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Short-Selling Bans Around the World: Evidence from the 2007–09 Crisis

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ABSTRACT

Most regulators around the world reacted to the 2007–09 crisis by imposing bans on short selling. These were imposed and lifted at different dates in different countries, often targeted different sets of stocks, and featured varying degrees of stringency. We exploit this variation in short-sales regimes to identify their effects on liquidity, price discovery, and stock prices. Using panel and matching techniques, we find that bans (i) were detrimental for liquidity, especially for stocks with small capitalization and no listed options; (ii) slowed price discovery, especially in bear markets, and (iii) failed to support prices, except possibly for U.S. financial stocks.

> "The emergency order temporarily banning short selling of financial stocks will restore equilibrium to markets" (Christopher Cox, SEC Chairman, 19 September 2008, SEC News Release 2008–211).

> "Knowing what we know now, I believe on balance the commission would not do it again. The costs (of the short-selling ban on financials) appear to outweigh the benefits." (Christopher Cox, telephone interview to Reuters, 31 December 2008).

MOST STOCK EXCHANGE REGULATORS around the world reacted to the 2007–09 financial crisis by imposing bans or constraints on short sales. These hurried interventions, which varied considerably in intensity, scope, and duration, were presented as measures to restore the orderly functioning of securities markets and limit unwarranted drops in securities prices capable of exacerbating the crisis. SEC News Release 2008–211, which announced the short-sales

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ban on U.S. financial stocks, summarizes regulators' view during the crisis: "unbridled short selling is contributing to the recent sudden price declines in the securities of financial institutions unrelated to true price valuation."

However, theoretical reasons and previous evidence cast doubt on the benefits of short-selling bans, suggesting instead that they may reduce market liquidity and hinder price discovery, while not necessarily supporting security prices. These concerns are particularly relevant in the context of the crisis: if short-selling bans did contribute to the decrease in stock market liquidity in 2008 and 2009, they would have inflicted serious damage on market participants who sorely needed liquidity and had difficulty obtaining it in fixed income markets. Further, it is worth asking whether short-selling bans met regulators' stated objective of stabilizing stock prices in the midst of the crisis.

In this paper, we exploit the regulatory interventions around the world in 2008 and 2009 to shed light on the above issues: the flurry of short-selling bans generated an unprecedented wealth of data that can be used to investigate their effects on market liquidity, on the speed of price discovery, and on stock prices. Short-sale restrictions were imposed and lifted at different dates in different countries; they often applied to different sets of stocks (only financials in some countries, all stocks in others), and they featured different degrees of stringency. These features make the data ideally suited to identify the effects of the bans through panel data and event study techniques. Moreover, compared to individual countries' data, multicountry evidence should be less affected by confounding effects arising from other country-specific policy interventions that occurred during the crisis period.

Our sample consists of daily data for 16,491 stocks in 30 countries, from January 2008 to June 2009. For each country, we determine if a short-selling ban was enacted in this period, and if so when; which stocks it applied to; and which restrictions it imposed on short sales. The primary focus of our study is the effect of short-selling bans on market liquidity, but we also investigate their effects on other dimensions of market performance considered in the literature, such as price discovery and the level of stock prices.

In assessing the impact of short-selling bans on liquidity, we take into account the fact that bid-ask spreads may be affected by stock-specific characteristics. Hence, in the estimation we use stock-level fixed effects, and in some specifications we also control for return volatility, whose changes may affect bid-ask spreads by changing the inventory risk of market makers, and for common changes in liquidity by including day fixed effects, to take into account commonality in liquidity. The latter is especially important in view of the fact that, during the crisis, increased uncertainty and acute funding problems are likely to have reduced stock market liquidity throughout the world.

Our results indicate that the short-selling bans imposed during the crisis are associated with a statistically and economically significant liquidity disruption, that is, with an increase in bid-ask spreads and in the Amihud illiquidity indicator, controlling for other variables. In contrast, the obligation to disclose short sales is associated with a significant improvement in market liquidity.

We also investigate whether these negative effects on liquidity disproportionately affect stocks with some characteristics, and find that that they are more pronounced for small-cap stocks. As a result, in countries where such stocks are overrepresented, the bans are associated with larger increases in bid-ask spreads. Moreover, the adverse liquidity effect of bans is stronger for stocks that do not have listed options than for stocks that do, suggesting that the availability of an option market allows investors to effectively express short views on the underlying stock affected by the ban. For the dually listed stocks in our sample, short-selling bans in the home market increase bid-ask spreads both on the home and on the foreign market, whereas foreign bans only reduce liquidity within the foreign market.

The evidence also shows that short-selling bans slow down price discovery, especially where negative news is concerned, in line with both theoretical predictions and previous empirical findings. Finally, the bans are not associated with better stock price performance, the United States being the only exception: we find that bans are not significantly correlated with excess returns in countries with short-selling bans on financials, except in the United States, where the correlation is positive and significant, in line with the results in Boehmer, Jones, and Zhang (2009). However, this result for the United States may reflect concomitant announcements of bank bailouts and thus may be spurious. Therefore, in contrast to the regulators' hopes, the overall evidence indicates that at best short-selling bans have left stock prices unaffected.

The paper is structured as follows. Section I briefly reviews the relevant literature to develop the testable hypotheses. Section II presents the data and methodology. Section III reports descriptive evidence and regression results for the impact of short-selling restrictions on market liquidity, and investigates whether it differs across stocks with different characteristics. Sections IV and V present results for the impact of short-selling restrictions on price discovery and on stock prices, respectively. Section VI concludes.

I. The Setting

Our analysis concerns the effects of short-selling bans on three variables: market liquidity, price discovery, and stock overpricing. As a starting point, we consider which effects are predicted by the theory for each variable, and give a brief account of the evidence so far.

A. Liquidity

The effects of short-selling bans on liquidity are in principle ambiguous. Diamond and Verrecchia (1987) analyze their effects in a variant of the Glosten-Milgrom (1985) model and show that, by preventing informed investors to trade on bad news, short-selling bans reduce the speed of price discovery, and such delayed resolution of uncertainty about fundamentals tends to increase the bid-ask spread.

However, this result only applies if the ban equally constrains informed and uninformed investors. If instead potential short sellers have superior information (consistent with intuition as well as much evidence), a short-selling ban lowers the fraction of informed traders on the sell side. On this account the ban would tend to reduce the bid-ask spread for given information revealed by past trades. But, because the ban also slows the revelation of such information, the overall effect on the bid-ask spread is ambiguous.

In a setting where bid-ask spreads compensate dealers for their inventory holding costs, a short-selling ban should widen bid-ask spreads: the inability to short the stock should impair market makers' inventory management, which is especially problematic in volatile market phases such as the crisis period. And even if market makers retain access to short selling, the ban limits competition by other liquidity suppliers, thereby allowing market makers to widen their spreads. Moreover, by sidelining investors with negative information, short-sale constraints make prices less informative and thus increase the risk to uninformed market participants (Bai, Chang, and Wang (2006)). So, if market makers are uninformed, they will widen their bid-ask quotes to cover their increased inventory holding costs.

Most of the evidence available so far is consistent with the idea that short-selling bans damage liquidity. The evidence most directly related to this study is provided by Boehmer, Jones, and Zhang (2009), who use panel data techniques to analyze the response of liquidity to the short-selling ban imposed from September 18 to October 8 in the United States, exploiting the difference between the financial stocks targeted by the ban and those that were not. They find that liquidity—as measured by spreads and price impacts—deteriorated significantly for stocks subject to the ban. This finding is confirmed by Kolasinski, Reed, and Thornock (2012), who find that the June 2008 emergency order that already restricted naked short selling for 19 stocks had a similar adverse effect on liquidity. Marsh and Payne (2012), who analyze order and transaction-level data for the United Kingdom, further find that, as soon as the ban applied to financial stocks, their bid-ask spreads widened and their market depth declined much more than those for exempt nonfinancial stocks, even though the prices and order flows of the two groups of stocks had behaved similarly before the ban.

However, other studies report more ambiguous or even conflicting evidence. Jones (2012) investigates how liquidity responded to changes in the stringency of short-sale constraints during the Great Depression in the United States, and finds that the 1932 requirement that brokers secure written authorization before lending customers' shares reduced liquidity, but in 1931 and 1938 the rule that short sales be executed only on upticks increased liquidity. Charoenrook and Daouk (2005), who investigate the effects of market-wide short-sale restrictions on several variables for 111 countries, find that short-sale restrictions correlate with greater market-wide liquidity, as measured by total stock market trading volume.

Although most of these studies are based on U.S. data, our contribution analyzes how liquidity reacted to short-selling bans in 30 countries, exploiting cross-country variation in the bans' enactment and lifting dates, in their stringency, and in their coverage to identify their effects and filter out the effect of other concomitant country-specific events or policies. Our study also differs from Charoenrook and Daouk (2005), as we rely on individual stock data rather than market indices, and we measure liquidity with bid-ask spreads and the Amihud illiquidity index rather than with trading volume, notoriously a problematic proxy for liquidity.[1] This is particularly true for the crisis period, when increases in bid-ask spreads were often associated with greater trading volumes.

B. Speed of Price Discovery

The predicted effect of short-selling bans on the speed of price discovery is more clear cut than that on liquidity, as should be clear from the above discussion of the Diamond-Verrecchia (1987) model: by preventing traders from short selling, a ban moderates the trading activity of informed traders who have negative information about fundamentals and thereby slows down price discovery, and does so asymmetrically—more in bear than in bull markets. Indeed, this is precisely what regulators hope to achieve with short-selling bans: preventing bad news from being rapidly impounded into stock prices, probably in the belief that such bad news is "unwarranted," in the sense that it reflects a negative bubble or herding behavior rather than fundamental information.

Bris, Goetzmann, and Zhu (2007) investigate whether short-sales restrictions affect the speed of price discovery using data on short-sale restrictions for 46 equity markets around the world. They find that prices incorporate negative information faster in countries where short sales are allowed and practiced, implying that short-selling bans are associated with less efficient price discovery at the individual security level. These findings accord with the evidence in Saffi and Sigurdsson (2011) and Boehmer and Wu (2012) that the ability to short-sell stocks increases the informational efficiency of market prices. They are also consistent with the result in Reed (2007) that short-selling bans determine an asymmetry in price adjustment in response to earnings announcements.

In apparent contrast with the evidence from these studies, Kolasinksi, Reed, and Thornock (2012) report that, during the 2008 ban period in the United States, the negative relation between short-selling volume and stock returns grew stronger, so that short-selling activity became more informative. But the contradiction is only apparent: in the presence of a partial short-selling ban, banned stocks may feature slower price discovery (in the sense that their *own* order flow becomes less informative), yet their price may become more sensitive to the short sales that investors are allowed to carry out on other stocks—especially if the ban is accompanied by increased disclosure

[1] Since our data are at a daily frequency, we cannot compute measures of liquidity such as effective or realized spreads and estimates of price impact, which require intraday data.

of short sales, as indeed was the case in the United States during the crisis.[2]

Also on this score, our contribution is to bring panel data to bear on the issue: although Bris, Goetzmann, and Zhu (2007) rely on cross-country variation in their data, we exploit time-series variation because of inception and lifting dates of bans, sometimes differentially across stock classes, to identify the bans' effect on price discovery. Indeed, we completely remove purely cross-sectional variation from our sample, as we include stock-level fixed effects.

C. Overpricing

Miller (1977) predicts that short-selling constraints lead to "overpricing," that is, to prices above the equilibrium level that would prevail absent such constraints. This prediction is based on the idea that, if investors have heterogeneous beliefs, prohibiting short selling will lead to stock prices that reflect only the valuations of bullish and bearish investors who currently own the stock. Bearish investors who do not own the stock are excluded from trading, so that their valuations do not affect the price. Hence, prices should rise above their full-information values when a ban is imposed, and decline when it is lifted.

This mechanical prediction of Miller's model does not survive in the rational expectations framework of Diamond and Verrecchia (1987), where market participants adjust their valuations to take into account the fact that short-selling constraints sideline investors with negative information, so that in equilibrium stocks are not systematically overpriced when short sales are banned.

However, the no-overpricing result of Diamond and Verrecchia (1987) hinges not only on the assumption of rational expectations but also on investors' risk neutrality. Bai, Chang, and Wang (2006) show that, when rational investors are risk averse, the slower price discovery induced by short-sales constraints increases the risk perceived by uninformed investors and leads them to require higher expected returns; hence, it induces lower prices, contrary to Miller's prediction. But they also show that with risk-averse investors a countervailing effect may also be at work: a ban on short sales also prevents investors from taking on negative positions to hedge other risks. This effect pushes up the demand for the stock and tends to increase its price.

Thus, with risk-averse investors the net effect of a short-selling ban on stock prices is ambiguous, and is more likely to be negative the greater the slowdown in price discovery induced by the ban. The prediction that a short-selling ban may aggravate a decline in prices, rather than prevent it, is also present in the model by Hong and Stein (2003), where the accumulated unrevealed negative information of investors who would have engaged in short sales surfaces only when the market begins to drop, thereby aggravating the price decline.

[2] The U.S. short-selling ban on financials was imposed on Friday September 19, 2008, and the obligation to disclose short sales on a weekly basis became effective on the subsequent trading day (Monday, September 22) and applied to all stocks for trades exceeding 0.25% of the relevant company's capital.

So the predictions of the theory regarding the effect of short sales on stock prices are ambiguous. Unfortunately, the evidence available so far is equally mixed. Bris, Goetzmann, and Zhu (2007) report cross-country evidence that short-sale constraints are significantly associated with less negative skewness for market returns, but not for individual stock returns. Evidence consistent with the overpricing hypothesis is reported by Jones and Lamont (2002), who use data about shorting costs on the New York Stock Exchange (NYSE) from 1926 to 1933, and by Chang, Cheng, and Yu (2007), who rely on data from the Hong Kong stock market. But in contrast to these findings, research on the suspension or removal of short-sale price tests such as the uptick rule in the United States finds no significant stock price effects (Boehmer, Jones, and Zhang (2008) and Diether, Lee, and Werner (2009)).

Recent studies of U.S. evidence about the 2008 short-selling ban on financials have produced equally controversial evidence on the overpricing effect. Boehmer, Jones, and Zhang (2009) document large price increases for banned stocks upon announcement of the ban, followed by gradual decreases during the ban period. Yet they recognize that the correlation with the ban could be spurious, as the prices of U.S. financials could have been affected by the concomitant announcement of the Troubled Asset Relief Program (TARP). Their skepticism is reinforced by the finding that stocks that were later added to the ban list experienced no positive share price effects. However, Harris, Namvar, and Phillips (2009) try to control for the concomitant bank bailout announcements by estimating a factor model of stock price changes that includes, among the factors, the return on an index of the banned stocks and a TARP index. Their estimates imply that banned stocks earned positive abnormal returns during the ban period, but these abnormal returns persisted after the lifting of the ban.

Reliance on data from the United States—where the inception of the short-selling ban on financials coincided with bank bailout announcements—makes it hard to identify the price effects of the ban. International evidence can be particularly valuable in this respect because in several other countries short-selling bans were not accompanied by bailout announcements, or at least such announcements were not concomitant with the bans. Moreover, in many countries bans also applied to nonfinancial stocks, which were not affected by bank bailout announcements, and in other countries financial stocks were not banned. As we shall see, by relying on cross-country as well as time-series variation in the inception and lifting of bans, we find that the overpricing effect apparently present in U.S. data is absent elsewhere.

II. Data and Method

Our data consist of daily stock bid and ask prices, volumes, short-selling ban characteristics, inception dates, and lifting dates for 17,040 stocks from 30 countries (most European markets and developed non-European markets) for the period spanning from January 1, 2008 to June 23, 2009. Data for bid and ask prices, volumes, and number of outstanding shares are drawn from

Datastream. Bid and ask prices are measured at the market close. Our initial data set contains 5,992,679 stock-day observations.[3] We winsorize the data by eliminating the observations corresponding to the top 1% of the bid-ask spread (thereby eliminating values exceeding 54.9%), as well as those corresponding to negative bid-ask spreads. The missing bid-ask prices for four countries and the application of the filters leave us with a sample of 5,143,173 stock-day observations and 16,491 stocks. The dates and characteristics of short-selling regimes come from the websites of national regulatory bodies and of the Committee of European Securities Regulators (CESR). In particular, this information allows us to distinguish between "naked" and "covered" bans: the former forbid naked short sales, that is, 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, transactions in which the seller manages to borrow the stock.[4]

Table I describes the structure of our data set. As a fraction of the total observations, 12.4% refer to stocks affected by a short-selling ban. As of October 1, 2008 (when most bans were in effect), 31.5% of the sample stocks were affected by a ban on short sales (whether naked or covered). However, the fraction varies considerably from country to country, from 0% in Austria and Denmark to 100% in Australia and Japan. Table I also shows that in many countries short-selling bans were accompanied by disclosure requirements, whereby existing short positions in financials or, for some countries, in all stocks, must be disclosed if they represent a significant fraction of existing shares (generally 0.25%). In some countries this information is reported to the national regulatory body, whereas in others it is disseminated to all market participants.

Figures 1 and 2 depict the extent of cross-country variation in short-selling regimes between September 2008 and June 2009. Figure 1 shows the period in which bans were enacted in our sample countries via color-coded lines. Dark and light blue lines correspond to naked bans of financial and nonfinancial stocks, respectively. Red lines indicate covered bans for financial stocks, whereas orange lines correspond to covered bans of nonfinancial stocks. The figure illustrates the variety of regimes and regime durations across countries, as well as the complex regime variation over time, even within the same country (the extreme example here being Italy).

Figure 2 gives a more synthetic illustration of the diffusion of short-selling bans across the world during the crisis by plotting the fraction of banned stocks in our sample separately for naked and covered bans. The two darker histograms show the weight of banned stocks in total market capitalization, whereas the lighter histograms show them as a fraction of the total number of stocks in our sample at the corresponding date. The overall fraction of banned

[3] Bid and ask prices are available for the stocks from all the countries in the sample except for the Czech Republic, Greece, Hungary, Israel, and Luxembourg. However, for these countries we can still compute the Amihud illiquidity ratio.

[4] See Gruenewald, Wagner, and Weber (2010) for a description of the different types of short-selling restrictions and for a discussion of their possible rationale.

stocks jumped from 0% to about 20% in September 2008, rose again to over 30% in October, and then gradually decreased back to 20% over the subsequent 8 months. Interestingly, in September and October 2008, covered bans were more widespread than naked ones, whereas their relative importance tended to reverse later on. As of June 2009, about 20% of the stocks in our sample were still subject to naked bans, whereas covered bans had almost disappeared.

A key feature of our data, which emerges clearly from Table I and from Figures 1 and 2, is that the regulation of short sales during the crisis differed across countries along many dimensions:

(i) different ban inception dates (e.g., Spain intervened after the United States);
(ii) different lifting dates (e.g., the United States and Canada were the first countries to lift the bans);
(iii) the presence of countries that imposed no bans (e.g., some Scandinavian countries);
(iv) differences in the scope of bans, which applied only to financials in some countries (e.g., the United States and most European countries) and to all stocks in others (e.g., Australia, Japan, South Korea, and Spain); and
(v) differences in the stringency of bans, which were naked in some cases and covered in others.

Interestingly, the regulatory response of the United States differed from that of all the other countries in terms of timing because it was the first to impose and lift the ban, and also in terms of stringency, as it imposed a covered ban from the start. Moreover, the SEC banned short sales only on financials, whereas several other countries banned them for all stocks and others did not ban them at all. Thus, our data contain much additional information beyond the U.S. data on which most existing studies of short-sale restrictions are based.

The hallmark of our estimation method is to exploit this international variation in short-sale regimes to identify the effect of short-selling bans on (i) liquidity, as measured by the quoted percentage bid-ask spread and the Amihud illiquidity ratio; (ii) the speed of price discovery, as captured by the extent to which individual stock returns correlate with past market returns instead of contemporaneous ones; and (iii) the overpricing of stocks, as measured by the excess returns on stocks subject to bans relative to those on exempt stocks.

In our regression analysis, we measure short-sales restrictions using two dichotomous variables that correspond to different degrees of severity—the milder one being the ban on naked short sales (*Naked Ban*), and the stricter one being the ban on covered short sales as well (*Covered Ban*). The *Naked Ban* variable equals one when *only* naked short sales are forbidden (covered ones being allowed), whereas the *Covered Ban* variable equals one when covered short sales are also forbidden. Therefore, the effect of *Naked Ban* is identified by the observations for which the ban does not extend to covered short sales. We also have a third dichotomous variable (*Disclosure*), which equals one when short sellers are required to disclose their trades and zero otherwise.

Table I
Structure of the Data Set

This table describes the main characteristics of the short-selling bans for our international sample of countries.

Country	Ban Start Date	Partial Ban Lift Date	Ban Lift Date	Scope of Ban	Disclosure	Duration[a] (Days)	Day/ Stock Observations	Day/ Stock Observations with Ban	Fraction of Day/ Stock Obs. with Ban	Number of Stocks on October 1, 2008	Number of Stocks with Ban on October 1, 2008
Australia	September 22, 2008	November 18, 2008	May 25, 2009	All stocks	All stocks	245	357,003	58,594	16.4%	956	956
Austria	October 26, 2008			Financials	Financials	240	31,094	660	2.1%	89	
Belgium	September 22, 2008			Financials	Financials	274	47,479	1,084	2.3%	120	6
Canada	September 19, 2008		October 8, 2008	Financials	All stocks	19	385,912	154	0.0%	1,136	11
Czech Rep.				No ban			9,113		0.0%	25	
Denmark	October 13, 2008			Financials		253	60,408	7,099	11.8%	171	–
Finland				No ban			52,343		0.0%	145	
France	September 22, 2008			Financials	Financials	274	269,636	3,454	1.3%	719	19
Germany	September 20, 2008			Financials		276	318,318	2,124	0.7%	845	12
Greece	October 10, 2008		June 1, 2009	All stocks	All stocks	234	102,822	41,217	40.1%	273	
Hong Kong				No ban			403,900		0.0%	1,058	
Hungary				No ban	All stocks		11,283		0.0%	31	
Ireland	September 19, 2008			Financials	Financials	277	17,343	736	4.2%	50	4

Israel				No ban			55,858		0.0%	170	
Italy	September 22, 2008[b]	January 1, 2009	June 1, 2009	Financials, then all		252	138,240	63,704	46.1%	360	53
Japan	October 30, 2008			All stocks	All stocks	236	776,840	362,625	46.7%	2,294	2,294
Luxembourg	September 19, 2008			Financials		277	11,588	2,231	19.3%	41	18
Netherlands	September 22, 2008		June 1, 2009	Financials	Financials	252	32,546	1,242	3.8%	117	8
New Zealand				No ban			30,382		0.0%	102	
Norway	October 8, 2008			Financials		257	73,303	1,945	2.7%	227	
Poland				No ban			24,485		0.0%	79	
Portugal	September 22, 2008			Financials	All stocks	274	17,277	1,311	7.6%	53	9
Singapore				No ban			144,116		0.0%	426	
Slovenia				No ban			7,044		0.0%	21	
South Korea	October 1, 2008	June 1, 2009		All stocks		265	208,199	98,592	47.4%	616	616
Spain	September 24, 2008			All stocks	Financials	272	64,112	30,137	47.0%	173	173
Sweden				No ban			98,102		0.0%	309	
Switzerland	September 19, 2008		January 16, 2009	Financials		119	128,907	56,181	43.6%	381	381
United Kingdom	September 19, 2008		January 16, 2009	Financials	Financials	119	575,811	2,188	0.4%	1,826	33
United States	September 19, 2008		October 8, 2008	Financials	All stocks	19	1,539,215	10,015	0.7%	4,253	776
Totals							5,992,679	745,293	12.4%	17,066	5,369

[a] As of June 23, 2009.
[b] The ban initially applied to financials, and was extended to all stocks on October 10, 2008.

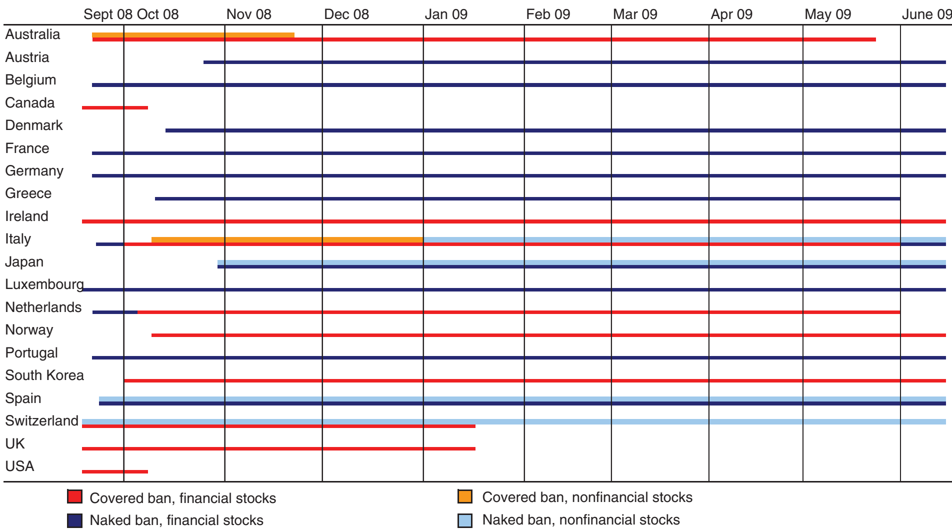

Figure 1. Short-selling ban regimes around the world, September 2008 to June 2009.

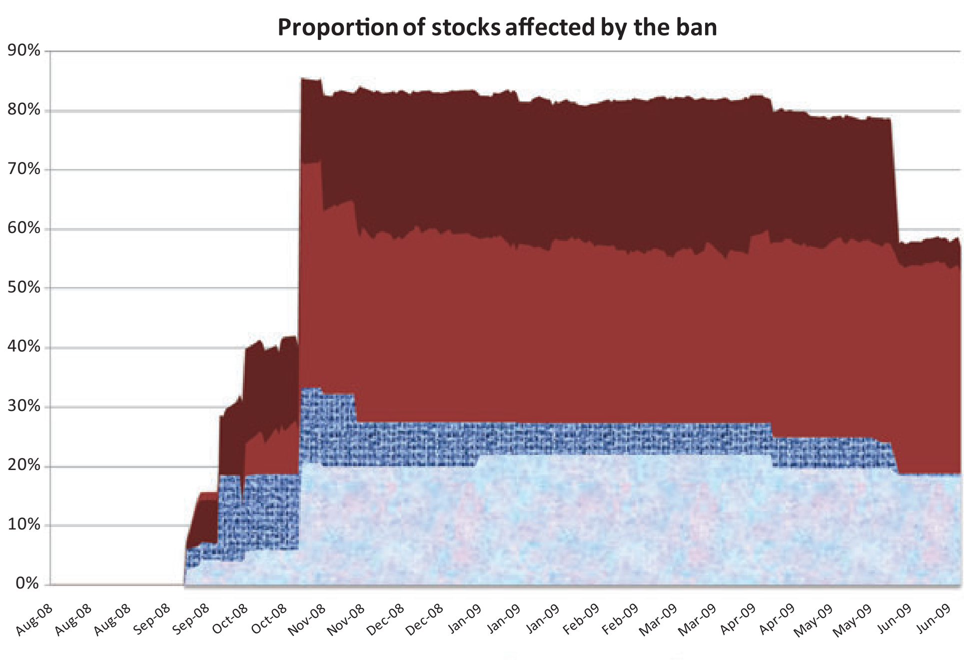

Figure 2. World percentage of stocks subject to short-selling bans. The two darker histograms plot the market capitalization of the stocks subject to naked and covered bans, respectively, as a fraction of total market capitalization. The two lighter histograms plot the fraction of stocks subject to naked and covered bans, respectively (as a percentage of the number of stocks in our sample on the corresponding date).

All our regressions include fixed stock-level effects to control for unobserved heterogeneity because of liquidity-related characteristics: stock characteristics such as risk, number of market makers, analyst coverage, capitalization, and size of public float, and country characteristics such as insider trading regulation and enforcement. Because models of the bid-ask spread based on adverse selection and inventory holding risk suggest that risk is a potentially important determinant of the bid-ask spread, in some specifications we also control for the changing stock-level volatility of returns.

In some regressions we use our entire sample, including observations from countries that imposed no ban or that imposed bans on all stocks, so that the control group is formed by stocks in countries that imposed no bans and exempt stocks in countries that imposed partials bans. These regressions fully exploit the identification arising from the cross-country diversity in ban regimes, but the estimated coefficient on the ban variables may reflect changes in the country-level behavior of bid-ask spreads. To perform a sharper "diff-in-diff" estimation, in other regressions we restrict the estimation to countries that imposed bans only on financial stocks, like the United States; whereas this has the drawback of leaving only financials in the treated group and only non-financials in the control group, it allows us to include time fixed effects and crisis-related control variables to take into account the commonality in liquidity or returns, especially important at a time when the whole world experienced increases in uncertainty and funding problems.

Besides panel data estimation, we also use an event study methodology to test for the effect of short-selling bans over a window of 50 days before and 50 days after the ban inception date. We apply this method only to the data of countries that imposed partial bans, where for each stock subject to a ban we identify a matching exempt stock with the same option listing status, with a criterion based on market capitalization and initial stock price as explained in Section III. Compared to panel data estimation, this method has the advantage of focusing on a time interval in which the effects of the ban should be less easily clouded by confounding factors, but at the cost of neglecting a considerable amount of information.

III. Market Liquidity

We examine the effects of short-selling bans on liquidity in two steps. We start with simple descriptive evidence about the pattern of quoted bid-ask spreads before, during, and after the bans, and then provide evidence based on regression analysis.

A. Descriptive Evidence

Figure 3 shows that, during the crisis, bid-ask spreads increased worldwide, and their peaks coincided with the salient moments of the crisis: the sudden collapse and distress sale of Bear Stearns to JPMorgan Chase on March 16, 2008, the failure of IndyMac Bank on July 11, the failure of Lehman Brothers

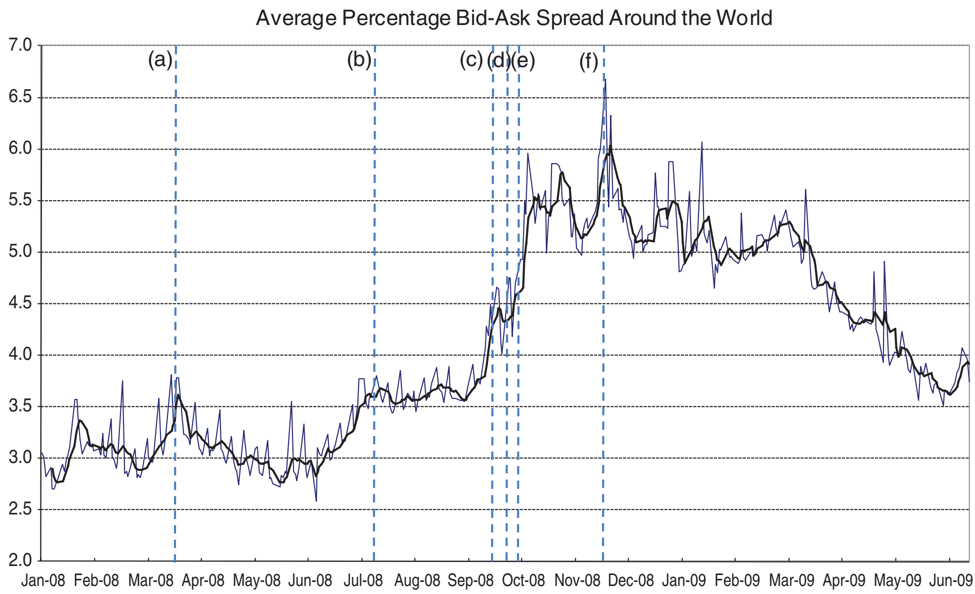

Figure 3. World average bid-ask spread and key events. The thin line plots daily values and the bold line plots the 5-day moving average of the bid-ask spread's cross-sectional average for our sample. The letters in the figure mark the following events: (a) March 16, 2008: Bear Stearns distress sale to JPMorgan Chase; (b) July 11, 2008: failure of IndyMac; (c) September 15 to 16, 2008: failure of Lehman Brothers and AIG rescue announcement; (d) September 29, 2008: rejection of the initial Emergency Economic Stabilization Act (EESA); (e) October 3, 2008: EESA approval; and (f) November 23, 2008: Citibank rescue announcement.

on September 15 and the AIG rescue announcement on September 16, the rejection by the U.S. Congress of the initial version of the Emergency Economic Stabilization Act on September 29 (followed by its approval on October 3), and the Citibank rescue announcement on November 23.

Short-selling bans were introduced in the wake of the dreadful news about the state of U.S. banks in September 2008: as shown by Table I, in most countries the inception date of the bans was in the second half of September. The ban was then lifted at different dates in Australia, Canada, Greece, Italy, the Netherlands, Switzerland, the United Kingdom, and the United States, whereas in the other countries it was left in effect through the end of our sample (June 2009). Figure 3 indicates that, whereas bid-ask spreads are higher when most countries banned short sales, their time pattern is also associated with financial turmoil per se: for instance, average bid-ask spreads started increasing in early September, when no country had banned short sales yet.

However, descriptive statistics reported in Table II suggest that short-selling bans further contributed to the deterioration in liquidity, as illustrated also by additional figures reported in the Internet Appendix of this paper.[5] Columns

[5] An Internet Appendix for this article is available online in the "Supplements and Datasets" section at http://www.afajof.org/supplements.asp.

1–3 of Table II document that stocks affected by a short-selling ban feature a significantly larger median bid-ask spread during the ban period. The difference is statistically different from zero at the 1% level for all countries, based on the Wilcoxon test for the difference between the median in the ban period and the median in the preban and (where available) the postban period. Columns 4 and 5 show that the median bid-ask spread during the ban period is on average 2.27 times as large as its preban value, and over three times as large for Canada, Ireland, Italy, the United Kingdom, and the United States. In the five countries that lifted the ban during our sample period (Australia, Canada, the Netherlands, the United Kingdom, and the United States), the bid-ask spread during the ban was on average 1.5 times as large as its postban value.

Admittedly, the period in which short-selling bans were imposed was especially turbulent, so that bid-ask spreads at that time may have been abnormally high even for stocks not targeted by bans. This is confirmed by the statistics in columns 6–8 of Table II: median bid-ask spreads were also significantly higher for stocks unaffected by short-selling bans, in all of our sample countries. But the corresponding statistics for the stocks affected by the ban are even higher, as can be seen by comparing the figures in column 4 with those in column 9 of the table. For instance, the median bid-ask spread for U.S. stocks affected by the ban increased by 243% (column 4), whereas for exempt stocks it increased by only 54% (column 9). Of course, this comparison can only be performed where the ban did not apply to all stocks, namely, in the countries shown in the lower part of the table. The econometric methods used in the next section rely on the different responses of banned and non-banned stocks to identify the effect of the short-selling bans.

B. Regression Analysis: Overall Liquidity Effect

We turn to regression analysis to investigate whether the correlation between bid-ask spreads and short-selling bans persists when one controls for different types of bans, for stock characteristics, and for time-varying stock-level and aggregate factors. Table III presents estimates of regressions in which the dependent variable is the percentage quoted bid-ask spread, and short-sales restrictions are measured by the three dummy variables described in Section II: *Naked Ban*, *Covered Ban*, and *Disclosure*.[6] More specifically, columns 1–6 show panel regression estimates with stock-level fixed effects, whereas column 7

[6] The effect of short-selling bans on the bid-ask spread may be spuriously inflated by the minimum tick size. A drastic drop in stock prices, such as the one induced by the crisis, may cause the percentage spread to increase mechanically, because the absolute spread cannot fall below the minimum tick size. This could bias the estimates of the coefficients of the ban variables, since short-selling bans were introduced at the time of sharply falling prices. However, we find that the distribution of absolute bid-ask spreads does not show any clustering of observations at the lowest boundary, except for Australia (where 5% of the observations cluster at an absolute bid-ask spread of 1/10 of 1 cent) and Hong Kong (where no short-selling ban was imposed). If we remove Australia from the sample, all our results remain qualitatively unaffected.

Table II

Median Bid-Ask Spreads before, during, and after Short-Selling Bans

The table provides median values for the bid-ask spread before, during, and after bans of short sales by country. Countries with total bans are those where there was a period of time during which a ban applies to all stocks, whereas countries with partial bans are those where bans applied to financial stocks only. For countries with total bans, columns 6 to 10 are empty because no stocks were exempt from bans whereas bans on short sales were enacted. Three asterisks on the figures in columns 2 and 7 indicate that the median bid-ask spread during the ban is significantly different from the median before and (if available) after the ban at the 1% level, based on a Wilcoxon test for differences between medians. The bottom row reports the simple average of the median values shown in the previous rows.

Country	Percentage Bid-Ask Spread for Stocks with Ban			Ratio of Bid-Ask Spread for Stocks with Ban		Percentage Bid-Ask Spread for Stocks without Ban			Ratio of Bid-Ask Spread for Stocks without Ban	
	Before (1)	During (2)	After (3)	During/before (4)	During/after (5)	Before (6)	During (7)	After (8)	During/before (9)	During/after (10)
Total bans:										
Australia[a]	3.3333	5.2632***	4.7244	1.58	1.11					
Italy[b,c]	0.5721	2.7682***		4.84						
Japan	0.6006	0.6976***		1.16						
South Korea[d]	0.4494	0.5249***		1.17						
Spain	0.5840	0.9611***		1.65						
Switzerland[e]	1.1599	1.5267***		1.32						
Partial Bans:										
Austria	0.2949	0.4807***		1.63		1.4500	1.4815***		1.02	
Belgium	0.2791	0.5239***		1.88		1.0929	1.7391***		1.59	
Canada	0.1877	0.6243***	0.3667	3.33	1.70	1.6901	3.3426***	3.9216	1.98	0.85
Denmark	1.9169	3.7736***		1.97		1.7493	2.3904***		1.37	
France	0.2946	0.6024***		2.04		1.4907	2.1108***		1.42	
Germany	0.2870	0.6764***		2.36		3.0457	4.1885***		1.38	
Ireland	0.4186	1.4047***		3.36		3.4782	5.9572***		1.71	

Netherlands[f]	0.2216	0.5144***	0.3302	2.32	1.56	0.8734	1.0292***	1.1959	1.18	0.86
Norway	2.1352	3.6433***		1.71		2.1201	3.3149***		1.56	
Portugal	0.4525	0.9479***		2.09		0.8608	1.3245***		1.54	
United Kingdom	0.1429	0.4619***	0.2930	3.23	1.58	4.6205	8.0101***	8.0000	1.73	1.00
United States	0.4904	1.6814***	0.9050	3.43	1.86	0.2793	0.4310***	0.4158	1.54	1.04
Average	0.7081	1.4248	1.1166	2.27	1.50	1.8411	2.8468	2.9934	1.49	0.99

[a]In Australia, a short-selling ban on all stocks was followed by a period during which the ban applied only to financials (November 19, 2008 to May 25, 2009), which prevents the identification of a clean control group of exempt stocks. In this case, postban data refer to nonfinancial stocks between November 19, 2008 and May 24, 2009 and to all stocks after May 24, 2009.

[b]In Italy and South Korea, there was a partial short-selling ban for a very short period, yielding too few observations to compare banned stocks with non-banned stocks.

[c]In Italy, the ban initially applied to financials only and to naked short sales only, then was extended to covered sales and later to all stocks. In 2009, it was restricted again to naked sales only for nonfinancials (January 1, 2009) and later for financials (June 1, 2009). The median during the ban period includes bid-ask spreads of financial stocks and other stocks for which naked and covered sales were banned in different periods.

[d]In South Korea, the ban on nonfinancials was lifted on June 1, 2009. As a result, the median bid-ask spread on banned stocks during the ban is computed on data for all stocks before June 1 and for financial stocks only after that date. We do not compute a postban median bid-ask spread because the ban was not lifted for financial stocks during our sample period.

[e]Switzerland issued a naked ban for nonfinancial stocks and a covered ban for financial stocks on the same date. Therefore, the median bid-ask spread shown in the table refers to all stocks. We show no figure for the postban period because only the covered ban on financials was lifted on January 16, 2009. The median bid-ask spread for financial stocks rose from 0.0853 in the preban period to 0.0957 in the covered ban period and reverted to 0.0800 after the ban lift. The increase during the ban period is significantly different from zero at the 1% level.

[f]The Netherlands initially issued a naked ban on financials, which was converted into a covered ban 2 weeks later. The median bid-ask spread for stocks during the ban period includes both the naked ban and the covered ban periods. The median bid-ask spread for stocks subject to the naked ban only (from September 22, 2008 to October 4, 2008) is 0.3075, about 1.4 times the median bid-ask spread before the ban.

Table III

Bid-Ask Spreads and Short-Selling Bans: Regression Analysis

The dependent variable is the percentage quoted bid-ask spread at the market close. In the first four columns, we use data for 25 countries (all the countries in Table I, except for the Czech Republic, Greece, Hungary, Israel, and Luxembourg). In the last three columns, we use data for 12 countries that banned short sales only for financial stocks (Austria, Belgium, Canada, Denmark, France, Germany, Ireland, the Netherlands, Norway, Portugal, the United Kingdom, and the United States). *Naked Ban* is a dummy variable that equals one if naked short sales are forbidden and covered sales are allowed and zero otherwise. *Covered Ban* is a dummy variable that equals one if even covered short sales are forbidden and zero otherwise. *Disclosure* is a dummy variable that equals one if the seller has to disclose his position and zero otherwise. *Volatility* is a moving standard deviation of returns based on the previous 20 observations. The regressions are estimated by OLS on daily data with robust standard errors clustered at the stock level in columns 1, 2, 5, and 6, and AR(1) correction in columns 3 and 4. The regressions in columns 1–6 include fixed effects at the stock level, and that in column 7 includes fixed effects at the stock-pair level. The estimates in columns 1– 6 are based on panel data, whereas those in column 7 are based on matched stocks using the event study methodology described in the text. The specifications in columns 6 and 7 also include day fixed effects. For computational reasons, in the regression of column 6, the estimation is implemented by replacing dependent and independent variables by their deviations from the respective stock-level average and including daily fixed effects in the regression. The numbers reported in parentheses are t-statistics. The estimates marked with three (two, one) asterisks are significantly different from zero at the 1% (5%, 10%) level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Countries	All	All	All	All	Partial Bans	Partial Bans	Partial Bans
Constant	3.93***	3.76***	4.97***	4.90***	4.20***	0.0005***	0.71***
	(1993.65)	(749.94)	(3290.72)	(3092.86)	(997.52)	(3.71)	(42.76)
Naked ban	1.28***	0.86***	0.89***	0.90***	2.43***	0.23***	0.56***
	(76.04)	(6.50)	(29.31)	(29.60)	(20.06)	(3.99)	(2.82)
Covered ban	1.98***	2.14***	1.63***	1.63***	2.75***	0.46***	1.19***
	(150.74)	(14.84)	(57.44)	(57.61)	(24.75)	(2.39)	(3.66)
Disclosure	−0.65***	−0.27**	−0.37***	−0.37***	−1.79***	−0.50***	−0.55*
	(−37.84)	(−1.84)	(−11.54)	(−11.59)	(−15.10)	(−2.25)	(−1.75)
Volatility				0.99***	−0.36***		
				(35.84)	(−14.65)		
Day fixed effects	No	No	No	No	No	Yes	Yes
Stock-level or pair-level(+) fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes+
AR(1) disturbances	No	No	Yes	Yes	No	No	No
Methodology	Panel	Panel	Panel	Panel	Panel	Panel	Matching
Number of observations	5,143,173	878,279	5,126,682	5,124,349	3,188,903	3,188,903	45,588
Included stocks	All	Financials	All	All	All	All	All
Number of stocks (pairs in column 7)	16,491	2,718	16,456	16,452	10,253	10,253	1,566

presents estimates of event study regressions with fixed effects for matched pairs of stocks.

B.1. Panel Regressions

The estimates in column 1 show that the ban on naked short sales is associated with an increase of 1.28 percentage points in the bid-ask spread, and the more stringent ban on covered short sales is associated with an increase of 1.98 percentage points. These are large effects compared with the 4.05% average bid-ask spread in our sample,[7] and both coefficients are significantly different from zero at the 1% level, with their huge t-statistics reflecting our large sample size. The bid-ask spread turns out to be negatively correlated with the obligation to disclose short sales: its coefficient—also very precisely estimated—indicates that short-selling disclosure is associated with a reduction of 0.65 percentage points in the spread. This suggests that disclosure may reduce adverse selection problems in the market because short sellers—feeling that they are under the scrutiny of market authorities and other market participants—trade less aggressively on their negative information. The specification of column 1 is estimated with OLS, stock-level fixed effects, and robust standard errors clustered at the stock level.

In column 2, we reestimate the regression on the subset of financial stocks only, using the same specification and estimation method as in column 1. We can still identify the effects of the short-selling bans because the ban on financial stocks was enacted at different times in different countries and, in some countries, financial stocks were not subject to any short-selling constraint. This regression allows us to check whether the results shown in column 1 do not simply reflect a liquidity differential between financial and nonfinancial stocks, considering that the ban applied mainly to financial stocks during the crisis. The estimates in column 2 show that, even within the subset of financial stocks, short-selling bans are associated with a larger bid-ask spread. Indeed, the coefficient on the covered ban dummy estimated for the subsample of financial stocks is not statistically different from that obtained for the overall sample; instead, the coefficient on the naked ban dummy is significantly smaller for the subsample of financial stocks.

Because the bid-ask spread is typically autocorrelated, in column 3 we reestimate the specification of column 1 with an AR(1) correction for the error term. Compared to the estimates in column 1, the coefficients on the three variables of interest are smaller in absolute value but remain sizeable and significantly different from zero at the 1% level. Column 4 shows the estimates of an expanded specification that includes volatility (measured as the rolling standard deviation of returns based on the previous 20 trading days) among the

[7] This large average bid-ask spread reflects the positive skew of our sample, arising from a tail of very illiquid small stocks. Indeed, the median is considerably lower (1.24%).

explanatory variables.[8] The coefficients on the three ban variables are virtually the same as in column 3, and the coefficient on volatility is positive, consistent with the idea that increases in risk should be associated with larger bid-ask spreads. Again, all estimates are significantly different from zero at the 1% level.

Very similar results also obtain if the specifications in columns 1–4 of Table III are estimated using the Amihud illiquidity measure (defined as the ratio of the absolute value of daily return to trading volume) instead of the bid-ask spread as the dependent variable. By using the Amihud illiquidity measure, we can exploit data for all 30 countries listed in Table I, instead of the 25 countries for which the bid-ask spread is available. Also in these regressions (whose estimates are reported in the Internet Appendix to save space), the coefficients on the *Naked Ban* and *Covered Ban* variables are positive, the coefficient on *Disclosure* is negative, and all three are significantly different from zero at the 1% level. Again, the results are almost identical if the estimation is restricted to financial stocks only, and are robust to the introduction of volatility among the explanatory variables.

The sample used in the first four columns of Table III includes countries that banned short sales on all stocks (where there is no benchmark group of exempt domestic stocks) and countries that imposed no bans. Hence, the estimated coefficient on the ban variables may be affected by changing differentials between country-level bid-ask spreads. To overcome this concern and perform a sharper "diff-in-diff" estimation, in columns 5–7 of Table III we restrict the estimation to the subset of 12 countries that applied short-selling bans only to financial stocks, so that in each country nonfinancial stocks perform the role of controls.

Comparing the estimates in column 5 with their counterparts in column 4 shows that in this smaller sample a short-selling ban is associated with a considerably larger increase in the bid-ask spread, and disclosure with a much larger decrease. (The same conclusion holds with the AR(1) correction.) In other words, the better identification strategy allowed by selective bans leads to stronger estimated effects than in the larger sample.

In this subsample, where bans apply only to some stocks in each country, one can also control for market-wide developments related to the financial crisis by adding day dummies to the list of explanatory variables.[9] To ease the burdensome computational task of estimating firm fixed effects and day effects at the same time, we first demean all the variables at the stock level and then perform a panel regression with day fixed effects. The resulting estimates of the short-selling variables' coefficients shown in column 6 of Table III are considerably smaller than those in column 5 (0.23 vs. 2.43 for *Naked Ban*, 0.46 vs. 2.75 for *Covered Ban*, and −0.50 vs. –1.79 for *Disclosure*), but their

[8] We also experiment with volatilities estimated on longer rolling horizons of 40 and 60 trading days. All the results are virtually unchanged.

[9] In contrast, in the subsample of countries where short-selling bans applied to all stocks, the ban dummies are perfectly collinear with the calendar dummies.

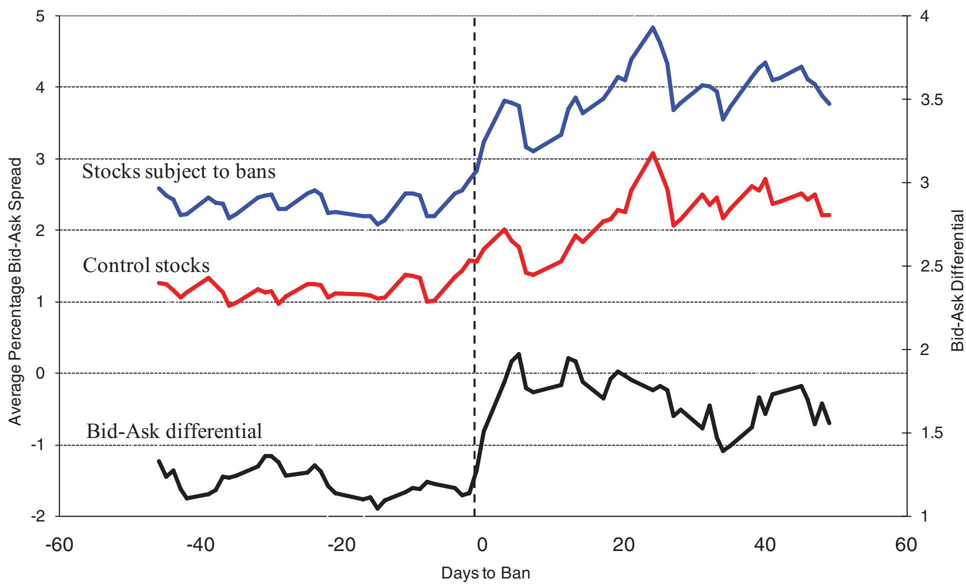

Figure 4. Average bid-ask spread of stocks subject to bans and of matched exempt stocks for countries with partial bans. The lines plots the 3-day moving average of the bid-ask spread's cross-sectional average for stocks subject to bans and control stocks (left scale) and their differential (right scale), in a 50-day window around the ban inception date (date 0). The data correspond to countries with partial bans: Belgium, Canada, Germany, Denmark, France, the Netherlands, Ireland, Norway, Austria, Portugal, the United Kingdom, and the United States.

signs and statistical significance remain the same. The estimate of the constant is close to zero because this panel regression is estimated on zero-mean variables.

B.2. Event Study Regressions

A possible concern with the panel regression estimates shown in columns 1–6 is that the impact of short-selling bans may be clouded by the inclusion of observations that are far away from the inception date of the bans. To address this concern, in column 7 we show the estimates obtained from an event study with a 50-day window before and after the ban inception date, again only for countries with partial bans.

To perform this regression, we follow Boehmer, Jones, and Zhang (2009) and match each stock subject to the ban with the exempt stock traded in the same country and with the same option listing status that is closest in terms of market capitalization and stock price (the distance criterion being the sum of the squared percentage differences in market capitalization and stock price at the beginning of the sample period). To provide a check on the quality of the control sample, in Figure 4 we plot the average bid-ask spreads of the banned stocks and their matching stocks during our event window, as well as that of their differential. The figure shows that the average bid-ask spreads of the two samples are very similar before the ban inception and diverge immediately after the ban date.[10]

The estimates from the event study regression shown in column 7 of Table III, which includes fixed effects for each pair of matched stock as well as day effects, are broadly consistent with those obtained in the panel data regressions, except for a stronger estimated impact of short-selling bans: the coefficients on the ban variables are roughly twice as large as those obtained from the panel estimation of the same specification shown in column 6 (which also includes day fixed effects), and are estimated with similar precision. In contrast, the coefficient on the disclosure variable is almost identical in size, though less precisely estimated.[11]

B.3. Endogeneity

Yet another concern with the estimates reported in Table III arises from the possible endogeneity of short-selling bans: if policy makers tend to impose such bans at times when stocks tend to become illiquid for some other reason, the correlation between short-selling bans and market illiquidity documented so far could not be interpreted as a causal relationship. To address this concern, we estimate an instrumental variables (IV) regression where the first stage is a linear probability model determining the likelihood of a ban and the second stage models its effects on liquidity. Our international panel data allow us to attack this identification problem, which would be unsolvable with a single-country data set.

As usual in these cases, the key requirement is identification of suitable instruments, that is, variables to be included only in the first stage that are correlated with the decision to impose a short-selling ban but not with the residuals of the bid-ask spread regression. In this choice, one must take into account the fact that the decision to impose a short-sale ban is a decision taken at the market-wide level, rather than a decision tailored to individual stocks. Therefore, the instruments must be market-wide variables, and must vary over time to avoid perfect collinearity with the stock-level fixed effects.

[10] In the Internet Appendix, we report the average and median spreads by country for stocks subject to the bans and for control stocks both before and during the ban, and perform statistical tests of differences in medians both before and after the ban, as well as difference-in-difference tests between pairs. The results show that the difference-in-difference in liquidity is significantly different from zero for all countries except Ireland.

[11] We also explore the robustness of these findings to the possibility that our matching criterion may generate some "bad matches" between stocks. We experiment with three simple screens. First, we exclude the pairs of matched stocks in the top 1% of the distance measure for each country; the results do not change. Second, to be more conservative, we exclude the pairs in the top 25% of the distance measure for each country; the results again do not change. Finally, we exclude from the sample all observations for the countries with the largest mean distance, since in these countries an accurate matching is harder to achieve; our findings are, if anything, even stronger than in the full sample. We report the results of these additional checks in the Internet Appendix.

We identify two candidate instruments: the lagged values of the country-level credit default swap (CDS) spreads for financial stocks and of the financial stress index proposed by Balakrishnan et al. (2009). The country average CDS spread of financial institutions is a market-based and timely assessment of insolvency risk in the financial sector. We expect countries in which this risk is greater to be more inclined to impose protective regulations such as short-selling bans on financials. The financial stress index has a similar logic, but focuses more on the systematic risk borne by financial institutions in each country, as it extracts information mainly from stock returns. Again, we expect countries in which banks are more exposed to systematic risk to be more likely to impose short-selling restrictions. Both variables turn out to have strong explanatory power in the first-stage regression. At the same time, being lagged, these two variables should not be correlated with liquidity at the individual stock level if the effect of an increase in default risk is fully impounded in contemporaneous bid-ask spreads. We find that, indeed, the instruments clearly pass the Sargan exogeneity test.

When these two variables are used as instruments in an IV panel regression with day and stock-level fixed effects, the coefficient on the ban variable is again found to be positive and significant: even accounting for their endogeneity, short-selling bans are associated with greater illiquidity. The estimated coefficient on the ban dummy (0.31) is comprised between those of the two ban dummies in column 6 of Table III, as one would expect, considering that in the IV regression we use a single ban dummy for both naked and covered bans. To preserve space, the IV estimates are reported in the Internet Appendix.

B.4. Distinguishing between Ban Inceptions and Ban Lifts

The specifications estimated in Table III impose the implicit restriction that the impact of short-selling bans on market liquidity is exactly reversed when these bans are lifted, that is, they constrain ban inceptions and ban lifts to have effects of the same magnitude and opposite sign. However, this constraint can be dropped by estimating a specification in which bid-ask spreads are regressed on two different dummy variables for ban inceptions and lifts: the first equals one for the duration of the ban and zero otherwise, exactly as the ban dummies used in Table III, and the second equals one after the ban is lifted and zero otherwise. This specification can be estimated only for bans of covered short sales because no naked bans were lifted in our sample period.[12]

In Table IV, we report the results obtained by estimating this specification using two alternative methods. In column 1, we estimate a panel OLS regression for the six countries that imposed a covered ban on financial stocks only (with nonfinancials in the same countries as the control stocks), including stock-level fixed effects. In columns 2 and 3, we instead adopt an event study method, using matched stocks for countries that lifted covered bans on financial stocks

[12] In our sample period, we only observe two countries partially lifting their bans on naked short selling of nonfinancial stocks, leaving in place the naked ban on financials.

Table IV

Bid-Ask Spreads and Short-Selling Ban Enactments and Lifts

The dependent variable is the percentage quoted bid-ask spread at the market close. In column 1, the estimates are based on the panel of daily data for the six countries that applied a covered ban to financial stocks only (Canada, Ireland, the Netherlands, Norway, the United Kingdom, and the United States). The regression is estimated by OLS with robust standard errors, and includes stock-level and day fixed effects. For computational reasons, the estimation is implemented replacing dependent and independent variables by their deviations from the respective stock-level average and including daily fixed effects in the regression. In columns 2 and 3, the estimates are based on the event study methodology described in the text using data for matched stocks in countries that lifted covered bans on financial stocks within our sample period (Canada, the Netherlands, the United Kingdom, and the United States). The regression in column 2 is estimated over a window of 50 days before and after the ban enactment date, and that in column 3 over a window of 50 days before and after the ban lift date. Both regressions are estimated by OLS with robust standard errors, and include fixed effects for matched-stock pairs and day fixed effects. *Covered Ban Enactment* is a dummy variable that equals one when covered short sales are forbidden, and equals zero otherwise. *Covered Ban Lift* is a dummy variable that equals one after a covered short sale ban was lifted, and equals zero otherwise. The numbers reported in parentheses below coefficient estimates are t-statistics. The coefficient estimates marked with three asterisks are significantly different from zero at the 1% level.

	(1)	(2)	(3)
Constant	−0.0023	0.03	0.06
	(−0.40)	(0.41)	(0.81)
Covered ban enactment	0.17***	0.61***	
	(3.79)	(3.70)	
Covered ban lift	−0.10***		−0.90***
	(−5.71)		(−2.68)
Day fixed effects	Yes	Yes	Yes
Stock-level fixed effects	Yes	Yes	Yes
Methodology	Panel	Event study	Event study
Number of observations	2,702,206	41,361	30,728
Number of stocks	7,092	710	710

within our sample period (Canada, the Netherlands, the United Kingdom, and the United States), where we employ the matching method described in Section III.B.2. The estimation period is a window of 50 days before and after ban inception in column 2, and a window of the same length around ban lift dates in column 3; this is done so as to obtain comparable estimates of the effect of ban inceptions and lifts for these four countries. Both regressions are estimated using OLS with robust standard errors, including matched-stock pairs and day fixed effects.

The results obtained using both methods show that the enactment of a ban is associated with a statistically significant increase in bid-ask spreads, and the lifting of a ban is associated with an equally significant decrease in bid-ask spreads, which provides further evidence that short-selling bans were responsible for a deterioration in market liquidity. More specifically, in the panel regression shown in column 1, the coefficient on ban enactment (0.17) exceeds that on ban lift (−0.10) in absolute value, the difference between their absolute magnitudes being statistically significant at the 5% level. In the event study regressions reported in columns 2 and 3, both estimated effects are larger than in the panel regression of column 1, and the coefficient on ban enactment (0.61) is smaller than that on ban lift (−0.90) in absolute value, although the difference between their absolute magnitudes is not statistically significant at conventional confidence levels.[13]

C. Regression Analysis: Differential Liquidity Effects

The previous section documents that the short-selling bans imposed during the financial crisis hampered stock market liquidity, whereas short-sales disclosure requirements had the opposite effect. It is thus natural to ask whether these effects were homogeneous across stocks or disproportionately affected stocks with some specific characteristics. To answer this question, in this section we investigate whether short-selling restrictions have differently affected (i) small-cap and riskier stocks (Section III.C.1), (ii) stocks with listed options (Section III.C.2), (iii) stocks listed in specific countries (Section III.C.3), and (iv) domestically or foreign listed stocks, when a cross-listing is present (Section III.C.4).

Apart from being of independent interest for policy makers, investors, and issuers, investigating whether the liquidity effects of short-selling bans differs across stocks provides a further test of our identification strategy. For instance, consider the differential impact of short-selling restrictions on stocks with and without listed options. If the availability of an option market allows traders to take short positions on the underlying stock, it should weaken the effect of short-selling restrictions on market liquidity. Therefore, finding a larger liquidity effect for nonoptionable stocks than for optionable ones would confirm that the liquidity effects documented in the previous section actually arise from short-selling restrictions.

C.1. Size and Volatility

We start by investigating whether short-selling restrictions have different effects for stocks with different market capitalization and different return volatility. It is well known that, even in the absence of short-selling constraints, market makers are more reluctant to provide liquidity for small-cap and riskier stocks than for other stocks (see Glosten and Harris (1988), Hasbrouck (1991), and Easley, Hvidkjaer, and O'Hara (2002), among others). This reluctance is likely to be compounded when market makers are unable to short stocks and hence must carry larger inventories to perform their role. In such circumstances, if faced with the choice of which stocks to stop (or reduce) trading,

[13] The regression results reported in Table IV are consistent with those obtained from country-by-country difference-in-difference tests between median bid-ask spreads for stocks subject to bans and control stocks during the ban period and after the ban is lifted. These tests, reported in the Internet Appendix, show that liquidity improves significantly after the ban is lifted in three out of the four countries that we examine (Canada, the U.K., and the U.S.).

Table V

Bid-Ask Spreads and Short-Selling Bans: Differential Effects by Size, Volatility, and Stocks with and without Listed Options

The dependent variable is the percentage quoted bid-ask spread at the market close for 25 countries (all the countries in Table I, except for the Czech Republic, Greece, Hungary, Israel, and Luxembourg). *Naked Ban* is a dummy variable that equals one if naked short sales are forbidden and covered sales are allowed, and zero otherwise. *Covered Ban* is a dummy variable that equals one if even covered short sales are forbidden, and zero otherwise. *Disclosure* is a dummy variable that equals one if the seller has to disclose his position, and zero otherwise. *Capitalization* is the company's percentile in the distribution of the capitalization of companies in its country, measured as the average of total market value in the first 6 months of 2008. Large-Cap (Small-Cap) stocks are those in the top (bottom) quartile by capitalization in the relevant country. Volatility is the standard deviation of returns, measured from the beginning of January 2008 to the end of June 2008. High (low) Volatility Stocks are those in the top (bottom) quartile by volatility in the relevant country. The regressions are estimated by OLS on daily data with robust standard errors clustered at the stock level. All regressions include fixed effects at the stock level. The numbers reported in parentheses below the coefficient estimates are *t*-statistics. The estimates marked with three asterisks are significantly different from zero at the 1% level.

	Large-Cap Stocks (1)	Small-Cap Stocks (2)	Low Volatility Stocks (3)	High Volatility Stocks (4)	Stocks with Listed Options (5)	Stocks without Listed Options (6)
Constant	4.19***	6.66***	2.59***	5.92***	0.60***	4.23***
	(563.77)	(722.20)	(314.26)	(747.00)	(193.48)	(1015.57)
Naked ban	1.24***	1.63***	1.17***	1.30***	0.33***	1.40***
	(8.83)	(7.78)	(5.59)	(9.92)	(5.94)	(12.24)
Covered ban	1.81***	2.57***	1.75***	1.85***	0.67***	2.14***
	(19.66)	(13.73)	(11.17)	(19.52)	(9.66)	(25.95)
Disclosure	−0.76***	−0.53***	−0.73***	−0.66***	−0.20***	−0.72***
	(−5.83)	(−2.44)	(−3.63)	(−5.22)	(−3.42)	(−6.54)
Stock-level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,846,401	1,069,289	1,314,501	1,193,031	427,164	4,716,009
Number of stocks	6,538	3,561	4,144	4,017	1,306	15,185

market makers should be more likely to withdraw from smaller and riskier ones.

The estimates in Table V are consistent with this prediction. In columns 1 and 2, the regression is estimated separately for the top and bottom quartiles of the companies by capitalization, where the quartiles are computed separately for each country and the capitalization is measured as the average of total market value in the first half of 2008. The coefficient on the ban dummies is about 30% to 40% larger for smaller stocks, the difference being significantly different from zero at the 1% level. Qualitatively similar results (not shown in the table) obtain if the regression is estimated separately for the stocks above and below the median capitalization in each country, as well as in an expanded specification where the ban dummy variable enters both in level and multiplicatively with the corresponding company's percentile in its country's distribution of stock capitalization in the first half of 2008. The estimates of this expanded specification imply that the ban had almost no effect on the stocks in the top percentile of the size distribution, whereas for those in the bottom percentile its effect was about twice as large as for the median stock.

A similar picture emerges when the estimation is performed separately for low and high volatility stocks, where volatility is measured using stock returns in the first 6 months of 2008. Columns 3 and 4 of Table V show that the coefficient on the ban dummy is about 10% larger for stocks in the top volatility quartile than for those in the bottom quartile. The difference between the ban coefficients across the two subsamples is not statistically significant, but if one uses a single ban dummy variable for both naked and covered bans, the coefficient on the ban variable for high-volatility stocks is statistically larger than for low-volatility stocks.

C.2. Optionable Stocks

During short-selling bans, investors could still effectively take short positions by trading in the option markets because ban regulations did not impose any direct restriction in derivative markets. Battalio and Schultz (2011) document that the ratio of option-to-stock volume for U.S. markets is comparable for banned and control stocks throughout the preban and ban periods. Although this evidence suggests that investors did not seem to migrate to the option market to gain short exposure in financial stocks, it also indicates that, for stocks with listed options, investors could use option markets to gain short exposure during the short-sale ban.

To investigate if bans' liquidity effects differ in the two cases, we classify stocks into those that have traded options and those that do not. To do so, we obtain a record of all stocks with traded options for all the countries in our sample using information from national option exchanges, and for most countries we are able to cross-check the list of stocks with the availability of equity option prices in Datastream.

As stated in the introduction to this section, we expect the effects of short-selling restrictions on bid-ask spreads to be stronger for stocks without a listed option than for those with a listed option.[14] The results are presented in columns 5 and 6 of Table V. As expected, we find a strikingly stronger effect of short-selling bans on liquidity for stocks without listed options. For countries that imposed a naked ban, the average percentage bid-ask spread increase is more than four times larger for stocks that do not have listed options. The economic impact is similar for countries that imposed a covered ban: the effect for stocks with no listed options is three times larger than for stocks with listed options. These differences are statistically different at the 1% level.

[14] The stocks with listed options in our sample tend to have relatively large capitalization and volatile returns, consistent with Mayhew and Mihov (2004), who show that exchanges tend to list options on stocks with high volatility and market capitalization. Based on the evidence of the previous subsections, these two characteristics should influence in opposite directions the effect of short-sale bans on the liquidity of optionable stocks.

Table VI

Bid-Ask Spreads and Short-Selling Bans: Country-by-Country Estimates

The dependent variable is the percentage quoted bid-ask spread at the market close. The estimation is effected via a separate OLS regression for each country with fixed stock-level effects (using the same specification as in column 1 of Table III), and is based on daily data for 25 countries (all the countries in Table I, except for the Czech Republic, Greece, Hungary, Israel, and Luxembourg). The table summarizes the individual regression estimates. *Naked Ban* is a dummy variable that equals one if naked short sales are forbidden and covered sales are allowed, and zero otherwise. *Covered Ban* is a dummy variable that equals one if even covered short sales are forbidden, and zero otherwise.

Constant	Average coefficient	3.83
	Number of estimates	25
	Number positive	25
	Positive and significant at 1% level	25
	Number negative	0
	Negative and significant at 1% level	0
Naked ban	Average coefficient	0.98
	Number of estimates	11
	Number positive	11
	Positive and significant at 1% level	10
	Number negative	0
	Negative and significant at 1% level	0
Covered ban	Average coefficient	1.24
	Number of estimates	10
	Number positive	10
	Positive and significant at 1% level	10
	Number negative	0
	Negative and significant at 1% level	0
Stock-level fixed effects		Yes
Total number of observations		5,143,173
Total number of stocks		16,491

As explained above, these results are important not only because they suggest that the presence of derivative markets mitigated the adverse effects of short-selling bans on liquidity, but also because they provide further evidence that the reduction in liquidity that we document is indeed related to the ban enactment.

C.3. Country of Listing

It is also worth exploring whether the effect of short-selling bans on liquidity is present in all the countries in our sample, and whether it differs appreciably across them. In Table VI we relax the implicit constraint of the panel analysis that the coefficients on the explanatory variables be the same across countries.[15] This is equivalent to estimating the regression separately for each

[15] The specification is the same as in column 1 of Table III except for the exclusion of *Disclosure*, which we exclude because it is perfectly collinear with *Naked Ban* or *Covered Ban* (except for Portugal, where disclosure was required for all stocks whereas the naked ban is on financials only, and for Hungary, which imposed disclosure but no ban).

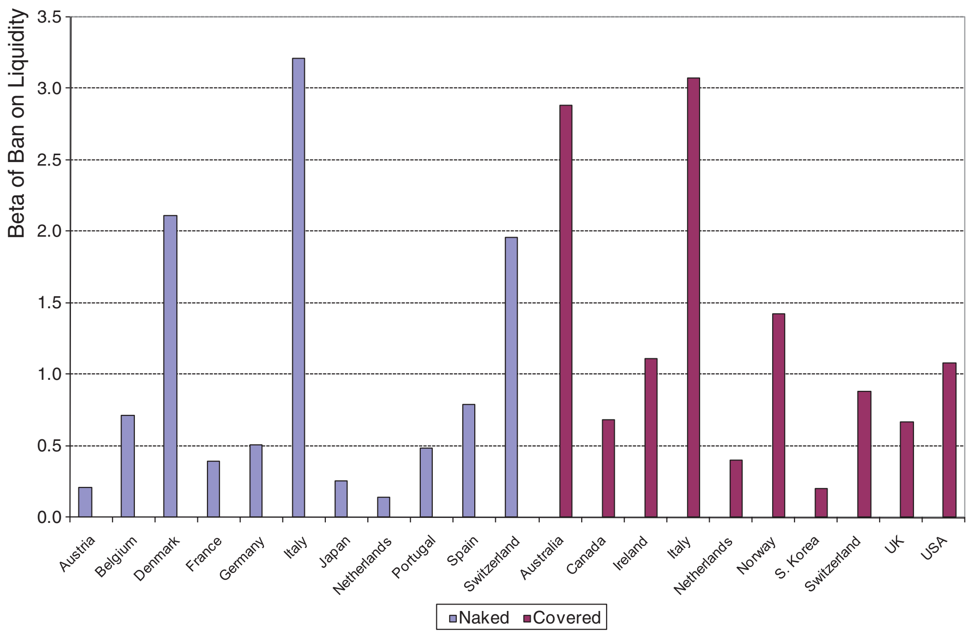

Figure 5. Impact of short-selling ban on the percentage quoted bid-ask spread, by country. The height of each bar corresponds to the estimated coefficient of *Naked Ban* or *Covered Ban* in the regressions of Table VI.

country while retaining stock-level fixed effects. The results indicate that, even when unconstrained, the slope coefficients on the short-selling restrictions are positive and significant for almost all countries.[16]

The individual country coefficient estimates are displayed in Figure 5, separately for the *Naked Ban* and *Covered Ban* variables. Italy emerges as the country where the ban on short sales was associated with the most dramatic deterioration in market liquidity, followed by Denmark, Australia, and Norway. The United States, the United Kingdom, and Ireland are in an intermediate group, whereas in the remaining countries' short-selling bans are associated with comparatively mild increases in bid-ask spreads—on the order of about 50 basis points or less.[17]

These large cross-country differences in the impact of short-selling bans partly reflect the different characteristics of national stock markets. In cross-country regressions reported in the Internet Appendix, we explore whether the estimates of the ban coefficients in the country-by-country regressions correlate with median stock size (as measured by market capitalization), median

[16] The only country for which *Naked Ban* on financial stocks is not significant is the Netherlands (p-value $= 0.14$). However, in that country, the naked ban lasted only 2 weeks before being converted into a covered ban.

[17] These differences between country-specific coefficients are statistically significant at the 1% level.

return volatility, and ownership concentration of the respective stock markets. The inclusion of size and volatility is justified by the results of Table V, which suggest that the effect of short-selling bans should be stronger in countries with a larger fraction of small-cap and volatile stocks. We also include the concentration of stock ownership because stocks with more concentrated ownership feature fewer floating shares and therefore lower liquidity. We therefore expect the effect of short-selling bans to be more dramatic in such countries. The results are consistent with these priors, even though the estimates are not very precise, probably because of the small number of observations: the ban coefficients are larger in the countries whose listed companies have smaller capitalization, more volatile returns, and more concentrated ownership, that is, in the markets where liquidity is more of an issue even in the absence of short-selling bans.[18]

C.4. Cross-Listed Stocks

Finally, it is interesting to consider how short-selling bans affected dual listed stocks, which were sometimes subject to a short-selling ban in only one of the two countries of listing. In this case, we need to control for the effects of two ban regimes, the domestic one and the foreign one. The question is whether the two regimes had the same effects on market liquidity, and whether short-selling restrictions have cross-border spillover effects.

We concentrate on the 126 non-U.S. stocks listed on NYSE or NASDAQ as well as on a non-U.S. market. When such stocks were subject to a short-selling ban, in 82% of the cases the ban applied to both the domestic market and the U.S. market; for most of the remaining dual listed stocks, the ban was enacted only domestically.

Table VII shows that a domestic ban decreases liquidity not only in the home market but also in the foreign one; in contrast, a ban in the foreign market decreases liquidity only within that market. So when a ban is imposed at home, its effects spill over abroad, whereas the opposite is not true. These results suggest that the domestic market is the key one for the provision of liquidity both at home and in the U.S. market, in line with its dominant role in trading activity highlighted by Halling et al. (2008).

IV. Price Discovery

As highlighted in Section I, although the effect of a short-selling ban on bid-ask spreads is in principle ambiguous, its effect on the speed of price discovery is unambiguously predicted to be negative. By restraining trading by investors with negative fundamental information, a short-selling ban should slow price discovery, and more so in bear markets.

[18] Other country and market characteristics, such as the quality of legal enforcement and the fraction of optionable stocks, turn out to have no explanatory power in these cross-country regressions for the differential effects of short-selling bans.

Table VII

Bid-Ask Spreads and Short-Selling Bans for Dual Listed Stocks

The dependent variable is the percentage quoted bid-ask spread on the domestic market (in columns 1 and 3) or on the U.S. market (in columns 2 and 4) for dually listed stocks. *Ban* is a dummy variable that equals one if short sales, either naked or covered, are forbidden, and zero otherwise. The regressions in columns 1 and 2 are estimated with daily data for all dual listed stocks in the United States. The regressions in columns 3 and 4 are estimated for the subset of stocks whose countries imposed a ban on financial stocks only. All estimates are obtained using OLS, with robust estimates of the standard errors clustered at the stock level, and include fixed effects at the stock level. The numbers in parentheses below the coefficient estimates are t-statistics. The estimates marked with two asterisks are significantly different from zero at the 5% level. The differences marked with three asterisks are significantly different from zero at the 1% level.

	Domestic Market Liquidity (1)	U.S. Dual Listing Liquidity (2)	Domestic Market Liquidity (3)	U.S. Dual Listing Liquidity (4)
Constant	1.00***	0.84***	0.51***	0.73***
	(97.28)	(37.93)	(22.81)	(4.55)
Ban on domestic market	0.17***	0.62***	0.08***	0.76***
	(3.07)	(5.35)	(3.36)	(13.44)
Ban on U.S. market	−0.03	0.79***	−0.03	0.36**
	(−0.78)	(5.20)	(−0.49)	(2.32)
Stock-level fixed effects	Yes	Yes	Yes	Yes
Number of observations	42,371	46,181	18,767	19,295
Calendar dummies	No	No	Yes	Yes
Number of stocks	131	133	56	56

To test this prediction, we estimate a market model regression in which weekly returns for each stock in our sample are regressed on the corresponding broad national stock market index from January 2008 to June 2009. The choice of the weekly frequency is motivated by similar approaches in the literature (e.g., Bris, Goetzmann, and Zhu (2007)) that find this horizon strikes an optimal balance between noise and information. The analysis is carried out on residuals, on the assumption that the ban should slow the discovery of firm specific rather than market-wide information. If the data are consistent with the predictions of the theory, the autocorrelations should be significantly higher during the ban period, especially for negative returns.

Column 1 of Table VIII shows the median autocorrelation of residuals for two subsamples: (i) stocks exempt from bans and nonexempt stocks in periods when no ban was imposed ($Ban = 0$) and (ii) nonexempt stocks during the ban period ($Ban = 1$). Importantly, this sample breakdown does not have a perfect correlation with time because different countries imposed bans at different points in time, and some imposed partial bans or did not impose any ban at all. The figures in column 1 show that the autocorrelation of residuals is positive in both subsamples, but is larger for stocks subject to short-selling bans. Because the distribution of the autocorrelation statistic is not normal, we test for the difference between the two samples using two nonparametric tests for the equality of medians: the K test and the two-sample Wilcoxon rank-sum

Table VIII

Price Discovery and Short-Selling Bans

Column 1 of the table shows the median value of the first-order autocorrelation of residuals from a market model regression of weekly returns for different subsamples. *Ban* is a dummy variable that equals one if short sales, either naked or covered, are forbidden, and is zero otherwise. The market model regression is estimated with weekly returns data for all individual stocks from 30 countries from January 2008 to June 2009 using a national broad stock market index as the market proxy. Column 2 shows the median cross-autocorrelation between individual stock returns and the corresponding lagged market return, when the latter is negative, in each of the two subsamples, and the difference between the two. Column 3 reports the same statistics for positive or zero market returns. Column 4 reports the median of the difference between the downside cross-autocorrelation and the upside cross-autocorrelation. The bottom row shows the difference between the medians of the two subsamples, and the numbers in parentheses are the p-value of the K nonparametric test for the equality of medians. The differences marked with three (two) asterisks are significantly different from zero at the 1% (5%) level.

	Median Autocorrelation of Market Model Residuals (1)	Median Downside Cross-Autocorrelation between Stock Returns and Market Returns (2)	Median Upside Cross-Autocorrelation between Stock Returns and Market Returns (3)	Median of the Difference between Downside and Upside Cross-Autocorrelation (4)
Ban = 0	0.0824	0.2833	0.2340	0.0358
Ban = 1	0.1011	0.3552	0.2638	0.0565
Difference	0.0187***	0.0719***	0.0298***	0.0207**
	(0.0000)	(0.0000)	(0.0000)	(0.0470)

(Mann–Whitney) test (not shown in the table). According to both, the difference is statistically significant at the 1% level. This finding is consistent with a lower speed of price discovery during the ban period.

We verify the robustness of this evidence using an alternative approach based on a variance ratio test, performed separately for stocks subject and not subject to a short-selling ban. We find that the hypothesis that stocks' returns are approximated by a random walk cannot be rejected in 53% of the cases for nonbanned stocks, but only in 39% of the cases for banned stocks, the difference being statistically different from zero at the 1% level. These findings confirm previous evidence that information is revealed more slowly when stocks are subject to a short-selling ban.[19]

Because short-selling bans are intended to limit the activity of investors with bearish views, they should slow price discovery more in overall declining markets than in rising ones. To gauge whether such an asymmetric effect is present in the data, we perform a test proposed by Bris, Goetzmann, and Zhu (2007): we compute cross-autocorrelations between individual stock returns and market returns lagged by 1 week separately for negative lagged market returns and

[19] The consistency between the analysis based on autocorrelations and the analysis based on variance ratio tests is in line with the latter being approximately a linear combination of the autocorrelation coefficient estimators of the first differences with arithmetically declining weights.

positive ones. More specifically, we calculate a "downside cross-autocorrelation" $corr(r_{ict}, r^{-}_{mct-1})$ and an "upside cross-autocorrelation" $corr(r_{ict}, r^{+}_{mct-1})$ for each stock i in country c (where r^{-}_{mct-1} and r^{+}_{mct-1} are negative and positive observations on market returns, respectively) and then compute the median values of these two sets of stock-level statistics. The results, respectively shown in columns 2 and 3, indicate that (i) both the median upside and median downside cross-autocorrelations are positive and significantly larger during ban periods, (ii) the median downside cross-autocorrelation exceeds the upside one, and, most importantly, (iii) the difference between the two is significantly larger when short sales are banned. In column 4, we show the median difference between downside and upside cross-autocorrelations in each of the periods when short sales are banned and not banned, and in the bottom cell we report the difference between the two subsamples. This evidence indicates again that not only do short-selling bans slow price discovery, but they do so especially during overall market declines, consistent with theoretical predictions.

V. Stock Prices

Regulators impose short-selling bans primarily because they expect such bans to help stem financial panics. The bans imposed during the 2007 to 2009 financial crisis were no exception in this respect. In terms of Miller's (1977) model, stock market regulators may have regarded the bans as necessary to prevent "underpricing" of stocks: they probably feared that, with optimistic investors largely neutralized by funding constraints, unbridled short sales would trigger an unwarranted collapse in share prices.[20] Indeed, Brunnermeier and Oehmke (2008) argue that such intervention may be temporarily justified for the stocks of financial institutions, when these become vulnerable to predatory short selling, as aggressive short selling may cause such institutions to violate their regulatory capital constraints and force them to liquidate long-term investments at fire-sale prices. In this section, we examine whether the bans provided effective support for the prices of financial stocks when benchmarked against exempt stocks.

The most immediate evidence obtains by focusing on the countries in which the ban did not apply universally, and comparing the postban median cumulative excess returns for stocks subject to bans with those of exempt stocks, where excess returns are defined as the difference between individual stock returns and the respective country's equally weighted market indices. This "visual diff-in-diff" evidence is presented in Figures 6 and 7, separately for the United States and for other countries that imposed bans only on financial stocks.

The reason for plotting excess returns separately for the United States and for other countries is that in the United States the effect of the ban on financial

[20] Shkilko, Van Ness, and Van Ness (2012) document that short sales may increase downward pressure on prices even in the absence of negative information: they study large negative price reversals on no-news days and find that short selling during these reversals substantially amplifies price declines.

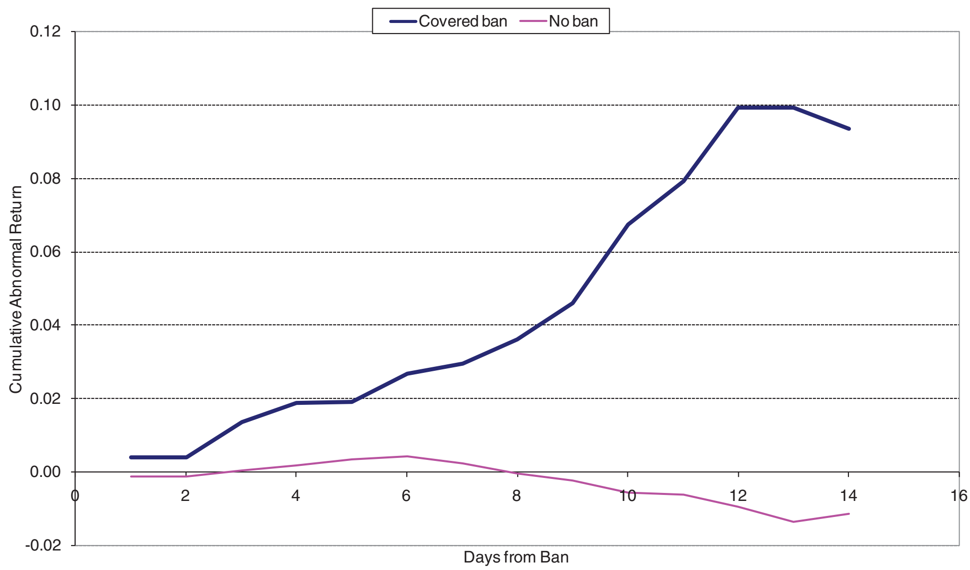

Figure 6. Cumulative abnormal returns in the United States for stocks subject to covered bans and for exempt stocks. The figure plots cumulative abnormal returns in the 14 trading days after the ban date, which corresponds to date 0 in the graph.

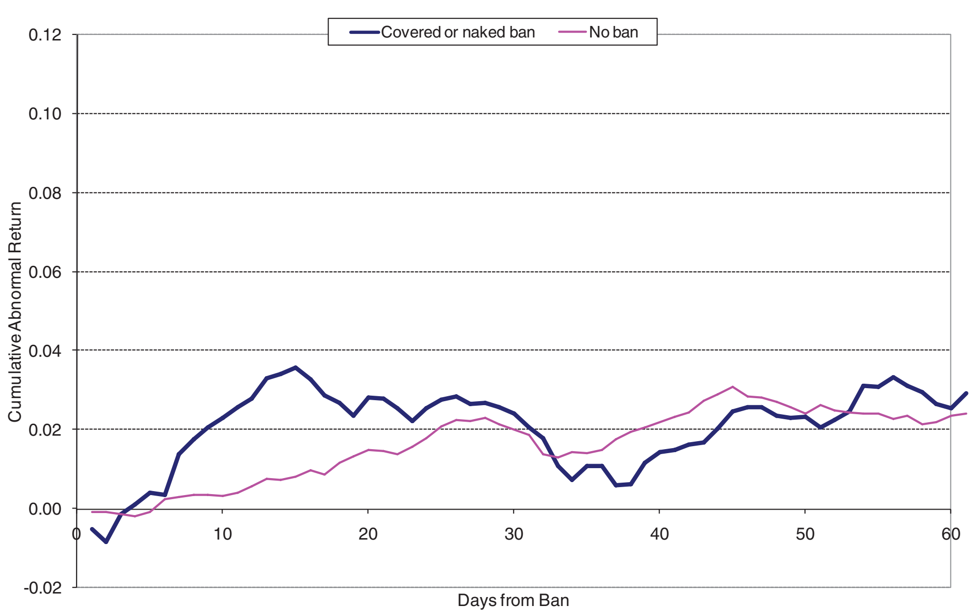

Figure 7. Cumulative abnormal returns in countries with partial bans (except the United States) for stocks subject to ban and exempt stocks. The figure plots cumulative abnormal returns in the 60 trading days after the ban date, which corresponds to date 0 in the graph.

stock prices may be clouded by the concomitant announcement of TARP, which aimed to support U.S. financial institutions, a confounding factor not present in other countries that banned short sales on financials. Indeed, returns appear to have behaved quite differently in the United States and elsewhere during short-selling ban periods. Figure 6 shows that the median cumulative excess return of U.S. financial stocks, which were subject to a covered ban, exceeded that of exempt stocks throughout the 14 trading days after ban inception (date 0 in the figure), a finding that agrees with that reported by Boehmer, Jones, and Zhang (2009) for the U.S. market. In contrast, Figure 7 shows that this did not occur in other countries: the line corresponding to the median excess return on stocks subject to naked and covered bans is very close to that for exempt stocks, and it lies above the line for exempt stocks only in about half of the first 60 days of trading after ban inception. Because, as noted above, the positive effect shown in Figure 6 for the United States may result from the TARP announcement rather than from the ban itself, Figure 7 is likely to convey a more accurate picture of the ban's effects on stock returns.

To go beyond the visual scrutiny of these figures, in Table IX we regress weekly excess returns on the *Naked Ban*, *Covered Ban*, and *Disclosure* dummies, plus stock-level fixed effects to control for the risk characteristics of individual stocks. The regressions in columns 1 and 2 correspond to the United States alone, whereas those in column 3 and 4 correspond to all other countries that imposed short-selling bans only on financial stocks. As in Figures 6 and 7, excess returns are defined as differences between raw returns and the respective equally weighted market indices. We drop observations for which the raw weekly return is zero, to avoid biases arising from stale prices because of nontrading.

In Table IX, we use two different approaches to identify the effect of short-sales restrictions. In columns 1 and 3, we report standard panel estimates where the control group is formed by all the stocks that were not subject to bans, respectively for the United States and for other countries with partial bans.[21] In columns 2 and 4, the estimates are obtained using an event study methodology—again, respectively for the United States and for other countries with partial bans—with a 50-day window before and after the ban inception date. As in the liquidity regressions shown in column 7 of Table III, each stock subject to the ban is matched with the exempt stock traded in the same country and with the same option listing status that is closest in terms of market capitalization and stock price.[22]

[21] In the Internet Appendix we report the average and median excess returns by country for stocks subject to the bans and for control stocks both before and during the ban, and perform statistical tests of differences in medians both before and after the ban, as well as tests of difference-of-difference of pairs. The results show that difference-of-difference of returns are statistically significant (and positive) only for the U.S., Canada, and Denmark, and marginally significant (but negative) for Belgium.

[22] This matching algorithm yields similar stock returns for banned and control stocks before the ban inception date: their difference before this date is not statistically different from zero (the t-statistic being 0.17 for U.S. stocks, -0.15 for non-U.S. stocks, and 0.16 for the pooled sample).

Table IX
Stock Returns and Short-Selling Bans

The dependent variable is the weekly excess return for each stock, defined as the difference between the raw return and the country equally weighted market index. We drop all observations in which the raw stock return is zero to avoid nontrading biases. *Naked Ban* is a dummy variable that equals one if naked short sales are forbidden and covered sales are allowed, and is zero otherwise. *Covered Ban* is a dummy variable that equals one if even covered short sales are forbidden, and is zero otherwise. *Disclosure* is a dummy variable that equals one if the seller has to disclose his position, and zero otherwise. The specifications in column 1 and 2 are estimated only on data for the United States and those in columns 3 and 4 are estimated on data for all the other countries with partial bans. The estimates in columns 1–3 are based on the panel data for these countries, whereas those in columns 2 and 4 are based on matched stocks using the event study methodology described in the text. All regressions are estimated using OLS with robust standard errors clustered at the stock level, and include fixed effects at the stock level and weekly time effects. The numbers reported in parentheses below the coefficient estimates are t-statistics. The coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1% (5%) level.

	(1)	(2)	(3)	(4)
Constant	0.0583***	0.0022***	−0.0017***	−0.0008***
	(29.82)	(10.78)	(−58.50)	(−1.77)
Naked ban			−0.0026	−0.0081***
			(−0.67)	(−3.13)
Covered ban	0.0611***	0.0041***	−0.0004	−0.0025
	(18.82)	(3.77)	(−0.12)	(−0.67)
Disclosure			0.0066	−0.0006
			(1.17)	(0.17)
Stock-level fixed effects	Yes	Yes	Yes	Yes
Weekly fixed effects	Yes	Yes	Yes	Yes
Countries in the sample	United States	United States	Countries with partial ban except United States	Countries with partial ban except United States
Methodology	Panel data	Event study	Panel data	Event study
Number of observations	245,631	43,973	299,980	7,695
Number of stocks	3,717	1,354	5,369	240

The estimates in Table IX confirm the visual evidence drawn from the figures. The U.S. stock market response to short-selling bans is positive and significant, regardless of whether we consider the panel estimates in column 1 or the event study estimates in column 2. In contrast, for other countries with partial bans, the coefficients on the ban variables are not significantly different from zero in the panel data estimates of column 3. The corresponding estimates obtained with the event study methodology are reported in column 4: the covered ban coefficient is again not significantly different from zero, and the naked ban's coefficient is negative and significant.[23] Therefore, in countries other than

[23] As for the liquidity regression in column 7 of Table III, the results reported in columns 2 and 4 of Table IX are also robust to potential "bad matches" generated by our matching criterion. Specifically, we exclude the pairs of matched stocks in the top 1% of the distance measure for each country, then those in the top 25% of this measure, and finally we drop from the sample observations for the countries with the largest mean distance. In all three cases, the findings in

the United States, short-selling bans are associated either with no significant change or with a decline in stock returns (consistent with the predictions of Hong and Stein (2003) and Bai, Chang, and Wang (2006)).[24]

Finally, we try to deal with the possible endogeneity of the ban enactment by estimating an IV regression for stock returns, as done for liquidity in Section III.B. Specifically, the first stage is a linear probability model determining the likelihood of a ban, whereas the second stage models its effects on excess returns and includes calendar and stock-level fixed effects. We use the same two instruments employed for the ban dummy variable in the liquidity regression, namely, the lagged values of country-level CDS spreads for financial stocks and of the financial stress index of Balakrishnan et al. (2009). In the IV panel regression, which is estimated on data for all countries with partial bans (including the United States), the coefficient on the ban is again not significantly different from zero. In this case, however, the instruments are weaker than in the liquidity regression, suggesting more caution in the interpretation of the IV findings.[25] To preserve space, the estimation results are reported in the Internet Appendix.

In summary, the results for the United States are the exception rather than the rule around the world—an exception that may be explained by the confounding effect of the concomitant TARP announcements, as argued by Boehmer, Jones, and Zhang (2009). In other countries, besides damaging market liquidity, bans on short sales appear to have failed to support market prices, thereby missing regulators' prime objective.

VI. Conclusions

The evidence in this paper suggests that the reaction of most stock exchange regulators around the globe to the financial crisis—imposing bans or regulatory constraints on short selling—was detrimental for market liquidity, especially for stocks with small market capitalization, high volatility, and no listed options. Moreover, it slowed price discovery, and hence was at best neutral in its effects on stock prices.

columns 2 and 4 of Table IX are qualitatively unchanged. We report the results of these robustness checks in the Internet Appendix.

[24] If the panel regressions are estimated by pooling U.S. and non-U.S. data, the coefficients on both ban variables turn out to be not significantly different from zero. In contrast, if the matching methodology is applied to the pooled data set, the results are similar to those obtained using non-U.S. data only: the *Naked Ban* variable has a negative and significant coefficient, while the coefficients on the *Covered Ban* and *Disclosure* variables are not significantly different from zero. We also re-estimate the regressions in Table IX with an AR(1) correction, and the results are virtually unchanged. Finally, we estimate event study regressions to assess the impact of ban lifts using a window of 50 days before and after the lift dates, and find that ban lift is associated with a significant reduction in U.S. excess stock returns, but there is no significant change in excess stock returns for the pooled data of Canada, the U.K., and the Netherlands. Thus, these results (which are reported in the Internet Appendix) are fully consistent with those shown in Table IX.

[25] The p-value for the robust Sargan test of the exogeneity of the instruments is 5%.

The ban-induced decrease in market liquidity is especially serious because it came at a time when bid-ask spreads were already high as a result of the crisis and investors were desperately seeking liquid security markets because of the freeze in many fixed-income markets. Our findings on international data complement and confirm the results reported for the United States by Boehmer, Jones, and Zhang (2009), and show that in other countries the ban's effects were worse than in the United States: the implied liquidity reduction was larger, and, in contrast with the United States, the effect on stock returns was not significantly positive. In fact, our estimates based on the matching methodology suggest that the ban of naked short sales is associated with lower returns for non-U.S. countries.

Perhaps the main social payoff of this worldwide policy experiment has been in generating a large amount of evidence about the effects of short-selling bans. The conclusion that this paper draws from this evidence is best summarized by the words of the former SEC Chairman quoted at the start of this paper: "Knowing what we know now, . . . [we] would not do it again. The costs appear to outweigh the benefits." It is to be hoped that this lesson will be remembered when security markets face the next crisis.

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