crisis, as it equals 1 if the systemic risk level $srisk_{jct}$ (as measured by the standardized SRISK variable) would have triggered a ban in the first crisis, and equals zero otherwise. For the stocks that were not banned in the first crisis the threshold is set equal to the highest level of systemic risk achieved during the first crisis.

Our instrument exploits not only the different timing of bans across countries but also the fact that in several countries bans were selectively imposed across financial stocks, rather than on all of them at the same time. For instance, in Austria, Belgium, France, Germany, and the Netherlands, only a fraction (between 6% and 14%) of financial stocks was affected by the short-selling ban. Even in the United States, the SEC emergency order of September 18, 2008 (Release No. 34-58592), prohibited short sales "in the publicly traded securities of *certain* financial firms, which entities are identified in appendix A ('Included Financial Firms')" [emphasis added]: indeed it banned short sales only for 472 stocks out of 558 financial stocks. This is why our instrument is not based on an aggregate measure, but on a stock-by-stock measure of systemic risk. (However, as explained below, for robustness we also consider an alternative instrument based on the idea that the decisions to impose short-selling bans were based on aggregate country-level measures of financial instability.)

We use the ban_rule_{jct} variable to instrument the decision to enact the ban in the second crisis. More precisely, using data for the 2011 sample, we estimate the following first-stage regression:

$$d_{jct} = \alpha_j + \beta_1 ban\_rule_{jct} + \beta_2 srisk_{jct} + \beta_3 r_{ct} + \epsilon_{jct}, \tag{2}$$

where the ban dummy d_{jct} is 1 if stock j is banned by the regulator of country c at time t, and 0 otherwise, $srisk_{jct}$ is the systemic risk of company j and r_{ct} is the market return of country c at time t (the latter variable being included only in the regression for individual stock returns). Our instrument varies not only across stocks but also across regulators (for the same stock and level of systemic risk) and over time (being a function of systemic risk), which avoids perfect collinearity with the stock-level fixed effects.

The validity of this instrument rests on the exogeneity of the regulator's preferences, namely, the assumption that the threshold level for systemic risk used by a regulator in its policy rule (1) during the first crisis is not affected by the probability of default, volatility or stock return of company j in the second crisis, once one controls for that company's systemic risk $srisk_{jct}$. It is important to realize that we do not assume $srisk_{jct}$ *per se* to be exogenous: it may well respond to company j's probability of default, volatility or stock return. Our identifying restriction is instead that the nonlinear impact of $srisk_{jct}$ on the ban enactment via the threshold policy rule is exogenous to that institution's solvency risk, volatility, and stock return once the linear impact of $srisk_{jct}$ is accounted for.

Table 11 reports the IV estimates. The first-stage estimates are reported in the odd-numbered columns, and the corresponding second-stage estimates are in the even-numbered columns. Columns 1 and 2 refer to the PD regression, columns 3 and 4 to the volatility regression, and columns 5 and 6 to the stock return ones.[9] The first-stage estimates indicate that the instrument is relevant, as its coefficient is significantly different from zero and the first-stage F-test statistic exceeds 13 in all specifications. Moreover, the estimated coefficient of the instrument has the expected sign: $\beta_1 > 0$. The second-stage estimates confirm the qualitative results of ordinary least squares (OLS) estimation conducted on the whole panel in Tables 3, 5, and 6, and on the matched sample in Table 9: the covered bans imposed in the second crisis appear to have increased the conditional default probability, the volatility and the drop in stock prices of the relevant financial institutions.

[Insert Table 11]

Indeed, short-sale bans appear to be even more destabilizing once the endogeneity of the policy response is taken into account, as the IV estimates of the bans' effects exceed the corresponding OLS estimates: for instance, in the volatility regression the covered ban's coefficient is 0.0127 in column 4 of Table 11, to be compared with the OLS estimate of 0.0011 in column 3 of Table 5.

A possible concern about the above IV strategy is that it assumes that each regulator triggers the ban for each stock based on a stock-specific threshold for its systemic risk, rather than in response to an aggregate, country-level measure of financial instability. To allay this concern, we adapt our instrument by assuming that the threshold used by each regulator is calibrated on the mean value of systemic risk for the financial companies in the relevant country, computed on the first day in which the regulator of country c imposed a short-selling ban (whether naked or covered) during the first crisis. Hence, the instrument becomes

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[9] Even though the specification of the first-stage regressions in columns 1 and 3 are identical, theirs coefficients differ because they are estimated on different samples, due to different data availability for the PD and volatility.

a country-time dummy that equals 1 if during the second crisis the mean systemic risk for the financial companies of country c at time t exceeds this threshold, and 0 otherwise. The results obtained using this alternative IV strategy, which are presented in Table A3 of the appendix, are similar to those shown in Table 11, the only difference being that the ban dummy coefficient estimates are smaller.

3. Conclusions

Previous research has shown that the bans on short sales in 2008–2009 reduced market liquidity, slowed price discovery, and were ineffective in supporting stock prices. Yet this dismal outcome did not deter a number of European Union (EU) regulators from a new wave of short-selling bans on financials when the European debt crisis broke out in 2010. In both crises, the main motivation for the bans offered in the regulatory debate was the danger that a collapse of bank shares could engender funding problems or even a full-fledged bank run.

This paper tests whether bans on short sales of bank stocks do stabilize vulnerable banks at times of market stress. We test this hypothesis by scrutinizing the evidence produced by the crises of 2008–2009 and 2010–2012. To assess empirically whether and how bans affect bank stability, we compare the evolution of solvency measures, volatility, and stock returns for a large set of corporations, specifically financial institutions and banks, only a subset of which were subject to the bans either once or repeatedly.

Our evidence indicates that short-selling bans are not associated with greater bank stability. In fact, our estimates, even controlling for the endogeneity of the bans, point to the opposite result, namely, that bans on short sales tend to be correlated with a higher probability of default, greater return volatility, and steeper stock price declines, particularly for banks. A possible interpretation of these detrimental effects of short-selling bans is that they weaken the discipline imposed by markets on bank managers' risk-taking, by silencing investors most critical of their strategies.

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Table 1
Structure of the data set

Country	Start date of ban	Lift date of ban	Scope of the ban	Stocks in 2008	Stocks with a covered ban in 2008	Stocks with a naked ban in 2008
Austria	Oct 26, 2008		4 financials	85	0	4
Belgium	Sep 22, 2008		4 financials	140	0	3
Denmark	Oct 13, 2008		47 financials	134	0	47
Finland			No ban	107	0	0
France	Sep 22, 2008	Feb 1, 2011	10 financials	612	0	9
Germany	Sep 22, 2008	Jan 31, 2010	11 financials	576	0	11
Greece	Oct 10, 2008	Jun 1, 2009	All stocks	214	0	214
Ireland	Sep 19, 2008	Dec 31, 2011	5 financials	42	5	0
Italy	Sep 22, 2008*	Jul 31, 2009	50 financials, then all	185	0	35
Luxembourg	Sep 19, 2008		15 financials	25	0	12
Netherlands	Sep 22, 2008	Jun 1, 2009	8 financials	98	0	5
Norway	Oct 8, 2008	Sep 28, 2009	5 financials	43	4	0
Portugal	Sep 22, 2008		8 financials	42	0	3
Spain	Sep 24, 2008		All stocks	132	0	132
Sweden			No ban	314	0	0
Switzerland	Sep 19, 2008	Jan 16, 2009	Financials	220	72	148
U.K.	Sep 19, 2008	Jan 16, 2009	Financials	712	142	0
U.S.	Sep 19, 2008	Oct 8, 2008	Financials	2,311	472	0
Australia	Sep 22, 2008	Nov 19, 2008**	All stocks	1,402	1,402	0
Canada	Sep 19, 2008	Oct 8, 2008	Financials	2,478	9	0
Japan	Oct 30, 2008		All stocks	3,217	0	3,217
Hong Kong			No ban	1,061	0	0
Israel			No ban	444	0	0
New Zealand			No ban	111	0	0
South Korea	Oct 1, 2008	Jun 1, 2009***	All stocks	1,278	1,278	0
Totals				15,983	3,384	3,840

* The ban initially applied to financials and was extended to all stocks on October 10, 2008. ** On November 19, 2008, only the covered ban on nonfinancials was lifted. *** On June 1, 2009, only the covered ban on nonfinancials was lifted.

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Table 1
Structure of the data set, continued

Country	Start date of covered ban	Lift date of covered ban	Scope of the covered ban	Stocks in 2011	Stocks with a covered ban in 2011
Austria			No ban	89	0
Belgium	Aug 12, 2011	Feb 13, 2012	4 financials	133	3
Denmark			No ban	116	0
Finland			No ban	91	0
France	Aug 12, 2011	Feb 11, 2012	10 financials	634	9
Germany			No ban	745	0
Greece	Aug 9, 2011		All stocks	221	221
Ireland			No ban	43	0
Italy	Aug 12, 2011*	Feb 24, 2012**	29 financials, then all	197	21
Luxembourg			No ban	24	0
Netherlands			No ban	75	0
Norway			No ban	44	0
Portugal			No ban	43	0
Spain	Aug 12, 2011	Dec 16, 2012	Financials	152	9
Sweden			No ban	353	0
Switzerland			No ban	237	0
U.K.			No ban	766	0
U.S.			No ban	2,499	0
Australia			No ban	1,601	0
Canada			No ban	2,927	0
Japan			No ban	3,311	0
Hong Kong			No ban	1,223	0
Israel			No ban	468	0
New Zealand			No ban	117	0
South Korea	Aug 10, 2011	Nov 9, 2011***	All stocks	1,477	1,477
Totals				17,586	1,740

* The ban initially applied to financials and was extended to all stocks on December 1, 2011. ** On February 24, 2012, only the covered ban on financials was lifted. *** On November 9, 2011, only the covered ban on nonfinancials was lifted.

Table 2
Banks in the two crises: Descriptive statistics, 2008 and 2011

	June to December 2008			May to November 2011		
Variable name	All countries	U.S.	Eurozone	All countries	U.S.	Eurozone
Returns	0.0000	0.0000	-0.0026***	0.0000	0.0000	-0.0005***
Daily volatility	0.0012***	0.0017***	0.0008***	0.0006***	0.0007***	0.0008***
Default probability	0.0011***	0.0009***	0.0015***	0.0005***	0.0003***	0.0012***
CDS spread	0.0105***	No obs.	0.0103***	0.0220***	0.0044***	0.031***
Leverage	10.6804***	9.6411***	18.1233***	11.5753***	10.8503***	27.3484***
Standardized SRISK	0.6020***	0.3134***	1.0154***	0.7064***	0.4134***	1.5692***
Tier 1 ratio	9.7400***	10.3000***	8.6000***	12.0900***	13.5800***	11.0000***
Stable funding ratio	0.8723***	0.8664***	0.5960***	0.9555***	0.9630***	0.5948***

The table shows the medians of several bank variables, broken down by crisis and by three geographical areas: all countries, the United States, and eurozone (12 countries). *** on the coefficient indicates that the median is significantly different from zero at the 1% confidence level, using a nonparametric Wilcoxon test.

Table 3
Probability of default and short-selling bans for all stocks, financials, and banks

	(1)	(2)	(3)	(4)	(5)	(6)
Naked ban	0.0007***	0.0015***	0.0011***			
	(223.70)	(82.05)	(49.88)			
Covered ban	0.0008***	0.0008***	0.0000	0.0001***	0.0005***	0.0008***
	(149.84)	(58.23)	(1.38)	(75.24)	(35.55)	(27.91)
Constant	0.0012***	0.0018***	0.0019***	0.0005***	0.0007***	0.0010***
	(1,052.80)	(505.75)	(321.37)	(1545.60)	(543.31)	(434.41)
Adjusted R^2	0.62	0.65	0.65	0.79	0.81	0.81
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample period	First crisis	First crisis	First crisis	Second crisis	Second crisis	Second crisis
Stocks included	All	Financial	Bank	All	Financial	Bank
Number of stocks	13,131	2,062	585	13,829	2,125	585
Observations	1,826,665	279,618	81,687	1,969,634	299,769	82,440

The dependent variable is the firm's probability of default at a 3-month horizon. Naked ban is a dummy variable that equals 1 if only naked short sales are forbidden and 0 otherwise. Covered ban is a dummy variable that equals 1 if even covered short sales are forbidden and 0 otherwise. Market return is the return on the market index of each country. Regressions in columns 1, 2, and 3 are estimated using daily data for the first crisis (June to December 2008). The estimates in column 1 are based on data for all stocks; those in column 2 are based on data for financial stocks only; and those in column 3 are based on data for bank stocks only. Regressions in columns 4, 5, and 6 are estimated using daily data for the second crisis (May to November 2011). The estimates in column 4 are based on data for all stocks; those in column 5 are based on data for financial stocks only; and those in column 6 are based on data for bank stocks only. The estimates are based on fixed effects panel regressions with robust standard errors and report t-statistics in parentheses. ***$p < .01$.

Table 4
CDS and short-selling bans for all stocks, financials, and banks

	(1)	(2)	(3)	(4)	(5)	(6)
Naked ban	0.0104***	0.0139***	0.0061***			
	(87.54)	(33.06)	(14.59)			
Covered ban	0.0131***	0.0163***	0.0049***	0.0126***	0.0189***	0.0204***
	(24.51)	(17.18)	(16.90)	(50.59)	(51.65)	(48.12)
Constant	0.0150***	0.0159***	0.0125***	0.0177***	0.0265***	0.0349***
	(238.37)	(117.28)	(97.13)	(484.48)	(192.84)	(121.67)
Adjusted R^2	.64	.55	.64	.86	.87	.87
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample period	First crisis	First crisis	First crisis	Second crisis	Second crisis	Second crisis
Stocks included	All	Financial	Bank	All	Financial	Bank
Number of stocks	448	91	39	446	91	42
Observations	60,320	11,837	5,386	61,847	11,960	5,512

The dependent variable is the 5-year CDS spread. Naked ban is a dummy variable that equals 1 if only naked short sales are forbidden and 0 otherwise. Covered ban is a dummy variable that equals 1 if even covered short sales are forbidden and 0 otherwise. Market return is the return on the market index of each country. Regressions in columns 1, 2 and 3 are estimated using daily data for the first crisis (June-December 2008). The estimates in column 1 are based on data for all stocks; those in column 2 are based on data for financial stocks only; and those in column 3 are based on data for bank stocks only. Regressions in columns 4, 5, and 6 are estimated using daily data for the second crisis (May to November 2011). The estimates in column 4 are based on data for all stocks; those in column 5 are based on data for financial stocks only; and those in column 6 are based on data for bank stocks only. The estimates are based on fixed effects panel regressions with robust standard errors and report t-statistics in parentheses. ***$p < .01$.

Table 5
Stock return volatility and short-selling bans for financials and banks

	(1)	(2)	(3)	(4)
Naked ban	0.0014***	0.0016***		
	(58.68)	(47.58)		
Covered ban	0.0019***	0.0010***	0.0011***	0.0016***
	(76.66)	(24.89)	(34.87)	(35.19)
Constant	0.0023***	0.0020***	0.0013***	0.0010***
	(350.65)	(189.47)	(419.76)	(212.12)
Adjusted R^2	.43	.40	.69	.57
Stock FE	Yes	Yes	Yes	Yes
Sample period	First crisis	First crisis	Second crisis	Second crisis
Stocks included	Financial	Bank	Financial	Bank
Number of stocks	1,424	424	1,646	440
Observations	156,153	46,824	204,494	56,102

The dependent variable is the stock return volatility estimated using a GJR-GARCH(1,1) model. *Naked ban* is a dummy variable that equals 1 if only naked short sales are forbidden and 0 otherwise. Covered ban is a dummy variable that equals 1 if even covered short sales are forbidden and 0 otherwise. All regressions are estimated using daily data for financial stocks during the first crisis (June to December 2008) in column 1, and only bank stocks for the same period in column 2; using financial stocks during the second crisis (May to November 2011) in column 3, and only bank stocks for the same period in column 4. The estimates are based on fixed effects panel regressions with autoregressive residual and report t-statistics in parentheses. ***$p < .01$.

Table 6
Stock returns and short-selling bans for all stocks, financials, and banks

	(1)	(2)	(3)	(4)	(5)	(6)
Naked ban	-0.0014***	-0.0016***	-0.0042***			
	(-5.33)	(-2.94)	(-4.27)			
Covered ban	-0.0021***	-0.0019***	-0.0019***	-0.0019***	-0.0016**	-0.0029***
	(-12.06)	(-6.54)	(-3.38)	(-3.17)	(-2.45)	(-2.84)
Market return	0.6479***	0.5865***	0.5922***	0.7198***	0.6280***	0.7952***
	(363.90)	(150.82)	(86.13)	(462.86)	(195.08)	(118.20)
Constant	-0.0032***	-0.0024***	-0.0012***	-0.0014***	-0.0010***	-0.0008***
	(-87.91)	(-30.38)	(-7.89)	(-65.92)	(-19.21)	(-8.34)
Adjusted R^2	.09	.12	.15	.10	.13	.21
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample period	First crisis	First crisis	First crisis	Second crisis	Second crisis	Second crisis
Stocks included	All	Financial	Bank	All	Financial	Bank
Number of stocks	13,473	2,390	577	16,654	2,897	669
Observations	1,854,942	311,209	82,684	2,361,811	395,759	98,384

The dependent variable is the individual stock return. Naked ban is a dummy variable that equals 1 if only naked short sales are forbidden and 0 otherwise. Covered ban is a dummy variable that equals 1 if even covered short sales are forbidden and 0 otherwise. Market return is the return on the market index of each country. Regressions in columns 1, 2, and 3 are estimated using daily data for the first crisis (June to December 2008). The estimates in column 1 are based on data for all stocks; those in column 2 are based on data for financial stocks only; and those in column 3 are based on data for bank stocks only. Regressions in columns 4, 5, and 6 are estimated using daily data for the second crisis (May to November 2011). The estimates in column 4 are based on data for all stocks; those in column 5 are based on data for financial stocks only; and those in column 6 are based on data for bank stocks only. The estimates are based on fixed effects panel regressions with robust standard errors and report t-statistics in parentheses. ***$p < .01$.

Table 7
Robustness: Augmented specifications for financial stocks

	(1)	(2)	(3)	(4)
Naked ban	0.0015***		0.0015***	
	(81.98)		(60.92)	
Covered ban	0.0008***	0.0005***	0.0019***	0.0011***
	(56.43)	(35.27)	(76.53)	(33.60)
Return	-0.0017***	-0.0009***	0.0010***	0.0006***
	(-15.69)	(-18.50)	(5.78)	(4.89)
Illiquidity			0.0021***	0.0011***
			(14.94)	(9.10)
Constant	0.0018***	0.0007***	0.0019***	0.0012***
	(500.17)	(538.13)	(198.62)	(152.14)
Adjusted R^2	.65	.81	.42	.69
Dependent variable	PD	PD	Daily volatility	Daily volatility
Sample period	First crisis	Second crisis	First crisis	Second crisis
Stock FE	Yes	Yes	Yes	Yes
Number of stocks	2,062	2,124	1,279	1,467
Observations	274,014	294,020	145,300	174,458

The dependent variable is the probability of default in columns 1 and 2 and the volatility of daily returns based on a GJR-GARCH(1,1) model in columns 3 and 4. Ban is a dummy variable that equals 1 if naked or covered short sales are forbidden. 0 otherwise, during the first crisis and equals 1 if covered short sales are forbidden and 0 otherwise, during the second crisis. Return is the contemporaneous return of the relevant stock. Illiquidity is measured by the relative bid-ask spread of the relevant stock. All regressions are estimated using daily data for the stocks of financial institutions in the first crisis (June to December 2008) and in the second crisis (May to November 2011), respectively. The estimates are based on fixed effects panel regressions with robust standard errors and report t-statistics in parentheses. $^{***}p < .01$.

Table 8
Statistics on matched samples

	First crisis				
	Treated group		Control group		
	Mean	SD	Mean	SD	Diff.
Leverage	6.05	7.54	3.63	6.04	2.43***
Market cap.	1,596	4,044	1,243	3,092	352*
Tier 1-RW	10.47	3.09	9.67	3.15	0.80*
Observations	1,419		999		
	Matched treated group		Matched control group		
Leverage	2.08	2.84	2.15	2.87	-0.07
Market cap.	835	2203	816	2185	19
Tier1 RW	8.88	1.64	9.05	1.67	-0.17
Observations	1,034		935		
	Second crisis				
	Treated group		Control group		
	Mean	SD	Mean	SD	Diff.
Leverage	10.03	12.79	5.50	8.52	4.53 ***
Market cap.	2,597	4,749	1,149	2,951	1,448 ***
Tier 1 RW	10.95	3.29	13.00	4.39	-2.05***
Observations	194		2,465		
	Matched treated group		Matched control group		
Leverage	6.16	9.74	6.03	9.73	0.14
Market cap.	1314	3163	1230	3239	84
Tier 1-RW	10.66	1.58	11.12	1.38	-0.46
Observations	165		1,617		

The table reports the mean and standard deviation of leverage, market capitalization, and Tier 1 capital for financial institutions included in the group of banned stocks (the treated group) and that of unbanned ones (the control group), before and after matching, separately for the two crisis episodes.

Table 9
Effects of bans on PD, volatility, and stock returns for matched financial institutions

	(1)	(2)	(3)	(4)	(5)	(6)
Ban	0.0012***	0.0019***	-0.0024***	0.0005***	0.0009***	-0.0017**
	(91.24)	(80.28)	(-8.16)	(31.46)	(24.05)	(-2.43)
Market return			0.4817***			0.5746***
			(62.16)			(90.87)
Return	-0.0013***	0.0007***		-0.0009***	0.0002	
	(-9.90)	(2.89)		(-8.19)	(0.94)	
Constant	0.0016***	0.0023***	-0.0024***	0.0008***	0.0012***	-0.0010***
	(358.98)	(247.88)	(-19.76)	(291.29)	(233.03)	(-11.70)
Adjusted R^2	.64	.44	.09	.79	.68	.11
Dependent variable	PD	Volatility	Stock return	PD	Volatility	Stock return
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample period	First crisis	First crisis	First crisis	Second crisis	Second crisis	Second crisis
Stocks included	Financials	Financials	Financials	Financials	Financials	Financials
Number of stocks	1,554	979	1,718	1,311	1,012	1,773
Observations	212,525	117,392	246,164	210,942	139,502	292,816

The dependent variable is the PD in columns 1 and 4; stock return volatility based on a GJR-GARCH(1,1) model in columns 2 and 5; and stock return in columns 3 and 6, respectively, for the two crises. Ban is a dummy variable that equals 1 if naked or covered short sales are forbidden and 0 otherwise, during the first crisis and equals 1 if covered short sales are forbidden and 0 otherwise, during the second crisis. Market return is the return on the market index of each country. All regressions are estimated using daily data for the matched sample of financial stocks in the first crisis (June to December 2008) and in the second crisis (May to November 2011). The estimates are based on fixed effects panel regressions with robust standard errors and report t-statistics in parentheses. ***$p < .01$.

Table 10
Short-selling bans and vulnerability of financial institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ban	0.0008***	0.0002***	0.0011***	0.0002***	0.0005***	0.0004***	0.0009***	0.0001***
	(29.57)	(15.24)	(48.44)	(13.67)	(26.80)	(15.00)	(37.94)	(6.93)
Vulnerability × Ban	0.0003***	0.0003***	0.0006***	0.0005***	0.0004***	0.0007***	0.0016***	0.0008***
	(8.27)	(11.07)	(15.24)	(15.96)	(8.54)	(9.62)	(27.87)	(17.61)
Constant	0.0013***	0.0007***	0.0019***	0.0009***	0.0017***	0.0011***	0.0018***	0.0011***
	(167.92)	(149.44)	(238.30)	(275.37)	(77.71)	(149.64)	(136.31)	(193.12)
Vulnerability defined as	**Leverage**	**Leverage**	Systemic risk	Systemic risk	Negative of Tier 1 capital ratio	Negative of Tier 1 capital ratio	Negative of stable funding ratio	Negative of stable funding ratio
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample period	First crisis	Second crisis	First crisis	Second crisis	First crisis	Second crisis	First crisis	Second crisis
Stocks included	Financials	Financials	Financials	Financials	Banks	Banks	Financials	Financials
Adjusted R^2	.67	.69	.69	.85	.63	.91	.62	.89
Number of stocks	376	345	640	697	87	45	218	182
Observations	44,769	55,371	78,482	114,285	12,765	7,130	31,995	30,396

The dependent variable is the probability of default. Ban is a dummy variable that equals 1 if naked or covered short sales are forbidden and 0 otherwise, in the first crisis and that equals 1 if covered short sales are forbidden and 0 otherwise, in the second crisis. Vulnerability is a dummy variable that equals 1 if the financial institution's vulnerability is greater than the median and 0 otherwise. We use four different measures of vulnerability: leverage in columns 1 and 2, standardized systemic risk in columns 3 and 4, the negative of the Tier 1 capital ratio in columns 5 and 6, and the negative of the stable funding ratio (defined as the ratio of deposits plus equity to long-term assets) in columns 7 and 8. All regressions are estimated using daily data for the matched sample of financial stocks in the first crisis (June to December 2008) and in the second crisis (May to November 2011). The estimates are based on fixed effects panel regressions with robust standard errors and report t-statistics in parentheses. *p <.1; **p <.05; ***p <.01.

Table 10
Short-selling bans and vulnerability of financial institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ban	0.0017***	0.0007***	0.0015***	0.0005***	0.0010***	0.0007***	0.0013***	0.0003***
	(32.45)	(13.11)	(41.84)	(7.42)	(11.54)	(12.02)	(26.61)	(8.02)
Vulnerability × Ban	0.0001**	0.0002**	0.0006***	0.0006***	0.0006***	0.0005***	0.0016***	0.0009***
	(1.97)	(2.08)	(10.71)	(7.75)	(5.13)	(5.78)	(16.99)	(13.33)
Constant	0.0019***	0.0014***	0.0024***	0.0013***	0.0015***	0.0016***	0.0018***	0.0016***
	(122.42)	(30.57)	(211.07)	(72.72)	(66.33)	(49.81)	(108.15)	(41.20)
Vulnerability defined as	**Leverage**	**Leverage**	Systemic risk	Systemic risk	Negative of Tier 1 capital ratio	Negative of Tier 1 capital ratio	Negative of stable funding ratio	Negative of stable funding ratio
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample period	First crisis	Second crisis	First crisis	Second crisis	First crisis	Second crisis	First crisis	Second crisis
Stocks included	Financials	Financials	Financials	Financials	Banks	Banks	Financials	Financials
Adjusted R^2	.40	.07	.45	.15	.47	.84	.45	.30
Number of stocks	405	391	672	749	69	45	167	185
Observations	44,737	63,525	73,819	125,616	8,893	7,244	22,304	30,948

The dependent variable is the return volatility of financial stocks based on a GJR-GARCH(1,1) model. Ban is a dummy variable that equals 1 if naked or covered short sales are forbidden and 0 otherwise, in the first crisis and that equals 1 if covered short sales are forbidden and 0 otherwise, in the second crisis. All regressions are estimated using daily data for financial stocks in the first crisis (June to December 2008) and in the second crisis (May to November 2011), respectively. Vulnerability is a dummy variable that equals 1 if the financial institution's vulnerability is greater than the median and 0 otherwise. We use four different measures of vulnerability: leverage in columns 1 and 2, standardized systemic risk in columns 3 and 4, the negative of the Tier 1 capital ratio in columns 5 and 6, and the negative of the stable funding ratio (defined as the ratio of deposits plus equity to long-term assets) in columns 7 and 8. The estimates are based on fixed effects panel regressions with robust standard errors and report t-statistics in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

Table 11
Stability of financial institutions and short-selling bans: IV estimates

	Prob. of default		Volatility		Return	
	First stage (1)	Second stage (2)	First stage (3)	Second stage (4)	First stage (5)	Second stage (6)
Ban		0.0062***		0.0127***		-0.0616***
		(3.15)		(6.76)		(-3.84)
Market return					0.2522***	0.8469***
					(5.18)	(74.01)
Srisk	0.0548***	0.0002*	0.0104***	0.0000	0.0110***	-0.0002
	(12.92)	(1.95)	(6.75)	(1.53)	(6.61)	(-0.82)
Instrument	0.0223***		0.0454***		0.0444***	
	(3.60)		(7.45)		(7.36)	
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-test		13		56		54
Observations	38,388	38,388	40,900	40,900	41,139	41,139

The table shows the IV estimates of regressions for financial institutions. Columns 1, 3, and 5 show the estimates of the first-stage regression coefficients, and columns 2, 4, and 6 those of the corresponding second-stage regressions. In the first-stage regressions, the dependent variable is the ban dummy. In the second-stage regressions, the dependent variable is the 3-month probability of default in column 2, stock return volatility based on a GJR-GARCH(1,1) model in column 4, and the stock return in column 6. The Ban dummy variable equals 1 if covered short sales are forbidden in the second crisis and 0 otherwise. The regression is estimated using daily data for financials only for the second crisis (from May 1, 2011, to November 30, 2011). In all regressions, the instrument used for the Ban variable is a stock-time dummy that equals 1 if during the second crisis the systemic risk for the relevant financial stock exceeds a threshold given by its systemic risk on the day in which a short-selling ban (whether naked or covered) was imposed on it during the first crisis, and 0 otherwise. For stocks that were not banned in the first crisis, the threshold is set equal to the highest level of systemic risk achieved during the first crisis. The specification includes stock-level fixed effects. The number in parentheses below each coefficient estimate is its t-statistic, obtained with robust standard errors. *$p < .1$; **$p < .05$; ***$p < .01$.

Table A.1
Daily stock return volatility and short-selling bans for all stocks, financials, and banks

	(1)	(2)	(3)	(4)	(5)	(6)
Naked ban	0.0100***	0.0155***	0.0161***			
	(173.68)	(80.32)	(64.87)			
Covered ban	0.0322***	0.0199***	0.0086***	0.0121***	0.0138***	0.0181***
	(257.44)	(99.49)	(28.84)	(111.37)	(47.93)	(40.68)
Constant	0.0508***	0.0431***	0.0409***	0.0361***	0.0301***	0.0265***
	(1876.44)	(688.26)	(438.77)	(2156.32)	(627.57)	(500.78)
Adjusted R^2	.46	.41	.43	.51	.45	.57
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample period	First crisis	First crisis	First crisis	Second crisis	Second crisis	Second crisis
Stocks included	All	Financial	Bank	All	Financial	Bank
Number of stocks	16,544	2,638	663	17,920	2,878	669
Observations	1,954,208	297,338	81,290	2,493,935	386,600	98,077

The dependent variable is stock return volatility, measured as the squared root of the 20-day moving average of squared daily returns. Naked ban is a dummy variable that equals 1 if only naked short sales are forbidden and 0 otherwise. Covered ban is a dummy variable that equals 1 if even covered short sales are forbidden and 0 otherwise. Regressions in columns 1, 2, and 3 are estimated using daily data for the first crisis (July to December 2008). The estimates in column 1 are based on data for all stocks; those in column 2 are based on data from financial stocks only; and those in column 3 are based on data from bank stocks only. Regressions in columns 4, 5, and 6 are estimated using daily data for the second crisis (May to November 2011). The estimates in column 4 are based on data for all stocks; those in column 5 are based on data from financial stocks only; and those in column 6 are based on data from bank stocks only. The estimates are based on fixed effects panel regressions with robust standard errors and report t-statistics in parentheses. $^{***}p<.01$.

Table A.2
Weekly stock return volatility and short-selling bans for all stocks, financials, and banks

	(1)	(2)	(3)	(4)	(5)	(6)
Naked ban	0.0185***	0.0332***	0.0373***			
	(153.49)	(78.32)	(64.40)			
Covered ban	0.0649***	0.0373***	0.0094***	0.0324***	0.0207***	0.0274***
	(306.03)	(104.55)	(18.23)	(151.86)	(27.44)	(23.01)
Constant	0.0973***	0.0848***	0.0775***	0.0660***	0.0542***	0.0506***
	(2230.88)	(797.72)	(454.33)	(2572.14)	(755.38)	(485.78)
Adjusted R^2	.44	.40	.43	.50	.44	.53
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample period	First crisis	First crisis	First crisis	Second crisis	Second crisis	Second crisis
Stocks included	All	Financial	Bank	All	Financial	Bank
Number of stocks	16,526	2,631	663	17,927	2,858	668
Observations	1,963,609	297,158	81,709	2,175,368	330,921	83,614

The dependent variable is the stock return volatility, measured as the square root of the 20-day moving average of squared weekly returns. Naked ban is a dummy variable that equals 1 if only naked short sales are forbidden and 0 otherwise. Covered ban is a dummy variable that equals 1 if even covered short sales are forbidden and 0 otherwise. Regressions in columns 1, 2, and 3 are estimated using daily data for the first crisis (July to December 2008). The estimates in column 1 are based on data for all stocks; those in column 2 are based on data for financial stocks only; and those in column 3 are based on data for bank stocks only. Regressions in columns 4, 5 and 6 are estimated using daily data for the second crisis (May to November 2011). The estimates in column 4 are based on data for all stocks; those in column 5 are based on data for financial stocks only; and those in column 6 are based on data for bank stocks only. The estimates are based on fixed effects panel regressions with robust standard errors and report t-statistics in parentheses. ***$p < .01$.

Table A.3
Stability of financial institutions and short-selling bans: IV estimates based on a country-level ban rule

	Prob. of default		Volatility		Return	
	First stage (1)	Second stage (2)	First stage (3)	Second stage (4)	First stage (5)	Second stage (6)
Ban		0.0041***		0.0060***		-0.0210*
		(11.36)		(12.15)		(-1.94)
Market return					0.2830***	0.7814***
					(5.85)	(87.49)
Srisk	0.0090***	0.0001***	0.0083***	0.0001***	0.0081***	-0.0002
	(17.42)	(9.45)	(18.71)	(8.27)	(18.67)	(-0.87)
Instrument	0.0611***		0.0577***		0.0562***	
	(15.55)		(15.49)		(15.54)	
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-test	242		240		241	
Observations	42,459	42,459	45,546	45,546	45,734	45,734

The table shows the IV estimates of regressions for financial institutions. Columns 1, 3, and 5 show the estimates of the first-stage regression coefficients, and columns 2, 4, and 6 shows those of the corresponding second-stage regressions. In the first-stage regression, the dependent variable is the ban dummy. In the second-stage regression, the dependent variable is 3-month probability of default in column 2, stock return volatility based on a GJR-GARCH(1,1) model in column 4, and the stock return in column 6. The Ban dummy variable equals 1 if covered short sales are forbidden in the second crisis and 0 otherwise. The regression is estimated using daily data for financial stocks only between May 1, 2011, and November 30, 2011. In all regressions, the instrument used for the Ban variable is a country-time dummy that equals 1 if during the second crisis the mean systemic risk for the financial stocks of country c exceeds the mean level for the same stocks on the first day in which a short-selling ban (whether naked or covered) was imposed in country c during the first crisis, and 0 otherwise. Data for the countries in which no ban was imposed in the first crisis are excluded from the sample. The specification includes stock-level fixed effects. The number in parentheses below each coefficient estimate is its t-statistic, obtained with robust standard errors. *$p < .1$; **$p < .05$; ***$p < .01$.