WORKING PAPER 23-08

Are Short-selling Restrictions Effective?

OFR
OFFICE OF FINANCIAL RESEARCH

October 11, 2023

Yashar H. Barardehi
Chapman University
ybaradehi@chapman.edu

Thomas Ruchti
Office of Financial Research
Thomas.Ruchti@ofr.treasury.gov

Andrew Bird
Chapman University
abird@chapman.edu

Stephen A. Karolyi
Office of the Comptroller of the Currency
Stephen.Karolyi@occ.treas.gov

Key Findings

1. **Short-lived short-selling restrictions increase returns.**

   The impact on price persist in the days after restrictions are lifted.

2. **These restrictions also lower spot volatility.**

   This decrease may indicate that restrictions on short selling stabilize prices.

3. **Short-selling restrictions result in narrower spreads and an increase in depth at best-ask price.**

   This is consistent with the policy's rule restricting short sellers from placing marketable limit orders. It is likely that at least some short sellers switch from removing liquidity from the bid side to providing liquidity on the ask side.

Why These Findings Are Important

Short selling is often cited as a threat to market stability and price efficiency in times of crisis, but past research does not support or contradict this premise.

Identifying the impacts of short selling and short-selling restrictions in stocks experiencing price declines is important to gaining an understanding of how and when such adverse feedback loops occur and how they may be avoided.

How the Authors Reached These Findings

We compare returns for stocks that do and do not trigger Rule 201 short-selling restrictions. These restrictions are temporarily put in place for a stock experiencing a price decline of at least 10% in one day.

The temporary nature of the restrictions lend themselves to generating a valid control sample while mitigating conflating dynamics that may drive prices in the medium run.

Are short-selling restrictions Effective?*

Yashar H. Barardehi
Andrew Bird
Stephen A. Karolyi
Thomas G. Ruchti

October 11, 2023

Abstract

Despite strong predictions based on theories of disagreement, limited empirical evidence has linked short-selling restrictions to higher prices. We test this relationship using quasi-experimental methods based on Rule 201, a threshold-based policy that restricts aggressive short selling when intraday returns cross 10%. When comparing stocks on either side of the threshold in the same hour of trading, we find that the restriction leads to short-sale volumes that are 8% lower and daily returns that are 35 bps higher. These price effects do not reverse after the restriction is lifted.

JEL Classification: G12, G14, G18.

Keywords: short selling, uptick rule, securities regulation, Rule 201, short-selling restrictions

*Views and opinions expressed are those of the author and do not necessarily represent official positions or policy of the Office of the Financial Research (OFR) or the U.S. Department of the Treasury. The views expressed here are those of the authors and not necessarily the views of the Office of the Comptroller of the Currency, the U.S. Department of the Treasury, or any federal agency and they do not establish supervisory policy, requirements, or expectations. Moreover, the Securities and Exchange Commission disclaims responsibility for any private publication or statement of any SEC employee or Commissioner. This arti-cle expresses the authors' views and does not necessarily reflect those of the Commission, the Commissioners, or other members of the staff. We are grateful for comments received from Rui Albuquerque, Torben Andersen, Ted Berg, Kelley Bergsma, Dan Bernhardt, Julio Crego, Hans Degryse, Brent Glover, Larry Harris, Peter Haslag, Terry Hendershott, Burton Hollifield, Harrison Hong, Eric Hughson, Olivia Huseman, PK Jain, Tim Johnson, Christopher Jones, Nikolaos Karagiannis, Daniel Karney, Andrew Karolyi, David Lesmond (discussant), Stefan Lewellen, Zhi Li, Vitaly Meursault, Nate Neligh, Lars Nordén, Yoshio Nozawa (discussant), Mark Paddrik, Jackson Pfeiffer, Dave Porter, Matthew Ringgenberg (discussant), John Ritter, Priya Sankar, Michael Schneider, Duane Seppi, Timothy Shields, Esad Smajlbegovic (discussant), Chester Spatt, Marc Weidenmier, Ingrid Werner, Mao Ye, Peyton Young, Andrew Zhang, and seminar participants at Carnegie Mellon University, Chapman University, Ohio University, Oklahoma, the Office of Financial Research, the Securities and Exchange Commission, Financial Management Association meetings, HKUST Finance Symposium, SAFE Market Microstructure, and the Western Finance Association meet-ings. We also thank Kevin Roshak and Jonathan Brogaard for generously making available to us a list of stock-dates with individual price limit tests. Yashar H. Barardehi (barardehi@chapman.edu) is at the Argyros College of Business and Economics, Chapman University and the U.S. Securities and Exchange Commission; Andrew Bird (abird@chapman.edu) is at the Argyros College of Business and Economics, Chapman University; Stephen A. Karolyi (stephen.karolyi@occ.treas.gov) is at the Office of the Comptroller of the Currency, Department of the Treasury; and Thomas G. Ruchti (thomas.ruchti@ofr.treasury.gov) is at the Office of Financial Research, Department of the Treasury. Any errors are our own.

1 Introduction

Theory states that short sellers exert downward pressure on prices by contributing negative opinions to the market (Miller 1977; Harrison and Kreps 1978; Figlewski 1981; Morris 1996; Duffie, Gârleanu, and Pedersen 2002; Scheinkman and Xiong 2003; Hong and Stein 2007). However, because managers respond to market incentives and short selling can aid in price discovery (Engelberg, Reed, and Ringgenberg 2012), the threat of short selling could have positive disciplining effects through reduced financial misconduct (Karpoff and Lou 2010) and misreporting (Massa, Zhang, and Zhang 2015; Fang, Huang, and Karpoff 2016), or improved contracting with managers (De Angelis, Grullon, and Michenaud 2017). On the other hand, policies that place restrictions on short selling for a given stock suppress the negative opinions of short sellers and discourage the positive disciplining effects of short selling threats, so such policies have theoretically ambiguous effects on prices.

Drawing inferences about the role and effects of short selling from these policies is a challenge for two reasons. First, these restrictions often constrain all short selling, regardless of the role short selling may play in price discovery or hedging activities. Past short-selling restrictions have typically been implemented using temporary marketwide or industry-specific bans (e.g., during the financial crisis), or blanket restrictions for individual stocks (e.g., the Regulation SHO Pilot Program). Second, these restrictions are often direct policy responses to a market stimulus, which itself may affect stock prices. For example, on September 17, 2008, in the aftermath of the bankruptcy of Lehman Brothers, the SEC announced a ban on naked short selling for all stocks, and on September 18, it announced a ban on all short selling for 797 financial stocks (Battalio and Schultz 2011). Within a week, markets in Australia and Spain had banned short selling for all stocks, Greece, South Korea, and Japan followed the next month, and 15 developed nations ultimately enacted bans for financials (Beber and Pagano 2013). These actions were not unprecedented: in response to the stock market crash of 1929, the NYSE instituted several restrictions or all-out bans on short selling (Jones 2012). The question of whether short selling lowers prices is therefore difficult to answer in the absence of a policy targeting short selling that is not confounded by market conditions.

To address this challenge, we use variation from Rule 201 restrictions on short selling. Rule 201 is triggered for a stock when the stock's price declines by 10% or more from the previous day's close. When a stock is triggered, traders can only execute short sales of the stock above the National Best Bid (NBB) price. Under Rule 201 triggers, short sellers can therefore only submit non-marketable sell orders. This is in contrast to previous restrictions, such as the Rule 10a-1 "uptick rule," which did not restrict marketable short-sell orders. Under Rule 10a-1, short sellers needed only to wait for an uptick, after which, any order could be submitted and executed. The non-marketability Rule 201 restrictions, on the other hand, make it difficult to assure execution timing. This practically limits the aggressiveness traders can employ in their strategies. Moreover, because the policy is only in effect until the end of the trading day following a trigger, it limits managers' ability to meaningfully implement corporate policies in response to their stock being triggered. Therefore, the well-documented governance effects of short selling threats are unlikely to have a differential effect on triggered stocks.

We use a threshold-based design that takes advantage of the unique cross-sectional and time series variation of Rule 201 triggers. We find that Rule 201 restrictions increase daily returns for triggered stocks. When comparing stocks that reach an intraday low return of just above -10% (i.e., unrestricted stocks) to stocks that reach an intraday low return of just below -10% (i.e., restricted stocks) in the same hour of trading, we find that restricted stocks have subsequent daily returns that are approximately 35 bps higher. Because our control group consists of stocks that had experienced similarly negative returns, our estimate of 35 bps is incremental to any price rebound or trading response that would counterfactually happen in the absence of the policy (Bremer and Sweeney 1991). Given that non-marketable limit orders are unrestricted, short sellers wishing to provide liquidity can submit orders freely. Nonetheless, these price effects do not subsequently reverse in the days following the trigger event, which is consistent with the restricted short-selling opportunities being transient in nature.

The finding of an incremental positive return from restricting short selling is robust to alternative empirical specifications. These include simple bandwidth comparisons and alternative bandwidth choices within the regression discontinuity setting, including those optimally selected for each test (Imbens and Kalyanaraman 2012; Calonico, Cattaneo, and Titiunik 2014), with or without distance weights, and with higher order polynomial control functions. We also show graphical evidence consistent with all of our specifications. We find no evidence of an abnormal mass on either side of the -10% returns threshold (McCrary 2008; Cattaneo, Jansson, and Ma 2019), and we find no evidence of a discontinuity in stock characteristics around the same threshold. Finally, we find no systematic patterns in any of the outcomes of interest around placebo cutoffs (e.g., -5% and -15%). Together, these tests support our inference that the outcomes we measure are indeed driven by Rule 201 restrictions.

Rule 201 restrictions only prevent short sellers (not other traders) from using marketable sell orders in their trading strategies. If the price effects we find are indeed due to the short-selling restriction, we would also expect to see a significant decline in short-selling activity. Given the sizable contribution of short selling to overall trading volume, the proportion of seller-initiated volume should fall for triggered stocks. We document a 5% reduction in the proportion of seller-initiated volume (Lee and Ready 1991) for triggered stocks relative to untriggered stocks.[1] We complement this evidence with more direct analysis of short-selling activity. Using data on intraday short-selling volume available from FINRA, we find that short-selling activity in fact decreases by 8%.[2] We also find lower spot volatility, indicating that restrictions on short sellers stabilize stock price movement–a result that is likely in line with policymaker objectives (Schapiro 2010). We also find narrower spreads and an increase in depth at best ask price, suggesting that at least some short sellers switch from removing liquidity from the bid side to providing liquidity on the ask side. Of note, we find that an asymmetric change in depth at best prices (ask side, but not bid side) is inconsistent with behavioral anomalies at −10% or trading algorithms treating −10% return stocks differently.

The literature has provided limited modern evidence that short selling affects stock prices as predicted by theories of disagreement (Alexander and Peterson 2008; Diether, Lee, and

[1] Our findings are robust to the use of trade classification algorithms proposed by Ellis, Michaely, and O'Hara (2000) and Chakrabarty, Li, Nguyen, and Van Ness (2006).

[2] These data correspond to the Monthly Short Sale Transaction File (STF) provided by FINRA, which contains short-sale transactions for National Market System (NMS) stocks reported to the Alternative Display Facility or a Trade Reporting Facility. Alternative sources of related data concern daily equity lending (Ringgenberg 2014) and bi-weekly short interest disclosures provided by FINRA, but these are not as useful for our empirical design, which uses intraday data.

Werner 2009; Autore, Billingsley, and Kovacs 2011; Beber and Pagano 2013; Boehmer, Jones, and Zhang 2013).[3] Two papers study the effects of short-selling eligibility on stock prices on the Hong Kong Stock Exchange and they find mixed results (Chang, Cheng, and Yu 2007; Crane, Crotty, Michenaud, and Naranjo 2018).[4] Using policies from the 1930s, Jones (2012) finds that markets react positively to the introduction of short-selling restrictions. Our results otherwise contrast with the extant literature. Prior empirical work on short-selling restrictions studies interventions that were implemented either for many stocks at once or under specific market conditions, with long-lived enforcement periods. For example, Diether, Lee, and Werner (2009) study the effects of a closely related policy, the 10a-1 uptick rule, which, unlike Rule 201,[5] placed long-lived restrictions on all types of short-selling activity. That means lifting these restrictions (as the Regulation SHO Pilot Program did) could confound the predictions of disagreement theory with changes in governance.[6] Our findings extend prior work on return predictability of short interest announcements by providing a test of disagreement theory that separates the effects of short selling from short-seller stock selection (Senchack and Starks 1993; Asquith and Meulbroek 1995; Desai, Ramesh, Thiagarajan, and Balachandran 2002; Asquith, Pathak, and Ritter 2005).

Most closely related to our paper is Jain, Jain, and McInish (2012). The authors of that paper study Rule 201 restrictions using an event study design and they find evidence

[3]Using different sources of variation in lendable share supply, two recent papers find some evidence that short selling has return effects (Kaplan, Moskowitz, and Sensoy 2013; Ringgenberg 2014).

[4]Both of these studies use quarterly changes in the set of firms eligible to be sold short on the Hong Kong Stock Exchange. Notably, even for eligible firms, an uptick rule was in place during the period studied.

[5]In Table 1, we find that 94.9% of trigger episodes in our sample last no longer than one trading day following the triggering event. Our results are also robust to excluding multi-trigger episodes from the sample.

[6]Diether, Lee, and Werner (2009) find no price response to the announcement of the pilot program, which could be interpreted as evidence against disagreement theory. However, using an event study approach places a joint hypothesis on the effects of short-selling restrictions and whether investors understand those effects.

that stocks with Rule 201 restrictions have lower short-selling volume after the restriction is implemented. We use a regression discontinuity design to highlight cross-sectional differences between stocks that trigger a Rule 201 restriction and stocks that just fail to trigger a Rule 201 restriction, and we find evidence that Rule 201 restrictions reduce short-selling volume and increase returns.[7] More recent studies have also attempted to shed light on the effects of Rule 201 restrictions, albeit with different empirical designs. Using matching and event study approaches, Florindo (2021) and Florindo, Penalva, and Tapia (2022) find some evidence that Rule 201 restrictions increase returns on the day following the restriction. However, Halmrast (2015) finds little evidence for price recoveries in the immediate minutes after a Rule 201 restriction is triggered. The mixed evidence from past studies provides scope for our methodology to shed light on the impacts of Rule 201 restrictions.

2 Institutional background, data, and methods

2.1 Institutional background

On May 2, 2005, the SEC initiated a pilot program to investigate the efficacy of the prevailing uptick rule.[8] After the pilot program, the SEC concluded that price tests were not needed because they upset order flow by distorting short-selling order placement, and had no significant effects on market quality (SEC 2006; Diether, Lee, and Werner 2009; Boehmer and Wu 2013). Thus, on June 13, 2007, the SEC voted to eliminate the uptick rule for all

[7] Table 5 of Jain, Jain, and McInish (2012) also presents summary statistics of subsequent returns that suggest that stocks with Rule 201 restrictions may have had higher returns even in the absence of the restrictions. Our empirical methodology and statistical tests are designed to address this counterfactual.

[8] The uptick rule required short sales to be placed at or above the last traded price of the security (e.g. under the pre-2007 NYSE uptick rule), or at or above the last posted bid (e.g. under the pre-2007 NASDAQ bid price rule).

stocks, and also voted to prohibit any exchange from imposing a price test in the future.

In the wake of the financial crisis of 2007–08, public support for short-selling bans mounted, and on April 8, 2009, the SEC sought comment on proposals to restore a modified version of the uptick rule (SEC 2009). On February 24, 2010, the SEC amended previous short-selling rules to adopt Rule 201, also known as "alternative uptick rule", with the required compliance date of February 28, 2011. This applied to stocks in the National Market System (NMS) and would be triggered following an intraday price decline of 10% or more from the previous day's closing price. The rule imposed a requirement that short sales be placed above the NBB at the time of order submission. The short-selling restriction would begin immediately following the breach of the 10% threshold, as determined by the listing exchange,[9] and would last through the end of the next trading day. Specifically, Rule 201(b)(1) states that a

> trading center shall establish, maintain, and enforce written policies and procedures reasonably designed to: (i) Prevent the execution or display of a short sale order of a covered security at a price that is less than or equal to the current national best bid if the price of that covered security decreases by 10% or more from the covered security's closing price as determined by the listing market for the covered security as of the end of regular trading hours on the prior day; and (ii) Impose the requirements of paragraph (b)(1)(i) of this section for the remainder of the day and the following day when a national best bid for the covered security is calculated and disseminated on a current and continuing basis by a

[9] `https://www.sec.gov/divisions/marketreg/rule201faq.htm`

plan processor pursuant to an effective national market system plan.

This meant that Rule 201 required trading venues, following a decrease in a stock's value of 10% or more, to have policies in place that would catch short sale orders at a price equal to or below the national best bid before they were executed or even displayed. The rule was not specific about the increment above the national best bid at which a covered security could be sold short, despite arguments that an increment of at least one penny was necessary to promote market stability.[10] Moreover, Rule 201 provides exemptions to the rule, such that a security marked "short exempt" would be executed regardless of price tests.[11] Such a designation was determined to be important for surveillance by self-regulatory organizations (SROs) and the Securities and Exchange Commission.

Short-selling restrictions imposed by Rule 201 differ from those imposed by Rule 10a-1(a) in that Rule 201 relies on the current NBB price to apply restrictions, but Rule 10a-1(a) uses the immediately preceding sale transaction price as the reference point.[12] Rule 10a-1(a) allows short selling either at a price that constitutes an uptick with respect to the immediately preceding sale price or at the last sale price if it is higher than the previous transaction price. Rule 201 requires that, following a trigger for a given stock, short selling

[10]See SECURITIES AND EXCHANGE COMMISSION 17 CFR PART 242 Release No. 34-61595; File No. S7-08-09 RIN 3235-AK35 Amendments to Regulation SHO (https://www.sec.gov/files/rules/final/2010/34-61595.pdf).

[11]For example, any short-sale order going to an exchange from a broker-dealer that the broker-dealer determines is above the current national best bid at the time of submission would marked "short exempt" and executed without restriction. Similar provisions are made for sellers known to own the security (rather than engaging in a "naked" short), the transaction of odd lots, and in the execution of domestic arbitrages, among others. See SECURITIES AND EXCHANGE COMMISSION 17 CFR PART 242 Release No. 34-61595; File No. S7-08-09 RIN 3235-AK35 Amendments to Regulation SHO (https://www.sec.gov/files/rules/final/2010/34-61595.pdf).

[12]See Exchange Act Release No. 1548 (Jan. 24, 1938), 3 FR 213 (Jan. 26, 1938).

is allowed only at prices above the current NBB price. This means that short orders must be submitted either at the NBB price plus the minimum tick on exchanges or above the NBB price on trading venues without minimum tick requirements. The consequence is that Rule 201, when in effect, requires that all short orders submitted to exchanges be non-marketable limit orders—meaning execution timing cannot be assured.[13] Rule 10a-1 did not restrict marketable short-sell orders, as long as there was an uptick. In the exhibit below, we outline notable differences between our setting and studying Rule 10a-1 restrictions in the Reg SHO Pilot Program. Additionally, Figure 1 presents a simple numerical example demonstrating the difference between the two types of restrictions.

Differences in short-selling restrictions settings. This table outlines the differences between the Rule 201 restrictions we study in our setting and the 10a-1 restrictions as studied using the Regulation SHO Pilot Program. *Source: Center for Research in Securities Prices, Muzan TAQ*

	Rule 201	Rule 10a-1
Dates in effect	February 28, 2011–Present	May 2, 2005–August 6, 2007
Restrictions assignment	Restrictions triggered when individual stock's returns reach intraday low of -10%	Restrictions lifted for stocks in the Reg SHO Pilot Program
Restriction assignment frequency	Daily	Permanent
Average restriction period	1.64 trading days	574 trading days
Restriction type	Short sales must be placed above the national best bid at the time of order submission	Short sales are only allowed when the most recent price change preceding the trade was an uptick
Practical implications	Traders can only sell short using non-marketable orders	Traders must wait for an uptick in price to sell short

Critics of Rule 201 argued that short selling had nothing to do with the financial crisis and that this new rule had the potential to harm markets by decreasing market efficiency

[13]In Section 2.2, we explore the practical importance of this restriction by investigating the average time between a downtick and the first following uptick. We find that there are significant periods of time when Rule 201 triggers would place meaningful restrictions on trading strategies.

and reducing the ability of markets to expose overvalued stocks (Dealbook 2010). The SEC estimated startup implementation costs of $1 billion and yearly costs of $1 billion for the industry to maintain compliance, but senators Ted Kaufman and Johnny Isakson believed the rule would not do enough, "helping only in the worst-case scenarios that could occur during a terrorist attack or financial crisis (Johnson 2010)." At the time, there was concern that Rule 201 would reduce market quality through lower volume, poor price efficiency, wider bid-ask spreads, and higher intraday volatility (Jain, Jain, and McInish 2012). SEC Chair Mary Schapiro admitted the possible benefits of short selling, but she stated, "We also are concerned that excessive downward pressure on individual securities, accompanied by the fear of unconstrained short selling, can destabilize our markets and undermine investor confidence in our markets (Schapiro 2010)."

2.2 Data

Our sample runs from March 1, 2011 to March 31, 2013, and includes U.S.-based common shares listed on the NYSE, AMEX, and NASDAQ. The sample period commences as compliance with Rule 201 becomes mandatory on February 28, 2011, and it ends before the implementation of Limit Up-Limit Down rules in April 2013 because the effects of those rules may confound those of Rule 201.[14] We obtain daily closing prices, dividend distribu-

[14] We also verify that the SEC's price limit rules with similar circuit breakers do not drive our findings. Beginning on Sept. 10, 2010, SEC and FINRA required temporary (shorter than 10-minute) trading halts following price declines of 10% or more that realize within 5-minute intervals. The universe of stocks subject to these restrictions expanded from S&P500 stocks to all NMS stocks by June 23, 2011. The price limit restrictions are sensitive to price movements between 9:45am and 3:35pm. Brogaard and Roshak (2016) find that price limit restrictions "reduce the frequency and severity of extreme price movements, but induce price underreaction." Using the list of stock-dates made available to us by Kevin Roshak, we verify that (1) there is minor interaction between the two rules, and (2) price limit circuit breakers do not drive our findings. There are a total of 777 stock-dates containing price movements of 6% or more within five-minute

tions, price adjustment factors, and four-digit SIC industry codes from CRSP. We use trade- and quote-level data from Daily TAQ between 9:30 a.m. and 4:00 p.m. EST during the period from March 1, 2011 to March 31, 2013. We obtain information on tick-by-tick prices, transaction sizes, and the exchange at which each transaction took place with millisecond time stamps from the Consolidated Trades Tape. We match each transaction to the mid-point of the prevailing best bid and offer prices and aggregate quoted depth at the end of the previous millisecond. We construct best national bid and offer prices and the corresponding aggregate quoted depth at the millisecond frequency using the Consolidated Quotes Tape and National Best Bid and Offer (NBBO) files from the Daily TAQ database.

We calculate various trading outcomes over six equal-length (i.e., 65-minute) time intervals each trading day.[15] Returns are calculated using the transaction prices at the beginning and end of each intradaily interval. To identify trigger times using transaction prices, we also calculate intra-bin low returns with respect to the most recent close price.[16] We use the total number of shares traded over each interval to measure trading volume.

In Figure 2, we show the timeline of the daily number of triggers throughout our sample period. In the figure, we mark Black Monday (August 8, 2011), which saw an outsized number of triggered stocks.[17] Similarly, Figure 3 shows a histogram of the number of daily

intervals. Excluding these stock-dates from our sample leaves our findings unaffected.

[15]The literature aggregates trading outcomes at different levels, depending on the research question and the availability of data. Two common aggregations are one observation per day (daily aggregation) and 13 equal length observations per day (aggregation over 30 minute intervals). A trading-day interval may bias our findings since part of the enforcement period takes place subsequent to the intraday low reaching the trigger. In constrast, breaking up the day into 13 equal portions reduces the number of control firms available for estimation in each bin. We trade off these two competing issues in our choice to divide the day into 6 equally long intervals.

[16]We calculate returns accounting for price changes driven by overnight price adjustments (such as for stock splits or dividend distributions).

[17]While we do include date fixed effects in our main specifications, in untabulated tests, we also find that

triggers within our sample. We also show a scatter plot of the natural log of the daily number of triggers against daily market returns. While there is indeed a negative correlation between the daily market return and the number of triggers, there remains substantial variation. The short-selling restrictions we study are relatively short-lived, but it is possible that a stock would have multiple successive triggers. We also investigate the length of trigger episodes in Table 1. We find that 94.9% of trigger episodes last only one full day following the trigger day, indicating no successive triggers. The proportion of trigger episodes by length is depicted in Figure 4. Multiple day episodes should be represented more, as a proportion of treated stock-days. Nevertheless, in our main sample, single trigger observations make up 91.4% of our treated observations. If anything, including these multiple day episodes in our tests is conservative, as it includes many of the treatment observations with the most extreme negative returns. However, in untabulated tests we find that our results are robust to the exclusion of multiple trigger episodes.

We measure the extent of seller-originated order flow using the proportion of seller-initiated dollar volume in each interval. Transactions are classified into buyer- and seller-initiated orders using the Lee and Ready (1991) algorithm, based on the midpoint of national best quoted prices at the end of the millisecond prior to each transaction. Our findings are robust to the use of alternative trade classification algorithms proposed by Ellis, Michaely, and O'Hara (2000) and Chakrabarty, Li, Nguyen, and Van Ness (2006). We also calculate bid side and ask-side quoted depth at the best price, trade-weighted averages of quoted and relative effective spreads,[18] and both the magnitude and the proportion of trading volume

our results are robust to excluding this date from the sample.

[18]A transaction's quoted spread is the difference between NBBO prices at the millisecond a transaction is

executed off-exchange. To identify off-exchange trades, we use the trade flag 'D' in TAQ data that identifies trades reported to FINRA's Trade Reporting Facility.

We follow Andersen, Bollerslev, and Diebold (2010) in employing a measure of spot volatility to estimate return volatility at intraday frequencies. Spot volatility is measured as the square root of the sum of squared trade-by-trade returns over 65-minute intervals. Each trade-by-trade return is calculated using the *mid-point* prices from the millisecond *prior* to the respective transactions. As such, market microstructure noise and bid-ask bounce are unlikely to contaminate our measures of spot volatility.

We also construct the following stock characteristics. From Monthly CRSP data, we construct monthly market capitalization using closing price and shares outstanding observations at the end of the previous month. From Compustat data, we construct book value measures as the sum of stockholders' equity and deferred taxes at the end of each fiscal year. The most recent book value observation is assigned to all dates prior to the end of the subsequent fiscal year. We take the corresponding market capitalization measure and divide it by this book value to construct market-to-book ratios. From Daily CRSP data, we use daily opening and closing prices, and trading volumes to construct monthly measures of Amihud's (2002) illiquidity proxy using observations from the preceding month.[19] We utilize daily returns to also construct monthly measures of return volatility using observations from the preceding month. We use Beta Suite by WRDS to construct market betas at monthly frequency (end-of-month estimates) for each stock using weekly observations from the preceding 24-month

recorded. *Relative effective spread* is the absolute difference between the transaction price and the midpoint of best quoted prices at the previous millisecond divided by the midpoint of quoted prices.

[19]Following Barardehi et al. (2021), we divide daily absolute open-to-close returns by daily dollar volumes to construct price impact proxies that underlie Amihud's (2002) measure.

rolling windows of data. For all these measures we require the existence of at least one year of CRSP data history.

We obtain detailed transaction level short trade data from the Monthly Short Sale Transaction File (STF) provided by FINRA. The database contains "trade information for short-sale transactions in NMS stocks reported to the Alternative Display Facility or a Trade Reporting Facility during regular and after-market hours that are submitted by FINRA to a tape plan for dissemination purposes."[20] Consistent with the construction of our other outcome variables, we compile each stock's short-sale volume over 65-minute time intervals each trading day. We then normalize these off-exchange short-volume observations by dividing them by the corresponding total off-exchange trading volume obtained from TAQ (i.e., the *total* trading volume reported to the Alternative Display Facility or a Trade Reporting Facility). This ratio of short volume to off-exchange volume serves as our measure of short-selling activity. There are two issues with merging short-volume data with our main sample: (i) FINRA's STF does not provide comprehensive coverage, and (ii) there are instances where short volume reported by FINRA in an intraday bin exceeds off-exchange volume reported by TAQ; we exclude such observations from the sample. These issues lead to a sample that is slightly smaller than our CRSP-TAQ-Compustat sample.

The sample is constructed in the following order. We identify 4,489 NYSE-, AMEX-, and NASDAQ- listed common shares in the CRSP-Compustat linking table whose active links fall in the 2011–2013 period. We match LPERMNO from the linking table file and PERMNO

[20]FINRA Information Notice 9/29/09, page 2 (`http://www.finra.org/sites/default/files/NoticeDocument/p120044.pdf`)

from CRSP to merge CRSP's TSYMBOL and NCUSIP with Compustat identifiers. We then merge CRSP-Compustat links with TAQ: we match 8-digit CUSIPs constructed from Daily TAQ's Master files between 2011 and2013 to NCUSIPs from CRSP. For stocks without such links we match SYM_ROOT from TAQ to TSYMBOL from CRSP. Hence, we successfully match 4,267 individual stocks across CRSP, Compustat, and TAQ databases. We then remove a stock from the sample if it features fewer than 100 individual trades per calendar year on the TAQ database, cutting our sample to 3,561 individual stocks. We then apply the following filters: minimum daily closing price over each year must exceed \$1, book values must be reported for a stock by Compustat, the stock must have daily observations in the preceding 12 months, and a stock-date combination is included only if the three-day window around the date features non-zero trading volume during each of the 18 corresponding 65-minute bins. The last set of filters reduces the number of individual firms in the sample to 2,979. We are able to match information on 2,865 of these firms with short-volume measures obtained from FINRA's STF.

As we discuss in Section 2.1, Rule 201 short-selling restrictions require that short-sale orders be placed as non-marketable limit orders, and as such, they may not be immediately filled. As a test of whether or not this rule should matter for trading in practice, we explore downtick durations around the -10% threshold. Specifically, we measure the time between a downtick transaction and the first following uptick. These durations represent periods of time when a short seller is unlikely to see her passive sell order executed against. As such, this measure, $DTDUR$, indicates the degree to which Rule 201 short-selling restrictions may be binding.

In Figure 5, we display sample statistics of $DTDUR$ for our sample. In the left subfigure we plot quartiles and means of $DTDUR$ by percent intraday returns. One can see a significant positive skew to the distribution, with a median of 58 seconds, and a mean of 222 seconds. In the right subfigure, we plot a histogram of the natural log of $DTDUR$. Overall, this measure shows there are periods of time when Rule 201 triggers would place meaningful restrictions on traders within our sample.

In Table 2, we investigate stock characteristics for the sample surrounding the relevant threshold of Rule 201 triggers (−12%,−8%). We examine market capitalization (which we calculated as the previous day's closing price multiplied by the previous day's shares outstanding), market-to-book (the market capitalization divided by the most recent book value of equity), Amihud (2002) illiquidity and volatility (using daily data from the previous month), and market beta (estimated using a market model of weekly data from the prior 24 months). The table illustrates that the distribution of stocks in our sample is reasonably representative of the population. However, stocks in our sample tend to be smaller and have higher market beta, on average, which is to be expected because their inclusion in our sample requires them to have experienced an extreme return.

In Figure 6, we further explore the representativeness of our sample of stocks based on distributional overlap with the population of stocks. We show density plots of the logs of market capitalization, market-to-book ratio, Amihud (2002) illiquidity, volatility, and market beta (the five characteristics investigated in Table 2). Overall, these density plots indicate significant overlap between our sample and the population of stocks, which provides some support for the generalizability of findings from the Rule 201 setting. However, we also caution against extrapolating results from this study to the population of stocks based on these statistics because there could be unobserved or unmeasured characteristics that differ between stocks in the region of the Rule 201 threshold and the population of stocks.

2.3 Methodology

Our goal in this section is to explain our methodology for evaluating Rule 201 short-selling restrictions. This begins with generating a valid control group for stocks that trigger Rule 201. The control group must account for intraday return and trading dynamics because a stock triggered earlier in the day necessarily experiences a different trigger period (i.e., the remainder of the day and the next day) than a stock triggered later in the day. To facilitate intraday comparisons, we break up the day into six equal bins that are 65 minutes each in length. For each stock-day-bin observation, we calculate returns and other dependent variables of interest from the end of that bin to the end of the following trading day and then we divide by the number of bins. For bin 1 out of 6 of the trading day, this would be from the end of bin 1 through the end of bin 6 of the next day (which we label as bin 12). Then we divide by 11.[21]

Figure 7 illustrates the way in which we split up bins as well as the contrast between treatment and control observations. In the figure, we show a hypothetical treated stock (stock A) experiencing an intraday low return of -10% at noon (i.e., bin 3) on day t, with respect to the value at close on day $t-1$. The nine subsequent 65-minute bins ending at close on day $t+1$ comprise a treatment group observation. The figure also shows a hypothetical control stock (stock B) that experiences an intraday return of -9.9% at noon on day t. Our

[21] In the case of returns, we instead calculate the geometric mean to obtain a per-bin return.

methodology contrasts trading outcomes over the subsequent nine bins for stock A with the matching nine bins for stock B.[22]

Formally, we estimate the following equation—which we refer to hereafter as equation (1)—to evaluate the effect of the policy:

$$y_{jt}^{[x+1,\,12]} \;=\; \alpha + \beta \cdot TRG_{jt}^{x} + F(lr_{jt}^{x}) + G(TRG_{jt}^{x} \times lr_{jt}^{x}) + \text{fixed effects} + \varepsilon_{jt}^{x} \tag{1}$$

where $y_{jt}^{[x+1,\,12]}$ is the dependent variable of interest, for stock j, on date t for bins x+1 through 12. TRG_{jt}^{x} is an indicator variable that equals one if stock j is triggered in bin x, on date t. $F(\cdot)$ and $G(\cdot)$ are polynomial functions of the running variable, lr_{jt}^{x}, which is the intraday low return for bin x, on date t, plus 10%,[23] and the running variable interacted with TRG_{jt}^{x}, respectively. The separate control functions for lr_{jt}^{x} on either side of the threshold flexibly controls for return dynamics that are unrelated to the policy. The coefficient measuring the impact of Rule 201 restrictions is therefore β. In our preferred specification, we include time of day and date fixed effects. We restrict the sample to observations with intraday low returns within a 2% bandwidth on either side of the policy threshold, from −12% to −8%.[24] In all of our results, we follow the design-based approach to statistical inference and present standard errors clustered at the stock and date levels (Abadie, Athey, Imbens, and Wooldridge 2020).

[22] All stocks with intraday returns above −10% are potential control stocks (i.e., stock B). However, we impose a bandwidth to ensure that control stocks are selected from a set of stocks that experience negative intraday returns similar to the returns of stocks with intraday returns below the −10% policy threshold.

[23] Centering this variable, lr_{jt}^{x}, at the threshold of −10% means β_0 captures the discontinuity in the dependent variable without bias.

[24] In Section 3.3, we explore the robustness of our findings to alternative bandwidths, polynomials, and observation weights.

3 Short-selling restrictions and stock prices

In this section, we explore whether Rule 201 short-selling restrictions affect stock prices, using quasi-random variation in the application of restrictions that arise from the policy itself. In particular, Rule 201 restrictions apply following a -10% intraday return. This allows us to compare outcomes for stocks with intraday returns just below this -10% threshold (which trigger Rule 201 restrictions through the next day's close) to outcomes for stocks with intraday returns just above this -10% threshold.

We first focus on whether the implementation of Rule 201 restrictions affects stock prices during the period when restrictions are in place (i.e., from the time a stock reaches -10% intraday return to the next day's close) to evaluate the direct effect of short-selling restrictions on prices. We then analyze whether this direct effect persists after Rule 201 restrictions are lifted. We conclude this section by exploring the robustness of these findings and the internal validity of our methodology.

3.1 Do short-selling restrictions affect prices?

We first investigate whether Rule 201 short-selling restrictions affect stock prices. To do so, we estimate equation (1) with returns as the dependent variable. Our measure of return is divided by the number of 65 minute bins between the reference bin and the next day's close, so our estimates correspond to differences in 65 minute returns over the period during which Rule 201 restrictions would be imposed.

We first explore univariate differences in our testing variables in Table 3. We calculate the univariate differences in returns between treatment and control groups for three different bandwidths (1%, 1.5%, and 2%) and we find statistically significant differences in all of them. The estimates are stable, hovering between 5.0 and 5.1 bps per 65-minute bin across bandwidths. These univariate comparisons are suggestive, but we next turn to a more rigorous and robust regression discontinuity analysis to flexibly control for any subtle differences in outcomes based on variation in intraday returns within even these narrow bandwidths.

To visualize this identifying variation within narrow bandwidths of the -10% threshold, we present graphical evidence in Figure 8. This figure presents two regression discontinuity plots, each of which uses variation in intraday returns between -8% and -12%. The left plot depicts fitted linear polynomials on each side of the -10% cutoff, and the right plot presents polynomials fitted with an Epanechnikov (1969) kernel. These figures demonstrate that the statistically and economically significant univariate difference in returns around the Rule 201 threshold is robust to flexibly controlling for return patterns in a narrow window around the -10% intraday return threshold. These polynomials explicitly and non-parametrically control for heterogeneity in return patterns that might exist for stocks with different levels of intraday returns in the neighborhood of the -10% threshold. For example, one might expect stocks with more negative intraday returns to experience stronger short-term reversals than stocks with less negative intraday returns (Bremer and Sweeney 1991), making it all the more important to flexibly control for the pattern of subsequent returns with respect to the distribution of intraday returns.

In Table 4, we estimate equation (1) and in column (1), we find that Rule 201 is associated with a 6.75 bps positive abnormal return per bin. This aggregates to a daily return effect of roughly 41 bps. That is, when comparing stocks that reach an intraday low return of just below -10% (i.e., restricted stocks) with stocks that reach an intraday low return of just above -10% (i.e., unrestricted stocks) in the same hour of trading, we find that restricted stocks have 41 bps higher subsequent daily returns. Given that the trigger period averages 9.83 bins (1.64 trading days), the total effect of a Rule 201 restriction is 66 bps, on average.

In subsequent columns of Table 4, we present specifications that augment the one presented in column (1) with additional fixed effects and control variables. In column (2), we include time-of-day fixed effects, which adjusts for the time of day in which the Rule 201 restriction is triggered. Column (3) presents estimates for our preferred specification, which includes both time-of-day and date fixed effects, meaning comparisons are made between stocks that are just above and just below the -10% intraday return threshold on the same date and at the same time of day. Finally, in column (4), we add a vector of stock characteristics as control variables (i.e., market cap, market-to-book, Amihud (2002) illiquidity, volatility, and market beta).

All four of these specifications yield estimates that are quantitatively similar to our preferred estimate in column (3) and are statistically significant at conventional significance levels. In particular, all of these estimates are statistically significant at the 1% level when we use bootstrapped standard errors. Two of four, including our preferred specification in column (3), are statistically significant at the 1% level when we double cluster standard errors by stock and date. The estimates presented in Table 4 provide evidence of a statistically and economically robust impact of Rule 201 restrictions on returns. Our preferred specification suggests that Rule 201 restrictions increase 65-minute bin returns by 5.79 bps, on average, which translates to daily returns that are 35 bps higher.

3.2 Do short-selling restrictions persistently affect prices?

In the previous section, we presented evidence that stock returns are higher during Rule 201 restriction enforcement. However, Rule 201 restrictions are short-lived, only being enforced until the market close on the day following a triggering event. Therefore, we ask whether these price effects persist beyond the enforcement of Rule 201 restrictions or whether they reverse—the latter of which would imply that Rule 201 restrictions simply delay selling pressure. The answer to this question will have implications for our evaluation of the impact of short-selling restrictions on prices and, more broadly, our understanding of short-selling behavior.

We investigate this question by estimating equation (1) for returns observed during the days following the treatment date. Table 5 presents coefficients and Figure 9 plots these effects for the ten days from the triggering date. Of note, our day 1 effects include effects from bins on the triggering day 0. From the first subfigure of Figure 9, one can see that the difference in average 65-minute returns remains marginally positive for one trading day, before disappearing on day 3 and remaining insignificant afterwards. What we find is consistent with Rule 201 restrictions having a persistent impact on prices, and not simply delaying selling pressure. This suggests that the short-selling opportunities that are restricted by Rule 201 are transient in nature.

3.3 Robustness and internal validity

In this section we address the robustness of our findings and our interpretation of the effects of Rule 201 enforcement at the -10% threshold. First, we address choices of fixed effects, bandwidths, sample weights, and polynomials in threshold-based estimators, and we explore the robustness of our findings to various sample selection screens. Second, we investigate the internal validity of our approach by testing for the presence of manipulation and covariate balance around the threshold and by using placebo tests to evaluate whether our findings could be explained by factors other than Rule 201 enforcement. Finally, we present simulation evidence to illustrate statistical properties of our empirical design.

3.3.1 Specification robustness

In our main tables, we show that our estimates of the price effects of Rule 201 restrictions are stable across a set of baseline specifications that range from no fixed effects or control variables to time-of-day and date fixed effects in addition to stock characteristics as control variables. In this section, we investigate the robustness of our findings to a series of alternative specifications and sample selection criteria. Each row of Table 6 presents the same four specifications presented in Table 4, but alters the specification or sample selection criteria. As in Table 4, our preferred specification is presented in column (3) because it includes time-of-day and date fixed effects.

In row (1), we include stock fixed effects in our baseline set of specifications, and we find quantitatively similar estimates that are statistically significant at conventional significance levels. In our main specification, we estimate that Rule 201 restrictions increase 65-minute bin returns by 4.90 bps, or 29.40 bps per day, which is a statistically significant at the 1% level. In row (2), we re-estimate our baseline specifications using weighted least squares in which we weight observations by the inverse of the distance to the -10% intraday return threshold. Again, these estimates are statistically significant and quantitatively similar to our baseline estimates. The estimates in row (3) use an alternative definition of the dependent variable. Instead of using raw returns, we include industry-adjusted returns based on Fama-French 49 industries and we find similar results to our baseline specifications. In rows (4) to (7), we estimate local quadratic, cubic, quartic, and quintic polynomials with MSE-optimal bandwidths following Calonico et al. (2014). Estimates from this set of sixteen specifications are quantitively similar to our baseline estimates.

The remaining rows of Table 6 investigate robustness to alternative sample selection criteria. In rows (8) and (9), we exclude dates in which the daily market return is in the top or bottom decile of daily market returns in our sample, respectively. Similarly, in rows (10) and (11), we exclude observations for which the stock's Fama-French 49 industry has daily return in the top or bottom decile of daily industry returns, respectively. In row (12), we exclude Black Monday (August 8, 2011). In each of these specifications, we obtain statistically significant estimates and, in some cases, estimates with larger economic magnitudes than our baseline estimates. Finally, in rows (13) and (14), we exclude stocks that frequently experience Rule 201 restrictions in our sample (i.e., top decile of in-sample trigger frequency) and stocks that trigger a Rule 201 restriction based on overnight returns, respectively. We obtain estimates that are qualitatively similar to our baseline estimates in these specifications.

3.3.2 Internal Validity

To establish the internal validity of our test design, we first examine whether stock characteristics vary significantly around the discontinuity. In Figure 10, we plot the linear fit of five stock characteristics—market capitalization, market-to-book, Amihud (2002) liquidity, volatility, and market beta—against intraday low returns within a 2% bandwidth of the threshold of -10%. Table 7 presents discontinuity estimates at the threshold. We find no evidence of a discontinuity in these stock characteristics around the threshold. This indicates that differences in stock characteristics are unlikely to be driving our findings because stocks just above and below the -10% intraday return threshold are comparable.

Although stocks on each side of the threshold appear to be comparable based on observed characteristics, one may be concerned that they differ on unobserved characteristics. In particular, stocks may differ in the extent to which traders can manipulate the stock price in order to accelerate or avoid the imposition of Rule 201 restrictions. If, in addition, these differences in manipulability are correlated with subsequent return patterns, they may confound our interpretation of the Rule 201 restriction effects documented earlier. We explore differences in manipulability by studying potential bunching behavior around the -10% intraday return threshold. For example, if traders prefer to avoid triggering Rule 201 restrictions, we would expect to see bunching just above -10%.

To directly address the possibility of manipulability, we investigate statistics of intraday returns around the -10% threshold. Following McCrary (2008), we first present kernel density estimates using 1-basis-point bins of the intraday return distribution on each side of the

-10% threshold and we test for a discontinuity in mass of the distribution at the threshold. In Figure 11, we present graphical evidence of the results of this test of the null hypothesis of "no density break" at the threshold. The densities appear to be continuous at the threshold, so we fail to reject the null hypothesis of no manipulation. We also investigate the existence of a density break using the more recent approach proposed in Cattaneo, Jansson, and Ma (2019). This alternative approach uses updated methods to select the optimal bin size. Again, we find no evidence of a density break with this alternative test using unrestricted quadratic triangular polynomial control functions on each side of the threshold.[25] Because we find no abnormal density around the intraday returns threshold, we infer that anticipatory trading is unlikely to drive our main results.

Finally, we are interested in isolating outcomes that are driven by the effect of Rule 201 short-selling restrictions from potentially spurious trading patterns. We therefore explore placebo Rule 201 implementation thresholds (i.e., other than at -10% intraday returns). In Table 8, we present the same specifications as in Table 4, but we replace the true -10% intraday return threshold with placebo thresholds of -5%, -6%, -14%, and -15%. We reproduce the estimates from Table 4 for comparison. None of the placebo thresholds produce statistically significant effects on subsequent returns in the same direction as our main estimates at the true -10% threshold.[26] In Figure 12, we present estimates using our preferred specification with time-of-day and date fixed effects for all outcome variables at the placebo

[25]In unreported tests, we find no evidence to reject the null hypothesis of no manipulation in specifications with higher order polynomial control functions.

[26]We note that two specifications at the -15% placebo threshold produce estimates that are statistically significant at the 10% level. These estimates are in the opposite sign of our main estimates at the true -10% threshold and inconsistent with estimates obtained at the nearby -14% placebo threshold.

thresholds and, for comparison, the −10% threshold. Only at the true −10% threshold do we find evidence of a systematic effect across all of these outcome variables.

3.3.3 Simulation evidence

To investigate the statistical properties of our econometric design, we conduct a simulation analysis. Our goal is to create many new samples following the same data structure as in our main tests, and then estimate regressions on these samples to evaluate whether any statistical bias exists in our estimates.

Our sample data is comprised of three populations of raw returns: overnight returns, intraday bin returns, and next-day returns. We simulate a single stock-date by drawing one overnight return, six intraday returns, and one next-day return. We identify Rule 201 triggers by calculating the cumulative return at the end of each of the six intraday bins (i.e. overnight return plus bin-1 return) and then adding each successive bin return. To approximate the size of our actual dataset, we use this methodology to simulate 2,000 stocks on 500 trading days for each sample. To do this, we build samples by drawing observations, with replacement, from a simulated population of 1,000,000,000 stock-date observations.

Using this procedure, we create 1,000 simulated samples, and estimate our baseline specifications from Table 4. We present the mean coefficient and standard error across the 1,000 samples in Table 9, Panel A. In each of these specifications, we obtain simulated treatment effect estimates that are close to zero and exhibit no clear pattern of positive or negative estimates. The magnitudes are always smaller than the standard error and at least one order of magnitude smaller than our baseline estimates presented in Table 4. Because the data-generating process in this simulation randomizes intraday returns, there indeed should be no relationship between passing an intraday return threshold and subsequent returns. Thus, this simulation evidence is consistent with the absence of bias in our econometric framework because these tests correctly obtained a null effect.

The previous simulation analysis obtains a null result when no Rule 201 trigger effect is present. To verify that our econometric design obtains a true Rule 201 trigger effect in which the trigger effect is present, we analyze an extension in which we mechanically impose a 5 bps Rule 201 trigger effect, which is approximately the sign and magnitude of our preferred estimate. All other aspects of the simulation are held constant. As shown in Panel B of Table 9, we indeed obtain a statistically significant Rule 201 trigger effect that is statistically indistinguishable from the true trigger effect of 5 bps. This additional simulation evidence further mitigates concern about bias inherent in the econometric design.

To give a more granular sense of the variation in these estimates, we return to the original 1,000 simulation samples that show no effect of Rule 201 restrictions. In Figure 13, we present three histograms of estimates from these simulated samples that correspond to each of the three main specifications shown in Table 9, Panel A. In our preferred specification with time-of-day and date fixed effects, the mean estimate across these simulation samples is 0.57 bps and has a standard deviation of 1.17 bps. None of the 1,000 simulation samples produced an estimate exceeding our baseline estimate of 5.79 bps, implying a p-value of less than 0.001. Each of the three distributions of coefficients is approximately symmetric, and even the most extreme estimates do not approach our baseline estimates from Table 4.

The standard deviation of 1.17 bps can be compared with the standard-error estimates from Table 4. We took two approaches to estimate standard errors in our baseline specification: (1) double-clustering by stock and date and (2) bootstrapping. In our preferred specification, we obtained standard-error estimates of 1.81 bps in the double-clustered case, and 1.74 bps in the bootstrapped case. This underscores our choice to present double-clustered standard errors throughout the paper as a conservative principle. Moreover, the simulation evidence suggests that the standard approach to significance testing is appropriate in our econometric framework.

We next extend the simulation analysis to allow for cross-bin correlations in returns. That is, we allow for the return during a particular stock date in bin 2 to differ depending on the return that stock experienced in bin 1. We do this non-parametrically by first partitioning the universe of intraday bin returns into N parts. Then, for each of these partitions n, we collect the subset of bin t+1 returns that followed the bin t returns in the range of returns denoted by n. To draw the return for bin t+1, we randomly choose a return, with replacement, from this subset.

This approach allows for the possibility of either short-term momentum or reversals, either on average or within specific intervals of the return distribution. For example, there could be larger short-term reversals for more extreme past returns, and this phenomenon could differ for more extreme positive vs. more extreme negative past returns. In Table 9, Panel C, we present results using the same parameters and specifications discussed above, except that we now use this adjusted return distribution with 2, 5, and 10 partitions (i.e., N=2, N=5, or N=10). That is, after the first bin of the day, each bin t+1 return is drawn from one of the N distributions of subsequent returns selected by the bin t return. We find evidence consistent with our baseline simulation methods and find no evidence that the treatment effect varies with the granularity of the partitions. This extension provides further evidence mitigating concern about bias inherent to the econometric design.

4 Short-selling restrictions and trading activity

In this section, we explore the impacts of Rule 201 restrictions on trading strategies to help shed light on the potential channels through which short-selling restrictions affect prices. We first investigate trading strategies that are likely symptomatic of constraints placed on short sellers, including a measure of seller-initiated volume that may proxy for short-selling volume. We then explore the direct effects of Rule 201 restrictions on short-selling volume, using a dataset of short-selling transactions from FINRA. By illustrating the direct effects of short-selling restrictions on short-sale transactions and trading strategies, these findings support the internal validity of our empirical design and suggest potential mechanisms by which Rule 201 restrictions affect prices.

4.1 Do short-selling restrictions affect trading strategies?

We present estimates of the impacts of Rule 201 restrictions on trading outcomes in Table 10. The table structure mimics that of our Table 4, with four specifications presented for each outcome variable. Our preferred specification is presented in column (3), and it includes both time-of-day and date fixed effects to control for secular trends in returns and the length of potential Rule 201 restrictions. As we describe in Section 2.1, Rule 201 short-selling restrictions are different from previous Rule 10a-1(a) restrictions in that they preclude traders from placing marketable short-sale orders. Given that any short orders will thus be submitted on the ask side of the order book, this should lead to lower seller-initiated volume. In Table 10, we show that the proportion of seller-initiated volume (as classified by the Lee and Ready 1991 algorithm) falls by 4.60% for triggered stocks. In unreported results, we document quantitatively similar declines in the proportion of seller-initiated volume when we utilize trade classification algorithms proposed by Ellis, Michaely, and O'Hara (2000) and Chakrabarty, Li, Nguyen, and Van Ness (2006) in place of the Lee and Ready (1991) classification.

If, under Rule 201 restrictions, traders submit non-marketable limit orders in order to sell short, it should lead to an increase in depth on the ask side of the limit order book. In the second panel of Table 10, we show that depth at the best ask price increases by 11.06% for triggered stocks. This finding is robust across specifications. When examining depth at the best bid price, we find a statistically insignificant coefficient that is not quantitatively robust across specifications.[27] In keeping with what one would expect given the economics of Rule 201, we find that ask-side depth increases under short-selling restrictions whereas bid side depth sees no significant change.

From the policymakers' perspective, this regulation is aimed at stabilizing markets (Schapiro 2010). We thus explore in Table 10 how short-selling restrictions affect stock return volatility. We find that triggers are associated with a decrease in spot volatility, indicating that Rule 201 restrictions likely stabilize markets. Moreover, this indicates that the impacts of Rule

[27]This result is not surprising in light of the fact that the policy does not directly target bid-side liquidity provision. Depth at the best bid price may change endogenously, but the direction is theoretically ambiguous. In equilibrium, depth at best bid price could decrease in response to decreased liquidity taking by short sellers. On the other hand, lower information asymmetry could itself induce greater depth at the best bid price.

201 restrictions on returns are not due to compensation for increased risk. Another potential impact of restricting short selling is that it might widen spreads if short sellers provide liquidity when other market makers do not (Comerton-Forde, Jones, and Putniņš 2016).[28] However, because Rule 201 restrictions preclude short sellers from posting marketable sell orders, depth at the best ask price increases. This means that restrictions, through increased liquidity provision, could drive traders to narrow spreads as well. Therefore, in Table 10, we also investigate the effects of triggers on quoted spreads and effective spreads. Both effective spreads and quoted spreads fall for triggered stocks, indicating that Rule 201 restrictions drive trading strategies. These findings on reduced spreads also illustrate another potential benefit of the policy.

Our results on reduced selling activity, increased ask-side depth, and narrower spreads are broadly consistent with past studies (Diether, Lee, and Werner 2009). However, with the exception of Jones (2012), our finding that short-selling restrictions have a positive effect on returns and are associated with a decrease in volatility contrasts with the existing literature. Other recent papers have that these restrictions have had no effect in supporting prices, whether the restrictions were in the form of policy experiments such as Reg SHO (Diether, Lee, and Werner 2009) or wholesale bans during the financial crisis (Beber and Pagano 2013; Boehmer, Jones, and Zhang 2013).[29]

[28]If instead short sellers hold private information and trade on it (Aitken, Frino, McCorry, and Swan 1998), then a short-selling restriction could be beneficial for liquidity.

[29]Autore, Billingsley, and Kovacs (2011) find higher abnormal returns associated with the 2008 short-sale ban, though Boehmer, Jones, and Zhang (2013) argue that the effects of the ban are confounded by TARP and other government efforts to help the financial sector.

4.2 Do short-selling restrictions affect short-selling activity?

Rule 201 restrictions were designed to restrict short-sale transactions and theory provides a natural connection between restricted short selling and the outcomes measured in previous sections. But do Rule 201 restrictions actually restrict short selling? In this section, we use short-sale volume data provided in FINRA's Monthly Short Sale Transaction Files (STF) to document the direct impacts of Rule 201 restrictions on short-selling activity. Our analysis provides evidence that the price recovery and trading effects documented in earlier sections are indeed associated with reductions in short-selling activity due to restrictions imposed by the policy.

The STF short-sale data exclusively contains short-sale transactions that are reported to the Alternative Display Facility or to a Trade Reporting Facility, and so that data does not include short volume from exchanges. Additionally, cross-stock coverage is not comprehensive. Thus, we conduct the analysis containing these short-volume data as a supplemental test of internal validity to support our main analysis. Within the sample of stock days featuring short-volume observations in STF, we find evidence of negative impacts of Rule 201 restrictions on short-selling activity.[30] In unreported results, we also estimate effects on returns, seller-initiated volume, depth at best ask price, and spot volatility within this FINRA subsample that are quantitaitively similar to those presented in Tables 4 and 10. This suggests that the FINRA subsample is representative of the full sample used in our earlier tests.

We measure each stock's short-selling activity by the ratio of off-exchange short volume

[30] This effect is consistent with Jain, Jain, and McInish's (2012) finding that for triggered stocks, short volume drops during the minutes after the intraday return crosses the -10% threshold.

to overall off-exchange trading volume over 65-minute intervals, accounting for the fact that FINRA's STF reports off-exchange short volume only. We aggregate stock-bin observations over relevant periods of measurement $[x+1, 12]$ for intraday triggers in bin x to construct the corresponding measures of short activity $STO_{jt}^{[x+1,12]}$. We then estimate equation (1) with the natural log of short activity as the dependent variable. In Table 11, we find that short-selling activity decreases by roughly 7%–8% while Rule 201 restrictions are effective, which is consistent with short-seller behavior underlying the patterns in other outcome variables.

5 Conclusion

We ask whether restrictions placed on short selling impact prices and trading. To do so, we apply a regression discontinuity design to the setting of Rule 201, which restricts the placement of marketable short-sale orders once stocks reach an intraday return of -10%. We find that Rule 201 restrictions lead to a persistent increase in prices for triggered stocks, relative to a control group of stocks experiencing similar intraday returns. These results are robust to various specification choices and sample choices. The restrictions on marketable short-sale orders reduce short selling and overall selling pressure, lead to increased ask-side depth, and decrease spot volatility.

Our finding of persistent price increases for triggered stocks suggests that the policy may restrict temporary price pressure, which is consistent with the stated objectives of policy-makers (Schapiro 2010). Since our findings are based on a threshold methodology, they may not extend to counterfactual thresholds or to stocks experiencing less-negative returns. However, we believe that the Rule 201 setting delivers a useful local average treatment effect since policymakers have historically been concerned with stocks in this part of the return distribution, as illustrated by where and when short-selling restrictions have been implemented. Although we expect that the fundamental economic mechanisms driving our findings are likely to be operative elsewhere in the distribution of intraday low returns, we look to future work using different sources of policy variation to further assess the quantitative importance of alternative short-selling restrictions.

References

Aitken, M., A. Frino, M. McCorry, & P. Swan, (1998). Short sales are almost instantaneously bad news: Evidence from the Australian Stock Exchange. Journal of Finance 53(6), 2205-2223.

Alexander, G., & M. Peterson, (2008). The effect of price tests on trader behavior and market quality: An analysis of Reg SHO. Journal of Financial Markets, 11(1), 84-111.

Amihud, Y. (2002). Illiquidity and stock returns: Cross-section and time-series effects. Journal of Financial Markets, 5(1), 31-56.

Andersen, T.G., T. Bollerslev, & F. Diebold, (2010). Parametric and nonparametric volatility measurement. In *Handbook of Financial Econometrics, Volume 1—Tools and Techniques.*, ed. Yacine Ait-Sahalia and Lars Peter Hansen, 67–137. North Holland.

Asquith, P., & L. Meulbroek, (1995). An empirical investigation of short interest. Working paper.

Asquith, P., P. Pathak, & J. Ritter, (2005). Short interest, institutional ownership, and stock returns. Journal of Financial Economics, 78(2), 243-276.

Autore, D., R. Billingsley, & T. Kovacs, (2011). The 2008 short sale ban: Liquidity, dispersion of opinion, and the cross-section of returns of US financial stocks. Journal of Banking & Finance, 35(9), 2252-2266.

Barardehi, Y., D. Bernhardt, T. Ruchti, & M. Weidenmier, (2021). The night and day of Amihud's (2002) liquidity measure. Review of Asset Pricing Studies, 11(2), 269-308.

Battalio, R. & P. Schultz, (2011). Regulatory Uncertainty and Market Liquidity: The 2008 Short Sale Ban's Impact on Equity Option Markets. Journal of Finance, 66(5), 2013-53.

Beber, A., & M. Pagano, (2013). Short-selling bans around the world: Evidence from the 2007-09 crisis. Journal of Finance, 68(1), 343-381.

Boehmer, E., C. Jones, & X. Zhang, (2013). Shackling short sellers: The 2008 shorting ban. Review of Financial Studies, 63(2), 491-527.

Boehmer, E., & J. Wu, (2013). Short selling and the price discovery process. Review of

Financial Studies, 26(2), 287-322.

Bremer, M., & R. Sweeney, (1991). The reversal of large stock-price decreases. Journal of Finance, 46(2), 747-754.

Brogaard, J., & K. Roshak, (2016). Prices and price limits. Working paper.

Calonico, S., M. Cattaneo, & R. Titiunik, (2014). Robust nonparametric confidence intervals for regression discontinuity designs. Econometrica, 82(6), 2295-2326.

Cattaneo, M., M. Jansson, & X. Ma, (2019). Simple local polynomial density estimators. Journal of the American Statistical Association, 1-7.

Chakrabarty, B., B. Li, V. Nguyen, & R. Van Ness, (2006). Trade classification algorithms for electronic communication networks. Journal of Banking and Finance 31, 3806-3821.

Chang, E., J. Cheng, & Y. Yu, (2007). Short-Sales Constraints and Price Discovery: Evidence from the Hong Kong Market. Journal of Finance, 62(5), 2097-2122.

Chen, K., Z. Chen, & K. Wei, (2009). Legal protection of investors, corporate governance, and the cost of equity capital. Journal of Corporate Finance, 15(3), 273-289.

Comerton-Forde, C., C. Jones, & T. Putniņš, (2016). Shorting at close range: A tale of two types. Journal of Financial Economics, 121(3), 546-568.

Crane, A., K. Crotty, S. Michenaud, & P. Naranjo, (2018). The causal effects of short-selling bans: Evidence from eligibility thresholds. Review of Asset Pricing Studies, 9(1), 137-170.

Dealbook, (2010). "Hedge funds slam short-sale rule." New York Times Dealbook, February 25.

De Angelis, D., G. Grullon, & S. Michenaud, (2017). The effects of short-selling threats on incentive contracts: Evidence from an experiment. Review of Financial Studies, 30(5), 1627-1659.

Desai, H., K. Ramesh, S. Thiagarajan, & B. Balachandran, (2002). An investigation of the informational role of short interest in the Nasdaq market. Journal of Finance, 57(5), 2263-2287.

Diether, K., K. Lee, & I. Werner, (2009). It's SHO time! Short-sale price tests and market quality. Journal of Finance, 64(1), 37-73.

Duffie, D., N. Gârleanu, & L. Pedersen, (2002). Securities lending, shorting, and pricing. Journal of Financial Economics, 66(2-3), 307-339.

Ellis, K., R. Michaely, & M. O'Hara, (2000). The accuracy of trade classification rules: Evidence from Nasdaq. Journal of Financial and Quantitative Analysis 35, 529-552.

Engelberg, J., A. Reed, & M. Ringgenberg, (2012). How are shorts informed? Short sellers, news, and information processing. Journal of Financial Economics, 105(2), 260-278.

Epanechnikov, V. A. (1969). Non-parametric estimation of a multivariate probability density. Theory of Probability & Its Applications, 14(1), 153-158.

Fang, V. W., A. Huang, & J. Karpoff, (2016). Short selling and earnings management: A controlled experiment. Journal of Finance, 71(3), 1251-1294.

Figlewski, S. (1981). The informational effects of restrictions on short sales: Some empirical evidence. Journal of Financial and Quantitative Analysis, 16(4), 463-476.

Florindo, O. (2021). Three Essays on Short Selling Bans, Asset Pricing, and Market Quality. Doctoral Thesis, Universidad Carlos III de Madrid.

Florindo, O., J. Penalva, & M. Tapia, (2022). Operate, not amputate: A surgical approach to dealing with toxic short selling. Working Paper.

Halmrast, N. (2015). Essays in Financial Economics and Banking. Doctoral Thesis, University of Toronto.

Harrison, J., & D. Kreps, (1978). Speculative investor behavior in a stock market with heterogeneous expectations. Quarterly Journal of Economics, 92(2), 323-336.

Hong, H., & J. Stein, (2007). Disagreement and the stock market. Journal of Economic Perspectives, 21(2), 109-128.

Imbens, G., & K. Kalyanaraman, (2012). Optimal bandwidth choice for the regression discontinuity estimator. Review of Economic Studies, 79(3), 933-959.

Jain, C., P. Jain, & T. McInish, (2012). Short selling: The impact of SEC Rule 201 of 2010.

Financial Review, 47(1), 37-64.

Johnson, F. (2010). "In 3-2 vote, SEC limits short sales." Wall Street Journal, February 25.

Jones, C. M. (2012). Shorting Restrictions: Revisiting the 1930s. The Financial Review 47(1), 1-35.

Kaplan, S., T. Moskowitz, & B. Sensoy, (2013). The effects of stock lending on security prices: An experiment. Journal of Finance, 68(5), 1891-1936.

Karpoff, J. M., & X. Lou, (2010). Short sellers and financial misconduct. Journal of Finance, 65(5), 1879-1913.

Lee, C., & M. Ready, (1991). Inferring trade direction from intraday data. Journal of Finance, 46(2), 733-746.

Massa, M., B. Zhang, & H. Zhang, (2015). The invisible hand of short selling: Does short selling discipline earnings management? Review of Financial Studies, 28(6), 1701-1736.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. Journal of Econometrics, 142(2), 698-714.

Miller, E. (1977). Risk, uncertainty, and divergence of opinion. Journal of Finance, 32(4), 1151-1168.

Morris, S. (1996). Speculative investor behavior and learning. Quarterly Journal of Economics, 111(4), 1111-1133.

Ringgenberg, M. (2014). Price pressure from short selling. Working paper.

Schapiro, M. (2010). SEC Open Meeting, February 24, Washington DC. `https://www.sec.gov/news/speech/2010/spch022410mls-shortsales.htm.`

Scheinkman, J. A., & W. Xiong, (2003). Overconfidence and speculative bubbles. Journal of Political Economy, 111(6), 1183-1220.

SEC, (2006). "Economic analysis of the short-sale price restrictions under the regulation SHO pilot." Working paper, Securities and Exchange Commission.

SEC, (2009). "SEC seeks comments on short sale price test and circuit breaker restrictions." Press release, Securities and Exchange Commission.

Senchack, A. J., & L. Starks, (1993). Short-sale restrictions and market reaction to short-interest announcements. Journal of Financial and Quantitative Analysis, 28(2), 177-194.

Figures

Table 1: **The length of trigger episodes.** This table displays trigger episode lengths in days. A usual trigger episode lasts one full day following the trigger day. As this figure depicts, a small proportion of trigger episodes lasts longer than one day. In all, there are 5,296 trigger episodes for 1,891 stocks in our main sample, corresponding to 5,547 full trading days. The median number of triggers among stocks that ever experience a trigger is 2. *Source: Center for Research in Security Prices, Muzan TAQ*

	Stock episode		Full sample		Main sample	
Rule 201 episode (days)	Observations	Frequency	Observations	Frequency	Observations	Frequency
1	8,162	94.85%	8,162	89.31%	5,071	91.42%
2	384	4.46%	768	8.40%	397	7.16%
3	41	0.48%	123	1.35%	60	1.08%
4	12	0.14%	48	0.53%	18	0.32%
5	3	0.03%	15	0.16%	1	0.02%
6	1	0.01%	6	0.07%	0	0.00%
7	0	0.00%	0	0.00%	0	0.00%
8	1	0.01%	8	0.09%	0	0.00%
9	1	0.01%	9	0.10%	0	0.00%
Total	8,605	100.00%	9,139	100.00%	5,547	100.00%

Table 2: **Summary statistics of key stock characteristics.** This table displays stock characteristics based on beginning-of-year observations, first for the sample of 2,387 stocks surrounding the relevant threshold of Rule 201 triggers (−12%, −8%) and then for the population of 2,979 stocks for which there is data during our sample period, March 2011–March 2013. We produce the mean, standard deviation, and 25$^{\text{th}}$, 50$^{\text{th}}$ (median), and 75$^{\text{th}}$ percentiles for market capitalization, market-to-book, Amihud (2002) illiquidity, volatility, and market beta. Market capitalization is the natural log of the product of closing prices and shares outstanding at the end of the previous month. Similarly, market-to-book is constructed using end-of-month market capitalization in ratio to the corresponding book value of equity. Illiquidity is the natural log of Amihud's (2002) measure, calculated using daily data from the previous month. Volatility is the daily-return standard deviation over the previous month. Market betas are estimated in a market model using weekly data from the 24 months ending the month prior to the trigger. *Source: Center for Research in Security Prices, Muzan TAQ, Wharton Research Data Services*

		Market cap.	Market-to-book	Illiquidity	Volatility	Beta
Main sample	Mean	2,644.1	4.88	−5.40	0.026	1.30
	Standard Deviation	8,507.3	50.11	2.42	0.023	0.54
	25^{th} percentile	211.3	1.18	−7.16	0.017	0.94
	Median	638.6	1.91	−5.53	0.022	1.26
	75^{th} percentile	1,940.5	3.51	−3.70	0.030	1.61
Population	Mean	4,887.7	4.22	−5.55	0.023	1.19
	Standard Deviation	19,655.7	39.01	2.72	0.019	0.54
	25^{th} percentile	223.4	1.11	−7.54	0.014	0.82
	Median	749.1	1.78	−5.72	0.020	1.15
	75^{th} percentile	2,716.8	3.17	−3.68	0.028	1.51

Table 3: **Univariate differences across different bandwidths.** This table displays the differences between the unconditional averages of 65-minute returns (R), in basis points, on the two sides of the -10% running variable threshold during our sample period, March 2011–March 2013. Bandwidths are presented for 1%, 1.5%, and 2% on either side of the threshold. Estimates control for both stock and date fixed effects. Standard errors, reported in parentheses, are clustered at the stock and date level. Symbols *, **, and *** reflect the statistical significance of differences at the 10%, 5%, and 1% levels, respectively. *Source: Center for Research in Security Prices, Muzan TAQ*

Dependent variable: R_{jt}	Control	Treatment	Difference
Bandwidth=1%	19.6	24.7	5.1***
	(3.7)	(3)	(1.8)
Bandwidth=1.5%	21.7	26.9	5.1***
	(3.6)	(2.5)	(1.8)
Bandwidth=2%	23.1	28.1	5.0***
	(3.3)	(2.2)	(1.8)

Table 4: **Impacts of short-selling restrictions on returns.** This table presents estimation results for 65-minute returns (R), in bps, using equation (1), given the policy trigger of -10%. Column (1) estimates include no fixed effects; column (2) estimates include trigger time-of-day fixed effects; column (3) estimates include both trigger time-of-day and date fixed effects; and column (4) augments (3) with stock characteristics (market cap, market-to-book, Amihud illiquidity, volatility, and market beta). The sample period is March 2011–March 2013. The first row of numbers in brackets report standard errors clustered at the stock and date level, and the second row of numbers in brackets reports bootstrapped standard errors with 1,000 replications. Symbols *, **, and *** reflect the statistical significance at the 10%, 5%, and 1% levels, respectively and they correspond to the more conservative significance levels among the displayed standard-error estimates. *Source: Center for Research in Security Prices, Muzan TAQ, Wharton Research Data Services*

Dependent variable: R	(1)	(2)	(3)	(4)
TRG	6.75**	6.64**	5.79***	5.87***
Double-clustered	(2.82)	(2.77)	(1.81)	(1.80)
Bootstrapped	(2.09)	(2.10)	(1.74)	(1.69)
Time-of-day FE	No	Yes	Yes	Yes
Date FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R^2	0.0032	0.011	0.32	0.32
N	25838	25838	25836	25836

Table 5: **Long-term effects of short-selling restrictions on returns.** This table estimates the dynamic effects of short-selling restrictions on returns several trading days after restrictions are reset. Effects for treated stocks on days subsequent to the trigger are estimated using equation (1). Estimates account for date and time-of-day fixed effects. The sample period is March 2011–March 2013. Standard errors are clustered at the stock and date level. Symbols *, **, and *** reflect the statistical significance at the 10%, 5%, and 1% levels, respectively. *Source: Center for Research in Security Prices, Muzan TAQ*

Dependent variable: R										
Days following trigger:	1	2	3	4	5	6	7	8	9	10
TRG	5.79***	2.33	−0.30	−1.41	0.82	2.61	−1.66	−2.99*	0.17	−1.28
	(1.81)	(1.61)	(1.88)	(1.84)	(1.70)	(1.59)	(1.74)	(1.60)	(1.47)	(1.76)
R^2 (%)	32.3	44.7	37.2	39.1	44.3	44.5	47.5	37.2	37.8	40.3
Observations	25,836	25,836	25,836	25,836	25,836	25,836	25,836	25,836	25,836	25,836

Table 6: **Impacts of short-selling restrictions on returns: Robustness.** This table presents estimation results for 65-minute returns (R), in bps, using equation (1), given the policy trigger of -10%. Column (1) estimates include no additional fixed effects, column (2) estimates include trigger time-of-day fixed effects, column (3) estimates include both trigger time-of-day and date fixed effects, and column (4) augments (3) with stock characteristics (market cap, market-to-book, Amihud illiquidity, volatility, and market beta). Each row reports robustness of these four specifications to different specification choices or sample restrictions. Row (1) includes stock fixed effects, row (2) weights observations based on the inverse distance to the threshold, row (3) adjusts for Fama-French 49 industry returns, row (4) uses a local quadratic polynomial, row (5) uses a local cubic polynomial, row (6) uses a local quartic polynomial, row (7) uses a local quintic polynomial, row (8) excludes dates in the top decile of daily market returns; row (9) excludes dates in the bottom decile of daily market returns, row (10) excludes stock dates when the stock's Fama-French 49 industry has top-decile industry returns, row (11) excludes stock dates when the stock's Fama-French 49 industry has bottom-decile industry returns, row (12) excludes Black Monday (August 8, 2011) from the sample, row (13) excludes stocks that have a trigger count in the top-decile, and row (14) excludes stock dates that follow overnight triggers (e.g., overnight return $< -10\%$); . The sample period is March 2011–March 2013. Robust standard errors are reported in brackets. Symbols *, **, and *** reflect the statistical significance at the 10%, 5%, and 1% levels, respectively. *Source: Center for Research in Security Prices, Muzan TAQ, Wharton Research Data Services*

Dependent variable: R Treatment variable: TRG	(1)	(2)	(3)	(4)
(1) Stock fixed effects	5.10*	4.79**	4.90***	4.95***
	(2.64)	(1.87)	(1.78)	(1.78)
(2) Weighted estimates	5.64*	5.51*	5.47***	5.57***
	(2.89)	(2.82)	(1.90)	(1.89)
(3) Industry-adjusted returns	5.17***	5.21***	5.17***	5.25***
	(1.86)	(1.84)	(1.63)	(1.62)
(4) Local quadratic polynomial	5.88***	6.22***	4.60***	4.67***
	(1.92)	(1.91)	(1.70)	(1.70)
(5) Local cubic polynomial	6.49***	7.83***	5.63***	5.61***
	(2.01)	(2.03)	(1.78)	(1.78)
(6) Local quartic polynomial	5.74***	7.12***	4.15**	4.55**
	(2.17)	(2.16)	(1.92)	(1.92)
(7) Local quintic polynomial	5.71**	6.37***	4.45**	4.96**
	(2.24)	(2.25)	(1.96)	(1.96)
(8) Up markets excluded	6.13**	6.01**	5.15***	5.19***
	(2.95)	(2.87)	(1.87)	(1.87)
(9) Down markets excluded	9.47***	8.72**	8.77***	9.02***
	(3.50)	(3.46)	(2.80)	(2.77)
(10) Up industries excluded	5.98**	5.86**	5.00***	5.04***
	(2.94)	(2.87)	(1.86)	(1.85)
(11) Down industries excluded	10.4***	9.95***	9.26***	9.50***
	(3.62)	(3.60)	(2.99)	(2.96)
(12) Black Monday excluded	4.82*	4.67*	4.94***	5.03***
	(2.63)	(2.64)	(1.88)	(1.88)
(13) Frequently-triggered stocks excluded	8.47*	8.27*	6.13**	6.25**
	(4.44)	(4.28)	(2.65)	(2.63)
(14) Overnight triggers excluded	6.63**	6.55**	5.66***	5.75***
	(2.82)	(2.76)	(1.82)	(1.81)

Table 7: **Stock characteristics around the −10% threshold.** This table presents estimation results for stock characteristics, using equation (1), given the policy trigger of −10%. All estimates include controls for time-of-day and date fixed effects, with linear polynomials. Each column corresponds to a different stock characteristic variable. Market capitalization is the natural log of the product of closing prices and shares outstanding at the end of the previous month. Similarly, market-to-book is constructed using end-of-month market capitalization in ratio to the corresponding book value of equity. Illiquidity is the natural log of Amihud's (2002) measure, calculated using daily data from the previous month. Volatility is the daily-return standard deviation over the previous month. Market betas are estimated in a market model using weekly data from the 24 months ending the month prior to the trigger. The sample period is March 2011–March 2013. Standard errors are clustered at the stock and date level. Symbols *, **, and *** reflect the statistical significance at the 10%, 5%, and 1% levels, respectively. *Source: Center for Research in Security Prices, Muzan TAQ, Wharton Research Data Services*

	Stock characteristic				
	ln(*Market cap.*)	Market-to-book	ln(*Illiquidity*)	Daily volatility (%)	Beta
	(1)	(2)	(3)	(4)	(5)
TRG	0.030	−0.223	−0.046	−0.137	0.009
	(0.045)	(0.434)	(0.063)	(0.096)	(0.019)
R^2 (%)	16.8	3.7	16.6	23.5	9.5
Observations	25,593	25,593	25,593	25,593	25,593

Table 8: **Impacts of short-selling restrictions on returns: Placebo tests.** This table presents estimation results for 65-minute returns (R), in bps, using equation (1) for a set of placebo thresholds. Column (1) estimates include no fixed effects, column (2) estimates include trigger time-of-day fixed effects, column (3) estimates include both trigger time-of-day and date fixed effects, and column (4), augments (3) with stock characteristics (market cap, market-to-book, Amihud illiquidity, volatility, and market beta). For comparison, we reproduce estimates presented in Table 4 for the -10% threshold. For the placebo thresholds, we re-sample with replacement to match the sample size of our tests at the -10% threshold. The standard errors are clustered at the stock and date level. Symbols *, **, and *** reflect the statistical significance at the 10%, 5%, and 1% levels, respectively. *Source: Center for Research in Security Prices, Muzan TAQ, Wharton Research Data Services*

Dependent variable: R	(1)	(2)	(3)	(4)
$TRG = -5\%$	2.00	1.80	2.13	2.11
	(2.06)	(2.03)	(1.68)	(1.68)
$TRG = -6\%$	1.17	0.89	1.28	1.30
	(2.20)	(2.19)	(1.58)	(1.57)
$TRG = -10\%$	**6.75****	**6.64****	**5.79*****	**5.87*****
	(2.82)	**(2.77)**	**(1.81)**	**(1.80)**
$TRG = -14\%$	1.96	1.89	0.88	1.08
	(2.33)	(2.32)	(2.10)	(2.11)
$TRG = -15\%$	−4.09	−4.34	−4.35*	−4.13*
	(2.73)	(2.71)	(2.37)	(2.37)

Table 9: **Impacts of short-selling restrictions on returns: Simulation.** This table presents estimation results for simulated 65-minute returns (R), in bps, using equation (1). Panel A presents baseline estimates in which no treatment effect is present in the simulated data. Panel B presents estimates in which a 5-bps treatment effect is present in the simulated data. Panel C presents estimates in which no treatment effect is present in the data and returns exhibit time series dependence given partitions in the return distribution. We present estimates for 2, 5, and 10 partitions in the distribution of returns. Column (1) estimates include no fixed effects, column (2) estimates include trigger time-of-day fixed effects, and column (3) estimates include both trigger time-of-day and date fixed effects. The sample is constructed using 2,000 simulated stocks over 500 trading days to approximate the size of the main sample. The standard errors are clustered at the stock and date level. Symbols *, **, and *** reflect the statistical significance at the 10%, 5%, and 1% levels, respectively. *Source: Authors' Analysis*

Panel A. No treatment effect

	(1)	(2)	(3)
TRG	0.247	0.094	0.571
	(1.168)	(1.159)	(1.174)

Panel B. Treatment effect of 5 bps

	(1)	(2)	(3)
TRG	5.235***	5.073***	5.556***
	(1.156)	(1.148)	(1.165)

Panel C. No treatment effect, return partitions

	(1)	(2)	(3)
TRG (2 partitions)	0.361	0.225	0.654
	(1.138)	(1.136)	(1.152)
TRG (5 partitions)	0.309	0.158	0.636
	(1.160)	(1.151)	(1.166)
TRG (10 partitions)	0.300	0.156	0.590
	(1.132)	(1.122)	(1.139)

Table 10: **Impacts of short-selling restrictions on trading strategies.** This table presents estimation results for natural logs of the 65-minute proportion of seller-initiated volume ($\ln(PSL)$; depth at best ask price ($\ln(ADP)$), depth at best bid price ($\ln(BDP)$); spot volatility, in basis points ($\ln(VOLAT)$); quoted bid-ask spread, in cents ($\ln(QSP)$); and effective relative spread, in basis points, ($\ln(EFSP)$); using equation (1), given the policy trigger of −10%. Column (1) estimates include no fixed effects, column (2) estimates include trigger time-of-day fixed effects, column (3) estimates include both trigger time-of-day and date fixed effects, and column (4) augments (3) with stock characteristics (market cap, market-to-book, Amihud illiquidity, volatility, and market beta). The sample period is March 2011–March 2013. Standard errors are clustered at the stock and date level. Symbols *, **, and *** reflect the statistical significance at the 10%, 5%, and 1% levels, respectively. *Source: Center for Research in Security Prices, Muzan TAQ, Wharton Research Data Services*

Dependent variable: $\ln(PSL)$

	(1)	(2)	(3)	(4)
TRG	−4.77***	−4.83***	−4.60***	−4.58***
	(0.50)	(0.49)	(0.45)	(0.45)
R^2	0.036	0.039	0.11	0.11
N	25836	25836	25834	25834

Dependent variable: $\ln(VOLAT)$

	(1)	(2)	(3)	(4)
TRG	−8.60***	−8.69***	−9.05***	−9.10***
	(3.18)	(3.09)	(2.36)	(2.31)
R^2	0.0015	0.014	0.20	0.23
N	25836	25836	25834	25834

Dependent variable: $\ln(ADP)$

	(1)	(2)	(3)	(4)
TRG	7.71**	8.57**	11.1***	11.9***
	(3.27)	(3.33)	(2.83)	(2.71)
R^2	0.0041	0.012	0.081	0.15
N	25838	25838	25836	25836

Dependent variable: $\ln(QSP)$

	(1)	(2)	(3)	(4)
TRG	−3.98	−4.56*	−6.12***	−6.26***
	(2.46)	(2.41)	(2.24)	(2.06)
R^2	0.0012	0.0079	0.073	0.16
N	25836	25836	25834	25834

Dependent variable: $\ln(BDP)$

	(1)	(2)	(3)	(4)
TRG	0.13	1.07	4.09	4.97*
	(3.30)	(3.36)	(2.91)	(2.80)
R^2	0.0019	0.010	0.084	0.15
N	25838	25838	25836	25836

Dependent variable: $\ln(EFSP)$

	(1)	(2)	(3)	(4)
TRG	−9.67***	−10.1***	−9.75***	−9.79***
	(3.01)	(2.98)	(2.98)	(2.78)
R^2	0.0025	0.0055	0.11	0.23
N	25838	25838	25836	25836

Table 11: **Impacts of short-selling restrictions on short-selling activity: FINRA sample.** This table presents estimation results for the natural log 65-minute short-selling activity (STO), using equation (1), given the policy trigger of -10%. Column (1) estimates include no fixed effects, column (2) estimates include trigger time-of-day fixed effects, column (3) estimates include both trigger time-of-day and date fixed effects, and column (4) augments (3) with stock characteristics (market cap, market-to-book, Amihud illiquidity, volatility, and market beta). The sample period is March 2011–March 2013. Standard errors are clustered at the stock and date level. Symbols *, **, and *** reflect the statistical significance at the 10%, 5%, and 1% levels, respectively, and they correspond to the more conservative significance levels among the displayed standard-error estimates. *Source: Center for Research in Security Prices, Financial Industry Regulatory Authority, Muzan TAQ, Wharton Research Data Services*

Dependent variable: ln(STO)	(1)	(2)	(3)	(4)
TRG	−7.03***	−7.08***	−7.74***	−7.85***
	(1.34)	(1.33)	(1.25)	(1.22)
Time-of-day FE	No	Yes	Yes	Yes
Date FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R^2	0.017	0.019	0.096	0.10
N	23103	23103	23101	23101

Figure 1: **Rule comparison timeline**

1. Following a transaction at $30.00, best bid and offer prices are $30.00 and $30.02, respectively.

NBB = $30.00

2. Someone places a new limit buy order at $30.01.

NBB = $30.01

3. Another trader is interested in selling the stock short,

. . . Can she sell short at $30.01?

Rule 10(a)-1
Yes

—because $30.01 is an uptick compared to the most recent transaction ($30.00)

Rule 201
No

—because short sales must be at prices that exceed the NBB.

Source: Authors' Analysis

Figure 2: **Number of stocks subject to Rule 201 short-selling restrictions.** This figure presents temporal variation in the daily number of stocks from the entire universe of NYSE-, AMEX-, and NASDAQ-listed common shares in the period from March 1, 2011, to March 31, 2013 that are subject to Rule 201 short-selling restrictions. The vertical axis features a break within which there are no observations. We show this break using a thick dashed line that represents the range between 350 and 850. *Source: Center for Research in Security Prices, Muzan TAQ*

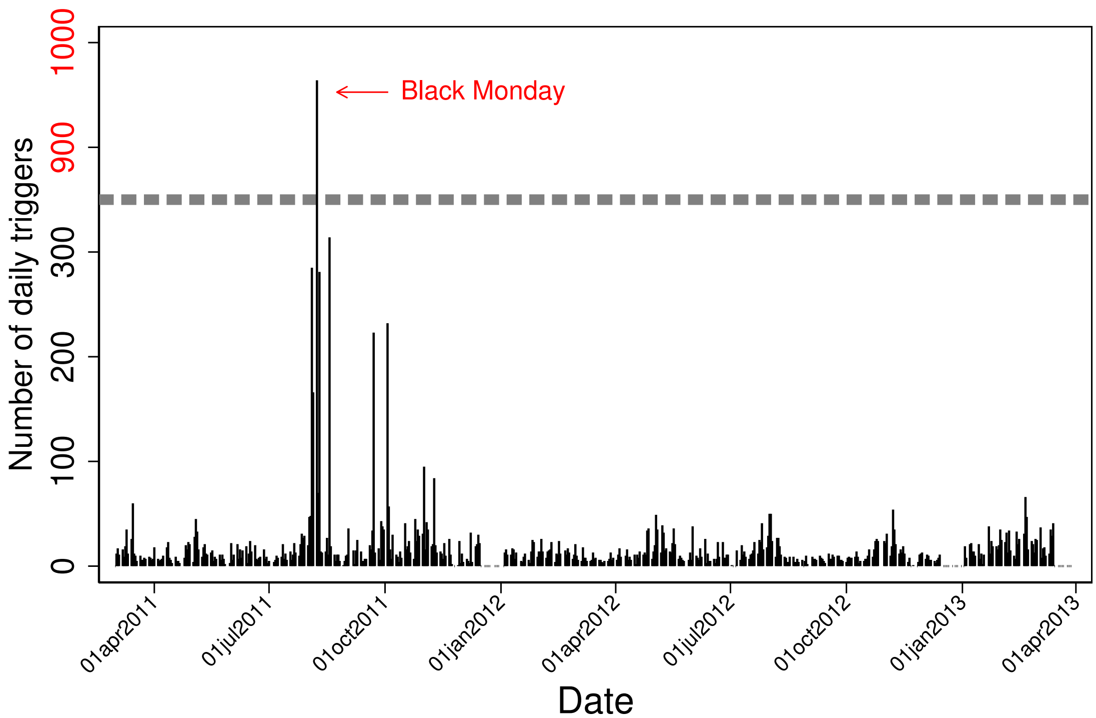
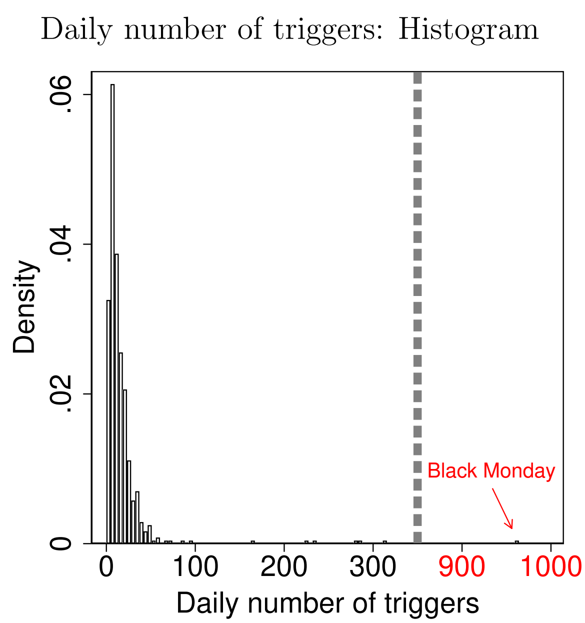


Figure 3: **Number of stocks subject to Rule 201 short-selling restrictions and overall market performance.** In this figure, the plot on the left displays the histogram of the daily number of stocks, from the entire universe of NYSE-, AMEX-, and NASDAQ-listed common shares in the period from March 1, 2011, to March 31, 2013 that are subject to Rule 201 short-selling restrictions. The horizontal axis features a break within which there are no observations. We show this break using a vertical dashed line that represents the range between 350 and 850. The plot on the right shows the association between the natural log of the number of stocks affected by Rule 201 short-selling restrictions and the corresponding overall (equally-weighted) market return. The vertical and horizontal dashed lines represent the first and the fourth quintile statistics of the relevant variable in the period from March 1, 2011, to March 31, 2013. *Source: Center for Research in Security Prices, Muzan TAQ*



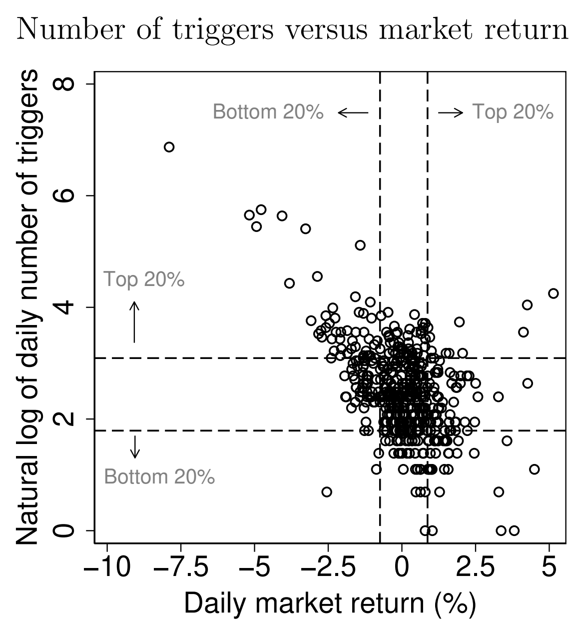


Figure 4: **The length of trigger episodes.** This figure displays the histogram of the episode lengths in full trading days beyond the initial trigger period when Rule 201's short-selling restrictions remain in effect. Restrictions may extend due to continued price drops that re-trigger Rule 201. The sample includes the entire universe of NYSE-, AMEX-, and NASDAQ-listed common shares in the period from March 1, 2011, to March 31, 2013 subject to Rule 201 short-selling restrictions. *Source: Center for Research in Security Prices, Muzan TAQ*

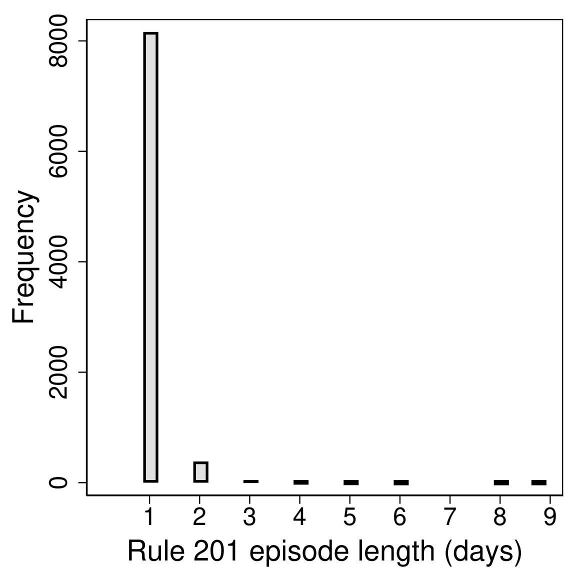


Figure 5: **Uptick duration around the -10% threshold.** This figure illustrates distributional properties of downtick time durations, labeled $DTDUR$, in seconds, between a downtick transaction and the first following uptick in the main sample. The three quartile statistics and the mean of uptick duration measures are calculated within 1% bins of the running variable in the 2% bandwidth of the -10% threshold, and they are presented in the left subfigure. The median of $DTDUR$ is 58 seconds while the mean is 222 seconds. The histogram of the natural logs of downtick durations is presented in the right subfigure. *Source: Center for Research in Security Prices, Muzan TAQ*

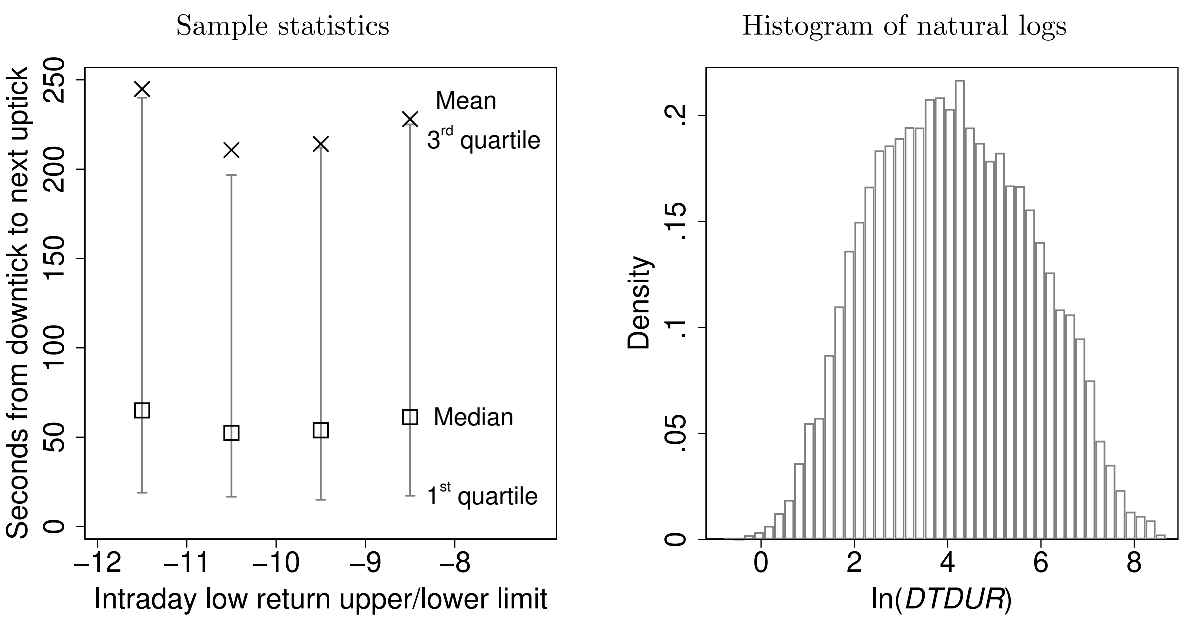


Figure 6: **Stock characteristics around the −10% threshold.** This figure illustrates similarities between the sample of 2,387 stocks surrounding the relevant threshold of Rule 201 triggers (−12%, −8%) and the population of 2,979 stocks for which there is data during our sample period, March 2011–March 2013. The measures of similarity are market capitalization, market-to-book ratio, Amihud (2002) illiquidity, volatility, and market beta, at the −10% threshold of intraday low returns. Market capitalization is the natural log of the product of closing prices and shares outstanding at the end of the previous month. Similarly, market-to-book is constructed using end-of-month market capitalization in ratio to the corresponding book value of equity. Illiquidity is the natural log of Amihud's (2002) measure, calculated using daily data from the previous month. Volatility is the daily-return standard deviation over the previous month. Market betas are estimated in a market model using weekly data from the 24 months ending the month prior to the trigger. Kernel densities of each stock characteristic are presented for the entire population and the relevant estimation sample. *Source: Center for Research in Security Prices, Muzan TAQ, Wharton Research Data Services*

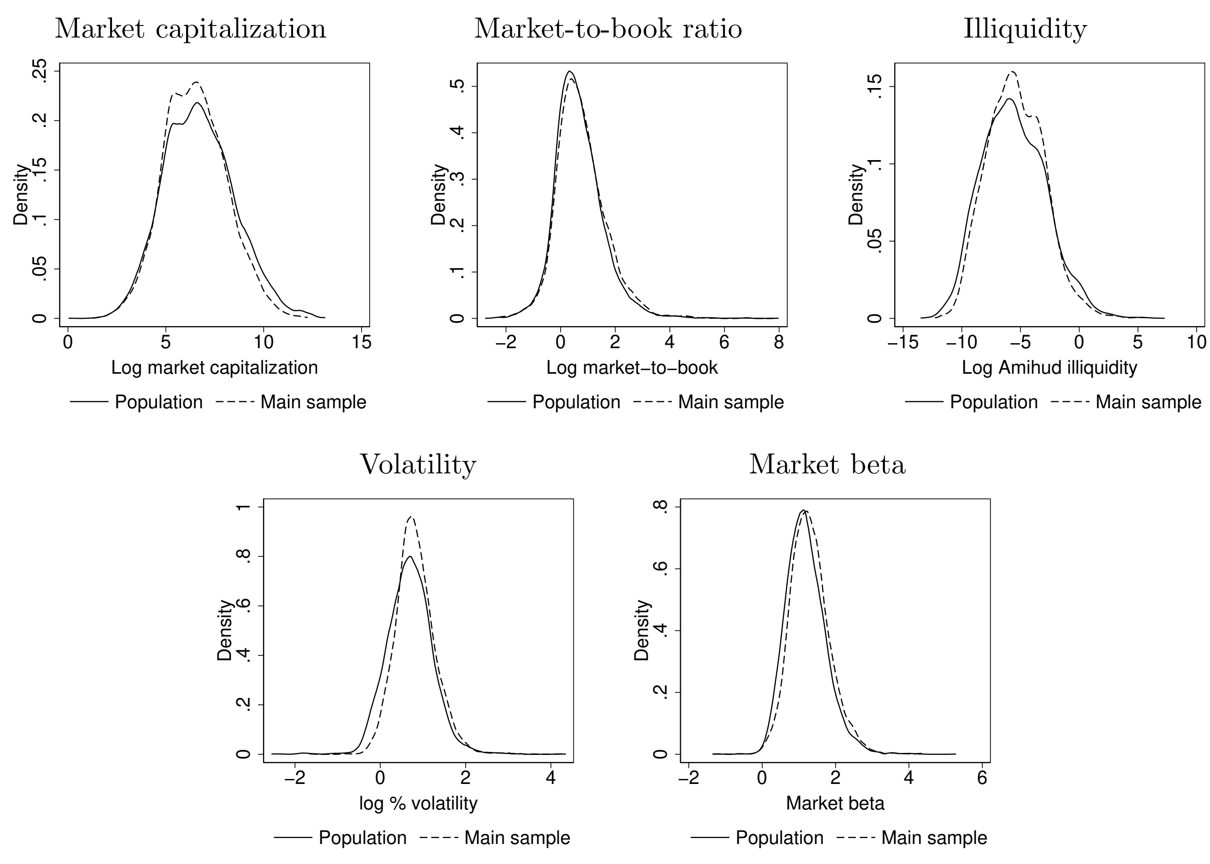


Figure 7: **Illustration of identification using Rule 201 short-selling restrictions.** This figure illustrates our panel data setup, showing the bins over which trading outcomes are contrasted for treated and control stocks when the circuit breaker is triggered at 12:00 noon on day t. In particular, the six 65 minute bins that cover the trading day are 9:30 a.m.–10:35 a.m., 10:35 a.m.–11:40 a.m., 11:40 a.m.–12:45 p.m., 12:45 p.m.–1:50 p.m., 1:50 p.m.–2:55 p.m., and 2:55 p.m.–4:00 p.m. As an example, we say stock A reaches an intraday low of -10% at 12:00 noon, which lies in the third bin, identified by "Trigger." This means that all bins for the remainder of the day (four through six) and the six bins on the next day are treated by Rule 201 for this stock. Stock B, in contrast, does not reach an intraday low of -10%, is not subject to the Rule 201 restriction, and, hence, serves as a control group firm. Intraday trading outcomes for the treatment group stock, identified using black boxes, are compared to the analogues for the control group stock, identified using blue boxes. The running vriable for both stocks is the intraday low return obtained in the "Trigger" bin (the last gray box on day t) with respect to the stock's most recent closing price. *Source: Authors' Analysis*

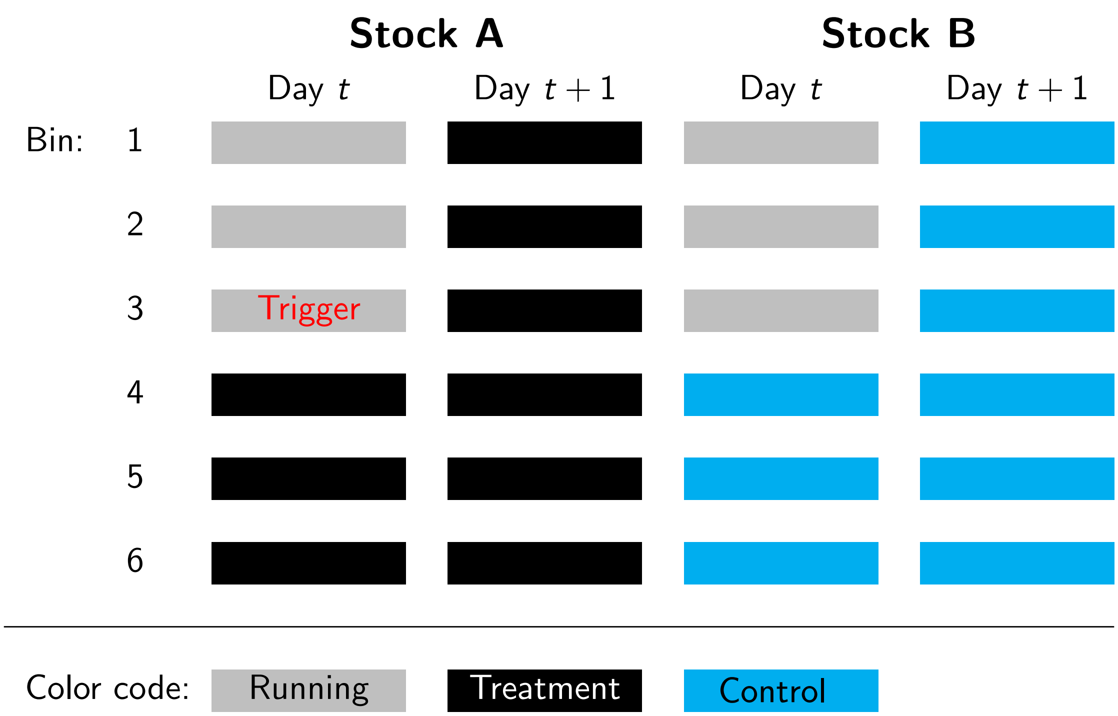


Figure 8: **Returns around the −10% threshold.** This figure illustrates discontinuities in returns, using fitted linear polynomials on each side of the −10% threshold (left) and using polynomials fitted with an Epanechikov kernel (right). More specifically, the left plot depicts linear estimates based on OLS fits and the corresponding 95% confidence intervals. The right plot depicts Epanechnikov (1969) kernels and corresponding 95% confidence intervals fitted separately on the two sides of the −10% threshold, with kernels estimated based on 25-basis-point internal bandwidths (8 bandwidths on each side of the −10% threshold) and local mean smoothing. *Source: Center for Research in Security Prices, Muzan TAQ*

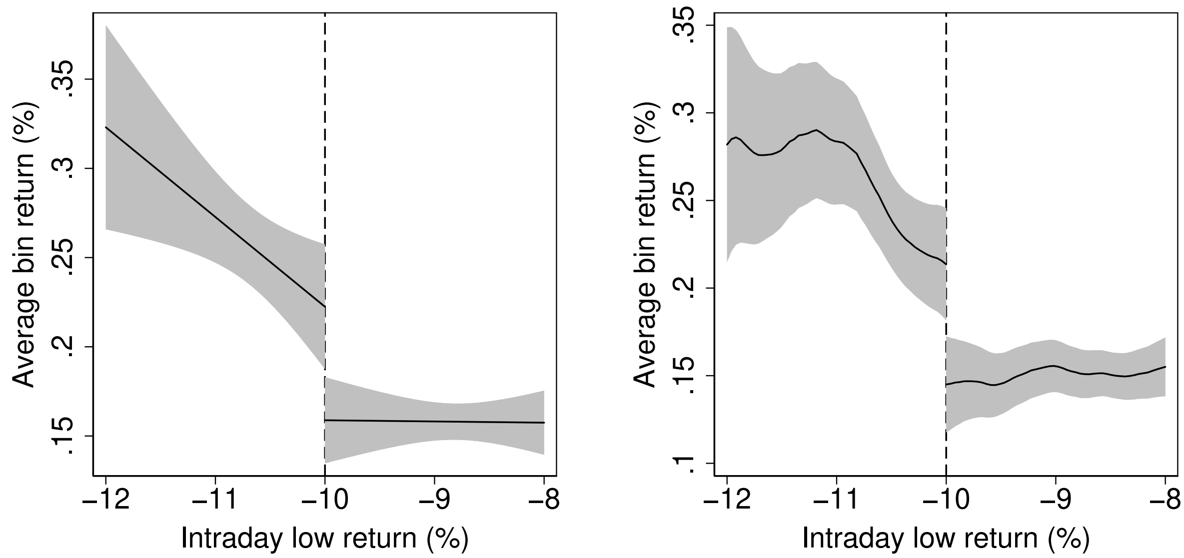


Figure 9: **Long-term effects of short-selling restrictions.** This figure illustrates the dynamic effects of short-selling restrictions on prices, short-selling activity, seller-initiated volume, depth at best ask price, and spot volatility—several trading days after restrictions are reset. Effects for treated stocks on days subsequent to the trigger are estimated using equation (1). Coefficients on natural log variables are rescaled by 100. Point estimates and 95% confidence intervals are plotted against the number days after trigger date $k \in \{1, \ldots, 10\}$. Estimates account for date and time-of-day fixed effects and standard errors are clustered at the stock and date level. The sample period is March 1, 2011–March 31, 2013. Estimates for short activity are based on the FINRA sample. *Source: Center for Research in Security Prices, Financial Industry Regulatory Authority, Muzan TAQ*

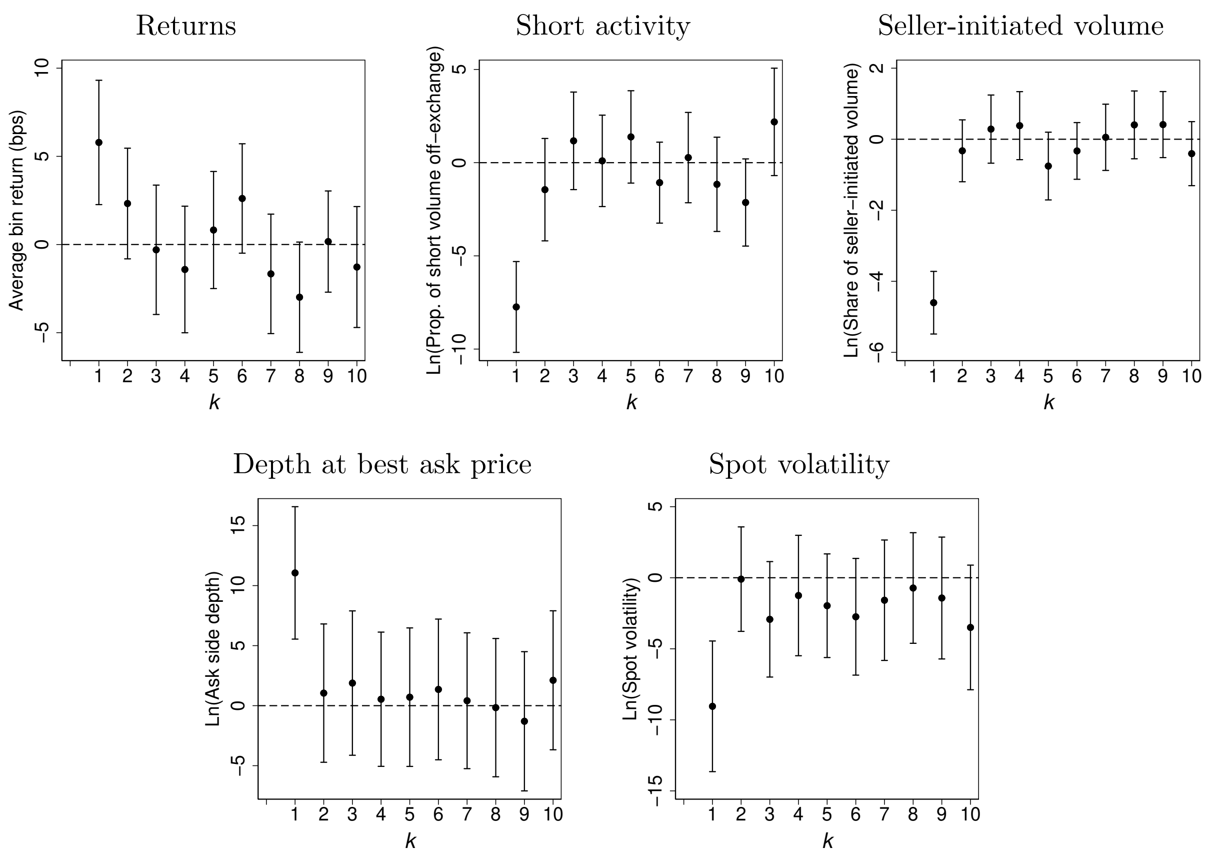


Figure 10: **Stock characteristics around the −10% threshold.** This figure illustrates the absence of any discontinuity in market capitalization, market-to-book ratio, Amihud (2002) illiquidity, volatility, or market beta at the −10% threshold of intraday low returns. Market capitalization is the natural log of the product of closing prices and shares outstanding at the end of the previous month. Similarly, market-to-book is constructed using end-of-month market capitalization in ratio to the corresponding book value of equity. Illiquidity is the natural log of Amihud's (2002) measure, calculated using daily data from the previous month. Volatility is the daily-return standard deviation over the previous month. Market betas are estimated in a market model using weekly data from the 24 months ending the month prior to the trigger. Linear estimates provide the OLS fits and the corresponding 95% confidence intervals for the predicted variable of interest within a 2% bandwidth on the two sides of the −10% threshold. *Source: Center for Research in Security Prices, Muzan TAQ, Wharton Research Data Services*

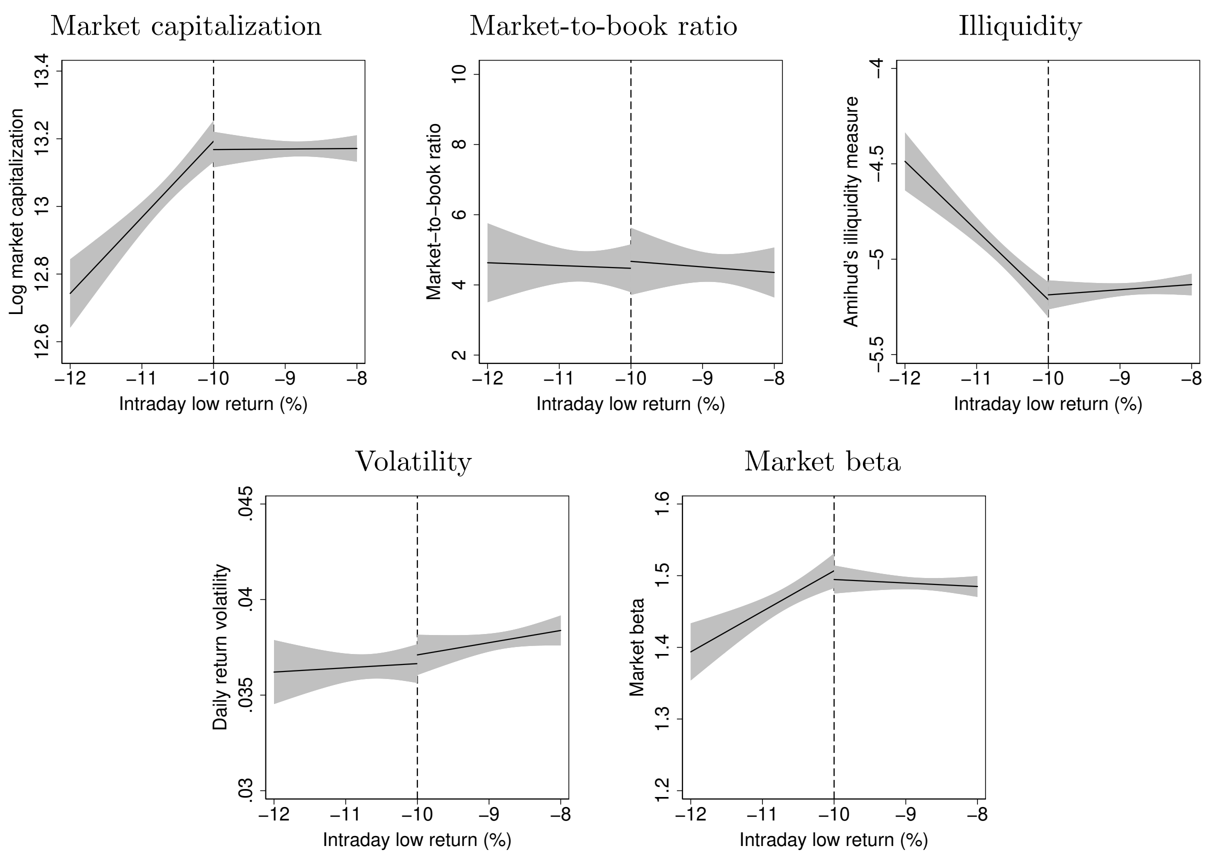


Figure 11: **Density break tests around the −10% threshold.** This figure presents density break estimates for the return at the beginning of our outcome measurement window. The plot on the left displays kernel densities of probability mass estimates over 1-basis-point bins that are estimated on the two sides of the −10% threshold. Predicted values and 95% confidence intervals of kernels are used to test the null of "no density break" at the hypothesized threshold following McCrary (2008). We do not reject the null. The plot on the right displays density estimates and 95% confidence intervals based on Cattaneo et al.'s (2019) approach. Densities are estimated using bandwidths (bins) that obtain from data-driven methods of Cattaneo et al.'s (2019), leading to 39.7- and 38.3-basis-point bandwidths on the left and right of the −10% cutoff, respectively. These wide bins underlie the larger estimates of frequencies in the plot on the right, compared to those in the plot on the left, where bins are as tight as 1 basis point. Quadratic local polynomials are fitted using unrestricted triangular kernels. We similarly fail to reject the null. The sample period is March 01, 2011–March 31, 2013. *Source: Center for Research in Security Prices, Muzan TAQ*

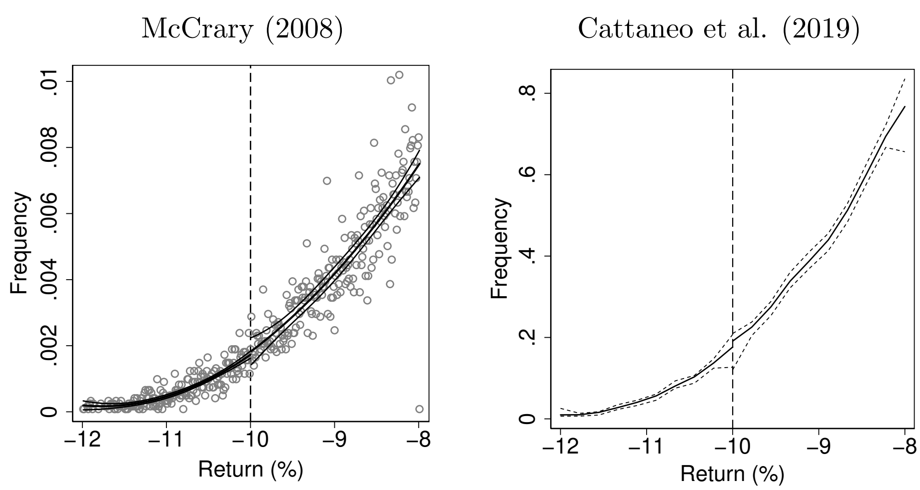


Figure 12: **Impacts of short-selling restrictions: Placebo cutoffs.** This figure presents point estimates and 95% confidence intervals from estimation results for 65-minute return (R), in bps; the natural logs of short activity ($\ln(STO)$); the proportion of seller-initiated volume ($\ln(PSL)$); depth at best ask price ($\ln(ADP)$); and spot volatility, in basis points ($\ln(VOLAT)$) using equation (1) for the true -10% policy threshold as well as a series of placebo thresholds (i.e., $-5\%, -6\%, -14\%, -15\%$). The estimates correspond to our preferred specification with time-of-day and date fixed effects. For comparison, we reproduce estimates presented in Table 4 and Table 10 for the -10% threshold. For the placebo thresholds, we re-sample with replacement to match the sample size of our tests at the -10% threshold. The standard errors are clustered at the stock and date level. Coefficients on natural-log variables are rescaled by 100. *Source: Center for Research in Security Prices, Financial Industry Regulatory Authority, Muzan TAQ*

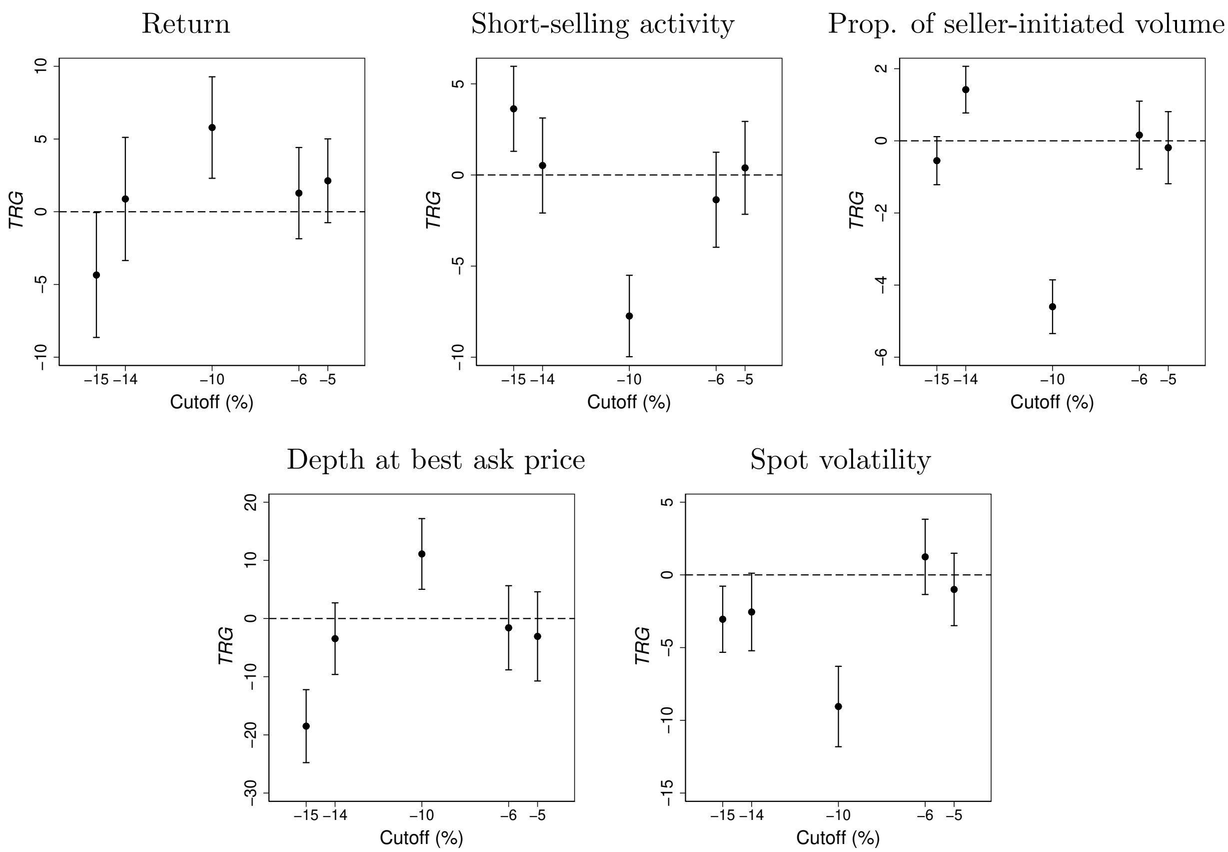


Figure 13: **Simulation evidence.** This figure presents estimation results for simulated 65-minute returns (R), in bps, using equation (1). Each panel of this figure presents a histogram of coefficient estimates from 1,000 samples of simulated data in which no treatment effect is present. Each sample consists of 2,000 simulated stocks over 500 trading days to approximate the size of the main sample. Panel (a) presents a histogram of coefficient estimates for a specification that includes no fixed effects. Panel (b) presents a histogram of coefficient estimates for a specification that includes time-of-day fixed effects. Panel (c) presents a histogram of coefficient estimates for a specification that includes time-of-day and date fixed effects.

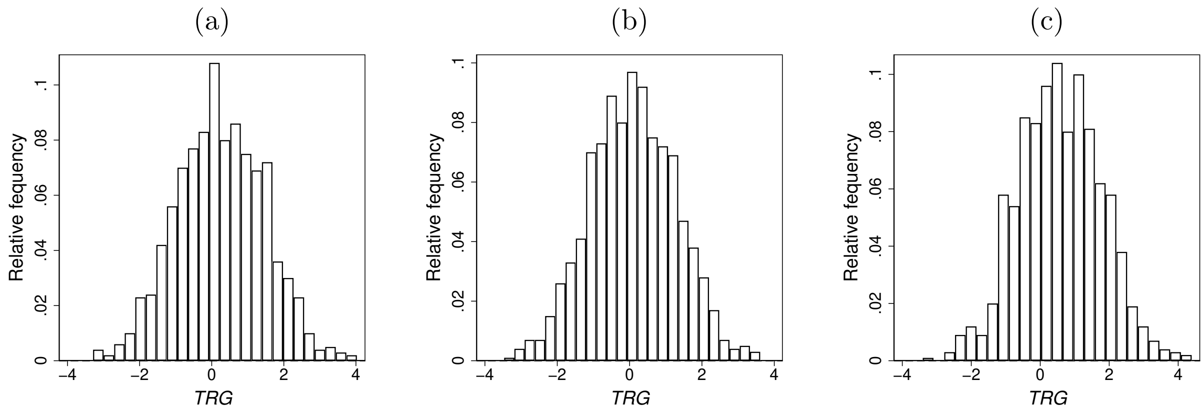


Figure 14: **Impacts of short-selling restrictions on trading strategies and pricing outcomes: Epanechnikov kernels.** This figure illustrates discontinuities in short-selling activity (top left), proportion of seller-initiated volume (top right), depth at best ask price (bottom left), and realized volatility (bottom right) at the −10% threshold. Epanechnikov (1969) kernels and corresponding 95% confidence intervals are fitted separately on the two sides of the −10% threshold. Kernels are estimated based on 25-basis-point internal bandwidths (8 bandwidths on each side of the −10% threshold) and local mean smoothing. Estimates for short activity are based on the FINRA sample. *Source: Center for Research in Security Prices, Financial Industry Regulatory Authority, Muzan TAQ*