

Shackling Short Sellers: The 2008 Shorting Ban

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In September 2008, the U.S. Securities and Exchange Commission (SEC) temporarily banned most short sales in nearly 1,000 financial stocks. We examine the ban's effect on market quality, shorting activity, the aggressiveness of short sellers, and stock prices. The ban's effects are concentrated in larger stocks; there is little effect on firms in the lower half of the size distribution. Although shorting activity drops by about 77% in large-cap stocks, stock prices appear unaffected by the ban. All but the smallest quartile of firms subject to the ban suffer a severe degradation in market quality. (*JEL* G14)

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 New York Stock Exchange's (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). However, 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

We thank Frank Hatheway, Robert Battalio, and NASDAQ for providing data. We thank Robert Battalio (a referee), Peter Dunne, Tim McCormick, David Musto, Maureen O'Hara, Laura Starks (the editor), Ingrid Werner, Avi Wohl, and an anonymous referee for their valuable comments and suggestions. We also appreciate feedback from seminar participants at the NASDAQ Economic Advisory Board, the New York Fed, Purdue University, University of Illinois, University of North Carolina, University of Washington, Yale University, the 2010 AFA and RMA Securities Lending meetings, the Central Bank Conference on Market Microstructure, University of Michigan Mitsui Life Symposium, University of Notre Dame Conference on the Future of Securities Market Regulation, and the NYSE Euronext TI Workshop on Liquidity and Volatility in Today's Markets. Send correspondence to Ekkehart Boehmer, EDHEC Business School, EDHEC Risk Institute-Asia, One George St., 049145 Singapore; telephone: +65.6631.8579. E-mail: ekkehart.boehmer@edhec.edu.

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 various liquidity measures, the rate of short sales, the aggressiveness of short sellers, and in stock prices before, during, and after the shorting ban. We compare banned stocks to a control group of nonbanned stocks to identify these effects.

We find that during the shorting ban, shorting activity in large-cap stocks subject to the ban drops by about 77%. All but the smallest stocks subject to the ban (those in the smallest size quartile) suffer a severe degradation in market quality, as measured by spreads, price impacts, and intraday volatility. In contrast, the smallest-quartile stocks see little impact from the shorting ban. Stock price effects are difficult to discern, as there is substantial contemporaneous, confounding news about the Troubled Asset Relief Program (TARP) and other government programs to assist the financial sector. 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 an artificial boost in prices.

Given this backdrop, it is not surprising that several papers contemporaneously address the recent short sale bans. Most are complementary, focusing on different aspects of the shorting restrictions. For example, our paper focuses on intraday data to shed light on the U.S. ban's effects on equity trading activity and market quality, whereas Battalio and Schultz (2011) study individual equity options markets during the ban (see also Grundy, Lim, and Verwijmeren 2012). Harris, Namvar, and Phillips (2013) gauge stock price effects, whereas Kolasinski, Reed, and Thornock (2013) study naked shorting prohibitions and analyze stock price responses to short interest announcements during 2008. Bailey and Zheng (2013) show that short selling has a stabilizing effect on prices during the crisis periods that surround the shorting ban. Ni and Pan (2011) show that it takes longer for negative information to be incorporated into share prices during the ban.

Closest to our analysis is the contemporaneous work by Beber and Pagano (2013), who look at an international panel of stocks that are subject to different types of shorting bans. Their main result is that shorting bans increase end-of-day bid-ask spreads, implying a decline in stock liquidity when shorting constraints are more severe. They also find some evidence of slower price discovery during shorting bans but detect no effect on share prices. Our study on the U.S. shorting ban complements Beber and Pagano's (2013) cross-country analysis well. Their data are broader as they cover thirty different countries, but this breadth confines the analysis to broadly available data. Specifically, Beber and Pagano (2013) use prices and the indicative (and possibly nonbinding) end-of-day quoted spreads from Datastream, rather than actual intraday transaction costs. They cannot measure short-selling activity across countries and therefore

do not know to which extent shorting bans were actually enforced across countries. In contrast, we use intraday data on trades and binding quotes to compute the standard measures of market quality (including effective spreads, realized spread, price impact, and intraday volatility) and link them to ban-induced changes in short-selling intensity. We also employ daily data on actual shorting flows to gauge the extent to which the ban is effective in reducing short selling across stocks and how this reduction affects market quality. Additionally, we use metrics of how difficult it is to borrow a stock and whether a stock is heavily traded by algorithmic traders to examine channels that potentially link the shorting ban to market quality in the affected stocks.

Owing mostly to these differences in the nature of the underlying data, Beber and Pagano's (2013) tests primarily describe how the effects of shorting bans differ across countries and how bans on naked shorting and bans on covered shorting have different effects. In contrast, we analyze one market in depth for which we can precisely measure changes in the quantity of shorting (a variable not available to Beber and Pagano 2013) and then link these changes to variation in the market quality of affected stocks. In terms of methodology, we construct difference-in-differences tests that allow us to isolate the effects of the ban, whereas Beber and Pagano (2013) employ a firm-day panel that gives more weight to firms in countries that experience longer bans than to firms in countries with short bans (such as the United States). Moreover, Beber and Pagano (2013) restrict their main parameters to be the same across countries in the interest of parsimony. This comes at the cost of ignoring cross-country differences, such as differences in financial market development, information environment, investor protection regulation, etc. In contrast, our one-country study is complementary in the sense that it neither requires subjective decisions on how to weight each observation nor suffers from cross-country heterogeneity. Instead, it allows a much more detailed look at the nature of equity trading before, during, and after the ban.

Other regulatory restrictions on shorting have been studied as well. Jones (2012) studies a variety of restrictions in the United States during the Great Depression and observes large stock price effects but only modest effects on liquidity. Diether, Lee, and Werner (2009) and Boehmer, Jones, and Zhang (2009) find small market-quality effects associated with the repeal of the U.S. uptick rule in 2005 and 2007. Bris, Goetzmann, and Zhu (2007) find slower adjustment to negative information in countries with more severe shorting restrictions, as predicted by Diamond and Verrecchia (1987), and Ho (1996) finds that shorting restrictions in Singapore increase volatility. Rhee (2003) finds some evidence of price effects in Japan following imposition of an uptick rule there.

Most previous theoretical and empirical work on shorting restrictions focuses on share price effects. There is less theory linking shorting restrictions to market quality. 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.¹ Based on this insight, their model predicts that if shorting is banned, bid-ask spreads will actually narrow, because liquidity providers will face less adverse selection. In contrast to their hypothesis, a shorting ban could hurt market quality if short sellers are important liquidity providers. Banning short sellers could reduce competition in liquidity provision, worsening the terms of trade for liquidity demanders. Our empirical investigation distinguishes between these two competing hypotheses.

The paper is organized as follows. A detailed time line of events related to the shorting ban is the subject of Section 1. Section 2 discusses the data, including proprietary intraday NYSE, NASDAQ, and BATS data on short sales, as well as our matching procedures. Section 3 discusses the methodology we use, particularly the firm fixed effects models used to isolate the effect of the shorting ban. Main empirical results are discussed in Section 4 with analysis of changes in shorting activity, changes in effective spreads, short-term volatility, and other market quality measures, as well as effects on share prices. Section 5 provides more analysis of the end of the ban and on interactions of the ban with hard-to-borrow stocks and algorithmic trading. Section 6 concludes.

1. Time Line of Events

The temporary ban on the shorting of financial stocks is the broadest and, at the time, probably the most unexpected, 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 nineteen financial stocks.² 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 12:01 a.m. (EST) on Thursday, September 18. On Thursday, September 18, the United Kingdom's Financial Services Authority (FSA) instituted a temporary ban on short sales in thirty-two 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

¹ Empirical evidence finds that short sellers are well informed and enhance price discovery. See, for example, Dechow et al. (2001), Desai, Krishnamurthy, and Venkataraman (2006), Boehmer, Jones, and Zhang (2008), Boehmer and Wu (2013), Saffi and Siggurdsson (2011), and Aitken et al. (1998), among others.

² Market makers were exempt from the July 2008 emergency order for naked short sales executed as a result of bona fide market-making activity. Kolasinski, Reed, and Thornock (2013) show that the July 2008 emergency order made it more costly to borrow shares in the affected stocks and reduced shorting activity in those stocks.

ban on all short sales in 797 financial stocks.³ 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 ten business days, terminating at 11:59 p.m. (EST) on October 2, 2008, but could be extended under the law to last for a maximum of thirty calendar days.⁴

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 NYSE banned all short sales in the wake of England's announcement that it was abandoning the gold standard. As Jones (2012) 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 up prices.

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 in the equity market that are 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 all decisions about the ban status of a listed firm to the exchanges. 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 (OTC) 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

³ The emergency order claimed to cover 799 stocks, but only 797 were actually listed in the order.

⁴ 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.

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 may have been 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 thirty-two stocks to the list on this day and forty-four 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 real estate investment trusts (REITs) as well as a few broker-dealers and asset managers, who may have been concerned about looking hypocritical given that at least some of their revenues relied on the continued viability of short sales. For some of our tests, we examine these withdrawing firms separately.

On October 2, 2008, at the end of the initial ten-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 11:59 p.m. (EST) 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.

2. 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 data from six different sources. Stock returns are from the Center for Research in Security Prices (CRSP), and the TAQ database is used to calculate market quality and other intraday measures. The NYX and NASDAQ Web sites provide dates and details about stocks initially included on, added to, and/or deleted from the shorting ban list. From the NYSE, NASDAQ, and BATS, we have data on

Table 1
Securities subject to the 2008 shorting ban

Panel A: Sample selection

Total number of stocks on SEC and exchange ban lists	931
Lost because tickers ambiguously refer to multiple share classes	-7
Stocks without information on CRSP	-55
Remove other than common stocks	-113
Remove AMEX common stocks	-29
Stocks in final sample	727
Sample stocks on the original ban list	665
Sample stocks added to the ban list later	62

Panel B: Matching statistics

	Full sample		Size quartile 1 (smallest)		Size quartile 2		Size quartile 3		Size quartile 4 (largest)	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Number of stocks	727		181		182		182		182	
Dec. 2007 market cap (\$ millions)	4,588.9	242.5	48.5	46.5	149.6	138.9	565.3	481.0	17,567.1	4,881.3
Ban										
Control	5,308.3	250.0	51.6	47.8	153.0	140.7	565.2	473.1	20,434.7	4,606.5
Difference	-719.5	0.1	-3.0	-0.2	-3.4	0.3	0.1	1.0	-2,867.5	1.1
p-value (t-test)	0.20		0.06		0.03		0.98		0.21	
p-value (Wilcoxon)	0.84		0.38		0.64		0.77		0.68	
Consolidated monthly trading volume in round lots	7,702.5	189.9	19.0	6.0	103.0	30.0	974.9	480.0	29,671.0	8,726.7
January-July 2008										
Ban										
Control	6,674.2	194.1	23.3	11.7	105.5	33.1	977.7	490.8	25,553.7	8,751.0
Difference	1,028.3	-0.6	-4.3	-0.5	-2.5	-0.8	-2.8	-0.2	4,117.3	5.8
p-value (t-test)	0.04		0.00		0.12		0.61		0.04	
p-value (Wilcoxon)	0.00		0.00		0.00		0.34		0.55	

This table describes sample selection and matching procedure. Panel A presents the sample selection procedure. We begin with all stocks that appear on either the SEC's or the exchanges' list of stocks in which short selling is not allowed during the 2008 ban. Panel B presents a comparison of the 727 ban firms with 727 matched firms. Matches require the same option listing status and the same listing exchange; the match then minimizes the sum of absolute percentage deviations in December 2007 market capitalization and January-July 2008 dollar volume. We present the mean and median for ban and match firms, and we provide p-values for the difference between ban and match firms.

barely distinguishable between the ban firms and the matched control firms within each quartile (and is statistically indistinguishable for the entire sample, which is not tabulated). The median pairwise size difference is less than 0.4% in each quartile and is not significantly different from zero, except in one case. Dollar volumes are also well matched, although significant differences remain for the two smaller size quartiles. Even in the smallest quartile, however, the median difference is only 5%, and it is only 2.2% in the next smallest quartile. Overall, the two samples appear to be well matched during the preban period, and matching quality tends to be better in the larger size quartiles. Note that in the regression tests, the set of control variables also includes these pairwise differences in market cap and dollar volume, to ensure that the results are not driven by differences in these stock characteristics between the two groups.

Table 1, Panel B, also reveals that most financials subject to the ban are quite small. The median December 2007 market caps for quartiles 1 and 2 are only \$46.5 million and \$138.9 million, respectively. In fact, all of the stocks in these two quartiles are in the bottom market cap decile based on NYSE break points. Similarly, the median stock in quartile 3 would find itself in the ninth NYSE market cap decile. Only the largest quartile of banned stocks would not be considered small-cap. The median stock in quartile 4 would be in the fourth NYSE decile. Of course, there are quite a few large cap financials, and in some of our tests, we consider these large financial firms separately.

In robustness tests, we also consider noncommon stocks and matches based on industry. Specifically, we take all three-digit SIC codes for which 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 sixty-two 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 nonfinancial industries with modest financial arms. It also differs from the base sample in that securities other than common stocks are included.

To create a subset of large, systemically important firms for separate analysis, we identify the nineteen large financials that were subject to the SEC's temporary emergency ban on naked shorting in July 2008. 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 assistance, and we 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.

3. Methodology

We describe the effects of the shorting ban graphically and in firm-pair fixed effects panel regressions. Most of the figures compare the 665 sample stocks on the original ban list to the 665 matched control stocks for which shorting is never banned. We use this subset of banned stocks in the figures because the event dates are the same for all of them, making it easy to visually identify the effects of imposing and ending the ban by comparing banned stocks to otherwise similar nonbanned stocks.

Our panel regression analyses incorporate all $727 \times 2 = 1,454$ stocks in the sample, including stocks that were added to the ban list after September 19 and the matching control stocks. Using this sample and various subsets, we estimate the following fixed effects model for a variety of left-hand side variables Y_{it} measured for matched pair i on day t :

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

where Y_{it} is the measured quantity Y for the banned stock less the measured quantity for its nonbanned match. On the right-hand side, a matched pair fixed effect is present, and D_{it}^{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 nonban period. 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 nonbanned stocks during the ban versus 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.⁶

Statistical inference is conducted using Thompson (2011) standard errors. This technique allows for both time-series and cross-sectional correlation of the regression errors, as well as heteroscedasticity. In general, we find that these robust standard errors are very similar to ordinary least squares standard

⁶ As a robustness check, we use a Fama-MacBeth approach that we construct as follows. We estimate model (1) using only the 665 firms on the original ban list and their matched control firms. We omit the ban dummy and instead add day fixed effects to the model. Fourteen of the day fixed effects represent days during the ban period and forty represent nonban days. Their respective means are an estimate of the conditional ban and nonban paired differences between ban and control firms. We use a two-sample t -test to see whether the mean time fixed effects coefficient of the two sets are different from each other. This procedure produces results that are qualitatively identical to the ones presented in the tables.

errors, suggesting that the matched-sample methodology and control variables are removing most of the correlation that is present across observations.⁷

4. Main Results

4.1 Effects on shorting activity and trading activity

Table 2 provides summary statistics on shorting activity for the different groups of stocks before, during, and after the ban. For the 665 sample stocks on the original ban list, short sales account for an average of 21.40% of trading volume during the preban period from August 1 through September 18. Not surprisingly, the shorting ban had a dramatic effect on short selling activity, but shorting does not decline to zero. During the shorting ban (September 19 through October 8), short sales drop to 9.96% of overall trading volume for stocks on the original ban list. 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. For this group of stocks, shorting then rebounds to an average of 17.62% of trading volume during the postban period (October 9 to October 31).

Figure 1 shows that large-cap stocks experience the sharpest reductions in shorting. In the large-cap quartile of banned stocks, shorting averages only 6.6% of shares traded during the ban versus 28.2% in the pre- and postban periods. In contrast, small stocks experience little change in the amount of shorting during the ban. For the smallest market-cap quartile of banned stocks, shorting accounts for an average of 10.5% of share volume during the ban, versus 12.7% before and after the ban. The cross-sectional difference probably reflects the differential importance of informal market makers. Informal market makers are subject to the ban and tend to participate in active stocks, where they can supply liquidity algorithmically. Traditional market makers remain important in small-cap stocks, where algorithmic trading and liquidity supply is less pervasive (see, e.g., Hendershott, Jones, and Menkveld 2011).

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

⁷ Thompson (2011) 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.

Table 2
Descriptive statistics around the shorting ban

	Original ban list			Matched sample of never banned stocks			Added to ban list later			Matched sample of never banned stocks		
	Preban	Ban	Postban	Preban	Ban	Postban	Preban	Ban	Postban	Preban	Ban	Postban
Number of stocks	665	665	665	665	665	665	62	62	62	62	62	62
Number of days	34	14	17	34	14	17	34	14	17	34	14	17
Market cap (\$ billions)	1,993	2,382	2,139	927	1,188	1,393	4,558	5,287	6,599	2,560	3,334	3,939
Daily trading volume (\$ thousands)	42,444	40,568	39,029	31,477	36,611	35,888	119,628	132,703	120,923	112,972	130,746	124,236
Relative range	0.0509	0.0933	0.0964	0.0548	0.0930	0.1155	0.0619	0.1214	0.1518	0.0435	0.0736	0.1031
Proportional quoted spread	0.0402	0.0553	0.0580	0.0356	0.0489	0.0565	0.0048	0.0122	0.0130	0.0033	0.0062	0.0069
Proportional effective spread	0.0278	0.0426	0.0427	0.0256	0.0362	0.0414	0.0038	0.0100	0.0099	0.0025	0.0046	0.0052
5-minute price impact	0.0072	0.0107	0.0113	0.0077	0.0104	0.0125	0.0017	0.0042	0.0037	0.0012	0.0022	0.0026
5-minute realized spread	0.0137	0.0213	0.0202	0.0105	0.0155	0.0166	0.0004	0.0017	0.0023	0.0001	0.0002	0.0001
Daily shorting volume (thousand shares)	468	184	429	239	296	338	1,152	544	1,282	694	825	968
Daily short transactions	2,327	841	2,120	1,329	1,616	1,909	5,281	2,559	5,891	4,183	4,944	5,753
RELSS	0.2140	0.0996	0.1762	0.1844	0.1699	0.1675	0.2627	0.1021	0.2070	0.2735	0.2455	0.2471
Shorting RES	0.0012	0.0104	0.0044	0.0018	0.0052	0.0030	0.0000	0.0011	0.0008	-0.0003	-0.0003	0.0002
Shorting RPI5	0.0004	0.0028	0.0008	0.0004	0.0007	-0.0009	-0.0002	0.0011	0.0008	-0.0002	0.0001	-0.0003
% aggressive shorting volume	0.5216	0.5622	0.5269	0.5173	0.5441	0.5217	0.5052	0.4932	0.5030	0.5052	0.5038	0.5040

This table reports summary statistics of sample firms and matching firms. The sample consists of 727 .S. stocks subject to the 2008 shorting ban and a matched control sample of stocks in which shorting was not banned. Matches require the same option listing status and the same listing exchange; the match then minimizes the sum of absolute percentage deviations in December 2007 market capitalization and January–July 2008 dollar volume. The preban period is 8/1/2008–9/18/2008; the ban period is 9/19/2008–10/8/2008; and the postban period is 10/9/2008–10/31/2008. Relative quoted spreads (RQS) are time weighted; relative effective spreads (RES) are trade weighted. RPI5 (the five-minute price impact) and RRS5 (five-minute realized spreads) are also proportional to the prevailing quote midpoint. Shorting and trading volume measures in Panel A are based on NYSE, NASDAQ, and BATS trades during regular trading hours. Relative range is a day's highest price minus the lowest price, divided by the day's VWAP. RELSS is shorting volume divided by share volume. Shorting RES is the actual RES accrued by executed short-sell orders, and shorting RPI5 is the five-minute price impact associated with executed short-sale orders. Aggressive short volume is the fraction of short sales that executed at prices below the quote midpoint.



the stocks underlying the ETF. However, it is not possible to directly assign all shorting during the ban 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 to provide liquidity to that buyer. Thus, the amount of shorting during the ban can be viewed as an upper bound on the substitution by hedge funds and other short sellers into derivatives that were then hedged by market makers. The low shorting numbers thus imply that little such substitution takes place in large-cap stocks during the ban. This is corroborated by Battalio and Schultz (2011), who show that volume in equity options on financial stocks does not change much during the ban.

It is interesting to examine the exact timing of the decline in shorting activity. On Thursday, September 18, the naked shorting restrictions go into effect for all stocks. For our sample of 665 matched control stocks, shorting accounts for only 14.1% of volume that day, compared with an average of 18.44% for the whole preban sample period. In fact, Table 2 also shows that shorting in nonbanned control stocks remains at a lower average level during and after the shorting ban (16.99% of volume during the ban, 16.75% of volume during the postban sample period), further suggesting that the naked shorting restrictions had at least some effect on shorting activity. The large amount of shorting activity in nonbanned stocks on September 19 is somewhat inconsistent with this story (shorting is 30.4% of trading volume in nonbanned stocks on that day), but there are several possible explanations. It could be that market participants anticipated an expansion of the shorting ban and rushed to get short positions in place. Nonbanned stocks might have served as substitutes for banned stocks. 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 nonbanned stocks.

Once the ban is lifted on October 9, shorting increases sharply in the banned large-cap stocks. Figure 1 seems to indicate that a shorting gap remains between the two groups (banned stocks versus control stocks). This gap gradually narrows over the next week, and thereafter the two groups again exhibit similar shorting activity. However, there is no statistical evidence of a postban gap. We cannot reject the null that the two groups have the same shorting prevalence during the whole postban sample period from October 9 through October 31.

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 1,791 ($t=6.31$). The average banned stock sees a decline of 366,516 shares sold short per day ($t=5.68$), and the fraction of trading volume involving a short seller declines by 10.7 percentage points ($t=18.24$).

Panel B of Table 3 partitions the sample by market-cap quartile, confirming the graphical evidence in Figure 1 that the shorting ban has the biggest and

Table 3
The effect of the shorting ban on trading and shorting activity

Panel A: Coefficients in matched sample panel regressions (727 pairs)

Dep. variable	BAN	MKT CAP	DVOL	RVOL	VWAP	Adj. R^2 (%)
Number of shorts	-1,791***	-0.131	24.994***	2,413**	-11	61
Shorting volume	-366,516***	-81.743**	5,615.302***	1,253,968*	4,295	57
RELSS	-0.107***	0.000	0.000***	-0.042**	-0.001	10
Number of trades	-733***	-0.158	49.184***	8,672***	-52	81
DVOL	-6,475,625	1,016.966		116,963,880***	-670,285	43

Panel B: Ban dummy coefficients for regressions on market cap quartile subsamples

Dep. variable	Quartile 1 (smallest)	Quartile 2	Quartile 3	Quartile 4 (largest)
Shorting volume	-723	-1,670	-47,088***	-1,535,593***
Number of shorts	-3	-17***	-320***	-6,810***
RELSS	-0.030**	-0.034***	-0.136***	-0.199***
Number of trades	-7*	-25***	-391***	-3,752***
DVOL	-8,646	-18,309	-966,695*	-43,041,440***
Number of pairs	181	182	182	182

Panel C: Ban dummy coefficients for regressions on various sample subsets

Dep. variable	(A) Largest TARP firms	(B) Industry match	(C) Later additions only	(D) Withdrawn firms
Shorting volume	-10,520,794***	-560,392***	-922,259***	-50,638*
Number of shorts	-40,583***	-2,618***	-4,153***	-304
RELSS	-0.146***	-0.110***	-0.158***	-0.076***
Number of trades	-10,021***	-1,612***	-2,859***	-180
DVOL	-165,996,682	-26,008,337***	-24,304,647*	-6,771,996
Number of pairs	8	62	61	4

This table reports how the shorting ban affects trading and shorting activity. Firm fixed effects regressions of short-selling activity or trading volume measures 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 sample stock subject to the shorting ban is matched to a similar stock for which shorting was not banned (see Table 2 for details). Panel A presents results for the overall sample of 727 matched stock pairs. Panel B reports results for quartile subsamples based on year-end 2007 market cap. Panel C includes results on four different subsamples. The ban dummy (BAN) equals one on stock days for which short selling is banned in the relevant stock and is zero otherwise. Shorting and volume measures are based on NYSE, NASDAQ, and BATS activity during regular trading hours. RELSS is shorting volume divided by share volume. Control variables include pairwise differences in market cap and daily dollar trading volume (DVOL) in \$millions, intraday price range (RVOL), and share price (VWAP); control variable coefficients are only reported in Panel A. Coefficients of DVOL and MKT CAP are multiplied by 10^6 . Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, using standard errors clustered by both firm and date.

most reliable effect on large-cap stocks. RELSS, the fraction of trading volume involving a short seller, is perhaps the easiest measure to interpret. In the smallest quartile, RELSS declines by 3.0 percentage points, and this decline is only marginally 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 13.6 and 19.9 percentage points.

Panel C of Table 3 reports results for other subsamples of interest. For example, there is much less shorting during the ban of our subsample of eight systemically important firms, with RELSS falling by 14.6 percentage points. In the industry match subsample, where we require the banned and nonbanned control stocks to have the same three-digit SIC code, RELSS falls

by a statistically significant 11.0 percentage points. Finally, firms that were added later to the ban list and firms that are withdrawn from the list before the ban ended show significant ban-induced declines in RELSS of 15.8% and 74.6%, respectively.

4.2 Effects on bid-ask spreads

Does the presence of short sellers tend to improve or worsen liquidity? In this section we use the shorting ban to investigate this question. The evidence in the previous section shows that the shorting ban eliminated a substantial subset of trading activity. Brogaard (2011) shows that high-frequency trading accounts for more than 50% of trading volume in recent years, making HFT an important source of short selling. In addition, the direct evidence in Menkveld (2013) indicates that high-frequency liquidity providers could account for a good bit of the observed shorting activity. Many of the high-frequency, or, more generally, algorithmic traders are not registered market makers and thus would be subject to the ban. This suggests that the shorting ban might worsen market liquidity, even though the ban contains 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 P_{it} in stock i at time t and the quote midpoint M_{it} prevailing at the time of the trade:

$$RES_{it} = 2|P_{it} - M_{it}|/M_{it}. \quad (2)$$

To calculate effective spreads, 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_{it} , 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 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.⁸ For buyer-initiated trades, the price impact measure $RPI5_{it}$ is measured as

$$RPI5_{it} = (M_{i,t+5min} - M_{it})/M_{it}, \quad (3)$$

which is the proportional difference between the quote midpoint five minutes after the trade and the quote midpoint prevailing at the time of the trade. For

⁸ Chakrabarty, Moulton, and Shkilko (2012) compare the Lee-Ready trade classification algorithm to the true trade direction from order data. They find that misclassification rates for both short and long sales are near zero at the daily level, which means that our daily effective spread measures should be quite accurate.

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. We focus on effective spreads, but the results for quoted spreads are very similar. During the August 1 to September 18 preban period, for example, average effective spreads are 2.78% for stocks on the initial ban list and 2.56% for the set of matching stocks. These are fairly wide average spreads, reflecting the fact that the sample contains many inactive, small-cap stocks.

While the shorting ban is in effect, these market quality measures diverge considerably. Average effective spreads widen to 3.62% for the control stocks, but effective spreads for the stocks on the initial ban list widen more, to an average of 4.26%.⁹ Statistical inference is conducted via panel regressions using all $727 \times 2 = 1,454$ sample stocks, including stocks that are added to the shorting ban list after September 19. Recall that the panel regressions employ matched pairs and include firm-specific 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 nonbanned stocks on a given day. Based on the full-sample numbers in Panel A of Table 4, the shorting ban is associated with quoted spreads that are 35 basis points wider ($t = 4.47$), and effective spreads that are also 35 basis points wider ($t = 5.45$). Price impacts show an increase as well; the shorting ban is associated with a ten-basis-point increase in five-minute price impacts ($t = 3.96$).

Table 4, Panel B, breaks out the results by market cap and confirms the earlier graphical evidence. Market quality worsens for the three largest market-cap quartiles. For the effective spread panel regression, for instance, the coefficient on the ban dummy is 65, 57, and 35 basis points for quartiles 2, 3, and 4, respectively. In contrast, for the small cap quartile, market quality (as measured by quoted spreads, effective spreads, or five-minute price impact) is not statistically different during the shorting ban. This is not particularly surprising given that the level of shorting activity does not reliably change for these firms during the ban, but it contrasts with Beber and Pagano's (2013) finding that small stocks suffer a greater decline in liquidity. We believe that the discrepancy arises from the different empirical approaches. We measure the domestic effect of the .S. ban only, whereas Beber and Pagano's panel design gives substantially greater weight to firms in countries with longer-lasting and broader shorting bans. For example, Japan and South Korea, both experiencing

⁹ It is possible that declining prices mechanically cause higher relative spread, especially if spreads are near the minimum tick size. However, Beber and Pagano (2013) find no clustering at the minimum tick spread for the United States, so this does not appear to be a problem in our sample.

Table 4
The effect of the shorting ban on market quality

Panel A: Coefficients in matched sample panel regressions (727 pairs)

Dep. variable	BAN	DVOL	MKT CAP	RVOL	VWAP	Adj. R ² (%)
RQS	0.0035***	-2.9460***	0.0214	0.0588***	90.0525**	37
RES	0.0035***	-3.2123***	0.0513	0.0677***	49.7666**	30
RPI5	0.0010***	-1.4293***	0.0342*	0.0321***	10.8682	10
RRS5	0.0015***	-0.3375	-0.0188	0.0033	30.1516	12
RVOL	0.0144***	49.5958***	-0.9587		-917.1359***	23

Panel B: Ban dummy coefficients for regressions on market cap quartile subsamples

Dep. variable	Quartile 1 (smallest)	Quartile 2	Quartile 3	Quartile 4 (largest)
RQS	-0.0047	0.0062***	0.0084***	0.0043***
RES	0.0002	0.0065***	0.0057***	0.0035***
RPI5	-0.0003	0.0013**	0.0024***	0.0015***
RRS5	0.0011	0.0037***	0.0010***	0.0004***
RVOL	-0.0138**	0.0099*	0.0242***	0.0342***
Number of pairs	181	182	182	

Panel C: Ban dummy coefficients for regressions on various sample subsets

Dep. variable	(A) Largest TARP firms	(B) Industry match	(C) Later additions only	(D) Withdrawn firms
RQS	0.0008**	0.0012*	0.0036***	0.0131***
RES	0.0006**	0.0010*	0.0033***	0.0065*
RPI5	0.0002	0.0004	0.0014***	0.0010
RRS5	0.0002	0.0002	0.0005	0.0052***
RVOL	0.0535***	0.0177**	0.0224***	0.0146
Number of pairs	8	62	61	4

This table reports how a shorting ban affects market quality. Firm fixed effects regressions of various market quality measures 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 sample stock subject to the shorting ban is matched to a similar stock for which shorting was not banned (see Table 2 for details). Panel A presents results for the overall sample of 727 matched stock pairs. Panel B reports results for quartile subsamples based on year-end 2007 market cap. Panel C includes results on four subsamples. The dependent variable in each regression is the relevant difference between the banned stock and its nonbanned match. The ban dummy (BAN) equals one on stock days for which short selling is banned in the relevant stock and is zero otherwise. Dependent variables include time-weighted relative quoted spreads (RQS), trade-weighted relative effective spreads (RES), and equal-weighted five-minute price impacts (RPI5) and five-minute realized spreads (RRS5), each of which is scaled by the prevailing quote midpoint. Relative range (RVOL) is a day's highest trade price minus the lowest price, divided by the day's VWAP. Control variables include pairwise differences in market cap and daily dollar trading volume (DVOL) in \$millions, intraday price range (RVOL), and share price (VWAP); control variable coefficients are only reported in Panel A. Coefficients of DVOL and MKT CAP are multiplied by 10¹², and coefficients for VWAP are multiplied by 10⁶. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, using standard errors clustered by both firm and date.

bans that lasted more than seven months and cover all stocks, account for 62% of the ban days in Beber and Pagano's sample (see their Table 1). In contrast, ban days of .S. firms, where the ban lasts only 19 days and is largely limited to financial firms, account for only 1% of ban days in that sample. As a result, the .S. effect does not have a major impact on their inferences. Moreover, firm size likely varies systematically across countries. As a result, Beber and Pagano's large cap versus small cap test (see their Table 5), which does not include country-level variables or effects, potentially contrast nationality rather than firm size. These differences in empirical design make it difficult to compare the inferences regarding firm size. In particular, our result that the ban has stronger

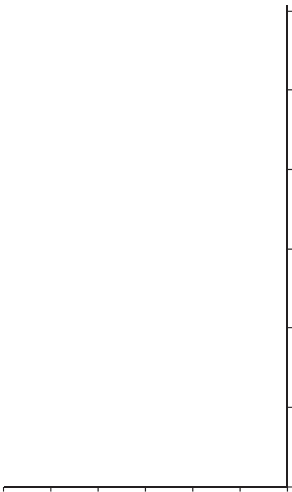
effects on larger firms is not necessarily inconsistent with Beber and Pagano's results.

Figure 2 shows the daily evolution of average effective spreads for the four market-cap quartiles, and Figure 3 provides a similar set of graphs for five-minute price impacts. The liquidity changes are particularly dramatic for large-cap stocks. In particular, the gap between large-cap ban stocks and control stocks opens up immediately at the start of the shorting ban, and stocks subject to the shorting ban remain extremely illiquid throughout the ban period. Effective spreads for large-cap ban stocks average 76 basis points during the shorting ban, compared with 29 basis points outside of the ban period. Analogous spreads for control stocks are 30 basis points during the ban versus 23 basis points pre- and postban. Once the ban ends, effective spreads and price impacts for the two groups move much closer together, though they do not coincide again until the end of October. Interestingly, liquidity remains very poor for both sets of stocks, perhaps because stock market volatility remains extremely high.

Although it is hard to imagine, given the magnitude and timing of the market-quality effects, it is possible that the degraded market quality during the shorting ban is due solely to confounding contemporaneous changes in the information environment, including the tremendous volatility of financial firm fundamentals and the rapid pace of news about TARP and other matters. We address this in several different ways. First, we add an industry match, which limits the analysis to industries in which some firms were banned and some were not. This removes nearly all pure financial firms but leaves sixty-two pairs of stocks for analysis. We also examine two subsets of firms that were added to or removed from the shorting ban list after September 19. Last, but not least, we examine the end of the ban in Section 5.1, where there are fewer potentially confounding events.

Panel C of Table 4 has the results for various subsamples. For the subsample that includes an industry match, effective spreads on ban stocks widen by ten basis points ($t = 1.89$) relative to control stocks. This is only marginally significant, probably because of a reduction in the power of the tests since the industry match requirement reduces the sample size by over 90%. There are similar marginally significant results for quoted spreads; price impacts do not change significantly.

Panel C also has the results for sixty-one firms that are added to the ban list later. For that sample, effective spreads widen by an average of thirty-three basis points ($t = 5.59$), and quoted spreads widen by virtually identical amounts. For this subsample, price impacts are reliably higher as well, increasing by fourteen basis points on average ($t = 5.32$). Column (D) of Table 4, Panel C, has the results for the four firms that withdraw from the ban list and meet our sample requirements. During the period that these firms are subject to the ban, quoted spreads and effective spreads are wider, though the effective spread result is only marginally significant, most likely due to low power from the small sample. Overall, the sharp widening of spreads seems to be a direct result of the shorting ban.





If the shorting ban removes some competing liquidity providers, the remaining liquidity providers may be able to earn greater profits. Although we cannot directly measure trading revenue earned by liquidity providers, realized spreads are a generally recognized proxy for this quantity. Realized spreads are the difference between the transaction price and the quote midpoint at some later time, typically five minutes. This would exactly equal the trading revenue earned by the liquidity supplier if the position is held for five minutes and unwound at the then-current quote midpoint. Formally, the five-minute proportional realized spread for a buyer-initiated transaction in stock i at time t at price P_{it} is given by

$$RRS5_{it} = 2(P_{it} - M_{i,t+5min}) / M_{it}, \quad (4)$$

where M_{it} is the quote midpoint prevailing at time t , and the realized spread is doubled so that it can be directly compared to an effective spread or quoted spread. Realized spreads for seller-initiated transactions have the opposite sign.

The full-sample matched-pair panel regressions are in Panel A of Table 4. All else equal, realized spreads for affected stocks increase by an average of fifteen basis points during the ban, and this effect is significant at the 1% level. When we divide the full sample into quartiles based on market cap (Panel B of Table 4), we find that realized spreads reliably increase for all but the smallest quartile. Figure 4 shows the daily evolution of average realized spreads for each of the quartiles.

For the smaller subsamples considered in Panel C of Table 4, the realized



SEC list versus 5.48% for the matched control stocks, based on the numbers in Table 2. The descriptive statistics show that both groups of stocks experience a sharp increase in intraday range volatility during the shorting ban (an average of 9.33% for initial ban stocks versus 9.30% for control stocks). Note that volatility remains high postban, averaging 9.64% for initial ban stocks versus 11.55% for control stocks.

Statistical tests for the differences in volatility are contained in Table 4, and based on the full sample results in Panel A, the shorting ban is associated with an additional 1.44 percentage points of intraday range ($t = 2.61$). Table 4, Panel B, and Figure 5 show that this result is driven by the two largest quartiles; in fact, the smallest quartile goes in the opposite direction by a significant amount. For the sixty-two banned firms that can be matched to a nonbanned firm in the same industry, Panel C shows a significant increase in average daily range volatility equal to 1.77 percentage points ($t = 2.27$).

Increased volatility during the shorting ban could be due to the worsening market quality, but it could also simply reflect greater tumult in the fundamentals during this time period. Thus, we do not rely on the volatility results to draw conclusions about the effect of the ban on market quality. We return to this issue in Section 5.1, when we study the end of the ban in more detail.

4.4 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 short seller's proportional effective spread F_{iu} as

$$F_{iu} = 2(M_{iu} - P_{iu})/M_{iu}, \quad (5)$$

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 two 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. In addition, we calculate the fraction of short sales that execute at prices below the prevailing quote midpoint, $P_{it} < M_{it}$. These are referred to as aggressive short sales.

We also calculate the five-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 6 graphs the daily average effective spreads for short sales for the four market-cap quartiles over the course of the shorting ban. Again, the most dramatic results are for large-cap stocks. For ban stocks and control stocks alike, the preban effective spreads are generally quite close to zero, indicating that liquidity demand and liquidity provision by short sellers roughly offset each other. When the shorting ban takes effect, short sellers do not seem to change their overall aggressiveness in nonbanned stocks, and there is no significant change in short sale price impacts for this set of stocks. In contrast, those who can still sell short are much more aggressive. This shorting aggressiveness disappears almost immediately once the shorting ban ends.

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 twenty-five basis points in effective spread, and short-sale price impacts are fourteen basis points higher in the banned stocks. Panel B shows that the effective spread results are significant for the two largest quartiles, whereas the results in Panel C indicate that the aggressiveness result is not present in the other subsamples that we have examined, most likely because these effective spreads paid by short sellers are fairly noisy.

What explains the increase in effective spreads associated with short sales during the shorting ban? The natural explanation is heterogeneity among short sellers, which comes to light only during the 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, whereas other short sellers tend to supply immediacy. This makes sense given the nature of market making. 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.

4.5 Effects on stock prices

If the disagreement models (e.g., Miller 1977) are correct and the shorting ban temporarily prevents at least some pessimists from taking a bearish position in a financial stock, the announcement of the shorting ban should temporarily cause prices of affected stocks to rise. This “Miller” effect would lead to overvaluation relative to the fundamentals. However, it is not clear that we should expect a very large stock price effect, because a market participant could use ETFs, puts, credit default swaps, or other derivative instruments to take a bearish position. The main challenge is the presence of confounding events during the shorting ban. On the same day that the shorting ban takes effect, Treasury secretary Henry Paulson announces the creation of what came to be known as the Troubled Asset Relief Program (TARP). It is not surprising that financial firm equity holders would respond positively to the announcement of a program “to remove these

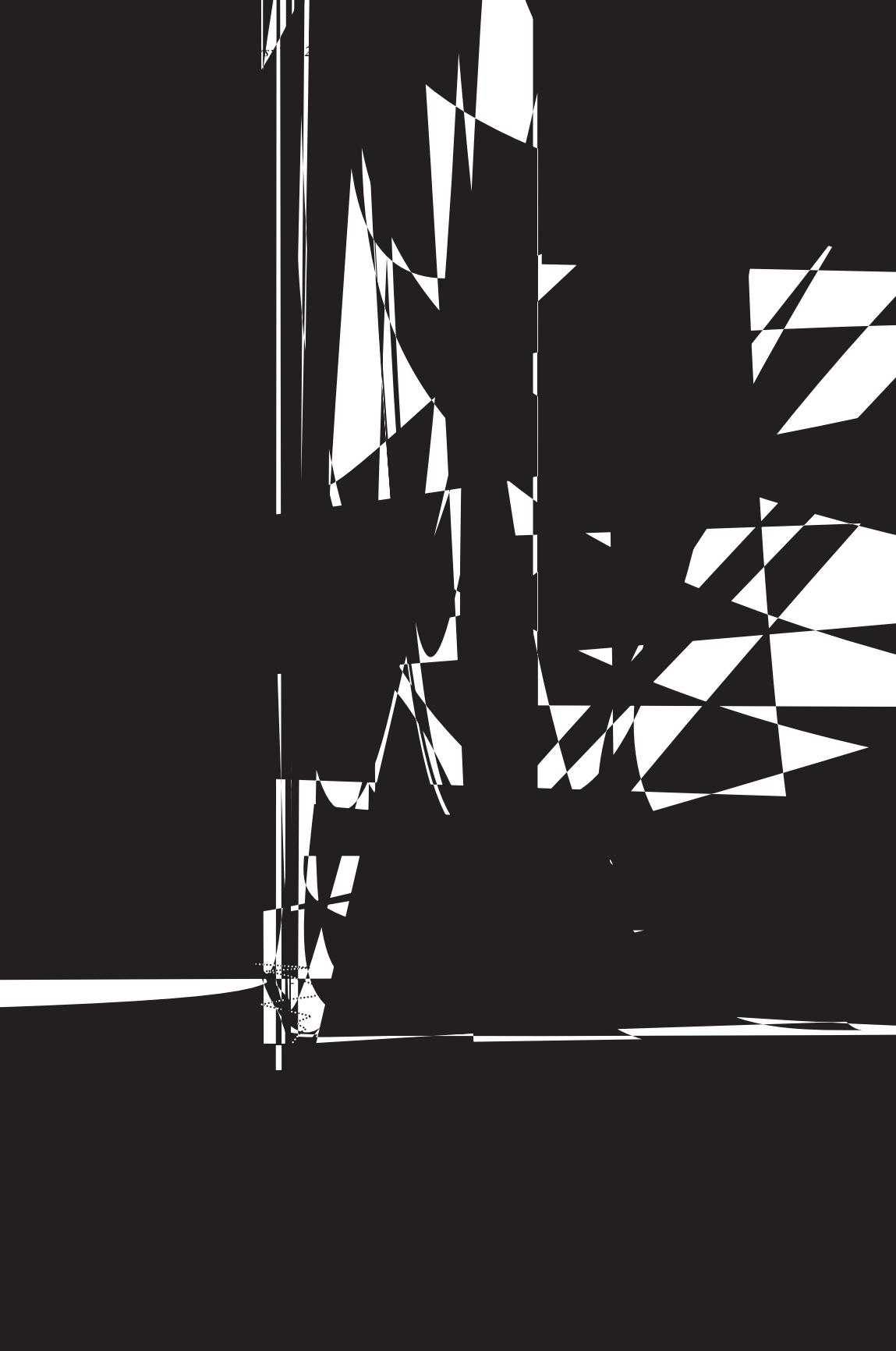


Table 5
The effect of the shorting ban on shorting aggressiveness

Panel A: Coefficients in matched sample panel regressions (727 pairs)

Dep. variable	BAN	DVOL	MKT CAP	RVOL	VWAP	Adj. R^2 (%)
Shorting RES	0.0025***	-0.2457	0.0257	0.0020	-11.5584	8
Shorting RPI5	0.0014***	-0.1071	0.0053	-0.0015	-1.9781	6
% aggressive shorting volume	0.0008	-2.7203	0.2964	0.0421	-88.7065	5
Shorting RES for aggressive shorts	0.0082***	-2.3267***	0.0342	0.0457***	22.8121	19
Shorting RES for passive shorts	-0.0049***	2.1041***	-0.0202	-0.0465***	-34.7839*	22

Panel B: Ban dummy coefficients for regressions on market cap quartile subsamples

Dep. variable	Quartile 1 (smallest)	Quartile 2	Quartile 3	Quartile 4 (largest)
Shorting RES	0.0131	0.0018	0.0028***	0.0017***
Shorting RPI5	0.0070***	0.0018*	0.0018***	0.0002
% aggressive shorting volume	0.0751***	0.0161	0.0172	-0.0259***
Shorting RES for aggressive shorts	-0.0019	0.0109***	0.0118***	0.0063***
Shorting RES for passive shorts	-0.0081	-0.0093***	-0.0071***	-0.0027***
Number of pairs	181	182	182	182

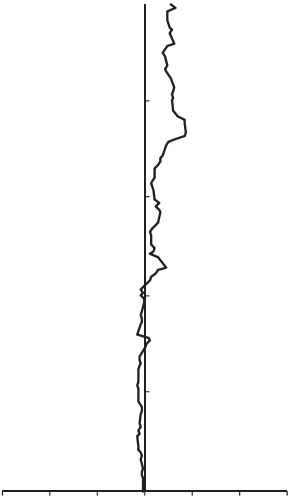
Panel C: Ban dummy coefficients for regressions on various sample subsets

Dep. variable	(A) Largest TARP firms	(B) Industry match	(C) Later additions only	(D) Withdrawn firms
Shorting RES	0.0000	0.0011	0.0016	0.0016
Shorting RPI5	-0.0006	0.0010**	0.0006	-0.0024
% aggressive shorting volume	-0.0435**	-0.0049	-0.0207	-0.0720
Shorting RES for aggressive shorts	0.0011**	0.0027*	0.0069***	0.0029
Shorting RES for passive shorts	-0.0003	-0.0017**	-0.0033***	-0.0061*
Number of pairs	8	62	61	4

This table reports how a shorting ban affects shorting aggressiveness. Firm fixed effects regressions of various shorting aggressiveness measures 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 sample stock subject to the shorting ban is matched to a similar stock for which shorting was not banned (see Table 2 for details). Panel A presents results for the overall sample of 727 matched stock pairs. Panel B reports results for quartile subsamples based on year-end 2007 market cap. Panel C includes results for subsamples. The dependent variable in each regression is the relevant difference between the banned stock and its nonbanned match. The ban dummy (BAN) equals one on stock days for which short selling is banned in the relevant stock and is zero otherwise. Dependent variables include the actual relative effective spread received by executed short-sell orders (shorting RES), the five-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. Control variables include pairwise differences in market cap and daily dollar trading volume (DVOL) in \$millions, intraday price range (RVOL), and share price (VWAP); control variable coefficients are only reported in Panel A. Coefficients of DVOL and MKT CAP are multiplied by 10^{12} , and coefficients for VWAP are multiplied by 10^6 . Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, using standard errors clustered on both firm and date.

illiquid assets that are weighing down our financial institutions and threatening our economy.” In contrast to a “Miller” effect, a TARP-related price change should be permanent and reflect the present value of the implied subsidy to our sample firms.

Figure 7 shows equal-weighted cumulative raw and abnormal returns for the 665 sample stocks on the SEC’s original shorting ban list, divided into market-cap quartiles, as well as cumulative returns for the set of matched control stocks for which shorting is never banned. On Friday, September 19, prices impound the news of the day, including the announcement and immediate



implementation of the emergency shorting ban. The 665 banned stocks rise by 6.68% that day, compared with an average 3.48% return for the matched control stocks for which shorting is never banned.

Most notably, each of the cumulative return differences appears to be permanent—there is no correction on October 9 when shorting resumes. In fact, we find that banned stocks as a group outperform the matched control firms by an average of about 30% through the end of 2008. This aggregate return difference is statistically significant at the 1% level (data not tabulated).

This indicates, at a minimum, that the price increases on September 19 do not represent a Miller-type equilibrium. In fact, the 30% excess return over this time period strikes us as a reasonable estimate of the value of the various government support programs for the financial sector. Specifically, our sample firms subject to the shorting ban have an aggregate market cap of $\$4.6 \text{ billion} \times 727 = \3.4 trillion . Thus, our estimated 30% abnormal return implies a subsidy of \$1.1 trillion. This is not too far from the initial nominal TARP value of \$700 billion if one takes into account potential implied guarantees and risk assumptions that were discussed at the time.

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 sixty-two such firms in our sample, and they are matched in the same way to a firm for which shorting is never banned. There are a handful of financial firms in this group, but many if not most are nonfinancial 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 8 displays the cumulative equally weighted excess returns on the firms added later to the ban list versus their matched nonbanned 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 sixty-two firms, average excess returns on this day are very close to zero (0.37%, to be precise). This suggests that market participants initially do not expect the ban to benefit these firms. 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 8 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 at the 5% level by October 8. This suggests that for these stocks, any artificial price boost from the shorting ban is outweighed by associated negative price effects.

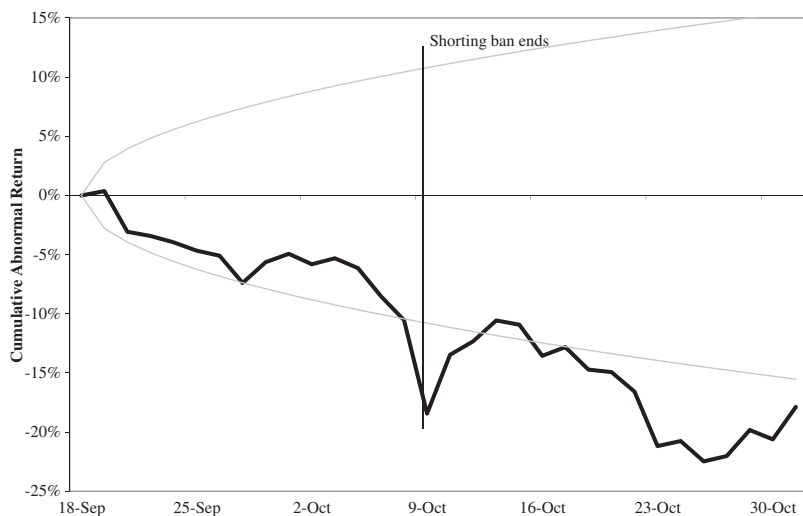


Figure 8
Cumulative abnormal returns on stocks later added to ban list

This figure plots cumulative abnormal returns for stocks added to the ban later. We compute cumulative returns for 62 common stocks listed on NYSE or NASDAQ that were subject to the shorting ban but were not on the original list of stocks in which shorting was banned and for a set of 62 matched control firms that are not subject to the shorting ban. For each day from September 18, 2008 through October 31, 2008, we report the daily equal-weighted average of cumulative abnormal returns, computed as the difference between ban firms and control firms. Control firms are matched by option status, listing exchange, market cap, and dollar trading volume. We also report 95% confidence intervals for the cumulative abnormal return (based on pre-event volatility from August 1 through September 17).

Overall, largely consistent with Beber and Pagano (2013), we see little concrete evidence that the shorting ban caused a temporary upward bump in prices. We can attribute the positive abnormal returns around the ban implementation date to the TARP subsidies that are announced contemporaneously. Moreover, the evidence in fact points the other direction for companies that asked to be added to the ban list and then experience negative returns, most likely due to the negative signal associated with that request.

5. Further Analysis

5.1 End of the ban

As noted earlier, the period from August 2008 to October 2008 is quite eventful. For instance, on the same day the ban was implemented, TARP was also announced, albeit with very few details. Given the contemporaneous news concerning the financial sector, and given that many initiatives to support the financial sector were announced and/or implemented around the same time, it is challenging to separately identify the effects of the shorting ban. The end of the ban may be cleaner, as there are no major confounding policy initiatives, and there are fewer major headlines concerning the financial sector during

this period. Thus, in this subsection we examine the period surrounding the expiration of the shorting ban.

Figures 1 through 7 suggest that the market quality in affected stocks improves right away as the ban ends. To conduct statistical inference, we use the matched-pair daily panel regression framework. Specifically, we compare the last week of the shorting ban to each of the first three weeks following its expiration. Taking the expiration date to be event day zero, we compare the shorting ban interval $[-5, -1]$ to event day window $[0, 4]$, event-day window $[5, 9]$, and event-day window $[10, 14]$ by estimating specifications of the following form using data from event day -5 to $+14$ for matched pair i on day t :

$$Y_{it} = \alpha_i + \beta_1 D_{it}^{POST1} + \beta_2 D_{it}^{POST2} + \beta_3 D_{it}^{POST3} + \theta X_{it} + \varepsilon_{it}, \quad (6)$$

where Y_{it} is the shorting, activity, or market-quality measure Y for the banned stock less the analogous value for its nonbanned match; X_{it} is the same vector of controls as before; and D^{POST1} is an indicator variable set equal to one if and only if the data comes from the first week after the ban expires (event days 0 to 4), with D^{POST2} and D^{POST3} defined similarly for the second and third postban week (event days 5 to 9 and event days 10 to 14, respectively). Here, the matched-pair fixed effect is the conditional mean during the last week of the ban, so the coefficients on the postban indicator variables reflect changes from the last week of the ban to the relevant postban period.

The results are in Table 6 and formalize the graphical evidence from the figures. We focus on the results for the first week after the ban expires; the results for later periods are mainly included to check whether there are any lagged effects or temporary effects on shorting and market quality postban. For all but the smallest quartile of stocks, short selling in affected stocks rebounds immediately, and quoted and effective spreads on affected stocks narrow in the first week after the ban expires. In the large-cap quartile, for example, shorting as a fraction of volume increases by 12.36 percentage points in the first week postban, and proportional effective spreads narrow by 17 basis points. Price impacts also decline significantly for the top two size quartiles, though realized spreads narrow significantly only in quartile 2. In the large-cap quartile, short sellers become slightly less aggressive. Excluding the smallest quartile, spreads continue to narrow further in the second and third week postban.

In contrast, the range volatility measure does not decline immediately when the ban ends. As discussed earlier, this measure mixes market quality with fundamental volatility, and there are sharp daily moves in financial sector stock prices on end-of-ban event day 0 and event day $+1$. In fact, given that bid-ask spreads are positively related to the volatility of fundamentals, all else equal, a postban improvement in market quality in the presence of continued high volatility is even more notable. Overall, we find that our main conclusions hold when we limit our analysis to the expiration of the shorting ban.

Table 6
The end of the shorting ban

Dependent variable	Size quartile 1 (smallest) postban dummies			Size quartile 2 postban dummies		
	day [0,4]	day [5,9]	day [10,14]	day [0,4]	day [5,9]	day [10,14]
Shorting volume	140	734*	-51	968	-585	-99
Number of shorts	3	4*	3	9	19***	11
RELSS	0.0069	0.0332	0.0478*	0.0512***	0.0281***	-0.0139
DVOL	18,171**	23,987*	19,826**	46,389	66,097	51,965
# of trades	2	12**	8	16	31***	36***
RQS	0.0048	0.0020	0.0014	-0.0063**	-0.0045	-0.0098***
RES	0.0037	0.0002	-0.0044	-0.0048**	-0.0072**	-0.0080***
RPI5	-0.0038*	0.0006	-0.0010	0.0001	-0.0018**	-0.0009
RRS5	0.0114*	-0.0024	-0.0005	-0.0051***	-0.0062***	-0.0038***
RVOL	-0.0016	0.0109	0.0039	-0.0123	-0.0105	-0.0118
Shorting RES	0.0123	-0.0005	-0.0021	0.0026	0.0014	-0.0020
	Size quartile 3 postban dummies			Size quartile 4 (largest) postban dummies		
	day [0,4]	day [5,9]	day [10,14]	day [0,4]	day [5,9]	day [10,14]
Shorting volume	31,929***	25,344***	41,465***	955,558**	979,772***	1,168,607***
Number of shorts	213***	223***	309***	4362**	4865***	5156***
RELSS	0.1236***	0.1206***	0.1310***	0.1236***	0.1618***	0.1840***
DVOL	327,770	1,005,403*	1,443,186***	15,664,297	3,674,999	-4,026,798
Number of trades	196	352***	395***	2,128	1,899***	1,309
RQS	-0.0036**	-0.0054***	-0.0062***	-0.0020**	-0.0033***	-0.0035***
RES	-0.0027**	-0.0038***	-0.0044***	-0.0017**	-0.0027***	-0.0030***
RPI5	-0.0013*	-0.0020***	-0.0021***	-0.0009***	-0.0013***	-0.0014***
RRS5	0.0000	-0.0001	0.0002	0.0001	-0.0001***	-0.0001
RVOL	0.0109	-0.0160**	-0.0031	0.0121	-0.0240***	-0.0157***
Shorting RES	-0.0021	-0.0037***	-0.0018	-0.0020*	-0.0024***	-0.0027**

This table reports how key shorting activity, market quality, and shorting aggressiveness change around the end of the shorting ban for different size quartiles. Daily panel matched pair regressions with firm fixed effects using only data from day -5 to day +14 relative to the ban expiration date. Each sample stock subject to the shorting ban is matched to a similar stock for which shorting was not banned (see Table 2 for details). A postban dummy equals one if the observation is in the relevant five-day postban window (postban event days [0,4], [5,9], or [10,14]). Only coefficients on postban dummies are reported; unreported control variables include pairwise differences in market cap and daily dollar trading volume (DVOL) in \$millions, intraday price range (RVOL), and share price (VWAP). Shorting and trading volume measures aggregate BATS, NYSE, and NASDAQ activity during regular trading hours. RELSS is shorting volume divided by share volume. Dependent variables also 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. Other dependent variables are the actual relative effective spread accrued by executed short-sell orders (shorting RES), the five-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. We estimate these models separately for stock pairs in each market cap quartile. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, using standard errors clustered on both firm and date.

5.2 Existing shorting constraints

In this subsection, we examine whether the impact of the short sale ban is different for hard-to-borrow stocks. The main hypothesis is that for hard-to-borrow stocks, shorting is already somewhat restricted, so the ban should have a smaller effect on these stocks, all else equal. During September 2008, we have proprietary daily reports from a large prime broker listing stocks that are easy to borrow. Stocks not on this list are considered hard to borrow. Clients are expected to contact the prime broker before shorting hard-to-borrow stocks, to determine whether the prime broker can locate loanable shares. Most stocks are in fact easy to borrow: An average of around 5,300 stocks appear on this

list each day. Based on the twelve trading days from September 2 through September 17, we calculate the fraction of days on which a stock is hard to borrow. The hard-to-borrow measure for each matched pair is the difference between the banned stock and its nonbanned match, and the hard-to-borrow indicator variable D_i^{HTB} is set equal to one if and only if the hard-to-borrow measure for matched pair i is above the cross-sectional median.

To assess whether the shorting ban affects hard-to-borrow stocks differently, we augment the basic panel regression specification (1) with an interacted indicator variable:

$$Y_{it} = \alpha_i + \beta D_{it}^{BAN} + \delta D_{it}^{BAN} D_i^{HTB} + \theta X_{it} + \varepsilon_{it}, \quad (7)$$

and we report the results in Panel A of Table 7. For stocks in the top two size quartiles, hard-to-borrow stocks are significantly less affected by the shorting ban. There is less of a decline in the amount of shorting and less degradation in market quality measures. For example, in the large-cap quartile, effective spreads for financial stocks that are easy to borrow widen by thirty-eight basis points during the ban. The widening for hard-to-borrow stocks is only twenty-two basis points. In sum, the evidence supports the hypothesis that a tight share lending market already imposes restrictions on shorting, and thus the effect of the shorting ban is smaller (but still nonzero) for these stocks.¹⁰

5.3 Algorithmic trading

As mentioned earlier, we hypothesize that liquidity is damaged during the shorting ban because informal market makers are prohibited from shorting and are thus unable to effectively provide liquidity on one side of the market. If true, the effects of the shorting ban should be more severe for stocks for which informal market makers are relatively more important. We do not have data that separates out these market makers. However, informal market-making strategies are a subset of algorithmic or high-frequency (AT-HFT) strategies, and stocks with more AT-HFT could have a greater incidence of informal market making.

BATS was the third largest trading venue in the United States at the time of the shorting ban. It was a favored destination for AT-HFT because of its low latencies. Thus, we use BATS market share as a proxy for the prevalence of AT-HFT in a given stock. We hypothesize that stocks with a greater BATS market share are more likely to experience more informal market making across all trading venues, and we expect these stocks to fare worse in terms of liquidity during the shorting ban.

¹⁰ In unreported tests, we regress preban effective spreads on a hard-to-borrow dummy and controls for market cap, volatility, dollar volume, and VWAP. Hard-to-borrow has a significantly positive coefficient, whether we use cross-sectional or firm-day panels to estimate the model. This suggests that shorting constraints have a detrimental effect on liquidity even outside the ban period. This is also consistent with a ban-induced deterioration of liquidity, because the ban directly affects shorting difficulty.

Table 7
Differential ban effects

Panel A: Interaction of shorting ban with borrowing difficulty (HTB)

Dependent variable	Size quartile 1 (smallest)		Size quartile 2		Size quartile 3		Size quartile 4 (largest)	
	BAN	BAN*HTB	BAN	BAN*HTB	BAN	BAN*HTB	BAN	BAN*HTB
RELSS	-0.0312	0.0033	-0.0385***	0.0192	-0.1470***	0.0508***	-0.2103***	0.0519***
DVOL	-11.639	8.456	58.478	178.148**	-1,472.527***	2,368.369	-43,538.853***	2,396,517
RQS	-0.0091***	0.0126*	0.0052***	0.0043	0.0096***	-0.0058***	0.0047***	-0.0022***
RES	-0.0039	0.0116*	0.0056***	0.0041	0.0063***	-0.0029***	0.0038***	-0.0016***
RPIS	-0.0036***	0.0095***	0.0008	0.0022*	0.0062	-0.0007*	0.0017***	-0.0008***
RRSS	0.0040	-0.0082	0.0037***	-0.0001	0.0013***	-0.0015***	0.0004***	0.0000
RVOL	-0.0115**	-0.0065	0.0116**	-0.0073	0.0232	0.0047	0.0319***	0.0110
Shorting RES	0.0160***	-0.0135	0.0023	0.0001	0.0031***	-0.0006	0.0020***	-0.0012

Panel B: Interaction of shorting ban with high-frequency trading propensity (HFT)

Dependent variable	Size quartile 1 (smallest)		Size quartile 2		Size quartile 3		Size quartile 4 (largest)	
	BAN	BAN*HFT	BAN	BAN*HFT	BAN	BAN*HFT	BAN	BAN*HFT
RELSS	-0.0292*	-0.0192	-0.0140	-0.0691***	-0.1254***	-0.0142	-0.2217***	0.0235
DVOL	-5,680	-64,180	-11,319	-23,924	-679,799***	-382,258	1,774,409	-47,493,179
RQS	-0.0054	0.0167	0.0057***	0.0016	0.0078***	0.0007	0.0063***	-0.0022
RES	-0.0009	0.0245***	0.0066***	-0.0004	0.0054***	0.0004	0.0045***	-0.0011
RPIS	-0.0005	0.0040	0.0010	0.0011	0.0024***	-0.0001	0.0017***	-0.0002
RRSS	0.0002	0.0204	0.0044***	-0.0024	0.0006	0.0005	0.0011*	-0.0007
RVOL	-0.0146**	0.0188	0.0053	0.0157*	0.0234**	0.0010	0.0172	0.0180
Shorting RES	0.0146	-0.0163	0.0010	0.0032	0.0026	0.0004	0.0035*	-0.0019

This table reports how shorting activity, market quality, and shorting aggressiveness change when a shorting ban interacts with borrowing difficulty and high-frequency trading. Using a daily panel of matched stock pairs from 8/1/2008 to 10/31/2008, we estimate firm fixed effect regressions of various short-selling activity and market-quality measures on a ban dummy, an interaction of the ban dummy with a dummy variable representing borrowing difficulty (Panel A) or high-frequency-trading propensity (Panel B), and other explanatory variables. The borrowing difficulty is based on proprietary daily reports from a large prime brokerage listing stocks that are easy to borrow. For each stock, we measure the percentage of days on which it was hard to borrow (not on the list). The dummy representing high-frequency-trading propensity is based on the market share of BATS between 8/1/2008 and 9/12/2008. For both variables, we then compute the difference between ban firms and matched control stocks. Finally, we create a dummy that is one if this difference is above the cross-sectional median and interact the resulting variable with the ban dummy. Each sample stock subject to the shorting ban is matched to a similar stock for which shorting was not banned (see Table 2 for details). Separate regressions are estimated for quartile subsamples based on year-end 2007 market cap. The dependent variable in each regression is the relevant difference between the banned stock and its match. The ban dummy equals one on stock days for which short selling is banned in the relevant stock and is zero otherwise. Dependent variables include time-weighted relative quoted spreads (RQS), trade-weighted relative effective spreads (RES), equal-weighted five-minute price impacts (RPIS), and five-minute realized spreads (RRS5), each of which 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. RELSS is shorting volume divided by share volume. Dollar volume refers to aggregate daily trading activity on BATS, NYSE, and NASDAQ. Shorting RES is the average effective spread received by executed short sell orders. Reported control variables include pairwise differences in market cap and daily dollar trading volume (DVOL) in \$millions, intraday price range (RVOL), and share price (VWAP). Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, using standard errors clustered by both firm and date.

The methodological approach is quite similar to that used in the hard-to-borrow analysis. We compute AT-HFT intensity each day for each sample stock as BATS trading volume over total market volume during August 2008. The AT-HFT intensity measure for each matched pair is the difference between the banned stock and its nonbanned match, and the AT-HFT indicator variable $D_i^{\text{AT-HFT}}$ is set equal to one if and only if the AT-HFT measure for matched pair i during August 2008 is above the cross-sectional median. We then interact the shorting ban dummy with the AT-HFT indicator in the daily matched pair panel regressions:

$$Y_{it} = \alpha_i + \beta D_{it}^{\text{BAN}} + \delta D_{it}^{\text{BAN}} D_i^{\text{AT-HFT}} + \theta X_{it} + \varepsilon_{it}, \quad (8)$$

and we test whether $\delta=0$. We estimate separate regressions for each of the market-cap quartiles, and the results are in Panel B of Table 7.

Almost none of the coefficients on the interaction variable are significantly different from zero. During the ban, stocks with more AT-HFT see similar drops in shorting activity and similar spread widening. It could be that informal market making is not the key mechanism underpinning the earlier results. It could be the case that BATS market share in a given stock is not a good proxy for the overall importance of informal market making across all trading venues. Finally, if there is little cross-sectional variation in the importance of informal market making, after controlling for observables, such as market cap and trading volume, this could also explain the insignificant regression results. Unfortunately, a more definitive test of our hypothesized mechanism may await better, more detailed data on the identities and activities of certain groups of high-frequency traders.¹¹

6. 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 data from the NYSE, NASDAQ, and BATS, we show that shorting activity drops by an average of 77% in affected large-cap stocks. The remaining short sellers (who should be exclusively market makers) tend to demand immediacy, probably to quickly satisfy their hedging needs. Small-cap stocks were largely unaffected, but large-cap stocks subject to the ban suffered a severe degradation in market quality, as measured by quoted and effective spreads, price impacts, and realized spreads. Market quality is less affected for stocks with pre-existing shorting restrictions. Banned stocks jump in price, but this appears to be due to the anticipated bailout programs rather than the shorting ban.

Market quality worsens because many algorithmic traders cannot act as informal market makers during the shorting ban. With less competition in

¹¹ Using a similar test design as for hard-to-borrow and high-frequency-trading stocks, we also test whether institutional ownership or option listing status affect the effect of the ban on market quality. The interaction coefficients tend to have high standard errors, and the analysis does not add significant insights to the tabulated results in Table 7.

liquidity provision, the remaining “formal” market makers are able to earn greater rents at the expense of those demanding liquidity.

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? Although 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. 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, and starting a Diamond-Dybvig (1983) bank run could theoretically lead to a share price of zero. However, Khanna and Mathews (2012) emphasize that the firm’s shareholders have every incentive to counter actions taken by manipulative short sellers. 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 to reduce systemic risk. Nevertheless, these somewhat elusive benefits would need to be quite large to offset the large costs that market participants experienced in terms of larger transactions costs and elevated volatility.

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