"Shooting the Messenger?" The Impact of Short Sale Bans in Times of Crisis

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Abstract:

We find that the bans on covered short sales, implemented in several countries during the financial crisis of 2008-09 improved market liquidity or at least had a neutral impact; a result we argue could be expected in theory, given a simple variation on the Diamond-Verrechia (1987) model. The result holds for daily data over an extended period as well as for intraday data over various time spans. In contrast to other recent studies, we use American Depository Receipts as the controls in a difference-in-difference analysis encompassing all banned non-U.S. shares with corresponding depository receipts listed in the United States. Furthermore, we find that bans on covered short sales generally succeeded in lowering volatility. Banning short selling is not good policy in normal times, but our findings indicate that such bans might prove useful in (temporarily) stemming liquidity loss during crises.

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Introduction

Short sellers often prove to be an easy scapegoat during times of financial crisis. During the Great Depression Herbert Hoover began a "crusade against short selling" that included asking exchanges to limit the number of shares available to short sellers (Geisst 1997). During the recent financial crisis, regulators have accused short sellers of exacerbating (if not causing) many of the problems in financial markets. Short sellers even have been implicated in the demise of Bear Stearns and Lehman Brothers; New York Attorney General Andrew Cuomo likened them to "looters after a hurricane" (*The New York Times* 9/19/08). Regulators continue to implement new short selling restrictions. These actions have included an outright ban on short selling of securities listed on the Athens Stock Exchange beginning in April 2010, and a ban on naked short sales for some bank shares and debt securities implemented less than a month later by the German regulator, BaFin. In an editorial discussing the German ban, *The Wall Street Journal* proclaimed, "Germany Shoots the Messenger."

During the most recent financial crisis, regulators around the world revisited the regulation of short sales. On July 15, 2008 the SEC issued an emergency order to ban naked short sales in 19 financial stocks on US exchanges (seemingly confirming regulations that were already in place).[3] Market makers remained exempt from this regulation. The SEC claimed that naked short sales in these securities might cause prices to "artificially and unnecessarily decline well below the price level that would have resulted from the normal price discovery process"

[3] Naked short selling, the practice of selling a security short without arranging to borrow it beforehand, was controversial prior to the financial crisis. For instance, on the Hong Kong Stock Exchange naked short selling has been completely banned since the Asian financial crisis of the late 1990's. Less extreme, the London Stock Exchange proposed more disclosure requirements for naked short sales in 2004, but its abolishment was deemed unnecessary (FSA 2009a). With Regulation SHO, US markets found a middle ground by limiting the prevalence of naked short sales. Enacted by the Securities and Exchange Commission on January 3, 2005, Regulation SHO required broker-dealers to obtain a "locate" (i.e. reasonable grounds to believe a security can be borrowed) before engaging in a short sale. However, several types of transactions (e.g., those made by a market maker) were exempt from this regulation.

(SEC 2008). This temporary measure expired one month later on August 15th. However, just one month following this expiration, regulators implemented further measures during what *The Wall Street Journal* called "a Category 5 test of our financial levees." On the morning of September 15th Lehman Brothers filed for Chapter 11 bankruptcy protection, and Bank of America announced a $50 billion acquisition of Merrill Lynch. The following day the Federal Reserve Board authorized the Federal Reserve Bank of New York to lend up to $85 billion to prevent the collapse of American International Group. Also that day, the net asset value of the Reserve Primary Money Fund "broke the buck" due to losses stemming from the Lehman bankruptcy.

In response to these events, the SEC banned all naked short selling for US stocks as of midnight on September 18th, and the FSA announced a short selling ban for 32 UK financial stocks. The FSA also required investors to disclose short positions larger that .25% of shares outstanding for a given stock. After the close of US markets on September 18th, the SEC strengthened its previous restrictions by announcing a ban on all short sales for 797 financial stocks. Fears of global contagion prompted financial market regulators to follow the lead of their counterparts in the US and UK and implement restrictions on short selling. In the majority of countries, these restrictions consisted of bans on naked short selling. However, in addition to the US and UK, nine other countries banned covered short sales for at least some shares. [4] These bans were implemented by October 10, 2008 and lasted anywhere from a couple of weeks to over a year.

In this paper, we find that, overall, the bans did not harm and possibly even improved market quality, based on effective spreads and volatility. We also argue that our findings, particularly on liquidity, could be expected based on theory. We motivate the liquidity result using a simple Glosten-Milgrom (1985)/Diamond-Verrecchia (1987) type model, in which only

[4] Australia, Canada, Greece, Ireland, Italy, Netherlands, Norway, South Korea, and Switzerland

informed investors engage in short sales—a reasonably realistic assumption. In such an environment, the bid-ask spread is lower when regulators impose a ban on short selling. Proponents of short selling often argue that one of its primary benefits is the addition of liquidity to markets. Such may be the case in normal times. Intuitively, restrictions on short sales mean that only those who already own a stock can sell it. Diamond and Verrecchia (1987) show that, under certain modeling assumptions, limits on short sales create an imbalance compared to the case of freely permissible short selling, and markets therefore can become less liquid.[5] This result requires that informed and uninformed investors use short selling at equal rates, but the limited evidence available would suggest that short sellers are more likely trading on information.

To study the impact of short selling bans on stock liquidity and volatility, we employ a novel empirical strategy: we use a difference-in-difference estimator comparing non-US stocks with their American Depository Receipts (ADR) equivalents. In particular, we analyze securities from all eight countries that implemented bans on covered short sales outside of the period of the US ban. Since the United States was the first country to lift its ban on short selling, the ADRs corresponding to the non-U.S. shares were open to short selling for most of the duration of the non-U.S. bans. Thus, ADRs serve as a nearly identical control group for the banned non-U.S. shares trading on their home markets.

Many observers predicted that the bans would sharply limit market liquidity. Beber and Pagano (2009) note that if this is the case, it "would be a serious indictment of their [the bans'] adoption, especially considering that they were enacted at a time when market participants

[5] The effect of a ban might be mitigated if market makers can still short securities. However, as is pointed out in Boehmer et al (2008), research suggests that high-frequency traders (often quantitative hedge funds) supply a significant amount of liquidity to markets as well. These firms would be subject to the ban, so we might still expect to see some effect.

desperately sought liquidity." Our results do contrast with some empirical studies that have identified an apparent loss of liquidity during short sale bans, but most are not properly controlled at all, and none has used our difference-in-difference method with ADRs as a control group. Clifton and Snape (2008) find that spreads increased by 140% and turnover dropped by 21% during the 2008 short selling ban in the United Kingdom. Bris (2008) reports a decline in liquidity for stocks subject to the July 15, 2008 emergency SEC order to ban naked short selling. Boehmer et al. (2008) find that median effective spreads increased by 77bp for financial stocks during the US ban compared to 32bp for the control stocks. Similarly, Hansson and Fors (2009) report an increase of 131% for equal weighted relative effective spread (RES) for UK banned stocks compared to an increase of 55% for the control group. Based on a panel of 27 countries, Beber and Pagano (2009) find that the 2008/09 short sale bans significantly increased percentage spreads and illiquidity and impaired price discovery. Similarly, Lobanova et al. (2010) use a non-parametric Wilcoxon matched-pairs signed rank test to conclude that the US ban increased spreads for impacted stocks.

Economists have also investigated the effects of short selling on asset volatility. The most comprehensive theoretical treatment of this topic is given in Bai et al. (2006). Using a rational expectations equilibrium model, the authors argue that a ban on short selling diminishes the information content of stock prices and therefore increases the market risk as perceived by less informed investors. This increased risk perception raises volatility of returns. Marsh and Niemer (2008) point out that the higher price level predicted by Miller results in a lower standard deviation of returns (everything else being equal).

Empirical studies offer mixed results on volatility. Chang and Yu (2004) use changes in the list of stocks that can be sold short on the Stock Exchange of Hong Kong to test the effect of

short-selling on return volatility. They find that the volatility of returns increases when short sales are permitted for a security. In contrast, Charoenrook and Daouk (2005) find that short sales reduce market volatility. Papers analyzing the 2008 short-selling ban find similarly inconsistent results. Boehmer et al. (2008) report an elevated proportional intraday range (RVOL) for US stocks on the SEC list during the ban. Hansson and Fors (2009) note that although realized volatility initially "exploded" for UK stocks subject to the ban, this increased volatility likely resulted from the simultaneous deluge of bad news and rumors regarding these financial institutions. By analyzing data from the second half of the UK ban (once markets had calmed down significantly), they conclude that the ban likely had very little effect on the volatility of returns. On the other hand, Lobanova et al. (2010) document an increase in volatility as measured by squared returns. In other words, volatility results are quite variable.

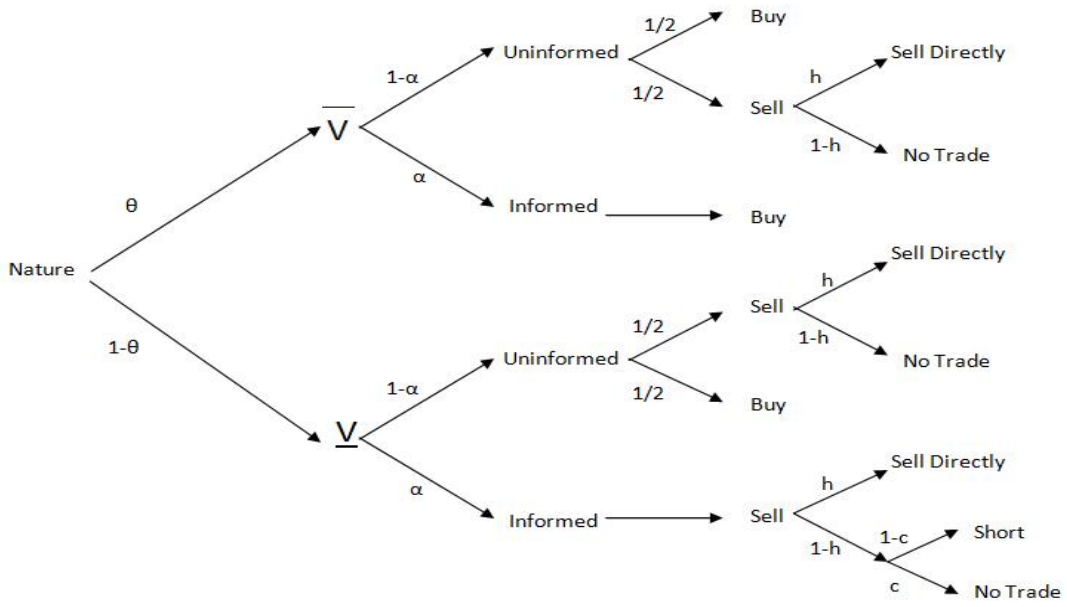
This rest of this paper is organized as follows. In the next section we set out the theoretical motivation for our liquidity findings. In the third section, we explain the methodology and describe the data used for this research. We present the results in section four. The final section concludes.

Why Banning Short-Sales Could Improve Market Liquidity

Diamond and Verrecchia (1987), one of a scant theoretical literature on short-selling, suggest that limits on short selling reduce information transmission and price discovery. Under certain assumptions, a ban also reduces liquidity (causes market makers to widen their bid-ask spreads). Here we develop a very simple extension of the Diamond-Verrecchia model that demonstrates

how a ban may improve liquidity due to differential use of short-selling between informed and uninformed traders.[6]

Since we are interested in measuring the impact of a short-sale ban on the bid-ask spread under the condition of asymmetric information between market makers and traders, we start with the standard tree diagram, based on the Glosten-Milgrom (1985) model and similar to that employed in Diamond and Verrecchia (1987).



To set their bid and ask prices, market makers must form expected values of the securities they trade, taking into account the possibility that the trader they are facing knows something about the true value of the security that he, the market maker, does not know. These expected values come from the above tree diagram. In this representation, $\tilde{V}$ is a random variable taking the value $\overline{V}$, with probability θ, in the state of good news and the value $\underline{V}$, with probability (1 –

[6] Beber and Pagano (2009) note that spreads may improve with a ban due to the reduced probability of market makers encountering informed traders but focus on the reasons, such as inventory control, for opposite effects to dominate.

θ), in the state of bad news. The variable α denotes the probability that a trader is informed. These traders know the true value of the security and therefore always sell the bad state and buy in the good state. Uninformed traders effectively trade for liquidity purposes and therefore buy and sell with equal probabilities.

When a trader decides to sell a given security, he sells directly from his portfolio if he owns the security (which occurs with proability h for both informed and uninformed traders). For simplicity, we assume that traders own either zero or one "unit" of each security and will not choose to sell in excess of what they own. Short selling enters the problem when a trader wishes to sell the security but does not already own it. In Diamond and Verrechia (1987), all traders sell short when they encounter this scenario. In reality, short sellers are more likely to hold private information relevant to valuation, and pure liquidity traders are less likely to sell short. This variation in short-selling propensities between informed and uninformed traders can substantially alter the bid-ask spread and the impact of banning short selling. Thus, here we assume that uniformed traders do not sell short, and will simply refrain from trading if they would like to sell but do not own the security. If he does not own the security (with probability 1-h) the informed trader will sell short if short sales are allowed (c = 1) and not trade if short-sales are banned (c = 0).

Now let B be the event that a buy order is observed and S be the event that a sell order is observed. In this framework, the ask (a) and bid (b) prices set by the market maker can be calculated by

$$a = E(\tilde{V} \mid B) = \underline{V}P(\tilde{V} = \underline{V} \mid B) + \overline{V}P(\tilde{V} = \overline{V} \mid B)$$

$$b = E(\tilde{V} \mid S) = \underline{V}P(\tilde{V} = \underline{V} \mid S) + \overline{V}P(\tilde{V} = \overline{V} \mid S)$$

Applying Bayes' rule (see Appendix), we can determine these values and compute the bid-ask spread ($a - b$) for both regulatory states: $c = 1$ (no-ban state) and $c = 0$ (ban state). The figure shows the difference between the 'no-ban' and 'ban' spreads versus the probabilities α and h for fixed values $\overline{V}$ =1.1, $\underline{V}$ =.9, and $\theta = 0.5$. The difference between the no-ban and ban spreads is always positive, regardless of the values of α and h, implying that the bid-ask spread during the no-ban period is larger than the spread during the ban period.

Figure 1 here

Methodology and Data

Our research design differs from other recent work in one key respect: our control group. We analyze stocks in non-U.S. markets that have corresponding American Depository Receipts (ADRs) trading on US exchanges. Depository receipts represent a fixed number of shares in a non-U.S. market. U.S. depository banks issue these ADRs, which allow the owner to convert into actual shares (at a fixed ratio) on the stock's home exchange for a minimal brokerage fee. Thus, the underlying asset (company) is the same; only the respective market's trading rules differ. In contrast to our study design, others have either examined only time period effects or have compared banned stocks with a control group of unbanned stocks in the same market.[7] This method creates problems, particularly during the most recent bans, since the restrictions primarily apply to the shares of financial institutions. During the financial crisis of 2008 these shares were subject to a deluge of announcements and rumors. It is therefore difficult to argue that a control group consisting of non-financial stocks satisfies the parallel trends assumption

[7] Boehmer, et al (2008) match financial stocks to nonfinancials using listing exchange, market capitalization, and dollar trading volume. Lobanova et al (2010) only study the time period effect, without any control group.

necessary for an unbiased difference-in-difference estimator. Beber and Pagano (2009) partially account for this by considering non-U.S. stocks that trade on both their domestic and U.S. markets and are banned on one or both. However, the analysis does not account for the time period differences or for potential permanent differences in the liquidity of the non-U.S. versus U.S. markets.

We gathered data on all non-U.S. stocks that were subject to a short sale ban that also had unbanned depository receipts listed on US exchanges. We do not consider stocks in countries that only imposed bans on naked short sales. We omit Canadian stocks because although regulators imposed a ban on covered short sales, its dates coincided with the US ban, therefore leaving them without a suitable control group. Also, Norway banned covered short sales for financial stocks but there are no corresponding American depository receipts for these securities. We obtained a list of depository receipts corresponding to stocks that trade in the countries of interest from BNY-Mellon. We do not include stocks or ADRs that were newly listed, delisted or acquired during the time period of interest. Our final sample consists of 35 stock-ADR combinations from eight countries (Australia, Greece, Ireland, Italy, Netherlands, South Korea, Switzerland, and the United Kingdom) for which we obtained daily open and close prices, closing bid and ask quotes, and trading volume from Bloomberg.[8] We consider the period from September 2007 (approximately one year before the bans were imposed) until July 2010—after most bans ended. We exclude the period of the US ban on short selling since this ban applied to some of the ADRs in our sample. For the UK stocks and corresponding ADRs, we also gathered the same data on an intraday basis—every 15 minutes—for the period November 17, 2008 through June 4, 2009. We analyze these data separately as an additional robustness check, since the intraday data allows us to examine a narrower time span and hone in on the actual policy

[8] See Table 1 for the specific securities included in the sample.

change potentially with fewer extraneous effects. The following methods apply to the daily data, but we compute the corresponding variants for the intraday London data and report those results following the cross-country daily results.

We use the daily relative effective spread (RES) and the Amihud ratio to measure illiquidity. RES is calculated as

$$RES = \frac{2|MQ - P|}{MQ}$$

Where MQ is the bid-ask midpoint and P is the daily closing price. The daily Amihud ratio is defined as

$$Amihud = \frac{|r|}{V}$$

where r is the daily return and V is the daily total volume. As in Beber and Pagano (2009) we use the rolling 20-day standard deviation of intraday returns to measure volatility. We remove the top and bottom 1% of observations for each of these measures from our analysis.

Table 2 summarizes the daily measures for the entire sample and for each country. Non-U.S. stocks averaged a relative effective spread of just under one percent (0.97%) during the periods before and after the ban. These measures increase to 1.17% during the ban periods. The corresponding ADRs experience 23% lower average RES (0.77%) during the non-ban periods and that measure rises to 1.13% during the ban period. The mean RES during the pres/post ban period ranges from 0.07% to 2.35% for non-U.S. stocks and 0.23% to 1.68% for the ADRs. During the ban period, the mean RES ranges from 0.18% to 2.17% for non-U.S. stocks and 0.57% to 2.13% for ADRs. In all but one of the countries (Australia) in the sample, the RES is higher during the ban period than in the pre/post ban period. Note that the relatively large range

of RES by country results from the small number of stocks in each country as well as the varying minimum tick size for each country.[9]

The average Amihud ratio for non-U.S. stocks during the pre/post ban period was 9.06E-08. During the ban period for these securities, the average Amihud ratio fell to 3.59E-08. For the corresponding ADRs, the average Amihud ratio remained relatively unchanged during these two periods. It is also important to note that if we instead use median values, we find that the Amihud ratio is higher for both non-U.S. stocks and ADRs during the ban period than in the pre/post ban period. Due to the generally higher trading volume of a security on its home exchange, we also see that the Amihud ratio for the non-U.S. stocks and ADRs differ by an order of magnitude. Also because of the wide range of trading volume for different securities, mean Amihud ratios vary considerably for each country. For non-U.S. stocks, it ranges between 5.71E-10 and 3.01E-07 during the pre/post ban period and between 1.59E-09 and 2.43E-07 during the ban period. For corresponding ADRs in our sample, the Amihud ratio ranges between 7.90E-09 and 1.41E-06 during the pre/post ban period and between 2.51E-08 and 1.56E-06 during the ban period. This illustrates the intuitive notion that stocks are more liquid on their home exchange than they are as equivalent depository receipts on US exchanges.

Finally, the average 20-day rolling standard deviation of returns averaged 2.30% for non-U.S. stocks in the pre/post ban periods and 4.17% during the ban period. Similarly, for the ADRs the mean volatility was 2.26% during the pre/post ban periods and 4.23% during the ban periods. Since price differentials between non-U.S. shares and corresponding ADRs can be

[9] For instance, Beber and Pagano note that 5% of their observations for Australia cluster around $1/10^{th}$ of 1 cent.

arbitraged for usually minimal transaction costs, it is not surprising that daily returns for these securities, and therefore rolling 20 day volatility, would be very similar. Furthermore, since short selling bans are presumably implemented during times of increased uncertainty, it is also not surprising that securities for in each of the countries exhibit higher volatility during the ban period than during the pre/post ban period. In particular, for non-U.S. shares the mean volatility for each country ranges between 1.72% and 2.91% during the pre/post ban period and between 2.94% and 7.17% during the ban period. For the ADRs, the mean volatility for each country ranges between 1.51% and 3.24% for the pre/post ban period and between 3.01% and 5.97% for the ban period.

Given the upheaval in financial markets coterminous with the short-selling bans, we cannot infer any policy impact from these figures. In order to home in on the effect of the bans themselves we use a difference-in-difference estimator. This approach allows us to isolate the effects of the ban (treatment) on the banned stocks, rather than muddying the inferences with additional, but irrelevant, time series effects that occurred simultaneously with the ban. In our framework, the treatment is defined as implementation of the bans. The treatment group consists of banned stocks in their home markets, and the control group includes the corresponding ADRs of those stocks. Since we exclude the period of the US ban, the control group was never subject to a ban on short selling.

The binary variable *foreigncountry* takes the value 1 for non-US stocks and 0 for ADRs. In addition, the binary variable *shortsaleban* equals zero during the period when short sales are not banned in a given non-U.S. country and 1 during the ban. We then regress our outcome variables—liquidity and volatility, respectively—on these two variables and their interaction, plus a constant.

That is,

$$Y_{it} = \alpha + \beta(foreigncountry_i) + \gamma(shortsaleban_{it}) + \delta(foreigncountry_i \times shortsaleban_{it}) + \varepsilon_{it}$$

where

α = constant term

β = non-U.S. stocks specific effects

γ=time period effects common to both groups

δ=effect of treatment

ε=error term

In other words, the coefficient of *foreigncountry* (β) accounts for the permanent difference between the non-U.S. stocks and corresponding ADRs, while the coefficient of the ban indicator (γ) accounts for changes over time that impact both groups equally. We are primarily interested in the parameter δ, the true effect of the lifting of the ban on the dependent variables of interest.

We also include results for the effect of the bans for each individual country. To accomplish this, we include dummy variables for each country. The regression used for this analysis is therefore given by

$$Y_{it} = \alpha + \gamma(shortsaleban_{it}) + \sum_i \left[\beta(country_i) + \delta(country_i \times shortsaleban_{it})\right] + \varepsilon_{it}$$

where the summation is taken over the $i = 8$ non-U.S. countries in the sample. The coefficients have the same interpretation as above, except that now there is a unique value of β and δ for each country.

Finally, for the London intraday data, the treatment is defined as the lifting of the UK ban on January 16, 2009. The treatment group still consists of banned UK stocks, and the control group includes the corresponding ADRs of those British stocks. Separately, to check robustness, we also analyze the 24 UK stocks that were never banned and their corresponding ADRs. In

case ADRs behave differently from underlying stocks, we provide a further robustness check by using the unbanned non-financial stocks as the control group for the banned financials. Similar to the daily data analysis, the binary variable '*country*' takes the value 1 for London stocks and 0 for ADRs. Because we are now studying the lifting of the UK ban, the binary variable, labeled '*shortsalelift,*' equals zero during the period when short-selling is banned in London and 1 after it is lifted. Otherwise, the model corresponds to that of the daily data: we regress our outcome variables—liquidity and volatility, respectively—on the two indicator variables and their interaction, plus a constant. That is,

$$Y_{it} = \alpha_2 + \beta_2(country_{it}) + \gamma_2(shortsalelift_{it}) + \delta_2(country_{it} \times shortsalelift_{it}) + \varepsilon_{it}, \text{ where}$$

α_2 = constant term

β_2 = London stocks specific effects

γ_2 = time period effects common to both groups

δ_2 = effect of treatment

ε=error term

The interpretation of these time period and treatment coefficients differ from the daily analysis, in that we are now examining the lifting of the ban, rather than the existence of the ban.

Results and Analysis

We estimate the difference-in-difference models using panel regressions with robust standard errors. Our analysis shows some surprising results, considering the past research on the subject. Most notably, we find that liquidity improved (ie, illiquidity *decreased*) for non-U.S.

shares (relative to their ADRs) when short selling bans were implemented.[10] In particular, we estimate a statistically significant difference in difference—i.e. a coefficient of the interaction term—of -0.196% for RES and -5.82E-09 for the Amihud ratio (see Table 3). Both of these coefficients are significant at the 1% level. The time effect (i.e. coefficient on ban) is positive for both RES and Amihud ratio, confirming that spreads generally increased during the ban period. Indeed, it is this temporal coincidence of the ban with the increase in spreads that might lead some to incorrectly conclude that the short selling bans caused the increase in spreads.

As a robustness check, we also provide results that specify the effect of the short selling ban for each country (see Table 4). We find a significant (at the 5% level) decline in spreads in four of the eight countries in our sample. One country shows a statistically insignificant decline, while three actually experienced an increase in spreads. The Amihud ratio shows improvement (at the 1% level) during the bans in seven of the eight countries. The country-by-country results suffer from small numbers in certain cases, however, the results still generally support the finding that the short selling bans improved liquidity.

We also examine the impact of the short selling bans on stock volatility, as measured by the 20 day rolling standard deviation of returns. The results indicate that regulators largely succeeded in reducing volatility in equity markets by imposing short selling bans, as evidenced by the negative coefficient of the interaction term in the regression for the entire sample (Table 3). In particular, for the pooled sample of all stock-ADR combinations, we estimate a difference-in-difference coefficient of -0.38% (significant at a 1% level) on the rolling 20-day standard deviation of returns. This coefficient is economically significant, as the volatility measure averages approximately 4.2% for both the ADRs and non-U.S. stocks during the period

[10] We imply a causal impact here, but note that causal inference depends on the assumption that we have successfully controlled for time series and country effects.

of the ban. We find a positive and significant coefficient for the ban variable, not surprisingly indicating that stocks experienced higher volatility during the tumultuous times during which the bans were imposed.

The country-by-country breakdown once again reveals important variation in the experiences across countries (Table 4). Indeed, we find that the ban significantly reduced volatility in four of the eight countries in our sample (Australia, Greece, Italy and South Korea). In the other four countries, however, the ban seemingly increased volatility (all significant at 1%). Interestingly, in five countries, the shares show more volatility on their home exchange than they do as ADRs, and in all but one of these cases (Australia) the sign of the country indicator variable matches that of the interaction (difference-in-difference) term. That is, for the stocks that already were more volatile on their home exchanges than they were as ADRs, we see that the bans further increased volatility for the most part. For stocks whose volatility was lower at home than on the US exchanges, the ban apparently lessened volatility. Naturally, we do still have to contend with the fact that some countries have only a few cross-listed stocks, so the inference relies primarily on the time-series dimension. Given our pooled results, however, it seems that the cases of a dampening effect on volatility outnumber those seeing an exacerbating effect.

London Intraday Analysis

Finally, we turn to the London intraday data to assess whether higher-frequency data in a more confined time period around the policy change produces any differing conclusions about the impact of the ban. On the contrary, we find complementary results.

Because of the intraday data, we adjust our market quality measures to take advantage of the higher frequency of observations. The equally weighted relative effective spread (RES) becomes the following:

$$RES = \frac{1}{n}\sum_{i=1}^{n}\frac{2|T_i - MQ_i|}{MQ_i},$$

where T_i = trade price for a security during the i^{th} 15 minute interval during the day, MQ_i = bid-ask mid-quote during interval i, and n = number of 15 minute intervals in the day. We also compute a transaction weighted version of the metric (TRES):

$$TRES = \frac{1}{\sum_{i=1}^{n} V_i}\sum_{i=1}^{n}\frac{2V_i|T_i - MQ_i|}{MQ_i},$$

where V_i is the volume of shares traded in the i^{th} 15 minute interval.

London financial stocks the RES/TRES averaged 0.0047/0.0056 in the first half of January (ban period), but increased to 0.0132/.0.0159 in the second half (post-ban period; see Table 1). The mean RES/TRES for American depository receipts measured 0.0021 (for both) during the ban period, and 0.0031/0.0032 once it was lifted.

We also examine volatility of the higher-frequency data, using the sum of squared intraday returns (i.e., realized volatility). We confine our analysis to the two hour period each day when both markets are open (9:30 AM to 11:30 AM EST). We denote this measure 'contemporaneous realized volatility.' By restricting the analysis to this window, we guarantee that prices for both UK shares and corresponding ADRs reflect new information at the same time. In particular, during the time period of interest the large magnitude of the overnight returns for the ADRs (-2.3% compared to -0.1% for the UK shares) suggests that events during trading

hours in the UK have a significantly larger effect on returns of the ADRs than information revealed during US market hours. Thus, when using the whole trading day, ADRs usually look more volatile, since their prices have to move all at once at the US market open, just to catch up with whatever events transpired in London during the trading day there. Contemporaneous realized volatility averaged 0.00033 for UK stocks during the ban period, compared to 0.00071 in the post-ban period . Similarly, the mean value for the realized volatility of ADRs more than doubles from 0.00031 during the first half of January to 0.00068 in the second half.

Just as with the daily data, we find a positive liquidity effect of the short-sale ban. Lifting the ban increases illiquidity for UK shares (relative to their ADRs).[11] In particular, we estimate a statistically significant difference in difference—i.e. a coefficient of the interaction term—of 0.8% for RES and 0.9% for TRES (see Table 5), measured over the month of January.[12] Both of these coefficients are significant at the 1% level.

Insert TABLE 5

Next we analyze the effect of lifting the UK ban on contemporaneous realized volatility, measured using the sum of squared 15 minute returns. Volatility rose markedly in the two weeks after the FSA lifted the short-sale ban, but there is not a significant country effect. Similarly, the difference-in-difference estimate is positive but not statistically different from zero. Thus, based on our estimates the ending of the ban did not affect volatility.

As we would expect, if we are truly measuring the impact of the ban, the non-financial UK stocks, which were not subject to a short selling ban, behave quite differently from the

[11] Again, we imply causality with the assumption that our method controls for parallel effects.
[12] The exact coefficient estimates vary depending on the time period we examine—extending as long as November, 2008 through June, 2009—but the qualitative finding remains very similar.

banned financials (Table 6). The lifting of the ban makes little difference in the liquidity of these stocks. The interaction effect for TRES is insignificant. The interaction effect for RES becomes statistically significant (p = 1%) and negative, but the coefficient estimate is very small—an order of magnitude less than that found for the financial stocks.

Insert TABLE 6

Like financials, volatility of non-financials increased in the two weeks following the rule change. In contrast to financials, non-financial stocks experienced lower volatility in their home market than in the ADR market. For non-financial stocks we obtain a slightly negative estimate for contemporaneous realized volatility, but once again the coefficient estimate is statistically insignificant.

The last check on the results comes by using the unbanned non-financial stocks as the control group for the banned financials, instead of using the ADRs. Using this alternate control group, we find results for the two spread measures that are very similar to those using the ADR controls (Table 7). The coefficients remain positive, with nearly identical magnitudes, but the statistical significance weakens (as it should, given the inferior control group we are using for this treatment group). Interestingly, the lifting of the ban appears to increase volatility for the financial stocks relative to the non-financial stocks. To the extent that we see an impact on UK financials relative to the non-financials, these effects might result from the negative news announcements in the days immediately after the ban was lifted.[13]

[13] In fact, that very day (January 16, 2009), a Reuters headline announced, "Shares crash in Barclays, other UK banks." The article went on to explain, "Shares in Barclays (nyse: BCS) slumped by a quarter on Friday and other UK bank stocks tumbled as worries about capital and writedowns resurfaced and the return of short-selling also hurt,

Insert TABLE 7

Conclusion

We study the impact of the 2008-10 short sale bans using a difference-in-difference approach on all non-U.S. stocks that traded ADRs in the United States. In contrast to common intuition and other recent studies of these bans, we find that the bans actually improved liquidity, and in many cases also reduced volatility. We motivate the liquidity results using a simple variation on the Glosten-Milgrom (1985) model, in which only informed traders use short selling—a realistic assumption in approximate terms. In this framework, a ban on short sales disproportionately restricts informed traders from selling, mitigates the adverse selection problem and thereby lowers spreads.

Why do our results contrast with those in other papers that have analyzed liquidity during the recent short-selling bans? We suspect that the answer lies in the fact that no one else has taken our approach and exactly matched banned non-U.S. stocks with their unbanned ADR counterparts. That methodological difference could explain much of the disparity in results. Moreover, by studying the period after the ban as well as its imposition, we partially avoid the problem of simultaneous enactment of other regulations, such as disclosure requirements, which were not lifted along with the outright ban on short selling. Particularly in our London intraday data, that focuses on the lifting of the UK ban, we dilute the impact of the barrage of bad news on financial stocks that hit along with the start of the bans. In fact, our results do not stand

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dealers said. They said there was no single reason for the fall, but talk of more writedowns following big losses by Bank of America (nyse: BAC) and Citigroup (nyse: C) added to concerns that banks might need to raise more capital."

completely at odds with recent work: within their broader study, Beber and Pagano (2009) examine data from the UK and point out in passing that the UK demonstrated much less liquidity loss from their ban than did the United States and others.

While we do not suggest that banning short selling makes good policy sense in general, our findings indicate that during acute periods of crisis, reining in short-selling temporarily and perhaps on a targeted group of securities, may actually stem liquidity loss. Given on-going policy discussions and changes, we will be able to analyze new evidence on the matter very soon.

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TABLE 1 – SAMPLE DETAILS

We consider eight non-U.S. countries that implemented bans on covered short sales for varying lengths of time. We exclude Canadian stocks from our analysis since their ban started and ended on the same dates as the US ban. Also, Norway implemented a covered ban for financial stocks but none of the impacted shares have ADRs. Thus, we are unable to include this country in the sample.

The final sample consists of 35 stocks from countries that implemented covered bans and have depository receipts that trade on major US exchanges (NYSE, NASDAQ, or NYSE Amex). The list of ADRs was obtained from BNY Mellon's depository receipt database.

Country	Company	Ban Start	Ban End
Austrailia	Alumina BHP Billiton Genetic Technologies James Hardie Industries Lihir Gold Novogen Prana Biotechnology Samson Oil and Gas Sims Metal Management Westpac Banking	9/22/2008	non-financial: 11/19/2008 financial: 5/25/2009
Greece	Coca-Cola HBC Hellenic Telecom National Bank of Greece	first ban: 10/10/2008 second ban: 4/28/2010	first ban: 6/1/2009 second ban: ongoing
Ireland	Allied Irish Banks Bank of Ireland	9/19/2008	ongoing
Italy	Eni Luxottica STMicroelectronics Telecom Italia	financial: 10/1/2008 non-financial: 10/10/2008	financial: 6/1/2009 nonfinancial: 1/1/2009
Netherlands	Aegon ING Group	10/5/2008	6/1/2009
South Korea	Korea Electric Power KT LG Philips LCD POSCO Shinhan Financial SK Telecom Woori Finance	10/1/2008	financial: ongoing nonfinancial: 6/1/2009
Switzerland	Credit Suisse UBS	9/19/2008	1/16/2009
United Kingdom	Barclays HSBC Lloyds Banking Group Prudential Royal Bank of Scotland	9/19/2008	1/16/2009

TABLE 2 – SUMMARY STATISTICS

The following table presents summary statistics for the total sample and each individual country. The daily relative effective spread is calculated as $RES = \frac{2|MQ - P|}{MQ}$ where MQ is the bid-ask midpoint and P is the daily closing price.

The daily Amihud ratio is calculated as $Amihud = \frac{|r|}{V}$ where r is the daily return and V is the daily total volume.

Outliers at the 1% and 99% levels are omitted from the sample.

		Relative Effective Spread			Amihud Ratio			20 Day Rolling Volatility		
Country	Security Type		Pre/Post Ban	Ban		Pre/Post Ban	Ban		Pre/Post Ban	Ban
		mean	0.0097	0.0117	mean	9.06E-08	3.59E-08	mean	0.0233	0.0417
	Non-U.S. Equity	median	0.0025	0.0047	median	1.87E-09	5.81E-09	median	0.0200	0.0363
		std. dev.	0.0025	0.0174	std. dev.	5.43E-07	2.82E-07	std. dev.	0.0129	0.0227
Total Sample										
		mean	0.0077	0.0113	mean	4.93E-07	4.79E-07	mean	0.0226	0.0423
	ADR	median	0.0027	0.0057	median	2.39E-08	4.84E-08	median	0.0183	0.0389
		std. dev.	0.0156	0.0170	std. dev.	1.57E-06	1.32E-06	std. dev.	0.0153	0.0216
		mean	0.0235	0.0217	mean	3.01E-07	2.43E-07	mean	0.0246	0.0338
	Non-U.S. Equity	median	0.0066	0.0070	median	2.92E-09	3.65E-09	median	0.0191	0.0339
		std. dev.	0.0313	0.0306	std. dev.	9.95E-07	8.85E-07	std. dev.	0.0158	0.0129
Austrailia										
		mean	0.0168	0.0207	mean	1.41E-06	1.56E-06	mean	0.0324	0.0597
	ADR	median	0.0058	0.0090	median	1.77E-07	5.57E-07	median	0.0239	0.0588
		std. dev.	0.0243	0.0273	std. dev.	2.56E-06	2.44E-06	std. dev.	0.0218	0.0257
		mean	0.0118	0.0130	mean	2.40E-08	3.29E-08	mean	0.0234	0.0337
	Non-U.S. Equity	median	0.0088	0.0098	median	1.23E-08	1.73E-08	median	0.0223	0.0314
		std. dev.	0.0101	0.0111	std. dev.	3.82E-08	4.48E-08	std. dev.	0.0074	0.0133
Greece										
		mean	0.0058	0.0114	mean	2.68E-07	3.61E-07	mean	0.0197	0.0361
	ADR	median	0.0036	0.0079	median	1.10E-07	1.48E-07	median	0.0188	0.0315
		std. dev.	0.0088	0.0127	std. dev.	4.01E-07	5.59E-07	std. dev.	0.0066	0.0167
		mean	0.0074	0.0200	mean	6.37E-09	1.37E-08	mean	0.0285	0.0611
	Non-U.S. Equity	median	0.0053	0.0127	median	4.01E-09	8.29E-09	median	0.0264	0.0596
		std. dev.	0.0077	0.0212	std. dev.	1.03E-08	1.94E-08	std. dev.	0.0105	0.0214
Ireland										
		mean	0.0027	0.0112	mean	1.09E-07	6.35E-08	mean	0.0197	0.0506
	ADR	median	0.0018	0.0067	median	6.10E-08	1.97E-08	median	0.0179	0.0476
		std. dev.	0.0036	0.0149	std. dev.	1.31E-07	1.29E-07	std. dev.	0.0088	0.0208
		mean	0.0041	0.0135	mean	3.85E-09	1.01E-08	mean	0.0191	0.0331
	Non-U.S. Equity	median	0.0031	0.0083	median	7.76E-10	1.97E-09	median	0.0181	0.0328
		std. dev.	0.0052	0.0165	std. dev.	7.14E-09	1.64E-08	std. dev.	0.0073	0.0060
Italy										
		mean	0.0023	0.0058	mean	4.20E-08	7.27E-08	mean	0.0151	0.0418
	ADR	median	0.0017	0.0040	median	1.89E-08	4.71E-08	median	0.0135	0.0411
		std. dev.	0.0028	0.0073	std. dev.	6.25E-08	8.08E-08	std. dev.	0.0059	0.0098
		mean	0.0007	0.0018	mean	1.06E-09	3.09E-09	mean	0.0246	0.0717
	Non-U.S. Equity	median	0.0005	0.0008	median	7.76E-10	2.39E-09	median	0.0235	0.0718
		std. dev.	0.0008	0.0048	std. dev.	1.04E-09	2.64E-09	std. dev.	0.0095	0.0177
Netherlands										
		mean	0.0037	0.0177	mean	1.11E-08	2.87E-08	mean	0.0187	0.0547
	ADR	median	0.0024	0.0104	median	7.12E-09	1.80E-08	median	0.0182	0.0532
		std. dev.	0.0060	0.0214	std. dev.	1.31E-08	3.11E-08	std. dev.	0.0074	0.0154
		mean	0.0048	0.0074	mean	1.95E-08	1.44E-08	mean	0.0172	0.0294
	Non-U.S. Equity	median	0.0028	0.0035	median	8.89E-09	6.95E-09	median	0.0161	0.0233
		std. dev.	0.0053	0.0097	std. dev.	2.61E-08	2.16E-08	std. dev.	0.0060	0.0176
South Korea										
		mean	0.0031	0.0057	mean	2.61E-07	7.25E-07	mean	0.0174	0.0301
	ADR	median	0.0021	0.0033	median	1.45E-08	6.15E-08	median	0.0167	0.0240
		std. dev.	0.0040	0.0075	std. dev.	1.05E-06	1.62E-06	std. dev.	0.0064	0.0177
		mean	0.0039	0.0191	mean	1.57E-09	3.56E-09	mean	0.0253	0.0519
	Non-U.S. Equity	median	0.0010	0.0147	median	1.06E-09	2.44E-09	median	0.0220	0.0524
		std. dev.	0.0059	0.0171	std. dev.	1.65E-09	3.33E-09	std. dev.	0.0115	0.0126
Switzerland										
		mean	0.0028	0.0120	mean	7.90E-09	2.51E-08	mean	0.0214	0.0569
	ADR	median	0.0020	0.0080	median	4.59E-09	1.43E-08	median	0.0174	0.0596
		std. dev.	0.0029	0.0134	std. dev.	9.11E-09	2.70E-08	std. dev.	0.0113	0.0125
		mean	0.0012	0.0027	mean	5.71E-10	1.59E-09	mean	0.0291	0.0570
	Non-U.S. Equity	median	0.0008	0.0015	median	2.46E-10	6.53E-10	median	0.0258	0.0572
		std. dev.	0.0021	0.0049	std. dev.	1.04E-09	3.42E-09	std. dev.	0.0166	0.0209
United Kingdom										
		mean	0.0043	0.0213	mean	5.14E-08	8.70E-08	mean	0.0224	0.0527
	ADR	median	0.0023	0.0097	median	1.17E-08	3.76E-08	median	0.0181	0.0540
		std. dev.	0.0076	0.0268	std. dev.	1.26E-07	1.25E-07	std. dev.	0.0141	0.0185

TABLE 3 – THE IMPACT OF SHORT-SELLING BANS ON LIQUIDITY AND VOLATILITY

This table reports the results of the following panel regression:

$$Y_{it} = \alpha + \beta(foreigncountry_i) + \gamma(shortsaleban_{it}) + \delta(foreigncountry_i \times shortsaleban_{it}) + \varepsilon_{it}$$

Foreigncountry equals 1 for the non-U.S. stocks and equals 0 for the U.S. depository receipts. *Shortsaleban* equals 1 during the ban period and 0 otherwise. The period of the US ban (9/19/08 – 10/8/08) is not included in the regressions since some ADRs were subject to the US covered ban (thereby leaving us without an appropriate control). Dependent variables are defined in table II.

	RES	Amihud Ratio	Roling Std. Dev.
Ban Effect	0.00686	6.54E-08	0.0236
	0	*8.43E-05*	*0*
Country Effect	0.00292	-5.16E-07	0.00177
	0.433	*0.0579*	*0.584*
Difference-in-Difference	**-0.00196**	**-5.82E-08**	**-0.00383**
	6.63E-08	***0.000663***	***0***
Constant	0.0077	6.28E-07	0.0238
	0.00229	*0.0135*	*0*
R-squared Overall	0.0053	0.0309	0.2886
Number of Stock-ADR Combinations	35	35	35
Observations	44964	45152	42672

Robust p-values in italics

TABLE 4 – COUNTRY SPECIFIC EFFECTS OF SHORT-SELLING BANS

This table reports the results of the following panel regression:

$$Y_{it} = \alpha + \gamma(shortsaleb\,an_{it}) + \sum_{i}\left[\beta(country_{i}) + \delta(country_{i} \times shortsaleb\,an_{it})\right] + \varepsilon_{it}$$

The *country* dummy variable is used to denote each non-U.S. country. *Shortsaleban* equals 1 during the ban period and 0 otherwise. The period of the US ban (9/19/08 – 10/8/08) is not included in the regressions since some ADRs were subject to the US covered ban (thereby leaving us without an appropriate control). Dependent variables are defined in table II.

	RES	Amihud Ratio	Roling Std. Dev.
Ban Effect	0.00686	6.54E-08	0.0236
	0	*8.25E-05*	*0*
Austrilia Country Effect	0.0179	-2.59E-07	0.0087
	0.0268	*0.586*	*0.0783*
Austrailia Difference-in-Difference	-0.000201	-3.00E-08	-0.00839
	0.857	*0.451*	*0*
Greece Country Effect	0.00409	-6.04E-07	-0.000569
	0.574	*0.0344*	*0.922*
Greece Difference-in-Difference	-0.00567	-5.66E-08	-0.013
	0	*0.000713*	*0*
Ireland Country Effect	-0.000349	-6.22E-07	0.00478
	0.98	*0.029*	*0.715*
Ireland Difference-in-Difference	0.00548	-5.80E-08	0.00856
	8.09E-11	*0.000495*	*0*
Italy Country Effect	-0.00374	-6.24E-07	-0.00487
	0.391	*0.0281*	*0.25*
Italy Difference-in-Difference	0.00269	-5.92E-08	-0.00931
	0.0183	*0.000375*	*0*
Netherlands Country Effect	-0.007	-6.27E-07	0.000422
	0.021	*0.0274*	*0.961*
Netherlands Difference-in-Difference	-0.00576	-6.34E-08	0.024
	0	*0.000137*	*0*
South Korea Country Effect	-0.00296	-6.10E-07	-0.00627
	0.447	*0.0318*	*0.189*
South Korea Difference-in-Difference	-0.00435	-6.45E-08	-0.0125
	0	*0.000107*	*0*
Switzerland Country Effect	-0.00381	-6.26E-07	0.00154
	0.571	*0.0275*	*0.859*
Switzerland Difference-in-Difference	0.00902	-6.32E-08	0.00464
	1.35E-08	*0.000145*	*8.31E-05*
United Kingdom Country Effect	-0.00654	-6.27E-07	0.00537
	0.0127	*0.0273*	*0.481*
United Kingdom Difference-in-Difference	-0.0059	-6.43E-08	0.00628
	0	*0.00011*	*1.58E-09*
Constant	0.0077	6.28E-07	0.0238
	0.00128	0.0271	0
R-squared Overall	0.1229	0.0365	0.2559
Number of Stock-ADR Combinations	35	35	35
Observations	44964	45152	42672

Robust p-values in italics

TABLE 5 - IMPACT OF LIFTING THE SHORT-SALE BAN USING HIGH-FREQUENCY DATA: UK FINANCIAL STOCKS VERSUS THEIR AMERICAN DEPOSITORY RECEIPTS

This table reports the results of the following panel regression:

$$Y_{it} = \alpha + \beta(country_{it}) + \gamma(shortsalelift_{it}) + \delta(country_{it} \times shortsalelift_{it}) + \varepsilon_{it}$$

Country equals 1 for the UK stock and equals 0 for the U.S. depository receipt. *Shortsalelift* equals 1 in the post-ban period and equals 0 during the ban. Short sales were banned for UK financial stocks during the first two weeks of the period we study, but allowed in the following two weeks. Short sales were never banned for corresponding ADRs during this time. The dependent variables are relative effective spread (RES), transaction-weighted relative effective spread (TRES), and contemporaneous realized volatility (all defined in Table 1). Robust p-values for the coefficients are in italics below coefficient estimates. Data obtained from Bloomberg.

	RES	TRES	Realized Volatility
Time Period Effect (*shortsalelift*)	0.0010	0.0010	0.0020
	0.0000	*0.0000*	*0.0001*
Country Effect	0.0030	0.0040	0.0000
	0.6760	*0.6470*	*0.9780*
Difference-in-Difference	**0.0080**	**0.0090**	**0.0004**
	0.0020	***0.0010***	***0.5530***
Constant	0.0020	0.0020	0.0006
	0.0000	*0.0000*	*0.0925*
Observations	205	205	205
R-squared Overall	0.1050	0.1060	0.1296

TABLE 6 – IMPACT OF LIFTING THE SHORT-SALE BAN USING HIGH-FREQUENCY DATA: UK NON-FINANCIAL STOCKS VERSUS THEIR AMERICAN DEPOSITORY RECEIPTS

This table reports the results of the following panel regression:

$$Y_{it} = \alpha + \beta(country_{it}) + \gamma(shortsalelift_{it}) + \delta(country_{it} \times shortsalelift_{it}) + \varepsilon_{it}$$

Country equals 1 for the UK stock and equals 0 for the U.S. depository receipt. *Shortsalelift* equals 1 in the post-ban period and equals 0 during the ban. However, since these regressions use securities of non-financial firms, short selling was not banned in either period for both the ADRs and UK shares. The dependent variables are relative effective spread (RES), transaction-weighted relative effective spread (TRES), and realized volatility (all defined in Table 1). Robust p-values for the coefficients are in italics below coefficient estimates. Data obtained from Bloomberg.

	RES	TRES	Realized Volatility
Time Period Effect	0.0000	0.0000	0.0000
	0.0000	*0.9470*	*0.0638*
Country Effect	-0.0010	0.0000	0.0000
	0.0140	*0.0320*	*0.7630*
Difference-in-Difference	**0.0000**	**0.0000**	**0.0000**
	0.0100	***0.5190***	***0.4880***
Constant	0.0020	0.0020	0.0002
	0.0000	*0.0000*	*0.0000*
Observations	1024	1024	1024
R-squared Overall	0.0990	0.0640	0.0009

TABLE 7 – IMPACT OF LIFTING THE SHORT-SALE BAN USING HIGH-FREQUENCY DATA: UK FINANCIALS STOCKS VERSUS UK NON-FINANCIAL STOCKS

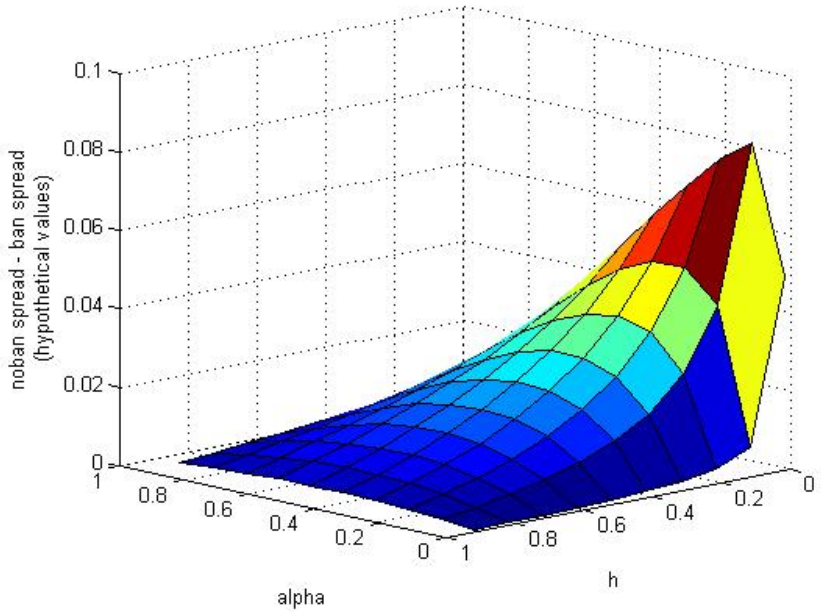
This table reports the results of the following panel regression:

$$Y_{it} = \alpha + \beta(financial_{it}) + \gamma(shortsalelift_{it}) + \delta(financial_{it} \times shortsalelift_{it}) + \varepsilon_{it}$$

Financial equals 1 for the financial (banned) stocks and equals 0 for the non-financial (unbanned) stocks. *Shortsalelift* equals 1 in the post-ban period and equals 0 during the ban. The dependent variables are relative effective spread (RES), transaction-weighted relative effective spread (TRES), and realized volatility (all defined in Table 1). Robust p-values for the coefficients are in italics below coefficient estimates. Data obtained from Bloomberg.

	RES	TRES	Realized Volatility
Time Period Effect	0.0000	0.0000	0.0000
	0.1780	*0.2250*	*0.9570*
Financial Effect	0.0037	0.0045	0.0003
	0.1930	*0.2160*	*0.4670*
Difference-in-Difference	**0.0084**	**0.0102**	**0.0025**
	0.2150	***0.2120***	***0.0000***
Constant	0.0010	0.0011	0.0003
	0.0000	*0.0000*	*0.0001*
Observations	630	630	630
R-squared Overall	0.1801	0.1742	0.2386

Figure 1


noban spread - ban spread
(hypothetical values)
0.1
0.08
0.06
0.04
0.02
0
1
0.8
0.6
0.4
0.2
0
alpha
1
0.8
0.6
0.4
0.2
0
h

Appendix - Computing the Bid-Ask Spread in Cases of "Ban" and "No-Ban"

We first consider $P(\tilde{V}=\overline{V}|B)$ and $P(\tilde{V}=\underline{V}|B)$

For the no-ban case, we have the following:

$$P(\tilde{V}=\overline{V}\mid B)=\frac{P(\tilde{V}=\overline{V})P(B\mid\tilde{V}=\overline{V})}{P(\tilde{V}=\overline{V})P(B\mid\tilde{V}=\overline{V})+P(\tilde{V}=\underline{V})P(B\mid\tilde{V}=\underline{V})}=\frac{\theta\left(\frac{1-\alpha}{2}+\alpha\right)}{\theta\left(\frac{1-\alpha}{2}+\alpha\right)+(1-\theta)\left(\frac{1-\alpha}{2}\right)}$$

and

$$P(\tilde{V}=\underline{V}\mid B)=\frac{P(\tilde{V}=\underline{V})P(B\mid\tilde{V}=\underline{V})}{P(\tilde{V}=\overline{V})P(B\mid\tilde{V}=\overline{V})+P(\tilde{V}=\underline{V})P(B\mid\tilde{V}=\underline{V})}=\frac{(1-\theta)\left(\frac{1-\alpha}{2}\right)}{\theta\left(\frac{1-\alpha}{2}+\alpha\right)+(1-\theta)\left(\frac{1-\alpha}{2}\right)}$$

Identical results are obtained for the ban case. Next we want to find expressions for $P(\tilde{V}=\overline{V}|S)$ and $P(\tilde{V}=\underline{V}|S)$ for both scenarios. First, for the no-ban case we have

$$P(\tilde{V}=\overline{V}\mid S)=\frac{P(\tilde{V}=\overline{V})P(S\mid\tilde{V}=\overline{V})}{P(\tilde{V}=\overline{V})P(S\mid\tilde{V}=\overline{V})+P(\tilde{V}=\underline{V})P(S\mid\tilde{V}=\underline{V})}=\frac{\theta\left(\frac{1-\alpha}{2}h\right)}{\theta\left(\frac{1-\alpha}{2}h\right)+(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha\right)}$$

$$P(\tilde{V}=\underline{V}\mid S)=\frac{P(\tilde{V}=\underline{V})P(S\mid\tilde{V}=\underline{V})}{P(\tilde{V}=\overline{V})P(S\mid\tilde{V}=\overline{V})+P(\tilde{V}=\underline{V})P(S\mid\tilde{V}=\underline{V})}=\frac{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha\right)}{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha\right)+(\theta)\left(\frac{1-\alpha}{2}h\right)}$$

For the ban case we obtain

$$P(\tilde{V}=\overline{V}\mid S)=\frac{\theta\left(\frac{1-\alpha}{2}h\right)}{\theta\left(\frac{1-\alpha}{2}h\right)+(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha h\right)}$$

$$P(\tilde{V}=\underline{V}\mid S)=\frac{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha h\right)}{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha h\right)+(\theta)\left(\frac{1-\alpha}{2}h\right)}$$

The above results imply that the ask values are identical for *c*=0 and *c*=1. In particular, we have

$$a = \overline{V}\left(\frac{\theta\left(\frac{1-\alpha}{2}+\alpha\right)}{\theta\left(\frac{1-\alpha}{2}+\alpha\right)+(1-\theta)\left(\frac{1-\alpha}{2}\right)}\right)+\underline{V}\left(\frac{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha\right)}{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha\right)+(\theta)\left(\frac{1-\alpha}{2}h\right)}\right)$$

The bid for the no-ban case ($c = 1$) is given by

$$b_1 = \overline{V}\left(\frac{\theta\left(\frac{1-\alpha}{2}h\right)}{\theta\left(\frac{1-\alpha}{2}h\right)+(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha\right)}\right)+\underline{V}\left(\frac{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha\right)}{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha\right)+(\theta)\left(\frac{1-\alpha}{2}h\right)}\right)$$

Similarly for the ban case ($c = 0$) the ask price is given by

$$b_0 = \overline{V}\left(\frac{\theta\left(\frac{1-\alpha}{2}h\right)}{\theta\left(\frac{1-\alpha}{2}h\right)+(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha h\right)}\right)+\underline{V}\left(\frac{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha h\right)}{(1-\theta)\left(\frac{1-\alpha}{2}h+\alpha h\right)+(\theta)\left(\frac{1-\alpha}{2}h\right)}\right)$$