

**Prohibitions versus Constraints:
The 2008 Short Sales Regulations***

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Abstract

We study the effects of the short sales regulations issued during the financial crisis of 2008. Specifically, we study the emergency order that effectively restricted naked short selling for nineteen stocks, and the outright ban on all short selling of financial stocks. We document that both regulations were effective in reducing settlement and delivery failures. However, they also resulted in large declines in short selling, and large increases (seven-fold for the emergency order) in the cost of short selling. The regulations provide a unique opportunity to test the empirical implications of Diamond and Verrecchia (1987), which distinguishes between outright prohibitions on short selling and constraints on short selling that merely make them more costly. Consistent with the model, we find that both the emergency order and the ban resulted in significant reductions in market quality. Further, we find that the ban decreased liquidity and increased the informativeness of short sales, and that both these changes were especially strong for stocks with listed options. As sophisticated, informed traders are more likely to make use of options as a substitute for short sales, this result supports the Diamond and Verrecchia (1987) model's prediction that some types of short sale constraints can actually increase the information content of short sales.

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The financial crisis of 2008 touches many areas of finance, and one group upon which regulators have focused is short sellers. Short sellers were blamed for some of the patterns in stock prices, and regulators imposed temporary rules to limit short sellers' effect on market prices. These rules offer a unique opportunity to see the effect short sellers have on markets. The rules affected two different aspects of short selling: the way short sellers must borrow stock and their ability to trade. In this paper, we provide some descriptive evidence as to the liquidity, price impact, securities lending fees, and trading patterns around the rules. In addition, we describe the effectiveness of the rules at mitigating illegal shorting practices. Furthermore, we use the short sales regulations as a setting in which to test theoretical predictions comparing short sales constraints to short sales prohibitions.

Diamond and Verrecchia (1987), DV hereafter, construct a model of short sellers' effect on market prices, and previous work has validated some of the empirical predictions of the model. One aspect of the model that is little explored is the difference between outright prohibitions on short selling versus constraints that merely make short selling more costly. DV argue that prohibitions on short selling drive all short sellers from the market, both those trading on private information (the "informed") and those trading for liquidity reasons (the "uninformed"). As a result, market liquidity and the extent to which prices reflect private information are reduced. On the other hand, DV argue that merely making short selling more costly tends to drive out more uninformed short sellers than informed. As a result, increasing shorting costs increases the informativeness of short sales, even as they likely reduce liquidity by driving out uninformed traders. The two distinct short selling regulations in 2008 provide a unique opportunity to test the distinction between complete prohibition and partial constraints on short selling in the DV model.

The first short selling regulation, adopted in June of 2008, was the Emergency Order, or EO hereafter. The order required investors wishing to short sell a selected group of stocks to borrow, or arrange to borrow, the stock before trading, rather than merely locating a potential lender, as was previously required. This rule increased the costs of short selling in two ways. First, by requiring borrowing before sales, short sellers had to borrow stock three days earlier than they would otherwise have to; prior to the rule, short sellers did not have to borrow until three days after the trade, and those who closed out their position before then did not have to borrow at all. Second, by increasing the overall demand for borrowing shares, the rule led to a dramatic increase in fee charged by share lenders. The EO, however, does not prohibit short selling. And, as a result, the EO fits closely the costly short sale constraints, rather than a prohibition, modeled in DV.

The second regulation, the Short Selling Ban, henceforth “the Ban”, prohibited short selling of a large group of financial stocks. Although market makers were exempt from this prohibition, the ban made it impossible for other market participants to short sell financial shares. Thus, this second regulation fits closely with the short selling prohibitions modeled in DV. The market maker exemption from the ban, however, is important. Investors prohibited from short selling can replicate a short position by buying and selling a combination of options from an options market maker. Since market makers were exempt from the ban, they could short sell directly in order to hedge their position when taking the other side of a synthetic short. In effect, a perfect substitute for short selling was available for stocks that had traded options. As a result, only those stocks covered by the ban that also did not have traded options actually experienced a true prohibition in the sense of DV. Furthermore, the options substitute was only available to

investors sophisticated enough to understand and implement the options trading strategy. Hence the ban likely increases the proportion of informed short sellers.

Our ability to analyze distinctions among short sale rules is inherently linked to our ability to measure those distinctions. Accordingly, we use several databases to analyze changes in short-selling patterns around the regulations. The first database is comprised of three variables: rebate rates from multiple lenders, the availability of shares for borrowing and aggregate rebate rates. In addition, we employ a collection of other proxies for short selling difficulty including the incidence of delivery failures and the status on threshold lists.

To get a complete picture of the effect of the regulations, it is also important to understand the volume of short sales around the time of the rules. To this end, we have obtained a second database of intra-daily short sales data from several of the major exchanges. This database is similar to the Regulation SHO database, but instead of ending in July 2007, it covers all of 2008 including both of the short sales regulation periods.

We find that both the emergency order and the ban were associated with some important changes in short selling activity. We find that short selling costs increased significantly for stocks affected by the emergency order. Specifically, the average cost of short selling increased nearly six-fold for firms subject to the EO. In addition, the overall volume of short selling decreased significantly both during the ban and during the Emergency Order. Finally, both regulations were associated with a reduction in short sellers failing to deliver shares to buyers. The daily dollar volume of fails-to-deliver decreased nearly 80% following the ban, which provides strong evidence that the regulations were effective at mitigating delivery failures.

We also find that the changes in market quality and the informativeness short sales are consistent with DV. The results strongly suggest that market quality, as measured by r-squared, decreased for all sample stocks during the EO period. Further supporting the predictions of the DV model, the ban reduced the average level of market quality for all measures, and we find the largest reduction in stocks with listed options. Finally, the ban affected the informativeness of short sales in a manner consistent with the DV model. Specifically, the ban increased the informativeness of short sales for affected stocks, especially those with listed options.

Ours is not the only study to examine the effects of the ban and EO on market quality. Bouton- and Braga-Alves (2009) and Bris (2009) examine changes in market quality around the EO, and Boehmer, Jones and Zhang (2009) examine changes around the ban. Our study is different in several dimensions. First, we show the regulations were effective in dramatically reducing settlement and delivery failures, which were the key motivation establishing them. Second, using a unique database of equity loans, we examine effect of the rules on the direct costs of short selling and show they increased dramatically. Finally, having established that the regulations increased costs, we have the unique opportunity to consider hypotheses that link cost-driven changes in the composition of short sellers to market quality. Our novel and unintuitive finding that the ban's negative effect on market quality is stronger for stocks with listed options demonstrates the importance of the connection between regulation, market quality, short sales costs, and the composition of short sellers.

The rest of this study is organized as follows. In section II, we describe the short selling rules in detail, review the relevant literature and develop hypothesis on the impact of the rules on both market quality and the informativeness of short sales. In Section III, we describe our data and the construction of variables used in our analyses. In Section IV, we provide descriptive

evidence as to the direct consequences of the 2008 short selling rules with respect to short selling costs, delivery failures and short selling patterns. In Section V, we test hypotheses on the effects of the rules on liquidity and overall market quality as implied by the Diamond and Verrecchia (1989) model and in Section VI, we test hypotheses on the informativeness of short sales as implied by the Diamond and Verrecchia (1989) model. Finally, section VII concludes.

II. Background & Hypothesis Development

A. The SEC's Short Selling Rules in 2008

On July 15th, 2008, the SEC announced an emergency rule which required short sellers to borrow stock before short selling. The rule affected 19 stocks: 17 primary dealers in treasury securities and Fannie Mae and Freddie Mac. The rule became effective on July 21st, was extended on July 29th and expired on August 12th. Anecdotal evidence indicates that initially, there was uncertainty about how the pre-borrow requirement would be implemented, and what the requirement meant operationally. The SEC released clarifications in the days that followed. In addition, there was an expectation that the SEC would likely apply the rules to the entire market in the near future.¹

Before passage of the order, settlement took place three days after the sale, or in market parlance, $t+3$. In other words, short sellers would conduct a sale in the spot market for stock, which established the economic position, three days before cash would be exchanged for borrowed shares. However, as shown in Evans, Geczy, Musto and Reed (2009), short sellers

¹ The SEC commissioner, Paul Atkins, said of the Emergency Order, "You can view it as a pilot." Source: <http://www.bloomberg.com/apps/news?pid=newsarchive&sid=aX0lxgEh8SfY>

would sometimes fail to deliver shares on the settlement day. This practice, also known as naked short selling, was the intended target of the emergency order.² Anecdotal evidence suggests that brokers borrowed in anticipation of short selling demand on behalf of their clients; in other words, brokers pre-borrowed shares so that clients could short sell even if these clients hadn't expressed any interest in short selling.

On September 17th, 2008, the SEC issued new rules addressed at short selling which became effective at 12:01am ET on September 18th, 2008.^{3,4} That same evening, the SEC followed the Financial Services Authority of the U.K. by imposing a temporary ban on short selling. The ban began on September 19th, 2008 and was initially set to expire in 10 days. The ban was extended on October 2, 2008 and expired on October 8th, 2008.

Initially, the short sales ban covered 799 financial stocks selected by the SEC. Subsequently, the SEC allowed exchanges to determine which firms would be included in (and excluded from) the ban. Eventually, over 1000 firms elected to be covered by the short selling rules, with many firms entering and exiting the list of firms covered by the ban. Although there initially was some controversy, the final rule exempted market makers from the ban and the delivery requirements.⁵

B. *Literature*

²See the Security and Exchange Commission's Release 58166, "Emergency Order" available at: <http://www.sec.gov/rules/other/2008/34-58166.pdf>

³ The new rules had several key elements. First, the new rules forced delivery to take place within three days and imposed severe penalties on those involved in delivery failures. In addition, the SEC implemented an anti-fraud provision that targets fraudulent short selling transactions. For more details, see <http://www.sec.gov/rules/other/2008/34-58572.pdf>.

⁴ Although disclosure and the anti-fraud provisions are clearly costs to short sellers, in this work we will focus on the short selling prohibition and the delivery requirements.

⁵ The SEC clarified the initial terms of the ban two days after the initial rule on September 21, 2008. <http://www.sec.gov/rules/other/2008/34-58611.pdf>

Even though the events under study have just happened, there are already a number of studies that attempt to analyze the effects.⁶ Bouton and Braga-Alves (2009) focuses on the EO, and the paper finds that announcement-day returns of the 19 EO stocks are not significantly different than a matched sample control group's returns. However, the paper finds evidence that the restrictions had a negative impact on various measures of liquidity and price informativeness. Similarly, Bris (2009) finds that market quality is significantly worse for the 19 EO stocks than comparable firms. Interestingly, the paper finds that the performance of the 19 EO stocks is worse than comparable stocks. Boehmer, Jones and Zhang (2009) focus on the ban, and they find that the shorting ban is associated with a significant share price increase for affected stocks, and stocks subject to the ban had lower market quality as measured by spreads, price impacts, and intraday volatility.⁷ Our study also examines the effects of the regulations on market quality. However, unlike the studies above, we consider how regulations' effects on market quality are linked to their effects on the relative proportion of short sellers in the market.

In addition, our paper looks at other aspects of the short sales regulations previously unexamined. First, we approach the regulations from both a trading and a lending perspective. We show that the regulations affected the rate of securities lending. Second, in our analysis, we directly observe changes in short sales volume, which has been unaddressed in the above studies. Third, we present evidence as to the effectiveness of the rules. Although the papers listed above examine the indirect effects of the rules, we examine whether the rules directly reduced the incidence of certain short selling techniques that some consider abusive. Finally, we use the changes in short selling restrictions as a setting in which to test theory regarding price discovery

⁶ In addition, there have been several reports presented by practitioners groups, which are available by request.

⁷ Several papers have examined the changes in short sales regulations in other countries (e.g., Clifton and Snape, 2009).

in the face of short sales constraints. Thus, our paper provides novel descriptive evidence regarding the short sales regulations of 2008, and in addition, tests hypotheses to substantiate relevant theoretical models.

Initial work in the area of short selling such as Seneca (1967) and Asquith and Muelbroek (1997) attempted to determine whether short selling was a positive or a negative signal. Since then, there has been an increased focus on the effects of short sale constraints, with particular attention to the connection between short selling and valuation. Miller (1977) shows that stocks with short sale constraints are likely to be overvalued. Furthermore, empirical work such as Asquith, Phatak, and Ritter (2005), Diether, Malloy, and Scherbina (2002), Ofek and Richardson (2003), and Boehmer, Jones and Zhang (2007) has verified that short sale constraints and heterogeneous expectations lead to low returns.

The focus of Diamond and Verrecchia (1987) is on the speed of adjustment of prices, and empirical work has verified many of the predictions of that model. Reed (2007) verifies that announcement-day returns are more volatile when short selling is expensive. Furthermore, Jennings and Starks (1986) use intra daily trade data to compare the speed of stock price adjustment for optionable firms to non-optionable firms. Skinner (1990) finds that the information content of firms' earnings announcements is lower after exchange traded options are listed on their stocks. Few papers, however, directly test the Diamond and Verrecchia predictions that changes in the relative proportion of informed and uninformed short sellers can affect markets. For reasons we discuss below, the rules in question provide a unique opportunity for us to do so.

C. Hypothesis Development

Diamond and Verrecchia (1987) present a model with two types of short sellers: informed short sellers who trade because they have a private signal that a stock is overvalued, and those who short sell for other reasons, or uninformed short sellers. While DV describe their uninformed short sellers as investors shorting for liquidity reasons, any short seller without private information would qualify. For example, hedge funds shorting to hedge an index arbitrage strategy would qualify, as would a convertible bond market maker shorting the stock to hedge inventory risk, or a constrained broker filling a sell order, or an irrational short-selling noise trader. Short sales become more informative, and prices better reflect private information, as the number of informed short sellers that are allowed to trade increases. The same is true as the ratio of informed to uninformed short increases. However, reducing the ability of liquidity traders to short also reduces liquidity and overall market quality.

According to the DV model, therefore, the effect of any short selling restriction is going to be governed by how it changes the quantity and composition of the types of traders willing to short sell. We now consider how both the emergency rule and the ban were likely to affect these variables.

The short sales ban provides a unique setting in which to test the consequences of removing informed short selling. For stocks in which a synthetic short position cannot be replicated by trading in options, the effect of the ban is straightforward. Since the ban prohibited short selling by all parties except market makers, it largely eliminated the ability of informed short sellers to trade. As a result, for non-option stocks, the ban likely decreased the informativeness of short sales. By also banning liquidity traders from shorting, it likely reduced liquidity and overall market quality as well. At the heart of DV is the notion of illiquidity. We implement a measure proposed by Amihud (2002) which is based on the impact of trading activity on returns.

Amihud's illiquidity ratio has a theoretical link to the Kyle (1985) lambda in that it measures the sensitivity of stock prices to trading volume.

On the other hand, for banned stocks with traded options, the ability of sophisticated informed traders to short sell was largely unaffected, as they could create synthetic short positions by trading in options (eg., Sorescu (2000) and Danielsen and Sorescu (2001) and Conrad (1989), Evans, Geczy, Musto and Reed (2009)). However, it likely decreased the ability of noise traders to short, since they are likely not sufficiently sophisticated to trade in options. Furthermore, a synthetic short is unlikely to meet the needs of some liquidity short sellers, such as broker-dealers with insufficient inventory filling sell orders. Hence by increasing the proportion of informed to uninformed short sellers, the ban likely increased the informativeness of short sales of affected stocks that had listed options. Microstructure models generally predict that a higher proportion of informed traders leads to lower liquidity (e.g., Kyle, 1985). Therefore the adverse effect of the ban on liquidity and market quality was likely higher for option-listed stocks.

Thus we have our first hypotheses:

H1a: Among ban stocks, liquidity and market quality decreased more for those with traded options than for those without.

H1b: Among ban stocks, short sales became more informative for those with traded options than for those without.

The Emergency Rule forced short sellers to borrow or arrange to borrow shares before trading. Prior to the rule, it was only necessary to locate a lender, but it was not necessary to actually borrow shares unless the short seller maintained her short position for more than three

trading days. As borrowing shares is costly, the emergency rule imposed a new cost on short sellers only intending to hold their position for three days or less. In addition, requiring these short sellers to borrow likely caused an outward shift in the demand schedule for share loans. Since the supply schedule of share loans is upward sloping most of the time (Kolasinski, Reed and Ringgenberg, 2009), an outward shift in the demand schedule likely caused an increase in the cost of borrowing for all short sellers, as we confirm in Section IV.

Diamond and Verrecchia (1987) argue that increases in borrowing cost are more likely to impact liquidity traders than informed traders, as costs are less likely to deter a trader with strong bearish information than a noise trader or liquidity trader. As a result, the increased costs associated with the emergency order likely increased the ratio of informed to uninformed short sellers. Reduction in shorting for liquidity reasons also likely decreased overall liquidity and market quality. Thus we present the following hypotheses:

H2a: Upon the adoption of the Emergency Rule, liquidity and overall market quality decreased more for stocks impacted the by emergency rule relative to comparable stocks not impacted.

H2b: Upon the adoption of the Emergency Rule, the informativeness of short sales increased more for stocks impacted by the emergency rule than for comparable stocks not impacted

Our study is subject to some limitations. The short selling rules above were passed in a time of extreme market fear, and the rules covered the financial institutions at the center of those fears. As such, the introduction of these rules is not a perfect natural experiment. In addition, because the financial crisis affected nearly all firms in the market, there is not an obvious control group for our treatment firms. And finally, it is difficult to disentangle the direct effect of the short sale rules from the signaling effect that arises as the government attempts to protect a

specific set of securities. Even with these limitations in mind, we seek to provide novel descriptive evidence regarding both regulatory events and use the events to test prevailing theories of short selling constraints.

III. Data and Control Groups

We employ a number of databases to examine the effects of short selling around the 2008 rules. In addition to the usual data on stock prices and accounting variables, we use short interest, short sales volume, threshold lists, failures to deliver, and transaction data from the equity loan market. In this section we will describe each database and our process of preparing the data for analysis.

A. Data

We use a database of short sales volume over the 2008 rule making period. As described in Diether, Lee and Werner (2009), our short sales volume database is an intra-daily record of short sales. The data was originally made available as part of the Securities and Exchange Commission's Regulation SHO, which required exchanges to make short sales volume data public. The exchanges have made these data public with short sales volume over the period January 2005 through July 2007. Even though these databases are not publicly available in the recent rule making period, we have obtained the short sales volume databases covering this more recent period through December 2008. Specifically, we have obtained databases from the NASDAQ Exchange, the NASDAQ Trade Reporting Facility, the NYSE Trade Reporting

Facility, NYSE/ARCA, and the FINRA Alternative Display Facility, generally under special permission from those exchanges.^{8,9} As described in Boemer, Jones and Zhang (2008) one important deficiency of the short sales volume data is the fact that the volume data are short sale initiations—we do not know when the short position is covered. As a result, this database (and all others used in the literature) provides no information on the duration of short positions.

We also employ threshold lists, which identify stocks with a relatively large number of delivery failures. Threshold securities have delivery failures amounting to at least 10,000 shares and one-half of one percent of shares outstanding. The threshold lists were mandated as part of the SEC's Regulation SHO, and they identified stocks with more stringent delivery requirements meant to curtail prolonged delivery failures. As we will describe further in the results section, the so-called “hard close out” requirement of the SEC's September 17th, 2009 rule made the threshold list less important.¹⁰ We obtain the threshold lists from the exchange websites of the NYSE, AMEX and NASDAQ from the period January 2005 through December 2008.^{11,12}

We also use a direct measure of the number of delivery failures from the SEC. The delivery failure data is balance of the total number of shares failed to deliver. The SEC only reports the share values of fails to deliver if the quantity failed exceeds 10,000 shares. If there is no record of fails reported by the SEC for a given firm on a given day, we record zero failed

⁸In the interest of expediency, we have only used NYSE ARCA in this draft. We plan to read and use the remaining databases in later versions of the paper

⁹Conversations with exchange officials indicate that these databases represent at least 50% of short sales volume.

¹⁰ <http://www.sec.gov/rules/final/2008/34-58773.pdf>.

¹¹http://www.nyse.com/regulation/memberorganizations/Threshold_Securities, <http://www.amex.com/amextrader/amextrader/tradingData/RegSHO>, and <ftp://ftp.nasdaqtrader.com/symboldirectory/regsho>.

¹²Two other exchanges, CE and ArcaEx, keep a list of threshold securities. We focus our analysis on the three largest exchanges.

shares on that date (Diether and Werner, 2009). This data set is publicly available but it is reported in a quarterly release that is available from the SEC after a two month delay.¹³

We employ a database from the equity loan market that captures equity loan rebate rates and equity loan volume. Our data provider is a data aggregator for twelve equity lenders which represent 36% of the securities lenders by number. The database comprises rebate rates and equity loan volumes for 6,972 unique U.S. equities.

We combine all of the short selling databases together with stock price and volume data from CRSP and financial statement data from Compustat. Table 1 provides the descriptive statistics for our sample across the all of the short selling databases. Panel A details the levels of short volume and interest for 14 of the 19 firms included in the July 2008 emergency order.¹⁴ Panel B describes the short selling characteristics of the firms covered by the October 2008 short sales ban. Panel C describes the short selling characteristics of all financial firms, which we define loosely as all firms with a one-digit SIC code of six.

B. Controls

As mentioned above, the nature of the events under investigation makes choosing a control group very difficult. In addition, the ban covered a large portion of the financial sector, so for our analyses of the ban, finding a control group within the same industry is difficult. We attempt to overcome these difficulties in choosing control groups while acknowledging their existence.

¹³ For more details, please visit: <http://www.sec.gov/foia/docs/failsdata.htm>

¹⁴ We are unable to obtain sufficient data on five of the firms covered in the EO: BNP Paribas, Daiwa Securities, Allianz SE, Bank of America, and Royal Bank of Scotland.

Seventeen of the 19 firms selected by the SEC for the July Emergency Order are (or were) prime brokers in U.S. Treasury securities. For this group of 17, we use as a control group all financial firms in the sample which fall into similar Global Industry Classification Standard (GICS) industry subgroups and hence have similar core business lines. Specifically, the control group consists of 66 firms from four GICS industry subgroups from Compustat: Diversified Banks, Investment Banking and Brokerage, Diversified Capital Markets, and Other Diversified Financial Services. The remaining two firms affected by the EO were Fannie Mae and Freddie Mac. We exclude them from our analysis since, as government sponsored entities, they have no plausible control.

For the short sales ban, we conduct two sets of tests. In the first set, we compare how the ban affected a treatment group of firms that were both affected by the ban and had listed options to a control group of firms that were affected by the ban but had no listed options. We also conduct a set of tests where we compare firms subject to the ban to a control group not subject to it.

To test the differential impact of short sales volume for option firms versus non-options firms, we implement a propensity score matching technique to control for systematic differences between the two groups. Mayhew and Mihov (2004) identify three factors that influence the selection of a stock for options listing: turnover, volatility and market capitalization. We include two additional factors that are important to and have significant variation within the financial industry: leverage and profitability. Using these five conditioning variables, we obtain a propensity score for each option firm and match it (with replacement) to its closest match firm without listed options with the same two-digit SIC code.

We follow nearly the same matching procedure for comparing ban firms to non-ban firms. However, because the ban covered nearly the entire financial sector, we drop the requirement that the matched firm be in the same industry. For both control samples, we exclude 20% of the matches with the largest differences in propensity scores (i.e., the worst matches).

We start in Section IV by examining how the rules changed market statistics such as short selling costs, short selling volume and settlement. We establish that short selling costs increased significantly, and we use this cost increase as the basis for our hypotheses regarding tests of the Diamond and Verrecchia (1987) model that follow. We start by testing the model's prediction about the effect of the short selling rules on market quality in Section V. We then turn to tests of the model's prediction about the information content of short sales in Section VI.

IV. How the markets changed after the rules

In this section, we document the effects of the short selling regulations that took place in 2008. We find an increase in short selling costs, delivery failures and short selling that occurred upon the adoption of the rules.

A. The Cost of Borrowing in 2008

As described above, the Emergency Order increased the difficulty of short selling by requiring short sellers to borrow stock in advance of the short sale. As a first pass, we take a look at the borrowing costs short sellers would face if they were to initiate a short sale. Figure 3 shows that there is a *huge* spike in borrowing costs on July 21st, 2008, the first day the pre-

borrow requirement becomes effective. The spike lasts about 4 days and then returns, slowly, to a pre-rule level. Notice that the spike in borrowing costs shows up as a decrease in the rebate rate that short sellers receive on their cash collateral. Specifically, on July 14th, the day before the announcement of the emergency rule, the average rebate rate on the 19 stocks was 1.68, approximately 38 basis points below the federal funds rate. On July 21st, the average rebate rate dropped to -0.96, or about 287 basis points below the federal funds rate.

A simple statistical test also makes the point. In Table 2, we present the results from a difference-in-differences t-test comparing rebate rates. In Panel B, we see that average *Specialness*, which is defined as the excess loan fee as in Geczy, Musto and Reed (2002), is 19 basis points for the 19 Emergency Order firms before the Order took effect. During the period over which the order is in place, the average specialness increases to 143 basis points on average, which is statistically significant. Moreover, the increase for the sample firms was significantly larger for the EO firms than the increase for non-EO firms over the same period.

Overall, we find strong evidence that one of the direct costs of short selling, the cost of borrowing stock, increases dramatically in the period of the Emergency Order. Hence, one of the potentially unintended consequences of the EO was to making equity lending more difficult and expensive.

B. Settlement and Delivery Failure

Table 2 shows the average pattern of delivery failures before and during the EO. The average number of shares that are failed deliveries as a fraction of the total number of shares outstanding is 0.0005 for the 19 EO stocks before the order took effect. After the order took

effect, that fraction drops to 0.0001. The drop is statistically significant at the 5% level. Interestingly, similar firms during the same period show an increase in the ratio of failed deliveries, although the increase is not statistically significant. The average fails-to-shares outstanding ratio is 0.0008 before the EO and 0.0015 during the EO. The difference-in-differences is also significant—the reduction in delivery failures for the EO firms was larger than the control group of similar firms. Hence, to the extent that the Emergency Order was issued to prevent settlement failures, the Emergency Order was effective.

Turning our attention to the short sale ban, we focus on the tightening of delivery requirements for the entire cross section of stocks, which occurred the day before the announcement of the ban. The amendment to Regulation SHO, Rule 204T required sellers, both long- and short-sellers, to deliver securities within three days. The consequence for delivery failure became much more severe. The SEC states that if delivery does not take place within three days, then “any broker-dealer acting on the short seller's behalf will be prohibited from further short sales in the same security unless the shares are not only located but also pre-borrowed.”¹⁵

Figure 2, Panels A and B demonstrate the effects of the hard closeout rules implemented by the SEC. Panel A of Figure 2 shows the number of firms on the threshold lists for 2005 through 2008. Beginning in 2005, stock exchanges were required to release a list of securities which had a relatively large number of delivery failures. The figure in Panel A shows that the number of firms on the threshold lists is relatively stable from 2005 through 2007. In early 2007, the number of firms on the threshold lists begins to trend upward. The peak is reached on July

¹⁵ The Securities and Exchange Commission’s release 2008-204 available at the following web address: <http://www.sec.gov/news/press/2008/2008-204.htm>.

24, 2008, during the EO period, when there are 750 firms on the threshold list. After the ban, the number of stocks on the threshold list drops dramatically, and we see that the trough is reached on December 15th, 2008, a point at which there are only five stocks on the threshold lists. On the last day that that data are available, December 31st, 2008, there are 78 stocks on the threshold list. The drop in the number of firms on the threshold list is strongly statistically significant (untabulated).

Panel B of Figure 2 shows that delivery failures dropped off dramatically at about September 2008. The economic significance of this drop in FTDs is striking—from September 2008 to the end of the year, there was a 98% decrease in delivery failures. On September 19th, 2008, the first day of the ban, there were \$13 Billion in failed deliveries. On December 31st, 2008, the last day for which data are available, there were \$325 Million in failed deliveries. This sharp drop-off is likely the result of the “hard-close-out requirement” and is strongly statistically significant (untabulated).

Overall, the analysis on the fails-to-deliver, as well as the threshold lists, reveals that the SEC actions to improve share settlement were effective. Following the SEC hard-close-out rules, the number of firms on the threshold reached all-time lows and the dollar value of delivery failures dropped by over ninety percent.

C. Short Sales Volume

Using data obtained under special permission for the exchanges, we examine short selling activity around the rule changes in 2008. We first look at the Emergency Order, and during this period we see a decrease in short selling as a fraction of total volume, or *RELSS*. Table 2 shows

the basic result. Panel B shows that short sales as a fraction of total volume is 0.09 before the EO and 0.08 when the EO becomes effective. The decrease is statistically significant, and moreover, it is significantly larger than the decrease in short sales volume for similar firms not affected by the EO. This decrease in relative short sales volume is not surprising—since the rule requires short sellers to borrow in advance, the cost of short selling increases, regardless of the level of borrowing costs. Interestingly, Panel B of Table 2 shows that there is no significant difference in short sales as a fraction of shares outstanding (*SS2SHR*). The difference in these two results could be interpreted as short sales volume remaining constant, in a statistical sense, while total volume is higher in the EO period.

We turn our attention to the ban where the reduction in short sales is dramatic. Panel B of Table 3 shows that short sales as a fraction of total volume (*RELSS*) decrease from 0.14 to 0.09 for firms without options and from 0.07 to 0.02 for firms with options. The decrease in short sales is statistically significant for both sets of firm with no difference between firms with options and firms without options.

When we measure short sales as a fraction of shares outstanding (*SS2SHR*), option trading makes a difference. Firms without options have short sales that amount to 0.03% of shares outstanding before the ban and 0.02% of shares outstanding after the ban, a statistically significant decrease. Firms with options have an even larger decrease: from 0.12% to 0.04%. It is not at all surprising to see short sales decrease during a ban on short sales, but it is interesting to find that firms with options have a larger drop in short sales than firms without options. The difference is statistically significant at the 1% level.

V. The Effect of the Short Selling Rules on Liquidity and Market Quality

In this section, we examine the changes in market quality following the implementation of the short sales regulations. For both regulations, we use as market quality measures two measures of liquidity, *Amihud Illiquidity* and *Turnover*, as well as a measure of the stock's comovement with the market, *R2*. We calculate *Amihud* as the mean absolute daily return scaled by the daily dollar share volume. Increases in this measure correspond to increases in illiquidity (i.e., decreased liquidity) for a given stock. *Turnover* is measured as the ratio of daily share volume to total shares outstanding. *R2* is the R-squared from regressing the firm's daily return on the market return, as measured by the CRSP value-weighted index. Stocks with better information environments will tend to have a lower *R2* as stock-specific news is likely to move an individual stock's price but not the market. Hence, increases in this measure correspond with reduced idiosyncratic information in the firm's stock price and hence lower market quality.

We begin our analysis by examining the changes in market quality associated with the Emergency Order. Recall that in Section II, we predict the EO to have decreased the market quality of stocks affected by it. To test this hypothesis, we regress changes in our three measures, *Amihud*, *Turnover*, and *R2*, against an indicator for firms covered by the emergency rule and control variables. Our sample includes firms covered by the EO, excluding Fannie Mae and Freddie Mac, as well as a matched control group of firms in the same GICS sub-industry groups, as described in Section III. Specifically, we run the following OLS cross-sectional regressions:

$$\Delta MEASURE_i = \alpha + \beta_1 F19_i + \beta_2 SIZE_i + \beta_3 LOWPRICE_i + \beta_4 PRERULE_{TO_i} + \varepsilon_i \quad (1)$$

where $\Delta MEASURE_i \in \{Amihud, Turnover, R2\}$ is the change in the market quality measure. *F19* is a dummy indicating that a stock was covered by the emergency rule. We also include the pre-ban log of market capitalization, *SIZE*, an indicator variable for a price below \$15, *LOW_PRICE*, and the pre-ban average level of turnover (*PRERULE_TO*) as control variables. We use white-adjusted standard errors to ensure our results are robust to heteroskedasticity.¹⁶ The use of changes on the left de-means the variable and thus effectively implements a firm fixed-effects regression. Hence, coefficients represent deviations from the mean of the average firm.

If illiquidity generally increased for all stocks during the EO, we expect a positive constant term in the equation (1) when *Amihud* (*Turnover*) is the dependent variable. Likewise, if average level of idiosyncratic information in stock prices fell for all stocks during this time period, we expect a positive constant term in equation (1) when *R2* is the dependent variable. If the EO is associated with increased illiquidity for EO firms, then we expect the coefficient on *F19* to be positive (negative) and significant when *Amihud* (*Turnover*) is the dependent variable. Likewise, if the EO is associated with increased comovement of a stock's return with that of the market (i.e., reduced firm-specific information) then we expect the coefficient on *F19* to be positive and significant when *R2* is the dependent variable.

The results of estimating equation (1) are presented in Table 4. We fail to find evidence for a general decrease in liquidity during the EO, as the constant terms are statistically indistinguishable from zero. However, we find evidence consistent with the hypothesis that the

¹⁶ For this analysis, we require a minimum of ten daily returns observations in both the pre-rule and during-rule periods. In addition, we remove observations where *R2* is equal to one.

EO decreased market quality, since $R2$ increased for firms covered by the EO. Specifically, the coefficient on $F19$ is positive and marginally significant. The value of this coefficient estimate, 0.08 is also economically meaningful, as $R2$ can only range between 0 and 1. This finding corroborates the theoretical prediction in DV that market quality, as measured by a decrease in firm-specific information in price, decreases when short selling becomes more costly.

We now turn our attention to the effect of the short selling ban on liquidity and market quality. The ban on short sales likely increased illiquidity for all affected stocks since it eliminated a whole host of liquidity traders from the market. Furthermore, the Diamond and Verrecchia model suggests that this effect is larger for option-listed stocks affected by the ban. As explained in section II, the presence of listed options provides an avenue for informed traders to short that is unlikely to be used by liquidity traders, thereby causing the proportion of informed to uninformed trading, and hence illiquidity, to increase more for the option-listed stocks.

We use the changes in the same measures as above, namely *Amihud*, *Turnover*, and $R2$, and estimate a similar regression. Specifically, we run the following OLS cross-sectional regressions:

$$\Delta MEASURE_i = \alpha + \beta_1 OPTION_i + \beta_2 SIZE_i + \beta_3 LOWPRICE_i + \beta_4 PRERULE_{TO}_i + \varepsilon_i \quad (2)$$

where $\Delta MEASURE_i \in \{Amihud, Turnover, R2\}$ is the change in the market quality or liquidity measure. *OPTION* is a dummy indicating the stock has listed options.¹⁷ All other variables are the same as presented above in equation (1). We run the above regression on the sample of stocks affected by the ban.

Our hypothesis that illiquidity increased during the ban implies a positive (negative) constant term in the above regression when *Amihud* (*Turnover*) is the dependent variable. Our hypothesis that this decrease was greater for option-listed stocks implies a positive (negative) coefficient on *OPTION* when we use *Amihud* (*Turnover*) as the dependent variable. Our predictions for regressions that utilize *R2* as the dependent variable are similar—increased comovement in general should be associated with a positive constant term. If the change in comovement is greater for firms with listed options (*OPTION*), as implied by the DV model, then we expect this coefficient to be positive.

The results in Table 5 suggest that the average level of illiquidity (*AMIHUD*) increased during the short sales ban for banned firms, as the constant term is positive and significant for *AMIHUD*. Also as predicted, the coefficient on *OPTION* is positive and significant, so the level of liquidity dropped even further for stocks affected by the ban that also had traded options.

For the tests of *Amihud* Illiquidity, the results for *OPTION* are also economically significant. The constant term implies that for all ban-affected stocks, illiquidity increased by an average of 6.70 units, which is extremely high compared to the unconditional standard deviation of illiquidity of 0.185 for all stocks affected by the ban. The coefficient on *OPTION* equal to

¹⁷ We (roughly) measure the availability of listed options by setting *OPTION* equal to one if the firm appears on the CBOE directory of listed options as of 12/6/08 and zero otherwise.

0.728 indicates that having listed options magnified the effect of the ban on a stock's illiquidity by more than four standard deviations.

The tests on *Turnover* reveal a similar pattern. The constant term is negative and significant, which suggests that ban firms in general had a reduction in liquidity. The *OPTION* indicator loads negatively, which suggests that the decrease in liquidity was larger for ban firms with listed options than for ban firm without listed options.

For the *R2*, the results support the hypothesis that the reduction in market quality was greater for firms with listed options. The value of the constant term implies the *R2* with the market increased by 0.65, a very large number indeed for a variable which, by construction, must lie between 0 and 1, though the same caution applies to this result as the constant term for our regressions using liquidity measures. The coefficient on the option dummy indicates the effect is approximately 7% stronger for firms with listed options. We conclude that the ban had a significantly greater impact on market comovement for stocks with listed options than without.

Overall, the results from Tables 3 and 5 indicate that an increase in the ratio of informed to uninformed short sellers, as modeled in Diamond and Verrecchia (1987), will tend to decrease liquidity and overall market quality.

VI. The Short Sales Rules and The Information Content of Short Sales

In this section, we test the notion in Diamond and Verrecchia (1987) some short sale constraints increase the information content of short sales. We first examine the short sales ban, and then the Emergency Order.

A. Short Sales Ban

Recall that in Section 2 we hypothesize that, among those stocks whose short selling was banned in the fall of 2008, the ratio of informed to uninformed traders likely increased for option-listed stocks relative to those stocks that did not have listed options. Thus, among stocks whose short selling was banned, we expect the informativeness of short sales to be relatively higher during the period of the ban for those stocks who have listed options. One way to test this hypothesis is to examine the relation between short selling volume and returns. If short selling becomes more informative for a stock, we expect the negative relation between short volume and returns to grow stronger to the extent that market participants can observe short sales volume.

Short sale volume was not publicly available at the time of the ban, so the extent and timeliness with which market participants could observe it is unclear. Nevertheless, large dealers and market makers likely can likely infer short selling volume from their ability to observe orders, at least to some extent. Hence it is likely that information about short sales volume should be impounded into prices, albeit with a lag. Therefore, to test hypothesis H2a that the informativeness of short sales increased relatively more for ban stocks with listed options than for those without, we regress daily returns on contemporaneous and lagged values of short volume:

$$\begin{aligned} ret_{it} = & \alpha + \beta_1 option_i + \beta_2 ban_t + \gamma_1 ssvol_{i,t} + \dots + \gamma_5 ssvol_{i,t-4} + \delta_1 ret_{i,t-1} + \dots + \delta_4 ret_{i,t-4} \\ & + \gamma_6 ssvol_{it} * option_i + \dots + \gamma_{10} ssvol_{i,t-4} * option_i \\ & + \gamma_{11} ssvol_{i,t} * ban_t + \dots + \gamma_{15} ssvol_{i,t-4} * ban_t \\ & + \gamma_{15} ssvol_{i,t} * ban_t * option_i + \dots + \gamma_{20} ssvol_{i,t-4} * ban_{t-4} * option_i + \varepsilon_{it} \end{aligned} \quad (3)$$

where *option* is a dummy variable indicating the stock had listed options, *ban* is a time series dummy indicating that an observation is during the period of the ban, *ret* is the daily stock return, and *ssvol* is the volume of short sales for that day, scaled by shares outstanding. We include lags of return to ensure that autocorrelation in returns does not confound our inferences. We use bootstrapped standard errors since the properties of the residual variance-covariance matrix are impossible to know ex-ante. The results of this regression are labeled as Model (3) in Table 7.

If our hypothesis H2b is correct, and the informativeness of short sales increased for banned stocks with listed options relative to banned stocks without listed options, we expect the coefficients on the triple interactions (γ_{15} through γ_{20}) to be less than zero. Consistent with this hypothesis, the sum of the interaction terms is strongly negative and significant at the 0.01% level. Furthermore, the first three coefficients are negative and strongly significant.

The results are also economically significant. The relation between short volume and the returns becomes resoundingly negative during the ban only for those stocks that have traded options. The effect of *ssvol* on returns is several orders of magnitude stronger for these stocks, as indicated by coefficients on the first three triple interactions of -0.67, -0.397, and -0.662, respectively.

As an additional test of H2b, we compare the effect of short volume on returns for the sample of ban firms to a control firms using the propensity score technique described above. Among stocks whose short selling was banned, we expect the informativeness of short sales to be relatively higher during the period of the ban for those stocks which were not covered by the ban. We implement this test with a specification similar to that in equation (4) but replacing

OPTION with *BANFIRM*, which is an indicator equal to one for firms covered by the ban, to examine the informativeness of short sales under complete prohibitions.

The findings from these tests are presented in Table 8 and are supportive of increased price impact of short selling during the ban, though not universally. The coefficients on the first two triple interactions are negative and significant, which suggests that the negative price impact of short sales was larger during the ban for ban firms than before it was before the ban for ban firm and during the ban for non-ban firms. However, the sum of the coefficients on the triple interactions is not statistically significant.

Overall, the results are supportive of the prediction that increased short sales constraints are associated with increased informativeness of short sales for the short sales ban. We do not find evidence that during the Emergency Order, short sales are associated with increased informativeness.

B. Emergency Rule

Recall that the DV model implies that short sales will become more informative for stocks affected by the emergency rule relative to those unaffected by the rule. To test this hypothesis, we run the following regression:

$$\begin{aligned}
 ret_{i,t} = & \alpha + \beta_1 F19_i + \beta_2 EO_t + \gamma_1 ssvol_{i,t} + \dots + \gamma_5 ssvol_{i,t-4} + \delta_1 ret_{i,t-1} + \dots + \delta_4 ret_{i,t-4} \\
 & + \gamma_6 ssvol_{it} * F19_i + \dots + \gamma_{10} ssvol_{i,t-4} * F19_i \\
 & + \gamma_{11} ssvol_{i,t} * EO_t + \dots + \gamma_{15} ssvol_{i,t-4} * EO_t \\
 & + \gamma_{15} ssvol_{i,t} * EO_t * F19_i + \dots + \gamma_{20} ssvol_{i,t-4} * EO_{t-4} * F19_i + \epsilon_{i,t}
 \end{aligned}
 \tag{4}$$

where $F19$ is a cross-sectional dummy indicating the stock was one of the 19 firms affected by the ban, EO is a time-series dummy indicating that the observations occurs during the time the emergency rule was in effect, and $SSVOL$ is the ratio of the firm's short volume to its total volume on that day and up to four lags.

This specification is designed to measure the impact of short sales transaction on current and future returns, which should give us an idea of the informativeness of shorting during the period of the Emergency Rule. The coefficients of interest in model (4) are those related to the triple interactions (i.e., $SSVOL_{it} * EO * F19$). If the emergency order increased the information content of short sales, as postulated by Hypothesis H2b, we expect the sum of the coefficients on the triple interaction term to be negative. The individual coefficients provide us a measure of a price informativeness of short sales for a particular lag, while the sum of the coefficients provides for us a measure of the total impact of shorting during the five days (i.e., $t = 0$ through $t = -4$).

The results of testing the information content of short sales for the EO are labeled as Model 3 in Table 6. Contrary to the hypothesis, the sum of the triple interaction terms is not statistically distinguishable from zero. In addition, only one of the individual coefficients on the triple interaction is significantly negative. Hence we fail to find evidence that the emergency rule had any impact on the informativeness of short sales volume. Our standard errors, however, are high, so our failure to find evidence in this case is potentially the result of low power. This explanation of our results is supported by the fact that are only 14 stocks within our sample that were impacted by the emergency rule, and they were only impacted for the relatively short time of 29 days.

VII. Conclusion

In this paper, we explore the consequences of the two temporary short sales regulations adopted during the financial crisis of 2008. Specifically, we examine the Emergency Order that required short sellers to pre-borrow a specific set 19 stocks, and we study the ban on short selling which prohibited short sales in financial stocks. We examine how shorting activity and shorting costs changed in response to the rule adoption, and we exploit the differences in the rule changes to test implications of the Diamond and Verrecchia (1987) model of short selling's impact on stock prices.

Diamond and Verrecchia (1987) make a distinction between short sales prohibitions and constraints that merely increase costs. Our first set of results confirms short selling constraints increase dramatically for all firms subject to the rules, and for firms without traded options, the short selling ban effectively prohibits short sales. Overall, the results are supportive of the theoretical predictions in Diamond and Verrecchia (1987). Specifically, we find support for the idea that the proportion of informed to uninformed short sellers increases when the rules take effect, and we find support for the idea that short sales become more informative during the ban, especially for firms with traded options.

The immediate consequences of the rule changes on shorting activity are significant. We find that the emergency order significantly increased the cost of short selling, as measured by the cost of borrowing shares, for the affected stocks. We also find that both the order and the ban resulted in a dramatic decline short sale activity. Finally, we find that both rules were associated with significant declines in failures to deliver, one of the stated goals of the rules changes. The

findings in this paper are important for researchers calibrating any study using short sales in this period because the increase in costs and differences in short selling statistics are dramatic.

For both regulations, we analyze changes in market quality as measured by Amihud's (2002) illiquidity, share turnover, as well as the R^2 measure of a stock's comovement with the market. The results strongly suggest that market quality, as measured by turnover and the R^2 , decreased for all financial stocks during the EO period and that market quality decreased even more for EO firms. Further supporting the predictions of the DV model, the ban reduced the average level of market quality for all measures, and we find the largest reduction in stocks with listed options.

The ban affected the informativeness of short sales in a manner consistent with the DV model. We examine informativeness of short sales by measuring the relation between short selling volume and returns. We find that this relation grows stronger during the short sales ban. Furthermore, we find a statistically and economically significant difference between ban firms with traded options and ban firms without traded options, indicating that short sales in ban firms with traded options are particularly informative.

Our study, along with other studies on the subject, has several, related, limitations. First, we are unable to disentangle the direct consequences of the rule changes from the indirect effects arising from the fact that the rule changes signal a new regulatory regime by the Securities and Exchange Commission. Second, these rule changes take place in an unusually volatile period of history. Finally, the firms subject to the rules are not well matched to firms that are not subject to the rules, which limits the ability of this study to use relevant control firms. Nevertheless, many of the results in this paper focus on measures surrounding short selling, as opposed to

broader measures such as prices, which reduces the influence of any potential signaling effect. Furthermore, many of the results in the paper focus on differences among firms subject to the rules, and finally, we employ multiple control samples to minimize the impact of any potentially misleading comparison.

This study shows that the government intervention has had mixed consequences. The study shows that emergency order had the intended result of curbing delivery failures, but the intervention has had the unintended consequence of increasing short sale costs dramatically. This increase in short sale costs reduced overall market quality, and increased the proportion of informed traders to uninformed trades, thereby increasing adverse selection in these markets. The academic literature has generally found that, as suggested by Diamond and Verrecchia (1987), short selling improves market quality and market efficiency, and that increases in short sale constraints have a detrimental effect on markets. This paper shows that for many stocks subject to the rule changes, the rule changes were effectively increases in costly short sale constraints that have the expected, negative, effects on markets.

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TABLE 1
Descriptive Statistics

This table presents descriptive short selling characteristics of the firms used in our analyses. Reported figures are averages of daily observations, except for market capitalization, which is measured as of year-end for 2007 and short interest, which is reported bi-monthly. *Market Cap* is year-end price multiplied by shares outstanding in billions of dollars. *Short interest* is the mean balance of shares held short scaled by shares outstanding. *RELSS* is the mean daily level of short volume scaled by daily total share volume. *SS2SHR* is the mean daily level of short volume scaled by shares outstanding. *Turnover* is the daily level of volume as a percentage of shares outstanding. *Rebate* is the mean daily lending rate for a given security (i.e., the interest rate earned on borrower's cash collateral). *FTD* is the mean daily level of shares failed to deliver scaled by shares out. *Amihud Illiquidity* is the mean daily absolute return scaled by dollar share volume. Panel A presents descriptive statistics for the firms included in the 2008 emergency rule regarding naked short selling for all of 2008. Panel B presents mean daily values of short-selling variables for all of 2008 for the firms included in the October 2008 ban on short selling. Panel C presents mean daily values of short-selling variables for all of 2008 for financial firms, identified as those with SIC codes beginning with the number six.

Panel A: Firms Included in the July 2008 SEC Emergency Rule

| Company | Market Cap | Short Interest | RELSS | SS2SHR | Turnover | Rebate | FTD | Amihud Illiquidity |
|---------------------------------|------------|----------------|-------|--------|----------|--------|--------|--------------------|
| Barclays Plc | 66.62 | 0.05 | 0.09 | 0.002 | 0.027 | 1.12 | 0.0008 | 0.0015 |
| Citigroup Inc | 147.04 | 0.02 | 0.07 | 0.002 | 0.025 | 2.48 | 0.0000 | 0.0000 |
| Credit Suisse Group | 61.34 | 0.03 | 0.08 | 0.002 | 0.028 | 1.93 | 0.0012 | 0.0007 |
| Deutsche Bank Ag | 64.84 | 0.01 | 0.12 | 0.000 | 0.002 | 1.75 | 0.0001 | 0.0006 |
| Federal Home Loan Mortgage Corp | 22.02 | 0.11 | 0.06 | 0.004 | 0.066 | 1.20 | 0.0008 | 0.0020 |
| Federal National Mortgage Assn | 38.94 | 0.09 | 0.06 | 0.003 | 0.050 | 1.66 | 0.0003 | 0.0012 |
| Goldman Sachs Group Inc | 88.54 | 0.03 | 0.09 | 0.004 | 0.042 | 2.39 | 0.0001 | 0.0000 |
| H S B C Holdings Plc | 195.56 | 0.05 | 0.10 | 0.002 | 0.018 | 2.02 | 0.0006 | 0.0001 |
| Jpmorgan Chase & Co | 146.99 | 0.01 | 0.07 | 0.001 | 0.014 | 2.45 | 0.0000 | 0.0000 |
| Lehman Brothers Holdings Inc | 33.31 | - | 0.09 | 0.007 | 0.085 | 1.98 | 0.0008 | 0.0003 |
| Merrill Lynch & Co Inc | 50.25 | 0.04 | 0.09 | 0.003 | 0.032 | 2.31 | 0.0000 | 0.0001 |
| Mizuho Financial Group Inc | 41.86 | 0.01 | 0.08 | 0.001 | 0.006 | 0.15 | 0.0010 | 0.0141 |
| Morgan Stanley Dean Witter & Co | 55.69 | 0.03 | 0.07 | 0.002 | 0.027 | 2.37 | 0.0000 | 0.0001 |
| U B S Ag | 88.11 | 0.01 | 0.09 | 0.000 | 0.003 | 2.11 | 0.0001 | 0.0005 |
| Mean | 78.65 | 0.04 | 0.08 | 0.002 | 0.030 | 1.85 | 0.0004 | 0.0015 |
| Median | 63.09 | 0.03 | 0.08 | 0.002 | 0.027 | 2.00 | 0.0002 | 0.0004 |
| Std. Dev. | 50.66 | 0.03 | 0.02 | 0.002 | 0.024 | 0.65 | 0.0004 | 0.0037 |
| Q1 | 43.96 | 0.01 | 0.07 | 0.001 | 0.015 | 1.68 | 0.0000 | 0.0001 |
| Q3 | 88.44 | 0.05 | 0.09 | 0.003 | 0.040 | 2.36 | 0.0008 | 0.0011 |

Panel B: Firms Included in October 2008 SEC Short Sales Ban

| Variable | N | Mean | Std. Dev. | Min | Q1 | Median | Q3 | Max |
|--------------------|----------|-------------|------------------|------------|-----------|---------------|-----------|------------|
| Market Cap | 848 | 7.98 | 26.74 | 0.00 | 0.24 | 0.68 | 2.97 | 370.24 |
| Short Interest | 848 | 0.06 | 0.07 | 0.00 | 0.01 | 0.05 | 0.09 | 0.92 |
| RELSS | 848 | 0.08 | 0.12 | 0.00 | 0.04 | 0.06 | 0.10 | 25.00 |
| SS2SHR | 848 | 0.001 | 0.001 | 0.000 | 0.000 | 0.000 | 0.001 | 0.095 |
| Turnover | 848 | 0.01 | 0.02 | 0.00 | 0.00 | 0.01 | 0.01 | 0.85 |
| Rebate | 848 | 1.02 | 4.22 | -75.00 | 0.75 | 1.92 | 2.41 | 7.40 |
| FTD | 848 | 0.001 | 0.004 | 0.000 | 0.000 | 0.000 | 0.000 | 0.167 |
| Amihud Illiquidity | 848 | 0.036 | 0.185 | 0.000 | 0.000 | 0.001 | 0.009 | 4.947 |

Panel C: All Financial Firms

| Variable | N | Mean | Std. Dev. | Min | Q1 | Median | Q3 | Max |
|--------------------|----------|-------------|------------------|------------|-----------|---------------|-----------|------------|
| Market Cap | 2,497 | 3.36 | 13.74 | 0.00 | 0.14 | 0.39 | 1.21 | 195.56 |
| Short Interest | 2,497 | 0.05 | 0.09 | 0.00 | 0.00 | 0.02 | 0.07 | 8.47 |
| RELSS | 2,497 | 0.11 | 4.22 | 0.00 | 0.03 | 0.06 | 0.11 | 2173.49 |
| SS2SHR | 2,497 | 0.004 | 0.021 | 0.000 | 0.000 | 0.000 | 0.001 | 1.652 |
| Turnover | 2,497 | 0.03 | 0.16 | 0.00 | 0.00 | 0.01 | 0.02 | 26.42 |
| Rebate | 2,497 | 0.84 | 3.54 | -75.00 | 0.13 | 1.50 | 2.21 | 20.00 |
| FTD | 2,497 | 0.003 | 0.027 | 0.000 | 0.000 | 0.000 | 0.000 | 5.762 |
| Amihud Illiquidity | 2,497 | 0.021 | 0.182 | 0.000 | 0.000 | 0.002 | 0.006 | 24.106 |

TABLE 2

Difference-in-Differences of Short Selling Measures for July 2008 SEC Emergency Rule on Equity Lending

This table presents the differences for various short selling measures before and after the naked short SEC emergency rule in July 2008. The sample includes the F19 firms as well as control firms with broker-dealer operations that fall within the same GIC sub-industries. The sample period covers January – August 2008 and the period is split at July 21, 2008 when the emergency rule went into effect. *SPECIALNESS* is the federal funds rate less the mean daily lending rate for a given security. *RELSS* is the daily level of short volume scaled by daily total share volume. *SS2SHR* is the daily level of short volume scaled by shares outstanding. *FTD* is the daily level of shares failed to deliver scaled by shares outstanding. For all tests, we take the firm-level mean of each measure for the period before July 21, 2008 and compare it to the mean during the emergency rule period, July 21 – August 8, 2008. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

| Specialness | | <u>Before EO</u> | <u>During EO</u> | <u>Difference</u> | |
|--------------------|------------|------------------|------------------|-------------------|-----|
| | | (A) | (B) | (B) - (A) | |
| Control Firms | (I) | 0.79 | 0.90 | 0.11 | |
| F19 | (II) | 0.19 | 1.43 | 1.24 | ** |
| Difference | (II) - (I) | -0.60 | 0.53 | 1.13 | *** |

| SS2SHR | | <u>Before EO</u> | <u>During EO</u> | <u>Difference</u> | |
|---------------|------------|------------------|------------------|-------------------|-----|
| | | (A) | (B) | (B) - (A) | |
| Control Firms | (I) | 0.0009 | 0.0010 | 0.0001 | |
| F19 | (II) | 0.0021 | 0.0021 | 0.0000 | |
| Difference | (II) - (I) | 0.0012 | 0.0011 | -0.0001 | *** |

| RELSS | | <u>Before EO</u> | <u>During EO</u> | <u>Difference</u> | |
|---------------|------------|------------------|------------------|-------------------|-----|
| | | (A) | (B) | (B) - (A) | |
| Control Firms | (I) | 0.08 | 0.08 | 0.00 | |
| F19 | (II) | 0.09 | 0.08 | -0.01 | * |
| Difference | (II) - (I) | 0.01 | 0.00 | -0.01 | *** |

| FTD | | <u>Before EO</u> | <u>During EO</u> | <u>Difference</u> | |
|---------------|------------|------------------|------------------|-------------------|-----|
| | | (A) | (B) | (B) - (A) | |
| Control Firms | (I) | 0.0008 | 0.0015 | 0.0008 | |
| F19 | (II) | 0.0005 | 0.0001 | -0.0004 | ** |
| Difference | (II) - (I) | -0.0002 | -0.0015 | -0.0012 | *** |

TABLE 3

Difference-in-Differences of Short Selling Measures for the October 2008 Short Sale Ban

This table presents the differences for various short selling measures before and during the short sales ban in October 2008. The sample includes only firms for which short selling was banned and covers January – October 2008. *SPECIALNESS* is the federal funds rate less the mean daily lending rate for a given security. *RELSS* is the daily level of short volume scaled by daily total share volume. *SS2SHR* is the daily level of short volume scaled by shares outstanding. *FTD* is the daily level of shares failed to deliver scaled by shares outstanding. For all tests, we take the firm-level mean of each measure for the period before September 18, 2008 and compare it to the mean during the ban period, September 18 – October 9, 2008. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Specialness

| | | <u>Before Ban</u> | <u>During Ban</u> | <u>Difference</u> |
|------------|--------------|-------------------|-------------------|-------------------|
| | | (A) | (B) | (B) - (A) |
| All | (I) | 1.15 | 1.30 | 0.15 |
| No Options | (II) | 1.09 | 1.06 | -0.03 |
| Options | (III) | 1.29 | 1.79 | 0.50 |
| Difference | (III) - (II) | 0.20 | 0.74 ** | 0.54 * |

SS2SHR

| | | <u>Before Ban</u> | <u>During Ban</u> | <u>Difference</u> |
|------------|--------------|-------------------|-------------------|-------------------|
| | | (A) | (B) | (B) - (A) |
| All | (I) | 0.0006 | 0.0002 | -0.0004 *** |
| No Options | (II) | 0.0003 | 0.0002 | -0.0002 *** |
| Options | (III) | 0.0012 | 0.0004 | -0.0007 *** |
| Difference | (III) - (II) | 0.0008 *** | 0.0002 *** | -0.0006 *** |

RELSS

| | | <u>Before Ban</u> | <u>During Ban</u> | <u>Difference</u> |
|------------|--------------|-------------------|-------------------|-------------------|
| | | (A) | (B) | (B) - (A) |
| All | (I) | 0.12 | 0.07 | -0.05 *** |
| No Options | (II) | 0.14 | 0.09 | -0.05 *** |
| Options | (III) | 0.07 | 0.02 | -0.05 *** |
| Difference | (III) - (II) | -0.07 *** | -0.07 *** | 0.00 |

FTD

| | | <u>Before Ban</u> | <u>During Ban</u> | <u>Difference</u> |
|------------|--------------|-------------------|-------------------|-------------------|
| | | (A) | (B) | (B) - (A) |
| All | (I) | 0.0008 | 0.0006 | -0.0002 * |
| No Options | (II) | 0.0005 | 0.0004 | -0.0001 |
| Options | (III) | 0.0014 | 0.0010 | -0.0003 |
| Difference | (III) - (II) | 0.0008 *** | 0.0006 *** | -0.0002 |

TABLE 4
Multivariate Regressions of the Effects of the SEC July 2008 Emergency Rule
on Liquidity, Equity Lending and Market Efficiency

This table presents the results of multivariate regressions of various liquidity and market efficiency measures on indicators for the firms affected by the rule (*F19*) and the time period during the July 2008 Emergency Order (*During_EO*). The sample includes the F19 firms as well as control firms with broker-dealer operations that fall within the same GIC sub-industries. The sample period covers January – August 2008 where the period is split at July 21, 2008 when the emergency rule went into effect. For each firm, we retain one observation in the period prior to the emergency order and one observation during the emergency order. *AMIHUD* is the mean absolute daily return divided by the daily dollar volume with the coefficients multiplied by 1,000,000 for expositional purposes. R^2 is the r-squared from a regression of firm returns on value-weighted market returns. *Turnover* is the daily level of volume as a percentage of shares outstanding. *SPECIALNESS* is the federal funds rate less the mean daily lending rate for a given security. *RELSS* is the daily level of short volume scaled by daily total share volume. *SS2SHR* is the daily level of short volume scaled by shares outstanding. *FTD* is the daily level of shares failed to deliver scaled by shares outstanding. We include *SIZE*, which is mean pre-rule log market capitalization, *LOW_PRICE*, which is an indicator for mean pre-rule price of less than fifteen dollars, and *Pre-Rule Turnover*, which is mean pre-rule turnover, as control variables. We use white standard errors and winsorize all continuous variables at the 1st and 99th percentiles. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Market Quality Measures

| Dependent Variable: | <u>Amihud</u> | | <u>R2</u> | | <u>Turnover</u> | | | |
|--------------------------|---------------|--------|-----------|--------|-----------------|--------|-------|----|
| | Coef. | t-stat | Coef. | t-stat | Coef. | t-stat | | |
| <i>Intercept</i> | 0.054 | 0.67 | -0.400 | -2.13 | ** | -0.009 | -1.08 | |
| <i>F19</i> | 0.004 | 0.29 | 0.080 | 1.65 | * | 0.005 | 0.89 | |
| <i>Size</i> | -0.003 | -0.70 | 0.024 | 2.03 | ** | 0.001 | 1.59 | |
| <i>Low_Price</i> | 0.000 | 0.01 | 0.099 | 1.37 | | -0.003 | -1.37 | |
| <i>Pre-Rule Turnover</i> | -0.002 | 0.00 | 1.049 | 0.71 | | -0.204 | -2.04 | ** |
| <i>N</i> | 77 | | 73 | | | 77 | | |
| R^2 | 0.006 | | 0.112 | | | 0.158 | | |

Panel B: Short Selling Measures

| Dependent Variable: | <u>Specialness</u> | | <u>RELSS</u> | | <u>SSRSRHR</u> | | <u>FTD</u> | | | | | |
|--------------------------|--------------------|--------|--------------|--------|----------------|--------|------------|--------|-----|--------|-------|----|
| | Coef. | t-stat | Coef. | t-stat | Coef. | t-stat | Coef. | t-stat | | | | |
| <i>Intercept</i> | 2.481 | 2.35 | ** | -0.095 | -4.71 | *** | -0.002 | -2.39 | ** | 0.003 | 1.29 | |
| <i>F19</i> | 1.107 | 2.07 | ** | -0.029 | -5.30 | *** | 0.000 | -1.05 | | -0.001 | -2.24 | ** |
| <i>Size</i> | -0.154 | -2.43 | ** | 0.007 | 5.36 | *** | 0.000 | 2.76 | *** | 0.000 | -0.75 | |
| <i>Low_Price</i> | -0.468 | -1.21 | | 0.014 | 1.85 | * | 0.000 | -0.07 | | 0.000 | -0.37 | |
| <i>Pre-Rule Turnover</i> | 12.065 | 1.82 | * | -0.241 | -1.60 | | -0.019 | -2.40 | ** | -0.024 | -1.48 | |
| <i>N</i> | 72 | | | 77 | | | 77 | | | 77 | | |
| R^2 | 0.096 | | | 0.221 | | | 0.191 | | | 0.039 | | |

TABLE 5
**Multivariate Regressions of the Effects of the October 2008 Short Sales Ban
on Liquidity, Equity Lending and Market Efficiency**

This table presents the results of multivariate regressions of various liquidity and market efficiency measures on indicators for the firms with listed options (*OPTION*) and the time period during the short sales ban (*During_BAN*). The sample includes only firms for which short selling was banned and covers January – October 2008. For each firm, we retain one observation in the period prior to the ban and one observation during the ban. *AMIHUD* is the mean absolute daily return divided by the daily dollar volume with the coefficients multiplied by 1,000,000 for expositional purposes. R^2 is the r -squared from a regression of firm returns on value-weighted market returns. *Turnover* is the daily level of volume as a percentage of shares outstanding. *SPECIALNESS* is the federal funds rate less the mean daily lending rate for a given security. *RELSS* is the daily level of short volume scaled by daily total share volume. *SS2SHR* is the daily level of short volume scaled by shares outstanding. *FTD* is the daily level of shares failed to deliver scaled by shares outstanding. We include *SIZE*, which is mean pre-ban log market capitalization, *LOW_PRICE*, which is an indicator for mean pre-ban price of less than fifteen dollars, and *Pre-Ban Turnover*, which is mean pre-ban turnover, as control variables. We use white standard errors and winsorize all continuous variables at the 1st and 99th percentiles. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Market Quality Measures

| Dependent Variable: | <u>Amihud</u> | | <u>R2</u> | | <u>Turnover</u> | |
|--------------------------|---------------|----------------|-----------|----------------|-----------------|----------------|
| | Coef. | <i>t</i> -stat | Coef. | <i>t</i> -stat | Coef. | <i>t</i> -stat |
| <i>Intercept</i> | 6.70 | 3.48 *** | 0.65 | 5.19 *** | -0.01 | -2.30 ** |
| <i>Option</i> | 0.73 | 2.93 *** | 0.07 | 2.45 ** | 0.00 | -1.86 * |
| <i>Size</i> | -0.49 | -3.48 *** | -0.04 | -4.78 *** | 0.00 | 2.51 ** |
| <i>Low_Price</i> | 0.30 | 1.22 | 0.07 | 2.52 ** | 0.00 | 0.92 |
| <i>Pre-Rule Turnover</i> | -7.35 | -2.05 ** | -0.13 | -0.14 | -0.06 | -1.11 |
| <i>N</i> | 761 | | 633 | | 761 | |
| R^2 | 0.052 | | 0.086 | | 0.031 | |

Panel B: Short Selling Measures

| Dependent Variable: | <u>Specialness</u> | | <u>RELSS</u> | | <u>SSRSRHR</u> | | <u>FTD</u> | |
|--------------------------|--------------------|----------------|--------------|----------------|----------------|----------------|------------|----------------|
| | Coef. | <i>t</i> -stat | Coef. | <i>t</i> -stat | Coef. | <i>t</i> -stat | Coef. | <i>t</i> -stat |
| <i>Intercept</i> | -0.75 | -0.76 | 0.06 | 1.46 | 0.00 | 0.37 | 0.00 | -3.60 *** |
| <i>Option</i> | 0.39 | 1.05 | 0.00 | 1.02 | 0.00 | -3.10 *** | 0.00 | -1.33 |
| <i>Size</i> | 0.02 | 0.29 | -0.01 | -2.76 *** | 0.00 | -0.92 | 0.00 | 3.96 *** |
| <i>Low_Price</i> | 0.64 | 2.67 *** | 0.01 | 0.78 | 0.00 | 2.29 ** | 0.00 | -1.77 * |
| <i>Pre-Rule Turnover</i> | 33.84 | 1.70 * | -0.22 | -1.39 | -0.03 | -6.15 *** | -0.05 | -5.61 *** |
| <i>N</i> | 695 | | 761 | | 761 | | 761 | |
| R^2 | 0.050 | | 0.027 | | 0.494 | | 0.158 | |

TABLE 6
Regressions of Returns on Lagged Returns, Lagged Short Volume
and Indicators for the Emergency Order

This table presents the results of regressions of contemporaneous returns on lagged returns (*RET*), lagged short volume (*RELSS*), and an indicator for the time period during the July Emergency Rule (*POST*). The sample includes daily observations for F19 firms as well as control firms with broker-dealer operations that fall within the same GIC sub-industries as the F19. The sample period covers January – August 2008. We use the daily change in the ratio of short volume to total volume to measure short volume, *RELSS*. *POST* is equal to one during the emergency rule period, July 21 – August 8, 2008, and zero otherwise. Standard errors are computed using a non-parametric bootstrap method which resamples observations (with replacement) from the data fifty times. All continuous variables are winsorized at 1st and 99th percentiles. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

| Explanatory Variable | Model | | | | | | | | |
|--|-----------|----------------|-----|-----------|----------------|-----|-----------|----------------|-----|
| | (1) | | | (2) | | | (3) | | |
| | Coef. | <i>t</i> -stat | | Coef. | <i>t</i> -stat | | Coef. | <i>t</i> -stat | |
| <i>a0</i> INTERCEPT | -0.001 | -3.88 | *** | -0.001 | -3.02 | *** | -0.001 | -3.02 | *** |
| <i>a1</i> <i>RET</i> _{<i>t</i>-1} | -0.067 | -3.76 | *** | -0.068 | -3.75 | *** | -0.067 | -3.71 | *** |
| <i>a2</i> <i>RET</i> _{<i>t</i>-2} | -0.038 | -4.94 | *** | -0.038 | -4.95 | *** | -0.037 | -4.89 | *** |
| <i>a3</i> <i>RET</i> _{<i>t</i>-3} | -0.069 | -6.77 | *** | -0.069 | -6.85 | *** | -0.069 | -6.99 | *** |
| <i>a4</i> <i>RET</i> _{<i>t</i>-4} | -0.028 | -2.12 | ** | -0.028 | -2.12 | ** | -0.029 | -2.15 | ** |
| <i>a5</i> <i>RELSS</i> _{<i>t</i>} | 0.012 | 1.25 | | 0.012 | 1.25 | | 0.013 | 1.37 | |
| <i>a6</i> <i>RELSS</i> _{<i>t</i>-1} | -0.019 | -1.15 | | -0.017 | -0.98 | | -0.017 | -0.94 | |
| <i>a7</i> <i>RELSS</i> _{<i>t</i>-2} | -0.003 | -0.17 | | 0.000 | 0.02 | | -0.001 | -0.05 | |
| <i>a8</i> <i>RELSS</i> _{<i>t</i>-3} | -0.001 | -0.11 | | -0.001 | -0.11 | | -0.003 | -0.24 | |
| <i>a9</i> <i>RELSS</i> _{<i>t</i>-4} | 0.009 | 1.24 | | 0.007 | 0.87 | | 0.006 | 0.78 | |
| <i>a10</i> <i>POST</i> | | | | 0.009 | 5.42 | *** | 0.009 | 5.44 | *** |
| <i>a11</i> <i>POST</i> * <i>RELSS</i> _{<i>t</i>} | | | | -0.052 | -1.08 | | -0.074 | -1.47 | |
| <i>a12</i> <i>POST</i> * <i>RELSS</i> _{<i>t</i>-1} | | | | 0.020 | 0.45 | | 0.008 | 0.19 | |
| <i>a13</i> <i>POST</i> * <i>RELSS</i> _{<i>t</i>-2} | | | | -0.024 | -0.82 | | -0.005 | -0.14 | |
| <i>a14</i> <i>POST</i> * <i>RELSS</i> _{<i>t</i>-3} | | | | 0.001 | 0.03 | | 0.029 | 0.72 | |
| <i>a15</i> <i>POST</i> * <i>RELSS</i> _{<i>t</i>-4} | | | | 0.025 | 0.70 | | 0.041 | 1.04 | |
| <i>a16</i> <i>F19</i> | | | | -0.001 | -1.01 | | -0.001 | -1.02 | |
| <i>a17</i> <i>F19</i> * <i>POST</i> | | | | -0.003 | -0.97 | | -0.003 | -1.10 | |
| <i>a18</i> <i>F19</i> * <i>RELSS</i> _{<i>t</i>} | | | | -0.006 | -0.15 | | -0.022 | -0.55 | |
| <i>a19</i> <i>F19</i> * <i>RELSS</i> _{<i>t</i>-1} | | | | -0.020 | -0.31 | | -0.029 | -0.42 | |
| <i>a20</i> <i>F19</i> * <i>RELSS</i> _{<i>t</i>-2} | | | | -0.039 | -0.89 | | -0.024 | -0.51 | |
| <i>a21</i> <i>F19</i> * <i>RELSS</i> _{<i>t</i>-3} | | | | 0.002 | 0.06 | | 0.023 | 0.66 | |
| <i>a22</i> <i>F19</i> * <i>RELSS</i> _{<i>t</i>-4} | | | | 0.034 | 1.30 | | 0.045 | 1.31 | |
| <i>a23</i> <i>F19</i> * <i>POST</i> * <i>RELSS</i> _{<i>t</i>} | | | | | | | 0.186 | 1.47 | |
| <i>a24</i> <i>F19</i> * <i>POST</i> * <i>RELSS</i> _{<i>t</i>-1} | | | | | | | 0.143 | 0.77 | |
| <i>a25</i> <i>F19</i> * <i>POST</i> * <i>RELSS</i> _{<i>t</i>-2} | | | | | | | -0.154 | -2.07 | ** |
| <i>a26</i> <i>F19</i> * <i>POST</i> * <i>RELSS</i> _{<i>t</i>-3} | | | | | | | -0.223 | -1.35 | |
| <i>a27</i> <i>F19</i> * <i>POST</i> * <i>RELSS</i> _{<i>t</i>-4} | | | | | | | -0.098 | -0.54 | |
| Standard Errors | Bootstrap | | | Bootstrap | | | Bootstrap | | |
| N | 9,908 | | | 9,908 | | | 9,908 | | |
| R-squared | 0.0123 | | | 0.0121 | | | 0.0127 | | |
| ChiSq Test: $a_{23} + \dots + a_{27} = 0$ | | | | | | | 0.28 | | |
| p-value | | | | | | | 0.591 | | |

TABLE 7
Regressions of Returns on Lagged Returns, Lagged Short Volume and Indicators for Options Listing and Shorting Ban

This table presents the results of regressions of contemporaneous returns on lagged returns (*RET*), lagged short volume (*RELSS*), and indicators for the firms with listed options (*OPTION*) and the time period during the short sales ban (*POST*). The sample includes only firms for which short selling was banned and covers January – October 2008. To measure short volume, *RELSS*, we use the daily change in the ratio of short volume to total volume. *POST* is equal to one during the short sales ban period, September 18 – October 9, 2008, and zero otherwise. Standard errors are computed using a non-parametric bootstrap method which resamples observations (with replacement) from the data fifty times. All continuous variables are winsorized at 1st and 99th percentiles. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

| Explanatory Variable | Model | | | | | |
|---|-----------|----------------|-----------|----------------|-----------|----------------|
| | (1) | | (2) | | (3) | |
| | Coef. | <i>t</i> -stat | Coef. | <i>t</i> -stat | Coef. | <i>t</i> -stat |
| <i>a0</i> INTERCEPT | -0.001 | -2.92 *** | 0.000 | -0.48 | 0.000 | -0.49 |
| <i>a1</i> <i>RET</i> _{<i>t-1</i>} | -0.058 | -3.76 *** | -0.063 | -3.97 *** | -0.055 | -3.64 *** |
| <i>a2</i> <i>RET</i> _{<i>t-2</i>} | -0.063 | -3.97 *** | -0.041 | -4.36 *** | -0.065 | -4.24 *** |
| <i>a3</i> <i>RET</i> _{<i>t-3</i>} | -0.040 | -4.44 *** | -0.018 | -1.74 * | -0.040 | -3.97 *** |
| <i>a4</i> <i>RET</i> _{<i>t-4</i>} | -0.018 | -1.74 * | 0.007 | 0.76 | -0.019 | -1.81 * |
| <i>a5</i> <i>RELSS</i> _{<i>t</i>} | -0.002 | -0.36 | -0.011 | -0.84 | -0.003 | -0.45 |
| <i>a6</i> <i>RELSS</i> _{<i>t-1</i>} | -0.022 | -2.66 *** | 0.014 | 1.01 | -0.018 | -1.47 |
| <i>a7</i> <i>RELSS</i> _{<i>t-2</i>} | -0.003 | -0.29 | 0.002 | 0.25 | 0.003 | 0.24 |
| <i>a8</i> <i>RELSS</i> _{<i>t-3</i>} | -0.003 | -0.39 | -0.005 | -0.60 | 0.003 | 0.38 |
| <i>a9</i> <i>RELSS</i> _{<i>t-4</i>} | 0.001 | 0.18 | -0.007 | -1.56 | 0.003 | 0.64 |
| <i>a10</i> <i>POST</i> | -0.007 | -3.16 *** | -0.270 | -1.45 | -0.005 | -1.24 |
| <i>a11</i> <i>POST*RELSS</i> _{<i>t</i>} | -0.271 | -1.45 | -0.479 | -5.41 *** | 0.003 | 0.01 |
| <i>a12</i> <i>POST*RELSS</i> _{<i>t-1</i>} | -0.482 | -5.32 *** | 0.168 | 0.96 | -0.229 | -1.63 |
| <i>a13</i> <i>POST*RELSS</i> _{<i>t-2</i>} | 0.166 | 0.94 | -0.156 | -1.75 * | 0.464 | 1.72 * |
| <i>a14</i> <i>POST*RELSS</i> _{<i>t-3</i>} | -0.157 | -1.80 * | -0.250 | -1.43 | -0.190 | -1.37 |
| <i>a15</i> <i>POST*RELSS</i> _{<i>t-4</i>} | -0.250 | -1.43 | -0.001 | -1.65 * | -0.438 | -1.66 * |
| <i>a16</i> <i>OPTION</i> | | | -0.001 | -0.36 | -0.001 | -1.65 * |
| <i>a17</i> <i>OPTION*POST</i> | | | -0.023 | -1.54 | -0.005 | -1.06 |
| <i>a18</i> <i>OPTION*RELSS</i> _{<i>t</i>} | | | -0.027 | -1.72 * | 0.004 | 0.43 |
| <i>a19</i> <i>OPTION*RELSS</i> _{<i>t-1</i>} | | | -0.042 | -2.13 ** | -0.010 | -0.71 |
| <i>a20</i> <i>OPTION*RELSS</i> _{<i>t-2</i>} | | | -0.012 | -0.89 | -0.013 | -0.92 |
| <i>a21</i> <i>OPTION*RELSS</i> _{<i>t-3</i>} | | | 0.014 | 0.89 | -0.014 | -1.33 |
| <i>a22</i> <i>OPTION*RELSS</i> _{<i>t-4</i>} | | | -0.004 | -0.19 | -0.007 | -1.09 |
| <i>a23</i> <i>OPTION*POST*RELSS</i> _{<i>t</i>} | | | | | -0.670 | -2.98 *** |
| <i>a24</i> <i>OPTION*POST*RELSS</i> _{<i>t-1</i>} | | | | | -0.397 | -2.51 ** |
| <i>a25</i> <i>OPTION*POST*RELSS</i> _{<i>t-2</i>} | | | | | -0.662 | -2.31 ** |
| <i>a26</i> <i>OPTION*POST*RELSS</i> _{<i>t-3</i>} | | | | | 0.115 | 0.73 |
| <i>a27</i> <i>OPTION*POST*RELSS</i> _{<i>t-4</i>} | | | | | 0.475 | 1.77 * |
| Standard Errors | Bootstrap | | Bootstrap | | Bootstrap | |
| N | 76,630 | | 76,630 | | 76,630 | |
| R-squared | 0.0308 | | 0.0311 | | 0.0391 | |
| ChiSq Test: <i>a11</i> + ... + <i>a15</i> = 0 | 20.35 | | 21.08 | | 1.72 | |
| p-value | 0.000 | | 0.000 | | 0.189 | |
| ChiSq Test: <i>a23</i> + ... + <i>a27</i> = 0 | | | | | 11.39 | |
| p-value | | | | | 0.001 | |

TABLE 8

Regressions of Returns on Lagged Returns, Lagged Short Volume and Indicators for Ban-Firm and During the Shorting Ban

This table presents the results of regressions of contemporaneous returns on lagged returns (*RET*), lagged short volume (*RELSS*), and indicators for the firms subject to the short sales ban (*BANFIRM*) and the time period during the short sales ban (*POST*). The control group is selected using propensity score matching as described in the text. The sample period covers January – October 2008. To measure short volume, *RELSS*, we use the daily change in the ratio of short volume to total volume. *POST* is equal to one during the short sales ban period, September 18 – October 9, 2008, and zero otherwise. Standard errors are computed using a non-parametric bootstrap method which resamples observations (with replacement) from the data fifty times. All continuous variables are winsorized at 1st and 99th percentiles. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

| Explanatory Variable | Model | | | | | | | |
|--|-----------|----------------|-----------|----------------|-----------|----------------|--|--|
| | (1) | | (2) | | (3) | | | |
| | Coef. | <i>t</i> -stat | Coef. | <i>t</i> -stat | Coef. | <i>t</i> -stat | | |
| <i>a0</i> INTERCEPT | -0.001 | -7.84 *** | -0.001 | -7.25 *** | -0.001 | -7.25 *** | | |
| <i>a1</i> <i>RET</i> _{<i>t-1</i>} | -0.039 | -3.54 *** | -0.040 | -3.62 *** | -0.040 | -3.65 *** | | |
| <i>a2</i> <i>RET</i> _{<i>t-2</i>} | -0.025 | -3.81 *** | -0.026 | -4.00 *** | -0.026 | -3.93 *** | | |
| <i>a3</i> <i>RET</i> _{<i>t-3</i>} | -0.009 | -1.29 | -0.010 | -1.43 | -0.010 | -1.45 | | |
| <i>a4</i> <i>RET</i> _{<i>t-4</i>} | 0.004 | 0.76 | 0.003 | 0.60 | 0.004 | 0.63 | | |
| <i>a5</i> <i>RELSS</i> _{<i>t</i>} | 0.005 | 2.19 ** | 0.014 | 4.16 *** | 0.012 | 3.37 *** | | |
| <i>a6</i> <i>RELSS</i> _{<i>t-1</i>} | 0.004 | 1.93 * | 0.016 | 3.51 *** | 0.014 | 2.84 *** | | |
| <i>a7</i> <i>RELSS</i> _{<i>t-2</i>} | 0.002 | 1.61 | 0.008 | 1.98 ** | 0.009 | 1.90 * | | |
| <i>a8</i> <i>RELSS</i> _{<i>t-3</i>} | 0.001 | 1.03 | 0.007 | 1.66 * | 0.000 | 1.45 | | |
| <i>a9</i> <i>RELSS</i> _{<i>t-4</i>} | 0.001 | 1.22 | 0.001 | 0.32 | 0.002 | 0.54 | | |
| <i>a10</i> <i>POST</i> | -0.008 | -10.02 *** | -0.012 | -10.93 *** | -0.012 | -10.79 *** | | |
| <i>a11</i> <i>POST*RELSS</i> _{<i>t</i>} | -0.061 | -4.27 *** | -0.060 | -4.31 *** | -0.012 | -0.44 | | |
| <i>a12</i> <i>POST*RELSS</i> _{<i>t-1</i>} | -0.113 | -3.83 *** | -0.110 | -3.83 *** | -0.047 | -1.18 | | |
| <i>a13</i> <i>POST*RELSS</i> _{<i>t-2</i>} | -0.075 | -2.88 *** | -0.070 | -2.67 *** | -0.078 | -2.12 ** | | |
| <i>a14</i> <i>POST*RELSS</i> _{<i>t-3</i>} | -0.046 | -2.38 ** | -0.041 | -2.09 ** | -0.023 | -0.72 | | |
| <i>a15</i> <i>POST*RELSS</i> _{<i>t-4</i>} | -0.032 | -1.38 | -0.026 | -1.14 | -0.029 | -1.07 | | |
| <i>a16</i> <i>BANFIRM</i> | | | 0.001 | 4.11 *** | 0.001 | 4.11 *** | | |
| <i>a17</i> <i>BANFIRM*POST</i> | | | 0.009 | 5.89 *** | 0.009 | 5.88 *** | | |
| <i>a18</i> <i>BANFIRM*RELSS</i> _{<i>t</i>} | | | -0.012 | -3.32 *** | -0.009 | -2.38 ** | | |
| <i>a19</i> <i>BANFIRM*RELSS</i> _{<i>t-1</i>} | | | -0.016 | -3.04 *** | -0.013 | -2.30 ** | | |
| <i>a20</i> <i>BANFIRM*RELSS</i> _{<i>t-2</i>} | | | -0.007 | -1.66 * | -0.008 | -1.59 | | |
| <i>a21</i> <i>BANFIRM*RELSS</i> _{<i>t-3</i>} | | | -0.007 | -1.59 | -0.007 | -1.38 | | |
| <i>a22</i> <i>BANFIRM*RELSS</i> _{<i>t-4</i>} | | | 0.000 | 0.02 | -0.001 | -0.19 | | |
| <i>a23</i> <i>BANFIRM*POST*RELSS</i> _{<i>t</i>} | | | | | -0.079 | -2.86 *** | | |
| <i>a24</i> <i>BANFIRM*POST*RELSS</i> _{<i>t-1</i>} | | | | | -0.103 | -2.06 ** | | |
| <i>a25</i> <i>BANFIRM*POST*RELSS</i> _{<i>t-2</i>} | | | | | 0.016 | 0.33 | | |
| <i>a26</i> <i>BANFIRM*POST*RELSS</i> _{<i>t-3</i>} | | | | | -0.018 | -0.39 | | |
| <i>a27</i> <i>BANFIRM*POST*RELSS</i> _{<i>t-4</i>} | | | | | 0.012 | 0.26 | | |
| Standard Errors | Bootstrap | | Bootstrap | | Bootstrap | | | |
| N | 194,132 | | 194,132 | | 194,132 | | | |
| R-squared | 0.0308 | | 0.0311 | | 0.0055 | | | |
| ChiSq Test: <i>a11</i> + ... + <i>a15</i> = 0 | 20.54 | | 18.37 | | 2.5 | | | |
| p-value | 0.000 | | 0.000 | | 0.114 | | | |
| ChiSq Test: <i>a23</i> + ... + <i>a27</i> = 0 | | | | | 1.44 | | | |
| p-value | | | | | 0.231 | | | |

FIGURE 1
Timelines of Emergency Rule and Short Sales Ban

Panel A: Timeline of SEC Emergency Rule of July 2008



| Event | Date | Notes | Link |
|-------|-----------|--|---|
| A | 7/15/2008 | SEC issues a temporary emergency rule to stop naked short-selling in 19 major financial firms. The rule required any person making a short sale in the listed securities to borrow the securities before the short sale is effected and deliver the securities on the settlement date. | http://www.sec.gov/rules/other/2008/34-58166.pdf |
| B | 7/21/2008 | The emergency rule becomes effective. | http://www.sec.gov/rules/other/2008/34-58190.pdf |
| C | 7/29/2008 | SEC extends through Aug. 12 the emergency rule aimed at curbing abusive naked short-selling. | http://www.sec.gov/rules/other/2008/34-58248.pdf |
| D | 8/12/2008 | The emergency rule expires. | http://www.sec.gov/divisions/marketreg/emordersshortsalesfaq.htm |

Panel B: Timeline of Short Sales Ban of October 2008



| Event | Date | Notes | Link |
|-------|----------|---|---|
| A | 09/18/08 | After normal trading closed, the SEC initiates a ban on short selling for 799 stocks | http://www.sec.gov/rules/other/2008/34-58592.pdf |
| B | 09/21/08 | The SEC issues additional technical amendments to the original ban, which a) allowed exchanges to manage ban list, b) allowed market market exemption and c) discourage market maker's use derivatives. | http://www.sec.gov/news/press/2008/2008-218.htm |
| C | 10/02/08 | The SEC extends emergency rule and issues new rules concerning short selling, including a) a hard T+3 closeout requirement, b) Repeal of exception for options market makers from short selling close-out provisions in Regulation SHO, and c) addressing the legality of naked short selling | http://www.sec.gov/news/press/2008/2008-235.htm |
| D | 10/08/08 | Following the 10/03/08 announcement, the short selling ban expires at 11:59pm ET | http://www.sec.gov/news/press/2008/2008-238.htm . |

Additional Notes/Links

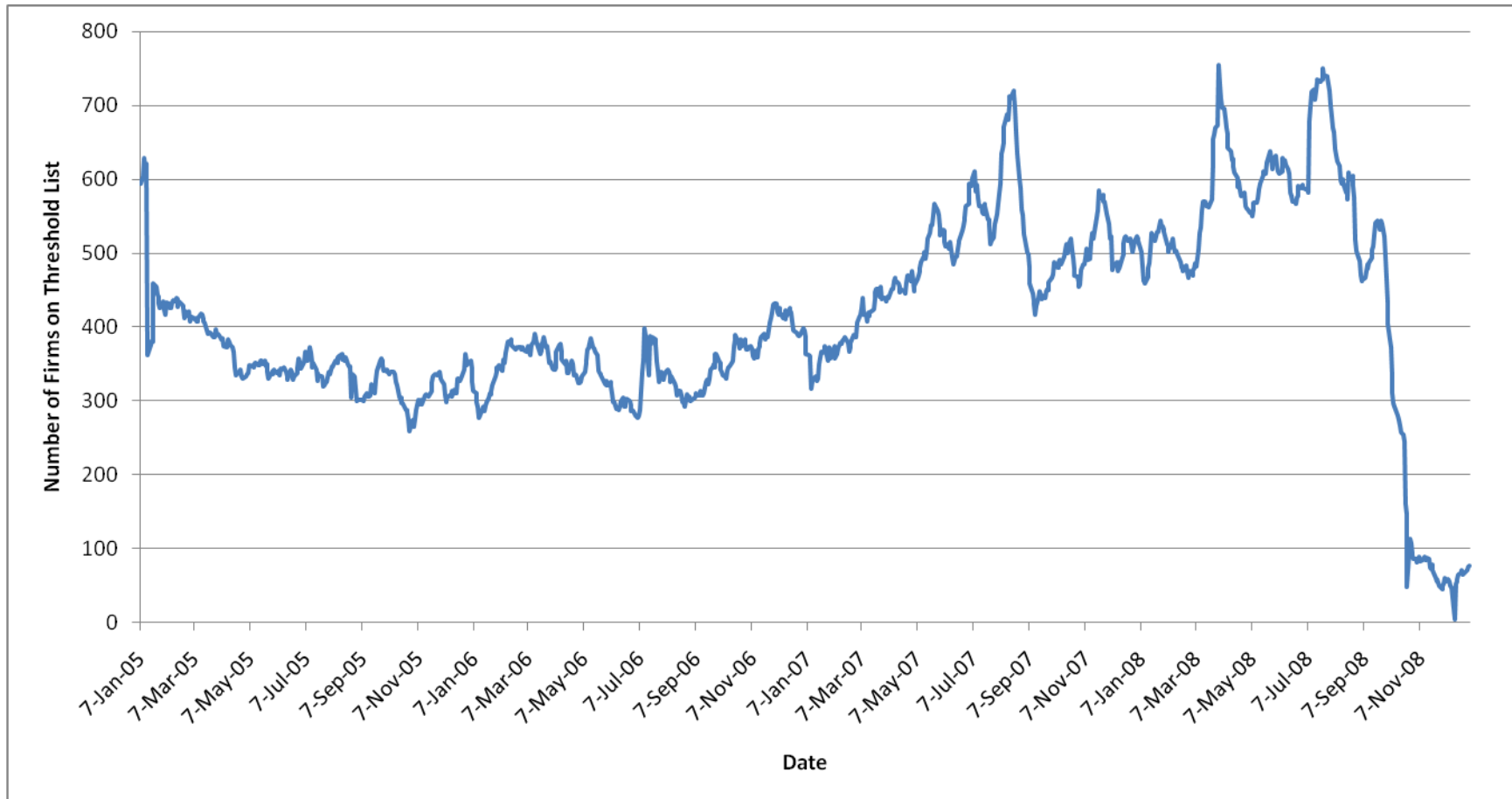
| | |
|--|---|
| List of All Firms Banned for All Exchanges | http://www.nyse.com/about/listed/1222078675703.html |
| Reuters Timeline of Short Selling Ban Events | http://www.reuters.com/article/regulatoryNewsFinancialServicesAndRealEstate/idUSN2336370820080923 |
| Short Selling Restrictions Around the World | http://dataexplorers.com/stock-restrictions-monitor |

FIGURE 2

Analysis of the Failures to Deliver for the NYSE, NASDAQ and AMEX

Panel A presents the total daily number of firms listed on the threshold list of the NYSE, NASDAQ and AMEX for the period 2005-2008. To create this figure, we downloaded the daily list available on each of the exchanges' websites and counted the number of firms for each day. The exchange websites are listed in the text above. Panel B presents the total daily dollar value fails-to-deliver from 2005 – 2008. The dollar value of failures to deliver is calculated as the number of shares failed to deliver times the closing price for the date of the fail. Failures to deliver are downloaded from the SEC website and closing price is from CRSP.

Panel A: Total Number of Firms on the Threshold List for 2005-2008



Panel B: Total Dollar Volume of Fails-to-Deliver for 2005-2008

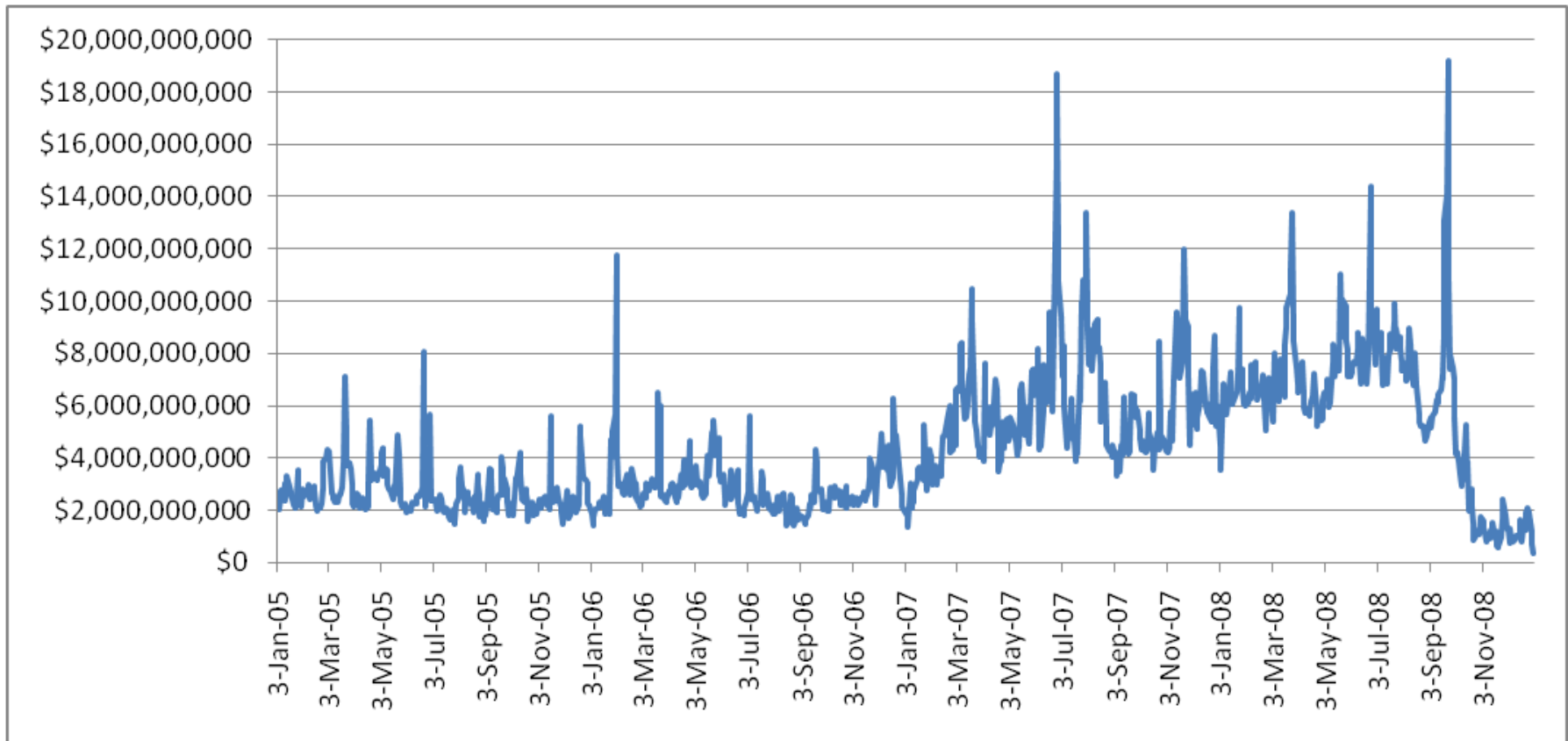


FIGURE 3

Average Daily Rebate Rates for Full Sample, Financial Firms and F19 Firms

This figure presents the mean daily rebate rate for the sample period of 2006 through mid-2008. We present the results for three subgroups of the sample: all firms in our sample (all_mean), all financial firms (fin_mean), which are firms with one-digit SIC code equal to six, and sample firms subject to the Emergency Order of July 2008 (mean_19). We also include the average daily federal funds rate (fedrate) for comparison.

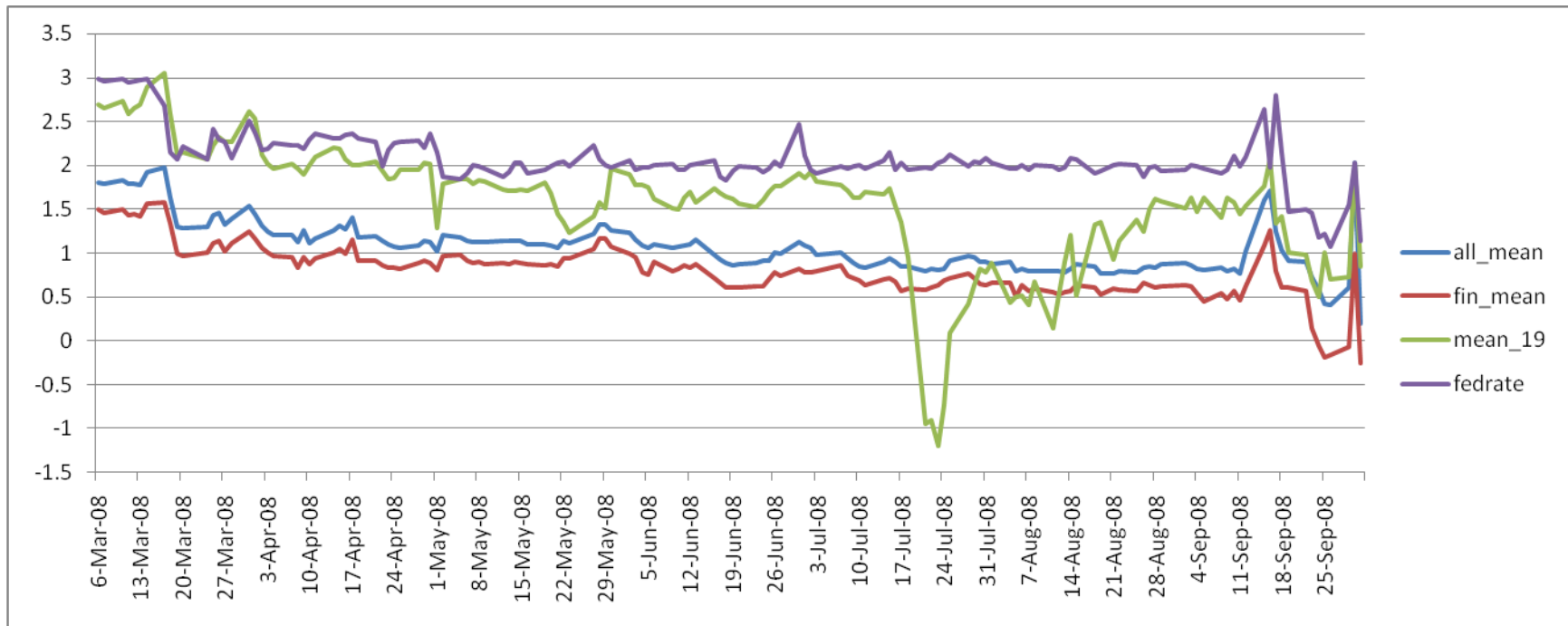


FIGURE 4

Short Interest Leading Up to the July 2008 SEC Emergency Order on Naked Short Selling

This figure presents the mean level of short interest for each bi-monthly announcement of short interest for the sample period of August 15, 2006 – September 15, 2008. Short interest is calculated as the level of outstanding short positions at settlement date as reported by the exchanges scaled by the number of shares outstanding from CRSP. We take the bi-monthly mean for three subgroups: all firms in our sample (all_mean), all financial firms in our sample (fin_mean) which are firms with one-digit SIC code equal to six, and the sample firms subject to the July Emergency Order (mean_19).

