Duisenberg school of finance - Tinbergen Institute Discussion Paper

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

Alessandro Beber and

Marco Pagano

January 2010

DSF/TI 2010-1

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

Alessandro Beber
Duisenberg school of finance, University of Amsterdam and CEPR

Marco Pagano
Università di Napoli Federico II, CSEF, EIEF and CEPR

Abstract

Most stock exchange regulators around the world reacted to the 2007-2009 crisis by imposing bans or regulatory constraints on short-selling. Short-selling restrictions were imposed and lifted at different dates in different countries, often applied to different sets of stocks and featured different degrees of stringency. We exploit this considerable variation in short-sales regimes to identify their effects with panel data techniques, and find that bans (i) were detrimental for liquidity, especially for stocks with small market capitalization, high volatility and no listed options; (ii) slowed down price discovery, especially in bear market phases, and (iii) failed to support stock prices, except possibly for U.S. financial stocks.

JEL classification: G01, G12, G14, G18.

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

Acknowledgements: We thank Viral Acharya, Bruno Biais, Dimitris Christelis, Anne de Graaf, Joost Driessen, Charles Jones, Ailsa Röell, Enrique Schroth, Erik Theissen, Tullio Jappelli and seminar participants at the 2009 NYSE-Euronext-Tinbergen Institute Workshop on "Liquidity and Volatility in Today's Markets", at the University of Naples Federico II and at the Autoritaet Financiële Markten. Richard Evers, Martijn Reekers and Piyush Singh provided outstanding research assistance. Financial support from Inquire Europe and Q-Group is gratefully acknowledged.

> "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).

1. Introduction

Most stock exchange regulators around the world reacted to the financial crisis of 2007-2009 by imposing bans or regulatory constraints on short-selling by market participants. These hurried interventions, which in many countries were selective and varied considerably in intensity and duration, were presented as measures to restore the orderly functioning of securities markets and limit unwarranted drops in securities prices.

However, theoretical reasons and previous evidence cast doubt on the benefits of short-selling bans, in particular for the liquidity and the price discovery function of securities markets. Since the crisis was accompanied by a steep increase in bid-ask spreads in stock markets, it is important to understand whether and to what extent short-selling bans contributed to their increase. If one were to conclude that, far from restoring "orderly market conditions" as claimed by policy makers, these interventions actually reduced market liquidity, this would be a serious indictment of their adoption, especially considering that they were enacted at a time when market participants desperately sought liquidity on stock markets, due to the freeze of the structured debt and interbank markets.

In this paper we bring the large amount of evidence generated by the crisis to bear on this issue: the flurry of short-selling bans generated a 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 featured different degrees of stringency: all these features make the data ideally suited to identify the effects of the bans through panel data techniques.

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

Since the literature suggests that bid-ask spreads differ across stocks, due to their risk characteristics, average trading volume, number of market makers, and so on, in the estimation we use stock-level fixed effects to control for time-invariant stock characteristics. We also control for return volatility, since its changes may affect bid-ask spreads by changing the inventory risk of market makers. Finally, in some specifications we control also for common changes in liquidity by including day fixed effects and other time-varying crisis-related factors, to take into account commonality in liquidity, especially 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 increase in bid-ask spreads, controlling for other variables. Instead, the obligation to disclose short sales is associated with a significant decrease in bid-ask spreads. The same effects are found when illiquidity is measured by the Amihud illiquidity indicator.

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 and more volatile 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, consistently with the idea 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, while bans in the foreign market only reduce liquidity locally, without spillover liquidity effects to the home market.

The evidence also shows that short-selling bans slow down price discovery, especially when negative news are concerned, in line with the predictions of the theory and with the findings of previous empirical studies. Finally, the bans are not associated with better stock price performance, the U.S. being the only exception. In fact, when we use our entire data set, bans on covered short sales turn out to be correlated with significantly lower excess returns relative to stocks unaffected by the ban, while bans on naked sales and disclosure obligation do not have a significant correlation with excess returns. When we restrict the analysis to countries with short-selling bans on financials only, we find that bans on excess returns are significantly correlated with positive excess returns only for the U.S. (in line with the results by Boehmer et al., 2009), not for other countries. However, the positive correlation for the U.S. may reflect concomitant policy announcements in support of U.S. financial institutions, and thus may be spurious. Therefore, in contrast to the regulators' hopes, the overall evidence indicates that short-selling bans have at best left stock prices unaffected, and at worst may have contributed to their decline.

The paper is structured as follows. Section 2 briefly reviews the relevant literature and on its basis it develops the testable hypotheses. Section 3 presents the data and methodology. Sections 4 reports descriptive evidence and regression analysis about the impact of short-selling restrictions on market liquidity, and investigates whether such liquidity effects differ across stocks with different characteristics. Section 5 and 6 present the results about the impact of short-selling restrictions on price discovery and on stock prices, respectively. Section 7 concludes.

2. Testable hypotheses and previous evidence

Most work on short-selling bans has considered their effects on three variables: market liquidity, price discovery and stock overpricing, with the latter taking the lion's share. In the present study we focus mainly on their effects on liquidity, but also address the other two. 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.

2.1 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 (consistently with intuition and with much evidence), a short-selling ban will reduce the fraction of informed traders in the pool of "sell-or-short" transactions. On this account the ban would tend to reduce the bid-ask spread, for a given amount of information revealed by past trades. But since the ban also slows the revelation of such information, the overall effect on the bid-ask spread is ambiguous.

The effect of short-selling bans on liquidity has not been examined in models of dealers with inventory holding costs. However, intuition suggests that in such models a short-selling ban should increase the bid-ask spread, by making it more costly for market makers to provide liquidity: the inability to short the stock renders inventory management more difficult for market makers, which is especially serious at times of high volatility, such as the crisis period. And even if market makers manage to retain access to short-selling, the ban will limit competition by other liquidity suppliers and therefore should allow market makers to widen their spreads on this account. Moreover, by excluding from trading informed investors with negative information, short-sale constraints make market prices less informative and thus increase the risk to uninformed market participants (Bai et al., 2006). Therefore, if market makers are uninformed, they will widen their bid-ask quotes to cover their increased inventory holding costs. So, when market participants are risk-averse, the predicted effect of a short-selling ban on bid-ask spreads is more clear-cut than in the Glosten-Milgrom setting with adverse selection.

The limited evidence available so far is on the whole consistent with the idea that short-selling bans damage liquidity, though not unambiguously. The piece of evidence most directly related to the present study is the concurrent paper by Boehmer, Jones and Zhang (2009), who analyze with panel data techniques the response of measures of liquidity to the short-selling ban imposed from September 18 to October 8 in the United States, exploiting the difference between the (financial sector) 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 Kolasinksi, Reed and Thornock (2009), who also find that the June 2008 emergency order that already restricted naked short selling for 19 stocks had a similar adverse effect on liquidity.

However, other studies report more ambiguous or even conflicting evidence. Jones and Lamont (2002), who investigate the change in liquidity around events during the Great Depression that altered the level of short-sale constraints in the U.S., find that the introduction of the requirement that brokers secure written authorization before lending a customer's shares in 1932 had a negative impact on liquidity, but the requirement that short selling be executed only on an up tick in 1938 had a positive effect on liquidity. Charoenrook and Daouk (2005), who investigate the effects of market-wide short-sale restrictions on a number of variables for 111 countries, find that short-sale restrictions correlate with greater market-wide liquidity, as measured by total stock market trading volume. However, trading volume is known to be a poor proxy for market liquidity.

While most of these studies are based on U.S. evidence, our contribution analyzes how liquidity reacted to short selling bans in 30 countries, and therefore exploits the considerable cross-country variation in the bans' enactment and lifting dates, in their stringency and in their coverage in order 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), because we rely on individual stock market data rather than country market indices, and measure liquidity with bid-ask spreads and the Amihud illiquidity index rather than with trading volume, notoriously a problematic proxy for liquidity. This is particularly true of the crisis period, when increases in bid-ask spreads have often been associated with greater trading volumes.

2.2 Speed of price discovery

The predicted effect of short-selling bans on the speed of price discovery is more clear-cut than it is for 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 in stock prices, probably in the belief that such bad news are "unwarranted", in the sense that they reflect a negative bubble or herding behavior rather than fundamental information.

This prediction is tested by Bris, Goetzmann and Zhu (2007) with data on short-sale restrictions for 46 equity markets around the world. They find that prices incorporate 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 by Saffi and Sigurdsson (2007) and by Boehmer and Wu (2008) that the ability to short sell stocks increases the informational efficiency of market prices. They are also consistent with the result by 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 (2009) report that during the 2008 ban period in the U.S. 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 of short sales, as indeed was the case in the U.S. during the crisis.[1]

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

2.3. Overpricing

Miller (1977) predicts that short-selling constraints lead to "overpricing", namely, to market prices exceeding the equilibrium level that would prevail in the absence of such constraints. This prediction is based on the idea that, if investors have heterogeneous beliefs, prohibiting short-selling will lead stock prices to reflect only the valuations of bullish investors and those of bearish investors who currently own the stock. Bearish investors who do not own the stock do not participate in the market, so their valuations do not affect the price. Hence, when a ban is enforced stock prices exceed their full-information values, and decline as soon as the short-selling prohibition is lifted.

This rather mechanical prediction of Miller's model does not survive in the rational expectations framework of Diamond and Verrecchia (1987), where less informed market participants adjust their valuations to take into account that short-selling constraints keep investors with superior negative information outside the market, 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 et al.

[1] In the U.S. the short-selling ban on financial stocks was imposed on Friday 19 September 2008, and the obligation to disclose short sales on a weekly basis became effective on the subsequent trading day (Monday 22 September) and applied to all stocks (not just financials), for trades exceeding 0.25 percent of the corresponding company's capital.

(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. On this account, a short-selling ban may have the opposite effect compared to the overpricing predicted by Miller. However, Bai et al. (2006) 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, and thereby increases their demand for the stock. This pushes up the demand for the stock and tends to increase its price.

Thus, with risk-averse investors a short-selling ban may either decrease or increase stock prices, the net effect being more likely to be a fall in stock prices 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 and possibly leading to a crash.

Since the predictions of the theory regarding the effect of short sales on stock prices are ambiguous, the verdict is essentially entrusted to empirical studies. Jones and Lamont (2002), who use detailed data about shorting costs in the New York Stock Exchange (NYSE) from 1926 to 1933, find evidence consistent with the overpricing hypothesis. Chang, Cheng, and Yu (2007) address a similar research question using data from the Hong Kong stock market, where only stocks on a list of designated securities can be sold short: the addition of individual stocks to the list tends to cause stock overvaluation, with more dramatic effects for individual stocks with wider dispersion of investor opinions. In contrast to these findings, however, research on the suspension or complete removal of short-sale price-tests such as the uptick rule in the U.S. finds no significant stock price effects (e.g., Diether, Lee and Werner, 2009; Boehmer, Jones, and Zhang, 2008).

Recent studies that investigate U.S. stock price performance in conjunction with 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. financial stocks could have been affected by concomitant announcements of bank bail-out interventions. 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 bail-out announcements, by estimating a factor model of stock price changes that includes among the factors the return of 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 did not disappear after the lifting of the ban.

Clearly, reliance on data from U.S. markets – where the inception of 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, since in several other countries short-selling bans were not accompanied by bank bailout announcements, or at least such announcements were not concomitant with the bans. Moreover, in many countries bans also applied to non-financial stocks, which were not affected by bank bailout announcements, and in other countries financial stocks were simply not banned. Therefore, also on this issue our paper provides a new contribution by relying on cross-country as well as time-series variation in the inception and lifting of bans, differentially across stock classes. As we shall see, owing to this larger data set and sharper identification strategy, we find that the overpricing effect apparently present in U.S. data are absent in the rest of the world.

3. Methodology and Data

As mentioned in the previous section, we test for the effects of short-selling bans (i) on liquidity, as measured by the quoted percentage bid-ask spread and the Amihud illiquidity ratio; (ii) on 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) on the overpricing of stocks, as measured by the excess returns on stocks subject to bans relative to those on exempt stocks.

The hallmark of our approach is to exploit the panel structure of our data to identify the effect of short-selling bans. More specifically, we exploit the following features of the data:

(i) different introduction dates for different countries (e.g., Italy and Spain intervened later than the U.S.);

(ii) different removal dates for different countries (e.g., the U.S. and Canada were the first countries to remove the ban);

(iii) the presence of countries that imposed no bans (e.g., some Scandinavian countries);

(iv) differences in the scope of ban regimes, which in some countries applied only to financials (e.g., in the U.S. and most European countries) and in others to all stocks (e.g., Japan, Italy, South Korea, and Spain);

(v) differences in the stringency of bans across countries: in some cases, the bans were "naked", i.e. only ruled out sales where the seller does not borrow (or arrange to borrow) the stock in time to deliver it to the buyer within the standard settlement period (naked short sales); in other cases, the bans were "covered", i.e. also ruled out sales where the seller manages to borrow the stock (covered short sales).[2]

Figures 1 and 2 convey the extent of the variation in short-selling ban regimes around the world between September 2008 and June 2009, as well as the various dimensions of such variation. Figure 1 shows the period in which bans were enacted in all the countries in our sample via color-coded lines: naked bans applying to financial stocks are displayed as dark blue lines, while those applying to non-financial stocks appear a light blue lines; covered bans applying to financial stocks are shown as red lines, while those applying to non-financial stocks are in orange. The figure visually conveys the variety of regimes and of their duration across countries, as well as the complex regime variation that in certain cases occurs within the same country over time (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 overall fraction of banned stocks jumped from 0 to about 20 percent in September 2008, rose again to over 30 percent in October, and then gradually

[2] See Gruenewald, Wagner and Weber (2009) for a description of the different types of short-selling restrictions and for a discussion of their possible rationale.

decreased back to 20 percent in the subsequent 8 months. Interestingly, in September and October 2008 covered bans were more widespread than naked ones, while their relative importance tended to reverse later on. As of June 2009 about 20 percent of the stocks in our sample were still subject to naked bans, whereas covered bans had almost disappeared.

In our regression analysis, we measure short sales restrictions by two dichotomic variables, corresponding 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 1 when *only* naked short sales are forbidden (covered ones being allowed), while the Covered Ban variable equals 1 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 dichotomic variable (Disclosure), which equals 1 when short sellers are required to disclose their trades and 0 otherwise.

All our regressions include fixed stock-level effects to control for unobserved heterogeneity due to 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. Since models of the bid-ask spread based on adverse selection and inventory holding risk suggest that risk is a potentially important determinant of bid-ask spread, in some specifications we also control for the changing stock-level volatility of returns. To take into account the commonality in liquidity, especially important at a time when increased uncertainty and acute funding problems were widespread throughout the world, in some specifications we also include day fixed effects and crisis-related control variables.

Our data consist of daily stock bid and ask prices, volumes, short-selling bans 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^{st}, 2008 to June 23^{rd}, 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. The dates and characteristics of short-selling regimes are obtained from the web sites of national regulatory bodies and of the Committee of European Securities Regulators (CESR).

Table 1 describes the structure of our data set. As a fraction of the total observations, 12.4 percent refer to stocks affected by a short-selling ban. As of 1st October 2008 (when most bans were operative), 31.5 percent of the stocks in the sample were affected by a ban on short sales (whether naked or covered). However, the fraction varies considerably from country to country, from zero in, say, Austria and Denmark to 100 percent in Australia and Japan.

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 we can still compute the Amihud illiquidity ratio. We winsorize the data by eliminating the observations corresponding to the top 1 percent of bid-ask spread (thereby eliminating values exceeding 54.9 percent), 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.

4. 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.

4.1. 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 on March 16 of Bear Stearns to JPMorgan Chase in March 2008, the failure of IndyMac Bank on July 11, the failure of Lehman Brothers 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 1, 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 U.K. and the U.S., while in the other countries it has been retained until the end of our sample (June 2009). The figure indicates that, while 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 start increasing in early September, when no country had imposed a ban on short sales yet.

Since the scope and inception dates of short-selling bans vary internationally, Figures 4 to 7 present evidence on the time pattern of bid-ask spreads for different groups of countries. In all the figures, the bans' dates refer to any ban on short sales, whether naked or covered. Figure 4 refers to the countries that banned short sales for *all* stocks, i.e. Australia, Italy, Japan, South Korea and Spain, aligning their different inception dates to a common time that appears as date 0 in the graph.[3] Their bid-ask spreads are scaled by their respective values 100 days before the ban, so as to have them start from a common base. The figure clearly shows that the average bid-ask spread of these five countries is higher in the period in which short-sales are banned.

All the other countries in our sample have forbidden short sales only for financial stocks, though at different dates. For these countries the average bid-ask spread of financial stocks can be benchmarked against that of other stocks, thereby controlling for the aggregate time pattern in bid-ask spreads. By looking at how the ratio between the two averages changes across the two regimes (without and with short-selling ban), we can perform a visual "difference-in-ratios approach". Figure 5 plots this ratio for all the countries whose short-selling ban for financial stocks was still in force as of 23 June 2009, mostly continental European countries. We realign their inception dates to a single date, which appears as date 0 in Figure 5, and standardize to 1 the initial ratio between the two average spreads. The ratio trends upward from 1 to 1.4 before date 0, indicating that the liquidity of these stocks

[3] Not all five countries applied the ban to all stocks for its entire duration, as shown in Table 1. In Italy the ban applied only to financials for the first 18 days, and went from naked to covered. In Australia it was left in place only for financials after November 18, about 60 trading days after the initial ban date.

deteriorated even before short sales were banned for these stocks. But it rises further after date 0, and remains above 1.4, reaching a maximum where the bid-ask spread for financials is almost twice that for non-financials. So, concomitantly with the ban, there is a considerable further deterioration of liquidity.

Figures 6 and 7 replicate this approach for two couples of countries that were the first to lift their short-selling bans on financials, and did so at the same time: the U.S. and Canada, which removed the ban on 8 October 2008; Switzerland and the U.K., which did so on 16 January 2009. In the case of Canada and the U.S., the ban period features higher bid-ask spreads than both previous and subsequent weeks. For Switzerland and the U.K., instead, the evidence is less clear, since in the ban period the bid-ask spread for financials exceeds its pre-ban level, but not its post-ban level.

The visual evidence discussed so far is confirmed by the statistics shown in Table 2. The first three columns of the table document that the ban period is associated with a significantly larger median bid-ask spread for the stocks affected by a short-selling ban at some point in the sample period. The difference is statistically different from zero at the 1 percent confidence level for all the countries: the figures in parentheses below the median values for the ban period are the Wilcoxon test statistics for the difference between the median in the ban period and the median in the pre-ban and (where available) the post-ban period. The fourth and fifth columns show that the median bid-ask spread during the ban period is on average 2.27 times as large as its pre-ban value, and over 3 times as large for Canada, Ireland, Italy, the U.K. and the U.S. In the four countries that first lifted the ban (Canada, Switzerland, U.K. and U.S.) the bid-ask spread during the ban period was on average 1.5 times as large as its post-ban value.

However, 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 figures in the sixth, seventh and eight columns of Table 2: median bid-ask spreads have been significantly higher also for stocks unaffected by short-selling bans, in all the countries of our sample. But the corresponding figures for the stocks affected by the ban are even higher, as documented by the penultimate column of the table. For instance, the median bid-ask spread for U.S. stocks affected by the ban increased by 243 percent, whereas for exempt stocks it only increased by 54 percent. 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.

4.2 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. The top panel of Table 3 presents the estimates of panel regressions with stock-level fixed effects, where the dependent variable is the percentage quoted bid-ask spread and short sales restrictions are measured by the three dummy variables described in Section 3: Naked Ban, Covered Ban, and Disclosure,.[4]

The estimates in columns 1 and 2 show that the ban on naked short sales is associated with an increase ranging between 0.77 and 1.28 percentage points in the bid-ask spread, and the more stringent ban on covered short sales with an increase between 1.64 and 1.98 percentage points. These are large effects compared with the 3.93 percent average bid-ask spread in our sample, and all three are very precisely estimated, being significant at the 1 percent significant level. The obligation to disclose short sales is instead seen to lower the bid-ask spread by 0.65 percentage points. This suggests that this form of disclosure may reduce adverse selection problems in the market, because short sellers – feeling under the scrutiny of market authorities and other market participants – trade less aggressively on their negative information. The specifications of columns 1 and 2 are estimated with OLS, stock-level fixed effects, and robust standard errors clustered at the stock level.

In column 3 we re-estimate the regression on the subset of financial stocks only. We can still identify the effects of the short-selling bans, because the ban on financial stocks was

[4] The effect of short-selling bans on 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 precisely 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 percent of the observations cluster at an absolute bid-ask spread of 1/10 of 1 cent) and Honk Kong (where no short-selling ban was imposed). If we remove Australia from the sample, all our results remain qualitatively unaffected.

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 of the first two columns are not simply a reflection of a liquidity differential between financial and non-financial stocks, considering that the ban applied mainly to financial stocks during the crisis. But the estimates in column 3 show that, even within the subset of financial stocks, short-selling bans have affected the bid-ask spread, and this effect is of the same order of magnitude as for other stocks.

Since the bid-ask spread is typically autocorrelated, in column 4 we re-estimate the specification of column 2 with an AR(1) correction for the error term. The estimates of the coefficients of the three variables of interest are smaller in absolute value but remain sizeable and significantly different from zero at the 1 percent confidence level.

In the last two columns of Panel A of Table 3 we investigate whether the results are robust to the introduction of volatility (measured as the rolling standard deviation of returns based on the previous 20 observations) among the explanatory variables.[5] When this expanded specification is estimated by OLS in column 5, the coefficient estimates of the short-selling ban variables are almost identical to those shown in column 2, while the coefficient of volatility is estimated to be negative, in contrast with theoretical predictions. But this surprising result disappears in column 6, where the same specification is re-estimated with an AR(1) error correction: the coefficient of volatility turns positive, while those of the three ban variables remain positive, and their magnitudes are virtually unchanged relative to the corresponding estimates in column 4. Again, all coefficients are significantly different from zero at the 1 percent confidence level.

The lower panel of Table 3 repeats the estimation replacing the quoted bid-ask spread with the Amihud illiquidity measure, defined as the absolute value of the stock daily return divided by its trading volume on the same day. Using this measure of illiquidity as dependent variable, the coefficients of the Naked and Covered Ban variables are still positive, while that of Disclosure is still negative, and all three are significantly different from zero at the 1 percent confidence level (columns 1 and 2). Again, the results are almost identical if the

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

estimation is restricted to financial stocks only (column 3). With the Amihud measure of illiquidity, the estimated coefficients become larger in absolute value when the regression is estimated with the AR(1) correction (column 4). Finally, as in the upper panel, the results are robust to the introduction of volatility among the explanatory variables (columns 5 and 6). In this case, however, the coefficient of volatility is positive whether it is estimated with OLS or with the AR(1) correction.

The sample used in Table 3 includes countries where short-sales restrictions apply to all the stocks, and therefore there is no benchmark set of domestic stocks unaffected by the ban to filter out completely the aggregate country-level behavior of bid-ask spreads. To overcome this concern and perform a sharper "diff-in-diff" estimation, in Table 4 we restrict the estimation to the subset of 14 countries that applied short-selling bans on stocks in the financial sector only, so that in each country non-financial stocks perform the role of controls. Comparing the estimates in columns 1 and 2 of Table 4 with their counterparts in columns 2 and 5 in Table 3 shows that in this sample a short-selling ban is associated with a considerably larger increase of the bid-ask spread, and disclosure with a much larger decrease. The same conclusion holds if we use AR(1)-corrected residuals. In other words, the better identification strategy allowed by selective bans leads to stronger estimated effects than in the larger sample.

A possible concern is that return volatility may not fully control for the effect of the crisis on stock market liquidity. In this subsample where bans apply only to some stocks in each country, we can better control for market-wide developments related to the financial crisis.[6] In the specification of column 3 we do so by adding day dummies to the list of the explanatory variables. To ease the burdensome computational task of computing firm fixed effects and day effects all at once, we first de-mean all the variables at the firm-level and then perform a panel regression with day fixed effects. The resulting estimates of the short-selling variables' coefficients become considerably smaller (from 1.8 to 0.2 for the Naked Ban, from 1.9 to 0.5 for the Covered Ban, and from −1.0 to −0.5 for the Disclosure dummy), but their signs and statistical significance remain the same as in columns 1 and 2. The value of the

[6] In contrast, in the subsample of countries where short-selling bans applied to all stocks, the ban dummies tend to be highly correlated with aggregate time-varying variables related to the crisis: indeed, they are perfectly collinear with calendar dummies.

constant is close to zero in this specification, because the panel regression is effectively estimated on demeaned variables.

As a further robustness test, in column 4 we expand the list of control variables to include the TED spread, i.e. the spread between the U.S. dollar LIBOR rate (known as Eurodollar rate) and the U.S. Treasury Bill rate, and the VIX, i.e. the CBOE implied volatility index for S&P 500 options. The TED spread is intended to capture the acute funding liquidity problems that during the crisis are thought to have interacted with stock market illiquidity (Brunnermeier and Pedersen, 2009), while the VIX should control for the increase in aggregate risk during the crisis. Comparing the estimates in column 4 with those in column 3, we see that adding these crisis-related controls does not meaningfully alter the magnitude and significance of any of the short-selling restriction coefficients.

4.3 Regression Analysis: Differential Liquidity Effects

The previous section documents that the short-selling bans imposed during the financial crisis hampered stock market liquidity, while short-sales disclosure requirements had the opposite effect. It is then natural to ask whether these effects were homogeneous across stocks or affected disproportionately stocks with some specific characteristics. To answer this question, in this section we investigate whether short-selling restrictions have differentially affected (i) small-cap stocks and riskier stocks (Section 4.3.1); (ii) stocks listed in specific countries (Section 4.3.2); (iii) stocks with listed options (Section 4.3.3); and (iv) domestically or foreign listed stocks, when a cross-listing is present (Section 4.3.3).

Apart from being of independent interest for policy makers, investors and issuers, investigating the differential liquidity effects of short-selling bans 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. Insofar as 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 non-optionable stocks than for optionable ones would confirm that the liquidity effects documented in the previous section actually arise from short-selling restrictions.

4.3.1 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 therefore must carry larger inventories to perform their role. In such circumstances, if faced with the choice of which stocks to stop (or reduce) trading, market makers should be more likely to withdraw from smaller and riskier ones.

The estimates in Table 5 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 of the ban dummy is about 30 percent larger for smaller stocks, the difference being significantly different from zero at the 1 percent confidence level. A qualitatively similar result (not shown in the table) obtains if the regression is estimated separately for the stocks above and below the median capitalization in each country.

In the specification of column 3, the ban dummy variable is entered both in level and multiplicatively with the corresponding company's percentile in its country's distribution of company capitalization during the first half of 2008. The coefficient of the interaction term implies that the ban had almost no effect on the stocks in the top percentile of the distribution (where the two coefficients almost offset each other), while for those in the bottom percentile its effect is 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 six months of 2008. Columns 4 and 5 of Table 5 show that the coefficient of the ban dummy is about 40 percent larger for stocks in the top volatility quartile that in those in the bottom quartile. The specification in column 6, where volatility is entered interactively with the ban, confirms that the effect of the ban on short sales is significantly and positively correlated with risk.

4.3.2 Country of Listing

It is also worth exploring whether the effect of the short-selling bans on liquidity is present in all the countries in our sample, and whether it differs appreciably among them. This is done in panel A of Table 6, where we relax the constraint that the coefficients of the explanatory variables be the same across countries, using the same specification as in column 3 of Table 3. This is equivalent to estimating the regression separately for each country, while retaining stock-level fixed effects.[7] The results indicate that even when unconstrained, the slope coefficients of the short-selling restrictions are estimated to be positive and significant for almost all countries,[8] and their average value is similar to that estimated in the corresponding constrained regression in column 1 of Table 3.

The individual country coefficient estimates are displayed in Figure 8, separately for the Naked and the Covered Ban variables. Italy emerges as the country where the ban on short sales was associated with the most dramatic deterioration of market liquidity, followed by Denmark, Australia and Norway. The U.S., U.K. and Ireland are in an intermediate group, while in the remaining countries short-selling bans have been associated with comparatively mild increases in bid-ask spreads – in the order of about 50 basis points or less.

These large cross-country differences in the impact of short-selling bans partly reflect the different characteristics of national stock markets: in Panel B of Table 6, we explore whether the estimates of the ban coefficients in the country-by-country regressions of Panel A correlate with the median stock size (as measured by market capitalization), median return volatility and ownership concentration of the respective stock markets. The inclusion of size and volatility is warranted by the results of Table 5, 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 less floating shares, and therefore lower liquidity; hence we expect the effect of short-selling bans to be more dramatic in such countries. The OLS estimates in Panel B of Table 6 are consistent with these priors, even though they are not

[7] The Disclosure variable was not included in the regressions because it is perfectly collinear with the Naked or Covered Ban variable, 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.

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

very precisely estimated, probably due to the paucity of the number of observations: the ban coefficients tend to be 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.[9]

4.3.3 Optionable Stocks

During the short-selling ban period, investors could still effectively take short positions by trading in the option markets, because ban regulations around the world did not impose any direct restriction in derivative markets. Battalio and Schultz (2010) document that the ratio of option-to-stock volume for U.S. markets is comparable for banned and control stocks throughout the pre-ban and ban period. While 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.

For all countries in our sample, we obtain a record of all stocks with traded options using information obtained from all the national option exchanges. For most countries, we are able to cross-check the list of stocks with the availability of equity option prices in Datastream. We use this information to segregate stocks into those that have traded options and those that do not, and then see if the bans' liquidity effects differ in the two cases. As stated in the introduction to this section, we expect the effects of short-selling restrictions on bid-ask spreads documented in Tables 3 and 4 to be stronger for stocks without a listed option than for those with it.

Table 7 presents the results. As expected, we find a strikingly stronger effect of short-selling bans on liquidity for stocks without listed options. For countries imposing 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 imposing a

---

[9] The 18 observations used in the regression for all the countries with a ban include the estimated coefficients of the covered ban dummy for Italy, Netherlands and Switzerland, where also the estimate for the naked ban dummy would be available. This choice is dictated by the fact that in Italy and the Netherlands the covered ban lasted much longer than the naked one (which in the Netherlands lasted only two weeks), and in Switzerland financials have always been subjected to a covered ban (and non-financials to a naked ban).

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 one percent level.

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 it provides further evidence that the reduction in liquidity that we document is indeed related to the ban enactment.

4.3.4 Cross-Listed Stocks

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

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

Table 8 shows that a domestic ban worsens stock market liquidity not only in the home but also on the foreign market; in contrast, a ban on the foreign market worsens liquidity only locally. 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). So when a ban is imposed at home, its effects spill over abroad, while the opposite is not true.

5. Price Discovery

As highlighted in Section 2, while 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 the trading activity of informed traders with negative information about fundamentals, a short-selling ban should slow down price discovery, and more so in bear market phases.

To test this prediction on our data, we estimate a market model regression, regressing weekly returns for each stock in our sample 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 et al., 2007) that find this horizon to be an optimal balance between noise and information. The analysis is carried out on residuals, on the assumption that the ban should slow down 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 9 shows the median autocorrelation of residuals for two sub-samples: (i) stocks exempt from bans and non-exempt stocks in periods when no ban was imposed (Ban = 0) and (ii) non-exempt 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, 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 to which short-selling bans apply. Since the distribution of the autocorrelation statistic is not normal, we test for the difference between the two samples using two non-parametric tests for the equality of medians: the *K* test and the two-sample Wilcoxon rank-sum (Mann-Whitney) test (not shown in the table). According to both, the difference is statistically significant at the 1 percent confidence level. This finding is consistent with a lower speed of price discovery during the ban period.

Since 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 asymmetric effect is present in the data, we perform a test proposed by Bris et al. (2007): we compute cross-autocorrelations between individual stock returns and market returns lagged by one week, separately for negative lagged market returns and for positive ones. More precisely, 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) 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 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. More specifically, in column 4 we show the median difference between downside and upside cross-autocorrelation in each of the two subsamples, and in the bottom cell we report the median difference of the differences. This evidence indicates again that not only short-selling bans slow down price discovery, but do so especially in the presence of falling market indices, consistently with theoretical predictions.

6. Stock Prices

The main reason why regulators impose short-selling bans is that they hope them to help stem financial panics, at least insofar as these are reflected in stock market prices. The bans imposed during the 2007-09 financial crisis were no exception in this respect. In this section, we examine whether in this sense the bans were effective, namely whether they provided support for stock prices, when benchmarked against exempt stocks.

The most immediate evidence is obtained by focusing on the countries where the ban did not apply universally, and comparing post-ban 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 equally-weighted market indices. This "visual diff-in-diff" evidence is presented in Figures 9 and 10, separately for the U.S. and for other countries that imposed bans only on financial stocks. The reason why we plot excess returns separately for the U.S. and for other countries is that they appear to have behaved quite differently during short-selling ban periods. Figure 9 shows that the median cumulative excess return of U.S. financial stocks, which were subjected to a covered ban, exceeded that of exempt stocks throughout the 14 trading days after the ban inception (date 0 in the figure), a finding that agrees with that reported by Boehmer et al. (2009) for the U.S. market. But Figure 10 shows that this is not the case for the other countries in our sample: 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 it only in about half of the first 60 days of trading after the inception of the ban. Moreover, the positive effect found for the U.S. may result from concomitant announcements of public policy measures in support of financial institutions, rather than from the ban itself. Since this confounding factor is not present in all the other countries that imposed a short-selling ban on financials, Figure 10 is likely to convey a more accurate picture of the effects of short-selling bans on stock returns than Figure 9.

To go beyond the visual scrutiny of these figures, and exploit the entire sample (including data for countries that imposed a ban on all stocks and for those that imposed no ban) to address this issue, in Table 10 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 refer to all countries, and show the OLS and AR(1) estimates respectively. The estimates in column 3 and 4 refer to the U.S. alone (without and with time dummies), and those in column 5 and 6 to other countries that imposed short-selling bans only on financial stocks (again, without and with time dummies). As in the figures, 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 due to non-trading.

The estimates in the first two columns of Table 10 show that neither naked bans short-selling nor disclosure requirements were associated with significantly better return performances, and covered bans were accompanied by statistically significant return underperformance relative to the local stock market index: multiplying the coefficient of the covered ban variable by the average duration of covered bans, we estimate that on average covered bans were associated with a 3.25 percentage points reduction in excess returns. In contrast, the evidence for the U.S. alone presented in columns 3 and 4 confirms the visual evidence drawn from the figures: the U.S. stock market response to short-selling bans was the exception rather than the rule around the world. This conclusion is also consistent with the estimates in column 5 and 6 of the table: for other countries where bans were imposed only on financials (like the U.S.), both coefficients of the ban variables are negative, though imprecisely estimated.[10]

In conclusion, besides damaging market liquidity, bans on short sales appear to have failed to support market prices, thereby missing the prime objective of regulators. In fact, the non-U.S. evidence appears to be rather consistent with the idea that banning covered short sales contributed to the decline in stock prices during the crisis, consistently with the predictions of the models by Bai et al. (2006) and Hong and Stein (2003).

7. Conclusions

The evidence in this paper suggests that the knee-jerk 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 down price discovery, and was at best neutral in its effects on stock prices.

The ban-induced decrease of 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 due to the freeze of many fixed-income markets. Our findings on international data complement and confirm the results reported for the U.S. by the concurrent study carried out by Boehmer et al. (2009), and show that in other countries the ban's effect were worse than in the U.S.: the implied liquidity reduction was larger, and in contrast with the U.S. the effect on stock returns not significantly different from zero – in fact, the ban of covered short-sales is associated with lower returns if the entire sample is used in the estimation.

Perhaps the main social payoff of this worldwide policy experiment has been that of generating a large amount of evidence about the effects of short-selling bans. The conclusion that this paper distils from this evidence is best summarized by the words of the former SEC

[10] The estimates in columns 3 to 6 of Table 10 are virtually unchanged when we use AR(1)-corrected residuals.

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.

References

Bai, Yang, Eric C. Chang and Jiang Wang Boehmer (2006), "Asset Prices under Short Sales," unpublished manuscript.

Battalio, Robert, and Paul Schultz (2010), "Regulatory Uncertainty and Market Liquidity: The 2008 Short Sale Ban's Impact on Equity Option Markets," unpublished manuscript, January.

Boehmer, Ekkehart, Charles M. Jones, and Xiaoyan Zhang (2008), "Unshackling short sellers: The repeal of the Uptick Rule," Columbia Business School, unpublished manuscript, December.

Boehmer, Ekkehart, Charles M. Jones and Xiaoyan Zhang (2009), "Shackling Short Sellers: The 2008 Shorting Ban," Columbia Business School, unpublished manuscript, September.

Boehmer, Ekkehart, and Julie Wu (2008), "Short Selling and the Informational Efficiency of Prices," Texas A&M University, unpublished manuscript.

Bris, Arturo, William N. Goetzmann, and Ning Zhu (2007), "Efficiency and the Bear: Short Sales and Markets around the World," *Journal of Finance* **62**(3), 1029-1079.

Brunnermeier, Markus K. (2008), "Deciphering the 2007-08 Liquidity and Credit Crunch," *Journal of Economic Perspectives*, forthcoming.

Brunnermeier, Markus K. and Lasse Heje Pedersen (2009), "Market Liquidity and Funding Liquidity," *Review of Financial Studies* **22**(6), 2201-2238.

Chang, Eric C., Joseph W. Cheng, and Yinghui Yu (2007), "Short-sales constraints and price discovery: evidence from the Hong Kong market," *Journal of Finance* **62**(5), 2097-2121.

Charoenrook, Anchada, and Hazem Daouk (2005), "Market-Wide Short-Selling Restrictions," unpublished manuscript.

Diamond, Douglas W., and Robert E. Verrecchia (1987), "Constraints on Short Selling and Asset Price Adjustment to Private Information," *Journal of Financial Economics*, **18**(2), 277–312.

Diether, Karl B., Kuan-Hui Lee, and Ingrid M. Werner (2009), "It's SHO time! Short-sale price tests and market quality," *Journal of Finance*, forthcoming.

Easley, David, Soeren Hvidkjaer, and Maureen O'Hara (2002), "Is Information Risk a Determinant of Asset Returns?" *Journal of Finance* **57**(2), 2185-2221.

Glosten, Lawrence R. and Lawrence Harris (1988), "Estimating the Components of the Bid/Ask Spread," *Journal of Financial Economics* **21**(1), 123-142.

Glosten, Lawrence R. and Paul Milgrom (1985), "Bid, Ask, and Transaction Prices in a Specialist Market with Heterogeneously Informed Traders," *Journal of Financial Economics* **14**(1), 71-100.

Gruenewald, Seraina, Alexander F. Wagner and Rolf H. Weber (2009), “Short Selling Regulation after the Financial Crisis – First Principles Revisited,” Swiss Finance Institute Research Paper Series N°09 – 28, June.

Halling, Michael, Marco Pagano, Otto Randl, and Josef Zechner (2008), “Where is the Market? Evidence from Cross-Listings in the U.S.,” *Review of Financial Studies* **21**(2), 724-761.

Harris, Lawrence E., Ethan Namvar and Blake Phillips (2009), “Price Inflation and Wealth Transfer during the 2008 SEC Short-Sale Ban,” June, unpublished manuscript.

Hasbrouck, Joel (1991), “Measuring the Information Content of Stock Trades,” *Journal of Finance* **46**(1), 179-207.

Hong, Harrison and Jeremy C. Stein (2003) “Differences of Opinion, Short-Sales Constraints, and Market Crashes,” *Review of Financial Studies* **16**(2), 487-525.

Jones, Charles M., and Owen A. Lamont (2002), “Short Sale Constraints and Stock Returns,” *Journal of Financial Economics* **66**(2-3), 207–239.

Kolasinksi, Adam C., Adam V. Reed and Jacob R. Thornock (2009), “Prohibitions versus Constraints: The 2008 Short Sales Regulations,” October, unpublished manuscript.

La Porta, Rafael, Florencio Lopez de Silanes, Andrei Shleifer and Robert Vishny (1998), “Law and Finance,” *Journal of Political Economy* **106**(6), 1113-1155.

Miller, Edward M. (1977), “Risk, Uncertainty and Divergence of Opinion,” *Journal of Finance* **32**(4), 1151-1168.

Reed, Adam (2007), “Costly Short-Selling and Stock Price Adjustment to Earnings Announcements,” University of North Carolina, unpublished manuscript.

Saffi, Pedro, and Kari Sigurdsson (2007), “Price Efficiency and Short Selling,” London Business School, unpublished manuscript.

Table 1. Structure of the Data Set

Country	Ban start date	Partial ban lift date	Ban lift date	Scope of ban	Duration** (days)	Day/stock observations	Day/stock observations with ban	Fraction of day/stock obs. with ban	Number of stocks on 1 Oct. 2008	Number of stocks with ban on 1 Oct. 2008	Fraction of stocks with ban on 1 Oct. 2008
Australia	22 Sep 08	18 Nov 08	25 May 09	all stocks	245	357.003	58594	16.4%	956	956	100.0%
Austria				financials	240	31.094	660	2.1%	89		0.0%
Belgium	26 Oct 08			financials	274	47.479	1084	2.3%	120	6	5.0%
Canada	19 Sep 08		8 Oct 08	financials	19	385.912	154	0.0%	1.136	11	1.0%
Czech Rep.				no ban		9.113		0.0%	25		0.0%
Denmark	13 Oct 08			financials	253	60.408	7099	11.8%	171	-	0.0%
Finland				no ban		52.343		0.0%	145		0.0%
France	22 Sep 08			financials	274	269.636	3454	1.3%	719	19	2.6%
Germany	20 Sep 08			financials	276	318.318	2124	0.7%	845	12	1.4%
Greece	10 Oct 08		1 Jun 09	all stocks	234	102.822	41217	40.1%	273		0.0%
Hong Kong				no ban		403.900		0.0%	1.058		0.0%
Hungary				no ban		11.283		0.0%	31		0.0%
Ireland	19 Sep 08			financials	277	17.343	736	4.2%	50	4	8.0%
Israel				no ban		55.858		0.0%	170		0.0%
Italy	22 Sep 08*	1 Jan 09	1 Jun 09	financials, then all	252	138.240	63704	46.1%	360	53	14.7%
Japan	30 Oct 08			all stocks	236	776.840	362625	46.7%	2.294	2.294	100.0%
Luxembourg	19 Sep 08			financials	277	11.588	2231	19.3%	41	18	43.9%
Netherlands	22 Sep 08		1 Jun 09	financials	252	32.546	1242	3.8%	117	8	6.8%
New Zealand				no ban		30.382		0.0%	102		0.0%
Norway	8 Oct 08			financials	257	73.303	1945	2.7%	227		0.0%
Poland				no ban		24.485		0.0%	79		0.0%
Portugal	22 Sep 08			financials	274	17.277	1311	7.6%	53	9	17.0%
Singapore				no ban		144.116		0.0%	426		0.0%
Slovenia				no ban		7.044		0.0%	21		0.0%
South Korea	1 Oct 08	1 Jun 09		all stocks	265	208.199	98592	47.4%	616	616	100.0%
Spain	24 Sep 08			all stocks	272	64.112	30137	47.0%	173	173	100.0%
Sweden				no ban		98.102		0.0%	309		0.0%
Switzerland	19 Sep 08		16 Jan 09	financials	119	128.907	56181	43.6%	381	381	100.0%
U.K.	19 Sep 08		16 Jan 09	financials	119	575.811	2188	0.4%	1.826	33	1.8%
U.S.	19 Sep 08		8 Oct 08	financials	19	1.539.215	10015	0.7%	4.253	776	18.2%
Totals						**5,992,679**	**745,293**	**12.4%**	**17.066**	**5.369**	**31.5%**

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

Table 2. Median Bid-Ask Spreads Before, During and After Short-Selling Bans

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	During	after	During/before	during/after	before	during	after	during/before	during/after
Australia[1,2]	3.3333	5.2632*** (55.0)	4.7244	1.58	1.11					
Italy[1,3]	0.5721	2.7682*** (168.1)		4.84						
Japan[1]	0.6006	0.6976*** (50.9)		1.16						
South Korea[1,4]	0.4494	0.5249*** (27.4)		1.17						
Spain[1]	0.5840	0.9611*** (36.6)		1.65						
Switzerland[5]	1.1599	1.5267*** (27.1)		1.32						
Austria	0.2949	0.4807*** (8.2)		1.63		1.4500	1.4815*** (3.5)		1.02	
Belgium	0.2791	0.5239*** (5.5)		1.88		1.0929	1.7391*** (33.9)		1.59	
Canada	0.1877	0.6243*** (4.9)	0.3667	3.33	1.70	1.6901	3.3426*** (23.1)	3.9216	1.98	0.85
Denmark	1.9169	3.7736*** (32.8)		1.97		1.7493	2.3904*** (26.2)		1.37	
France	0.2946	0.6024*** (13.6)		2.04		1.4907	2.1108*** (46.7)		1.42	
Germany	0.2870	0.6764*** (23.8)		2.36		3.0457	4.1885*** (63.1)		1.38	
Ireland	0.4186	1.4047*** (21.1)		3.36		3.4782	5.9572*** (21.3)		1.71	
Netherlands[6]	0.2216	0.5144*** (13.0)	0.3302	2.32	1.56	0.8734	1.0292*** (7.3)	1.1959	1.18	0.86
Norway	2.1352	3.6433*** (9.2)		1.71		2.1201	3.3149*** (35.9)		1.56	
Portugal	0.4525	0.9479*** (11.3)		2.09		0.8608	1.3245*** (6.3)		1.54	
U.K.	0.1429	0.4619*** (16.4)	0.2930	3.23	1.58	4.6205	8.0101*** (60.3)	8.0000	1.73	1.00
U.S.	0.4904	1.6814*** (36.3)	0.9050	3.43	1.86	0.2793	0.4310*** (32.6)	0.4158	1.54	1.04
Average[7]	0.7081	1.4248	1.1166	2.27	1.50	1.8411	2.8468	2.9934	1.49	0.99

LEGEND:

The figures in parentheses are Wilcoxon tests for differences between the median during the ban and the median before and (if available) after the ban. *** denotes significance at the 1 percent confidence level.

NOTES:

[1] For Australia, Italy, Japan, South Korea, Spain and Switzerland, we cannot compute the bid-ask spread for the stocks not subject to the ban. In the case of Japan, Spain and Switzerland, this is because the ban on short sales applied to all stocks, so that the control group of stocks exempted from the ban does not exist. In the case of Italy and South Korea, the period in which a short-selling ban did not apply to non-financial stocks is extremely short. In the case of Australia, a short-selling ban on all stocks was followed by a period in which the ban applied only to financials, which prevents the identification of a clean control group of exempt stocks.

[2] The Australian ban was applied to all-stocks, but it was lifted at different dates for non-financials (19 November 2008) and for financials (25 May 2009). As a result, post-ban data refer to non-financial stocks between 19 November 2008 and 24 May 2009 and to all stocks after 24 May 2009.

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

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

[5] Switzerland issued a naked ban for non-financial 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 post-ban period, because only the covered ban on financial was lifted on 16 January 2009. The median bid-ask spread for financial stocks rose from 0.0853 in the pre-ban 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 percent confidence level.

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

[7] Simple average of the median values shown in the previous rows.

Table 3. Market Liquidity and Short-Selling Bans: Regression Analysis

In Panel A the dependent variable is the percentage quoted bid-ask spread at the market close for 25 countries (all the countries in Table 1, except for the Czech Republic, Greece, Hungary, Israel and Luxembourg); in Panel B it is the percentage Amihud illiquidity measure for all 30 countries. Naked Ban is a dummy variable that equals 1 if naked short sales are forbidden and covered sales are allowed and 0 otherwise. Covered Ban is a dummy variable that equals 1 if even covered short sales are forbidden and 0 otherwise. Disclosure is a dummy variable that equals 1 if the seller has to disclose his position and 0 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, 3 and 5, and AR(1) correction in columns 4 and 6. All regressions include fixed effects at the stock level. The numbers reported in parenthesis below the coefficient estimates are *t*-statistics. The estimates marked with three (two) asterisks are significantly different from zero at the 1 (5) percent confidence level.

Panel A. Dependent Variable: Bid-Ask Spread

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	3.93***	3.93***	3.76***	4.97***	3.96***	4.90***
	(1991.03)	(1993.65)	(749.94)	(3290.72)	(1922.84)	(3092.86)
Naked Ban	0.77***	1.28***	0.86***	0.89***	1.26***	0.90***
	(135.04)	(76.04)	(6.50)	(29.31)	(74.99)	(29.60)
Covered Ban	1.64***	1.98***	2.14***	1.63***	1.96***	1.63***
	(109.10)	(150.74)	(14.84)	(57.44)	(149.10)	(57.61)
Disclosure		-0.65***	-0.27**	-0.37***	-0.65***	-0.37***
		(-37.84)	(-1.84)	(-11.54)	(-37.70)	(-11.59)
Volatility					-0.44***	0.99***
					(-55.31)	(35.84)
Stock-Level Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
AR(1) Disturbances	No	No	No	Yes	No	Yes
Number of Observations	5,143,173	5,143,173	878,279	5,126,682	5,140,836	5,124,349
Included Stocks	All	All	Financials	All	All	All
Number of Stocks	16,491	16,491	2,718	16,456	16,487	16,452

Panel B. Dependent Variable: Amihud Illiquidity Measure

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.56***	0.56***	0.73***	0.69***	0.55***	0.66***
	(592.42)	(591.24)	(211.73)	(763.55)	(562.70)	(691.97)
Naked Ban	0.11***	0.20***	0.20***	0.39***	0.20***	0.40***
	(24.48)	(21.96)	(4.20)	(32.94)	(22.29)	(33.82)
Covered Ban	0.06***	0.12***	0.12***	0.21***	0.12***	0.21***
	(13.71)	(18.39)	(3.07)	(19.13)	(18.69)	(19.49)
Disclosure		-0.12***	-0.10**	-0.20***	-0.12***	-0.20***
		(-13.63)	(-2.24)	(-16.44)	(-13.52)	(-16.08)
Volatility					0.06***	0.33***
					(14.57)	(43.33)
Stock-Level Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
AR(1) Disturbances	No	No	No	Yes	No	Yes
Number of Observations	4,373,944	4,373,944	707,054	4,357,092	4,373,944	4,357,092
Included Stocks	All	All	Financials	All	All	All
Number of Stocks	16,852	16,852	2,804	16,822	16,852	16,822

Table 4. Bid-ask Spreads and Short-Selling Bans Applying to Financial Stocks Only

The dependent variable is the percentage quoted bid-ask spread at the market close. Naked Ban is a dummy variable that equals 1 if naked short sales are forbidden and covered sales are allowed, and is 0 otherwise. Covered Ban is a dummy variable that equals 1 if even covered short sales are forbidden, and is 0 otherwise. Disclosure is a dummy variable that equals 1 if the seller has to disclose his position and 0 otherwise. Volatility is a moving standard deviation of returns based on the previous 20 observations. TED Spread is the spread between the Eurodollar rate, i.e. the U.S. dollar LIBOR rate, and the Treasury Bill rate. VIX is the model-free implied volatility index for the S&P 500 options. The regression is estimated by OLS with robust standard errors clustered at the stock level, using daily data for the 13 countries that issued a ban applying to financial stocks only. All regressions control for stock-level fixed effects. The specifications in columns 3 and 4 include day fixed effects: for computational reasons 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 parenthesis below the coefficient estimates are *t*-statistics. The coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5) percent confidence level.

	(1)	(2)	(3)	(4)
Constant	4.18***	4.20***	0.0005***	–0.005***
	(1112.91)	(997.52)	(3.71)	(–3.29)
Naked Ban	2.44***	2.43***	0.23***	0.16***
	(20.18)	(20.06)	(3.99)	(2.74)
Covered Ban	2.76***	2.75***	0.46***	0.41**
	(24.90)	(24.75)	(2.39)	(2.13)
Disclosure	–1.79***	–1.79***	–0.50***	–0.45**
	(–15.14)	(–15.10)	(–2.25)	(–1.96)
Volatility		–0.36***		
		(–14.65)		
TED Spread				–0.38***
				(–6.39)
VIX				12.19***
				(18.59)
Day Fixed Effects	No	No	Yes	Yes
Stock-Level Fixed Effects	Yes	Yes	Yes	Yes
Number of observations	3,188,903	3,188,903	3,188,903	3,188,903
Number of stocks	10,253	10,253	10,253	10,253

Table 5. Bid-Ask Spreads and Short-Selling Bans: Differential Effects by Size and Volatility

The dependent variable is the percentage quoted bid-ask spread. Ban is a dummy variable that equals 1 if short sales, either naked or covered, are forbidden, and is 0 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 to the end of June 2008. High (Low) Volatility Stocks are those in the top (bottom) quartile by volatility in the relevant country. The regression is estimated with daily data for 25 countries (all the countries in Table 1, except for the Czech Republic, Greece, Luxembourg, Hungary and Israel). The estimates are all obtained with 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 three asterisks are significantly different from zero at the 1 percent confidence level.

	Large-Cap Stocks	Small-Cap Stocks	Interaction with Market Capitalization	Low Volatility Stocks	High Volatility Stocks	Interaction with Volatility
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	4.19***	6.66***	3.02***	2.59***	5.92***	3.93***
	(580.10)	(700.13)	(820.79)	(314.26)	(747.00)	(1024.33)
Ban	1.16***	1.50***	1.55***	0.94***	1.30***	0.90***
	(18.03)	(18.52)	(21.10)	(16.79)	(13.45)	(18.82)
Interacted Ban			-1.36***			0.72***
			(-12.15)			(3.32)
Stock-Level Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,846,401	1,069,289	4,411,450	1,314,501	1,193,031	5,133,169
Number of stocks	6,538	3,561	13,468	4,144	4,017	16,432

Table 6. Bid-ask Spreads and Short-Selling Bans: Country-by-Country Estimates

In Panel A 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, and is based on daily data for 25 countries (all the countries in Table 1, except for the Czech Republic, Greece, Hungary, Israel and Luxembourg). Panel A summarizes the individual regression estimates. Naked Ban is a dummy variable that equals 1 if naked short sales are forbidden and covered sales are allowed and 0 otherwise. Covered Ban is a dummy variable that equals 1 if even covered short sales are forbidden and 0 otherwise. Panel B reports the estimate of a cross-country regression of the ban dummies' coefficients in the individual country regressions of Panel A on country characteristics. Median Size and Median Volatility are the country-level medians of total market value and stock return volatility in the first 6 months of 2008. Ownership Concentration is the average percentage of common shares owned by the three largest shareholders in the 10 largest non-financial, privately owned domestic firms in a given country (drawn from La Porta et al., 1998).

Panel A. Dependent Variable: Bid-Ask Spread

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

Panel B. Dependent Variable: Ban Coefficients from Individual Country Regressions

	All Countries with Ban	Countries with Covered Ban
Constant	0.99***	1.44***
	(5.11)	(7.05)
Median Size	–0.11	–0.44*
	(–0.51)	(–1.80)
Median Volatility	0.45*	0.49**
	(1.84)	(2.41)
Ownership	0.44*	1.13***
Concentration	(1.80)	(4.36)
R^2	0.26	0.79
Observations	18	10

Table 7. Differential Impact on Bid-Ask Spreads of 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 1, except for the Czech Republic, Greece, Hungary, Israel and Luxembourg). Naked Ban is a dummy variable that equals 1 if naked short sales are forbidden and covered sales are allowed and 0 otherwise. Covered Ban is a dummy variable that equals 1 if even covered short sales are forbidden and 0 otherwise. Disclosure is a dummy variable that equals 1 if the seller has to disclose his position and 0 otherwise. 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 parenthesis below the coefficient estimates are *t*-statistics. The estimates marked with three asterisks are significantly different from zero at the 1 percent confidence level.

	(1) Stocks With Listed Options	(2) Stocks Without Listed Options
Constant	0.60***	4.23***
	(193.48)	(1015.57)
Naked Ban	0.33***	1.40***
	(5.94)	(12.24)
Covered Ban	0.67***	2.14***
	(9.66)	(25.95)
Disclosure	-0.20***	-0.72***
	(-3.42)	(-6.54)
Stock-Level Fixed Effects	Yes	Yes
Number of Observations	427,164	4,716,009
Number of Stocks	1,306	15,185

Table 8. Bid-Ask Spreads and Short-Selling Bans for Dually 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 1 if short sales, either naked or covered, are forbidden and 0 otherwise. The regressions in columns 1 and 2 are estimated with daily data for all dually listed stocks in the U.S. 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 with 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 three (two) asterisks are significantly different from zero at the 1 (5) percent confidence level.

	Domestic Market Liquidity	U.S. Dual Listing Liquidity	Domestic Market Liquidity	U.S. Dual Listing Liquidity
	(1)	(2)	(3)	(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
Monthly Dummies	No	No	Yes	Yes
Number of Stocks	131	133	56	56

Table 9. 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 1 if short sales, either naked or covered, are forbidden, and is 0 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 parenthesis are the *p*-value of the K non-parametric test for the equality of medians.

	Median Autocorrelation of Market Model Residuals	Median Downside Cross-autocorrelation between Stock Returns and Market Returns	Median Upside Cross-autocorrelation between Stock Returns and Market Returns	Median of the Difference between Downside and Upside Cross-autocorrelation
	(1)	(2)	(3)	(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)

Table 10. 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 non-trading biases. Naked Ban is a dummy variable that equals 1 if naked short sales are forbidden and covered sales are allowed, and is 0 otherwise. Covered Ban is a dummy variable that equals 1 if even covered short sales are forbidden, and is 0 otherwise. Disclosure is a dummy variable that equals 1 if the seller has to disclose his position and 0 otherwise. The specification in column 1 and 2 is estimated on the full sample. The specification in columns 3 and 4 is estimated only on data for the U.S. and that in columns 5 and 6 for all other countries with partial bans, except the U.S. The estimates in columns 1, 3, 4, 5 and 6 are obtained by OLS with robust standard errors clustered at the stock level. The regression in column 2 is estimated with a correction for AR(1) disturbances. All regressions include fixed effects at the stock level. The regressions in column 4 and 6 also include weekly time effects. The numbers reported in parenthesis below the coefficient estimates are *t*-statistics. The coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5) percent confidence level.

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	–0.0007***	–0.0007***	0.0002***	0.0583***	–0.0017***	–0.0017***
	(–34.58)	(–8.74)	(21.29)	(29.82)	(–58.50)	(–58.50)
Naked Ban	–0.0036	–0.0044			–0.0043	–0.0026
	(–0.94)	(–0.57)			(–1.08)	(–0.67)
Covered Ban	–0.0014***	–0.0014**	0.0411***	0.0611***	–0.0005	–0.0004
	(–2.54)	(–2.02)	(13.64)	(18.82)	(–0.13)	(–0.12)
Disclosure	0.0012	0.0012			0.0035	0.0066
	(0.90)	(1.11)			(0.63)	(1.17)
Stock-Level Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
AR(1) Disturbances	No	Yes	No	No	No	No
Weekly Fixed Effects	No	No	No	Yes	No	Yes
Countries in the sample	All	All	U.S.	U.S.	Countries with partial ban exc. U.S.	Countries with partial ban exc. U.S.
Number of observations	915,440	899,424	245,631	245,631	299,980	299,980
Number of stocks	16,016	15,814	3,717	3,717	5,369	5,369

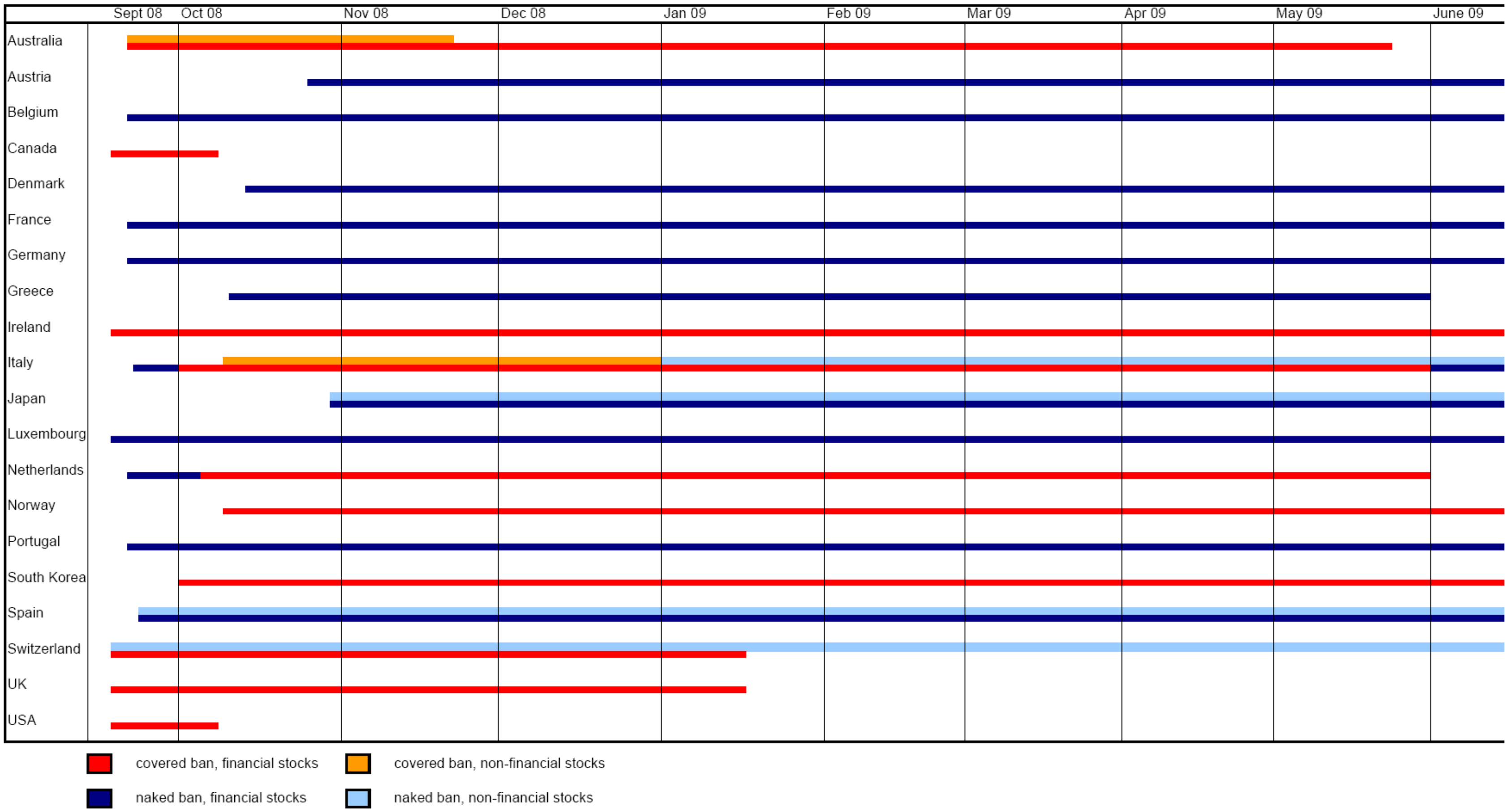

Figure 1: Short-Selling Ban Regimes Around the World, September 2008 – June 2009

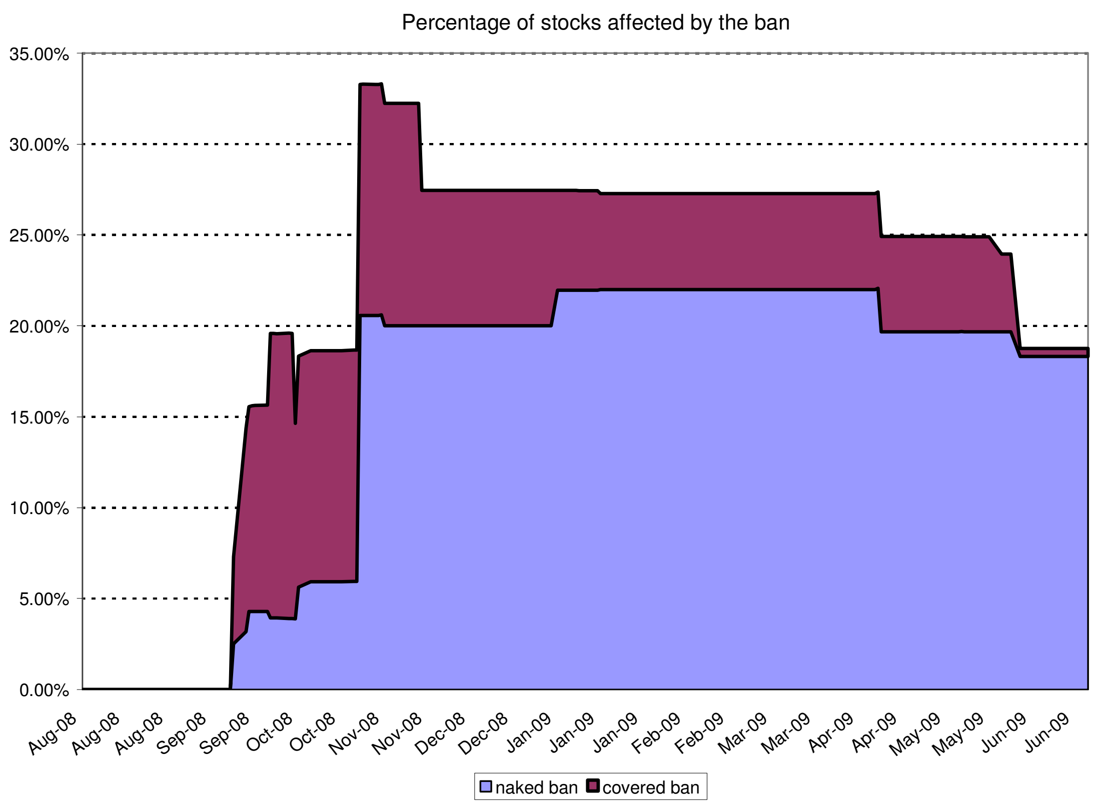

Figure 2. Fraction of Stocks Affected by Short-Selling Bans around the World

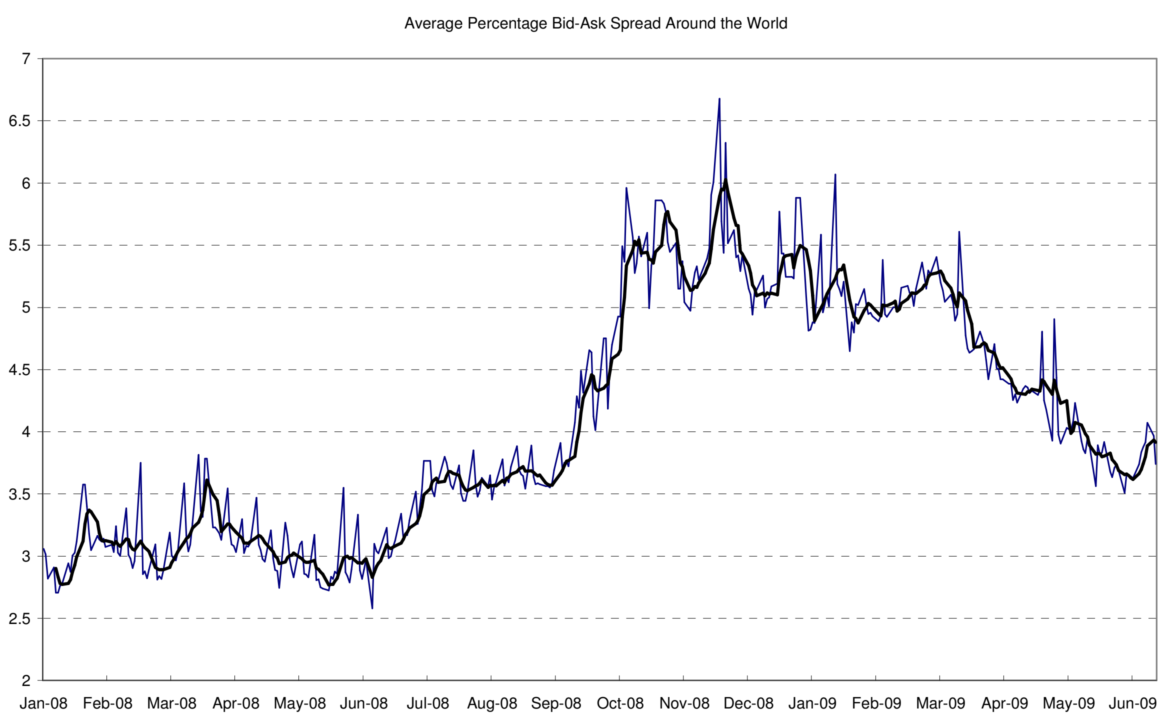

Figure 3. Average Bid-Ask Spread around the World
(thin line: daily values, bold line: 5-day moving average)

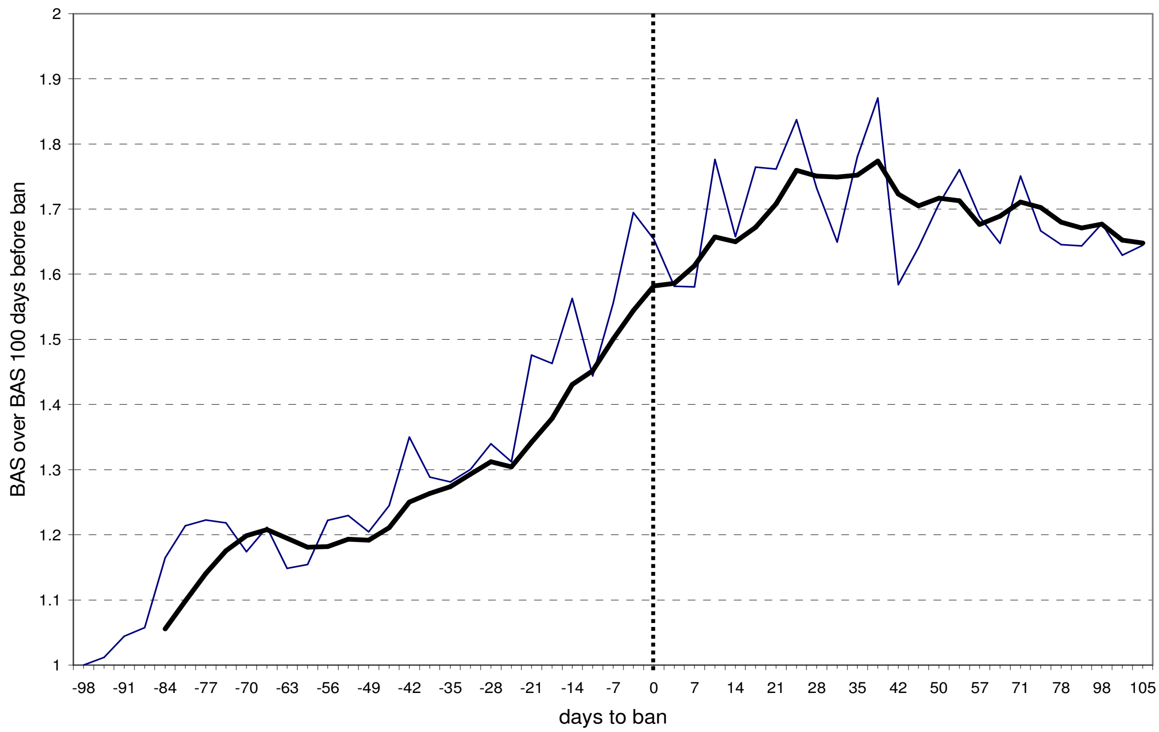

Figure 4. Average Bid-Ask Spread over Bid-Ask Spread 100 Days before Ban in Australia, Italy, Japan, South Korea and Spain
(date 0: start of ban; thin line: daily values, bold line: 5-day moving average)

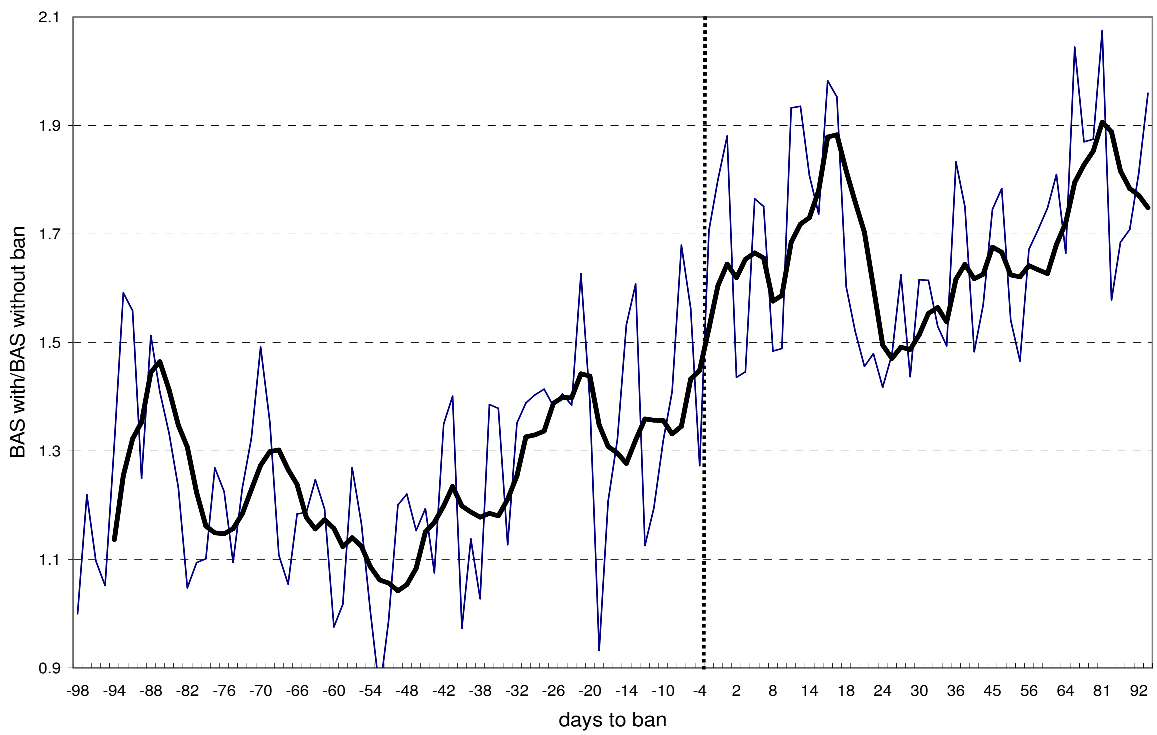

Figure 5. Ratio between Average Bid-Ask Spread for Stocks With and Without Ban in Austria, Belgium, Denmark, France, Germany, Ireland, Netherlands, Norway and Portugal
(date 0: start of ban; thin line: daily values, bold line: 5-day moving average)

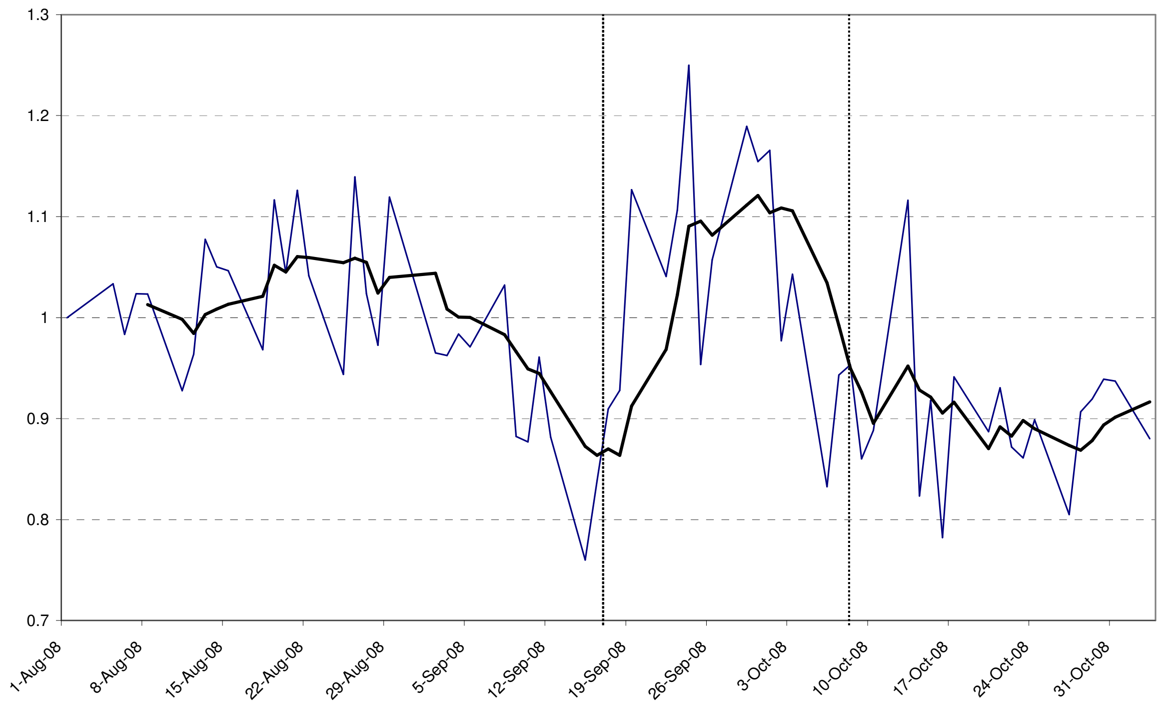

Figure 6. Ratio between Average Bid-Ask Spread for Stocks With and Without Ban in Canada and the U.S.

(vertical lines: start and end of ban; thin line: daily values, bold line: 5-day moving average)

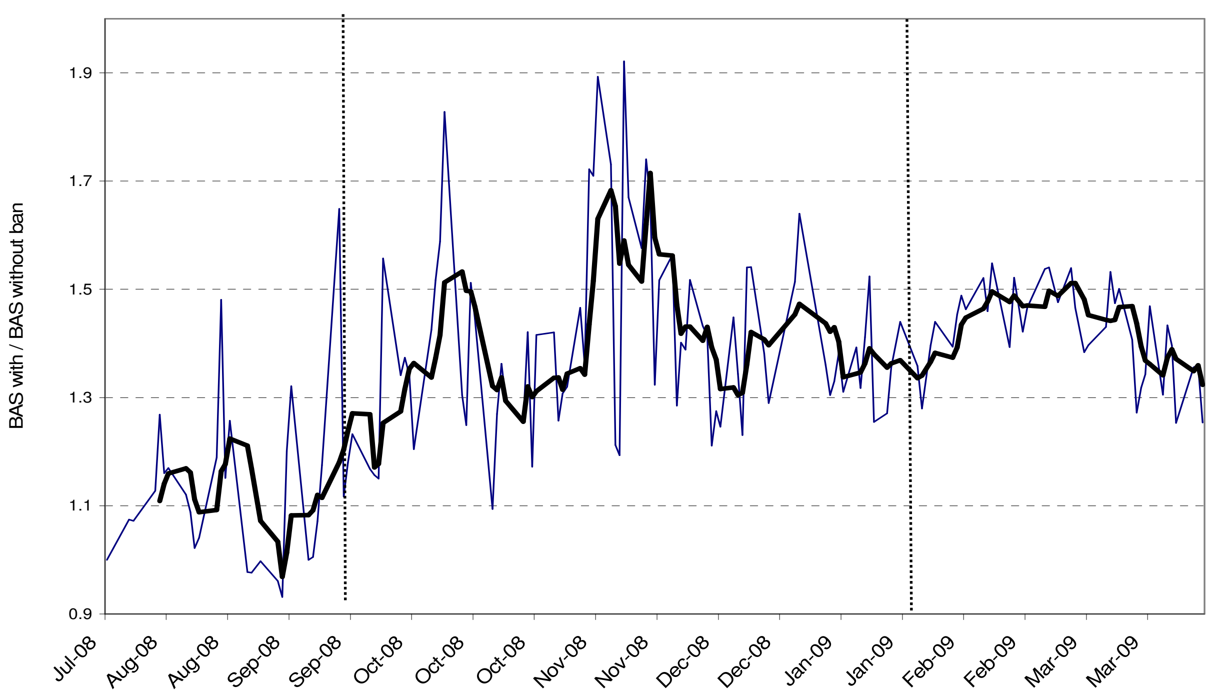

Figure 7. Ratio between Average Bid-Ask Spread for Stocks With and Without Ban in Switzerland and the U.K.

(vertical lines: start and end of ban; thin line: daily values, bold line: 5-day moving average)

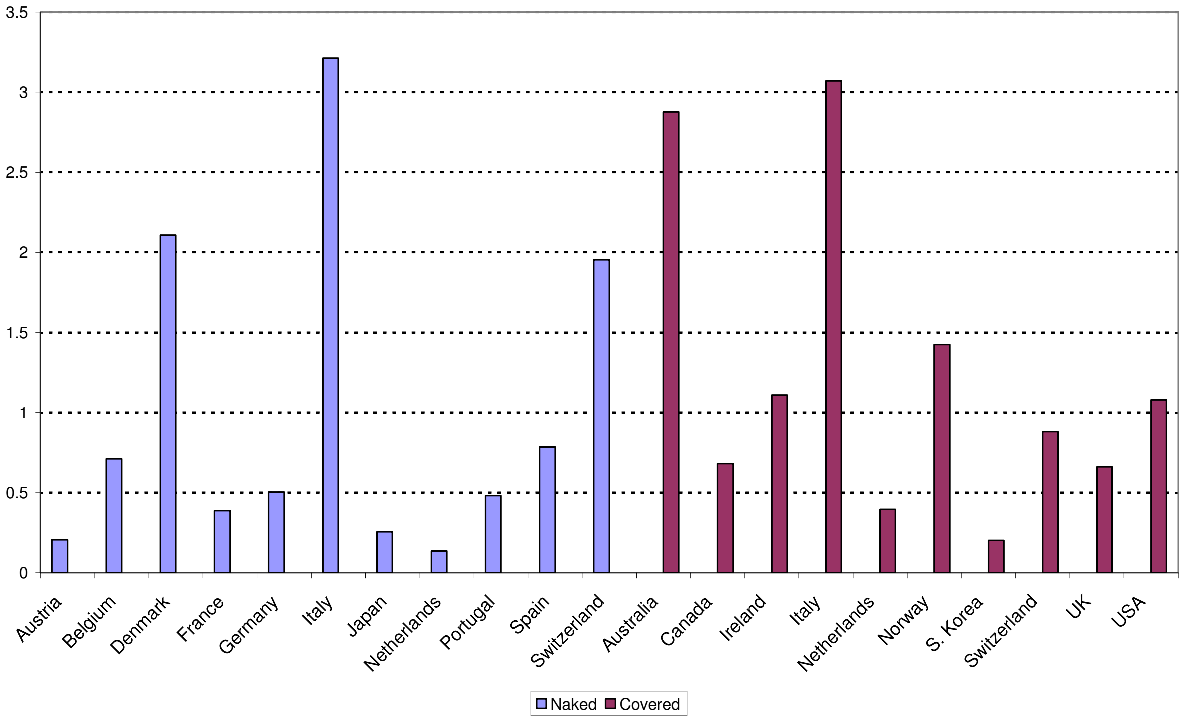

Figure 8. Impact of Short-Selling Ban on the Percent Quoted Bid-Ask Spread, by Country
(bars: estimated coefficient of Naked or Covered Ban in the regressions of Table 6)

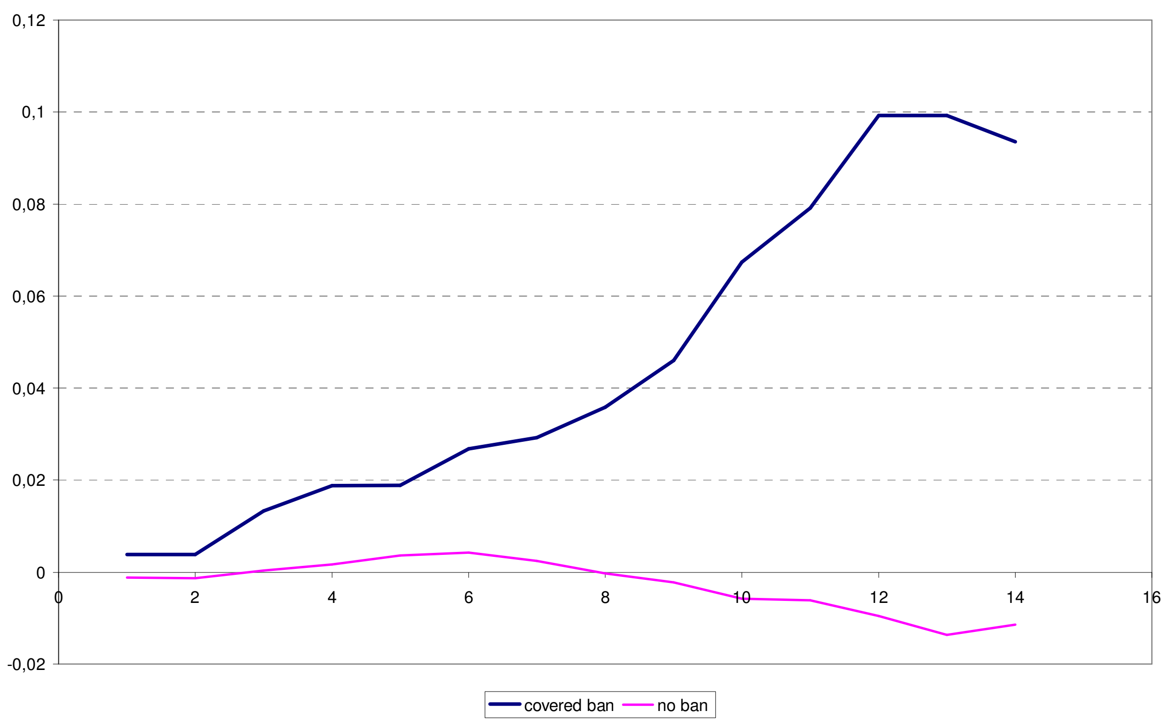

Figure 9. Cumulative Abnormal Returns over 14 Trading Days after Ban Date in the U.S., Stocks Subject to Covered Ban and Exempt Stocks

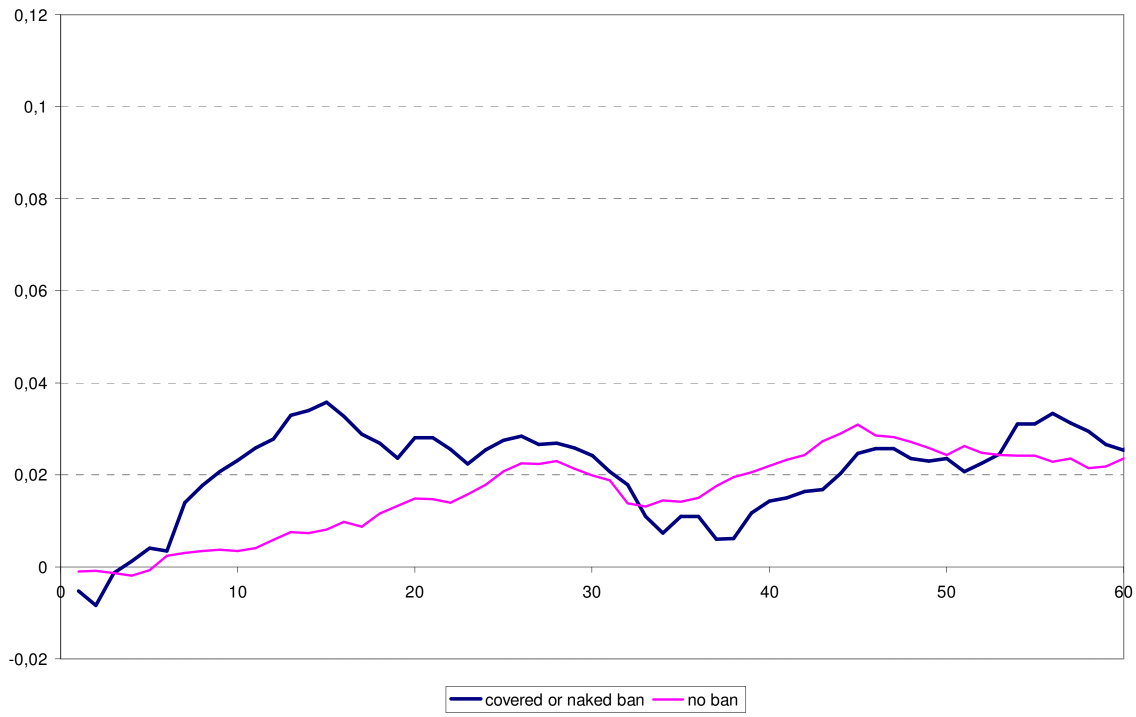

Figure 10. Cumulative Abnormal Returns over 60 Trading Days after Ban Date in Countries with Ban on Financials Only (except U.S.), Stocks Subject to Ban and Exempt Stocks