

PRICE INFLATION AND WEALTH TRANSFER DURING THE 2008 SEC SHORT-SALE BAN

Lawrence E. Harris^a, Ethan Namvar^b and Blake Phillips^{c,*}

We estimate that the ban on short-selling financial stocks imposed by the SEC in September 2008 led to price inflation of 10–12% in the banned stocks based on a factor-analytic model that extracts common valuation information from the prices of stocks that were not banned. This inflation reversed approximately two weeks after the ban for stocks with negative pre-ban performance. In contrast, similar magnitude price inflation was sustained following the ban for stocks with positive pre-ban performance, suggesting that the ban was successful in stabilizing prices for these stocks. Cross-sectional analysis reveals that inflation was isolated to stocks without traded options, suggesting that option markets provided a mechanism for traders to circumnavigate the ban. Further, we find that the level and change in short interest associated with the ban are unrelated to the level of inflation. These results suggest that price pressure associated with closing short positions at the start of the ban is unrelated to the noted price inflation. If prices were inflated, buyers paid more than they otherwise would have for the banned stocks during the period of the ban. We provide a conservative estimate of \$2.3 to \$4.9 billion for the resulting wealth transfer from buyers to sellers, depending on how post-ban reversal evidence is interpreted. Such transfers should interest policymakers concerned with maintaining fair markets.



^aUSC Marshall School of Business, University of Southern California, Los Angeles, CA 90089, USA.

^bHaas School of Business, University of California, Berkeley, CA 94720, USA.

^cUniversity of Waterloo, School of Accounting and Finance, Ontario, Canada, N2L 3G1.

*A portion of this work was completed while Phillips was at the University of Alberta, School of Business.

1 Introduction

In response to the financial crisis of 2007–2009, financial regulators around the world responded by imposing bans on short-selling financial sector stocks. Their objective was to restore market equilibrium, stabilize prices, and provide a disincentive to the dissemination of false rumors

seen as contributing to price spirals.¹ These short-sale bans create a unique opportunity to analyze the effects of short-selling in financial markets as they represent a time-series discontinuity in trading rules, whereas most prior research examines cross-sectional effects of short-selling bans.² The analysis of ban effects is important from a policy perspective for market regulators who are interested in their efficacy and collateral effects. In this paper, we focus on the stated objective of restoring market equilibrium and examine the effect of the short-sale ban in the U.S. on short-selling and the prices of the banned stocks.

Identification of short-sale ban effects during the financial crisis faces a series of challenges. First, the ban occurred during an extraordinary time period that coincided with significant market uncertainty and overlapping confounding effects. Second, the ban focused on financial sector stocks which were at the center of the crisis. Several authors have examined these issues, attempting to isolate the effect of the ban from endogenous influences. For example, Beber and Pagano (2011) examine the effect of the short-sale ban on prices for 30 countries and try to isolate ban effects by comparing post-ban, median cumulative, excess returns for countries subject to bans with those exempt. They also analyze individual stocks, benchmarking stock returns relative to the broad index for each country. A concern with this approach is that risk factor sensitivities likely vary fundamentally between countries and between individual stocks and their respective country indices. For example, many of the largest and most developed financial markets enacted bans (e.g., the U.S., U.K., Germany, Japan, and Canada) raising concerns regarding the quality of risk factor matches between ban and control countries.³ If risk factor sensitivities vary between the sample and control stocks, cumulative return differences may reflect time variation in risk factors and not short-sale ban effects.

Our contribution to this literature is the use of a factor-analytic approach to estimate the market values that would have been observed for the banned stocks had the ban not been imposed. In the first stage, we estimate stock-specific, risk factor loadings over the year preceding the ban, for both short-sale banned and not-banned stocks. In the second stage, using only the not-banned stocks, we estimate daily aggregate factor loadings utilizing the first-stage factor estimates. Use of only the not-banned stocks in the second stage allows us to estimate counterfactual aggregate factor loadings that would have been observed in the absence of the ban. We then use the counterfactual aggregate factor loadings to obtain predicted daily returns for the banned stocks based on their cross-sectional differences.

This approach has several advantages over the control sample methods used in other studies. First, stock-level risk factor loadings are used to generate predicted returns, thus mitigating the potential for risk factor sensitivity disparity in a control sample to bias our results. Second, we are able to include unique risk factors to address specific, potentially confounding, simultaneous events. For example, in addition to the three Fama–French (1992) and the Carhart (1997) momentum factors, which form the foundation of our first stage, we include banned stock and Troubled Asset Relief Program (TARP) factors. The banned stock factor captures crisis risk factors unique to the Fama–French and Carhart factors. The TARP factor captures potential inflation which may be attributable to investors speculating on firms expected to receive funding under the TARP legislation that the U.S. Congress was debating during the period of the ban. During model validation we find that both of these factors are priced and add incremental accuracy to our model, in addition to the commonly considered four-factor model.

Finally, by estimating the model, before, during, and after the ban, we are able to validate the accuracy of the model in the periods surrounding the ban, giving us greater confidence that any noted price effects can accurately be attributed to the ban. However, the factor-analytic model does have limitations. For example, in aggregate, the factor betas we estimate in the first stage must be reasonably consistent over the time frame of analysis. Second, although the magnitude of factor sensitivity may vary between the banned and not-banned samples, the sensitivities must be a linear extension of each other. We validate the model before and after the ban, benchmarking actual and predicted banned stock returns, and find that the model is highly accurate in both periods. For example, the correlation between the predicted and actual means in the pre- and post-ban periods is 0.98 and 0.96, respectively, and the t -statistics for equality of means are 0.37 and 0.32. These results confirm the suitability of the model design and indicate aggregate factor sensitivity consistency. If factor loadings were not reasonably consistent across the estimation period, a decline in model accuracy would have been noted in the post-ban period. Details of our model and the validation tests are presented in greater detail below.

We focus our analysis on the U.S. as in this market the effect of the short-sale ban is most unresolved. The U.S. is unique, being the only country for which price correction was not noted following the ban, potentially reflecting the influences of TARP legislation (Beber and Pagano, 2011). Given the size and position of the U.S. in global financial markets, it also is perhaps the most difficult market to accurately benchmark.

Our results suggest that, during the short-sale ban, the stock prices of financial sector firms were inflated by approximately 10–12%, depending on the weights used to compute benchmark returns.

Cross-sectional analysis suggests that the noted inflation was more marked for nonoptionable stocks. As option market makers were exempt from the ban, option markets served as a potential mechanism for investors to circumnavigate the ban by purchasing put options. We find that price effects of the ban on optionable stocks were negligible. Our results suggest that options provided an effective substitute for direct short-sales during the ban and consequently, the options exchanges likely benefited from the ban via increased or more sustained transactions revenue.

We also examine the role of short interest in ban effects. Although the ban did not require the termination of existing short positions, analysis of mean trends surrounding the ban reveals that short interest dropped by approximately 50% coincident with the ban. Thus, the inflation we document may have resulted from buying pressure as short-sellers closed and covered positions. Perhaps surprisingly, we find that neither the pre-ban short interest level nor the change in short interest associated with the ban is predictive of the magnitude of inflation.

Potentially of greatest interest to policymakers is the sustainability of ban effects. In the post-ban period we find limited evidence of a reversal of the noted inflation in the aggregate banned stock subsample. In aggregate, it required two months for the estimated inflation to correct, a time frame inconsistent with a post-ban reversal of prices. The ban was applied to a broad set of stocks based on SIC codes, with no attempt made to specifically target stocks under short-sale pressure. In the year preceding the ban, on average, banned stocks lost 30% of total value but pre-ban losses were not pervasive, approximately half of stocks in the ban sample experienced positive pre-ban performance in the six months preceding the ban. During the ban, both the broad market index and the banned stock index continued to decline,

reflecting predominantly negative information revealed during this period and perhaps also an increase in aggregate investor risk aversion. To allow a more detailed analysis of ban effects and post-ban sustainability, we sort the banned stock subsample by return in the six months preceding the ban, as a proxy for aggregate crisis risk factor sensitivity. As short-sale constraints impede negative information from being impounded in prices, we hypothesize that banned stocks with greater sensitivity to aggregate crisis risk factors would realize greater inflation. Surprisingly, we find that the magnitude of inflation is similar for the two subsamples, but for the negative return subsample, inflation resulting from the ban is reversed within two weeks of the end of the ban. For the positive performing subsample, prices remained inflated until at least the end of 2008.

If financial stocks were indeed overvalued, or if they were merely properly valued before the ban, the ban on short-selling had a potentially significant unintended consequence. By preventing short-sellers from trading, the SEC created a bias toward higher prices. The unintended consequence of this bias is that many buyers bought at prices above fundamental value. These buyers incurred significant losses when prices ultimately adjusted downward toward their true, intrinsic values.

Anecdotal evidence suggests that this scenario indeed occurred. Before the September 2008 ban on short-selling, Freddie Mac (FRE) and Fannie Mae (FNM) common shares were trading nearly 30 cents and 50 cents, respectively. During the ban, their shares rose to nearly \$2.00 per share. Following the end of the ban, the share prices of both firms soon returned to approximately 60 cents per share. If the ban inflated FRE and FNM share prices by preventing short-sellers from supplying liquidity to an imbalance of buyers, then buyers traded at artificially high prices.

For long sellers, the ban on short-selling provided an unexpected windfall. We estimate that during the period of the ban, inflation transferred \$597M from buyers to sellers in the shares of FRE and FNM. Depending on how the reversal evidence is interpreted, we estimate that buyers transferred \$2.3 to \$4.9 billion more to sellers than they would have had the SEC not imposed the ban.

The remainder of this paper is organized as follows. Section 2 provides an overview of the related literature. We describe the data used in the analysis in Section 3, and introduce our analytic methods in Section 4. Discussion of potential endogeneity biases appears in Section 5, our inflation estimation results appear in Section 6, and our analysis of post-ban reversals and wealth transfers between buyers and sellers appears in Section 7. In Section 8 we conclude.

2 Related literature

The effect of short-sale constraints on market efficiency is well documented in the finance literature. Early theoretical work by Miller (1977) argued that short-sale constraints exclude pessimistic investors from the market. Thus, a subset of value opinions is excluded from the cross-section of opinions which converge to form prices, resulting in an upward, optimistic bias in short-sale constrained stock prices. Diamond and Verrecchia (1987) extended the theoretical work of Miller, arguing in a rational framework that option introduction provides the opportunity for pessimistic investors to realize synthetic, short positions, which could potentially mitigate short-sale constraints. In support of this theory, Phillips (2011) finds that option introduction mitigates 79% of the price efficiency disparity between short-sale constrained and unconstrained stocks in relation to negative information. But, the empirical evidence on the potential for options to mitigate short-sale constraints is not conclusive (for example, see Bris *et al.*, 2007 below).

In aggregate, the majority of empirical analyses finds that short-sale constraints contribute to overpricing and a reduction in market quality and efficiency.⁴ Our analysis relates most closely to the literature focusing on aggregate market effects of short-selling and short-sale constraints. For example, Bris *et al.* (2007) analyze cross-sectional and time-series information from 46 equity markets and find that short-sale restrictions do not have noticeable effects at the individual stock level and find the effect of put options to be insignificant in the presence of short-selling restrictions. On the other hand, they find that markets with active short-sellers are informationally more efficient than markets without significant short-selling. Charoenrook and Daouk (2005) examine 111 countries to determine the effect of market-wide short-sale restrictions on value-weighted market returns. They find that index returns are less volatile and markets are more liquid when short-sales are allowed.

More recently, a new literature has emerged that examines actions taken in 2008 by the SEC intended to mitigate the effects of short-sales on the market. Boulton and Braga-Alves (2010) analyze the 2008 SEC ban on naked short-sales and find that the ban had an adverse effect on liquidity and price informativeness. Boehmer *et al.* (2009) find that during the 2008 short-sale ban in the U.S. shorting activity dropped by approximately 65% and that stocks subject to the ban suffered a degradation in market quality as measured by spreads, price impact, and intraday volatility. As previously discussed, Beber and Pagano (2011) examine short-sale bans in 30 countries between 2007 and 2009 and find that the bans were detrimental for liquidity, slowed price discovery, and failed to support all studied stock prices with the possible exception of U.S. financial stocks. Beber and Pagano explain their results by suggesting that TARP activities may have slowed or confounded identification of a correction within U.S. markets.

In contrast to this literature, which focuses mostly on market quality issues and limits analysis of price inflation to excess stock returns, we use a more sophisticated model that allows a detailed and rigorous analysis of counterfactual prices for the banned stocks had the ban not been enacted in the U.S. Through this process, we seek to isolate the effects of the ban from potentially confounding events, such as TARP, to provide direct estimates of the magnitude and cost of the inflation to buyers. This calculation is of obvious importance to the debate about whether the ban was sensible.

3 Data

Our sample includes all stocks listed on the New York Stock Exchange (NYSE), the American Stock Exchange (AMEX), and the National Association of Securities Dealers Automated Quotations (NASDAQ) stock exchanges between September 18, 2007 and December 31, 2008. We divided the sample into three subperiods: the *pre-ban period* (September 18, 2007 to September 18, 2008), the *ban period* (September 19 to October 8, 2008), and the *post-ban period* (October 9 to December 31, 2008). In total, the SEC placed 987 stocks on the banned list, 88% of which were included on the original list released on September 19. An additional 10% were added on September 22 and 23, and the remaining 2% were added between September 24 and as late as October 7.⁵

We obtain stock price, volume, and shares outstanding data from the Center for Research in Security Prices (CRSP) database, and short interest data from the Short Squeeze database.⁶ The CRSP dataset includes 7,639 stocks in our sample period. We exclude all stocks with an incomplete data record (1,733 securities), all stocks with market capitalization less than \$50 million on September 18, 2008 (1,067 securities), and all

stocks for which trading volume exceeded five times shares outstanding on any given day in the sample (five securities).⁷ We also exclude stocks for which inclusion on the SEC short-sale ban list is ambiguous, including stocks added and subsequently deleted at the request of the firm (10 securities), or securities added after September 26, 2008 (10 securities). Finally, we exclude four stocks for which short interest data are missing from the Short Squeeze database. The resulting sample includes 4,810 stocks, 676 of which appeared on the SEC ban list and 127 of which received TARP funds between October 28, 2008 and December 31, 2008. The returns analyzed in this study are dividend- and split-adjusted log price relatives.

Figure 1 plots cumulative return indices over the 15-month sample period. On a value-weighted basis, not-banned and banned stocks lost 8% and 30% of value, respectively, in the pre-ban period. The banned and not-banned subsamples lost an additional 18% and 14% of market value during the ban, cumulating in total losses of 32% and

54%, respectively, over our 15-month sample. Losses were greater for banned stocks for which a substantial fraction of their float was sold short as of September 15, 2008. Over the entire sample period, the short interest-weighted banned index lost 67% of market value.⁸ Finally, to provide a sense of aggregate performance of firms which received TARP funds, we report index returns weighted by the fraction of each firm's October 28, 2008 common stock market capitalization that it received in TARP funds. The TARP index decreased by 68% over our sample period, reflecting that companies which received TARP funds were, on average, more financially distressed.

Figure 2 reports bimonthly, mean short interest for the not-banned and banned stocks in 2008. The reported means are weighted by market capitalization and by the fraction of float sold short on September 15, 2008 to make the trends comparable with the corresponding value and short interest-weighted index returns shown in Figure 1. Both weighting methods produce similar results. From January through June, short

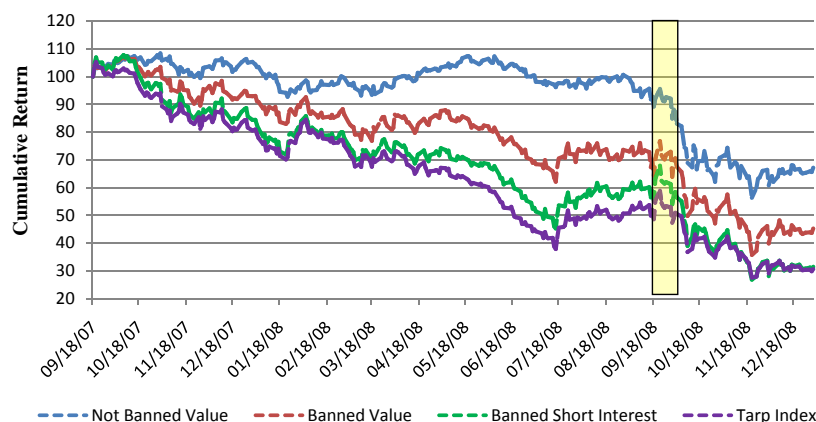


Figure 1 Cumulative index returns. The figure summarizes cumulative index returns to NYSE, AMEX, and NASDAQ stocks sorted by inclusion on the SEC short-sale ban list between September 19, 2008 and October 8, 2008. The figure displays value-weighted cumulative index returns for the banned and not-banned subsamples. We also report cumulative banned stock index returns weighted by short interest on September 15, 2008 and by TARP funds received in 2008 as a fraction of market capitalization on October 28, 2008. We calculate all returns as dividend- and split-adjusted log price relatives. The short-sale ban period is shaded.

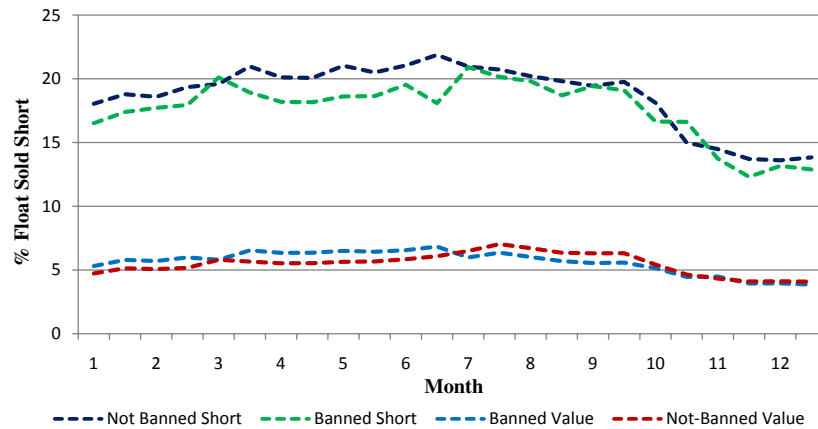


Figure 2 Mean short interest. The figure plots mean short interest for the not-banned and banned stock subsamples between January 15, 2008 and December 31, 2008, where short interest is defined as the percentage of float sold short and not repurchased. Value and short interest-weighted means are reported, where the short interest weight is the percentage of float sold short. Stocks with missing float data in the Short Squeeze database are excluded.

interest gradually increased for both banned and not-banned stocks. Short interest then rapidly declined in the second half of the year as short-sellers closed positions. Several processes explain these results. On the demand side, short-sellers may have believed prices had run their course and covered their positions and financing issues may have caused them to reduce their leverage. On the supply side, stock lenders concerned about the integrity of their collateral funds were withdrawing shares from the lending market as were those lenders who were selling stock. Finally, the short-sale ban also contributed to the decline in short interest following its imposition.

Visual inspection of the cumulative index returns in Figure 1 suggests that the short-sale ban had a limited effect on arresting the decline in value of the banned (primarily financial sector) stocks. In fact, stock value declines during the ban, for both not-banned and banned stocks, were more rapid than any other equivalent time span in the pre- or post-ban periods. The remainder of this paper examines prices during and around the ban period in greater detail.

4 The factor-analytic model

We use a factor-analytic approach to estimate counterfactual market values that would have been observed for the banned stocks had the SEC not imposed the short-sale ban. To do so, we use the information in the prices of the not-banned stock returns to project returns for the banned stocks. Our method is a two-stage process. In the first stage, for each stock, over the year before the short-sale ban, we estimate factor loadings associated with the three Fama–French factors (Fama and French, 1992), the momentum factor (Carhart, 1997), the value-weighted banned stock index, and the TARP index using the following time-series regression:

$$r_{i,t} = \alpha_i + \beta_{1,i}exMkt_t + \beta_{2,i}SMB_t + \beta_{3,i}HML_t + \beta_{4,i}MOM_t + \beta_{5,i}BAN_t + \beta_{6,i}TARP_t + \varepsilon_{i,t}, \quad (1)$$

where $r_{i,t}$ is the dividend- and split-adjusted log price relative for stock i on day t . $exMkt$, SMB , HML , and MOM are the Fama–French and momentum factors, BAN is the value-weighted

return to the banned stocks, and *TARP* is the previously defined TARP-weighted return to the banned stocks. This regression identifies factor loadings for six market-based risk factors for each stock in the sample. Factor loadings on the variable *BAN* will help identify risk factors unique to the crisis as crisis effects are most pronounced in these stocks. Loadings on the *TARP* variable will help identify the effect, if any, that optimism about the passage of the TARP legislation may have had on the banned stock returns.

Table 1 reports the average coefficient value for each of the six market factors calculated for the entire sample, and separately for the banned and not-banned subsamples, in the pre-ban period.

The explanatory power of the Fama–French and the Carhart momentum factors in relation to the cross-section of returns is well documented. Thus, we undertake this comparison primarily to investigate the potential incremental explanatory power of the *BAN* and *TARP* factors. When the full sample is examined, for both the market capitalization and short interest weighting schemes, the *BAN* and *TARP* factors are highly significant (p -values < 0.0001 , for both factors, in both models). When the banned and not-banned samples are examined separately, we see slightly contrasting results. Utilizing the value-weighting scheme, the *TARP* factor has limited additional explanatory power for the not-banned subsample, but the short interested-weighted results are consistent with

Table 1 Factor-analytic model average factor loadings.

Factor	Full sample		Banned		Not-banned	
	Value	Short	Value	Short	Value	Short
ExMkt	0.0096 (70.89)	0.0092 (88.95)	0.0018 (5.79)	0.0031 (9.80)	0.011 (78.10)	0.010 (101.26)
HML	−0.00006 (0.74)	−0.00016 (2.02)	0.00010 (0.43)	0.00043 (1.75)	−0.00009 (1.02)	−0.00025 (3.15)
SMB	0.0041 (35.66)	0.0046 (42.82)	0.0048 (15.02)	0.0015 (4.84)	0.0040 (32.39)	0.0052 (45.25)
MOM	−0.00066 (7.73)	−0.00044 (5.10)	0.00031 (1.33)	−0.00040 (2.22)	−0.00082 (8.92)	−0.00045 (4.65)
BAN	−0.081 (7.97)	−0.064 (6.75)	0.389 (12.65)	0.450 (11.53)	−0.158 (15.45)	−0.148 (17.94)
TARP	0.042 (8.62)	0.068 (11.02)	0.296 (15.35)	0.084 (3.33)	0.00062 (0.14)	0.065 (11.13)

Table reports the average coefficient values of the following six-factor model (Equation (1)), calculated over the pre-ban period (September 18, 2007 to September 18, 2008). Subsample averages are also reported for the stocks included (banned) and not included (not-banned) in the short-sale ban.

$$r_{i,t} = \alpha_i + \beta_{1,i}exMkt_t + \beta_{2,i}SMB_t + \beta_{3,i}HML_t + \beta_{4,i}MOM_t + \beta_{5,i}BAN_t + \beta_{6,i}TARP_t + \varepsilon_{i,t}$$

In this model, $r_{i,t}$ is the dividend- and split-adjusted log price relative for stock i on day t . *ExMkt*, *HML*, *SMB*, and *MOM* are the Fama–French and momentum factors on day t , *BAN* is the value-weighted return to the banned stocks on day t , and *TARP* is the TARP-weighted return to the banned stocks on day t . Two forms of the *BAN* index are calculated, a market capitalization weighted index (Value columns) and a short interest as of September 15, 2008 weighted index (Short columns). t -Statistics are reported in brackets below each coefficient ($H_0: \beta_n = 0$).

the full sample. Across all model specifications, the *BAN* factor has significant incremental power beyond the typical four-factor model to explain the cross-section of stock returns.

In the second stage, we estimate a cross-sectional return model for each day in the sample period utilizing the market-based risk factor loadings from the first stage as regressors. In addition, we also include three stock characteristics (inverse price, turnover, and volatility) to better identify how stock prices varied in the cross-section.⁹ Our cross-sectional model is given by:

$$r_{i,t} = \gamma_i + \delta_{1,i}\beta_{1,i} + \delta_{2,i}\beta_{2,i} + \delta_{3,i}\beta_{3,i} + \delta_{4,i}\beta_{4,i} \\ + \delta_{5,i}\beta_{5,i} + \delta_{6,i}InvP_{i,t} + \delta_{7,i}TURN_{i,t} \\ + \delta_{8,i}VOLAT_{i,t} + v_{i,t}, \quad (2)$$

where $r_{i,t}$ and β_1 through β_6 are as described above and *InvP* is the daily inverse price, *TURN* is aggregate trading volume over the previous 10 trading days divided by shares outstanding, and *VOLAT* is the root mean squared return over the previous 10 trading days. As in the first stage, we calculate the average coefficient values for the three additional factors included in the second stage (*InvP*, *TURN*, and *VOLAT*). Across the various model specifications, *p*-values are typically < 0.0001 , 0.10 , and > 0.20 for *InvP*, *TURN*, and *VOLAT*, respectively. This analysis suggests that *InvP* and *TURN* have incremental explanatory power within the model but the incremental improvement in the model via the addition of *VOLAT* may be limited. We discuss the predictive accuracy of each of the different model variants in greater detail below.

We estimate the factor model using only the not-banned stocks as we want to capture aggregate factor loadings which would have been realized in the absence of the ban. We weight the cross-sectional model by value (market capitalization) to give greater weight to stocks for which we believe that market prices are most accurate and

are economically most significant. The coefficients are estimates of the realized factor returns associated with each of the regressors, based only on information in the returns to the not-banned stocks. We then use these factor estimates to obtain predicted daily returns for the banned stocks based on their cross-sectional characteristics. Finally, we aggregate the daily return estimates for each banned stock to produce a value-weighted index of the prices that we estimate would have been observed in the absence of the short-sale ban.

5 Model validation and endogeneity controls

5.1 Model suitability and stability

As previously discussed, the factor-analytic model has two potential limitations. First, *in aggregate*, the factor betas we estimate in the first stage must be reasonably consistent over the time frame of analysis. Second, although the magnitude of factor sensitivity may vary between the banned and not-banned samples, the sensitivities must be a linear extension of each other. We address both of these concerns by conducting model validation tests before and after the ban. As the ban was unexpected, the pre-ban period provides an unbiased time frame to validate overall model accuracy. We then contrast model accuracy before and after the ban to ensure that the factor betas we estimate in the pre-ban period, in aggregate, are still representative in the post-ban period.

We use three methods to measure predictive accuracy: (1) the correlation between estimated and actual mean returns, (2) paired *t*-tests between mean estimated and actual daily returns, and (3) the correlation between actual factor return values and those estimated with equation (2). We examine these measures for four different specifications of our basic model, varying exclusion and inclusion of the HML, SMB, and MOM

factors and the three stock characteristics (InvP, TURN, and VOLAT). We considered different specifications to determine to what extent our results depend on our assumptions, and to try to find a parsimonious model that could accurately estimate stock returns in the absence of the short-sale ban. For those cross-sectional models that only use three return factors, we obtained their factor loadings from time-series regressions that included only those three factors.

Based on our three accuracy measures (Table 2), all four models perform very well. In the pre-ban period, the correlation between the actual and estimated daily value-weighted banned stock index returns (based on the factor returns implied from the not-banned stocks) is above 0.92 for the two models with three return factors and is above 0.98 for the two models with six return factors. Inclusion of the three stock characteristics does not appreciably increase these correlations. The means of the daily actual and estimated banned stock index returns in the pre-ban period are statistically indistinguishable for all four-model specifications (t -statistics for the paired t -test range from 0.06 to 0.47). These results indicate that our methods are not producing significant drift in the return estimates that would bias our return inflation estimates.

Panel B of Table 2 presents correlations between the daily estimates of the six return factors and their corresponding actual factor values.¹⁰ These correlations are all above 0.90 in the pre-ban period, with correlations for the most critical factors (market, banned stock index, and TARP) all above 0.96 in the six return factor models. The correlations are lower for the three-factor models, which suggest that the additional factor structure increases estimation accuracy. The addition of the three stock characteristic factors does not appreciably affect the estimation of the return factor values, most probably because they

convey orthogonal information. The correlations are all lower (though still generally quite high) in the post-ban period, probably due to greater volatility and possibly due to the smaller sample period.

The evidence from these analyses suggests that the six return, three stock characteristic factor model (as described in Equations (1) and (2)) is the most accurate model of the four models we examine and we therefore rely on it in the remainder of this paper.¹¹ Visual evidence of the high correlation between the actual and estimated banned index returns is shown in Figure 3, which plots cumulatives of the actual and estimated index over the pre-ban period.

The root mean squared difference between the actual and estimated banned stock index returns in the year before the ban is 0.20%, and the first-order autocorrelation of these differences is 0.013. The low serial correlation and the essentially zero mean difference documented above indicate that the predicted variance of the cumulative differences will be approximately equal to the length of the accumulation period multiplied by the mean squared difference. We will use this result (and others) to make inferences about the significance of any inflation that we observe during and following the ban.

5.2 *Endogeneity controls*

It is important to recognize that our model estimates inflation relative to the not-banned sample, thus only endogenous events unique to either the banned or not-banned sample have the potential to bias our estimates. Given the diversity of stocks within the not-banned sample, it is highly unlikely that a risk factor idiosyncratic to that sample exists. The short-sale ban stock list was formed based on broad SIC codes, which mitigates the likelihood of unique, crisis-based risk

Table 2 Factor-analytic model return estimate accuracy.

Panel A: Index return accuracy

Model	Correlation coefficient		Equality of means	
	Pre	Post	Pre	Post
Three return factor model	0.9274	0.9340	0.37	0.47
Three return factor model with three stock characteristic factors	0.9306	0.9335	0.08	0.09
Six return factor model	0.9824	0.9640	0.19	0.06
Six return factor model with three stock characteristic factors	0.9829	0.9606	0.37	0.32

Panel B: Factor return accuracy

Model	Correlation coefficients					
	ExMkt	HML	SMB	MOM	BAN	TARP
<i>Pre Period (N = 254)</i>						
Three return factor model	0.9716	—	—	—	0.9255	0.9075
Three return factor model with three stock characteristic factors	0.9738	—	—	—	0.9247	0.9038
Six return factor model	0.9789	0.9164	0.9013	0.9519	0.9773	0.9688
Six return factor model with three stock characteristic factors	0.9819	0.9171	0.8859	0.9502	0.9778	0.9664
<i>Post period (N = 58)</i>						
Three return factor model	0.8970	—	—	—	0.8434	0.7542
Three return factor model with three stock characteristic factors	0.8767	—	—	—	0.8190	0.6948
Six return factor model	0.9272	0.6314	0.8717	0.4903	0.9139	0.8522
Six return factor model with three stock characteristic factors	0.9167	0.6560	0.8571	0.4360	0.9041	0.8219

Table reports three measures of the predictive accuracy of the factor-analytic model for the banned subset of stocks in the pre-ban period (September 18, 2007 to September 18, 2008) and the post-ban period (October 9, 2008 to December 31, 2008). The first measure is the correlation coefficient between actual and estimated value-weighted index returns. The estimated value-weighted returns are computed from the estimates of daily cross-sectional models that decompose the returns of the not-banned stocks into common factors. The second measure is the t -statistic for the paired t -test of the equality of the daily mean returns. The third measure is the correlation coefficient between the factor returns estimated in the cross-sectional model (Equation (2)) and the actual values of those factors. Results are presented for four model specifications. The three return factor models include the excess market (*ExMkt*), the TARP (*TARP*), and the banned stock (*Ban*) index returns. *BAN* is the value-weighted index return to the banned stocks on day t . *TARP* is the index return to the banned stocks weighted by TARP funds received in 2008 standardized by common stock market capitalization. The six return factor models are augmented to include the Fama–French size (*SMB*) and value (*HML*) factors as well as the Carhart momentum factor (*MOM*). The models are estimated separately including and excluding three stock characteristics: inverse price, turnover calculated as the sum of trading volume over the last 10 trading days divided by shares outstanding, and volatility is calculated as the square root of mean squared returns over the prior 10 trading days.

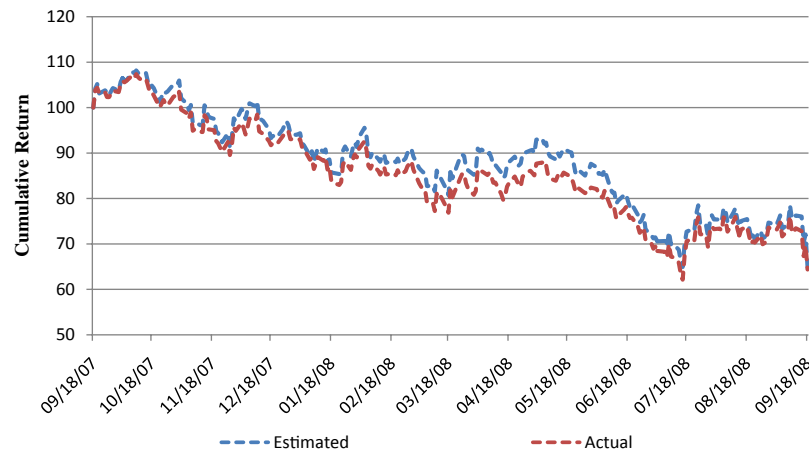


Figure 3 Actual and estimated cumulative banned index returns in the pre-ban period. The figure plots value-weighted cumulative indices of actual returns and corresponding returns estimated from the factor-analytic model, in the pre-ban period, for the banned stock subsample. Estimated returns are computed using the six return factor model with three stock characteristic factors presented in Equation (2).

factors to this subsample (in contrast to specifically banning short-sales for stocks experiencing abnormal short pressure). Regardless, the focus of the ban on financial sector stocks creates the risk of idiosyncratic risk influences, most prominently perhaps is TARP (which we discuss in more detail below).

A natural endogeneity control, implicit in the model, is the unique influence of options on short-sale constraints. As previously discussed, option market makers were exempt from the ban and investors could, in theory, circumnavigate the ban by trading put options. In support of this hypothesis, Berber and Pegano (2011) find that ban effects on liquidity and price formation were more pronounced for stocks without traded options. Given that stock size, price volatility, and liquidity are direct components of our model, an alternative channel which would result in differential effects for optionable stocks (other than via short-sales) is not apparent. As we discuss in more detail below, we find that the inflationary effects of the short-sale ban were isolated to stocks without traded options, providing an implicit validation of our model.

Our method almost certainly underestimates the difference between the actual prices and those that would likely have been observed in the absence of the ban. We attribute this underestimation to the trading of speculators who explicitly or implicitly use factor-analytic models to identify and profit from mispricing. In particular, if they (and other traders who trade on relative prices) observe that banned financial stocks are rising, they will buy stocks that load on factors common to the banned stocks and sell the financial stocks (if they can).

The resulting price pressures will reduce the difference that we estimate between the actual prices of the banned stocks and the prices that we would have observed without the ban. In particular, the speculators' trading will transmit some of the price inflation associated with the ban to the other stocks, which will cause us to overestimate the common factor returns. This issue will significantly affect the results if the speculators do not realize that the banned financial stocks may be rising relative to the other stocks because of the ban. Any differences that we identify in our results thus will likely underestimate the actual effect of the ban on market prices.

5.3 Potential bias from TARP

Other factors, not fully considered in the model, may also affect the banned sample. Foremost, perhaps, is the TARP legislation which was being debated in Congress at the time of the ban. TARP legislation had two potential effects. First, capital infusions into the banking sector would likely stabilize that sector with market-wide, broad influences of greater magnitude for financial stocks. As previously discussed, as long as the TARP factor loads in both samples and the factor magnitude maps linearly between the samples, then our model should accurately control for TARP broad market effects. As shown in Table 1, the TARP factor does indeed load in both samples, and given that TARP announcements occurred both during and after the ban, the consistency in the accuracy of the model in the post-ban period suggests that the assumption of linear mappings in factor magnitudes is also reasonable. If it were not, a significant decline in model accuracy in the post-ban period would result.

Second, investors may have speculated on specific firms which would receive TARP funding and, as these firms are concentrated within the ban sample, this speculation may bias our results. At the initiation of the short-sale ban, TARP had not yet passed in Congress and guidelines regarding how the funds would potentially be allocated were not available. Clearly, only troubled firms would receive funds, but the ability of investors to make finer forecasts is unclear. Bayazitova and Shivdasani (2012) report that banks which were larger, with weaker capital ratios and which were exposed to more financial risk, were more likely to receive TARP capital infusions (i.e., the stocks with the heaviest weight in the ban index). They also report that TARP capital injection announcements, which started on October 14, 2008, were associated with average excess returns of 10.9%. Two conclusions can be drawn from the return results. First, TARP announcements

affected *both* the banned and not-banned samples but with different magnitudes. Second, TARP announcements appear to be largely unexpected, given the relatively large one-day returns associated with TARP announcements for both samples. For example, the announcement to switch to bank capital infusions on October 13 related to a return of 11.7% and 9.8% for the banned and not-banned samples, respectively, even though bank stocks were isolated to the banned sample. Similarly, on September 29 when the House of Representatives rejected the initial stabilization plan, the banned and not-banned subsamples realized returns of -12.4% and -7.8% , respectively.

6 Price inflation associated with the ban

Figure 4 presents cumulative, value- and short interest-weighted actual banned stock index returns and the corresponding counterfactual estimates of these indices obtained from factor returns implied from the not-banned stocks. The plot covers the period 14 trading days before to 14 trading days after the ban. Focusing first on the value-weighted index (Panel A), in the pre-ban period the estimated and actual index returns overlap substantially with differences between the actual and estimated cumulative indices being realized only during the ban period. Over the course of the ban, we estimate that prices of banned stocks were inflated by 10.5%, reflected by the difference in the cumulative returns of the estimated and actual return series at the end of the ban.

An analysis of the time-series properties of the daily differences in the year before the ban indicates that the cumulative 14-day difference during the ban period is statistically different from zero based on the variance of this difference in the year before the ban. The standard deviation of the difference between 14-day actual index returns and 14-day estimated index returns, computed from

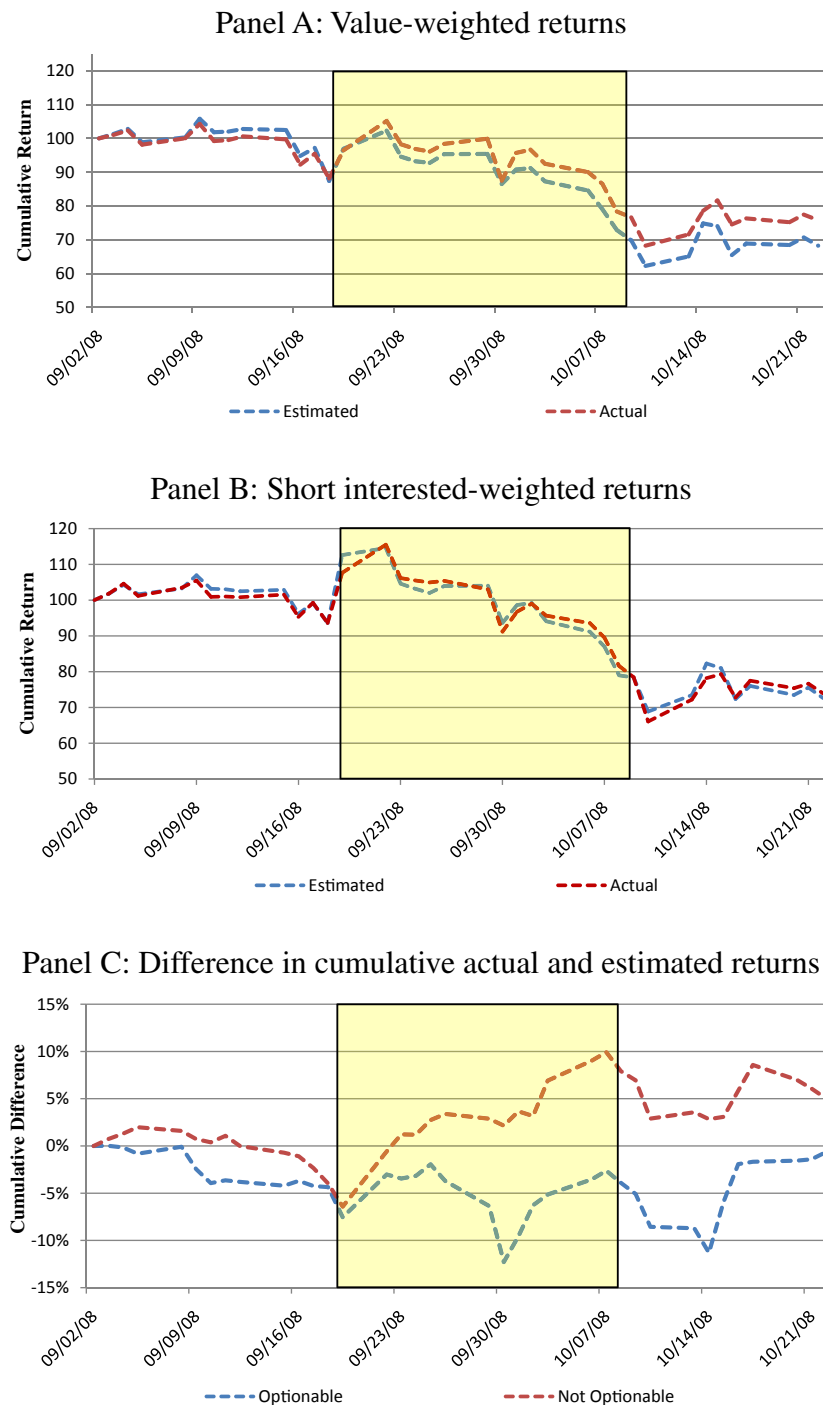


Figure 4 Actual and counterfactual cumulative returns. The figure plots value and short interest-weighted cumulative indices of actual returns and corresponding counterfactual returns estimated from the factor-analytic model, for the banned stock subsample over the period of 14 trading days before after the short-sale ban and. Panel C plots the difference in cumulative actual and estimated counterfactual returns for the short interest-weighted index, segmented by option availability. The period of the ban is shaded.

overlapping returns, is 2.9% in the year before the ban. The 10.5% 14-day difference in the ban period thus corresponds to a z -statistic of 3.12. Since variances rose during the ban period, this result is overstated. A paired t -test of the difference in the 14 daily returns during the period of the ban gives a t -value of 1.47, which corresponds to a p -value of 17%. However, this result is understated because the serial correlation of daily differences during the ban period is -0.55 . The negative serial correlation indicates that the difference series has transitory volatility that is increasing the variance of the daily difference that appears in the denominator of the paired t -test. These results indicate that the difference is significant compared to its previous history, but perhaps not notably significant given its current volatility. If the increased volatility in the ban period were due to the ban, the former statistic would provide the appropriate measure of significance. But if the increased volatility were due to other factors, the latter statistic would be more appropriate. The truth undoubtedly lies somewhere in between these two extremes.¹² To summarize, these results indicate that, although financial sector stocks lost value during the short-sale ban, the ban appears to have stabilized their prices, reducing average losses to financial sector stocks by 10.5% over 14 trading days.

Actual and estimated short interest-weighted indices for the banned stocks appear in Panel B. The two indices do not vary significantly from each other before, during, or after the ban. Apparently, the ban had less effect on these already heavily shorted stocks than on the other banned stocks. As reported in Figure 1, prices for these stocks fell the most in the year before the ban. These results also suggest that short covering, which would have been most pronounced in stocks with the highest level of short interest, does not explain the inflation that we note in the aggregate sample.

We obtain different results when we compute the indices separately for optionable stocks and stocks without listed options. During the ban, stocks with listed options could be shorted by options dealers who were hedging positions they acquired in the options market. Their customers thus could form synthetic, short positions through the options market. Panel C presents the difference between the actual and estimated short interest-weighted banned stock index returns, separately for stocks with and without listed options. The banned stock sample of 676 stocks includes 363 optionable and 313 not-optionable stocks.¹³ During the ban period, the difference between actual and estimated index returns for the optionable stocks was 1.8% (statistically insignificant), which suggests that the ban had no appreciable impact on stocks that could be synthetically shorted in the options markets. For the stocks without listed options, the actual index increased 12.8% relative to the estimated index during the ban period. The paired t -statistic for the test of equality of mean daily returns is 1.62.¹⁴ These results suggest that some short-selling continued in the highly shorted stocks with listed options whereas the ban had a greater effect for highly shorted stocks which could not be shorted in the options markets. These results further support the conclusion that the inflation we note is related to the short-sale ban and cannot be attributed to other coincidental events unlikely to have differential effects on optioned relative to not-optioned stocks.

The result that inflation from the short-sale ban was more pronounced for not-optionable stocks contrasts Battalio and Schultz (2011) who examine the initiation of short exposure on the CBOE and ISE in August and September 2008 for short-sale banned stocks and a matched control sample. They report that initiation of short exposure was not significantly higher for the banned sample relative to the control, from which they conclude

that investors seeking short positions did not migrate to the option market. However, their results also show a marked increase in option trading coincidental with the ban for both the control and the ban subsamples. They also report that option trading costs significantly increased for the banned stocks. Thus, a viable alternative hypothesis to Battalio and Schultz is that a segment of investors who normally traded in options for the banned stocks dropped from the market due to increased trading costs and that those investors were replaced by an influx of investors from the short-sale market. As previously discussed, our results are consistent with international evidence from Beber and Pagano (2011) who find differential effects of short-sale bans for optionable stocks.

We also analyze cross-sectional variation in stock price inflation using multivariate regression methods to help determine, on a stock-by-stock basis, whether the inflation was due to the ban or other factors such as TARP. We estimate three models. In each model, the dependent variable is inflation for a given stock during the ban period, calculated as the cumulative difference between actual and estimated daily returns. In the first model, we regress inflation on indicator variables for inclusion in the short-sale ban (BAN), availability of option trading (OPTION), and provision of TARP funding in 2008 (TARP). We also include market capitalization (SIZE), the percentage of float sold short on September 15, 2008 (SHORT), average illiquidity (AMIHU), and volatility (VOLAT) over the year before the short-sale ban as control variables.¹⁵

The estimation results are presented in the first column of Table 3. The positive and significant (*t*-statistic 7.77) BAN coefficient indicates that inflation was significantly greater for the banned stocks. OPTION is insignificant suggesting that inflation was not statistically different

Table 3 Inflation determinants.

Model	Dependent variable = inflation		
	(1)	(2)	(3)
BAN	0.12 (7.77)	0.14 (5.52)	
OPTION	−0.019 (1.30)	0.009 (0.54)	−0.14 (3.38)
TARP	0.013 (0.84)	0.010 (0.67)	0.030 (0.80)
SIZE	0.062 (4.29)	0.067 (4.19)	0.040 (1.03)
SHORT	−0.023 (1.56)	−0.026 (1.75)	−0.088 (2.00)
AMIHU	0.019 (1.32)	0.022 (1.18)	0.00 (0.00)
VOLAT	0.084 (5.76)	0.053 (3.28)	0.25 (6.15)
OPTION*BAN		−0.10 (4.53)	
SIZE*BAN		−0.006 (0.38)	
AMIHU*BAN		−0.012 (0.61)	
VOLAT*BAN		0.098 (4.56)	
Observations	4810	4810	676
<i>R</i> ²	0.03	0.04	0.06

Table summarizes regression estimates that characterize the cross-sectional relation between inflation during the short-sale ban (measured in percent) and indicators of whether a stock was on the short-sale ban list, whether it was optioned, and whether the issuer received TARP funding in 2008. For each stock we measure inflation as the difference between the cumulative return estimated from the factor model and the actual cumulative return. Models 1 and 2 are estimated with the full stock sample, while Model 3 is estimated only with the banned stock sample. BAN, OPTION, and TARP are 0,1 indicators of whether the stock was included on the short-sale ban list, it had listed options, and the issuer received TARP funding before December 31, 2008. The control variables are SIZE, market capitalization on October 8, 2008; SHORT, the percentage of float sold short on September 15, 2008; AMIHU, the mean Amihud measure of illiquidity (Amihud, 2002); and VOLAT, the square root of mean squared returns. The last two means are measured over the year before the short-sale ban. The table reports standardized coefficient estimates with *t*-statistic values in parentheses below.

for optionable relative to nonoptionable stocks across the entire sample. Similarly, the TARP coefficient is insignificant; suggesting that inflation for the TARP subsample was not significantly greater than for the other banned and not-banned stocks. The control variables are insignificant with the exception of VOLAT (t -statistic 5.76), suggesting that stocks with greater value uncertainty were more effected by the short-sale ban.

To examine cross-sectional determinants of inflation within the banned stock subsample, in Model (2) we interact the banned stock indicator variable with each of the variables of interest.¹⁶ The OPTION*BAN interaction variable is negative and significant (t -statistic 4.53), which suggests that within the ban sample, optionable stocks realized significantly lower inflation than non-optionable stocks. This finding supports the conclusion that options mitigated the short-sale ban by allowing investors to create synthetic short positions. We find the same result in Model (3) which we estimate using the banned stock subsample. Similar to the full model specifications, the TARP coefficient is insignificant, which suggests no noteworthy difference in inflation between the TARP and non-TARP funded stocks in the banned stock subsample. In all three models, the coefficient on SHORT is negative but is only significant in the third model. The coefficient sign suggests that inflation was, if anything, lower for stocks with higher short interest, confirming our previous conclusion that short covering at the start of the ban has little relation to the level of inflation. In unreported tests, we include the change in short interest between August 15, 2008 and October 15, 2008 as an alternative proxy for the reduction in short interest and find the same result.

As a final robustness check, we estimate cumulative inflation for the not-banned subsample during

the banned period. We find that throughout the ban period, the level of cumulative inflation was not significantly unique to zero. Given the size of this subsample and the fact that the second-stage factor loadings were estimated from this sample, this result is not surprising. But, given that this subsample was subject to the same increase in price volatility as the banned subsample, this result gives us an added level of confidence that our factor-analytic model is accurately capturing factor mappings during the ban period.

7 Post-ban correction and the consequences for buyers

Our ban period models suggest that the short-sale ban imposed by the SEC resulted in the inflation of financial sector stock values, on average, by 10–12%. At least two plausible explanations can account for this result, each with significantly different consequences for traders who made purchases during the ban. First, the price inflation may have been due to the reduced threat of liquidity death-spirals, as the SEC intended. If so, all holders, including those who bought during the ban, would have benefited. Alternatively, by excluding traders with negative value opinions from the market, the ban may have only temporarily inflated prices relative to what we otherwise would have expected. If so, traders who bought during the ban would lose as prices returned toward their true intrinsic values following the ban.

As previously discussed, given the potentially event-induced change in volatility surrounding the short-sale ban, we evaluate the statistical significance of the post-ban cumulative difference results utilizing two tests. The first is a t -test of whether the mean cumulative difference in actual and estimated returns from the end of the ban to day T is equal to the negative of the mean

cumulative difference accrued during the ban. In the second test we utilize the “standardized cross-sectional test” (SCS) developed by Boehmer *et al.* (1991) to analyze the same return series. The SCS test incorporates information from both the estimation period (pre-ban period) and the event period (post-ban period):

$$t_{\text{SCS}} = \frac{\frac{1}{N} \sum_{i=1}^N \text{SD}_i}{\sqrt{\frac{1}{N(N-1)} \sum_{i=1}^N (\text{SD}_i - \frac{1}{N} \sum_{i=1}^N \text{SD}_i)^2}}, \quad (3)$$

where SD_i is the standardized difference in the actual and estimated return for the i th stock, calculated by dividing the post-ban difference in returns for the i th stock on day T by the standard deviation of the difference in returns in the pre-ban period. Boehmer *et al.* (1991) demonstrate that the SCS test statistic is not affected by event-induced variance changes.

The two hypotheses compared are:

$$H_o : \text{CD}_i = 0 \quad \text{for all } i \text{ (no correction)}$$

$$H_a : \text{CD}_i = -\text{BD}_i \quad \text{for all } i \text{ (full correction)}$$

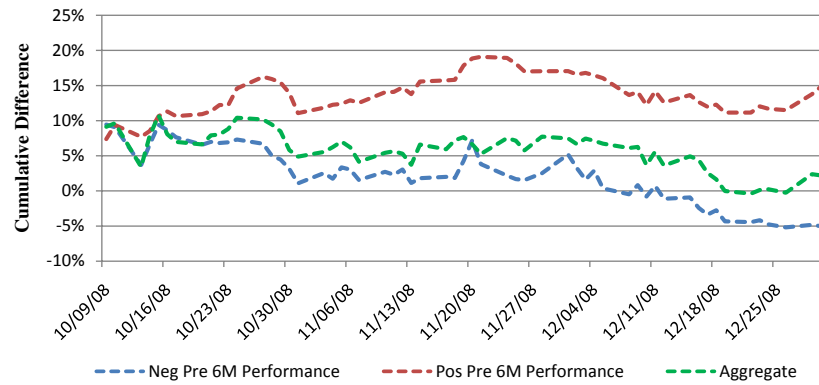
where CD_i is the mean cumulative difference between actual and estimated returns for stock i from the end of the ban period to day T after the ban and BD_i is the mean cumulative difference between actual and estimated returns for stock i during the ban period. Under the null hypothesis, the cumulative difference between actual and estimated returns follows a random walk during the post-ban period, culminating in a value not statistically unique to zero. Under the alternative hypothesis, the cumulative difference follows a negative trend in the post-ban period, ultimately reversing the inflation impounded in stock prices during the ban.

Panel A of Figure 5 displays the value-weighted, mean cumulative difference between actual and estimated returns in the post-ban period for the

aggregate sample and subsamples weighted by performance in the six months preceding the ban. For each sample, the cumulative difference is initially set to the value at the end of the ban. Focusing first on the aggregate sample, we find that short-sale ban related inflation was sustained until mid-December, 2008. Panel B presents test statistics to more accurately determine the date in which the difference between the estimated and actual return series becomes statistically indistinguishable from zero. The t -statistic of both tests remains in excess of 2.0 until December 17, 2008 or 18, 2008 (depending on the test), suggesting that the null hypothesis cannot be rejected with 95% confidence (correction of ban inflation has occurred) until 49 trading days after the ban. It would be expected that any reversal in ban period inflation would occur rapidly once the ban is lifted or at least in a period similar to the inflation accumulation period. In short, in aggregate, we find that inflation realized during the ban was sustained at least to the end of 2008 and the timing of the reversal is inconsistent with reversal of ban-related price inflation.

To examine post-ban returns in more detail, we sort the banned stock subsample by pre-ban performance. When the SEC selected stocks for inclusion in the ban, the foundation of the list was financial sector SIC codes, with no consideration of stock performance. While on average, financial sector stocks realized negative returns prior to the ban, negative performance was not pervasive. Only stocks with highly negative returns before the ban would be at risk of liquidity price-spirals and be likely candidates for price manipulation by short-sellers. It is also these stocks for which negative information or investor sentiment would most likely be excluded from the market during the short-sale ban, contributing to transitory price inflation. It is not clear what effect, if any, the short-sale ban would have had on stocks experiencing relatively positive investor sentiment.

Panel A: Difference in cumulative actual and estimated returns



Panel B: Test statistics for the aggregate sample

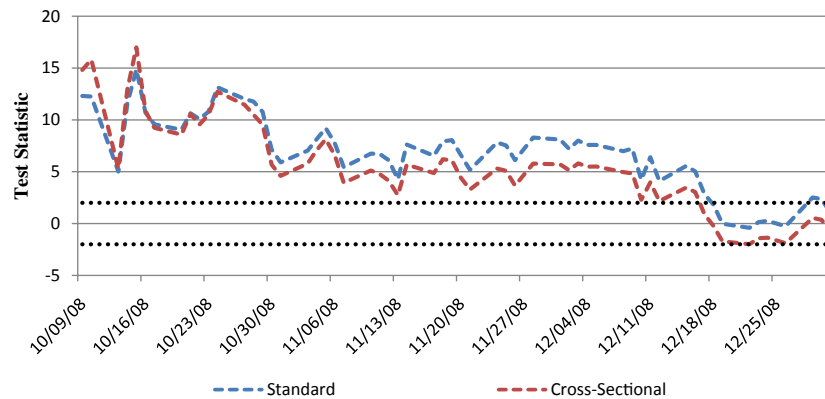


Figure 5 Post-ban period analysis. The figure plots the value-weighted, mean cumulative difference between actual and estimated returns for the banned stock subsample from the end of the short-sale ban to the end of 2008. At the start of the analysis the cumulative difference is set equal to the cumulative difference between actual and estimated returns accrued during the ban period. The cumulative difference is presented for the aggregate sample and subsamples based on performance (positive or negative) six months prior to the ban. Panels B and C report test statistics for the aggregate sample and the negative performance subsample, testing the two hypotheses:

$$H_o : CD_i = 0 \quad \text{for all } i \text{ (no correction)}$$

$$H_a : CD_i = -BD_i \quad \text{for all } i \text{ (full correction)}$$

where CD_i is the cumulative difference between actual and estimated returns for stock i from the end of the ban period to day T after the ban and BD_i is the cumulative difference between actual and estimated returns for stock i during the ban period. t -Test and standardized cross-sectional (Boehmer *et al.*, 1991) test statistics are reported. The conventional t -test statistic significance threshold values of 2.0 and -2.0 are referenced by dotted lines.

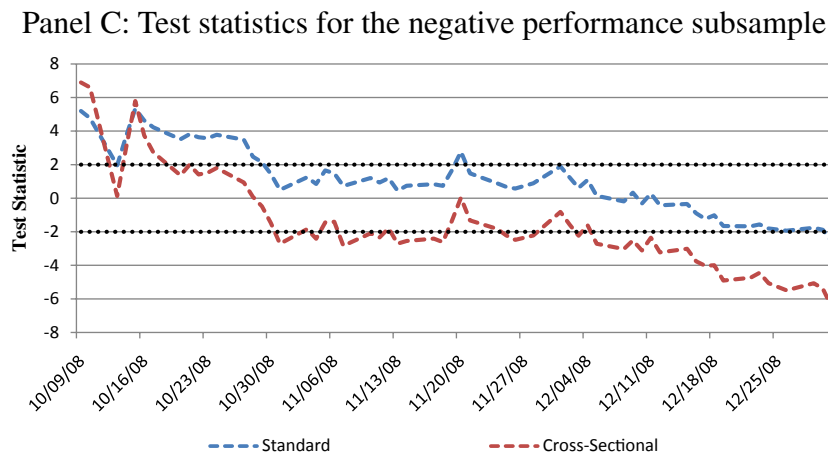


Figure 5 (Continued)

To examine the differential effect of the short-sale ban on negatively and positively performing stocks we utilize two additional weighting factors. The negative performance weighting factor is equal to the absolute six month return prior to the ban, with the weighting factor set to zero for stocks which realized a positive return in that period. The positive performance factor is calculated in the same way but the weighting factor is set to zero for stocks which realized a negative return in the six months prior to the ban.¹⁷ As previously discussed, we hypothesize that these weighting factors will capture aggregate exposure to crisis risk factors across stocks.

Returning to Panel A of Figure 5, although the magnitude of inflation was similar for the positive and negative performance-weighted subsamples (10%), inflation is sustained at, or above, that level for the positive performing stocks. In contrast, inflation is reversed for the negative performance-weighted index approximately two weeks after the ban. Panel C reports the test statistic results for the negative performance-weighted index. For stocks with negative pre-ban performance, the null hypothesis can be rejected with 95% confidence (suggesting full correction) 8 or 16 days following the ban based on the SCS or t -test results, respectively. With the exception of

two days, the t -test statistic remains between 2.0 and -2.0 for the entire remainder of the post-ban period. Results are similar for the SCS test statistic. However, during the month of November, the test statistic drops slightly below -2.0 on several occasions and stays below -2.0 in December suggesting that the null hypothesis cannot be rejected two months following the ban. As previously discussed, given the blended source of the volatility change coincidental with the ban, the true value of the test statistic likely lies somewhere between the two extremes of the t -test and SCS test statistics.

To obtain an estimate of the dollar cost of the inflation to buyers during the ban period, for each banned stock on each day during the ban, we compute the product of our estimate of the percentage inflation in that stock multiplied by the dollar value of volume in the same stock.¹⁸ Summing this measure over all banned securities gives a total dollar value of inflation of \$4.9 billion. If only the subsample of stocks with negative pre-ban performance is considered, the total dollar value of inflation is estimated to be \$2.3 billion. As discussed above, this measure is likely biased downward by the price effects of speculators who traded on the basis of potential on differences in the valuations of the banned and not-banned stocks. Regardless of whether or not

the full banned stock sample or just the negative performance stocks are considered, this wealth transfer is of sufficient size that it should concern public policy makers at the SEC and elsewhere.

8 Conclusion

The analyses in this paper indicate that the short-sale ban imposed by the SEC on financial stocks in September 2008 inflated prices relative to where they likely would have traded without the ban. Although speculating on counterfactuals is always difficult, we believe that our factor-analytic model provides a reasonable lower bound on the degree of price inflation that occurred. Our model estimates common daily valuation factors using the sample of stocks that were not banned and uses this information to estimate returns for the banned stocks.

The ability to confidently identify trading effects in a one-shot event study in the midst of so much volatility is quite challenging. We believe that we have substantially improved our inferences through the use of our factor model, but the results are not definitive. In particular, if during the ban period, factors that we did not model affected the banned stocks but not the other stocks, the inflation we identify could be due to those factors. Foremost among such factors would be concerns about the then pending TARP legislation. Our results, however, suggest that TARP was not a significant factor in the outcome. Assuming that the price effects that we document are indeed due to the ban, we estimate that buyers paid \$4.9 billion more for the banned stocks than they otherwise would have. Focusing only on stocks with negative pre-ban performance, for which we find more conclusive evidence of a more immediate post-ban reversal of inflation, the total dollar value of inflation is estimated to be \$2.3 billion. Our results suggest that the short-sale ban was, to a degree, effective at stabilizing prices for a

segment of the market. However, for stocks at the center of the crisis, which suffered substantial reductions in market value preceding, during, and following the ban, the ban appears to have had limited efficacy as for this subset of stocks, price inflation corrected shortly following the ban.

Acknowledgments

We thank Mark Huson, Raghavendra Rau, Terrence Hendershott, seminar participants at the University of Southern California, the Q-Group, University of Warwick and University of Waterloo and session participants at the 2009 NBER Market Microstructure Research Group Meetings, 2009 Northern Finance Associations Meetings and The Economics, and Econometrics of Recurring Financial Market Crises Conference for their helpful comments.

Notes

- ¹ See SEC Release No. 34-58592 and SEC Release No. 2008-211 for discussion of the objectives of the short-sale ban in the U.S. by SEC Chairman Christopher Cox and SEC Acting Secretary Florence E. Harmon.
- ² See, for example, Bris *et al.* (2007).
- ³ Boehmer *et al.* (2009) undertake a similar analysis focusing on the U.S. and benchmarking the returns of banned stocks to a control sample matched on listing exchange, pre-ban trading volume, and market capitalization.
- ⁴ See for example, Chen *et al.* (2002), Lamont (2012), Nagel (2005), and Asquith *et al.* (2005).
- ⁵ On September 19, 2008, the SEC banned short-sale transactions for banks, insurance companies, and securities firms identified by SIC codes 6000, 3020-22, 6025, 6030, 6035-36, 6111, 6140, 6144, 6200, 6210-11, 6231, 6282, 6305, 6310-11, 6320-21, 6324, 6330-31, 6350-51, 6360-61, 6712, and 6719. The September 19, 2008 ban list included 848 firms. Many firms filed complaints asking to be included on the list. The SEC subsequently added 149 more firms to the list between September 22, 2008 and October 7, 2008. Ten firms initially included on the list requested removal. Our classification of

banned stocks includes all stocks added to the ban list between September 19, 2008 and September 26, 2008. We exclude stocks added after September 26 and stocks removed from the list after initial inclusion.

- ⁶ The Short Squeeze database compiles short interest data for over 16,000 stocks listed on the NASDAQ, NYSE, AMEX, OTCBB, and Pink Sheets drawn from exchange publications.
- ⁷ Such securities were primarily ETFs for which we suspect information about shares outstanding was often inaccurate.
- ⁸ We use the percentage of float sold short on September 15, 2008 to compute the short interest-weighted indices. Float data were not available for 735 of the stocks in our 4,810 stock sample. For stocks missing float, we used shares outstanding instead.
- ⁹ See Daniel and Titman (1997) for a similar application of this modeling methodology.
- ¹⁰ We cannot conduct a similar analysis for the cross-sectional stock characteristic factors, because their actual values are unknown.
- ¹¹ For simplicity, the six-factor model with stock characteristics is referred to as the factor-analytic model in the remainder of this paper.
- ¹² Further support of the significance of the inflation for the banned subsample is provided in the multivariate regression analysis discussed below.
- ¹³ Optionable stocks include stocks listed on the NYSE/AMEX, Chicago Board, or the Philadelphia Options Exchanges at the time of the ban.
- ¹⁴ Variation in the magnitude of inflation between optionable and non-optionable stocks is noted only for the short interest-weighted and multivariate regression models. For the value-weighted results, no appreciable difference was found in the magnitude of inflation for optionable and non-optionable stocks (9.5% and 10.1%, respectively, relative to the aggregate sample result of 10.6%). This result is not unexpected. Within the banned subsample, optionable stocks are on average over twice the size (market capitalization) of non-optionable stocks. Thus, utilizing a value-weighting method to calculate mean inflation biases against finding a difference in inflation between optionable and non-optionable stocks. For this reason, the short interest-weighted method and multivariate analysis give a more reliable assessment of the difference in inflation between optionable and non-optionable stocks, and we focus upon these methods for this analysis.

- ¹⁵ We measure illiquidity using the Amihud Illiquidity Ratio (Amihud, 2002) calculated as the daily ratio of absolute stock return to the dollar value of trading volume.
- ¹⁶ Note that we cannot include the interaction of TARP and BAN as all TARP stocks were also banned, thus the interaction variable is not full rank with inclusion of the TARP base variable.
- ¹⁷ The short-sale ban subsample is approximately evenly split between positive and negative performers. Of the 676 stocks in the subsample, 315 stocks realized negative returns in the six months prior to the ban.
- ¹⁸ We use stock inflation estimates from the value-weighted cross-sectional regression results in this analysis.

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Keywords: Short selling; short-sale ban; short-sale constraints; financial crisis