

SHACKLING SHORT SELLERS: THE 2008 SHORTING BAN

Ekkehart Boehmer

ekkehart@uoregon.edu

Lundquist College of Business, University of Oregon

Charles M. Jones

cj88@columbia.edu

Columbia Business School

Xiaoyan Zhang

xz69@cornell.edu

Johnson Graduate School of Management, Cornell University

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ABSTRACT

In September 2008, the U.S. Securities and Exchange Commission (SEC) surprised the investment community by adopting an emergency order that temporarily banned most short sales in nearly 1,000 financial stocks. In this paper, we study changes in stock prices, the rate of short sales, the aggressiveness of short sellers, and various liquidity measures before, during, and after the shorting ban. We match banned stocks to a control group of non-banned stocks in order to identify these effects. Shorting activity drops by about 65%. Stocks subject to the ban suffered a severe degradation in market quality, as measured by spreads, price impacts, and intraday volatility. Prices of stocks subject to the ban increase sharply, but it is difficult to assign this effect to the ban because the Troubled Asset Relief Program (TARP) and other initiatives were announced the same day. In fact, we find no positive share price effects in stocks that were added to the ban list later, suggesting that the ban may not have provided much of an artificial price boost.

1. Introduction

For the most part, financial economists consider short sellers to be the “good guys,” unearthing overvalued companies and contributing to efficient stock prices. Even as late as the summer of 2007, regulators in the United States seemed to share this view, as they made life easier for short sellers by repealing the NYSE’s uptick rule and other short-sale price tests that had impeded shorting activity since the Great Depression (see Boehmer, Jones, and Zhang (2009) for an analysis of this event). But short sellers are often the scapegoats when share prices fall sharply, and regulators in the United States did a sharp U-turn in 2008, imposing tight new restrictions on short sellers as the financial crisis worsened.

In September 2008, the U.S. Securities and Exchange Commission (SEC) surprised the investment community by adopting an emergency order that temporarily banned most short sales in nearly 1,000 financial stocks. In this paper, we study changes in stock prices, the rate of short sales, the aggressiveness of short sellers, and various liquidity measures before, during, and after the shorting ban. We compare banned stocks to a control group of non-banned stocks in order to identify these effects.

We find that during the shorting ban, shorting activity drops by about 65%. Stocks subject to the ban suffered a severe degradation in market quality, as measured by spreads, price impacts, and intraday volatility. Price effects are a bit harder to assign to the ban, as there is substantial confounding news about TARP and other government programs to assist the financial sector on the day that the ban is announced and implemented. When we look at firms that are added later to the ban list (for these firms, confounding contemporaneous events are less of a problem), we do not find a price bump at all. In fact, these stocks consistently underperform during the whole period the ban is in effect. This suggests that the shorting ban did not provide much of an artificial boost in prices.

The paper is organized as follows. Section 2 reviews related literature, and a detailed timeline of events related to the shorting ban is the subject of section 3. Section 4 discusses the data, including proprietary intraday NYSE and Nasdaq data on short sales, and provides some overall summary statistics. Section 5 discusses the methodology we use, particularly the two-way fixed effect models we use to isolate the effect of the shorting ban. Results start in section 6 with an analysis of changes in shorting activity. Section 7 discusses effects on share prices. Section 8 examines changes in effective spreads, short-term volatility, and other market quality

measures. Market-makers were still able to short during the ban, and in section 9, we characterize their shorting style. Section 10 concludes.

2. Literature review

Theoretical models with differences in beliefs predict that constraints on short sales should cause stock prices to rise and become overvalued. In models such as Miller (1977) and Harrison and Kreps (1978), shorting restrictions mean that pessimists are shut out of the market, and optimists do not take into account the absence of pessimists in setting prices. Prices thus become too high. In Duffie, Garleanu and Pedersen (2002), the shorting restrictions come in the form of search frictions in the share lending market, and these frictions make it impossible for short sellers to return prices all the way to fundamentals. On the other hand, if all agents have rational expectations, as in Diamond and Verrecchia (1987), they do not agree to disagree, and shorting prohibitions do not cause stock prices to be biased on average (though shorting prohibitions slow down the adjustment of prices to negative news).

A few recent papers consider the possibility that so-called “bear raids” or other kinds of manipulative shorting activity could lead to lower prices. Goldstein and Guembel (2008) show that short sellers have an incentive to manipulate if they can somehow cause the company to experience negative real effects. However, Khanna and Mathews (2009) emphasize that those with long positions have strong incentives to counter such activity via their own trading behavior.

Diamond and Verrecchia (1987) point out that short sellers are more likely to be informed, as they would never initiate a short sale for liquidity reasons. The empirical evidence in the literature provides uniform support for this hypothesis. Dechow et al. (2001), Desai, Krishnamurthy, and Venkataraman (2006), Cohen, Diether, and Malloy (2007), and Boehmer, Jones, and Zhang (2008) show that in aggregate short sellers appear to trade based on (and be well-informed about) fundamentals, and they earn excess returns. Aitken, Frino, McCorry, and Swan (1998) show that in Australia, where a short sale is publicly identified as such immediately on execution, short sales have a larger impact on price than regular-way sales.

Generally, the evidence supports the models with differences in beliefs rather than the rational expectations or bear raid alternatives. When short sellers’ information is not

incorporated into prices because shorting is costly, difficult, or prohibited, the evidence indicates that stocks can get overvalued. For example, Lamont and Thaler (2003) and Mitchell, Pulvino, and Stafford (2002) show that during the late 1990's, spinoffs in the tech sector were so overpriced that arbitrage (or something very close to arbitrage) should have been possible, but short positions were very difficult to establish. Pontiff (1996) provides similar evidence for closed-end funds. Jones and Lamont (2002) show that in the 1920's and 1930's, stocks that were expensive to short had abnormally low future returns, even after accounting for shorting costs.

A number of researchers have also studied market structure changes that make it easier or harder to short. For example, Jones (2008) finds significant price effects when shorting is restricted during the Great Depression. Danielsen and Sorescu (2001) show that the introduction of listed options on a given stock eases shorting constraints and reduces share prices slightly. Ho (1996) finds an increase in stock return volatility when short sales were restricted during the Pan Electric crisis in the Singapore market in 1985-1986. Chang, Cheng, and Yu (2007) find price effects in Hong Kong when specific stocks are designated as eligible for shorting. Rhee (2003) finds some evidence of price effects in Japan following imposition of an uptick rule there. In contrast, Diether, Werner, and Lee (2009) find that Regulation SHO's pilot program to suspend short sale price tests does not affect share prices, and Boehmer, Jones, and Zhang (2009) find similar evidence when U.S. short sale price tests are removed completely in 2007.

Shorting restrictions also affect liquidity and the adjustment of prices to new information. Diamond and Verrecchia (1987) predict that if there are shorting constraints, prices will adjust more slowly to negative information. Reed (2007) finds an asymmetric price adjustment in response to information about earnings, and Bris, Goetzmann, and Zhu (2007) find that downward price moves are slower in markets where shorting is prohibited. Using weekly data on share lending supply and borrowing fees from 26 markets, Saffi and Sigurdsson (2007) show that less constrained firms (proxied by high lending supply and a low borrowing fee) are more efficiently priced in that they have shorter price delays. Boehmer and Wu (2008) document that short selling makes prices more informationally efficient and reduces post-earnings announcement drift. Diether, Werner, and Lee (2009) find that the 2005 pilot program to suspend price tests in the U.S. slightly worsens some measures of market quality, and Boehmer, Jones, and Zhang (2009) find that market quality worsens further when the uptick rule is repealed completely.

Finally, we are aware of at least four other papers written contemporaneously with ours analyzing the same events. Harris, Namvar, and Phillips (2009) focus on stock price effects of the U.S. event using a factor approach, while Beber and Pagano (2009) use recent shorting bans in various jurisdictions and an international panel of stocks to assess the effect of shorting bans on liquidity. Battalio and Schultz (2009) focus on migration to the individual equity options markets during the ban, while Kolasinski, Reed, and Thornock (2009) also study naked shorting prohibitions and analyze the effects on the stock lending market.

3. Timeline of events

The temporary ban on the shorting of financial stocks is the most recent in a sequence of regulatory efforts to throw sand in the gears of short sellers and make it more difficult or costly to take a short position in embattled financial stocks. The first move in this direction took place in July 2008, when the SEC issued an emergency order restricting naked shorting (where the short seller fails to borrow shares and deliver them to the buyer on the settlement date) in 19 financial stocks. After the emergency order expired in mid-August, the SEC returned on the evening of Wednesday, September 17 with a permanent ban on naked shorting in all U.S. stocks, effective at 12:01am ET on Thursday, September 18. On Thursday, September 18, the U.K.'s Financial Services Authority instituted a temporary ban on short sales in 32 financial stocks, effective the next day (Friday, September 19). The FSA shorting ban was accompanied by a requirement to disclose short positions in these stocks that were in excess of 0.25% of the shares outstanding. Both measures were to remain in force until January 16, 2009.

That same day (Thursday, September 18, 2008), after the U.S. market closed for the day, the SEC matched the FSA, surprising the market with a temporary ban on all short sales in 797 financial stocks.¹ The SEC's emergency order (release no. 34-58592) was issued pursuant to its authority in Section 12(k)(2) of the Securities Exchange Act of 1934, and it was effective immediately. The initial order covered 10 business days, terminating at 11:59 p.m. ET on Oct. 2, 2008, but could be extended under the law to last for a maximum of 30 calendar days.²

¹ The emergency order claimed to cover 799 stocks, but only 797 were actually listed in the order.

² At the same time, the Commission announced that all institutional short sellers would have to report their daily shorting activity, and the Commission announced aggressive investigations into possible manipulation by short sellers.

The details of the shorting ban are important for understanding the effect of the event. For example, the last time shorting was banned in the United States was in September 1931, when the New York Stock Exchange banned all short sales in the wake of England's announcement that it was abandoning the gold standard. As Jones (2008) recounts, all short sales were banned in that case, including short sales by specialists and other market-makers, which provoked something akin to a short squeeze by buyers who realized that at least in the short-term there would be few that could stand in the way of their efforts to drive prices up.

In 2008, the SEC did not repeat the NYSE's earlier mistake. The emergency order contained a limited exception for market-makers (defined in the emergency order as "registered market makers, block positioners, or other market makers obligated to quote in the over-the-counter market") that were selling short as part of bona fide market making activity. Also, the shorting ban became effective on a so-called "triple witching day," the last day of trading before expiration of index options, equity options on individual stocks, and index futures. Barclay, Hendershott, and Jones (2008) provide some recent evidence on the very large order imbalances and excess volatility present on these days. To prevent large price swings around these expirations, the SEC decided to grant options market makers a 24-hour delay so that they too could sell short as part of their market making and hedging activities.

The ban was implemented quite hastily, and many details evolved over time. On Sunday, September 21, the SEC announced (in release 34-58611) technical amendments to the original ban, all of which were effective immediately. There were three main elements. First, the SEC delegated to the exchanges all decisions about the ban status of a listed firm. Listing markets were to designate the individual financial institutions to be covered, and were authorized to exclude firms from the ban list on their request. Second, options market-makers were to remain exempt from the shorting ban for the duration of the emergency order, and the SEC clarified that all registered market makers were exempt, including over-the-counter market makers and those making markets in exchange traded funds (ETFs). Third, the SEC stated that "a market maker may not effect a short sale...if the market maker knows that the customer's or counterparty's transaction will result in the customer or counterparty establishing or increasing an economic net short position (i.e., through actual positions, derivatives, or otherwise) in the issued share capital of a firm covered by this Order." This language seems designed to discourage the use of listed or OTC derivatives to take a bearish position in the covered stocks, though its main result was

probably to provide market makers with considerable incentives to avoid knowledge of a customer or counterparty's net positions.

On Monday, September 22, the three major exchanges announced a number of additions to the list of banned stocks. For example, the NYSE added 32 stocks to the list on this day and 44 stocks on the following day. Many of these additions were clearly financial stocks that were simply overlooked by the SEC as it drew up its initial list, but industrial firms with a large finance subsidiary (such as General Motors and General Electric) were added to the shorting ban list as well. Additions continued on subsequent days at a slower pace. For example, the NYSE added 13, 9, and 7 stocks on Wednesday, Thursday, and Friday, respectively. Also, four NYSE firms and four Nasdaq firms asked to be removed from the shorting ban list on various days. These removals included REITs as well as a few broker-dealers and asset managers who did not want to look hypocritical, given that at least some of their revenues relied on the continued viability of short sales.³

On October 2, 2008, at the end of the initial 10-day effective period, the SEC extended the ban to the earlier of October 17, 2008 or three business days following enactment of TARP (formally known as H.R. 1424, the Emergency Economic Stabilization Act of 2008). President Bush signed the bill into law on the afternoon of Friday, October 3 immediately after it passed both houses of Congress, and the SEC then announced that the ban would expire at 11:59pm ET on Wednesday, October 8, 2008. As of October 9, shorting was again permitted in all listed stocks as long as market participants complied with the requirement to borrow shares in advance, as mandated by the naked shorting ban, which continued to remain in effect.

4. Data

Most of the analysis covers the period from August 1 through October 31, 2008. We also examine stock returns through the end of 2008. We merge five different datasets. Stock returns are from CRSP, and the NYSE's TAQ database is used to calculate market quality and other intraday measures. The NYX and Nasdaq websites provide dates and details about stocks initially included on, added to, and/or deleted from the shorting ban list. From both the NYSE and Nasdaq, we have data on all executed short sales from August 1, 2008 through October 31,

³ We examine the removed firms separately, but given the small sample size, the results are too noisy to draw any useful statistical inference about the effects of voluntarily withdrawing from the ban list.

2008. The format is the same as the data required to be made public from January 2005 to July 2007 under Regulation SHO. For each transaction executed on either the NYSE or Nasdaq involving one or more short sellers, a record identifies the time of the transaction, the ticker symbol, the trade price, and the share volume that involves a short-seller. Finally, the exchanges collect and release short interest data twice per month for each listed stock, and we examine the evolution of short interest through all of 2008.

To be included in the sample, stocks must be listed on the NYSE or Nasdaq from December 31, 2007 through October 31, 2008, and there must be TAQ data each day from August 1, 2008 through October 31, 2008. Listing in 2007 is required because we create a matched sample based on trading activity during the first half of 2008. Based on the match to CRSP, we initially retain only common stocks (CRSP share codes 10 and 11), which means we exclude securities such as warrants, preferred shares, American Depositary Receipts, closed-end funds, and REITs. After applying these filters, there are 404 stocks in the sample out of the original SEC list of 797 stocks subject to the shorting ban, and an additional 61 stocks in our sample later become subject to the shorting ban, for a total of 465 NYSE and Nasdaq common stocks in the sample that are subject to the shorting ban at some point. Table 1 provides further details on the filters applied.

We also create a matched sample of 465 stocks where shorting was never banned, matched on listing exchange, the presence or absence of listed options, market capitalization at the end of 2007, and dollar trading volume from January through July 2008. The distance metric is the absolute value of the proportional market-cap difference between the non-banned match candidate and the banned stock plus the analogous absolute value of the proportional dollar trading volume difference. For each stock subject to the ban, we choose with replacement the non-banned stock that is listed on the same exchange, has the same options listing status, and has the smallest distance measure.

In robustness tests, we also consider non-common stocks and matches based on industry. Specifically, we take all 3-digit SIC codes where at least one firm appears on the ban list and at least one firm does not. Then we exclude ADRs, closed-end funds (but not REITs), ETFs, and partnerships. For each of the 62 ban list firms in this subset, we then find a matching firm that is listed on the same exchange and minimizes our distance metric based on market cap and volume. This subsample is small, because in most of the financial industries, all stocks were subject to the

ban. Thus, this matching procedure yields a sample that is dominated by firms in non-financial industries with modest financial arms. It also differs from the base sample in that securities other than common stocks are included.

One last subsample is of interest. In July 2008, the SEC announced a temporary emergency ban on naked shorting in 19 large financial stocks. These firms included all of the primary dealers in Treasury securities as well as Fannie Mae and Freddie Mac, so this list includes the largest investment and commercial banks with the most extensive debt securities market operations. Eight institutions on this list survive our filters, including Bank of America, Goldman Sachs, Morgan Stanley, Citigroup, and J.P. Morgan Chase. These firms were probably the ones expected to receive the most government assistances, and we sometimes refer to this group as the “largest TARP firms.” We examine them separately, because it appears the shorting ban was designed in part to assist these large, systemically important firms.

Table 2 provides summary statistics for the sample of firms subject to the shorting ban and the matched sample of firms where shorting was never banned, using data for the period from August 1 to October 31, 2008. For each of these groups we report means (Panel A) and medians (Panel B) for the pre-ban period, the ban period, and the post-ban period. The matching procedure seems to work well, as banned and non-banned stocks have very similar liquidity measures during the pre-ban period. For example, effective spreads average 0.74% for stocks on the original ban list vs. 0.70% for matching control stocks during the pre-ban period.

5. Methodology

We describe the changes around the duration of the shorting ban graphically and in two-way fixed effect panel regressions. Most of the figures compare the 404 sample stocks on the original ban list to the 404 matched control stocks where shorting is never banned. We use this subset of banned stocks in the figures because the event date is the same for all of them, making it easy to visually identify the effects of the ban by comparing banned stocks to otherwise similar non-banned stocks.

The panel regression analyses in Tables 3 through 5 formally incorporate all $465 * 2 = 930$ stocks in the sample, including stocks that were added to the ban list after September 19 as well as matching control stocks. Using this sample and various subsets, we estimate the

following two-way fixed effects model for a variety of left-hand side variables Y_{it} measured for matched pair i on day t :

$$Y_{it} = \alpha_i + \gamma_t + \beta D_{it}^{BAN} + \theta X_{it} + \varepsilon_{it} \quad (1)$$

where Y_{it} is the measured quantity Y for the banned stock less the measured quantity for its non-banned match. On the right-hand side, a matched pair fixed effect and calendar day dummies are present, and D^{BAN} is an indicator variable set equal to one if and only if the shorting ban is in effect for the banned stock in matched pair i on day t . Also included is X_{it} , a vector of pairwise differences for the following control variables: market cap, dollar trading volume, the proportional daily range of transaction prices, and the daily volume-weighted average share price (VWAP).

The matched pair fixed effect means that we take out any differences between two stocks in a pair that are present during the non-ban period. The calendar effects take out any broad market moves in the quantity of interest. The control variables are designed to pick up time-variation in the matching variables as well as any effects due to volatility or share price level, though it turns out that none of those effects are important – all of our inference is unchanged when we exclude these control variables. Thus, our overall strategy is to identify the effect of the ban on a particular quantity Y by comparing banned stocks to matching non-banned stocks during the ban vs. at other times. Said another way, this panel is a differences-in-differences methodology that can accommodate the staggered introduction and removal of the shorting ban across stocks.

Statistical inference is conducted using Thompson (2009) standard errors. This technique allows for both time-series and cross-sectional correlation of the regression errors, as well as heteroskedasticity. In general, we find that these robust standard errors are very similar to OLS standard errors, suggesting that the two-way fixed effects are removing most of the correlation that is present across observations.⁴

⁴ Thompson (2009) variance-covariance matrices are not guaranteed to be positive definite, and estimated standard errors can turn out negative in finite samples if the true error terms are close to being independent across observations. In about 1% of all cases, we obtain negative standard error estimates for coefficients of interest. When this happens, we report and use White (1980) standard errors for inference.

6. Effects on shorting activity and trading activity

Table 2 and Figure 1 provide details on shorting activity for the different groups of stocks before, during, and after the ban. Note first that banned and non-banned stocks have very similar amounts of shorting activity prior to the ban, suggesting that our matching procedure produces a set of very comparable stocks. For the 404 sample stocks on the original shorting ban list, short sales account for a cross-sectional average of 21.78% of trading volume during the pre-ban period from 1 Aug through 18 Sep (RELSS). For the matched set of control stocks that are never subject to the shorting ban, the corresponding number is 19.48%. Not surprisingly, the shorting ban had a dramatic effect on short selling activity, but shorting does not decline to zero. During the shorting ban (19 Sep through 8 Oct), short sales are 7.71% of overall trading volume for stocks on the original ban list, compared to 18.23% of overall trading volume over the same time interval for the matching set of non-banned stocks. Recall that market-makers (including but not limited to specialists and options market-makers) are able to short as part of their market-making and hedging activities, and these are probably the short sales that we observe during the ban period.

These remaining short sales could reflect trades by market-makers acting as a middleman for market participants who are now forced to take an economic short position using derivatives. For instance, a hedge fund could buy puts on financial stocks instead of shorting them directly. An options market-maker might sell this put to the hedge fund and then delta hedge its risk by shorting the appropriate amount of the underlying stock. As another example, a hedge fund could short a financial stock ETF (ETFs were not subject to the shorting ban). A market-maker might purchase the ETF shares and hedge its risk by shorting the stocks underlying the ETF. However, it is not possible to directly assign the whole 7.71% shorting figure to bearish traders that are attempting to circumvent the ban on short sales, because market-makers short for other reasons. For instance, if an entity wants to take a long position in a financial stock, a market-maker may sell short in order to provide liquidity to that buyer. Nevertheless, the low shorting numbers suggest there was not massive substitution by hedge funds and other short sellers into derivatives that were then hedged by market-makers.

It is interesting to examine the exact timing of the decline in shorting activity. On Thursday, September 18, the naked shorting ban goes into effect, and on this day there is statistically less shorting activity across the board. Compared to an average of 19.5% for the

whole pre-ban sample period, shorting accounts for only 15.6% of volume in the control stocks that Thursday, and 16.6% for the financial stocks that will appear on the SEC's original list on Thursday evening. (Recall that at this point, market participants do not know that a shorting ban is on its way.) These are the smallest shorting numbers in the whole pre-ban period, and they suggest that the pre-borrow requirement imposed by the naked shorting ban significantly impeded shorting activity. In fact, Table 2 Panel A shows that shorting in non-banned control stocks remains at a lower average level during and after the shorting ban (18.2% of volume during the ban, 18.7% of volume during the post-ban sample period), further suggesting that the naked shorting ban had at least some effect on shorting activity. The large amount of shorting activity in non-banned stocks on September 19 is somewhat inconsistent with this story (shorting is 33.5% of trading volume in non-banned stocks on that day), but there are at least two possible explanations. It could be that market participants anticipated an expansion of the shorting ban and rushed to get short positions in place. Non-banned stocks might have served as substitutes. September 19 was also a witching day, and the imminent expirations of September options and futures could account for that day's burst of shorting activity in the non-banned stocks.

Once the ban is lifted on October 9, shorting in the banned stocks increases sharply. For a few days, though, there is still a gap between the two groups that gradually narrows. The gap seems to be gradually closing over those days, so it could be that it takes some time for market participants to adjust their trading strategies. It could also be that the naked shorting ban, which continues even when shorting is again permitted, has a differential effect, limiting shorting in financial stocks more than it limits shorting in other stocks. Alternatively, the increasing scrutiny on short positions in financial stocks may discourage some traders. Whatever the explanation, within a week the two groups again exhibit similar shorting activity. In fact, over the whole post-ban sample period from 9 Oct through 31 Oct, Table 2 Panel A shows that shorting is on average 18.7% of volume in non-banned stocks vs. 18.2% of volume for our sample stocks on the original ban list, and this difference is not statistically significant ($t = 1.64$).

Short interest in banned stocks (measuring the total short positions outstanding on a given date) also declines sharply during the shorting ban and stays at lower levels thereafter. Figure 2 shows the cross-sectional median level of short interest for our sample stocks during 2008, measured as a fraction of shares outstanding. Short interest is collected twice per month, based on positions of record around the 15th and the end of each month, so only the September 30

observation is inside the ban period. Median short interest in banned stocks during the latter half of September falls by 0.6 percentage points to 5.8% of shares outstanding. This decline is strongly significant, both in raw terms and relative to short interest in matching control stocks, as it is the biggest half-month change in short interest observed during 2008. After the ban is lifted, short interest continues to fall modestly through the rest of 2008 for both groups of stocks.

The effects of the ban on shorting activity are so strong that there is probably little need for additional formal tests, but in Table 3, we use panel regressions on all three shorting activity measures to show that the ban reduced shorting activity. Based on the full sample results reported in Panel A, the shorting ban reduces the average stock's daily number of trades involving a short seller by 1,967 ($t = 7.03$). The average banned stock sees a decline of 513,087 shares sold short per day ($t = 5.51$), and the fraction of trading volume involving a short seller declines by 11.8 percentage points ($t = 7.12$). Panel B reports results for the subsample that also includes an industry match, and the results are quite similar.

Panel C of Table 3 partitions the sample by market-cap quartile, and it is clear that the impact of the shorting ban differs cross-sectionally. The shorting ban has the biggest and most reliable effect on large-cap stocks. Perhaps the easiest measure to interpret is RELSS, the fraction of trading volume involving a short seller. In the smallest quartile, RELSS declines by 6.2 percentage points, and this decline is not statistically significant. By contrast, the reliability of the effect increases monotonically with size, and in the two largest-cap quartiles, shorting as a fraction of volume falls by a strongly significant 14.8 and 16.0 percentage points. For the sample of 8 systemically important firms, shorting also declines sharply, with RELSS falling by 10.7 percentage points. One possible explanation for this cross-sectional variation is that hedge funds and other high-frequency short sellers may disproportionately trade large-cap stocks because of their greater liquidity, and these market participants may have been the hardest hit by the shorting ban.

Table 3 also shows that under the ban, short sellers are not replaced by an equal number of long sellers. Compared to the stocks not subject to the ban, daily trading volume in the banned stocks falls by an average of \$34.8 million ($t = -2.56$) during the ban period. The average number of reported trades per day falls by 1,257 relative to the non-banned stocks ($t = 4.01$). In results not reported, we also find that average trade sizes during the ban increase significantly on the banned financial stocks. We are not sure why this is so, but anecdotal evidence indicates that

the NYSE floor became more important during this volatile period, and even on the all-electronic Nasdaq market there may have been fewer computer-driven algorithmic trades during this period.

7. Effects on stock prices

If the disagreement models are right and the shorting ban prevents at least some pessimists from taking a bearish position in a financial stock, the announcement of the shorting ban should cause prices of affected stocks to rise, leading to overvaluation relative to the fundamentals. However, it is not clear whether we should expect a very large stock price effect. First, a market participant could use ETFs, puts, credit-default swaps, or other derivative instruments to take a bearish position. Second, the shorting ban is temporary. When the shorting ban is announced, the market knows that the SEC is initially limited to a 30-day ban absent some sort of legal change in the regulator's rulemaking abilities. If there were common knowledge that a shorting ban would last at most 30 days, one might expect only a modest stock price reaction. However, this does not describe the situation at the start of the shorting ban. The market was very surprised by the SEC's move and might have expected additional actions after expiration of the emergency order. For example, the SEC could attempt to make the shorting ban longer-lived through formal rulemaking or impede short sales in other ways in the future. Ultimately, the magnitude of the stock price effect is an empirical question, and we let the data speak.

The other challenge is the presence of confounding events during the shorting ban. On the same day that the shorting ban takes effect, the US Treasury announces a temporary guarantee program for money market funds, and the Fed announces a program to lend against high-quality asset-backed commercial paper. Most importantly, Treasury secretary Henry Paulson announces the creation of what came to be known as the Troubled Asset Relief Program (TARP). While there are no details in Paulson's initial statement and press conference, it is not surprising that financial firm equity holders would respond positively to the announcement of a program "to remove these illiquid assets that are weighing down our financial institutions and threatening our economy."

Figure 3 shows cumulative raw returns for the 404 sample stocks that are on the SEC's original shorting ban list, as well as cumulative returns for the set of 404 matched control stocks where shorting is never banned. Interestingly, the banned and control stocks have statistically indistinguishable mean returns during the 1 Aug to 17 Sep pre-event period, suggesting that the treated stocks and the control stocks are quite similar. The next day (18 Sep), the naked shorting ban is announced and implemented, only one day before the ban on shorting financial stocks. The naked shorting ban affects the prices of both groups of stocks: the non-banned control stocks rise by an average of 4.37%, while the financial stocks in our sample that will appear on the next day's shorting ban list rise by an average of 12.38% (!) on this day. This 8.01% return difference could be due to the differential effect of the naked shorting ban on financial stocks. But it is difficult to assign the stock price effect to the prohibition on naked shorting, because there was other news on this day that could have a differential effect on financial stocks. For instance, Christopher Cox announced on Wednesday evening that the SEC would require disclosure of large short positions, and pension funds in New York and California announced on Thursday that they would temporarily stop lending financial stocks to short sellers. The Federal Reserve also added \$300 billion of liquidity to financial markets on Thursday, and rumors about what would become TARP started circulating widely on Thursday afternoon.⁵

On Fri 19 Sep, prices impound the news of the day, including the announcement and immediate implementation of the emergency shorting ban. The banned stocks rise by an additional 8.71% that day, compared to an average 4.02% return for the matched control stocks where shorting is never banned. As the ban continues, both sets of stocks decline sharply, losing about 30% of their value over this period, but the effects are broadly similar for banned and non-banned stocks.

If the disagreement models are right, a temporary ban should have a temporary price effect, and the price rise should reverse when the ban ends. There is some evidence of that price reversal right around the end of the ban. On October 9, when shorting resumes, the 404 banned stocks fall by an average of 12.38%, compared to a price decline of "only" 7.62% for the matched non-banned stocks. By itself, this suggests a price effect of almost 5% associated with the ban, which is virtually identical to the analogous estimate of the price effect from the start of

⁵ Vikas Bajaj and Jonathan D. Glater, "Bid to Curb Profit Gambit as Banks Fall" and Edmund L. Andrews, "Federal Reserve and Treasury Offer Congress a Plan for a Vast Bailout," New York Times, Sep 19, 2008. pg. A.1.

the ban on September 19. But this 5% relative price decline is completely reversed the next day (October 10), making it hard to be sure that the shorting ban is behind these price changes. In fact, Figure 2 tells a cautionary tale, as it indicates that banned stocks outperform the non-banned match stocks by about 30% through the end of 2008. Much of this accrues during the shorting ban, and the permanence of the cumulative return suggests it is due to news about fundamentals, not the shorting ban.

To try to escape the confounding news about TARP and fundamentals, we look at the subset of firms that are added to the ban list at a later date. There are 61 such firms in our sample, and they are matched in the same way to a firm where shorting is never banned. There are a handful of financial firms in this group, but many if not most are non-financial firms with a financing arm. For example, firms in this group include General Electric, General Motors, and IBM. In addition to avoiding the contemporaneous confounding news about TARP, these later additions should have share prices that are on average much less sensitive to TARP news and other government efforts to stabilize the financial sector.

Figure 4 follows cumulative equally-weighted excess returns on the firms added later to the ban list vs. their matched non-banned counterparts. The displayed confidence interval extends 1.96 standard deviations above and below zero, calculated using excess return data for these firms during the pre-event period from August 1 through September 17. The figure begins with the announcement of the ban on September 19. Note that for these 61 firms, average excess returns on this day are very close to zero (0.37%, to be precise). Perhaps market participants do not expect the ban to be expanded; perhaps they do not expect it to increase share prices even if it is expanded. As we continue to follow these firms, they gradually become subject to the ban. The majority are added to the ban list over the next two days, with the rest added over the course of the next two weeks. Interestingly, Figure 3 never shows a price bump for these stocks as a group. They underperform the matched sample during the entire ban interval, and this underperformance is borderline statistically significant by October 8. This suggests that for these stocks, any artificial price boost from the shorting ban is outweighed by associated negative price effects.

What could these negative price effects be? We have thought of two possibilities. One is an illiquidity discount. The evidence in the next section indicates that market quality was severely degraded during the shorting ban, and this could have been reflected in lower share

prices. Another possibility is that the market interpreted the addition to the ban list as a negative signal about that company's prospects. After the initial ban list came out, company management generally had to put in a request to be on the ban list, and market participants may have wondered whether those companies had something to hide.

What should we expect at the end of the shorting ban? For the vast majority of stocks subject to the shorting ban, investors know when the ban is to expire well before it actually happens, and it is not clear when this information should be incorporated into prices.⁶ For example, if agents have different valuations but are otherwise rational, the price should react when the market learns that the shorting ban is to end. In contrast, if optimists and pessimists rely only on their own valuations, as implied by Miller (1977), stock price effects should appear only on the day that shorting resumes. In our event, the market gradually learns about the end date of the shorting ban. On October 2, 2008, the SEC extended the ban to the earlier of October 17, 2008 or three business days following enactment of the bailout package. Prior to the SEC announcement, most participants expected the ban to last until October 17, and by this point it was pretty clear that the bailout package was close to passage, so this announcement of the extension probably shortened participants' expected duration of the shorting restrictions. On Fri 3 Oct, the bailout passed and was signed into law, and the SEC that day announced that shorting in affected stocks would resume on Thu 9 Oct.

Thus, if there are price effects related to the ending of the ban, we might see them on Thu 9 Oct, or we might see them on Thu 2 Oct or Fri 3 Oct. Figures 3 and 4 show the evolution of banned stocks' cumulative abnormal returns (relative to the non-banned match firms) as the resumption of shorting approaches. On Thu 2 Oct and Fri 3 Oct, the abnormal returns on the 404 original-list banned stocks relative to matched control stocks are 2.15% and 0.97%, respectively. For the sample of 61 firms that are added later to the ban list, abnormal returns are relatively small in magnitude on these two days, at -0.88% and 0.51%, respectively. None of these are significant, which suggests that the positive effect of TARP, if any, has been incorporated into prices earlier.

For the two days surrounding the end of the ban (8 Oct and 9 Oct), the abnormal returns on these 404 banned financial stocks are quite negative, averaging -2.35% and -4.77%,

⁶ For four NYSE stocks and four Nasdaq stocks, the end of the ban is announced and implemented on the same day, prior to the end of the ban, in response to a request from the firm's management to be removed from the list of banned stocks.

respectively. Over the next several days, however, the previously banned stocks recoup virtually all of these losses. Results are similar for the 61 firms that are added to the list later. Excess returns are -8.10% on 9 Oct and 4.97% on the following day. Thus, the evidence here remains mixed. Abnormal returns are significantly negative, particularly on the first day that shorting was again permitted, which suggests that the shorting ban was indeed propping up the prices of the affected stocks. But since the price effect is soon reversed, a longer view points to no lasting price effects from lifting the ban. This, in turn, is not consistent with temporary overvaluation as predicted by Miller (1977). Instead, it suggests that the positive price response at the beginning of the ban is related to the anticipated bailout package and other initiatives announced contemporaneously.

8. Effects on market quality

Do short sellers tend to provide or demand liquidity on average? In this section we use the shorting ban to investigate this question. The evidence in Diether, Lee, and Werner (2009) and Boehmer, Jones, and Zhang (2009) indicates that short sales are extremely prevalent in recent years. In late 2007, approximately 40% of trading volume involves a short seller. In contrast, short interest represents only about 4% of shares outstanding. Boehmer, Jones, and Zhang (2008) show that this discrepancy means that short sellers on average maintain their positions for a much shorter period of time. Anecdotal evidence indicates that much of this high-frequency shorting activity is conducted by quant hedge funds. These hedge funds are typically not registered market-makers and so would be subject to the ban. But the evidence in Khandani and Lo (2007) suggests that many of these quant funds do in fact provide liquidity. This suggests that the shorting ban might worsen market liquidity, even with an exception for registered market-makers.

For each common stock each day, we calculate RES, the trade-weighted proportional round-trip effective spread on all trades. The effective spread is defined as twice the (proportional) distance between the trade price and the quote midpoint prevailing at the time of the trade. We use trades at all market venues, and we use the national best bid and offer prices to calculate the quote midpoint prevailing the second prior to the transaction. In a similar fashion we also calculate RQS, the proportional quoted spread based on the national best bid and offer

prices. However, we focus more on effective spreads, because transactions sometimes take place at prices within the quoted bid and ask prices, due to the presence of hidden orders or due to price improvement by intermediaries. Note that spreads are really an illiquidity measure: the wider the effective spread or quoted spread, the less liquid is the stock.

We also calculate the five-minute price impact of a trade. We sign trades as either buyer-initiated or seller-initiated based on the Lee and Ready (1991) algorithm, and the price impact measures the proportional distance the quote midpoint moves in the direction of the trade. For buyer-initiated trades, the price impact measure RPI5 is measured as the proportional difference between the quote midpoint five minutes after the trade and the trade price. For seller-initiated trades, the price impact is the same proportional price change but of opposite sign. Again, price impacts are an illiquidity measure: the bigger the price impact, the more a given trade tends to push the price over the next five minutes.

Table 2 provides some descriptive statistics for the various groups of stocks in various intervals of time. For each group of stocks, we calculate a time-series average over the stated time interval and then calculate a cross-sectional mean (Panel A) or median (Panel B). We focus on effective spreads, but the results for quoted spreads are very similar. As mentioned earlier, original ban list stocks and control stocks have very similar average market quality measures prior to the ban, suggesting that the matching procedure works well. During the 1 Aug to 18 Sep pre-ban period, average effective spreads are 74 basis points for stocks on the initial ban list and 70 basis points for the set of matching stocks. While the shorting ban is in effect, these market quality measures diverge wildly. Median effective spreads do widen from 33 to 52 basis points for the control stocks, but median effective spreads for the stocks on the initial ban list more than triple, from 42 to 145 basis points. This represents a dramatic change in the liquidity of stocks subject to the shorting ban.

The magnitude of the changes is even more apparent in Figure 5. Note that the figure corresponds to Table 2 Panel A, as it uses cross-sectional means. Stocks subject to the shorting ban become far less liquid during the ban period. Once the ban ends, the two groups move much closer together in terms of effective spreads, but a gap persists for at least two more weeks. Only at the very end of October do effective spreads for banned and non-banned stocks again coincide. Interestingly, liquidity remains very poor for both sets of stocks, perhaps because stock market volatility remains extremely high. From Table 2 Panel A, during the post-ban period the mean

effective spread for stocks on the initial shorting ban list is 164 basis points, compared to 146 basis points for the matching set of control stocks.

Price impacts also show similar behavior. During the pre-ban period, mean 5-minute price impacts are 33 basis points for banned stocks vs. 29 basis points for the matching set of control stocks. During the shorting ban, average price impacts increase to 76 basis points for banned stocks vs. 47 basis points for the non-banned stocks. During the post-ban period, these price impacts remain very high, averaging 68 basis points for banned stocks vs. 59 basis points for the matched non-banned stocks.

Panel regressions confirm these results using all $465 * 2 = 930$ sample stocks, including stocks that are added to the shorting ban list after October 19. Recall that the panel regressions employ matched pairs and include firm-specific and calendar dummies as well as other control variables, so broad market effects are eliminated, and the change in market quality is identified by comparing otherwise similar banned and non-banned stocks on a given day. Based on the numbers in Panel A of Table 4, the shorting ban is associated with quoted spreads that are 37 basis points wider ($t = 2.11$), and effective spreads that are 32 basis points wider ($t = 3.01$). Price impacts show an increase as well; the shorting ban is associated with an 11 basis point increase in 5-minute price impacts ($t = 2.01$).

While it is hard to imagine, it is possible that the degraded market quality during the shorting ban is due solely to confounding changes in the information environment, including the tremendous volatility of fundamentals and the rapid pace of news about TARP and other matters. We try to control for this in two ways. First, we limit the analysis to firms that were added to the shorting ban list after 19 Sep. Second, we add an industry match, which limits the analysis to industries where some firms were banned and some were not. This removes nearly all financial firms from the analysis.

Panel B of Table 4 has the results for the subsample that includes an industry match. Compared to matched stocks that are not subject to the shorting ban, effective spreads widen out by 18 basis points ($t = 2.71$). While the point estimate of the effect on market quality is smaller than for the whole sample, it is more precisely estimated, perhaps because this subset excludes most of the very volatile large financial stocks. There are similar results for quoted spreads. Panel C has the results for firms that are added to the ban list later. For that sample, effective spreads widen by an average of 33 basis points ($t = 6.18$), and quoted spreads widen by virtually

identical amounts. In both subsamples, price impacts are reliably higher as well, though the effects are more modest for this measure. Overall, the sharp widening of spreads seems to be clearly caused by the shorting ban.

The shorting ban is also associated with a large increase in price volatility. We measure intraday volatility using the proportional intraday range (RVOL), defined as the difference between the highest and lowest transaction price recorded for a given stock on a given trading day, divided by the stock's volume-weighted average trade price for that day. Figure 6 shows the cross-sectional average of the range variable for each trading day, comparing the original ban list stocks to matched stocks that are never subject to the shorting ban. Here the matching does not seem to have completely eliminated differences in the two groups prior to the ban. For example, average intraday price ranges are 6.09% for stocks on the original SEC list vs. 5.16% for the matched control stocks, based on the numbers in Table 2 Panel A. Stocks on the original SEC list experience a sharp increase in intraday range volatility during the shorting ban (an average of 11.58% for initial ban stocks vs. 8.55% for control stocks). This gap seems to continue, but only for a few days after the ban comes to an end. Interestingly, the banned stocks and the matched non-banned stocks look most similar during the latter part of October, as there is essentially no difference in volatility between the two groups for that part of the post-ban period. The formal statistical tests are contained in Table 4, and based on the full sample results in Panel A the shorting ban is associated with an additional 3.81 percentage points of intraday range ($t = 4.98$). When we remove most financial institutions by limiting the sample to firms added to the ban list after 19 Sep, Panel C shows that range volatility increases by a very similar 3.74 percentage points on average. For the 62 banned firms that can be matched to a non-banned firm in the same industry, Panel B shows a significant increase in average range volatility equal to 2.08 percentage points ($t = 2.35$).

When we sort banned firms into four market cap quartiles, we find some notable cross-sectional differences. The results are in Table 4 Panel C. Market quality for the smallest quartile does not get worse during the shorting ban. This is true for all four of our market quality measures: effective spreads, quoted spreads, price impacts, and range volatility. But for all of the other quartiles, market quality worsens markedly during the ban. Compared to the matched control stocks, effective spreads widen by 22 basis points for the top quartile, and 58 and 45 basis points for the middle two quartiles. For the subset of eight systemically important firms,

effective spreads widen by 18 basis points. All of these estimates are reliably different from zero, and all represent a similar proportional widening in spreads. The same is true of the other market quality measures.

Overall, it seems quite clear that market quality is markedly worse for all but the smallest stocks subject to the shorting ban. This makes sense, as the shorting ban temporarily excluded many market participants, including hedge funds and proprietary trading desks, that were not formally market-makers but typically would provide substantial amounts of liquidity via shorting.

9. Short-sale aggressiveness

Next we examine whether the market-makers who short during the shorting ban are different from the population of short sellers at other times. Because we have intraday data on the time and price of every executed short sale, we can measure the average effective spread (in basis points) that short sellers pay. For a transaction in stock i at time u , we measure the proportional effective spread F_{iu} as:

$$F_{iu} = 2 (M_{iu} - P_{iu}) / M_{iu}, \tag{2}$$

where P_{iu} is the price at which shares are sold short at time u , and M_{iu} is the prevailing quote midpoint at the time of the short sale. We scale by 2 to make these effective spread numbers comparable to the broader market effective spreads discussed earlier. We then compute trade-weighted averages for each day to aggregate these individual-firm effective spreads up to a trading day level. Note that this measure is positive if short-sellers demand liquidity on average and negative if they supply liquidity on average.

We also calculate the 5-minute price impact for all short sales as in the previous section, except that we use only executed short sales in the calculation. Because we are only dealing with sales here, a positive price impact for short sales measures the midquote decline in the five minutes after the short sale, and it is a measure of the short-term informativeness of the short sale.

Figure 7 and Table 2 compare the measures across different groups and over time. Along this dimension, the matching procedure seems to have worked well. Prior to the shorting ban, there is no difference in the behavior of short sellers for the 404 initially banned financial stocks in the sample vs. the matched non-banned control stocks. Median effective spreads for short sellers in Table 2 Panel A are -1 basis point in both groups, indicating that on average short sellers have a very slight bias toward supplying liquidity in the pre-ban period. Average 5-minute price impacts on short sales are similarly tiny.

When the shorting ban takes effect, short sellers do not significantly change their overall aggressiveness in non-banned stocks, and there is no significant change in short sale price impacts for this set of stocks. For example, the median effective spread for short sellers for the control group is still -1 basis point. In contrast, for the stocks on the original ban list, those who can still sell short become much more aggressive on average, paying a median effective spread of 15 basis points to complete their short sales during the shorting ban. They are also slightly more informed during the shorting ban, at least based on subsequent five minute returns. Median short sale price impacts in banned stocks go from -2 basis points during the pre-ban period to 8 basis points during the ban. Panel A of Table 5 has the full-sample panel regression results for short sale effective spreads and short sale price impacts. During the shorting ban, those who short sell affected stocks pay an additional 21 basis points in effective spread, and short sale price impacts are 20 basis points higher in the banned stocks. Interestingly, while the divergence in the two groups during the shorting ban is very clear from Figure 7, the regression results are only marginally significant, with t-statistics below two, perhaps because effective spreads paid by short sellers are quite noisy. Finally, Figure 7 also shows that short sale effective spreads in banned stocks return to their normal value near zero a few days after shorting is again permitted, though these numbers are much more volatile during the post-ban period. The aggressiveness result is also present for the subsample of firms that were added to the ban list later, but Panel B shows that changes in aggressiveness during the ban are not statistically significant for the subsample of banned firms that are matched to a non-banned firm in the same industry. Panel C shows that there are no clear cross-sectional patterns in shorting aggressiveness.

To the extent that the results are significant, what explains the increase in effective spreads and price impacts associated with short sales during the shorting ban? The only market participants who can short sell during the ban are market-makers, so the results simply indicate

that these traders tend to demand immediacy. This makes sense. If an options market-maker sells a put, for example, she will have a strong desire to hedge that trade as quickly as possible, and may use market orders to eliminate her exposure to price moves in the underlying stock. Similar arguments apply to any other market-maker who finds himself with a need to short to hedge away some sort of long exposure.

10. Conclusions

In this paper, we study the September 2008 SEC emergency order that temporarily banned most short sales in nearly 1,000 financial stocks. Using proprietary NYSE and Nasdaq data, we show that shorting activity drops by an average of 65% in affected stocks. The remaining short sellers (who should be exclusively market-makers) tend to demand immediacy, probably to quickly satisfy their hedging needs. Stocks subject to the ban suffered a severe degradation in market quality, as measured by spreads, price impacts, and intraday volatility.

Banned stocks jump in price on September 19, and there is a sharp but temporary decline in price when the shorting ban ends and shorting resumes on October 9. But there is significant confounding news on September 19. TARP is announced (though without any details), Treasury announces a money-market fund guarantee program, and the Fed announces new lending programs against asset-backed commercial paper. When we look at those firms added to the ban list after September 19, we find no price increase at all, suggesting that the ban itself did little to boost returns. Instead, financial firms on the ban list appear to have benefitted largely from the anticipated bailout programs.

Given the evidence, it is not at all clear that the SEC achieved its unstated goal of artificially raising prices on financial stocks, and it is clear that market quality was severely compromised. Should the SEC have done it? While it may seem clear that the answer is no, it is certainly possible that manipulative shorting was a risk for financial stocks during this time period. If there are indeed multiple equilibria, and if short sellers can somehow get a Diamond-Dybvig (1984) bank run started, they will ensure a share price of zero. In fact, earlier in American history, bank managers and owners were well acquainted with this possibility, and most banks were privately held as a result.

Another possibility is that this was a time when we needed financial stocks to be overvalued. Perhaps systemic risk imposes negative externalities throughout the system, and it was optimal for the SEC to artificially support financial stocks in order to reduce systemic risk. Or perhaps behavioral finance is right and investors are prone to waves of excessive pessimism and optimism due to herding, extrapolation bias, or some other form of bounded rationality. If investors were unduly pessimistic, perhaps this kind of regulatory intervention could have changed investors' outlook and confidence.

Nevertheless, with the benefit of some hindsight, it now seems much less likely that short sellers are to blame for the sharp declines in financial stocks during 2008. It is now clear that financial firms such as Citigroup had and have extremely troubled fundamentals, and this rather than abusive shorting probably explains the low stock price levels on these types of stocks. It may be too soon to judge, but we suspect that after some time has passed, future observers will look back at this shorting ban with the same kind of wonder that economists reserve for Nixonian price controls and other similarly fruitless and costly government interventions.

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Table 1. Securities subject to ban on short sales

Panel A. Sample selection	
Total number of stocks on SEC and exchange ban lists	931
Lost because three ticker pairs ambiguously refer to class A/B stocks	-6
Stocks are not continuously listed and traded between 12/31/2007 and 10/31/2008	-56
Remaining stocks	869
Panel B. Distribution of 869 stocks across listing venues	
NYSE	299
AMEX	30
Nasdaq	540
Panel C. Distribution of 839 NYSE and Nasdaq-listed stocks from Panel B	
Other than common stocks	111
Stocks with no trading on at least one day during Aug-Oct 2008	257
Stocks removed due to other data issues	6
Stocks in final sample (202 NYSE and 263 Nasdaq)	465

Table 2. Descriptive statistics

The pre-ban period is 8/1/2008 - 9/18/2008, the ban period is 9/19/2008-10/8/2008, and the post-ban period is 10/9/2008-10/31/2008. The original ban list includes sample stocks that are included on the SEC's initial short-ban list. Added stocks refer to securities that were later added to the shorting ban list. The sample of never-banned stocks is matched based on listing exchange, market cap, trading volume; stocks must also match on whether they have listed options. Relative quoted spreads (RQS) are time-weighted; relative effective spreads (RES) are trade-weighted. RPI5 is the five-minute price impact. Each of these three measures is scaled by the quote midpoint. Number of trades and share volume refer to trading activity on the listing exchange. Relative range is a day's highest price minus the lowest price, divided by the day's VWAP. Shorting volume and number of shorts include both NYSE and Nasdaq shorting flow during official trading hours. RELSS is shorting volume divided by share volume. Shorting RES is the actual RES accrued by executed short sell orders. Similarly, Shorting RPI5 is the actual price impact associated with short orders. % aggressive short volume is the percentage of short sales that execute at prices below the quote midpoint.

Panel A. Means	original ban list			matched sample of never banned stocks			added to ban list later			matched sample of never banned stocks		
	pre-ban	ban	post-ban	pre-ban	ban	post-ban	pre-ban	ban	post-ban	pre-ban	ban	post-ban
Number of stocks	404	404	404	404	404	404	61	61	61	61	61	61
Number of days	34	14	17	34	14	17	34	14	17	34	14	17
RQS	0.0100	0.0256	0.0235	0.0094	0.0157	0.0204	0.0037	0.0101	0.0098	0.0031	0.0061	0.0067
RES	0.0074	0.0188	0.0164	0.0070	0.0117	0.0146	0.0030	0.0083	0.0076	0.0024	0.0045	0.0051
RPI5	0.0033	0.0076	0.0068	0.0029	0.0047	0.0059	0.0014	0.0038	0.0035	0.0012	0.0022	0.0025
Daily number of trades	6,680	6,433	7,790	4,588	6,052	7,279	10,477	11,411	14,765	9,414	12,588	14,645
Market cap (\$ billions)	3.272	3.919	3.517	1.508	1.938	2.276	4.632	5.373	6.707	2.601	3.387	4.003
Daily trading volume (\$ millions)	69.841	66.749	64.214	51.777	60.225	59.043	121.591	134.878	122.906	114.832	132.882	126.266
Relative range	0.0609	0.1158	0.1196	0.0516	0.0855	0.1159	0.0604	0.1198	0.1505	0.0435	0.0737	0.1036
Daily shorting volume (shares)	575,140	190,396	541,267	293,317	362,316	422,721	896,320	380,262	1,015,791	537,931	636,525	745,514
Daily short-sale transactions	2,559	694	2,399	1,492	1,798	2,176	3,711	1,504	4,155	3,006	3,497	4,025
RELSS	0.2178	0.0771	0.1821	0.1948	0.1823	0.1867	0.2194	0.0807	0.1754	0.2271	0.2013	0.2067
Shorting RES	-0.0004	0.0023	0.0008	-0.0006	-0.0007	-0.0004	-0.0001	0.0019	0.0002	-0.0002	-0.0002	-0.0005
Shorting RPI5	-0.0001	0.0015	-0.0003	-0.0002	0.0000	-0.0004	-0.0002	0.0009	-0.0002	-0.0002	0.0001	-0.0003
% aggressive shorting volume	0.5041	0.5275	0.5131	0.5001	0.5060	0.5012	0.5078	0.5075	0.5018	0.5122	0.5098	0.5071
Panel B. Medians												
RQS	0.0055	0.0184	0.0142	0.0042	0.0070	0.0105	0.0025	0.0088	0.0070	0.0021	0.0040	0.0044
RES	0.0042	0.0145	0.0100	0.0033	0.0052	0.0068	0.0020	0.0070	0.0057	0.0016	0.0030	0.0031
RPI5	0.0020	0.0066	0.0046	0.0016	0.0026	0.0034	0.0010	0.0036	0.0027	0.0008	0.0014	0.0015
Daily number of trades	1,015	670	891	1,321	1,267	1,380	4,713	3,782	5,174	5,504	5,861	5,864
Market cap (\$ billions)	0.184	0.140	0.169	0.256	0.280	0.310	1.205	0.703	1.053	0.978	1.251	1.658
Daily trading volume (\$ millions)	3.446	2.648	2.812	4.265	3.716	3.226	17.880	11.349	9.851	21.063	28.292	22.328
Relative range	0.0545	0.1035	0.1138	0.0480	0.0763	0.1070	0.0572	0.1123	0.1310	0.0424	0.0674	0.1004
Daily shorting volume (shares)	39,576	14,019	30,879	49,568	53,575	54,180	250,408	69,569	203,518	230,164	245,737	306,546
Daily short-sale transactions	276	41	200	329	289	324	1,212	431	1,184	1,408	1,467	1,768
RELSS	0.2179	0.0630	0.1849	0.1973	0.1908	0.1962	0.2189	0.0664	0.1718	0.2217	0.2003	0.2096
Shorting RES	-0.0001	0.0015	0.0001	-0.0001	-0.0001	-0.0001	-0.0001	0.0005	0.0000	-0.0001	0.0000	-0.0001
Shorting RPI5	-0.0002	0.0008	-0.0002	-0.0001	0.0002	-0.0002	-0.0001	0.0005	-0.0001	-0.0001	0.0002	-0.0001
% aggressive shorting volume	0.5047	0.5174	0.5041	0.5023	0.5067	0.5048	0.5038	0.4938	0.5026	0.5084	0.5152	0.5032

Table 3. The effect of the shorting ban on trading and short selling volume

Two-way fixed effect regressions of market quality on a ban dummy and other explanatory variables, using a daily panel of matched stock pairs from 8/1/2008 to 10/31/2008. Each stock where shorting is banned is matched to a stock listed on the same exchange that never appears on the ban list. Matching minimizes the sum of the percentage differences in market capitalization and dollar trading volume; pairs must also match on whether they have listed options. Panel B also matches on 3-digit SIC code, and thus only includes industries where some stocks are subject to the shorting ban and some are not. Each regression variable is the difference between the banned stock and its match. The ban dummy equals one on stock-days when short selling is banned in one of the two paired stocks, and is zero otherwise. Number of trades and dollar volume refer to aggregate trading activity on NYSE and Nasdaq. Shorting volume and number of shorts include both NYSE and Nasdaq shorting flow during official trading hours. RELSS is shorting volume divided by share volume. Control variables include pairwise differences in market cap and trading volume (dvol) in \$millions, intraday price range, and share price (vwap). T-statistics based on Thompson (2009) standard errors are in parentheses below the coefficient estimates. In Panel C, only the ban dummy coefficient for each regression is reported in the table. Market cap quartiles in Panel C are based on average market value of equity during the first half of 2008.

Panel A. Matched on exchange, options-listing status, market cap, and trading volume (465 matched pairs)

Dependent variable	ban_dummy	mkt cap	dvol	relnge	vwap	adj. R ²
Number of trades	-1,257 (-4.01)	-0.148 (-1.56)	48 (8.35)	19,072 (4.17)	-38 (-1.11)	82%
Dollar volume	-34,759,711 (-2.56)	1,146 (0.21)		225,943,842 (3.11)	-628,803 (-0.49)	45%
Shorting volume (shares)	-513,087 (-5.51)	-64 (-2.48)	4,151 (3.61)	2,498,764 (2.12)	7,882 (1.39)	54%
Number of shorts	-1,967 (-7.03)	-0.077 (-0.81)	16 (3.96)	4,570 (2.31)	13 (0.69)	60%
RELSS (%)	-11.782 (-7.12)	0.000 (0.81)	-0.001 (-3.03)	-2.422 (-1.23)	0.014 (0.68)	25%

Panel B. Also matched on industry (62 matched pairs)

Dependent variable	ban_dummy	mkt cap	dvol	relnge	vwap	adj. R ²
Number of trades	-1,750 (-3.08)	-0.495 (-1.06)	53 (5.79)	12,835 (2.17)	2 (0.03)	87%
Dollar volume	-17,692,672 (-1.26)	702 (0.35)		60,874,244 (1.35)	-353,291 (-1.00)	74%
Shorting volume (shares)	-406,568 (-3.41)	-12 (-0.36)	4,180 (3.85)	976,532 (2.55)	143 (0.04)	77%
Number of shorts	-1,591 (-3.94)	0.045 (0.32)	17 (5.10)	2,756 (1.80)	-16 (-0.98)	80%
RELSS (%)	-8.344 (-4.55)	0.000 (1.51)	-0.002 (-1.24)	-7.178 (-2.12)	-0.023 (-1.09)	33%

Panel C. Ban-dummy coefficients for regressions on various sample subsets

Dependent variable	Largest TARP firms	Later additions only	Quartile 1 (small cap)	Quartile 2	Quartile 3	Quartile 4 (large cap)
Number of trades	-9,827 (-0.97)	-2,526 (-3.77)	-177 (-4.51)	-16 (-1.05)	-699 (-3.19)	-5,182 (-5.83)
Dollar volume	26,488,283 (0.05)	-25,636,922 (-2.58)	24,282 (0.06)	-505,658 (-0.50)	-1,355,172 (-0.39)	-81,050,576 (-4.83)
Shorting volume	-6,124,872 (-4.57)	-766,324 (-2.90)	-15,789 (-1.95)	1,941 (0.07)	-158,397 (-4.11)	-2,114,384 (-4.74)
Number of shorts	-25,010 (-4.61)	-3,119 (-3.62)	-93 (-3.11)	-178 (-3.49)	-807 (-4.23)	-7,575 (-6.43)
RELSS (%)	-10.718 (-5.53)	-15.518 (-11.36)	-6.230 (-0.86)	-11.025 (-2.76)	-16.014 (-5.07)	-14.833 (-9.72)
Number of pairs	8	61	116	116	117	116

Table 4. The effect of the shorting ban on market quality

Two-way fixed effect regressions of market quality on a ban dummy and other explanatory variables, using a daily panel of matched stock pairs from 8/1/2008 to 10/31/2008. See Table 3 caption for a description of matching and explanatory variables. Dependent variables include time-weighted relative quoted spreads (RQS), trade-weighted relative effective spreads (RES), and equal-weighted five-minute price impact (RPI5). Each of these three measures is scaled by the quote midpoint. Relative range is a day's highest trade price minus the lowest price, divided by the day's VWAP. T-statistics based on Thompson (2009) standard errors are in parentheses below the coefficient estimates. In Panel C, only the ban dummy coefficient for each regression is reported in the table. Market cap quartiles in Panel C are based on average market value of equity during the first half of 2008.

Panel A. Matched on exchange, options-listing status, market cap, and trading volume (465 matched pairs)

Dependent variable (in %)	ban_dummy	mkt cap (\$b)	dvol (\$b)	relrange	vwap	adj. R ²
RQS	0.373 (2.11)	0.001 (0.33)	-0.125 (-3.38)	3.456 (8.40)	0.002 (0.74)	39%
RES	0.321 (3.01)	0.002 (0.82)	-0.146 (-3.79)	3.921 (10.01)	0.002 (1.15)	36%
RPI5	0.109 (2.08)	0.002 (1.53)	-0.062 (-3.74)	1.957 (10.78)	0.000 (0.45)	22%
Relative range	3.815 (4.98)	-0.099 (-1.81)	4.146 (3.68)		-0.097 (-4.79)	31%

Panel B. Also matched on industry (62 matched pairs)

Dependent variable (in %)	ban_dummy	mkt cap (\$b)	dvol (\$b)	relrange	vwap	adj. R ²
RQS	0.171 (1.65)	0.027 (1.38)	-0.162 (-1.30)	1.447 (3.82)	0.000 (0.07)	62%
RES	0.182 (2.13)	0.021 (1.34)	-0.130 (-1.34)	1.972 (4.96)	0.000 (0.14)	60%
RPI5	0.072 (1.74)	0.007 (1.14)	-0.068 (-1.72)	1.039 (5.07)	0.000 (0.35)	46%
Relative range	2.076 (2.10)	-0.015 (-0.11)	3.698 (1.26)		-0.038 (-0.71)	26%

Panel C. Ban-dummy coefficients for regressions on various sample subsets

Dependent variable (in %)	Largest TARP firms	Later additions only	Quartile 1 (small cap)	Quartile 2	Quartile 3	Quartile 4 (large cap)
RQS	0.202 (2.65)	0.351 (5.07)	-0.849 (-1.56)	0.848 (6.65)	0.570 (6.56)	0.271 (5.80)
RES	0.183 (2.58)	0.331 (6.18)	-0.145 (-0.38)	0.576 (5.21)	0.453 (9.76)	0.225 (5.98)
RPI5	0.059 (2.18)	0.160 (4.53)	-0.168 (-0.97)	0.228 (4.21)	0.231 (7.24)	0.092 (5.07)
Relative range	3.257 (1.14)	3.738 (2.74)	-1.621 (-1.00)	5.496 (3.79)	3.886 (2.94)	5.040 (3.48)
Number of pairs	8	61	116	116	117	116

Table 5. The effect of the shorting ban on shorting aggressiveness

Two-way fixed effect regressions of market quality on a ban dummy and other explanatory variables, using a daily panel of matched stock pairs from 8/1/2008 to 10/31/2008. See Table 3 caption for a description of matching and explanatory variables. Dependent variables include the actual relative effective spread accrued by executed short sell orders (shorting RES), the 5-minute price impact associated with short orders (shorting RPI5), the percentage of short sales that execute at prices below the quote midpoint (aggressive shorting volume fraction), and RES for short sales partitioned by aggressiveness. T-statistics based on Thompson (2009) standard errors are in parentheses below the coefficient estimates. In Panel C, only the ban dummy coefficient for each regression is reported in the table.

Panel A. Matched on exchange, options-listing status, market cap, and trading volume (465 matched pairs)

Dependent variable (in %)	ban_dummy	mkt cap (\$b)	dvol (\$b)	relrange	vwap	adj. R ²
Shorting RES	0.206 (1.65)	0.001 (1.38)	0.017 (0.37)	0.082 (0.00)	-0.002 (-1.13)	2%
Shorting RPI5	0.199 (1.92)	0.000 (0.88)	-0.001 (-0.90)	-0.072 (-0.25)	-0.001 (-0.96)	2%
Aggressive shorting volume fraction	-1.831 (-0.20)	-0.001 (0.33)	0.282 (0.46)	0.845 (0.40)	-0.026 (-1.39)	5%
Shorting RES for aggressive shorts	0.877 (6.76)	0.001 (0.19)	-0.096 (-2.72)	3.207 (7.56)	0.000 (3.38)	19%
Shorting RES for passive shorts	-0.459 (-1.58)	0.001 (0.12)	0.099 (2.89)	-2.932 (-7.32)	-0.003 (-5.75)	21%

Panel B. Also matched on industry (62 matched pairs)

Dependent variable (in %)	ban_dummy	mkt cap (\$b)	dvol (\$b)	relrange	vwap	adj. R ²
Shorting RES	0.001 (0.48)	0.154 (1.37)	0.251 (0.40)	0.003 (0.50)	-0.055 (-2.55)	7%
Shorting RPI5	0.001 (1.71)	0.038 (0.65)	-0.111 (-0.23)	0.000 (-0.06)	-0.013 (-0.91)	1%
Aggressive shorting volume fraction	0.025 (1.07)	0.415 (0.41)	11.894 (0.42)	0.028 (0.55)	-0.473 (-1.53)	5%
Shorting RES for aggressive shorts	0.003 (1.35)	0.422 (1.44)	-0.437 (-0.28)	0.026 (3.80)	-0.066 (-1.70)	28%
Shorting RES for passive shorts	-0.003 (-2.39)	-0.180 (-1.25)	0.774 (0.79)	-0.022 (-3.56)	-0.025 (-0.69)	27%

Panel C. Ban-dummy coefficients for regressions on various sample subsets

Dependent variable (in %)	Largest TARP firms	Only later additions	Quartile 1 (small cap)	Quartile 2	Quartile 3	Quartile 4 (large cap)
Shorting RES	0.024 (0.68)	0.169 (2.03)	0.888 (0.78)	0.194 (2.93)	0.204 (2.91)	0.015 (0.69)
Shorting RPI5	-0.023 (-0.63)	0.104 (2.03)	1.737 (1.72)	-0.030 (0.59)	0.133 (3.71)	0.007 (0.42)
Aggressive shorting volume fraction	-2.481 (-0.99)	-0.879 (-0.20)	-10.158 (0.19)	-8.511 (-0.38)	3.126 (-1.80)	-2.269 (1.27)
Shorting RES for aggressive shorts	0.256 (3.25)	0.668 (4.80)	2.474 (1.50)	1.200 (4.93)	0.826 (8.51)	0.379 (6.26)
Shorting RES for passive shorts	-0.140 (-1.81)	-0.394 (-2.99)	-0.434 (0.50)	-0.725 (-1.86)	-0.446 (-4.74)	-0.296 (-4.62)
Number of pairs	8	61	116	116	117	116

Figure 1. Short-sale activity around the U.S. shorting ban. The firm-level short-selling activity measure (RELSS) is calculated for each stock each day as the number of shares sold short on the NYSE and Nasdaq in that stock divided by total trading volume in that stock. For each day from 1 Aug 2008 through 31 Oct 2008, we report the equal-weighted average for 404 NYSE and Nasdaq common stocks on the initial shorting ban list vs. 404 common stocks that are never banned from short-selling and which are matched on listing exchange, pre-sample market cap and dollar trading volume, and whether or not the stock has listed options.

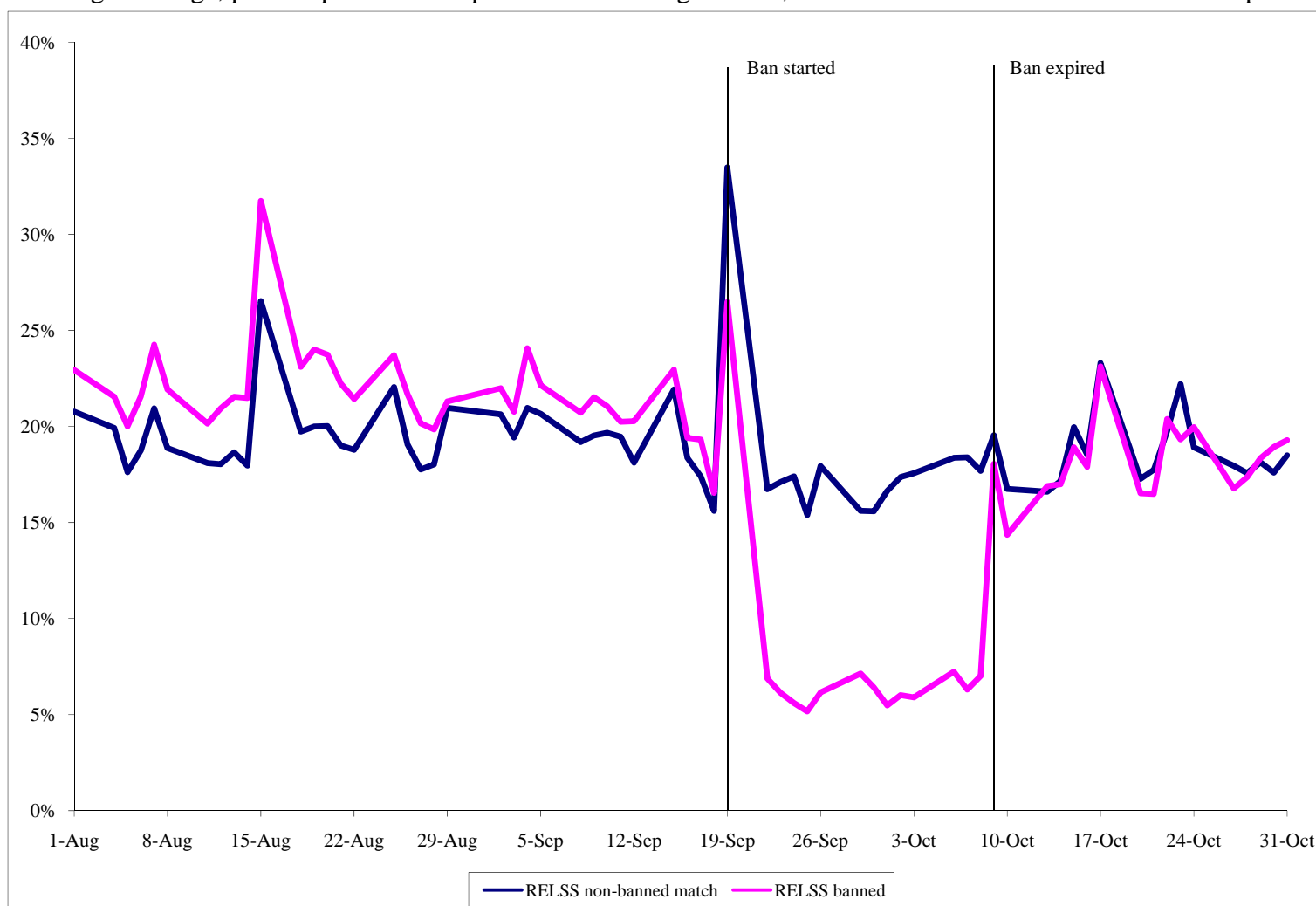


Figure 2. Short interest during 2008 on banned and non-banned stocks. Median short interest (measured as a fraction of shares outstanding) for the sample of 465 NYSE and Nasdaq stocks where shorting is banned vs. an equal number of matched stocks that are not subject to the ban. Dates are the short interest report dates, which are the 1st and 15th of each month. Thus, only the Oct 1 figure reflects short interest during the shorting ban.

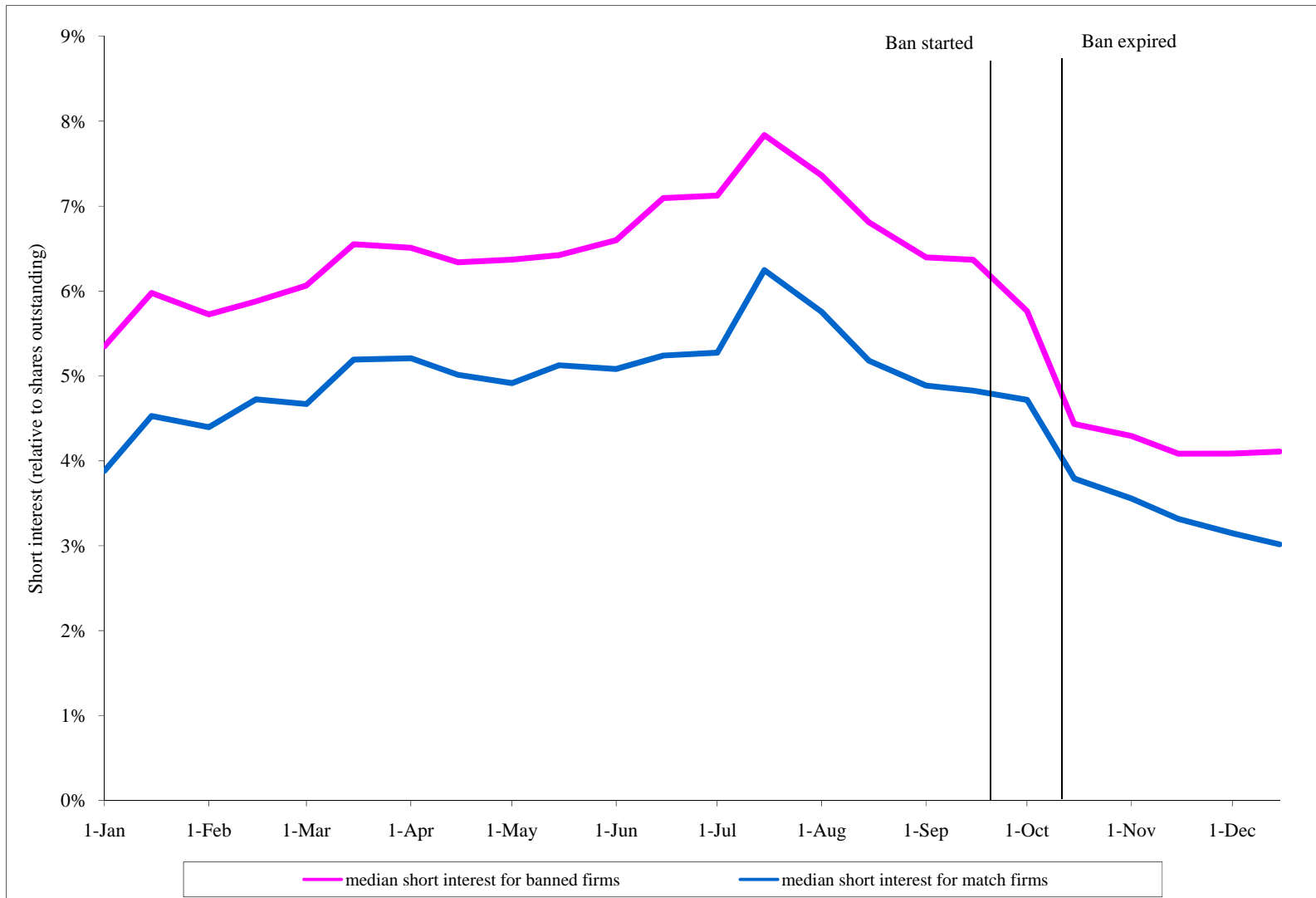


Figure 3. Cumulative returns on banned and non-banned stocks. For each day from 1 Aug 2008 through 31 Dec 2008, we report the equal-weighted average cumulative return for 404 NYSE and Nasdaq common stocks on the initial shorting ban list, the equal-weighted average cumulative return for a matched sample of 404 common stocks where shorting was never banned, and the cumulative return difference between the two.

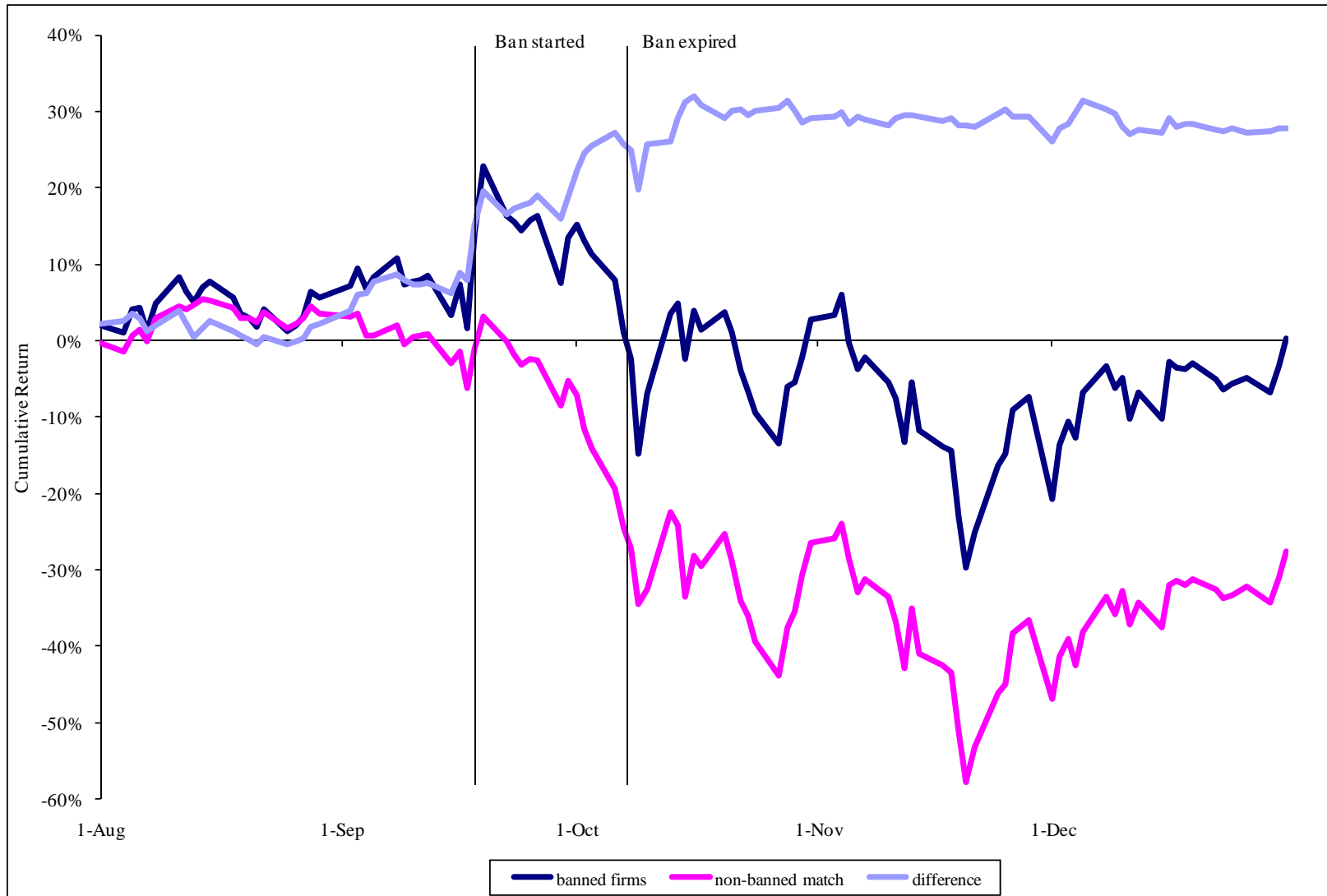


Figure 4. Cumulative abnormal returns on stocks later added to ban list. Abnormal returns are computed as the equal-weighted average for 61 stocks added to the shorting ban list after 19 Sep 2008 less the equal-weighted return on a matched sample of non-banned stocks. 95% confidence intervals are for the cumulative abnormal return since 18 Sep 2008 and are based on pre-event volatility from 1 Aug through 17 Sep.

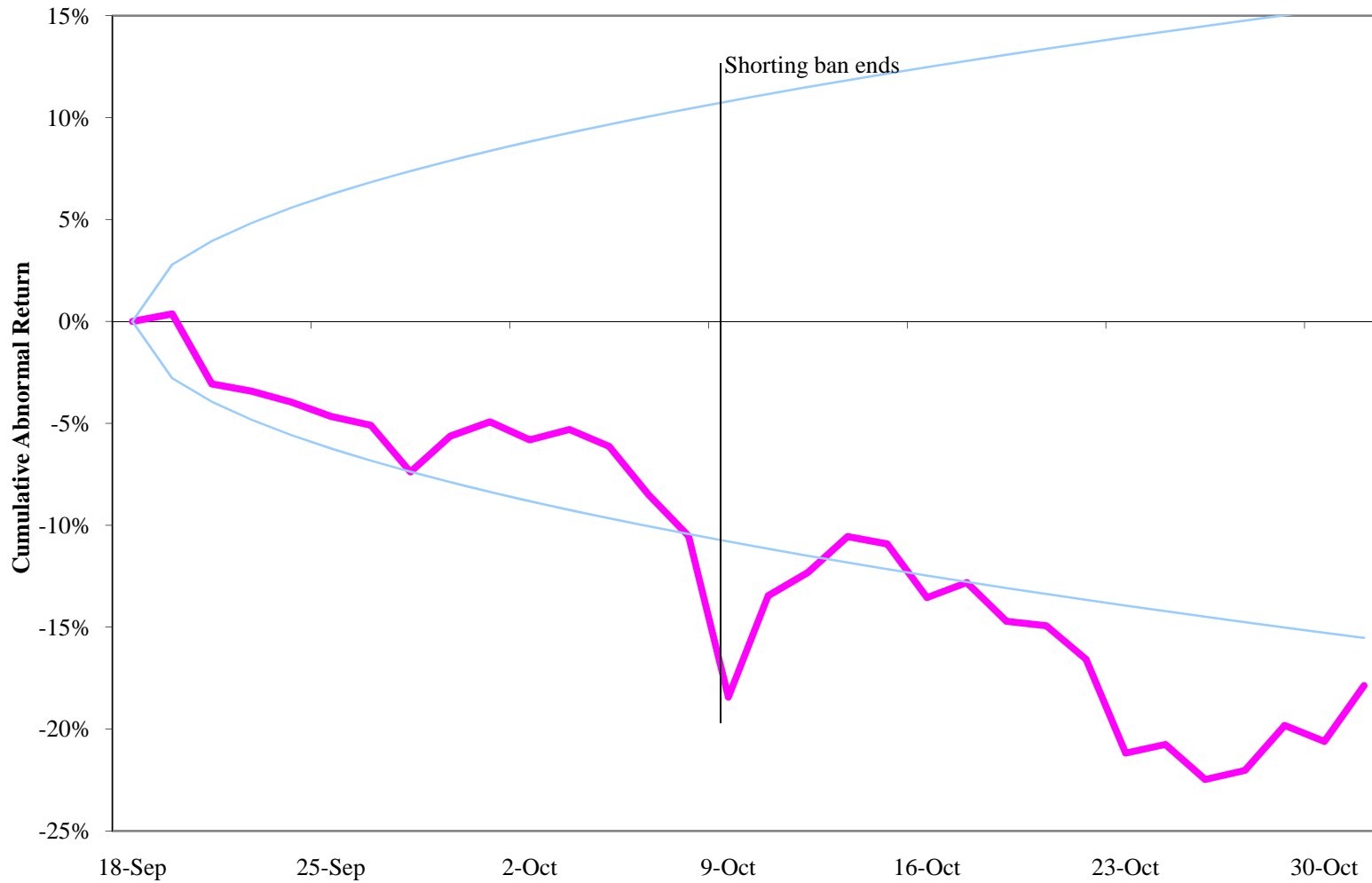


Figure 5. Market quality around the U.S. ban on short-selling. Proportional effective spreads (RES) and 5-minute price impacts (RPI5) are measured for each stock each day. For each day from 1 Aug 2008 to 31 Oct 2008, we report the equal-weighted average for 404 NYSE and Nasdaq common stocks on the initial shorting ban list vs. a matched sample of 404 common stocks that are never banned from short-selling.

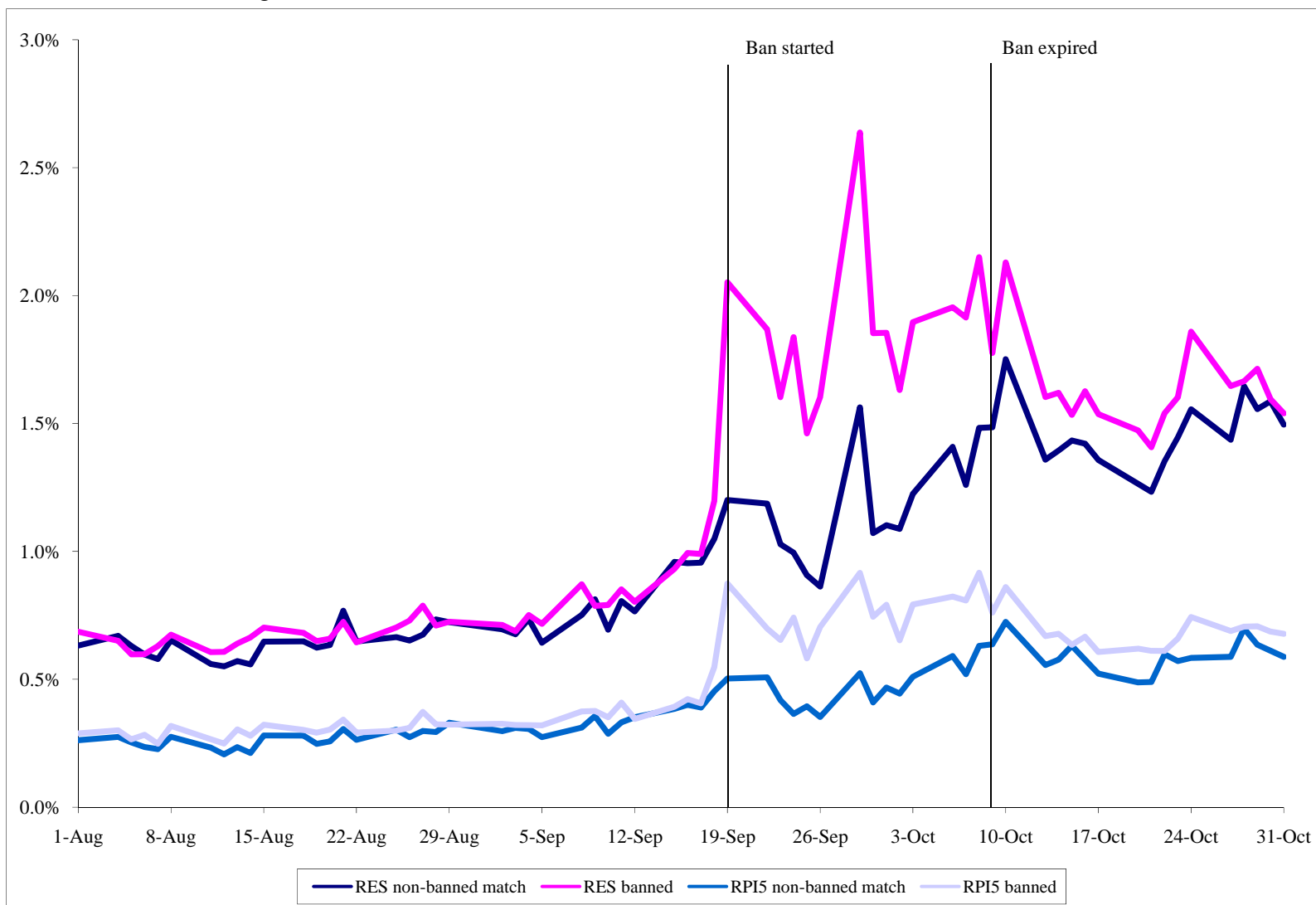


Figure 6. Average-firm level volatility around the U.S. ban on short selling. The volatility measure is the proportional trading range (RVOL), defined as the difference between the highest and lowest transaction price recorded for a given stock on a given trading day, divided by the volume-weighted average trade price for that day. For each day from 1 Aug 2008 through 31 Oct 2008, we report the equal-weighted average for 404 NYSE and Nasdaq common stocks on the initial shorting ban list vs. a matched sample of common stocks that are never banned from short-selling.

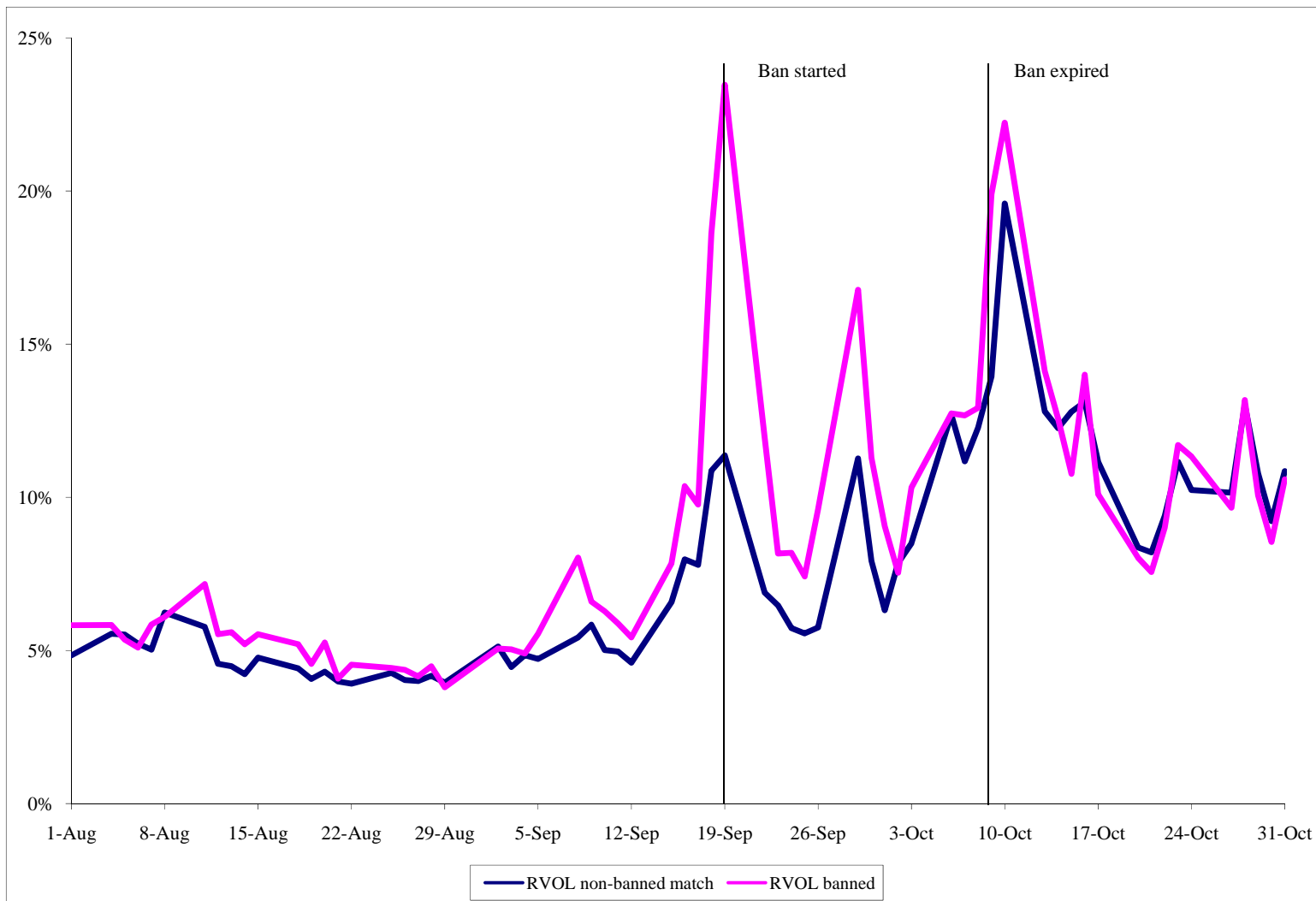


Figure 7. Effective spreads for short sales around the U.S. shorting ban. Average proportional effective spreads on short sales are measured for each stock each trading day. For each day from 1 Aug 2008 through 31 Oct 2008, we report the equal-weighted average for 404 NYSE and Nasdaq common stocks on the initial shorting ban list vs. a matched sample of common stocks that are never banned from short-selling.

