Three Essays on Short Selling Bans, Asset Pricing and Market Quality

by

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Dedication

To Paula, Chema, July and Nata. You gave me the inspiration to start and the strength to finish. This is as mine as it is yours.

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Abstract

This Doctoral Thesis includes three essays on short selling bans and their effects on market microstructure, with the SEC Rule 201 as the cornerstone of the three chapters. In chapter one, we examine the current state of the art, identifying a series of methodological shortcomings and present proposals of solutions building on the specific context of the new regulation. In chapter two, we apply the recommendations from chapter one in a daily event study of more than seven years, focusing on price efficiency. Chapter three brings the analysis to a more detailed level, assessing the immediate effects of the regulation from the intradaily perspective. Overall, our assessment show that the Rule 201 short selling ban has significant consequences for asset pricing, price informativeness and the provision of liquidity for affected stocks.

Introduction

The experimental opportunities to study short selling constraints are scarce as of today. Most empirical studies have exploited temporary shocks on regulation such as the SEC Pilot Program on the Russell 3000 or the emergency actions undertaken as a response to the global financial crisis. Either source of external variation is however produced under very exceptional circumstances (at the peak of a rising bubble and its burst), which raises the concern of how representative those market situations are to analyze such a hot topic for researchers, practitioners and regulators. We start by analyzing the new short lived ban active since 2011 (Rule 201) and report the potential it represents in terms of designing experimental studies. More specifically, we assess the level of randomness of the circuit breaker and the set of conditionals on the likelihood of the treatment. Our results suggest that the new circuit breaker has a substantial potential as a quasi random experiment, allowing for larger heterogeneity in the sample and longer term event studies. However, it requires a detailed research design to isolate effects robustly and faces a set of limitations related to its mechanism of activation and lift of the constraints.

In the next chapter, we employ data from the Rule 201 circuit breaker to test the overpricing hypothesis (Miller, 1977) under this new short selling regulation, and whether the removal of short sellers is associated with a delayed price discovery (Boehmer and Wu, 2012). Based on the recommendations established in chapter one, we apply a differences-in-differences approach combined with a matched pairs analysis to perform an event study that covers more than seven years with a rich heterogeneity of data. We find significant evidence of Rule 201 being a cause of stock inefficient pricing and a slower price discovery on affected stocks. The results are robust to different specifications, sampling procedures and metrics. Our conclusions indicate that the new design of Rule 201 does not solve previous anomalies identified under other flavors of regulation and that the long lived discussion about the optimal degree of short selling regulation is far from done.

In the last chapter, we provide a detailed analysis of the intraday dynamics of assets that are covered by Rule 201 short selling ban to assess the immediate effects of the ban on market microstructure. Our main findings regarding short selling activity reveal a substantial shift of short selling orders towards off-exchange trading venues and short sellers aggressiveness being translated into aggressive limit orders, in line with Comerton-Forde et al. (2016). In terms of liquidity, we observe a degradation of visible market liquidity metrics (depth and quoted spreads). However, the effective cost of transactions (effective spreads) is reduced due to the increased competition for liquidity. Finally, we show that the immediate minutes before the actual trigger are substantially different, with an abnormal increase in trading activity. This last paper contributes to the literature by providing novel evidence on the nature of the Rule 201 microstructure dynamics and a quantification under this framework of many of the main dimensions under question in the scope of short-selling bans.

Chapter 1

Inference and Causality in the Context of Short Selling Bans: The Opportunity of Rule 201

1.1 Introduction

The question of the true effects and consequences of short selling restrictions are still debated today among researchers, regulators and practitioners. Under times of extraordinary market volatility or sustained price declines, most regulatory authorities across the globe have historically reacted by limiting short-selling, in the expectation that these measures helped markets to return to a status of operating normality and reduced investors' distrust. The regulatory authorities justify their position as an answer to a growing concern[1] among investors.

[1] "As market conditions continued to worsen, investor confidence eroded, and the Comission received many requests from the public to consider imposing restrictions with respect to short selling, based in part on the belief that such action would help restore investor confidence." SEC Release No. 34-61595.

Despite the economic importance of the regulation of such market practice,[2] there is still a substantial disagreement among researchers on the capacity of short selling bans in meeting their goals of market stabilization. Classic microstructure models regard short selling limitations as market frictions that yield less efficient markets, suggesting that they go against their initially stated objectives. Recent innovations in terms of policy changes have opened the field for establishing a myriad of empirical studies that have tested the classical hypotheses, but some methodological concerns about the research frameworks employed could question the utility of the inference up to date. Given the transcendental impact that short selling restrictions can potentially have on market quality (Diether et al., 2009a), asset pricing (Scheinkman et al., 2003), information channeling (Boehmer and Wu, 2012) or disciplining of management practices (Karpoff and Lou, 2010) it is essential that the analyses are reliable in terms of representativeness, extrapolation and unbiasedness.

The lack of a suitable empirical framework for analyzing short selling bans has been the major difficulty in studying the topic for researchers up to very recent years. For more than seven decades, the American financial markets were subject to the uptick rule: a market-wide short selling restriction that prevented any empirical comparison between constrained and unconstrained groups. This scarcity of empirical frameworks was reverted by the early 2000s, with the introduction of a pilot program in 2005, a repeal of the uptick rule in 2007 and the temporary reinstatement of short selling ban via an emergency order by 2008. In less than five years, there were more policy changes than in the preceding fifty. Since the enactment of the Russell 3000 pilot program, the research productivity in the field has skyrocketed, establishing the empirical foundations for most of what today we know about the topic. However, the particular characteristics under which the majority of the studies are conducted could be a source of concern with respect to the relevance

[2] Diether et al. (2008) find that in their sample, 24% of NYSE share volume and 31% of NASDAQ share volume is attributable to short selling operations.

and extrapolation of the conclusions. A recent paper by Boehmer et al. (2020b) analyzing the 2005 Russell pilot program supports our concerns and sets the scope on the necessity for finding a suitable, reliable and general enough framework upon which more definitive conclusions can be drawn.

In the paper, we document a set of historical shortcomings of prior research on the topic of short selling bans and analyze the usability of the new regulation in the US since 2011 (the Rule 201) as a solution for these caveats. Our results indicate a substantial potential of this new rule to offer heterogeneous long-term event studies that could isolate the ban effect accurately. Moreover, we document that Rule 201 short selling bans are not fully random, listing the set of factors significantly correlated with the propensity of receiving the treatment that would be useful for future research in the topic to guide the correct empirical design of the studies on the matter. Overall, Rule 201 poses a significant opportunity to solve the documented flaws, but it is also limited to a demanding empirical design to establish clean causality. We believe the discussion, results and analyses gathered in the paper will represent a substantial contribution and be helpful for future research building on this particular framework.

The rest of the chapter is organized as follows: Section 2 reviews the historical difficulties faced by researchers in the study of short selling constraints and highlights the most relevant contributions in the topic. Section 3 details the goals, scope and functioning of the Rule 201 short selling restriction and introduces the reasoning why it represents a potentially rich research opportunity. Section 4 performs a detailed analysis of all the Rule 201 events in the period 2011-2017, assessing the circuit breakers in several dimensions to understand the nature of the triggers. Section 5 builds on this knowledge about the Rule 201 events and discusses its utility as a research framework for analyzing the effects of short-selling constraints, concludes and offers some advice on future research.

1.2 The Difficulty of Analyzing Short-Selling Constraints.

1.2.1 A Brief History of Short-Selling Bans and Related Literature

Historically, researchers have found systematic difficulties when analyzing short selling restrictions mostly due to the lack of a suitable empirical framework. The reason for this scarcity traces back to the imposition of a short selling restriction in the US that was implemented back in 1938: the **uptick rule**. Designed and implemented by the SEC due to a growing demand at the time, the uptick rule[3] was a market wide mechanism that prevented short selling of an asset at any price that was lower[4] than the previous transaction price.

The particular situation of the American financial markets in the decade of 1930 fed a growing concern of market practitioners and regulatory authorities regarding the role of short sellers in the most remarkable events at the time. In the aftermath of 1929 stock market crash and following a period of international instability, a sharp declining market in the fall of 1937 acted as the trigger flag for the SEC to start regulating short selling activity. After a brief but crucial study[5] of the trading activity during the worse time of the market turmoil, the SEC determined that short sellers were particularly active during the declining market. This resolution added up to the shared perception of short sellers as the causal agents (up to a substantial degree, at least) of the prior market crashes and was determinant for the SEC to take a step forward and regulate short selling, being the regulation effective from

[3] Formally named Rule 10a-1.

[4] Originally, the rule prohibited also short selling at a price equal to the last transaction (known as a zero uptick). However, the regulation was amended less than a year later to include a provision that allowed this type of short sales.

[5] Surprisingly enough, the study upon which the SEC based its decision to limit short selling activity analyzed 10 days of market activity on 20 securities only.

February, 1938.

Given that the uptick rule was a market wide restriction, it was virtually impossible for researchers to establish a representative comparable group of stocks that traded in equivalent conditions to the constrained stocks with the exception of the possibility of selling short at any price. This population-wide treatment effect moved the discussion from the empirics to the scope of theoretical models, specially during the decades of 1970 and 1980. Some of the issues still discussed today arise from conclusions of models developed at the time, such as the role of short selling bans on overpricing (Miller, 1977), liquidity (Diamond and Verrecchia, 1987) or as the cause of larger investors' disagreement (Lintner, 1969).

Across the longevity of the uptick rule, the regulatory authorities questioned periodically the effectiveness of the restriction, although most of the revisions judged the restriction as being favorable for the general functioning of the markets, preventing sharp price declines and highlighting the negative influence of unrestricted short selling, this approach being particularly popular among practitioners and legislators.[6] As part of its periodic reviews of the regulation, the SEC designed a pilot program in 2004 that would provide an unseen situation: a random subset of stocks would be exempt from the uptick rule, allowing for a clean comparison of stocks subject to the short selling ban with respect to those operating under free market conditions. More precisely, roughly one third of the Russell 3000 Index components were selected as pilot stocks, allowing unrestricted short selling on the pilot group for more than two years.[7]

The SEC assessment of the pilot program concluded that the removal of the price

[6] See as an example the House Report No. 102-414 (1991) which defended the necessity of a limitation of short selling to "stabilize the market for exchange-listed stocks for the benefit of issuers and investors."

[7] The pilot starting date was originally January 3, 2005, but it was postponed until May 2, 2005; finishing on August 6, 2007.

restriction in the subset of pilot stocks had no substantial impact on the main metrics of market quality or liquidity; and more importantly, there was no evidence of the removal being related with "bear raids"; a crucial concern for the main defenders of the uptick rule. After a thorough evaluation of the pilot program data, there was a substantial agreement among academics and practitioners about the convenience of removing the price tests. The main argument highlighted the significant differences between the financial markets as of 2007 compared to the situation back in the 1930s, when the rule was implemented: "These commenters believe that price test restrictions are no longer necessary in today's markets, which are more transparent and where there is real-time regulatory surveillance that can easily monitor for and detect any short sale manipulation."[8]

In terms of research opportunities, this pilot program opened the field for the empirical testing of the classical results of the microstructure models regarding the restrictions on short sellers. In the myriad of papers using this framework, there is substantial heterogeneity both in the focus of the analysis and in the conclusions. As an example of this variety of results, Alexander and Peterson (2008) indicate that the removal of the restriction is beneficial for traders in terms of quicker order execution, while no reduction in market quality is observable. Diether et al. (2009a) report minor effects on intraday spreads, volatility and no significant changes in terms of daily metrics. Grullon et al. (2015) on the contrary, finds significant reduction of investment and equity issues in the group of pilot stocks, a reduction attributed to the higher exposition to short selling. However, Fang et al. (2016) document the positive role of short sellers in terms of prevention of accounting fraud, helping asset prices to be more informative about the true situation of the firm. Other analyses conducted under this framework include a comparison of auditor fees related to the disciplining mechanism of short sellers (Hope et al., 2017), return predictability

[8] SEC Release No. 34-55970 (July, 2007) about the removal of price test restrictions in the US financial markets.

(Diether et al., 2008) or voluntary disclosure of information (Clinch et al., 2019).

While it is undoubted that the pilot program was immensely helpful in contributing to our understanding of the impact of the uptick rule, we are concerned about the generalization of the results obtained under this framework mainly for two reasons. First, one should be cautious and limit the validity and interpretation of results drawn in a particular context, especially if the conditions of the study are substantially different from what we could expect of an 'average' situation. In other words, we question the representativeness of the study sample relative to the population. In the context of the pilot study, the choice of the Russell 3000 index is not arbitrary, and it is actually intended to capture a comprehensive set of stocks that accurately represents[9] the US equity market. This selection criteria, however, leaves out of the scope the stocks with a set of specific characteristics such as smaller market capitalization or volume. Although the economic significance of such assets is substantially overwhelmed by those included in the Russell 3000, this exclusion of a particular subset of the population should be relevant if a posterior evaluation of the treatment is to be conducted under a population of these characteristics excluded from the first study. As we will analyze more in detail in further sections, this is of special relevance in the context of the Rule 201 short selling ban, which due to its specific design, concentrates a substantial degree of incidence of the restriction in stocks with specific characteristics, that are potentially underrepresented in the pilot program experiment (highly volatile stocks and less traded ones).

The second dimension on which the representativeness of the pilot program is limited relates to the specific context of the financial markets at the time the experiment was designed and implemented. Recall that the pilot program ran for only a period of twenty-six months between May 2, 2005 and August 6, 2007. As we will

[9]According to FTSE, the Russell 3000 Index is able to capture around 98% of the total US equity market capitalization, with a periodic reconstruction strategy designed to keep the index accurate across time.

argue, this period is not representative of average market conditions in general.

In an ideal experiment, we would capture every possible context under which the population can be subject to the treatment, to ensure that we measure the treatment effect accurately and independently of any particular context. To visualize this concept, let us think of a substantially relevant medical controlled experiment intended to assess the effectiveness of a population-wide treatment such as COVID-19 vaccination. An experiment in which the cohort is composed exclusively of people with a residual social interaction due to the restrictions associated to the outbreak, the inference could overestimate the effectiveness of the vaccine due to the lack of it being tested against more challenging scenarios, such as when restrictions are completely lifted and the use of face masks is no longer mandatory. Using this parallelism, a study of the effect of short selling restrictions when markets are specially calmed, or undergoing a sustained rally on prices without negative scenarios could bias our assessment about the effects of such restrictions.

One could argue that in the vaccination example it may be of little interest to analyze other scenarios: in the context of the vaccination, we may be interested in testing the effectiveness under these set of conditions, which can be maintained artificially across time until full (or a substantial degree of) immunity is reached against the disease, thus leaving the infection as a residual problem (if not completely eradicated). This would yield the analysis of the specific vaccine in other contexts irrelevant, as these scenarios lay outside the sample space, Ω.

However, the particular setting of interest is dealing with financial markets, with two substantial differences from the aforementioned example. First, the context in which the treatment is analyzed cannot be arbitrarily defined nor maintained, as it does not depend on an external authority but arises from an equilibrium result that depends on an almost infinite number of exogenous, endogenous and participantspecific factors. Second, whilst vaccination may have the power to eradicate the particular infection solving in this way the question of interest (as by definition, one cannot be infected with an eradicated disease); is hard to argue that financial markets would likely never face any situation outside the ones produced during the pilot study.

By definition and according to the main established foundations of the literature, financial markets should be unpredictable (market efficient hypothesis). We capture this by defining the sample space Ω as one of infinite granularity, that is, with an unlimited population of possible scenarios ($x_i \in \Omega \ \forall \ i \in \{1, 2, ..., \infty\}$). This characterization of financial markets makes the dynamic updating and reevaluation of previously established knowledge absolutely essential to ensure the best possible understanding of the topic of interest at the time of the analysis. In fact, the Russell 3000 pilot program arises precisely from this will of the SEC to revisit prior evaluations on the convenience of maintaining the uptick rule, and there is no reason why this dynamic reevaluation should stop at the pilot program. It is not the point of our analysis to critique the numerous research contributions that were developed in this framework and inspired this paper, but to address the question of whether financial markets as of today are still represented by such sample and whether we can still rely on the same conclusions to be applicable.

To address whether this concern about representativeness has any support, we must clarify whether the financial markets at the time of the pilot reflect accurately what we could consider the 'average' financial market. Or even more importantly, whether they are representative of contemporaneous financial markets. To do so, we study and compare the evolution of four basic dimensions that define the market: return (S&P500 daily return), implied volatility (CBOE Volatility Index on the S&P500 options), total market turnover[10] and the proportion of stocks that

[10]We define market turnover as the share of the total market capitalization that is traded daily.

experiment an extreme result[11] for each given day.

Figure 1.1 highlights the differences in the distributions for the market return, volatility, turnover and incidence of extreme outcomes. The graphical evidence confirms our suspicions that the pilot program conditions differ substantially from the market conditions before and after it, especially in terms of volatility and volume. As further verification, we test for the equality of distributions with the Kolmogorov-Smirnov statistic, finding a significant difference[12] in distributions across groups (pilot vs. non-pilot) for all the factors analyzed except that of market return. The visual evidence and the Kolmogorov-Smirnov tests indicate a weak representativeness of the pilot program situation with respect to contemporaneous financial markets and moreover, highlight the fact that the time window of study of the pilot is remarkably less volatile and with a much smaller proportion of extreme outcomes than succeeding years. This is shocking and reveals a potential flaw of the evaluation due to the context in which it is realized, as it does not allow for the comparison of the effectiveness of the restrictions in the contexts that precisely justified the imposition of the bans: periods of sustained market decline or extraordinary volatility.

A second concern, is that the fact that the market is undergoing a pilot program could represent a unique setting by itself regardless of whether the context that defines the market situation is representative or not. A recent paper by Boehmer et al. (2020b) highlights that the pilot program represents an unique framework by itself finding significant differences in the trading behavior of market participants regarding the partial repeal of the uptick rule in 2005 for the sample of pilot stocks compared to the full population when the uptick rule is totally removed. A plausible explanation for this difference is found in the 'screening effect', this is, the fact

[11] Defined as an absolute close-to-close return of more than 10%

[12] As a robustness check, we calculate the Kolmogorv-Smirnov tests both including and excluding a period of one year surrounding the global financial crisis of 2008, without this making a difference in the statistical significance of the test, rejecting the null hypothesis of equality of distributions for the volatility, turnover and extreme results series.

that short sellers know that they are under the scope of the regulator and external observers during the pilot makes them act differently than they would under a context of a permanent change in short selling limitations.

The reasoning traces back to a core requirement in the determination of causal inference: the **stable-unit-of-treatment-value-assumption**. If the treatment effect is contaminated by the treatment of other units, the causal inference may be severely biased and should not be extrapolated to a context in which the treatment is assigned differently or where this contamination is not present. In the paper, Boehmer et al. (2020b) find evidence in favor of the pilot stocks receiving a particular degree of short selling aggressiveness that is not comparable to that received by the full population of stocks once the uptick rule is repealed, suggesting that the market reaction is conditional on whether the removal is due to a pilot or to a permanent change, opening the question of whether the inference conducted under pilot programs is truly comparable to other situations.

Shortly after the pilot program and the removal of the uptick rule, the financial crisis of 2008 generated pressure on the SEC to act and undertake emergency actions. For the purpose of this paper, it is of major relevance the prohibition of short sales on financial stocks, which were especially vulnerable at the time. The justification was to stop abusive shorting behavior, blamed for the numerous stock crashes observed during the period in which the limitations on short selling were lifted. This new emergency ban again allowed the study of short-sale constraints under a different setting of extraordinary volatility and a declining market, fostering the appearance of additional studies that used this emergency ban as an experimental framework. Boulton and Braga-Alves (2010) find evidence of stock overpricing at the announcement of the restriction, with a significant price decline at the expiration of the emergency ban. In the same context, Boehmer et al. (2013) find no significant effect on asset prices, but a substantial market quality degradation.

Kolasinski et al. (2013) argue that the short selling restrictions in the aftermath of the financial crisis acted as a filter, increasing the number of informed trades in the short seller population. This result would imply that shorting restrictions are beneficial for market participants as noisy trading from short orders is reduced, yielding more efficient prices.

Nevertheless, the concerns aforementioned regarding the extrapolation of the inference in the context of the pilot program are still present when considering the results obtained from the 2008 emergency ban due to the (even more) particular context of the financial markets under which the conclusions are drawn.

Overall, the research on the role of short selling constraints has experienced an exponential increase in the output from academics due to the new opportunities for testing the classical hypotheses. However, given the contradicting results and the caveats associated with the prior research, there is still a major field to be exploited before definitive conclusions on the topic can be established. In this paper, we aim to contribute to the literature by analyzing the potential of the new uptick rule or Rule 201 as a research framework and its usefulness in solving the previously documented shortcomings.

1.3 The New Framework: Rule 201

1.3.1 Definition, Objectives and Differences with Previous Regulation

The alternative uptick rule is a short selling restriction measure adopted by the Securities and Exchange Commission on February 26, 2010. Hereinafter, we will refer to it as the **Rule 201**, but to be precise, this new price test is the result of a series of amendments to Rule 201 Regulation SHO. Originally, this directive from the SEC removed all the previous price tests after the conclusions on the pilot study that deemed the short-selling ban as ineffective and costly for exchanges.

After the global financial crisis and the severe market instability that characterized 2008 and 2009 the SEC decided to undertake action to recover investors confidence in the markets. With this goal, in 2010 the SEC announced a modification in Rule 201 Regulation SHO that introduced a new short-selling ban. Similar to previous regulation, this new rule prohibits short selling any security at or below the national best bid. Contrary to the previous uptick rule, there exists a major difference in the scope of the restriction. Whereas the previous rule was applied permanently in a market-wide basis, Rule 201 is designed as a circuit breaker whose triggering condition is set as a -10% intraday price decline from the last trading day's closing price.

Once the trigger condition is met, short sale orders at or below the best bid are immediately prohibited for the asset for the remainder of the current trading day and the whole of the next one. The rule does allow for the possibility of an activation of the circuit breaker on consecutive days. If this happens, the ban extends for an additional trading day after the last trigger. Trading centers are required to comply with the new regulation since February 28, 2011.[13] In the design of the Rule 201 short-selling ban, the SEC establishes three main goals that the new regulation should prioritize. First, as the SEC recognizes the documented benefits of unconstrained short-selling in terms of market efficiency, liquidity and price formation, the new rule should allow a relatively unregulated shorting activity under normal market contexts. To comply with this target, the circuit breaker requirement of a 10% intraday decline is specifically chosen to make sure that the

[13] Division of Trading and Markets: Responses to Frequently Asked Questions Concerning Rule 201 of Regulation SHO. Accessed: Sep 28, 2017.

ban is effective only for stocks undergoing a substantially volatile day, creating a selective mechanism that applies the rule only to a small fraction[14] of the total population.

The second objective is to prioritize the access of long sellers vs. short sellers to the market liquidity by making only the former able to sell at the bid once the restriction is active. According to the SEC assessment and consistent with market microstructure, an speculative short seller would benefit from exhausting the liquidity levels by shorting at the bid. By allowing only long sellers to access the bid quotes (while short sellers must sell above the bid) the regulation seeks to limit unjustified excessively downward pressure on asset prices, provided that if long sellers continue to sell driving the price further down, this will be related to economically justified factors rather than to unexplained noise or shorting strategic trading. In the own words of the SEC "[...] by making such bids accessible only to long sellers [...] Rule 201 will help to facilitate and maintain stability in the markets and help ensure that they function efficiently."[15] In closing, the second goal can be summarized as fostering price efficiency preventing downward deviations from the economic valuation of the asset and at the same time allowing long sellers to liquidate their positions preferentially in times of difficulty, facilitating market participation by offering this 'escape route' when things do not go as planned.

Lastly, the time period during which Rule 201 constraints short-selling (the remainder of the trigger day plus the day after) is selected so that the Rule 201 can

[14] "[...] a 10% circuit breaker threshold, on average, should affect a limited percentage of covered securities. Given the variations in the facts and assumptions underlying the estimates submitted by commenters, the Staff also looked at trading data to confirm the reasonableness of those estimates. the Staff found that, during the period covering April 9, 2001 to September 30, 3009 the price test restrictions of Rule 201 would have been triggered, on an average day, for approximately 4% of covered securities. The Staff also found that for a low volatility period, covering January 1, 2004 to December 31, 2006, the 10% trigger level of Rule 201 would have, on an average day, been triggered for approximately 1.3% of covered securities". SEC Release No. 34-61595 Amendments to Regulation SHO.

[15] SEC Release No. 34-61595 Amendments to Regulation SHO.

serve as a 'reflexion opportunity' for investors to revisit their positions when the stock is undergoing a specially volatile day, with the goal of preventing 'bear raids' or any other price manipulation that feeds from panic among asset holders.

1.3.2 The Circuit Breaker Mechanism

The short-selling ban regulated in Rule 201 is a selective treatment whose application is dependent only on the own stock's behavior during a given day. Short selling is unrestricted for any stock until a trade is executed such that the asset price decreases by or more than by 10% compared to the closing price of the listing exchange of the asset for the immediately preceding trading day. Once the circuit breaker has been triggered, short selling for that specific asset is forbidden at or below the best bid for the remainder of the day plus the trading day immediately after. The Rule 201 allows for repetitions of triggering days, this is, a stock that repeatedly meets the -10% intraday decline in a series of consecutive dates. In those cases, the Rule reapplies the ban, with the practical result being in the original ban to be extended for an extra day until the requirement is not met on a given day, in which case the restriction is lifted the second day after. To summarize and visualize the concept, Figure 1.2 describes graphically the circuit breaker mechanism for triggering the ban and the consequences on the surrounding days in the market.

To illustrate the functioning of Rule 201 with a numerical example, let us define an asset (A) whose closing price at day t is \$50. According to framework, if on $t + 1$ a trade is executed at 45\$ or a lower price, Rule 201 is triggered and short selling is prohibited at any price equal or lower than the best bid. To understand the implications, let us assume that such trade is executed on 10:05:00 AM. Assume that the limit-order-book of A by 10:04:59 AM is as follows:

Ask		A - Last Trade: 45.1	Bid	
P	Q	Time: 10:04:59 AM	P	Q
45.5	500		44.5	760
45.6	900		44.2	850
45.9	1500		44.0	990
46.1	3000		43.8	1060
46.4	5000		43.5	4050

At 10:05:00, a market order to sell 350 units of A arrives, being executed against the first bid, at a price of \$44.5. This trade triggers the Rule 201 circuit breaker as $(\frac{44.5-50}{50} = -11\%)$. Assume that there exist no new orders in the transition from 10:05:59 to 10:05:00, so that the LOB now looks as follows:

Ask		A - Last Trade: 44.5	Bid	
P	Q	Time: 10:05:00 AM	P	Q
45.5	500		44.5	410
45.6	900		44.2	850
45.9	1500		44.0	990
46.1	3000		43.8	1060
46.4	5000		43.5	4050

As the prohibition of short selling orders at any price at or below the best bid is activated immediately, the only short selling orders that would be displayed and allowed in the market are those posted at a price of at least \$44.5 + τ, being τ the minimum tick size of the asset A. Therefore, for the price decline to continue once the Rule 201 is triggered, it must be the case that long sellers execute market orders against the different levels of the bid. According to the regulators, this is designed such that if the true economic valuation of the asset is i.e \$42, the market converges to such true valuation due to the action of long sellers liquidating their positions rather than by the force of short sellers that may have speculative reasons behind. In the same fashion, if the \$44.5 trade was part of a sequential strategy from an speculative short seller that would profit if price reached the third level of the bid (\$44.0) it is impossible for him/her to complete the strategy unless long sellers agree with the speculator valuation (even if it is biased) and exhaust all bids until they reach \$44.0. Otherwise, if long sellers valuation is e.g. \$44.7, once the speculative pressure is removed from the market, the price will converge back to its valuation; discouraging in the first stage the short seller from driving the price below the Rule 201 threshold with an speculative move, as he/she would not be able to complete his strategy.

This 'official' reasoning has an implicit assumption underneath that is of high relevance for the Rule 201 to work as expected: that on equilibrium, long sellers are able to convey the necessary information through their trades to ensure that price converges to its true valuation as effectively as short sellers. This is, if they know that the value of the asset is below the threshold, they will continue to sell until the true value is obtained. This assumption however, is not trivial to prove and there is substantial evidence on the superior ability of short sellers to anticipate future underperformance due to the more efficient use of public and private information (Boehmer et al., 2020a), anticipation of news (Christophe et al., 2004) as well as having strong economic incentives to become skilled information processers (Engelberg et al., 2012). According to this view of short sellers as traders with the capacity to anticipate negative returns better than the average trader, a restriction such as the Rule 201 could hinder price discovery, induce larger volatility and diminish investors' confidence, what would go against its foundational goals.

Let us reuse the previous example to illustrate this issue. Assume that long sellers and short sellers have different skills at news processing; to simplify it in this context, we assume that short sellers can process news during the same day of the release, whilst long sellers take two days after the release to process the new information. Let us add to previous example the true economic value of the asset by 10:05:00 is \$43.5 and will perpetually be as there will be no future generation of information. This value is instantly known for short sellers, while long sellers will be aware only after two days. According to the situation described in the example, and assuming only liquidity trading and no change in investors beliefs until the appearance of short sellers, we should expect the price to remain stable around the commonly (biased) valuation of the asset at $44.7. Once short selling is allowed (on the beginning of day $t+2$) short sellers would rationally sell the overvalued asset, as will also long sellers for which the asset true value is revealed. This trading would increase the downward pressure on prices efficiently, causing the asset to fall by an extra 2.7% approximately. It is then straightforward that if short sellers can process information better, their removal would cause a deferral of true asset value revelation.

More importantly, the removal could lead to scenarios where the long sellers and buyers asset valuation differs even more largely than in the described situation, causing an artificial price increase that would later be reverted by a larger crash once short sellers are back in the market, causing more volatility than if the price adjusted immediately by not constraining short sellers. This uncertainty about potential and sudden price adjustments would make the financial markets less appealing to investors due to the increase in the perceived risk, which would diminish investors' confidence and liquidity as well. This reasoning is heavily inspired in the **overpricing hypothesis** (Miller, 1977), where a substantial inefficient overpricing is produced in a situation where the investors' divergence of opinions is large and a strong valuation mismatch exists between the bearish traders and the bullish traders. When a given portion of traders is removed, this causes an imbalance towards the contrary opinion, thus driving asset prices away from the fundamental values. Although the described example is an extreme example, one could also contemplate a situation of progressive revelation. In such case, assume that among long sellers, the proportion of traders informed about the true value is a growing function of time, so that by day $t+2$ all long sellers are informed, without this less extreme scenario changing the idea that restricting short sellers could delay price discovery and lead to increased volatility.

This example, albeit somewhat extreme, allows us to see the complexity of the new universe for market microstructure under the context of the Rule 201 ban and how it could potentially affect many of the trading strategies and outcomes differently from how other regulations did, therefore reinforcing our argument of the need of a revaluation for the previously established results.

In summary, this new regulatory policy mechanism, also known as the **alternative uptick rule** represents an innovation with respect to previous short sale restrictions. In contrast with previous bans, the trigger condition is endogenously determined. Whether the prohibition is imposed or not depends solely on the behavior of the stock's price in the market. Previous research was based upon regulations that arbitrarily forbade (2008 emergency ban) or allowed (Russell 3000 pilot program) short-selling for a list of stocks. Furthermore, Rule 201 acts as a temporary correction mechanism, that is automatically reverted shortly after its application, which contrasts with previous bans which were in force for much longer time periods. This circuit breaker approach allows for a surgical restriction of short selling rather than a market wide approach for which costs would outweigh the benefits. Besides this selective application of the restrictions, as there is no limit in the number of stocks that can trigger the Rule 201 on a given day, for extremely volatile contexts, Rule 201 would be covering a substantial part of the assets, functioning as a (partially, at least) market-wide restriction.

1.3.3 The Research Opportunity

The vast majority of the empirical research on the assessment of the effect of short selling constraints have exploited the numerous policy changes in recent years to design experimental studies in the context of the Russell 3000 pilot or the 2008 emergency ban on financial firms in the US financial markets. This avenue of research has been largely exploited as well in other markets, mostly adapting the experimental designs to the specific frameworks designed by the competent regulatory authorities in most of the developed economies: such as the United Kingdom (McGavin, 2010), Australia (Helmes et al., 2017) or Spain (Morales-Zumaquero and Sosvilla-Rivero, 2015) among many others. See Beber and Pagano (2013) for a detailed study of many different regulations undertaken in the wake of the financial crisis as an attempt to correct the extraordinarily underperformance and volatility of exchanges around the globe.

While regulatory shifts may result appealing for the treatment analysis from an econometric point of view as an exogenous treatment assignation, we already discussed several considerations regarding the representativeness of these policy evaluations and the material differences between the situation of the financial markets then and now. Contrary to previous short selling restrictions imposition or lifts, Rule 201 has had as of today a much longer trajectory, as the restriction was effective since February 28, 2011. Moreover, it operates as a surgical treatment that allows for the coexistence of evaluation units affected by the treatment with those that are not. Recall that previous inference was conducted under contexts of total restriction vs. total lifts, such as in the case of the roughly 1,000 stocks for which the uptick rule was repealed during the Russell 3000 pilot program.

We believe that the possibility of studying Rule 201 bans vs. unconstrained assets that cohabit during a large window of observation will allow us to solve the problems related with the lack of representativeness as with the new framework, researchers will be able to study the same treatment under a much more heterogeneous context with different scenarios of market stability, steep price declines, higher volatility, lower trading volume and so on. A main advantage arises from this heterogeneity in the context of the study. The larger the sample size the closer we get to the true population, reinforcing the power of the inference. This way, provided the research design identifies causality between the treatment and the variable of interest in a large heterogeneous sample, the result is more generalizable, and less conditional to the specific conditions under which the study is conducted.

However, there exists a major consideration to be accounted for before proceeding with the inference in the Rule 201 framework. As the ban is applied only to stocks that have undergone a substantial intraday price decline, the restriction on short selling cannot be considered as an exogenously applied treatment. Nevertheless, given that the treatment is applied based on a given threshold or cut-off in an observable variable (stock returns) this setting represents a good candidate for a regression discontinuity design (to which we will refer as RDD, from now onwards). A growing body of the finance literature has effectively implemented the RDD methodology in assessing causality in threshold events, such as Flammer (2015) in the context of corporate social responsibility and firm performance or Bakke et al. (2012) analyzing the effects of delisting in stocks to mention some.[16] We now review the basic identification conditions to establish causality in the framework of short-selling bans, analyzing whether the Rule 201 exhibits potential for the correct estimation of the treatment effects via RDD. Our review follows closely the methodology description in Hahn et al. (2001) and Angrist and Pischke (2008).

Let us define y_i as the variable of interest upon which we seek to assess the

[16]See also Acharya and Xu (2017), Kahraman and Tookes (2017), Li et al. (2018) or Gonzalez-Uribe and Leatherbee (2018) as other examples for the regression discontinuity design in the recent finance literature.

causality from a determined treatment. D_i is a binary variable that captures treatment: $D_i = \{0, 1\}$. In our specific setting, $D_i = 1$ reflects that the given stock i is subject to the Rule 201 short selling ban. Every individual i has two potential outcomes for the variable of interest: y_{1i} when the unit of analysis i has received treatment, and y_{0i} when it has not. The ideal setting would be to observe the difference $y_{1i} - y_{0i}$, this is, the treatment effect of the short-selling ban on variable y for stock i. However, for each unit of analysis i we observe a single outcome: either y_{1i} or y_{0i}. As we cannot observe what would have happened for stock i had it not been subject to the ban (the counterfactual), y_{0i}, an estimation of this factor is required. The necessity for this estimation drives the inference away from the identification of individual effects towards the analysis of the average effect of the treatment on the population or a group of the population. From the analysis of the variable of interest Y we can quantify the observable difference in the measurable outcome variable on average: $E[y_i|D_i = 1] - E[y_i|D_i = 0]$ which is decomposed in two terms as follows:

$$\begin{aligned} E[y_i|D_i = 1] - E[y_i|D_i = 0] = E[y_{1i}|D_i = 1] - E[y_{0i}|D_i = 1] \\ +E[y_{0i}|D_i = 1] - E[y_{0i}|D_i = 0] \end{aligned} \tag{1.1}$$

The term $E[y_{1i}|D_i = 1] - E[y_{0i}|D_i = 1]$ captures the treatment effect we want to analyze, while $E[y_{0i}|D_i = 1] - E[y_{0i}|D_i = 0]$ is known as the **selection bias**, or in our framework, the sum of effects associated with specific characteristics of stocks not subject to the ban that would affect the outcome variable. Taking into account that the short-selling ban of Rule 201 is applied only to stocks that have undergone difficulties during a given day (intraday fall of 10% or more) it seems likely that the population of non treated stocks shares several characteristics that makes them different from the treatment group on average and that such differences affect the outcome variable. As an example, it seems reasonable that if stock A triggers Rule 201 on day t while stock B does not because it has a positive return on such day the stocks behave differently in many of the outcomes of interest such as volatility or post-treatment stock returns. Let us consider stock A, for which a news release drives the economic value of the asset to a point 20% lower than before. We should expect a Rule 201 trigger that market participants will anticipate this loss of value and readapt their positions. However, if we analyze stock A the day after the announcement (when the Rule 201 ban is still active for A), a naive regression that compares A with another stock which did not receive the treatment could lead us to the wrong conclusion, as we should expect that such a loss of value has implications on the immediate future of the stock in terms of augmented volatility or the possibility of price reversals in subsequent days. Therefore, it would not be clear whether we are capturing the treatment effect of the ban, or the causal effects associated with a loss value of the given magnitude. Considering that Rule 201 is activated after a major price decline, this concern should be addressed carefully in any framework.

Pure randomization in the treatment, as in the case of the pilot program, eliminates the selection bias, as one would not expect substantial differences between two samples drawn randomly from the same population. However, the Rule 201 treatment is associated with an endogenous selection mechanism: stock returns. While the efficient market hypothesis removes the scenarios in which stock returns can be accurately predicted, it does not necessarily remove the fact that some stocks are more likely to experience more drastic price changes than others according to some characteristics of the asset. For example, we should expect a higher likelihood of Rule 201 triggers for highly volatile stocks, or those with a larger market beta, as they amplify market movements. A thorough analysis of the factors associated with the likelihood of the treatment is done in the upcoming section, where we empirically show how the Rule 201 events show a higher degree of incidence in stocks with specific characteristics.

A core assumption for the causal interpretation of the results is that we are able to measure and control for the set of covariates that explain the likelihood of the given asset to be subject to the treatment (X_i). We are referring, of course, to the **conditional independence assumption (CIA)**, this is, that conditional on the set of covariates X_i the potential outcome in the variable of interest is independent of the treatment. Mathematically:

$$\{y_{0i}, y_{1i}\} \perp\!\!\!\perp D_i | X_i \tag{1.2}$$

Therefore, conditional on the set of covariates X_i, the comparisons of the average observed outcome variable between the treated and control groups equal the treatment effect:

$$E[y_i|X_i, D_i = 1] - E[y_i|X_i, D_i = 0] = E[y_{1i} - y_{0i}] \tag{1.3}$$

The Rule 201 ban is a rule-based treatment status that depends on the stock's intraday behavior. More specifically, the determinant for the treatment is that a given trade is executed at any price that is -10% or lower when compared to the previous closing price. We are therefore interested in the Maximum Intraday Decline for a stock i on day t, which we define as $\mathrm{MID_{i,t}}$:

$$\mathrm{MID_{i,t}} = \frac{\min(P_{i,t}) - P^{CL}_{i,t-1}}{P^{CL}_{i,t-1}} \tag{1.4}$$

Where $\min(P_t)$ stands for the minimum trading price of a stock across the trading day t, while $P^{CL}_{i,t-1}$ stands for the previous day closing price. As the Rule 201 treatment probability jumps from 0 to 1 when $MID_{i,t} \leq$ -10%, we focus on the sharp regression discontinuity design, where the treatment is assigned on the basis of an observable continuous parameter reaching a given threshold. This is the selection variable s_i. In this framework $\mathrm{MID_{i,t}}$ takes this role. Note that s_i is not indexed by time factors t while $\mathrm{MID_{i,t}}$ is. For simplicity, let us stick to the atemporal notation, and we will address this issue later.

To establish causality, it is essential that the parameter that decides the treatment assignation is observable at each side of the threshold. This is satisfied, as all traded stocks have a public record of prices and quotations. Moreover, it is required that the distribution density is smooth at the discontinuity point, this is, that the units of analysis cannot manipulate their record in the selection variable. In this context, it seems unlikely that firms can manipulate their quotation, given that stock prices are given by the law of demand and supply in the financial markets. Observations above the threshold are assigned to the treatment group whereas those which do not reach it remain as the non-treated sample.

To understand how RDD can help in capturing causality let us define the average treatment effect (ATE) in a naive regression form:

$$y_i = \beta_0 + ATE \cdot D_i + u_i \tag{1.5}$$

An OLS regression of (1.5) yields biased results as the error term u_i contains the set of effects that explain differences between the treated and control groups and is therefore correlated with the outcome variable y_i. To solve this bias, RDD assumes that by taking only observations close to the threshold $\bar{s}$ that defines treatment we are imitating random sampling, thus removing the correlation between the error as there are no substantial differences between groups. Formally, if we define $Pr(D_{i,t} = 1|s_i)$ as the probability of being assigned to the treatment conditional on the observable s_i we have:

$$\begin{aligned} &\lim_{s \downarrow \bar{s}} Pr(D_i = 1|s_i) = 1 \\ &\lim_{s \uparrow \bar{s}} Pr(D_i = 1|s_i) = 0 \end{aligned} \tag{1.6}$$

From here, we can derive that an observation of the difference between those stocks that marginally meet the threshold vs. those who do not reach it marginally is our estimator of the treatment effect (ATE):

$$ATE = \lim_{s \downarrow \bar{s}} E[y_i|s_i] - \lim_{s \uparrow \bar{s}} E[y_i|s_i] \tag{1.7}$$

This expression holds as long as two basic conditions are satisfied: that the treatment likelihood shows a clear jump in the threshold $\bar{s}$ and that the error term is continuous in the selection variable s_i. Mathematically, we begin with the naive model of (1.7) and subtract the left limit (just above the threshold) from the right limit (just below the threshold), obtaining:

$$\begin{aligned} \lim_{s \downarrow \bar{s}} E[y_i|s_i] - \lim_{s \uparrow \bar{s}} E[y_i|s_i] = ATE \cdot \left(\lim_{s \uparrow \bar{s}} E[D_i|s_i] - \lim_{s \downarrow \bar{s}} E[D_i|s_i] \right) \\ + \left(\lim_{s \uparrow \bar{s}} E[u_i|s_i] - \lim_{s \downarrow \bar{s}} E[u_i|s_i] \right) \end{aligned} \tag{1.8}$$

For simplicity, we rewrite (1.8) in terms of the average treatment effect and two factors A and B:

$$\begin{aligned} \lim_{s \downarrow \bar{s}} E[y_i|s_i] - \lim_{s \uparrow \bar{s}} E[y_i|s_i] &= ATE \cdot A + B \\ A &\equiv \lim_{s \uparrow \bar{s}} E[D_i|s_i] - \lim_{s \downarrow \bar{s}} E[D_i|s_i] \\ B &\equiv \lim_{s \uparrow \bar{s}} E[u_i|s_i] - \lim_{s \downarrow \bar{s}} E[u_i|s_i] \end{aligned} \tag{1.9}$$

From (1.8) we have two terms of special interest. For the expression in (1.7) to hold we require that A = 1 and B = 0. In the context of Rule 201 A will always equal 1, as all stocks whose MID (our s_i) falls below the -10% cut-off ($\bar{s}$) will be subject to the treatment, while those with a MID of -10% + ϵ will not for any $\epsilon > 0$, no matter how infinitesimal ϵ is.

Assessing whether the that the error term u_i is (on expectation) continuous on s_i (that would imply B = 0) is not trivial in this setting. The continuity of the error term in the discontinuity implies that no factor in the error term is in any way correlated with the selection variable. In our context, this would imply that the stocks that show a MID around the cut-off $\bar{s}$ show similar characteristics, at least conditional on the fact that their MID is within a reasonable distance Δ from the discontinuity. This would be translated into the belief that, for instance, all stocks that reach a MID of -11% do so for the same reasons. Economically, there exist several challenges to this statement. As an example, let us think of two stocks (ABC and XYZ) with remarkably different degrees of returns volatility ($\sigma_{ABC} > \sigma_{XYZ}$). We assume that stock returns are normally distributed with zero mean and the corresponding volatility. Consider the extreme example in which $\sigma_{ABC} = 15\%$ and $\sigma_{XYZ} = 5\%$. Simply because of the return distribution we would expect that the stock ABC triggers Rule 201 bans once every four trading days, while XYZ would do so once every 43 days:

$$\begin{aligned} \Pr(r \leq -10\% | R \sim \mathcal{N}(0, \sigma_{ABC})) = \Phi(-10\%) \approx 25\% \\ \Pr(r \leq -10\% | R \sim \mathcal{N}(0, \sigma_{XYZ})) = \Phi(-10\%) \approx 2.28\% \end{aligned} \tag{1.10}$$

Where $\Phi(x)$ stands for the cumulative distribution function of the normal distribution. This simple scenario allows us to understand that beyond the MID, one may need to account also for other factors. While ABC can trigger the Rule 201 during any trading day, when XYZ triggers it (or is close to, and is selected in the control group), there could be additional factors that explain why XYZ reaches a MID of -10% + Δ with an strictly positive Δ. For example, the release of new firm information, or because XYZ belongs to the financial sector and a new taxation is announced for this type of firms, causing a rational adjustment of stock prices in the sector (including XYZ). At this point, we refer to the conditional independence assumption in (1.2) and rewrite factor B in (1.9) as:

$$B \equiv \lim_{s \uparrow \bar{s}} E[u_i | X_i, s_i] - \lim_{s \downarrow \bar{s}} E[u_i | X_i, s_i] \tag{1.11}$$

If the set of covariates X_i that defines the particular context of the MID close to the discontinuity threshold is observable, then the continuity of u_i on s_i conditional on X_i holds, thus setting B equal to 0. Then, conditional on X_i, the expression in (1.8) equals (1.7), although we have to redefine the estimated effect as:

$$ATE = \lim_{s \downarrow \bar{s}} E[y_i|X_i, s_i] - \lim_{s \uparrow \bar{s}} E[y_i|X_i, s_i] \tag{1.12}$$

At this point, a deep understanding of the reasons behind the activation of the Rule 201 becomes essential before conducting any causal inference to make sure that we comply with the necessary assumptions for identifying causality in this framework. Moreover, one could argue that there exists a subset of X_i that is unobservable. On top of that, it seems reasonable that not only factors associated with firm i affect the reasons why the stock reaches (or comes close to) the trigger requirement. For example, we should observe a higher incidence of Rule 201 triggers across the whole population under days of substantial generalized volatility, thus highlighting the necessity for also controlling for time factors.

While regression discontinuity in this context offers researchers the opportunity of identifying similar units of analysis, it is not sufficient in the context of Rule 201 to isolate causality. In this framework, the similarity in terms of the selection variable is not enough to ensure similarity between units of analysis.

Nevertheless, the fact that Rule 201 is a short lived restriction and that it has been effective since 2011 opens the possibility of analyzing similar units of analysis by incorporating stock as well as time fixed-effects into the inference. The differences-in-differences framework seems then a suitable technique to apply in this context. Given that unobservable fundamental factors will be captured by differences in the stock fixed-effects and date-specific factors will be captured by time fixed-effects, the causality is dependent only on the correct selection of the set of covariates X_i

that are related with the identification of similar units of analysis. In other words, the goal is now to find the correct X_i that define the reasons why a given stock triggers the Rule 201 marginally, find a comparable unit of analysis that lies at the other side of the discontinuity and analyze the differences between them.

The identification of the right covariates that define a good enough similarity between treated stocks and candidates for the control group in the context of Rule 201 remains as of today an unexplored field. In the upcoming section we address this issue by empirically analyzing the characteristics of a substantially large sample of Rule 201 triggers with the goal of contributing in the correct identification of this similarity factors, helping future researchers using this framework in the correct identification of causality.

1.4 The Rule 201 Framework: An Evaluation

1.4.1 Sample

Our study of Rule 201 covers all circuit breaker triggers in a period slightly smaller than seven years approximately: since February 28, 2011 until December 29, 2017. The amount of events in the sample is of 111,582 Rule 201 circuit breakers, which account for a mean of 65 events per trading day[17] in the time span of the study. The record of the events (including the exact time of the trigger) are collected from the Philadelphia Stock Exchange web.[18] Then, we match our records on the Rule 201 events with the combined CRSP-Compustat database to obtain data on firm fundamentals and market activity. Finally, Fama-French factors are obtained from Ken French's website.

[17] There are 1,721 trading days in our sample.

[18] The records include data from stocks in all major exchanges in the US such as NASDAQ, AMEX or NYSE, not only from the PHLX.

1.4.2 Distribution of the Selection Variable

We begin our analysis of Rule 201 by assessing the density of the selection variable in our framework: MID, as defined in (1.3). A necessary assumption for the identification under the regression discontinuity framework is that the selection variable s is defined smoothly around the discontinuity cut-off and that no clustering of observations is located at either side of the threshold. This ensures that no manipulation is present in the data as analysis units do not manipulate their s to lay at one side or another. Depending on the specific s used in the framework, we cannot rule out that this assumption may not be satisfied. In our case, given that the selection variable depends on the market equilibrium of interactions between buyers and sellers we should not expect that firms are capable to knowingly manipulate their stock price dynamics, less even when looked at the intraday scope, for which returns should be unpredictable given market efficiency. Still, following McCrary (2008) we plot in Figure 1.3 the density for MID around the -10% threshold.

What can be clearly seen in this figure is the smoothness of the density around the discontinuity threshold, with an absence of kinks or signs of clustering at either side. Moreover, taking into account the design of the MID variable, which is an specific measure for stock returns, we see a similar pattern to the one we should expect in any return distribution: a high concentration of the density towards the mean value (which we would expect to be slightly shifted towards the left from 0) that rapidly decreases as we move away from the mean.

1.4.3 Market Context and Time Series Analysis

We now continue by analyzing the distribution of the circuit breaker triggers across the time span of our study window. In this part, we analyze how does the likelihood of Rule 201 triggers relate to specific events in the financial markets, to assess whether controlling for time fixed-effects is needed in this framework. Our analysis starts by looking at the relationship between market volatility and the number of Rule 201 events.

From an economic point of view, we should expect that given a fixed threshold for the trigger, more and more stocks hit the -10% flag when markets are undergoing a significantly abnormally volatile day. Moreover, Rule 201 is designed to work this way during these specific contexts, to prevent sustained price declines and reduce market instability. Hence, if the rule works (partially at least) as it was designed to work we should take into account what is the relationship between the incidence of Rule 201 and the specific volatility of a given day. Figure 1.4 plots the daily distribution of Rule 201 triggers along with the evolution of the Chicago Board of Exchange Volatility Index (VIX) to visualize the possible correlation between the series.

Figure 1.4 reveals that the series of daily events remains quite stable across time, with some notorious peaks related to specifically troublesome market conditions. The maximum number of events registered during a single day is of 2,233, corresponding to August 24$^{\text{th}}$, 2015 market crash. In general, correlation between the VIX series and the Rule 201 events is around 31% for the period studied. Visually, we can contextualize this correlation as being specially notorious during turmoil market times, as the greater peaks of both distributions coincide. However, some smaller peaks on the VIX series are not translated to the Rule 201 event series. This could indicate that there exists a "threshold" of volatility that the market can suffocate before it is translated into a higher occurrence of Rule 201 triggers. This explanation is perfectly reasonable taking into account that the Rule 201 mechanism is only activated once the 10% negative return level is reached, so only during specially volatile periods we should observe the correlation being more strong, as market movements are more sudden and extreme, enough to reach the triggering condition for many stocks in the market. Other days of high volatility, but not high enough, stocks could be close to the trigger threshold but without reaching it.

Another source of concern regarding time-fixed effects would arise in the presence of seasonality effects, as economically, markets could be subject to a repeated pattern if, for example, large information releases are always done during the same dates of the year. The literature provides some empirical evidence of return patterns around earnings announcement dates (Kaniel et al., 2012) or publication of quarterly results (Chen et al., 2011) among others, that could be reflected as a statistically significant drift on the circuit breaker series. To clarify this issue, we present Table 1.1. In this table, we split the events month-by-month to understand which months are statistically different and why.

From Table 1.1, we can see that, as advanced by our graphical analysis, there is a certain stability in the series around the conditional mean values by year and specific month. The data reveal certain recent market events, such as the aforementioned 'Black Monday' on August, 2011; the 2015's Flash Crash (Aug. 2015) and also the global market selloff between the end of 2015 and the start of 2016. Nevertheless, there is no evidence of any significant seasonality pattern that seems to be affecting the series.

As a last step in our time-series analysis, we now turn our attention to the trading hours distribution of circuit breakers. In Figure 1.5 we can observe the distribution of the circuit breaker series in the different windows of the trading day.

The graph below is quite informative with respect of understanding when do things happen. Except for the first half hour of trading, the distribution of triggers is quite uniform, though there is a small smile pattern with peaks at the beginning and the end of the trading day. The shape of this distribution indicates that, although most of the triggers happen across the day, there exists evidence of clustering around the first minutes of trading, with 11,631 records happening exactly at market opening.

When analyzing the implications of the short-selling ban, one must always be conscious of the actual trigger time to understand the timing of events. As described in Switzer and Yue (2019), given the mechanism for the circuit breaker, a stock with a trigger could be subject to only one day of ban (if the trigger is in the last minutes of the day, short-selling the stock would only be banned for the remainder of that day and the next one) or to almost two (if the trigger is at the beginning of the day, the ban would last for almost the entirety of that day and the whole next one).

The U-shape that we observe in Figure 1.5 has been a matter of discussion among researchers when dealing with market intraday patterns. Garvey and Wu (2014) find evidence of specific investor interest at the final minutes, explained by the fact that informed traders are more active (Gao et al., 2018) because they prefer to disseminate the private information at the end of the day.

The release of private information from informed traders at the end of the day could explain why more than 20% of the triggers happen at market opening. A potential explanation would be that the bearish pressure around the stock at the end of the trading day is not enough to halt the circuit breaker and then the negative tendency continues into after-hours trading.

When the bearish price pressure is high enough, the overnight markets could be the reason for the triggers that happen just as market opens, because the stock suffered a severe devaluation enough to trigger the circuit breaker once markets are active again. The Rule 201 halt will be triggered whenever the opening price is, at least, 10% smaller than the last closing price.

At this point, we are not discussing the reasons behind this bearish market by the end of day or after market hours. It is not the goal of this paper to study this particular phenomenon,[19] but definitely it is an issue to be accounted for. The reasoning traces back to the idea of finding comparable stocks for which the trigger assignment can be considered random conditional on what we know about them (the selection on observables) and accounting for the unobservables through firm fixed-effects. However, it is not trivial whether these controls exhaust the set of covariates X_i that could influence the potential outcome. We believe that the treated stocks should only be compared with assets that not only experience a price dynamic of a reasonably similar magnitude (around the threshold), but that both the candidate and the counterfactual stock are experiencing such dynamic at the same date and (ideally) at the same time. This way, the observable similarity between stock pairs (they belong to the same industry, similar market capitalization or similar return volatility) supports the claim that both stocks are experiencing similar reactions to a unobservable or non-measurable factors.

1.4.4 Stock Specific Factors

At this point of the analysis, we have already discussed the lack of seasonality or any other significant time series pattern on the daily circuit breaker series and how important is to control for the date and time of the trading day the trigger is halted. The next natural step is to ask ourselves what are the differential characteristics of a given stock that make it more prone to trigger the circuit breaker. An accurate definition of the factors that influence the likelihood of the treatment will be crucial for defining the covariates X_i that need to be accounted for in order to comply with

[19] As an explanation, Boudoukh et al. (2019) find evidence that private information dissemination through trading is one of the main drivers of stock prices and volatility, especially during overnight trading. Without getting into any detail, it seems likely that the information component of each asset should play a fundamental role both for the design and interpretation of any empirical research.

the conditional independence assumption.

Before proceeding, we will move from the general description of the Rule 201 events to focusing only on those lying around the -10% discontinuity; which will be the ones around which inference can be causally interpreted when compared to counterfactuals at the other side of the threshold. A concern that may arise at this point is whether these events are numerous enough to draw representative (or interesting enough) inference. To address this concern, we plot the distribution of the price decline that triggers Rule 201, what is equivalent to the distribution of MID conditional on receiving the treatment ($MID_{i,t}|D_{i,t}$):

The analysis of Figure 1.6 shows a marked difference in proportions for Rule 201 events that trigger the threshold marginally when compared to those that reach substantial intraday declines. In terms of proportions, an 8.64% of the observed events lie in a very close region to the threshold (MID $\geq$ -10.5%). This proportion more than doubles when we consider a distance of 1% from the discontinuity ($\approx$ 20%) and reaches 42% when we expand the distance up to -2.5% away from the discontinuity (MID $\geq$ -12.5%). These numbers show that while restricting identification to events close to the threshold inspired by RDD strategies imposes a limitation on the total sample that we can analyze, the substantial concentration of events around the discontinuity highlights the potential contribution to be made by studying these observations, as they contain a substantial subset of the population of events of interest.

Moreover, given that Rule 201 is specifically designed to act only under times of substantial difficulty for a given asset it makes sense that we focus in these scenarios for which the regulation is applied. It is true that this consideration would question the generalization of the conclusions obtained with this framework to the effects of short selling bans in the general population. However, given that the position of the regulator has been flexibilized substantially, and bans are now being only applied in this subpopulation of stocks with difficulties (contrary to market-wide restrictions, for instance) it makes sense that research would concentrate in analyzing this particular phenomenon for the population of interest notwithstanding the possibility of generalizing the results to other groups for which extra identification assumptions should be considered. We leave that extent for future research.

Once we have established the relevance of studying the events close to the threshold, we move now on to the identification of the stock specific characteristics which will define the conditional independence between the events and the potential candidates to be used as counterfactuals. So far, we have argued that the closeness to the threshold is a necessary but not sufficient condition to establish inference in this framework. The focus on these events and candidates around the threshold inspired by RDD should be the first step, as they identify the population upon which inference can establish causality. However, events and candidates may reach this region of interest in the selection variable due to different reasons. We have analyzed the role of time fixed-effect and also unobservables that could be correlated with the potential outcomes, highlighting the necessity of applying panel data techniques. On top of that, we argue that this type of analysis should match events and controls based on the similarity of a set of factors that influence the treatment assignation and the specific reasons for the stock to be included in the analysis sample, recommending that the first requisite for the matching is the comparison of stocks that have the close-to-the-threshold event during the same date (and ideally time) to ensure that the information component behind the trigger (or the almost-trigger) is shared among comparable pairs and not affecting the potential outcome. Our study of the likelihood of the trigger conditional on different stock-specific factors should help future researchers in finding the dimensions that must be controlled and (reasonably) equal among pairs to establish causality with this strategy.

1.4.4.1 The Conditional Likelihood

The issue of assessing the risk or propensity of triggering the Rule 201 according to the stock characteristics is not a trivial one. Empirically, analyzing the likelihood of a Rule 201 trigger is the same as analyzing the likelihood of any given stock to suffer a severe price fall (10% or larger) during the trading hours. Historically, the prediction of any extremely negative results has been of interest for researchers and practitioners for obvious reasons. In fact, stock return predictability (of any kind and magnitude) is among the most widely studied topics in the academia. Research on this topic has clearly split into two fields: one testing the predictability of returns in the long-term and one more focused on short-term price shocks and whether they could be anticipated.

Given the nature of Rule 201, we are mostly interested in the latter approach, as our aim is to fully understand whether the circuit breaker halts are predictable upon some extent, and which are the factors at the stock-level that correlate with a higher propensity to the treatment. The efficient market hypothesis, although not criticism-free, has established the foundations for the wide majority of what today is known about financial markets and asset pricing.

Fama (1970) already provides a valid framework to analyze our issue. According to his theory, semi-strongly efficient markets is enough to rule-out the possibility of any anticipation of price movements, as prices move closely following a random walk. Only private information (non-observable) has predictive power, and its release onto the markets could explain large stock price shifts. Some authors have attempted to estimate and measure the unobservable factors using several proxies such as the language employed in firm communications (Tetlock et al., 2008) or employee satisfaction (Edmans, 2011). However, the evidence provided in these stream of research are still oriented to larger windows of analysis and forecast and still not conclusive enough for our interests.

As commented before, the efficient market hypothesis serves as the cornerstone of many asset pricing theories and results, but several researchers have found questions left unanswered that challenge its validity. Among them, we highlight the concept of momentum and the theories of market timing and magnitude of market participants' reaction are highlighted above any others.

Momentum can be defined as the trend of positive returns on stocks with a history of positive growth in the near past and the trend of negative results in stocks with losses in the past. The contributions of Carhart (1997) and later Fama and French (2012) have found evidence in favor of the momentum factor as one of the main drivers of stock returns, and most of the empirical work in finance is based upon these models whenever dealing with returns and excess or abnormal results.

An interesting observed fact about momentum strategies is that they are likely to lead to crashes, especially at times of market declines (Daniel and Moskowitz, 2016), so a potential factor that could increase the propensity of a stock to fall a 10% is that it accumulated several days with a positive return that overvalues the stock and then the market reacts to correct stock value. In fact, Barroso and Santa-Clara (2015) find evidence of the risk of momentum crash to be manageable due to its predictability.

All of the factors highlighted are related to explaining why strange situations could happen for a given stock so that the price could be translated into the Rule 201 halt. Characterizing this likelihood of 'strange' situations is still today one of the hot topics in the literature. Most of the research shows the role of the higher moments of the return distribution, mainly skewness and kurtosis, as priced factors when explaining asset returns due to the information contained in these parameters.

Statistically, given how the Rule 201 assigns the treatment, it seems reasonable to analyze the incidence of the Rule 201 triggers conditional on these factors, as we should expect that events concentrate around assets which are specially volatile, as they reach the -10% requirement more easily as in the example defined in (1.10). Also assets which exhibit a positive return skewness should trigger more Rule 201 bans, given that they show an imbalance between negative and positive returns. Finally, we would also expect that the kurtosis of the stock returns distribution is positively correlated with the propensity of triggering Rule 201. These triggers are likely to be considered as extreme events away from the mean, which is precisely the characteristic of the population that kurtosis is measuring (Westfall, 2014).

Conrad et al. (2013) employ the derivatives market to calculate the implied third and fourth moments of the underlying securities and find a strong link between these factors and stock performance. In their study, they find evidence in favor of positively skewed stocks to have lower returns in the short-term. Other authors, such as Chang et al. (2013) or Amaya et al. (2015) find supporting evidence in line with these conclusions. Kadan and Liu (2014) also highlight the role of these higher moments in decomposing the extra returns obtained by momentum and value-investing strategies. For the purpose of our study, it seems clear that we must account for these higher moments to disentangle whether they have any influence on the likelihood of treatment.

1.4.4.2 Sector Analysis, Size and Volume

We start the identification of the necessary covariates by analyzing the incidence of Rule 201 triggers by sector. One could argue that industry-related events could be a potential reason for stocks to suffer severe price devaluations that would be translated into Rule 201 events. Then, it seems reasonable to control for plausible concentration of events in specific sectors. A potential source of concentration would be related to the perceived risk that each sector has. For instance, one should expect a higher individual volatility on rapidly growing industries, such as hi-tech. For very mature sectors (such as energy) the Rule 201 events would likely be more related to common factors across the industry with a substantial clustering around specific dates.[20]

In line with this idea, some academics (Brunnermeier and Oehmke, 2013) as well as regulatory authorities are concerned about the possible negative effects of short selling in certain strategic industries that due to their specific characteristics could have major negative spillovers. A clear example is that of financial stocks, as the rapid decline in bank valuations could be translated into a market panic scenario whose aftermath could severely shock economic welfare. In fact, this was precisely the reasoning behind the emergency ban of 2008 that covered only stocks in the financial sector.

In Table 1.2 we report a detailed frequency analysis of the distribution of all the stock-day observations in our sample and the share of each sector in the total sample, the sample of Rule 201 events and the sample of events close to the threshold.

From Table 1.2, we can observe some remarkable differences in the incidence by sectors. For instance, stocks classified in the financial sector, such as banks or insurance companies account for roughly 43% of the sample, while only for 11% of Rule 201 events. A contrary observation is located in the manufacturing sector, where the proportion between sample and treated sample jumps from 22% to 30%. The mining sector as well experiences a jump from 4.82% to 9.24%. Overall, the data

[20]For instance, a change in the oil extraction policies would affect all stocks in the energy sector. This sector that (on average) we expect it to be composed of blue chips and low volatility stocks, could show a marked correlation or, more precisely, clustering around specific dates. Therefore, we must account for these reasons behind the trigger that could differ from the reasons of stocks in other sectors.

from Table 1.2 reveals that Rule 201 events are not randomly distributed conditional on sector, as some exhibit much larger incidence than expected according to the share of the total sample they represent. This highlights the necessity of controlling for sector when selecting the ideal counterfactuals, as the incidence of Rule 201 appears to be sector-dependent to a certain degree, at least.

Our next analysis of Rule 201 incidence focuses on two additional factors that have been widely used in previous inference in the context of short-selling bans: trading volume and market capitalization. Several authors have shown their preference for the size and volume factors as good candidates for counterfactual variables, given that they could proxy for many different dimensions that define the asset. Beber and Pagano (2013) in the context of the financial crisis emergency bans and Barardehi et al. (2019) follow a similar strategy in the context of Rule 201.

In Table 1.3 we repeat a similar analysis of the Rule 201 events distribution by nine different groups; constructed as interactions between groups of size and volume. The procedure for ranking the stocks is as follows: first, for every stock-day we calculate the last 10 day average trading volume and market capitalization, excluding the observation day. The use of lagged averages ensures that whether the stock under analysis triggers Rule 201 on day t does not contaminate either variable of interest. Second, we split the sample in three different groups for each variable with proportions 30-40-30. Finally, the interaction of the three groups in size with the three groups in volume form the nine groups of analysis.

Table 1.3 exhibits a clear correlation between size and volume, which should be expected. In addition, both series of events show a marked asymmetry, as almost half of the population is concentrated in stocks that rank in the group of the smallest firms and less traded stocks. Considering the design of Rule 201 and its -10% requirement, this result is consistent with commonly observed results in the literature that relate small firm size (Duffee, 1995) or smaller trading volume (Chan and Fong, 2000) to volatility and price shocks.

1.4.4.3 Common Risk Factors and the Distribution of Returns

As a last step, to increase the granularity of our analysis of incidence, we will focus on the most representative realization of the information about the company status and its near future: the behavior in the stock market. More precisely, the variables of analysis will be the common risk factors as defined in the Carhart (1997) four factor model and the first four moments of the return distribution. For each close-to-the-threshold event we calculate dynamically the four factor market model using an observation window of the previous forty trading days. A similar approach is used to estimate the sample first four moments, during the same window.

We argue that the common risk factors should be kept comparable between the treated stock and its counterfactual to ensure that both assets react similarly to market events. A similar argument holds for the analysis of the return distribution. Both the counterfactual and the treated stock should exhibit similar return patterns to claim that they behave in a similar manner and that ex-ante, they have statistically equivalent volatilities, propensities to negative results (skewness) and density in the propensity to outliers (kurtosis). Otherwise, an omission of these observables could lead to wrong interpretation of causality, given that one could compare two stocks that are close to the threshold for different reasons.

In this setting, we perform a graphical comparison of the shape of Rule 201 close-to-the-threshold events distribution of the four estimates of Carhart (1997) plus the stock α against the full population, to analyze whether substantial differences are observed. To do so, we compare both groups via quantile plots in Figure 1.7. In a similar fashion, we repeat the quantile plot for the four factors of the return distribution in Figure 1.8.

The analysis of the quantile plots of these figures reveals a clear difference in the distribution of the Rule 201 events for all the variables analyzed. In terms of the market factors, the Rule 201 events show a marked inclination towards extreme values in the five factors compared in Figure 1.7. In terms of the four common risk factors' estimates, this result makes sense as higher absolute values on the β coefficients are related with stocks that amplify the variation in returns of the different portfolios, therefore revealing a pattern of stocks whose returns are more likely to experience sudden moves. This same reasoning is confirmed by the analysis of the volatility quantile plot in Figure 1.8. All of the first four moments of the distribution differ between samples but the most remarkable by far is the one observed in the return variance. The visual evidence from this graph confirms the crucial role volatility has in the likelihood of Rule 201 events. Overall, the visual analysis of the differences in distributions highlights the necessity for including these covariates in the empirical design to ensure comparability across the units of analysis.

In short, we have found several stock specific factors related with a higher incidence of Rule 201 triggers and that should be kept equal among comparable pairs to ensure causal interpretation. We will conclude the analysis by assessing the degree of relationship between the covariates previously highlighted. We believe it is of interest to address the question of whether some covariates already control for the effect of other factors. It is reasonable to expect that stocks with a larger firm size are also less volatile stocks. However, the key to understand whether both covariates should be included or not depends on the capacity of one to explain the effect that the other could have on the potential outcome. In other words, the researcher must consider how good some covariates are as proxies for others in a given setting for the outcome of interest. To address this concern in a general form, we calculate the correlation matrix for the covariates under study to evaluate the relationship among them.

Table 1.4 calculates the Pearson's correlation coefficient between all the pairs of covariates that we have so far analyzed separately in this section. The size and volume factors seem to capture a substantial part of the variation in the market beta and stock variance (correlation between size and variance is of approximately -0.4). Overall, apart from volatility and size, the linear relationship across the covariates is not as direct as one could expect and for many pairs of factors it is close to 0. This supports our view that the accurate identification and selection based on these covariates remains crucial in this framework as heterogeneity in the reasons for the trigger remains even when close to the threshold. We argue that under this setting, RDD is a necessary condition to restrict the analysis to stocks with similar dynamics, but **not sufficient** to satisfy the CIA, requiring selection on observables and panel data techniques to rule out time and stock fixed-effects that may affect the outcome of interest or the reasons why the stock triggers the circuit breaker.

1.5 Conclusions

Historically, the opportunities for analyzing causality in the framework of short selling bans have been scarce. Considering the significant dispersion of opinions about the topic among academics, practitioners, and regulators, there is a substantial need for establishing causality that sheds light on the true effects of these restrictions. Regulators justify the existence of short selling bans as a mechanism that increases investor confidence in the financial markets and helps in the prevention of steep price declines, specially when stocks are undergoing extraordinarily volatile times.

The literature offers mixed and contradicting evidence about the true effect of restrictions in outcome variables of major interest, such as liquidity, asset pricing

(Beber and Pagano, 2013), market quality (Boehmer et al., 2013), or the speed of information incorporation into prices (Boehmer and Wu, 2012), among many others. Other authors defend the effectiveness of short selling bans in preventing predatory behavior (Brunnermeier and Oehmke, 2013) or in increasing the information of trades by removing noisy short selling (Kolasinski et al., 2013). Most of the empirical contributions on the topic build on the series of policy changes that happened across the early 2000s (Russell 3000 pilot program between 2005 and 2007) and after the 2008 global financial crisis (emergency bans). While these settings could provide a good setting for causal identification, we are concerned about the representativeness of the conditions under which the studies are conducted. In the same line, a recent paper by Boehmer et al. (2020b) supports this concern by empirically showing that the Russell 3000 pilot program created by itself an unique framework, compromising the generalization of the results in this framework.

Considering the numerous implications that the regulation could potentially have in many dimensions, the evaluation of the role of short selling bans with alternative frameworks or techniques that shed light on the still unresolved questions is as necessary as ever. We contribute to the literature at this point. In this paper we perform a review of the short selling regulation in the US, describing the most relevant contributions and identifying some caveats that affect previous research designs. We also analyze the most recent (and currently active) short selling regulation: Rule 201 as a plausible research framework.

Despite some authors having already used Rule 201 in their works (i.e. Jain et al. (2012); Barardehi et al. (2019) or Switzer and Yue (2019)), we are not aware of any paper that covers the issue of causality and idoneity of Rule 201 as a research framework; which definitely should be the first step before drawing conclusion about any outcome of interest.

Through an econometric development and a thorough analysis of the Rule 201 circuit breakers for more than six years, we show that Rule 201 events exhibit potential for studying many of the still unresolved questions. Its main advantages arise from its unique design plus the fact that it has been active for a long period of time, offering a large, rich and heterogeneous sample that covers many different market states, rather than very specific frameworks as was the case with the pilot program or the 2008 emergency ban.

Throughout the paper, we offer advice on the design of an empirical setup that combining features of regression discontinuity, experimental studies and differences-in-differences, can effectively identify causality in this framework. We believe this paper should be of special relevance for those researchers interested in employing Rule 201 as their research framework, guiding them in the process of designing the correct strategy to obtain insightful analyses that help academics, practitioners and regulators to understand the implications of short selling bans more accurately, hence contributing to a better understanding of the topic and the pursue of the optimal regulation level.

Figures

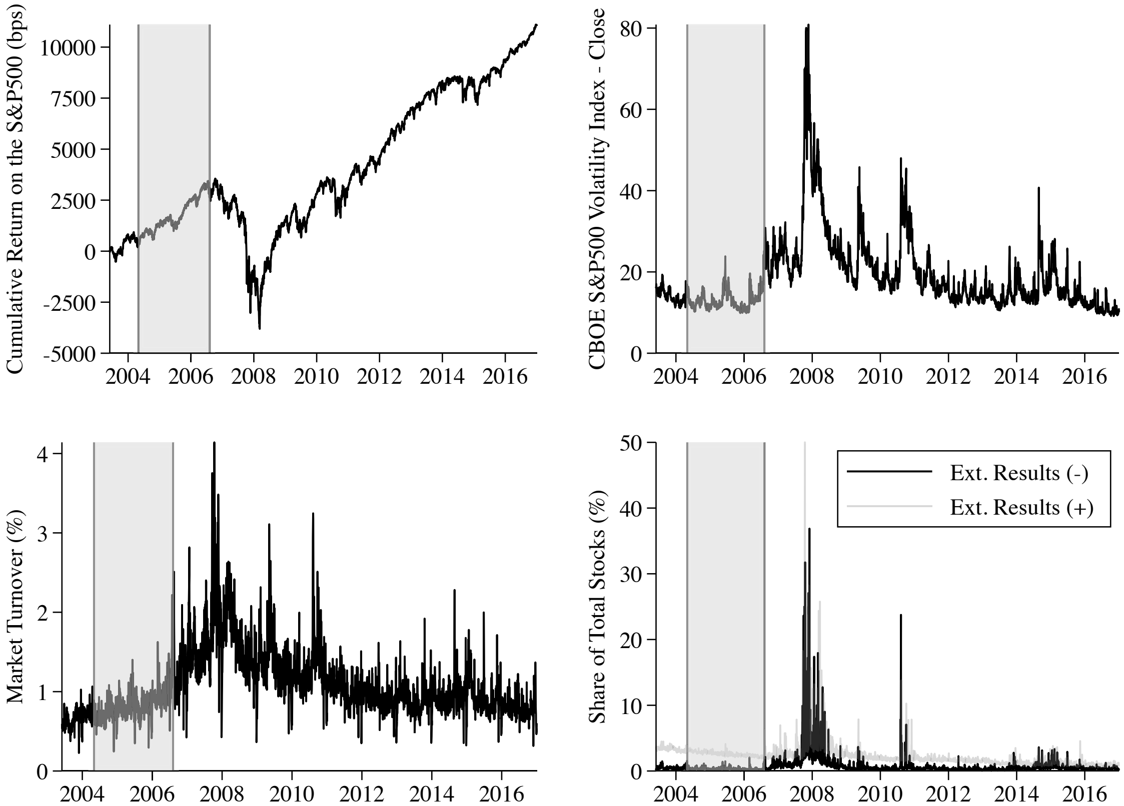

Figure 1.1 - Financial Markets (Pilot Program vs. No Pilot)

The four graphs plot the evolution of the S&P500 cumulative close-to-close return, the CBOE Volatility Index on S&P500 options (VIX), the total market turnover and the proportion of traded stocks that experience an extreme result (more than an absolute 10% close-to-close return), respectively. The studied time period goes from June, 2004 until December, 2017. In all four graphs, the pilot period (May 2, 2005 - August 6, 2007) is marked by the shaded area.

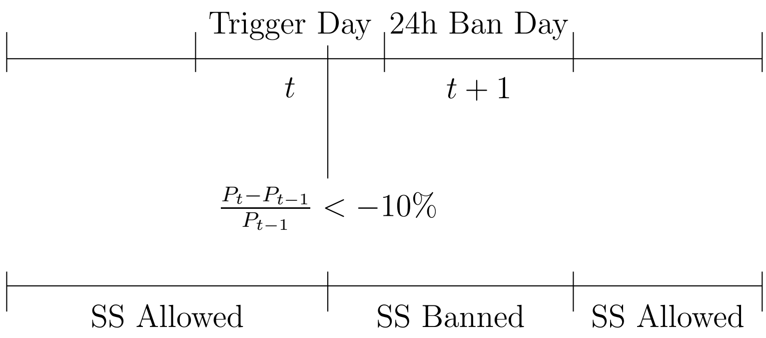

Figure 1.2 - Rule 201 Trigger and Time Span

This diagram explains the application of the short sale ban and its length. Given that the stock price falls below a 10% with respect to the previous closing price, at any time during the trading day, the stock is subject to the restriction for the rest of the current day and the next one. If the price does not fall again below 10% in the next day (t+1), unrestricted shorting activity is then permitted starting on t+2.

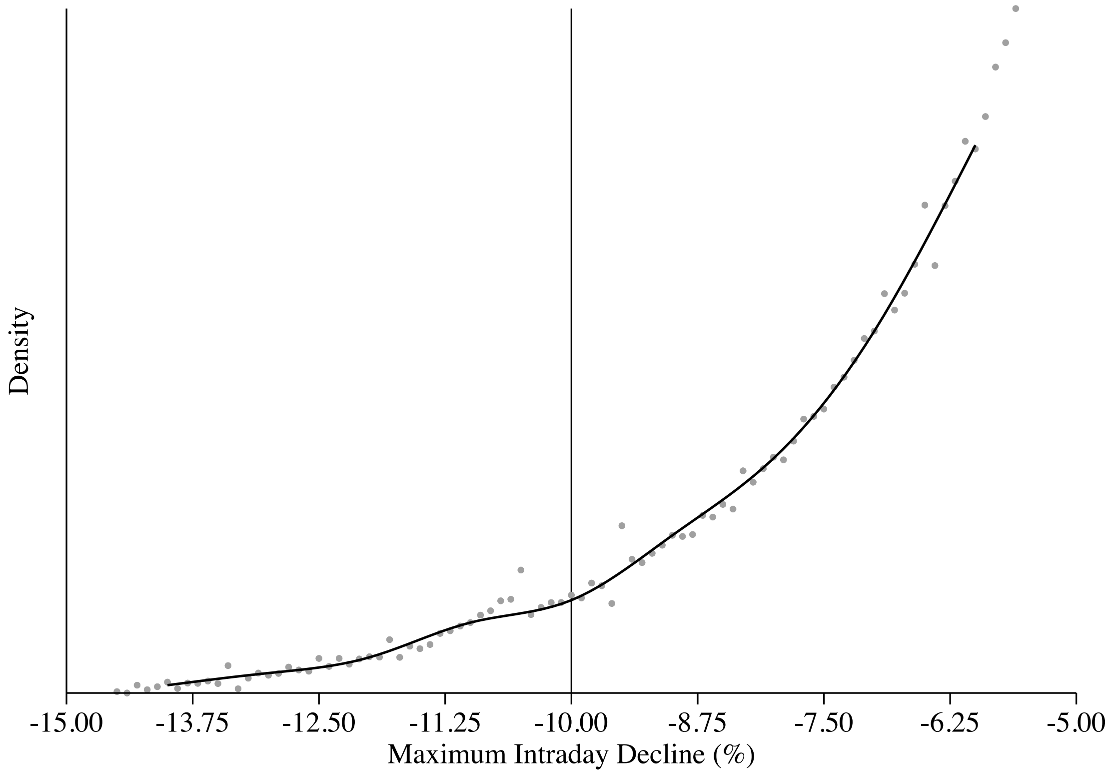

Figure 1.3 - Density of MID Around the Discontinuity

This graph evaluates the density of the maximum intraday decline (MID) variable around the discontinuity threshold of -10%. MID is defined for each asset i and date t as:

$$\mathrm{MID_{i,t}} = \frac{\min(P_{i,t}) - P_{i,t-1}^{CL}}{P_{i,t-1}^{CL}}$$

Where $\min(P_t)$ stands for the minimum trading price of a stock across the trading day t, while $P_{i,t-1}^{CL}$ stands for the previous day closing price. Overall, the distribution is smooth and no sign of clustering is perceived arounf the -10% point.

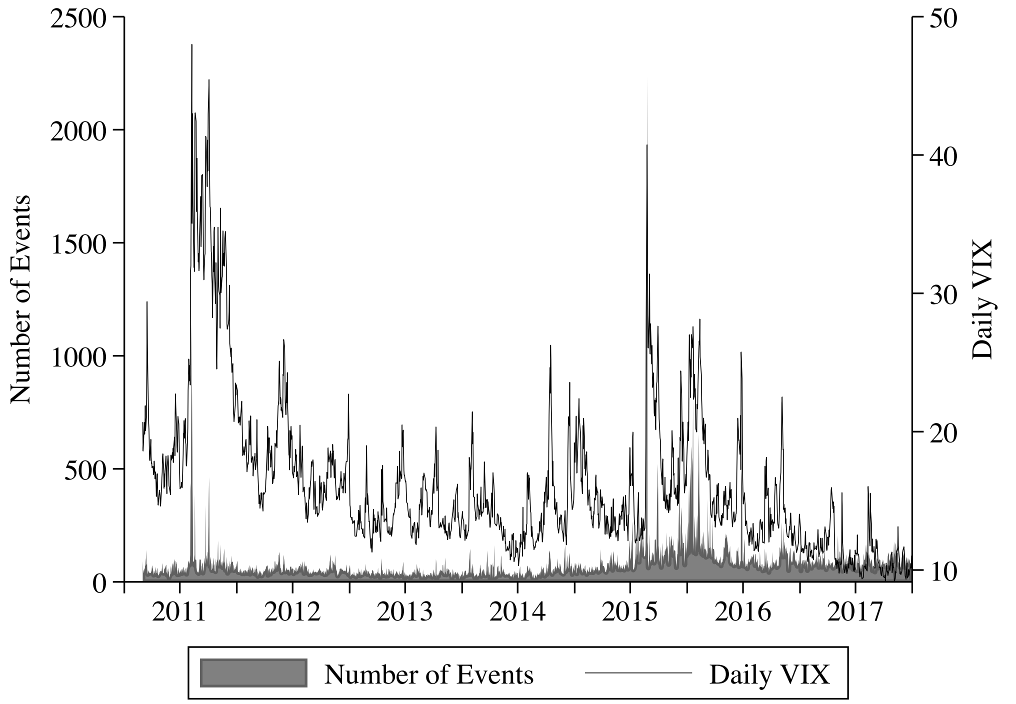

Figure 1.4 - Time Distribution of Rule 201 Events

This diagram summarizes information about the distribution of Rule 201 circuit breakers and market volatility. The solid black series reflects the number of daily events across the period of study (2011-2017). The dashed series reflects the daily VIX. Overall, the series of events remains stable across time. The two larger peaks correspond to August 8^{th}, 2011 (Black Monday) and August 24^{th}, 2015 (2015's Flash Crash).

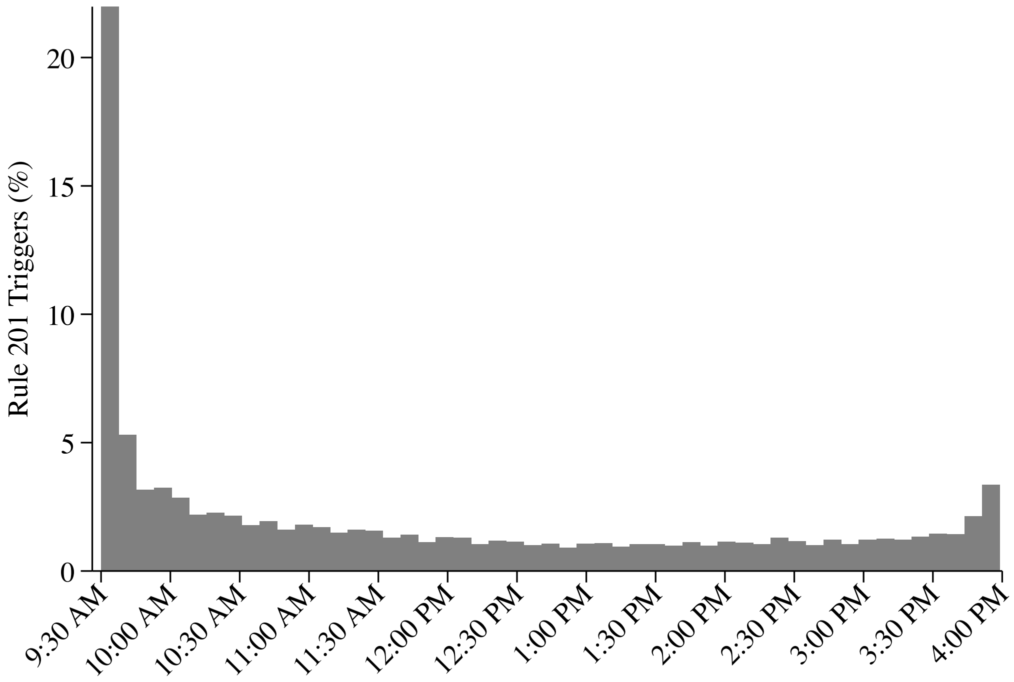

Figure 1.5 - Circuit Breakers Distribution (Trading Hours)

This graph plots the histogram of frequencies for the circuit breakers series across the trading hours. In the original sample we record 896 (roughly 0.8% of the sample) events that happen outside trading hours. For economy of resources and for reasons of comparability and traceability we decide to remove these 'out of hours' triggers from the study. The graph is therefore limited to the active trading hours, containing information about 111,000 triggers approximately.

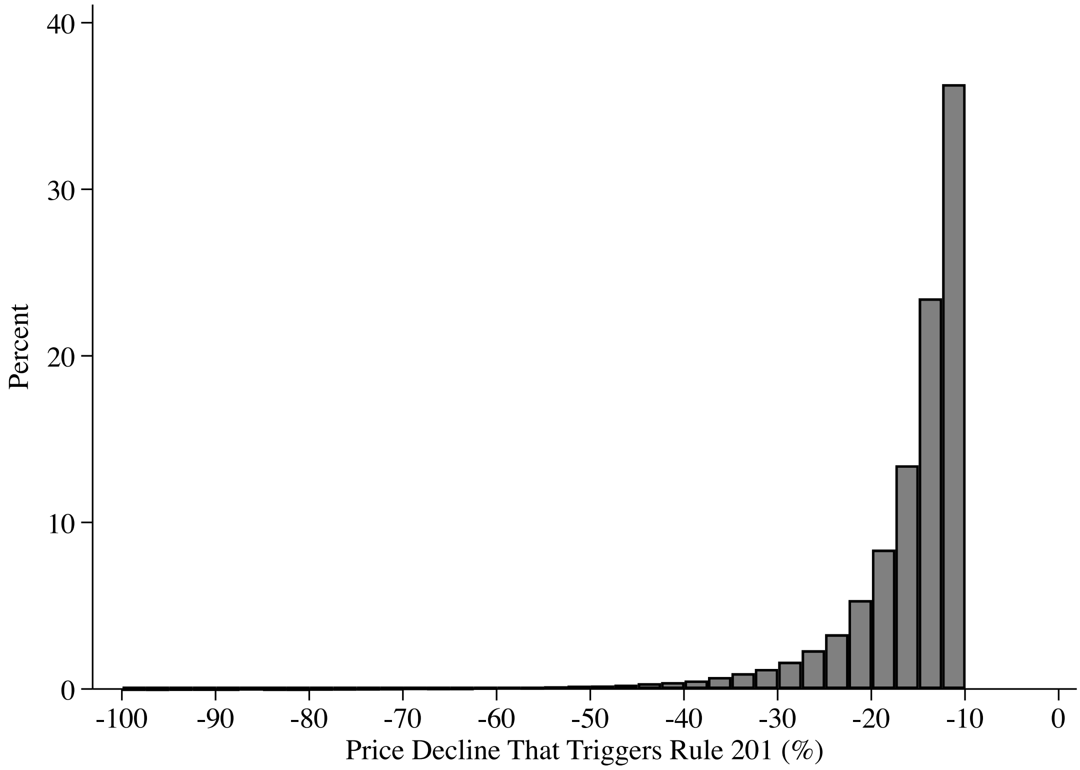

Figure 1.6 - Conditional Distribution of MID on Rule 201

This histogram provides a visualization of the distribution of the price movement that has triggered Rule 201. This graph depicts a clear concentration of events close to the circuit breaker threshold. In numbers, more than 36% of the events lie in the interval [-12.5%, -10%]. In terms of the identification of any effect, this indicates that studying the events close to the threshold is interesting as it is precisely the most numerous subset in the event population. Moreover, the series shows a marked decline once we move away from the threshold point.

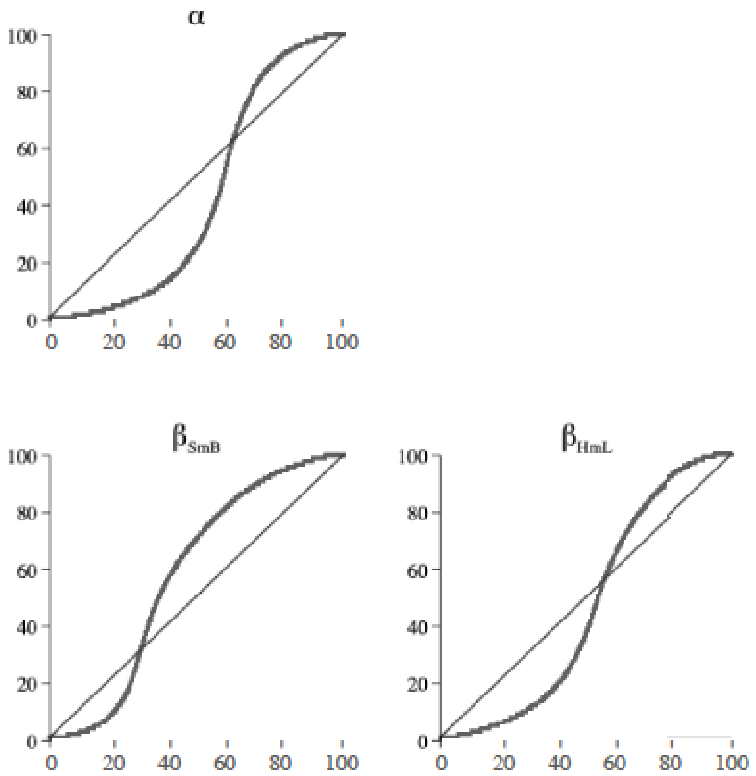

Figure 1.7 - Carhart (1997) Four Factors Quantile Plots

These five plots compare the distributions of the four factor market model estimates plus the stock α for the full population and the subgroup of Rule 201 events around the discontinuity. The diagonal black line stands as the reference population, in which the quantiles smoothly increase constantly, distributing the sample in groups of equal sizes. The dark gray stepped line stands for the quantile distribution of the sample of events.

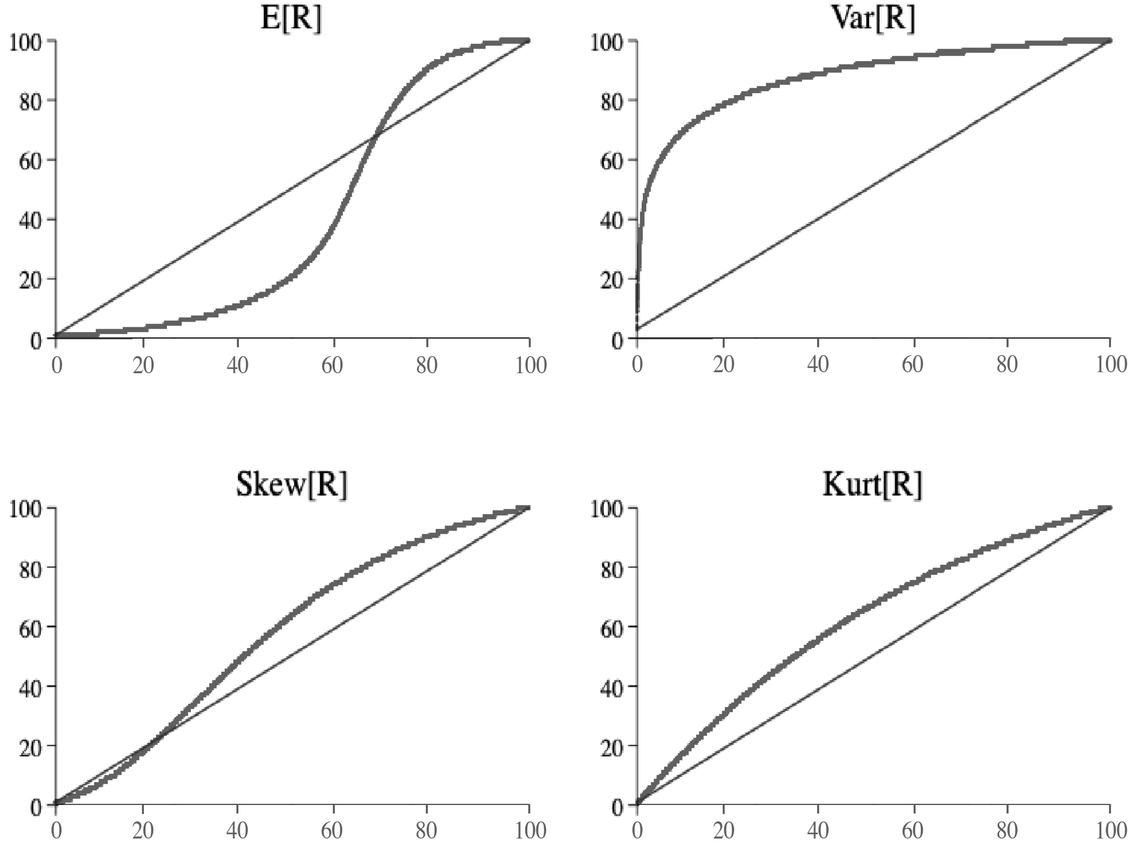

Figure 1.8 - Return Distribution Quantile Plots

These four plots compare the distributions of the first four moments of the return distribution for the full population and the subgroup of Rule 201 events around the discontinuity. The diagonal black line stands as the reference population, in which the quantiles smoothly increase constantly, distributing the sample in groups of equal sizes. The dark gray stepped line stands for the quantile distribution of the sample of events.

Tables

Table 1.1 - Rule 201 Events Month-Year Distribution

	Year						
Month	2011	2012	2013	2014	2015	2016	2017
January	0	955	778	953	1.248	6.049†§	1.778
February	0	1.053	818	722	1.019	4.700§	1.688
March	1.260	936	744	969	1.415	3.526§	2.109
April	881	1.038	875	1.058	1.536	2.043	1.530
May	1.034	1639†	923	989	1.685	2.941	2.372
June	1.276	1.213	789	658	1.684	2.852	1.921
July	1.122	1.219	711	722	2.728	1.864	1.561
August	4.990†	1.267	913	793	5.428†	2.108	2.460
September	1.994	895	592	869	3.394	1.527	1.633
October	2.084	1.017	1.030	1.495	2.822	1.951	2.106
November	1.911	1.305	955	1.093	2.889	3.098	2.512
December	1.494	1.011	710	1.549	3.924§	2.109	1.935

This table reports the distribution of Rule 201 circuit breakers by year and month for the period of the study. We conduct two different statistical tests to highlight the specific months where the amount of events is significantly higher than the mean. Marked with †, we have the months that have a number of events higher than two standard deviations above the mean number of events per month of that year. Marked with §, we have the months that have a number of events higher than two standard deviations above the mean number of events in that specific month across the years. January and February are marked with 0 because Rule 201 did not become effective until February 28, 2011.

Table 1.2 - Rule 201 Events: Incidence by Sector

	Total		Rule 201		Threshold Events	
	N	(%)	N	(%)	N	(%)
Agriculture	23510	0.20	258	0.23	85	0.25
Construction	86249	0.73	907	0.81	267	0.79
Financials	5117469	43.53	12700	11.38	3588	10.56
Manufacturing	2576316	21.92	33122	29.68	10307	30.34
Mining	566632	4.82	10305	9.24	3532	10.40
Non Classifiable	860789	7.32	24725	22.16	7503	22.09
Public Admin.	4254	0.04	12	0.01	6	0.02
Retail Trade	367997	3.13	3333	2.99	948	2.79
Services	1174469	9.99	16656	14.93	4955	14.59
Transport & Util's.	748962	6.37	6951	6.23	1976	5.82
Wholesale Trade	218769	1.86	2405	2.16	739	2.18
Total	11755363	100.00	111582	100.00	33967	100.00

This table reports the distribution of Rule 201 events by sectors for the period of the study. We classify stocks in 9 groups according to their trading volume and market capitalization. Columns (1) and (2) report the absolute (N) and relative (%) frequency of the observations (day-stock) for each sector in our sample. Columns (3) and (4) report the same statistics but only relative to day-stock observations in which a Rule 201 circuit breaker is triggered. Finally, Columns (5) and (6) repeat the statistics but only for the day-stock observations that are close to the threshold. More specifically, those for which the Maximum Intraday Decline is between -12% and -10%.

Table 1.3 - Rule 201 Events: Incidence by Market Cap. & Volume

	Total		Rule 201		Threshold Events	
Mkt. Cap. × Volume	N	(%)	N	(%)	N	(%)
Low 30% × Low 30%	2879351	24.49	56355	50.51	15631	46.02
Low 30% × Mid 40%	608619	5.18	14224	12.75	4118	12.12
Low 30% × High 30%	27523	0.23	958	0.86	235	0.69
Mid 40% × Low 30%	595825	5.07	3907	3.50	1328	3.91
Mid 40% × Mid 40%	3620491	30.80	22097	19.80	7727	22.75
Mid 40% × High 30%	483713	4.11	5436	4.87	1938	5.71
High 30% × Low 30%	14706	0.13	48	0.04	13	0.04
High 30% × Mid 40%	518414	4.41	974	0.87	324	0.95
High 30% × High 30%	2995994	25.49	7504	6.73	2653	7.81
Total	11755363	100.00	111582	100.00	33967	100.00

This table reports the distribution of Rule 201 events by sectors for the period of the study. We classify stocks in nine groups according to their size and volume. Columns (1) and (2) report the absolute (N) and relative (%) frequency of the observations (day-stock) for each sector in our sample. Columns (3) and (4) report the same statistics but only relative to day-stock observations in which a Rule 201 circuit breaker is triggered. Finally, Columns (5) and (6) repeat the statistics but only for the day-stock observations that are close to the threshold as defined in Table 1.2.

Table 1.4 - Rule 201 Events: Correlation Matrix

	MC	TV	α	β_{Mkt}	β_{SmB}	β_{HmL}	β_{Mom}	E[R]	Var[R]	Skew[R]	Kurt[R]
Market Capitalization (MC)	1.000	0.848	0.035	0.280	0.172	-0.041	-0.052	-0.031	-0.396	-0.275	-0.073
Trading Volume (TC)	0.848	1.000	0.064	0.297	0.179	-0.039	-0.071	0.014	-0.168	-0.182	-0.021
α	0.035	0.064	1.000	-0.088	0.020	0.010	0.019	0.687	0.060	0.301	0.057
β_{Mkt}	0.280	0.297	-0.088	1.000	0.013	-0.030	-0.106	-0.074	0.019	-0.049	-0.050
β_{SmB}	0.172	0.179	0.020	0.013	1.000	0.136	0.035	-0.033	-0.006	-0.020	-0.039
β_{HmL}	-0.041	-0.039	0.010	-0.030	0.136	1.000	0.226	0.011	-0.003	0.010	0.006
β_{Mom}	-0.052	-0.071	0.019	-0.106	0.035	0.226	1.000	0.066	-0.061	-0.030	0.043
E[R]	-0.031	0.014	0.687	-0.074	-0.033	0.011	0.066	1.000	0.107	0.397	0.114
Var[R]	-0.396	-0.168	0.060	0.019	-0.006	-0.003	-0.061	0.107	1.000	0.363	0.143
Skew[R]	-0.275	-0.182	0.301	-0.049	-0.020	0.010	-0.030	0.397	0.363	1.000	0.129
Kurt[R]	-0.073	-0.021	0.057	-0.050	-0.039	0.006	0.043	0.114	0.143	0.129	1.000

This table reports the correlation matrix for the set of covariates that have been separately analyzed in the study: firm size (market capitalization), trading volume, the four factor market model estimates and the stock's α and the first four moments of the return distribution. The diagonal term is always equal to 1 as it captures the correlation of each factor with itself. Variable definitions and calculations remain equal as the ones previously described: the stock return distribution moments and market model estimates are calculated dynamically with an observation window of 40 trading days before each day. Firm size is calculated as the average market capitalization of the stock during the past two weeks (ten trading days). Finally, trading volume reports the average dollar volume in the same period (ten previous trading days). Neither volume nor size factors include the current day observation to prevent contamination of the factors by the trigger of the circuit breaker.

Chapter 2

Short Selling Bans, Overpricing and Price Efficiency

2.1 Introduction

The debate about the true consequences of short selling constraints, their effectiveness and the consequences on market microstructure is open among academics, regulators and practitioners. The controversy lies around whether short sellers are active contributors to market efficiency or rather act as predatory traders which induce unjustified bearish pressure. Often, regulators have opted for short selling constraints as a measure to stabilize markets, especially, as a reaction in times of extraordinary volatility and uncertainty.

This restrictive approach is not fully supported in the literature. In some contexts, such as financial stocks, a restriction on short selling can help prevent predatory behavior that affects severely this kind of assets (Brunnermeier and Oehmke, 2013). Nevertheless, the vast majority of the literature highlights the valuable role of short sellers as effective channelers of information about future underperformance, fostering price efficiency (Karpoff and Lou, 2010) and reducing overreaction (Boehmer and Wu, 2012).

The evidence is combined with the main theories building on the efficient market hypothesis, which indicate that the removal of short sellers should be associated with worse market quality in terms of illiquidity (Diamond and Verrecchia, 1987) and inefficient overpricing (Miller, 1977) due to the over-weighting of optimistic trading. Recent changes in the regulations across the world have opened the field for testing these classical results under frameworks of event studies, showing evidence in favor of constraints being related with unjustified stock returns and less liquidity, especially for more vulnerable and smaller stocks (Beber and Pagano, 2013).

In the aftermath of the global financial crisis and as a response to the growing concern among the financial sector, the SEC revisited its unconstrained short selling policy and established the alternative uptick rule or Rule 201. This new constraint was designed as a temporary restriction, specifically shaped to correct documented flaws associated with prior regulations while helping to stabilize the market for stocks under periods of distress. Up to date, very few authors have assessed how different is Rule 201 from its predecessors in terms of its effects on the market microstructure and the price formation process. In this paper, we focus on the effect of this new constraint in asset prices, more specifically in the generation of stock overpricing when the ban is active and also in price efficiency, by analyzing how constrained stocks take more time to incorporate publicly known information.

This chapter contributes to the literature in a variety of aspects. Beyond previous work, we are one of the first papers in the short selling constraints literature to study constrained versus unconstrained stocks during a long time of observation, contrasting with previous results highly focused on short term periods. In our results, we find evidence about the effect of the ban on supporting asset prices artificially and in reducing the speed of price discovery. We argue that precisely due to the artificial price support that Rule 201 provides, stocks undergoing difficult times could be experience an increase in short-term volatility. Our evidence connecting short selling bans and reduced price efficiency are in line with those of Engelberg et al. (2012), Boehmer and Wu (2012) and Boehmer et al. (2020a). Overall, our assessment of the ban effects on stock prices raises the question of whether the market friction that the ban supposes is really helpful for reducing volatility and increasing investors' confidence, two of the main goals highlighted by the regulatory authority when designing the rule.

The rest of the chapter is organized as follows. Section 2 summarizes the history of short selling constraints in the US, the documented results and related literature. Section 3 describes the data. Section 4 details the causal identification in the context of the Rule 201 and our measure of the variables of interest. Section 5 presents the main results. Section 6 tests the robustness of the results and Section 7 concludes.

2.2 The Effects of Short Selling Constraints

Many times, the regulatory authorities have reacted with a short selling ban as a reaction to control and reduce market instability, hence assuming that part of the severe corrections are related with the predatory behavior of some short traders. After the stock market crash in 1929, the SEC started the procedures to control and limit short selling, to prevent aggressive strategies from being future threats to market stability. The result was the uptick rule, which limited short selling at any price below the last trade, therefore preventing short sellers to profit from predatory strategies. This regulation was virtually the same until its removal in 2007.

Financial theory offers several perspectives about short sellers and their role in market microstructure. In the classical models, short sellers are beneficial for the market efficiency, as they anticipate information from future underperformance. According to Miller (1977) a removal of short sellers should be associated with an inefficient overpricing of assets, as the negative opinions on the stock cannot be channelled through trading by short sellers. Several authors provide evidence in favor of Miller's hypothesis. Jones and Lamont (2002) find significant overpricing of stocks that are specially expensive to borrow in the period before the uptick rule was implemented. Dechow et al. (2001) also support the idea that short sellers convey valuable information through their trading as explained by the short interest in a set of stocks that show poor fundamental performance in terms of profitability and valuation ratios.

As a response to a growing concern, the SEC started questioning the true effect of the uptick rule by 2004 and designed a pilot program[1] study around the restriction. The motivation was to provide a natural experiment for researchers to identify accurately the effects of the regulation, as up to 2004 there was a substantial lack of an adequate framework to isolate the effects.

The main conclusions on the pilot program deemed the restriction as inefficient in preventing price manipulation or enhancing market stability. Also, the portion of temporary unconstrained stocks outperformed the counterparts in terms of liq-

[1]We refer to the Russell 3000 pilot program, enacted in SEC's Release No. 50104 / July 28, 2004, by which one third of the Russell 3000 Index stocks were no longer subject to any of the short selling restrictions. See "Order Suspending the Operation of Short Sale Price Provisions for Designated Securities and Time Periods" at https://www.sec.gov/rules/other/34-50104.htm for further information.

uidity.[2] As a result, the SEC eliminated the uptick rule and implemented Rule 201 of Regulation SHO which effectively prohibited exchanges from exerting any short selling restrictions from July, 2007 onwards. However, the unconstrained short selling policy was reverted shortly after. In the context of the global financial crisis, the SEC ordered an emergency ban on the short selling of financial stocks to prevent further damage from predatory trading orders driving prices beyond fundamental values.[3]

Using either the Russell 3000 pilot program (see, e.g. Diether et al. (2009b) or Fang et al. (2016)) or the 2008 emergency ban (see, e.g. Billingsley et al. (2011) or Beber and Pagano (2013)) these authors document a significant relation between constraints and overpricing. Karpoff and Lou (2010) also relates short sellers to more efficient prices as they are capable of incorporating valuable information. Boehmer and Wu (2012) find a similar conclusion and show how short orders are valuable vehicles of fundamental information that reduce the market overreaction to news.

On the other hand, Brunnermeier and Oehmke (2013) defend the effectiveness of the bans in reducing predatory behavior on especially vulnerable assets. Kolasinski et al. (2013) find causality between the short selling constraints and the informa-

[2] "On July 28, 2004, the Commission issued an order creating a one-year pilot temporarily suspending the tick test and any short sale price test of any exchange or national securities association for certain securities. The pilot was created so that the Commission could study the effectiveness of short sale price tests. The Commission's Office of Economic Analysis and academic researchers provided the Commission with analyses of the empirical data obtained from the pilot. In addition, the Commission held a roundtable to discuss the results of the pilot. The general consensus from these analyses and the roundtable was that the Commission should remove price test restrictions because they modestly reduce liquidity and do not appear necessary to prevent manipulation. In addition, the empirical evidence did not provide strong support for extending a price test to either small or thinly-traded securities not currently subject to a price test." Extracted from "Press Release: SEC Votes on Regulation SHO Amendments and Proposals; Also Votes to Eliminate "Tick" Test" at https://www.sec.gov/news/press/2007/2007-114.htm.

[3] "Under normal market conditions, short selling contributes to price efficiency and adds liquidity to the markets. At present, it appears that unbridled short selling is contributing to the recent, sudden price declines in the securities of financial institutions unrelated to true price valuation. Financial institutions are particularly vulnerable to this crisis of confidence and panic selling because they depend on the confidence of their trading counterparties in the conduct of their core business." Extracted from "SEC Halts Short Selling of Financial Stocks to Protect Investors and Markets" at https://www.sec.gov/news/press/2008/2008-211.htm

tiveness of these transactions, suggesting that they are helpful for the markets to efficiently incorporate news into prices.

After the emergency actions of 2008, the SEC started procedures to implement an updated version of the uptick rule, going back to the regulatory approach. By 2010, the SEC presented the amendments to previous Rule 201, removing the prohibition on establishing restrictions and defining a new price test:[4] the **alternative uptick rule** or **Rule 201 circuit breaker**. The new mechanism was designed specifically to benefit from the stabilization effect of restricting short selling in a declining market[5] but minimizing the observed consequences of overpricing and reduced price efficiency arising from removing short sellers from the market.[6]

The alternative uptick rule is a short selling restriction measure adopted by the Securities and Exchange Commission on February 26, 2010. Hereinafter, we will refer to it as the **Rule 201**, but to be precise, this new price test is the result of a series of amendments to Rule 201 Regulation SHO. Originally, this directive from the SEC removed all the previous price tests after the conclusions on the Pilot study that deemed short-selling bans as ineffective.

[4]In this context and along the paper we refer to price tests as the mechanisms that evaluate a given short selling order and authorize or deny it according to the terms in which the order was submitted. Most of the short selling regulations, including the old uptick rule and the new Rule 201 short selling ban employ this approach by "testing" the price at which a given order is sent and only allowing it if it is above a certain threshold (e.g. for the Rule 201 ban, the threshold is the National Best Bid and Offer). This test ensures that given a short selling constraint only those short selling orders that would not accelerate the price decline are incorporated into markets.

[5]"The circuit breaker approach of Rule 201 will help benefit the market for a particular security by allowing participants, when a security is undergoing a significant intra-day price decline, an opportunity to re-evaluate circumstances and respond to volatility in that security. We also believe that a circuit breaker will better target short selling that may be related to potential bear raids and other forms of manipulation that may be used to exacerbate a price decline in a covered security." Extracted from Release No. 34-61595, Securities and Exchange Comission at https://www.sec.gov/rules/final/2010/34-61595.pdf.

[6]"At the same time, however, we recognize the benefits to the market of legitimate short selling, such as the provision of liquidity and price efficiency. Thus, by imposing a short sale price test restriction only when an individual security is undergoing significant downward price pressure, the short sale price test restrictions of Rule 201 will apply to a limited number of securities, rather than to all securities all the time." Extracted from Release No. 34-61595, Securities and Exchange Comission at https://www.sec.gov/rules/final/2010/34-61595.pdf.

The Rule 201 ban prohibits the short selling of any security at or below the national best bid if that security's price has fallen below a threshold of 10% relative to the last closing price. Once the trigger condition is met, short sale orders at or below the best bid are immediately prohibited for the asset for the remainder of the current trading day and the whole of the next one. The rule does allow for the possibility of an activation on consecutive days. If this happens, the ban extends for an additional trading day after the last trigger. Trading centers are required to comply with the new regulation since February 28, 2011.[7]

This alternative uptick rule represents an innovation with respect to previous short sale restrictions. In contrast with previous bans, the trigger condition is endogenously determined. Whether the prohibition is imposed or not depends on the behavior of the stock's price in the market. Previous research was based upon regulations that arbitrarily forbade (2008 emergency ban) or allowed (Russell 3000 pilot program) short-selling for a list of stocks. Furthermore, Rule 201 acts as a temporary correction mechanism, that is automatically reverted shortly after its application, which contrasts with previous bans which were in force for much longer time periods. The effects of this new short lived constraint and whether they differ from those observed with previous regulation is still an open question.

Up to date, research on the true effects of Rule 201 is scarce. Jain et al. (2012) analyze the ban had it been applied under the flash crash of 2010 and during the most volatile trading dates of the 2008 crisis, documenting ineffectiveness at reducing price declines. Halmrast (2015) finds no significant effect of the ban on stock prices. However, both research designs overweight the retrospective analysis, with little contribution in the analysis of the actual events. In Jain et al. (2012) only two months are included after February, 2011 (the compliance date) and Halmrast (2015)

[7]Division of Trading and Markets: Responses to Frequently Asked Questions Concerning Rule 201 of Regulation SHO. Accessed: Sep 28, 2017.

excludes part of 2012, precisely the most volatile months in which assessing the effects of the ban is more interesting for market participants and regulators.

Davis et al. (2017) find evidence of price clustering, a sign of price inefficiency while Switzer and Yue (2019) document no effect on the main metrics of market quality. Both studies do not go beyond 2012 in their analysis and their results arise from differences-in-differences analyses that compare the Rule 201 affected stocks with a before treatment situation (before February, 2011) that allows no comparison with contemporaneous counterparts. Given the significant informational component we could expect from a price decline of 10% (or more) we believe there is potential for a better identification of the effects when the units of analysis share informational sets, which cannot be the case when the treated unit and the control unit are selected with time gaps.[8]

Our article connects to the literature at this point. We are one of the first large scale empirical studies[9] of Rule 201 that isolates the effect of the short selling ban on stock prices and price informational efficiency. In line with previous research and the overpricing theory we expect the restriction to be associated with overvaluation of

[8]As later discussed in Section 3, and in line with H. Florindo (2020a), we show how the Rule 201 likelihood of treatment assignation is not purely random and uniformly distributed, but rather closely related to both idiosyncratic factors of the asset and also specific situations in the markets and specific days. A misinterpretation of the covariates influencing the likelihood of the treatment and the lack of controlling for them could severely bias the results (Rosenbaum, 2002). If the likelihood of the treatment is time variant and dependent on the specific day-market situations, non-contemporaneous control units should not be considered as candidates for the control.

[9]We are aware of only one other working paper conducting a similar analysis to ours (Barardehi et al., 2019). These authors focus on intraday high frequency data only for the period 2011-2013. They find evidence of the ban being associated with lower trading volume and higher returns. Their identification is based on regression discontinuity design (RDD), assuming that stocks with similar price dynamics are comparable once they control also for the size and volume factors. This has been a common approach in the literature (i.e. Beber and Pagano (2013)) but H. Florindo (2020a) raises the question of whether in the framework of Rule 201 this is enough for causal identification. In the paper, the author reviews the econometric basics for conducting inference under the setting of the new regulation and conclude that while RDD may offer partial identification, it is highly recommended to include other covariates (including time and stock fixed-effects) that account for differences in the reasons why assets may reach the threshold. In this paper, we follow these recommendations and bring to the empirics the more detailed research strategy that is proposed in H. Florindo (2020a) to ensure a correct identification of the effects and remove the concerns about confounding bias or selection bias.

the constrained stocks. Also, in line with Saffi and Sigurdsson (2011) and Boehmer and Wu (2012) we hypothesize that the limitation of short selling is related with a delayed price discovery and reduced price efficiency.

We propose a differences-in-differences methodology combined with a matched pairs analysis that will compare two sets of stocks (events and controls) which will differ only in the treatment assignment. The procedure is substantially analogous to the recommendations of H. Florindo (2020a) to conduct inference in the context of Rule 201. We first set as a discontinuity threshold between groups the -10% price fall (this is, the Rule 201 circuit breaker threshold) and allow a bandwidth of 1% at each side of the discontinuity. This approach is inspired in regression discontinuity designs, to keep the focus on stocks that had similar behaviors. However, as H. Florindo (2020a) points out, depending on the outcome of interest, a reasonable similarity in the selection variable (in this case the price dynamic) by using only observations very close at each side of the threshold may not guarantee a correct identification.

In the context of Rule 201, and more important, in the context of the outcome of interest of this paper (stock prices) it seems reasonable to ask whether the same price fall may mean the same for every stock. It seems clear that we should not consider as comparable stocks that, for instance, reach a -10.5% price fall for different reasons. The strategy[10] is based on matching stocks according to two sets of covariates: firm fundamentals (industry, size, trading volume) and market dynamics (returns distribution and market model factors). In addition, we control also for time and stock fixed-effects by using a differences-in-differences panel data methodology and select the comparable control stocks (counterfactuals) only from a pool of assets that had the near-the-threshold price fall during the same day and in a similar trading day point as that of the event that we analyze.

[10] A more detailed exposition of the actual procedure can be found in Section 2.4.

Our strategy ensures that we compare two stocks with similar market-wide (market factors and same date price movement) and idiosyncratic (fundamentals) characteristics, that have similar information shocks at the same time and that therefore, comply with the assumption that conditional on these covariates, the application of the treatment can be considered as random between the treated and the control group.

Following Beber and Pagano (2013), we test for the presence of overpricing by studying stock returns and the residuals of market model regressions or abnormal returns. We find statistical evidence in favor of the overpricing hypothesis, as the short selling ban supports prices artificially and delays the price adjustment to known information. Moreover, the artificial support on prices translates into a non-fundamental price shock that is produced when short sellers are allowed back in the market again. This suggests that Rule 201 bans prevent short sellers from channeling all their valuable information in a timely manner, delaying this until they can operate again. The delayed price shock raises the question on whether Rule 201 is actually helpful for reducing uncertainty around stocks undergoing substantial difficulties. This effect is robust to the use of different specifications, controls and market models used.

Inspired by Hou and Moskowitz (2005) and Boehmer and Wu (2012) we use low frequency price delay measures that capture the price adjustment to public information as a proxy for the information contained in the stock price at a given time. Our tests show evidence in favor of the short lived ban being detrimental for price informativeness not only at the time of the ban, but for several days after as well. The results are in line with the literature supporting short sellers as channelers of valuable information about future underperformance (Engelberg et al., 2012).

2.3 Data

We collect the data on Rule 201 bans from the Philadelphia Stock Exchange website,[11] which publishes the list of stocks that trigger the circuit breaker in a daily basis. Our period of study covers all observations from February 28th, 2011 (the first day in which the ban was in effect) until December 29, 2017. The almost seven years of study period account for more than 100,000 different events. We match the events with the Center for Research in Security Prices (CRSP) database to obtain daily data on prices, returns, trading volume, shares outstanding and the Standard Industrial Classification (SIC). Finally, we match our sample with the daily set of the Fama-French factors, gathered from Ken French's website.[12] After cleaning for duplicates, missing observations and incomplete data, the final sample accounts for 2,900 events plus their corresponding matched counterparts.

2.4 Identification Strategy and Methodology

2.4.1 Causality and Event Study Design

The design of the Rule 201 mechanism represents a clear innovation with respect to previous short selling constraints. Its functioning once it is active is very similar to that of the previous uptick rule, as it forbids short sellers to post orders at any price at or below the NBBO. This is specifically designed to prevent short sellers from driving prices further down and allow long sellers to liquidate their positions at the bid price if they want to. According to the SEC, this design will prevent predatory behavior from short sellers who want to profit quickly from a declining market while protecting long sellers and prioritizing their orders (Securities Exchange Comission, 2010).

[11] https://www.phlx.com
[12] http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/DataLibrary/.

Moreover, the short selling ban is not permanently active, but linked to a circuit breaker: an intraday price decline of 10% or more from the last day's closing price. Also, the ban is effective only for the current trading day in which the circuit breaker is triggered plus the totality of the next trading date, with the possibility of consecutive triggers.[13]

Contrary to previous permanent (uptick rule) or one time (emergency ban of 2008) regulations, the Rule 201 ban does not generate a static sample of events, but rather a growing one that evolves daily depending on the price dynamics of the market. This constantly growing sample ensures the heterogeneity of the context surrounding the events, easing the identification of the ban effect and reducing the concern about a set of unobservable factors commonly shared by the selected treated group.[14] To assess if the events are clustered to specific dates / market events or whether they are distributed uniformly H. Florindo (2020a) analyzes the frequency distribution across the whole study period, February, 2011 to December, 2017 together with the daily evolution of a market-wide volatility measure: the Chicago Board Options Exchange Volatility Index (VIX).

Figure 1.4 reveals a continuum of events around their mean, which for this period is of 65 triggers per trading date. Regarding the VIX, we should expect a certain degree of correlation between the series, given the motivation behind the implementation of this restriction: reducing market volatility and stabilizing prices in highly

[13]This is, a given stock that again suffers a price decline of -10% or more in the day after a trigger. Suppose stock XYZ's closing price at day t is \$20. If at any point during $t+1$ XYZ is traded at a price of or lower than \$18 (fall of 10% from \$20 $\rightarrow$ -\$2) the circuit breaker is triggered and short selling is restricted for the remainder of $t+1$ and the whole date $t+2$. If XYZ closes at \$17 at $t+1$ and, during $t+2$ a trade is executed at or below \$15.30 (fall of 10% from \$17 $\rightarrow$ -\$1.70), then short selling would continue to be constrained for the whole day $t+3$ and so on until no 10% fall is recorded for the given asset.

[14]This concern is specially relevant in those studies using the emergency ban of 2008, focused only in financial stocks which were selected specifically because of their unique exposition to the market volatility at that time. An excessive similarity among the units of analysis may confound the identification of the effect with an idiosyncratic factor of the individuals belonging to the treated group.

volatile markets. On average, the correlation between the events' count and the VIX series is of 0.31, although it is highly asymmetrical. As the sample is restricted to the days with more records, the correlation between the series increases notably. In the group of days above the 75th percentile by the number of triggers the metric goes up to 0.35, with a correlation of 0.5 when we consider only days above the 95th percentile. For the left tail of the distribution, however, the series are practically uncorrelated (-0.0049) when considering days below the median (days with 53 triggers or less). From Figure 1.4, we observe that the time window of our sample includes many different types of days in terms of heterogeneity in the treatment assignation of Rule 201. In all our specifications we will include a control for the amount of events recorded during each day, to ensure our results are not biased by the specific characteristics of a given day with specially high volatility.[15]

Next, we check the likelihood of treatment assignation, this is, the probability of each stock to trigger the Rule 201 short selling ban. One possibility is to consider a random assignation, as in Davis et al. (2017) or Switzer and Yue (2019), where the sample of events is directly compared to the rest of stocks, without a sample selection. We are concerned about this approach.

As first pointed out by H. Florindo (2020a), in the context of Rule 201 it is likely that such an intraday substantial price decline as the one required by the circuit breaker triggers may be linked with stock specific factors. One of them is stock volatility, as one should expect Rule 201 events to be more likely for highly volatile assets, with a record of aggressive price movements. We verify the argument with a proportion test of Rule 201 triggers proportions between groups of volatility.

To define the groups, we classify all stock daily observations in our sample into

[15] Although we propose a panel data estimation with time fixed-effects and this should rule out this possibility, we believe it is appropiate to explicitly control for an observable covariate that could identify different degrees of generalized market instability.

quartiles according to volatility. The percentile assignation is done dynamically, i.e. we conduct the grouping each trading date, to ensure we account for market-wide volatility in the assignments. We need to ensure we are comparing proportions across dynamic groups, this is, across the most volatile stocks each day vs. the least volatile. Otherwise, a pooled quartile calculation of the full sample would distort the validity of the argument as the estimated proportion difference could arise from day-specific factors rather than from group-specific factors (in this case return volatility). From Table 2.1 we can observe a substantial increase in the proportion as the stocks are classified into more volatile groups. The differences in proportions are statistically significant across the six groups, with the most volatile stocks having an observed likelihood of more than 600 times higher than the assets in the least volatile group. This is perfectly reasonable, as the fixed nature of the threshold for all stocks makes it more likely for assets with a higher fundamental volatility to reach the trigger easily.

Following a similar logic, higher moments of the distribution of returns could play a role in influencing the treatment likelihood.[16] Given the extraordinary event that an intraday 10% fall represents for an average asset, we could reasonably expect a higher likelihood on positively skewed assets (more prone to negative extreme results) or those with higher kurtosis (fatter tails in the return distribution - more prone to extreme results in either direction).

There exists a second source of concern about the factors influencing the Rule 201 likelihood of treatment: the exposition to common risk factors, such as those from the CAPM, Fama and French (1993) or Carhart (1997). As the treatment is assigned contingent on a given return, the portion of such returns explained by

[16]In addition, the literature supports the idea that the higher order moments are priced factors by investors (see e.g. Dittmar (2002), Conrad et al. (2013), Chang et al. (2013) or Amaya et al. (2015)). To avoid the omitted variable bias, it is essential to account for potential differences in these factors that could differ across the event sample and the average population.

common factors can contribute in assessing the probability of trigger for a given asset: e.g. we should expect events to be concentrated in the population of stocks that historically show a larger absolute systematic risk (β) as they amplify market movements in either direction (thus, reaching the trigger threshold easier).

To check whether there exist significant anomalies or asymmetries between the Rule 201 triggers and the rest of the population we provide a graphical analysis comparing both samples. Figure 2.1 plots a box graph to compare the shape of the distribution of the aforementioned factors across groups: The visual evidence from Figure 2.1 supports our concerns, as except for the market β factor, there is a marked difference in the distribution between the stock-day population and the events observations. This suggests that considering the Rule 201 triggers as a randomly assigned treatment is far from trivial, as we have evidence of event clustering around stocks with a particular set of characteristics about its recent past. Recall that all our statistics from Figure 2.1 are calculated with rolling windows of 40 trading days before each daily observation and therefore reflect the particular performance of a given asset in its recent past rather than a historical perspective. In the context of Rule 201, it is important to contextualize stocks according to their recent performance as the treatment assignation is dynamic as well, rather than a fixed, one-time ban enactment.

Regarding the clustering around market model factors, we find that events are associated with stocks that show specific underperformance (low α), more exposed to the size premium (higher SMB factor), and therefore smaller companies on average. We also find a substantially lower momentum, indicating more propensity for the treatment in the group of loser stocks. With respect to the first four moments of the stock return distribution, none of the factors is close (in terms of the median) when comparing the treated group and the rest of the population, but clearly, the largest difference is in the comparison of return volatility. As expected, there is a significant clustering of events around the stocks which had a specially volatile recent past. Also, our concern about the higher skewness and kurtosis looks justified from the graphical results of Figure 2.1. H. Florindo (2020a) study the correlation between these factors and find that the size and volume covariates traditionally used show a certain capacity of proxying for volatility, but lack power in terms of other covariates that show explanatory power on the incidence of the Rule 201 events. To ensure that results are not attributable to an omitted variable bias, we stick to the original conclusions of the author and include the covariates analyzed in Figure 2.1 as well as size and volume as our criteria for selecting the most similar counterfactuals.

Overall, the analysis of the distributions of this set of factors reveals two characteristics about the sample of stocks subject to the ban. First, there is a lot of variability among the stocks that trigger the circuit breaker as for all factors, the IQR is significantly larger than that of the population, except for return volatility. Second, the shape of the distributions and the mean-median comparison suggest the inviability of treating Rule 201 triggers as random, as there is evidence of the recent past of the stock being associated with the likelihood of triggering the circuit breaker.

To properly identify the ban effect and solve this concern surrounding the likelihood of treatment, we propose a matched pairs sampling that selects only events and controls with reasonably equivalent characteristics that would ensure the treatment assignation is a random process given the similarity among the treated and the control sample.

We start our sample selection by setting a discontinuity threshold equal to that of Rule 201 circuit breaker (-10%) allowing a bandwidth of 1% at each side of the threshold. We reduce our sample of events and potential controls only to those trading days in which we record a maximum intraday fall for the asset in the interval

[-11%, -9%]. Those stock-day observations with a maximum intraday decline in the interval [-11%, -10%] and registered in our record of Rule 201 triggers will be our **events**. Those stock-day observations with a maximum intraday decline in the interval (-10%, -9%] will be our **candidates** for the control group. We calculate the maximum intraday fall as the log return between the minimum price of the security in a given trading day and the previous day closing price (similar to the Rule 201 definition for the trigger):

$$MaxIntradayFall_{i,t} = log\left(\frac{min(P_t)}{P_{t-1}}\right) \quad (2.1)$$

The design of this discontinuity is intended to capture only stocks that have had similar price dynamics during the day and in which both events and controls are simply selected by a marginal difference in their price decline. Moreover, we impose that for any control candidate to be matched with an event the price decline must have happened at the same date. This ensures that the price movement is not only reasonably similar but also happened with the same informational set for both the event and the control. With this in mind, we also control when the price decline is reached across the trading hours. Switzer and Yue (2019) introduces the document a significant U-shape pattern in the distribution of events across the trading hours that is present in our framework, as shown in Figure 1.5.

Given the large concentration of events at certain points (especially at the market opening) we require the matched pairs to be formed by an event and a control both of which experience the decline in the same time window. Given that the frequency of our data is daily and that the exact trigger time is recorded only for the events we must estimate when the control stock reaches its low. To do so, we separate the trading day in three blocks: market opening (up to the first thirty minutes of trading), during the trading day (from 9:31 AM to 3:30 PM) and the last thirty minutes of trading. Events are classified in any of these three groups according to their time of occurrence. Controls are classified in the first block (at opening) if their opening price is already in the discontinuity bandwidth we set, i.e the stock opens at already a 9% fall or higher (up to -10%) and also, this opening price is reasonably the same as its day minimum. Our similarity requisite is of a difference between the CRSP minimum day price and opening of less than 0.0001. A very similar algorithm classifies the control candidates into the third block (the last 30 minutes of the trading day), in this case comparing the day minimum and the closing price. All other candidates not classified in either block are assigned to the second one, the one compiling triggers across the trading day.

As we use daily close-to-close data from CRSP, we are concerned with the capacity our strategy to identify causality during the day in which the stock triggers Rule 201, as we have no exact record for transactions other than for the exact time of the trigger for the treated group. However, we argue that the day after the trigger, during which short selling is prohibited for the full trading day is still a relevant and interesting framework to analyze. On this day, we can compare two assets that had a similar price shock the day before, that show a statistical level of similarity that allows for considering the restriction to be randomly applied to one of the assets. Moreover, we focus on the day in which the ban is lifted, this is, once the event stock is back again shortable, to assess whether the re-entrance of short sellers has any effects on the outcomes of interest. Regardless of our research focus, we believe it is necessary to account for this 'trigger moment' to ensure we are pairing assets with similar information shocks, in terms of when the information is revealed at the day and moment of trading day levels. To the best of our understanding, H. Florindo (2020a) is the first to highlight this concern of the 'informational set' surrounding the triggers and we are the firsts to control for it in the context of the Rule 201 events. We believe it will be useful for future research on the topic to consider this moment of treatment assignation as a pairing covariate, to ensure that stocks have had similar price dynamics for similar reasons.

Once we have classified our control candidates, we remove from our pool of events and candidates incomplete observations (missing data from CRSP), duplicates and extremely thinly traded assets (less than 30 trades/day on average) or those with the smallest quoted price (less than $2) to ensure our results are not affected by the specific characteristics of these assets.

We construct the matched pairs with a nearest neighbor methodology with replacement.[17] The matching factors are the four estimates for the Carhart (1997) model, the first four moments of the return distribution, market capitalization, trading volume and also the stock average price. The market model factors and return distribution moments are calculated during the 40 days prior to the event date, similar to the approach we use in Figure 2.1. Additionally, we require that the event and the control stock in each pair are classified in the same industrial group[18] to account for potential unobservable sector-wide changes in the informational set.

We will impose a final requirement for selecting our sample regarding the quality of the matching and require a minimum score for the stock pair to be included. we impose a final requisite on our pairs regarding the quality of the matching. To do so, we define our **pair score** measure:

$$\text{PairScore} = 10 \cdot \left(1 - \frac{\sum_{f=1}^{F} |x^{e}_{f,p_{100}} - x^{c}_{f,p_{100}}|}{F * 99}\right) \tag{2.2}$$

The expression in Equation 2.2 sets a score for each pair event - control candidate

[17] The matching is conducted using the percentile ranking of the covariates rather than the actual values themselves to avoid the continuous covariate bias (Abadie and Imbens, 2006).

[18] Classified by the 10 major groups according to their SIC.

that is obtained as the sum of the absolutes differences in percentiles for all matching factors, divided by the maximum possible difference, which would correspond to the product of the number of factors F times the maximum possible difference in each evaluation (100 - 1 = 99). Then, the result is scaled such that a score of 10 represents a perfect match and 0 is the worst possible match. In the pairing algorithm, each event is then assigned the control stock for which the pair score is maximized, and only those pairs whose score is at least 7.5 are included in our final set of events to ensure the similarity between the treated and control groups. Table 2.2 below summarizes the sampling mechanism, the loss of data in each step taken and the final amount of pairs.

The final sample is then constructed by building an event window of 81 days for each pair: 40 days before, 40 days after and the day of the trigger. In this setting, the identification of the ban effect relies in analyzing the differences between the event and control sample in the days after each trigger, more precisely we will focus on days 1 (when short selling is constrained for the full day) and day 2 (when the ban is lifted). On day 0, several mixed effects could render the inference confuse, as the daily frequency of the data does not allow for the differentiation between the part of the trading day that operated without the restriction and the part in which short selling is constrained.

On the contrary, on day 1 we are completely certain that the short selling restriction is active for the entire day while the control sample operates unrestrictedly. On day 2 we are also absolutely certain that the ban is lifted at the same time for all events and therefore short sellers start operating again at the same point for all affected stocks. The comparison between pairs at days 1 and 2 should exhibit potential for identifying causality in the effect of the removal (day 1) and the effect of the comeback (day 2). Pairs are formed such that up to day 0 both event and control stocks are statistically equivalent and the ban is applied to the events only by a marginally higher decline that is not related to any controllable covariate that we use for pairing, thus allowing for the identification of the difference between two equal stocks that only differ in the capacity of short sellers to post any order (controls) or only at a price higher than the NBBO (events).

In Table 2.3 we compare the matching factors for the control and event sample to assess the effectiveness of our pairing mechanism and describe our sample of study. Neither of these differences are found to be statistically relevant. This supports the validity of our design to study Rule 201 as an experimental study once we control for these set of factors. Overall, Table 2.3 reflects a sample of assets where assets with a recent volatile past, and with an unusual frequency of extreme results are overrepresented.

2.4.2 Measuring Overpricing

Following the usual approach in the literature we use two different measures to capture the ban effect on overpricing: stock close-to-close returns and abnormal returns. We define the latter as the difference between the close to close return from CRSP ($R_{i,t}$) and the estimated return ($\widehat{R}_{i,t}$). We estimate returns with three different specifications: capital asset pricing model (CAPM), Fama and French (1993) and Carhart (1997)). Our main conclusions are derived using the latter. To ensure that any of the potential effects observed in the analysis is economically significant, we also estimate the relative effect on both returns and abnormal returns. We define the standardized effects in terms of deviations from the estimated return volatility or the standard deviations of the out-of-sample residuals (the idiosyncratic volatility).

of each model estimation. We define standardized return and abnormal returns as:

$$\begin{aligned} StRet_{i,t} &= \frac{R_{i,t}}{\widehat{\sigma_{(R),i}}} \\ StAR_{i,t} &= \frac{R_{i,t} - \widehat{R_{i,t}}}{\widehat{\sigma_{(u),i}}} \end{aligned} \tag{2.3}$$

Where $\widehat{\sigma_{(R),i}}$ stands for the return volatility and $\widehat{\sigma_{(u),i}}$ is the standard deviation of the in-sample residuals of the estimation of $\widehat{R_i}$, the estimated idiosyncratic volatility parameter.

To ensure that the observed effects are attributable to the ban we estimate the model using a sample that is not the same as the one we use as the comparison basis (the period previous to the trigger, from days -40 to -1). To do so, we perform an out-of-sample estimation for each stock using a window of 40 trading days in the window of days -80 to -41 relative to the event date. At the same time, we want to ensure the model is still valid for the stock at the time of the ban; e.g. if we estimate the models using observations from a year ago, we could argue that the estimates are likely to change in a one year time gap. As a robustness check we select different windows in terms of size and starting periods, excluding in all cases the dates used in the model estimation from the event study. No significant difference is found when moving forward the window of estimation (e.g. to days -60 to -20 or days -50 to -10) or reducing the sample size (e.g using 20 days instead of 40) to estimate the average treatment effect.

The main specification of our regression model is a typical DiD design,

$$\begin{aligned} OP_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=\omega}^{T} \{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} + \\ \eta \cdot LIQ_{i,t} + \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t} \end{aligned} \tag{2.4}$$

$OP_{i,t}$ is our overpricing metric, either close to close returns or the abnormal returns

$(R_{i,t} - \hat{R}_{i,t})$ measured in basis points; or the relative effects as described in (2.3) measured as the number of standard deviations. $Event_i$ is a dummy variable that takes value 1 when stock i belongs to the treated group. Next we define $Day^{\tau}_{i,t}$ as a dummy variable that takes value 1 for those dates t that for asset i represent τ trading days away (from) the the trigger date that is defined as $\tau = 0$. As an example, $Day^{1}_{i,t}$ takes value 1 when date t is one trading day after the Rule 201 trigger for stock i and zero otherwise. The dummy for the trigger day is therefore $Day^{0}_{i,t}$. In our framework, it is necessary to index $Day^{\tau}_{i,t}$ by i because in our panel, the time variable stands for the actual date, rather than the relative days before or after the trigger. This way, we account for potential time fixed-effects that are clustered around certain dates of extraordinary volatility.[19] In the same fashion, we control also for specific days of higher incidence of the Rule 201 triggers by including a control variable $NTRG_t$ that captures the amount of Rule 201 triggers on the specific date t. This variable accounts for all Rule 201 triggers, not only those close to the threshold.

Our DiD estimates will be the coefficients β_τ of the interaction terms $Day^{\tau}_{i,t} \cdot Event_i$. These interaction terms will capture the differential effect associated with the stocks that have triggered Rule 201 vs those who do not at the specific relative day τ. The design of the model is flexible enough to meet the demands of assessing different windows of relative days. Parameters ω and T are arbitrarily selected in the model, to specify which is the window of observation for the relative days in each estimation. For instance, for parameters $\omega = -2$ and $T = 5$, the set of relative days that include fixed effects and interactions is {-2, -1, 0, 1, 2, 3, 4 and 5}.

According to Amihud and Mendelson (1986), the overpricing of constrained stocks can arise as an illiquidity premium (due to the removal of short sellers as

[19]As a robustness check, we also repeat the regression analysis (unreported) with a panel data structure setting relative days to the trigger as the time variable and controlling for each actual date with dummy fixed effects. Results are unchanged with this approach.

liquidity providers) rather than to the dispersion of opinions (Miller, 1977) and the removal of bearish investors. To account for this, we include a control for liquidity ($LIQ_{i,t}$). We conduct alternate tests using the closing bid-ask spread and the Amihud (2002) illiquidity measure. Finally, ν_i stands for stock i fixed effects and λ_t stands for date t fixed effects.

2.4.3 Measuring Price Efficiency

We measure price efficiency using a set of low-frequency metrics based on Hou and Moskowitz (2005) price delay factor, more specifically, the modified version introduced in Boehmer and Wu (2012). The base specification from Hou and Moskowitz (2005) is the following two-step procedure: First, they estimate stock returns as a function of the actual market excess return plus a set of five lagged terms of the same factor.

$$R_{i,t} = \alpha_i + \beta_{i,0} \cdot MKT_t + \sum_{k=1}^{5} \beta_{i,k} \cdot MKT_{t-k} + \epsilon_{i,t} \tag{2.5}$$

After the estimation of the model with the lagged factors, they run a restricted model in which all coefficients from the lagged terms are set to 0 ($\beta_{i,k} = 0 \;\forall\; k$) and define price delay as:

$$PriceDelay = 1 - \frac{R^2_{Restricted}}{R^2_{Unrestricted}} \tag{2.6}$$

The $PriceDelay$ measure captures the proportion of stock return variation that is explained by the lagged market returns. According to the efficient market hypothesis, the stock price at day t should incorporate all public available information at that time, thus, the return from t to $t+1$ must be related only to the contemporaneous market factor or newly released information, but not with past information. As we approach perfect market efficiency:

$$R^2_{Restricted} \approx R^2_{Unrestricted} \Rightarrow PriceDelay \approx 0 \tag{2.7}$$

Therefore, a higher price delay (closer to 1) is associated with a slower incorporation of public information into the price. Hou and Moskowitz (2005) employ this metric with a weekly sampling frequency to assess monthly price delay and Boehmer and Wu (2012) use effectively this low-frequency measure with daily observations to construct weekly factors.

Additionally, they define a modified version in which only negative market returns are considered. Boehmer and Wu (2012) define price delay as in Equation 2.6, but they estimate Equation 2.5 with the following specification:

$$R_{i,t} = \alpha_i + \beta_{i,0} \cdot MKT_t^- + \sum_{k=1}^{5} \beta_{i,k} \cdot MKT_{t-k}^- + \epsilon_{i,t} \tag{2.8}$$

MKT^- equals market return if it is negative and 0 otherwise. In this fashion, they study information incorporation conditional on days of negative information releases. The reason to do so is to capture the speed of information incorporation when short sellers are more active. The rationale for limiting to this subsample of market information is that short sellers mainly channel information about future underperformance (Christophe et al., 2010) by adopting a short position in an asset which they consider overpriced. This inevitably causes stock prices to decrease, so therefore, negative market performance should be a good indicator of short sellers activity in the market. Using either specification, Boehmer and Wu (2012) find a significant faster incorporation of information related to short sellers activity. We will estimate the effect of the short selling ban on price delay following a similar DiD approach as in Equation 2.4 using the Boehmer and Wu (2012) $PriceDelay$ as our dependent variable. The actual regression model is as follows:

$$\begin{split} PriceDelay_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=\omega}^{T} \{\gamma_\tau \cdot Day_{i,t}^\tau + \beta_\tau \cdot (Day_{i,t}^\tau \cdot Event_i)\} + \\ + \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t} \end{split} \tag{2.9}$$

Expression (2.9) is equivalent to the DiD model in (2.4) with the price delay measure as the dependent variable. All controls definitions are defined equivalently. We maintain the liquidity control due to the ambiguous evidence on the role of liquidity in determining price delay (e.g. Hou and Moskowitz (2005) found their price delay measure to be not related with liquidity while Brogaard et al. (2014) finds that liquid assets readjust their prices more timely to news).

2.5 Results

2.5.1 Overpricing

Regulatory authorities impose constraints on short selling with the main objective of preventing situations of severe underpricing of assets that may be caused from predatory behavior, sinking the price away down from its economic value. Therefore, it is expected that the ban should have an effect on prices, more specifically, the ban should help in dampening the price decline and help the stock recover to its fundamental value.

The recovery to the fundamental value could be reflected in two ways. The first is that it could be reflected by a positive return as the ban takes place (as once the participants exerting the bearish pressure are removed, there no longer exist participants that drive the price away from the fundamental valuation). Then, as buyers and long sellers conduct the transactions, the price returns to its true value - higher than the minimum price it reached during the stress period of shorting pressure. Then, this overpricing is a symptom of an improved market efficiency when the ban takes place, as the asset comes back to its true value.

This reasoning is perfectly valid under the assumptions that the price declines are not related to asset revaluation (e.g. they are not related to the disclosure of negative information) and also, that short sellers are not providing any extra information that is unknown to the rest of participants.

While Brunnermeier and Oehmke (2013) defend the effectiveness of the bans in preventing predatory (harmful) short selling on specially sensitive stocks (from the financial sector), the literature offers evidence in favor of short sellers as providers of useful information capable of anticipating future negative returns (e.g. Dechow et al. (2001), Diether et al. (2009b) or Karpoff and Lou (2010), among others). Therefore, their removal would be associated with overpricing but not because of an unjustified shorting pressure, but rather an overweighting of mistaken asset valuations from optimist investors that defer the convergence between the asset's economic value and its price. Miller (1977) is the first to introduce this theory of divergence of opinions, tested empirically several times, but with mixed evidence and ambiguous results.

It is not trivial to assess whether the presence of overpricing under short selling constraints is positively or negatively related with market efficiency or whether overpricing exists at all. Our first step is an analysis of the evolution of the stock prices around the day of the trigger. We start by quantifying the ban effect on stock returns and testing for its statistical and economic significance under the context of the Rule 201 short-lived bans. Table 2.4 reports the results for these analyses.

The evidence from Table 2.4 shows that no significant effect is found during the day in which short sellers are removed from the market. Surprisingly however, our estimates show a significant underperformance of around 35 basis points for the event stocks once that short selling is allowed back in the market. The estimates of further interaction terms ($\omega = 3, 4, 5$) are not statistically significant. The significant underperformance when short sellers come back to the market for this stock could be a sign of overpricing. According to the Miller (1977) framework, overpricing is defined as assets being overvalued compared to their true economic value because of the imbalance in investors' aggregate demand for the asset.

In other words, overpricing should imply a difficulty for the asset value to converge towards its fundamental value that could lead to a stabilization away from its true value. Miller's overpricing hypothesis is defined in this specific setting, with short selling constraints as a potential factor that would increase the imbalance and lead to higher deviations from the economic value of the asset. Analyzing the dynamics during the days after the trigger, our results suggest that after triggering a Rule 201 circuit breaker, the stock price stabilizes at a level above its true value during day 1. The stabilization is likely to be artificially caused by the removal of short sellers and not related to any other factor. Once short sellers are back allowed, they re-balance the asset supply and demand, moving the price back down, closer to the true value the asset should have had from the start. Our results support this view. An alternative explanation for this result that we observe is that when short sellers return they are driving the price down beyond fundamental valuation to make profits from predatory trading. However, under such scenario we should observe a pattern of return reversals in subsequent days. Nevertheless, our results suggest that market participants incorporate the information released by short sellers with their trades during day 2. In subsequent days the stock does not perform differently than comparable stocks, which due to not being short selling constrained were more accurately valued. Our results in the analysis of the speed of adjustment of prices to publicly known information (price delay) supports this argument.

To further disentangle the overpricing phenomenon, we now repeat our analysis of Table 2.4 but using abnormal returns as our dependent variable. We define abnormal returns as the difference between the actual daily close-to-close return and the estimated return based on a given market model. Recall that the model coefficient estimates are obtained from an out-of-sample estimation (in the window

-80 to -41 for each event and control) to ensure the return estimations are not influenced by the ban or its surrounding window.

The results from Table 2.5 reveal a similar pattern to that of analyzing close-to-close returns. The previously estimated underperformance stays around 35 basis points for treated stocks when short sellers are allowed at day 2. Still, no other interaction term (apart from that of day 0) is found to be significant. Moreover, the analysis by stages of Table 2.5 should allow us to identify if adding the different common risk factors mitigates the estimated effect because they partially capture it. Quantitatively, we observe that the inclusion of the different market factors do not affect the estimates for the interaction term during day 2, which is statistically equal across all six models. The similarity of the estimates of underperformance of the stock either by using returns or abnormal returns as the outcome of interest reveals that common risk factors cannot explain any part of the effect observed in stock prices at day 2. In other words, this result shows that the estimated underperformance arises from an idiosyncratic characteristic in which the event stocks differ from their counterparts. Given the statistical similarity of our sample, the matching strategy and the regression design, the only difference we can identify resides in the treatment.

In line with previous results, we successfully identify an overpricing effect associated with the short selling ban stabilizing prices artificially. Price stabilization was one of the goals of the regulation in the spirit of making financial markets more appealing to investors, even in times of difficulties. However, it is also necessary that for the Rule 201 to contribute to investors' confidence the price stabilization works in favor of more accurate prices. This is, that the stabilization prevents prices to drift away from their true value in a downwards spiral, rather than providing an artificial support level that is periodically removed and imposed as stocks trigger Rule 201. This fake stabilization could actually increase volatility and uncertainty

because of the regulation.

2.5.2 Price Efficiency

Whereas the empirical evidence on overpricing is more subject to discussion, there is a major agreement in the literature about the positive role of short sellers as traders with high quality information (Boehmer et al., 2020a), better understanding of publicly available data (Engelberg et al., 2012), anticipation and prevention of accounting fraud (Karpoff and Lou, 2010) and the negative effects that their removal generates on price informativeness (Chang et al., 2007).

Price informativeness and the speed of price discovery are essential features of the market for all its participants, not only investors for obvious reasons, but also for firms, to make decisions with better informational sets (Chen et al., 2007) and as a reference for fundamental asset valuation and optimal resource allocation(Goldstein and Guembel, 2008). Moreover, more efficient markets, with more informative prices prevent large sudden market movements that diminish investors' confidence in the financial markets as a tool, which is precisely the main concern of the regulatory authorities and actually, one of the main reasons they wield to reinstate the short selling bans.

Then, the natural question that arises is why do regulators approve a constraint for which the majority of the past empirical evidence supports the idea that it diminishes rather than improves price efficiency? The unique nature of Rule 201 and its mechanism is precisely designed to take into consideration all the lessons from past studies as acknowledged by the SEC:[20] "At the same time, however, we recognize the benefits to the market of legitimate short selling, such as the provision

[20] Retrieved from Release No. 34-61595, Securities and Exchange Comission at https://www.sec.gov/rules/final/2010/34-61595.pdf.

of liquidity and price efficiency. Thus, by imposing a short sale price test restriction only when an individual security is undergoing significant downward price pressure, the short sale price test restrictions of Rule 201 will apply to a limited number of securities, rather than to all securities all the time."

Therefore, an adequate evaluation of the true effects of Rule 201 on price efficiency is economically relevant for market participants, regulators and even firms, as it will shed light on the question whether the new design of the short selling ban is able to reduce the harmful effect documented in its predecessors on the speed of price discovery. In contrast to previous short selling bans (like the old uptick rule), the change of policy from regulators is substantial, as they now value the information provided from short sellers and regard the short selling ban as a 'last resort' mechanism to stabilize a stock undergoing difficulties.

Following Hou and Moskowitz (2005) and Boehmer and Wu (2012), we test the statistical significance of the ban using a price delay measure that captures the speed with which asset prices reflect market-wide known information. In Table 2.7 we report the results for the regression of the Price Delay statistic described in Expression (2.8) following our DiD specification. Considering the previous evidence on the price adjustment in day 2, the analysis of the speed of with which publicly known information is incorporated into prices should help us assess whether the removal and the reintroduction of short sellers and its effect on prices is also be related to changes in the speed of price adjustments.

According to the estimates in Table 2.7, the treated stocks are significantly slower at adjusting price to public information once the ban is effective. On day 2, the effect continues to be statistically significant. By day 3, there is only weak evidence of differences between treated events and their counterfactuals. Days 4 and 5 interaction terms reveal no significant differences between groups. Overall, the results on price delay are consistent with the argument that short sellers possess valuable information about the future economic value of assets and that they are capable of processing information more efficiently. In the context of Rule 201, as we are limited to analyze only stocks that have had a substantial price shock, we show that removing short sellers could be detrimental for market participants via less accurate valuations and an increased volatility that aggravates the particular situation of such assets (for longer than the additional day in which the constraint is implemented).

In short, we identify a causal effect of Rule 201 short selling ban deferring information incorporation into prices until short sellers are allowed into the market. The return of short sellers is associated with a negative underperformance of the treated asset that drives the price closer to its true value. The analysis by stages reveals also that common risk factors cannot capture any degree of this effect. The analysis of Boehmer and Wu (2012) price delay in the days following the trigger reveal significantly less informative prices during days 1 and 2, with weak evidence on this delay being statistically different from the counterfactual. Given the implications that our results have, we now test their robustness in the next section to ensure that the estimates are reliable and can help in achieving insightful conclusions.

2.6 Robustness Checks

2.6.1 Alternative Sampling

The identification strategy and its reliability is essential for an experimental design as the one proposed in this article. We test for different bandwidths for the discontinuity in both restricting the sample or allowing a greater flexibility. A more restrictive approach does not seem to incorporate any advantages in terms of the parallel trends assumption for the DiD model, although it reduces dramatically the sample size inducing more noise in the estimations.

A greater flexibility on the contrary, increases sample size exponentially as well, but at the cost of a much smaller similarity and lower quality matches. We also test for different pairing mechanisms, including propensity scores or nearest-neighbor matching using fewer discriminant factors. Under a framework where the propensity is on the estimation of a given return, the noise on any of the classical prediction models yields weaker matches because of the difficulty in predicting the selection variable. To assess whether such an approach would have been a better alternative, we recreate our matched pairs with a propensity score calculated with a logistic regression of the probability that a given stock triggers Rule 201 conditional on the same set of observable factors that we control for.

When matching with propensity scores, stocks are paired with less representative counterparts, limiting the usefulness of the differences-in-differences methodology. A reduction in the number of discriminant factors does not affect the sampling significantly, although depending on the actual factors removed, the matches become less and less similar as well. We assess matching quality using our $PairScore$ measure defined in Equation 2.2.

In our original specification, the average pair score is of 8.23, with a minimum requirement of 7.5 to be included in the final sample. Either allowing greater flexibility (wider bandwidth) or being more restrictive shows a worsening of the research framework. When the bandwidth is reduced to only 0.5% above or below the threshold, the number of valid pairs plummets to less than 600, with no significant improvement on the pair score. When the bandwidth is enlarged, the number of candidate events increases, but the average pair score falls to 7.7, with a large number of pairs being discarded because of the minimum requirement of 7.5 for the inclusion in the final sample.

To complement the analysis, we estimate the same models of overpricing and price efficiency using these new two samples. As we should expect, the reduction of events caused by a more restrictive bandwidth yields lower goodness-of-fit metrics, while the usage of a larger bandwidth is related with more noisy estimates of the ban effects. Nevertheless, we find similar conclusions using either sample. We are confident that our bandwidth selection balances more efficiently the sample size (necessary for the power of the tests) and the quality of the matches (necessary to identify accurately the ban effect).

2.6.2 Placebo Tests

The second set of checks focuses on addressing an artificial discontinuity situation to verify that the estimates are not affected by any step in the design or the calculations and are actually attributable to the ban. To increase the power of this test, we repeat the same analysis for four different artificial discontinuity points, two above the original threshold of -10% and two below. The analysis is then repeated for the discontinuities of -2%, -6%, -14% and -18%. The ban is then artificially assigned to the stocks within a 1% bandwidth below the discontinuity. Observations within a 1% above the discontinuity act as the control pool of candidates in these placebo replications.

Then, we run our sampling mechanism and matched pairs construction, replicating the analysis of the results section with the newly defined treated and control samples. From these placebo tests, there is no evidence that the experimental design is affecting the final results in any stage. Both the results on overpricing and price efficiency are only observable in our discontinuity and associated to the ban, while they are not statistically significant in any of the four placebo samples. These placebo tests are reported in the Appendix.

2.6.3 Alternative Model Specification

The last of our robustness analyses tests whether we obtained results are consistent with changes in the model specifications and the inclusion of new or different definitions of controls. We first repeat the analysis for overpricing and price efficiency by including a new set of fixed effects: industry, days relative to trigger and monthly and weekly fixed effects. The inclusion of these new controls raises R^2 marginally, but the coefficients are mostly not statistically significant and those that are show an economically insignificant magnitude. This supports the validity of our empirical design, as most of the variation from unobservables is effectively captured by the matched pairs selection.

Also, in the overpricing tests, we repeat the analysis both by using either the CAPM or the Fama and French (1993) models to estimate the abnormal returns, to exclude potential endogeneity issues related with the momentum factor. When using either of these models, the ban effect is still statistically significant but estimated to be higher than in our reported results with the Carhart (1997) model. We prefer to be conservative and keep the control for the momentum factor to avoid any concern about the magnitude of the effect of the ban.

Finally, we include in the overpricing regressions controls for many fundamentals of the asset, such as trading volume or market capitalization. Both factors are deemed insignificant, as we would expect if the pairing algorithm is correct.[21] Results

[21] Given that we use both factors in our set of covariates for defining the similarity across neighbors, we should not expect differences across the event and the control group, but rather a constant factor that is already contained in the fixed effects we already include in our original specification.

are still robust to the inclusion of historical controls such as idiosyncratic volatility[22] or observed return volatility, both estimated in the pre-event window of days [-80,-41]. We also include a set of firm-specific fundamentals, mainly past month KPIs[23] such as ratios of profitability (RoE and RoA), solvency (Capitalization Ratio) or liquidity (Acid Test and Cash Ratio).

None of the alternative specifications, the inclusion of new control variables, nor the different sampling or the placebo tests offer any indication that the design of the experiment and the identification strategy generates a bias in the results, neither in its magnitude, direction or in their statistically significance or economic relevance.

2.7 Conclusion

In this paper we examine how the Rule 201 short lived restriction on short selling affects stock pricing in two principal dimensions: price efficiency and overpricing. We find evidence of the Rule 201 short selling ban supporting prices above their economic value, which is associated with Miller's (1977) overpricing theory. As the short selling restriction is repealed, short sellers drive the price down towards the true asset value. We argue that the removal of short sellers could defer the convergence of the stock's price and economic value, generating a sudden adaptation of prices when short sellers return to the market. This sudden adjustment would be associated with higher short-term volatility and distrust in the financial markets, which are two of the issues that the Rule 201 was designed specifically to target. Our results support this argumentation and highlight the importance of correctly assessing the pricing dynamics that surround the Rule 201 triggers and bans.

Our concerns are supported by our analysis of price efficiency. Using the Boehmer

[22] Calculated as the volatility of the residuals from the market model estimation.
[23] Sources: Compustat and Datastream.

and Wu (2012) specification we assess the speed with which assets incorporate publicly known information. Our tests for price delay show a statistically significant effect of the ban related to a delayed speed of price discovery for stocks subject to the ban. The effect is not limited only to the day during which short sellers are removed, revealing a sticky effect that lasts for up to three days after the actual trigger. This result raises questions about the true effectiveness of Rule 201 in improving the situation of stocks under distress.

Our event-study design of the Rule 201 ban, with a matched pairs analysis that allows for contemporaneous comparison is currently one of the largest empirical studies analyzing the issue of short selling bans, with a wider time window that prior studies using short lived one time bans such as the emergency ban of 2008 or the Russell 3000 pilot program lacked. We also show empirically the validity of the Rule 201 events as candidates for a quasi experimental study pointed out by H. Florindo (2020a). Our research design and robustness should be helpful for future research about the Rule 201 framework that contributes to a better understanding of the implications of the new regulation. Given the importance of the topic under question and the substantial degree of dimensions to analyze, we consider there is still a major field to be explored in this setting and we expect that our methodology offers an insightful guidance for future researchers in this framework.

The paper contributes to the literature in short selling bans in several aspects: first, by documenting a still significant overpricing effect for Rule 201 events and relating it with the classical Miller's (1977) framework and the superior informational efficiency of short sellers (Engelberg et al., 2012). As we find evidence on the ban being related with a delayed price discovery and slower rate of public information adjustment, these results contribute to the discussion about the actual benefits of Rule 201 in supporting prices or reducing investors' distrust on financial markets.

Figures

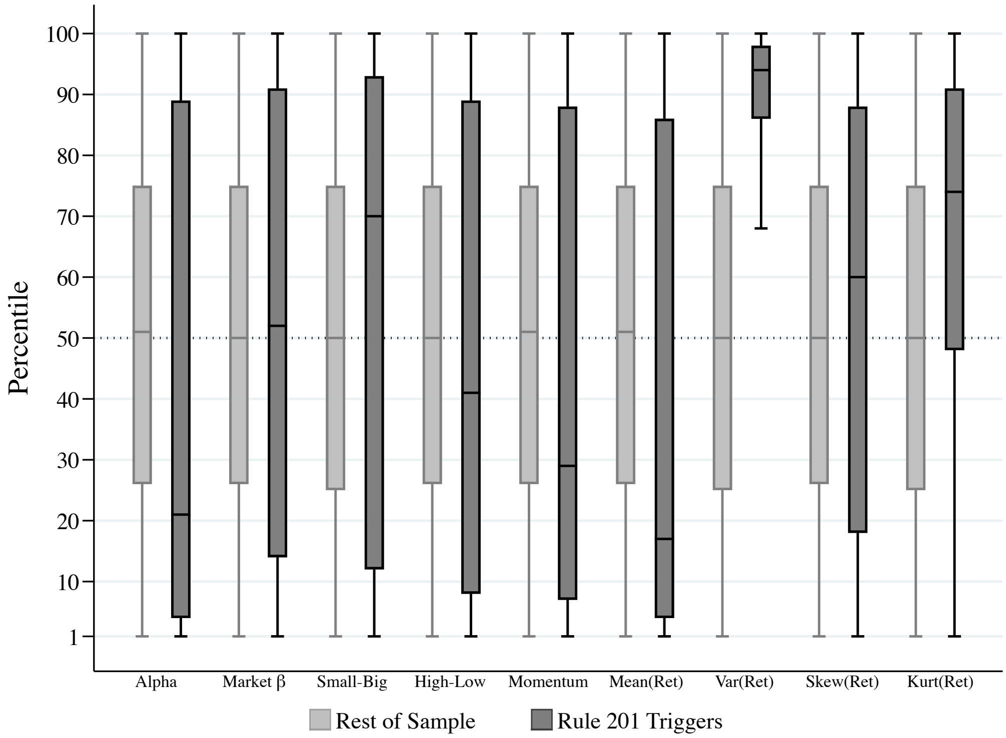

Figure 2.1 - Rule 201 Triggers vs. Rest of Stock Days

This graph compares the first four moments of the return distribution and the four market model factors in Carhart (1997) plus stock alpha for the stock-day observations that capture a circuit breaker trigger (in dark gray boxes) with respect to the rest of the population (the rest of stock-day observations without a trigger - in light gray boxes). The metrics are all calculated using a window of 40 days before each day following H. Florindo (2020a) approach. We present the results in terms of percentiles to visually compare across factors without issues related with the units of measure.

Tables

Table 2.1 - Proportion Differences Across Groups of Volatility

Group	Proportion	St. Error	95% Confidence Interval		
Least Volatile (1 - 10)	0.0082%	0.0009%	0.0064%	-	0.0099%
(11 - 25)	0.0309%	0.0014%	0.0281%	-	0.0336%
(26 - 50)	0.0540%	0.0014%	0.0512%	-	0.0568%
(51 - 75)	0.3368%	0.0036%	0.3298%	-	0.3438%
(76 - 90)	1.4013%	0.0094%	1.3829%	-	1.4196%
Most Volatile (91 - 100)	5.5113%	0.0222%	5.4677%	-	5.5549%

This table presents the results for the 95% confidence intervals for the sample proportion of stocks that trigger the Rule 201 circuit breaker in the different subsets of the sample. For each trading day in the sample we classify each stock according to the stock return volatility of that asset in the past 40 trading days. In parentheses we report the percentiles that form each group.

Table 2.2 - Sample Decomposition

Event Sample	N	As % of Total
Total Rule 201 Events	111582	100.00 %
Events in Required Bandwidth [-11%, -10%]	13561	12.15 %
Valid Events	8399	7.53 %
Events with Matched Counterpart	4265	3.82 %
Events With a Pair Score Higher Than 7.5	2900	2.60 %
Control Sample	N	As % of Total
Candidates in Required Bandwidth (-10%, -9%]	47539	100.00 %
Valid Candidates	29170	61.36 %
Candidates Matched to Events	4265	8.97 %
Candidates With a Pair Score Higher Than 7.5	2900	6.10 %

This table details the different steps undertaken in the sampling mechanism and the amount of events and controls that compose the final sample used in the estimations. ‘Valid Events’ and ‘Valid Candidates’ include only those observations from the pool of events that are not either thinly traded or too small, to avoid the influence of extremely small illiquid assets (less than 30 daily average trades and/or a stock price below $2) on our inference.

Table 2.3 - Matching Statistics

Factor	Events	Controls	Diff. (test)
Alpha (bps)	-0.10	0.20	-0.30
Market Beta	1.12	1.13	-0.01
SMB Beta	0.99	0.99	-0.01
HML Beta	-0.33	-0.35	0.01
MOM Beta	-0.50	-0.45	-0.06
Mean(Ret.) - (bps)	-0.61	-0.37	-0.24
SD(Ret.) - (bps)	417.51	406.10	11.41
Skew(Ret.)	0.24	0.25	-0.00
Kurt(Ret.)	5.27	5.28	-0.01
Market Cap. (Bn. USD)	1327.30	1288.14	39.15
Trading Volume (Bn. USD)	26.06	24.67	1.39
Stock Price	15.56	16.65	-1.10

This table summarizes the main matching statistics and test their differences across the event and control groups. In Column 3 we test the estimated difference between groups. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.4 - Bans and Returns

	(1)	(2)	(3)	(4)
Event x Day = 0	-55.445***	-54.886***	-54.916***	-54.582***
	(7.601)	(10.341)	(10.348)	(10.335)
Event x Day = 1	-3.998	-3.046	-4.250	-2.572
	(14.677)	(14.819)	(14.820)	(14.816)
Event x Day = 2	-34.523*	-34.577**	-35.036**	-34.994**
	(13.792)	(13.370)	(13.394)	(13.373)
Event x Day = 3	13.502	13.996	13.825	14.253
	(14.617)	(14.667)	(14.709)	(14.651)
Event x Day = 4	-5.202	-6.041	-6.043	-6.028
	(13.626)	(13.368)	(13.378)	(13.371)
Event x Day = 5	0.193	-0.288	-0.337	-0.257
	(13.673)	(13.482)	(13.509)	(13.480)
Controls				
Amount of Events	No	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	469736	465154	463948	465154
Num. of Groups	5800	5800	5800	5800
R-Squared (%)	2.3260	5.4765	5.4828	5.5015

This table presents the results for the DiD estimates of the Rule 201 ban effect on returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$Ret_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5}\{\gamma_\tau \cdot Day^\tau_{i,t} + \beta_\tau \cdot (Day^\tau_{i,t} \cdot Event_i)\}+$$
$$\eta \cdot LIQ_{i,t}\xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.5 - Bans and Abnormal Returns

	(1)	(2)	(3)	(4)	(5)	(6)
Event x Day = 0	-57.897***	-56.592***	-56.163***	-57.524***	-56.259***	-55.773***
	(9.824)	(9.690)	(9.801)	(9.818)	(9.683)	(9.795)
Event x Day = 1	-2.289	-0.366	-3.282	-1.068	1.003	-2.198
	(13.371)	(13.173)	(13.183)	(13.362)	(13.162)	(13.170)
Event x Day = 2	-35.407**	-35.003**	-36.905**	-35.419**	-35.215**	-37.082**
	(12.751)	(12.531)	(12.527)	(12.732)	(12.511)	(12.508)
Event x Day = 3	14.066	12.796	11.032	14.298	13.011	11.298
	(13.562)	(13.518)	(13.361)	(13.510)	(13.465)	(13.310)
Event x Day = 4	-7.641	-13.916	-11.486	-7.403	-13.626	-11.286
	(12.726)	(12.607)	(12.518)	(12.720)	(12.600)	(12.514)
Event x Day = 5	1.907	5.376	5.189	1.849	5.366	5.147
	(12.619)	(12.394)	(12.374)	(12.592)	(12.367)	(12.346)
Controls						
NTRG	Yes	Yes	Yes	Yes	Yes	Yes
LIQ	Amihud	Amihud	Amihud	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Standard Error	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Model	CAPM	FF3	Carhart	CAPM	FF3	Carhart
Num. of Obs.	461955	461955	461955	463154	463154	463154
Num. of Groups	5800	5800	5800	5800	5800	5800
R-Squared (%)	1.8117	1.4121	1.2262	1.8309	1.4321	1.2444

This table presents the results for the DiD estimates of the Rule 201 ban effect on abnormal returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$AbRet_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5}\{\gamma_\tau \cdot Day_{i,t}^{\tau} + \beta_\tau \cdot (Day_{i,t}^{\tau} \cdot Event_i)\} + \\ \eta \cdot LIQ_{i,t}\xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Abnormal returns *AbRet* are defined as the difference between the observed return and the estimated one based on an out-of-sample estimation of either capital asset pricing model (CAPM), Fama and French (1993) three factor (FF3) or Carhart (1997) four factor model. We estimate each of these three models for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. Columns (1), (2) and (3) estimates the effect on abnormal returns controlling for liquidity via the Amihud and Mendelson (1986) illiquidity measure. Columns (4), (5) and (6) estimates the model controlling for liquidity via the bid-ask spread. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.6 - Standardized Effects

	(1)	(2)	(3)	(4)
Event x Day = 0	-0.077**	-0.132**	-0.144***	-0.151***
	(0.029)	(0.041)	(0.042)	(0.044)
Event x Day = 1	-0.013	-0.012	-0.004	-0.019
	(0.033)	(0.043)	(0.045)	(0.046)
Event x Day = 2	-0.082**	-0.112**	-0.123**	-0.131**
	(0.030)	(0.041)	(0.043)	(0.044)
Event x Day = 3	0.011	0.024	0.031	0.023
	(0.030)	(0.042)	(0.044)	(0.045)
Event x Day = 4	-0.019	-0.041	-0.061	-0.058
	(0.027)	(0.039)	(0.040)	(0.041)
Event x Day = 5	0.003	0.010	0.017	0.024
	(0.028)	(0.039)	(0.040)	(0.041)
Controls				
NTRG	Yes	Yes	Yes	Yes
LIQ	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	465154	463154	463154	463154
Num. of Groups	5800	5800	5800	5800
R-Squared (%)	7.4307	2.2731	1.9320	1.7260

This table presents the results for the DiD estimates of the Rule 201 ban effect on the standardized measures of overpricing. The unit of measure are deviations from the standard deviation of either returns or abnormal returns. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$StOP_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day_{i,t}^{\tau} + \beta_\tau \cdot (Day_{i,t}^{\tau} \cdot Event_i)\} + \\ \eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Our variable of interest is our measure for standardized overpricing as defined in Expression (2.3). The table reports the standardized return in Column (1) whereas standardized abnormal returns estimated from CAPM, FF3 and Carhart, are reported in Columns (2) to (4), respectively. Return volatility is obtained from the out-of-sample return distribution between days -80 to -41. For the market models, we estimate each of them for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. All estimations include the bid-ask spread to control for liquidity as well as the total number of records (NTRG). Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.7 - Bans and Price Delay: Boehmer and Wu (2012)

	(1)	(2)	(3)	(4)
Event x Day = 0	0.009	0.009	0.009	0.009
	(0.006)	(0.006)	(0.006)	(0.006)
Event x Day = 1	0.013*	0.013*	0.013*	0.013*
	(0.006)	(0.006)	(0.006)	(0.006)
Event x Day = 2	0.015*	0.015*	0.015*	0.015*
	(0.006)	(0.006)	(0.006)	(0.006)
Event x Day = 3	0.011†	0.010†	0.010†	0.010†
	(0.006)	(0.006)	(0.006)	(0.006)
Event x Day = 4	0.007	0.008	0.007	0.007
	(0.006)	(0.006)	(0.006)	(0.006)
Event x Day = 5	0.007	0.007	0.006	0.007
	(0.006)	(0.006)	(0.006)	(0.006)
Controls				
Amount of Events	Yes	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	121796	120722	120454	120712
Num. of Groups	5800	5800	5800	5800
R-Squared (%)	0.0898	0.1535	0.1509	0.1547

This table presents the results for the DiD estimates of the Rule 201 ban effect on Boehmer and Wu (2012) price delay measure. (2.8). The base model is that of expression 2.4. The specific model tested is that of (2.9) with parameters $\omega = 0$ and $T = 5$:

$$PriceDelay_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} +$$
$$\eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively. A † symbol represents weak statistical significance (the estimate has a p-value below 0.1 but larger than 0.05).

Appendix

A. Placebo Tests

The following tables repeat the exact same analysis as in Tables 2.4, 2.5, 2.6 and 2.7 using new samples defined by four artificial thresholds: -2%, -6%, -14% and -18%. As in the original identification procedure, we retain only observations that lie in the bandwidth of 1% around the threshold and repeat the sampling, matching and estimations to ensure that the methodology is not biasing nor causing spurious results. Overall, the reported coefficients are statistically insignificant in the interaction terms where we find our main results in all of the placebo tests, supporting the robustness of the original estimations.

Threshold = -2%

Table 2.8 - Bans and Returns - Placebo (-2%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-72.669***	-73.242***	-73.140***	-73.435***
	(5.799)	(5.836)	(5.880)	(5.830)
Event x Day = 1	-13.160	-14.817	-15.645	-14.716
	(11.554)	(11.273)	(11.315)	(11.271)
Event x Day = 2	-1.609	-0.709	-0.143	-0.700
	(11.257)	(11.324)	(11.370)	(11.325)
Event x Day = 3	7.146	7.860	8.720	7.697
	(11.903)	(11.366)	(11.420)	(11.367)
Event x Day = 4	11.357	12.144	11.823	12.060
	(11.959)	(11.878)	(11.934)	(11.873)
Event x Day = 5	6.420	8.303	8.675	8.210
	(12.298)	(11.961)	(12.024)	(11.964)
Controls				
Amount of Events	No	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	115181	113259	112452	113259
Num. of Groups	1422	1422	1422	1422
R-Squared (%)	0.2684	3.8389	3.8778	3.8455

This table presents the results for the DiD estimates of the Rule 201 ban effect on returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$Ret_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day_{i,t}^{\tau} + \beta_\tau \cdot (Day_{i,t}^{\tau} \cdot Event_i)\} + \eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.9 - Bans and Abnormal Returns - Placebo (-2%)

	(1)	(2)	(3)	(4)	(5)	(6)
Event x Day = 0	-65.876***	-64.578***	-65.876***	-66.113***	-65.159***	-66.467***
	(5.918)	(5.907)	(6.045)	(5.861)	(5.853)	(5.992)
Event x Day = 1	-12.208	-15.046	-14.074	-11.548	-14.434	-13.506
	(9.546)	(9.588)	(9.673)	(9.515)	(9.557)	(9.641)
Event x Day = 2	-4.115	-1.374	-1.992	-5.275	-2.644	-3.375
	(9.935)	(9.780)	(9.884)	(9.899)	(9.748)	(9.855)
Event x Day = 3	9.467	13.129	12.532	8.363	11.708	11.351
	(10.342)	(10.014)	(10.134)	(10.288)	(9.965)	(10.073)
Event x Day = 4	9.130	9.880	11.493	8.927	9.930	11.541
	(10.315)	(10.220)	(10.388)	(10.257)	(10.157)	(10.324)
Event x Day = 5	8.143	8.854	8.361	8.414	8.996	8.347
	(10.624)	(10.432)	(10.567)	(10.562)	(10.371)	(10.506)
Controls						
NTRG	Yes	Yes	Yes	Yes	Yes	Yes
LIQ	Amihud	Amihud	Amihud	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Standard Error	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Model	CAPM	FF3	Carhart	CAPM	FF3	Carhart
Num. of Obs.	112245	112245	112245	113037	113037	113037
Num. of Groups	1422	1422	1422	1422	1422	1422
R-Squared (%)	0.2092	0.1515	0.1653	0.2084	0.1518	0.1655

This table presents the results for the DiD estimates of the Rule 201 ban effect on abnormal returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$AbRet_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5}\{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\}+$$
$$\eta \cdot LIQ_{i,t}\xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Abnormal returns *AbRet* are defined as the difference between the observed return and the estimated one based on an out-of-sample estimation of either capital asset pricing model (CAPM), Fama and French (1993) three factor (FF3) or Carhart (1997) four factor model. We estimate each of these three models for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. Columns (1), (2) and (3) estimates the effect on abnormal returns controlling for liquidity via the Amihud and Mendelson (1986) illiquidity measure. Columns (4), (5) and (6) estimates the model controlling for liquidity via the bid-ask spread. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.10 - Standardized Effects - Placebo (-2%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-0.413***	-0.519***	-0.536***	-0.552***
	(0.041)	(0.050)	(0.053)	(0.054)
Event x Day = 1	-0.109*	-0.120†	-0.151*	-0.143*
	(0.054)	(0.062)	(0.065)	(0.067)
Event x Day = 2	0.041	-0.039	-0.022	-0.021
	(0.056)	(0.067)	(0.070)	(0.072)
Event x Day = 3	0.037	0.047	0.060	0.038
	(0.056)	(0.072)	(0.073)	(0.076)
Event x Day = 4	0.080	0.070	0.073	0.074
	(0.058)	(0.075)	(0.079)	(0.081)
Event x Day = 5	0.042	0.031	0.051	0.038
	(0.056)	(0.080)	(0.084)	(0.087)
Controls				
NTRG	Yes	Yes	Yes	Yes
LIQ	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	113259	113037	113037	113037
Num. of Groups	1422	1422	1422	1422
R-Squared (%)	4.8195	0.4395	0.3364	0.3698

This table presents the results for the DiD estimates of the Rule 201 ban effect on the standardized measures of overpricing. The unit of measure are deviations from the standard deviation of either returns or abnormal returns. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$StOP_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5}\{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} + \\ \eta \cdot LIQ_{i,t}\xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Our variable of interest is our measure for standardized overpricing as defined in Expression (2.3). The table reports the standardized return in Column (1) whereas standardized abnormal returns estimated from CAPM, FF3 and Carhart, are reported in Columns (2) to (4), respectively. Return volatility is obtained from the out-of-sample return distribution between days -80 to -41. For the market models, we estimate each of them for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. All estimations include the bid-ask spread to control for liquidity as well as the total number of records (NTRG). Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.11 - Bans and Price Delay: Boehmer and Wu (2012) - Placebo (-2%)

	(1)	(2)	(3)	(4)
Event x Day = 0	0.012	0.012	0.012	0.012
	(0.011)	(0.011)	(0.011)	(0.011)
Event x Day = 1	0.006	0.007	0.007	0.007
	(0.012)	(0.012)	(0.012)	(0.012)
Event x Day = 2	-0.006	-0.006	-0.006	-0.006
	(0.012)	(0.012)	(0.012)	(0.012)
Event x Day = 3	-0.014	-0.015	-0.014	-0.015
	(0.012)	(0.012)	(0.013)	(0.012)
Event x Day = 4	-0.021†	-0.021†	-0.022†	-0.021†
	(0.012)	(0.012)	(0.012)	(0.012)
Event x Day = 5	-0.001	-0.002	-0.003	-0.002
	(0.012)	(0.012)	(0.012)	(0.012)
Controls				
Amount of Events	Yes	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	29859	29435	29226	29434
Num. of Groups	1422	1422	1422	1422
R-Squared (%)	0.0785	0.1615	0.1691	0.1634

This table presents the results for the DiD estimates of the Rule 201 ban effect on Boehmer and Wu (2012) price delay measure. (2.8). The base model is that of expression 2.4. The specific model tested is that of (2.9) with parameters $\omega = 0$ and $T = 5$:

$$PriceDelay_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5}\{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\}+$$
$$\eta \cdot LIQ_{i,t}\xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively. A † symbol represents weak statistical significance (the estimate has a p-value below 0.1 but larger than 0.05).

Threshold = -6%

Table 2.12 - Bans and Returns - Placebo (-6%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-78.821***	-79.690***	-80.824***	-79.738***
	(10.542)	(12.936)	(13.000)	(12.937)
Event x Day = 1	-0.489	0.234	-0.452	0.221
	(19.562)	(19.110)	(19.148)	(19.107)
Event x Day = 2	-10.817	-10.855	-11.031	-10.857
	(21.347)	(20.928)	(20.945)	(20.928)
Event x Day = 3	-2.897	-5.597	-5.345	-5.623
	(20.211)	(20.445)	(20.468)	(20.443)
Event x Day = 4	-9.092	-7.405	-6.913	-7.408
	(17.466)	(17.285)	(17.322)	(17.285)
Event x Day = 5	-19.201	-20.759	-20.669	-20.788
	(20.842)	(20.751)	(20.824)	(20.752)
Controls				
Amount of Events	No	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	129600	127636	127243	127636
Num. of Groups	1600	1600	1600	1600
R-Squared (%)	1.2219	4.7880	4.7882	4.7890

This table presents the results for the DiD estimates of the Rule 201 ban effect on returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$Ret_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day^\tau_{i,t} + \beta_\tau \cdot (Day^\tau_{i,t} \cdot Event_i)\} +$$
$$\eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.13 - Bans and Abnormal Returns - Placebo (-6%)

	(1)	(2)	(3)	(4)	(5)	(6)
Event x Day = 0	-76.459***	-75.779***	-73.118***	-75.742***	-75.003***	-72.233***
	(11.846)	(11.748)	(12.199)	(11.790)	(11.703)	(12.152)
Event x Day = 1	-1.428	7.948	14.883	-0.723	8.630	15.553
	(17.136)	(17.055)	(17.118)	(17.102)	(17.023)	(17.085)
Event x Day = 2	-10.785	-10.552	-5.190	-10.712	-10.662	-5.335
	(18.156)	(17.689)	(17.886)	(18.139)	(17.675)	(17.872)
Event x Day = 3	-16.660	-7.381	-11.272	-16.869	-7.370	-11.158
	(18.386)	(18.135)	(17.982)	(18.366)	(18.103)	(17.952)
Event x Day = 4	-6.270	-16.000	-17.003	-6.703	-16.145	-17.068
	(15.267)	(15.353)	(15.490)	(15.235)	(15.318)	(15.453)
Event x Day = 5	-24.012	-14.724	-17.847	-24.070	-14.907	-18.044
	(19.133)	(18.952)	(19.157)	(19.067)	(18.887)	(19.090)
Controls						
NTRG	Yes	Yes	Yes	Yes	Yes	Yes
LIQ	Amihud	Amihud	Amihud	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Standard Error	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Model	CAPM	FF3	Carhart	CAPM	FF3	Carhart
Num. of Obs.	126926	126926	126926	127319	127319	127319
Num. of Groups	1600	1600	1600	1600	1600	1600
R-Squared (%)	0.6612	0.5120	0.4406	0.6615	0.5128	0.4385

This table presents the results for the DiD estimates of the Rule 201 ban effect on abnormal returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$AbRet_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} + \eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Abnormal returns *AbRet* are defined as the difference between the observed return and the estimated one based on an out-of-sample estimation of either capital asset pricing model (CAPM), Fama and French (1993) three factor (FF3) or Carhart (1997) four factor model. We estimate each of these three models for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. Columns (1), (2) and (3) estimates the effect on abnormal returns controlling for liquidity via the Amihud and Mendelson (1986) illiquidity measure. Columns (4), (5) and (6) estimates the model controlling for liquidity via the bid-ask spread. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.14 - Standardized Effects - Placebo (-6%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-0.157**	-0.275***	-0.315***	-0.300***
	(0.048)	(0.061)	(0.063)	(0.068)
Event x Day = 1	-0.002	0.025	0.066	0.106
	(0.056)	(0.077)	(0.081)	(0.083)
Event x Day = 2	-0.016	-0.025	-0.040	-0.022
	(0.058)	(0.070)	(0.074)	(0.078)
Event x Day = 3	-0.016	-0.030	0.016	0.008
	(0.056)	(0.086)	(0.090)	(0.092)
Event x Day = 4	-0.006	0.058	0.013	-0.005
	(0.049)	(0.065)	(0.067)	(0.070)
Event x Day = 5	-0.064	-0.116†	-0.082	-0.087
	(0.052)	(0.070)	(0.077)	(0.079)
Controls				
NTRG	Yes	Yes	Yes	Yes
LIQ	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	127636	127319	127319	127319
Num. of Groups	1600	1600	1600	1600
R-Squared (%)	6.8152	0.8630	0.7298	0.6735

This table presents the results for the DiD estimates of the Rule 201 ban effect on the standardized measures of overpricing. The unit of measure are deviations from the standard deviation of either returns or abnormal returns. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$StOP_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5}\{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\}+$$
$$\eta \cdot LIQ_{i,t}\xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Our variable of interest is our measure for standardized overpricing as defined in Expression (2.3). The table reports the standardized return in Column (1) whereas standardized abnormal returns estimated from CAPM, FF3 and Carhart, are reported in Columns (2) to (4), respectively. Return volatility is obtained from the out-of-sample return distribution between days -80 to -41. For the market models, we estimate each of them for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. All estimations include the bid-ask spread to control for liquidity as well as the total number of records (NTRG). Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.15 - Bans and Price Delay: Boehmer and Wu (2012) - Placebo (-6%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-0.005	-0.005	-0.005	-0.005
	(0.017)	(0.017)	(0.017)	(0.017)
Event x Day = 1	0.002	0.003	0.002	0.003
	(0.010)	(0.010)	(0.010)	(0.010)
Event x Day = 2	-0.009	-0.009	-0.009	-0.009
	(0.011)	(0.011)	(0.011)	(0.011)
Event x Day = 3	-0.015	-0.016	-0.016	-0.016
	(0.010)	(0.010)	(0.010)	(0.010)
Event x Day = 4	-0.009	-0.009	-0.009	-0.009
	(0.010)	(0.010)	(0.010)	(0.010)
Event x Day = 5	-0.013	-0.013	-0.013	-0.013
	(0.010)	(0.010)	(0.010)	(0.010)
Controls				
Amount of Events	Yes	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	33600	33158	33061	33158
Num. of Groups	1600	1600	1600	1600
R-Squared (%)	0.1561	0.1619	0.1679	0.1759

This table presents the results for the DiD estimates of the Rule 201 ban effect on Boehmer and Wu (2012) price delay measure. (2.8). The base model is that of expression 2.4. The specific model tested is that of (2.9) with parameters $\omega = 0$ and $T = 5$:

$$PriceDelay_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} +$$
$$\eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively. A † symbol represents weak statistical significance (the estimate has a p-value below 0.1 but larger than 0.05).

Threshold = -14%

Table 2.16 - Bans and Returns - Placebo (-14%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-78.994***	-80.968***	-81.085***	-81.323***
	(16.071)	(23.951)	(23.950)	(23.984)
Event x Day = 1	73.529*	75.554†	75.930*	74.937†
	(37.185)	(38.627)	(38.672)	(38.615)
Event x Day = 2	5.588	9.097	10.844	9.731
	(29.705)	(28.011)	(28.052)	(27.992)
Event x Day = 3	-3.390	-3.799	-5.018	-4.377
	(27.285)	(28.022)	(28.039)	(28.027)
Event x Day = 4	5.273	4.180	4.212	4.315
	(28.185)	(27.974)	(28.039)	(27.938)
Event x Day = 5	-58.532*	-57.129*	-56.662*	-58.043*
	(27.898)	(27.972)	(28.002)	(27.979)
Controls				
Amount of Events	No	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	140451	139215	138732	139215
Num. of Groups	1734	1734	1734	1734
R-Squared (%)	4.2651	6.4252	6.4314	6.4764

This table presents the results for the DiD estimates of the Rule 201 ban effect on returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$Ret_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day_{i,t}^{\tau} + \beta_\tau \cdot (Day_{i,t}^{\tau} \cdot Event_i)\} +$$
$$\eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.17 - Bans and Abnormal Returns - Placebo (-14%)

	(1)	(2)	(3)	(4)	(5)	(6)
Event x Day = 0	-76.935**	-74.298**	-75.732***	-77.059**	-74.372**	-75.861***
	(23.530)	(22.605)	(22.557)	(23.556)	(22.631)	(22.576)
Event x Day = 1	75.380*	71.632*	73.287*	74.836*	71.260*	72.671*
	(33.398)	(33.074)	(32.915)	(33.360)	(33.040)	(32.874)
Event x Day = 2	3.895	-4.259	-1.204	4.556	-3.027	-0.208
	(25.801)	(25.586)	(26.000)	(25.742)	(25.521)	(25.932)
Event x Day = 3	-3.889	-12.604	-15.171	-3.628	-12.821	-15.381
	(24.033)	(24.116)	(23.923)	(24.022)	(24.110)	(23.914)
Event x Day = 4	-1.020	-10.416	-17.697	-1.361	-10.593	-18.190
	(26.240)	(25.377)	(25.351)	(26.144)	(25.278)	(25.252)
Event x Day = 5	-58.642*	-55.310*	-59.323*	-59.614*	-56.232*	-60.336*
	(24.598)	(24.005)	(23.817)	(24.582)	(23.984)	(23.799)
Controls						
NTRG	Yes	Yes	Yes	Yes	Yes	Yes
LIQ	Amihud	Amihud	Amihud	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Standard Error	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Model	CAPM	FF3	Carhart	CAPM	FF3	Carhart
Num. of Obs.	138184	138184	138184	138647	138647	138647
Num. of Groups	1734	1734	1734	1734	1734	1734
R-Squared (%)	2.3319	2.0728	1.9750	2.3800	2.1157	2.0161

This table presents the results for the DiD estimates of the Rule 201 ban effect on abnormal returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$AbRet_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} + \\ \eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Abnormal returns *AbRet* are defined as the difference between the observed return and the estimated one based on an out-of-sample estimation of either capital asset pricing model (CAPM), Fama and French (1993) three factor (FF3) or Carhart (1997) four factor model. We estimate each of these three models for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. Columns (1), (2) and (3) estimates the effect on abnormal returns controlling for liquidity via the Amihud and Mendelson (1986) illiquidity measure. Columns (4), (5) and (6) estimates the model controlling for liquidity via the bid-ask spread. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.18 - Standardized Effects - Placebo (-14%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-0.060	-0.194†	-0.180	-0.178
	(0.064)	(0.115)	(0.120)	(0.124)
Event x Day = 1	0.111	0.274*	0.252*	0.291*
	(0.077)	(0.120)	(0.125)	(0.126)
Event x Day = 2	0.068	-0.016	-0.038	-0.008
	(0.058)	(0.096)	(0.099)	(0.103)
Event x Day = 3	-0.032	0.043	0.007	0.005
	(0.058)	(0.081)	(0.085)	(0.088)
Event x Day = 4	0.003	-0.030	-0.055	-0.087
	(0.051)	(0.091)	(0.094)	(0.096)
Event x Day = 5	-0.124*	-0.155†	-0.153†	-0.174*
	(0.055)	(0.079)	(0.082)	(0.085)
Controls				
NTRG	Yes	Yes	Yes	Yes
LIQ	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	139215	138647	138647	138647
Num. of Groups	1734	1734	1734	1734
R-Squared (%)	9.1609	3.0180	2.8537	2.7688

This table presents the results for the DiD estimates of the Rule 201 ban effect on the standardized measures of overpricing. The unit of measure are deviations from the standard deviation of either returns or abnormal returns. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$StOP_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} + \\ \eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Our variable of interest is our measure for standardized overpricing as defined in Expression (2.3). The table reports the standardized return in Column (1) whereas standardized abnormal returns estimated from CAPM, FF3 and Carhart, are reported in Columns (2) to (4), respectively. Return volatility is obtained from the out-of-sample return distribution between days -80 to -41. For the market models, we estimate each of them for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. All estimations include the bid-ask spread to control for liquidity as well as the total number of records (NTRG). Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.19 - Bans and Price Delay: Boehmer and Wu (2012) - Placebo (-14%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-0.003	-0.004	-0.004	-0.004
	(0.010)	(0.010)	(0.010)	(0.010)
Event x Day = 1	-0.002	-0.003	-0.003	-0.003
	(0.011)	(0.011)	(0.011)	(0.011)
Event x Day = 2	-0.010	-0.010	-0.011	-0.011
	(0.010)	(0.010)	(0.010)	(0.010)
Event x Day = 3	-0.000	-0.001	-0.001	-0.001
	(0.010)	(0.010)	(0.010)	(0.010)
Event x Day = 4	-0.005	-0.005	-0.006	-0.005
	(0.010)	(0.010)	(0.010)	(0.010)
Event x Day = 5	-0.006	-0.007	-0.007	-0.007
	(0.010)	(0.010)	(0.010)	(0.010)
Controls				
Amount of Events	Yes	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	36414	36110	36008	36110
Num. of Groups	1734	1734	1734	1734
R-Squared (%)	0.0521	0.1098	0.1103	0.1329

This table presents the results for the DiD estimates of the Rule 201 ban effect on Boehmer and Wu (2012) price delay measure. (2.8). The base model is that of expression 2.4. The specific model tested is that of (2.9) with parameters $\omega = 0$ and $T = 5$:

$$PriceDelay_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5}\{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\}+$$
$$\eta \cdot LIQ_{i,t}\xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively. A † symbol represents weak statistical significance (the estimate has a p-value below 0.1 but larger than 0.05).

Threshold = -18%

Table 2.20 - Bans and Returns - Placebo (-18%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-21.837	-22.139	-22.254	-21.984
	(44.475)	(76.211)	(76.213)	(76.184)
Event x Day = 1	-51.651	-51.953	-52.076	-51.327
	(72.473)	(75.649)	(75.658)	(75.633)
Event x Day = 2	-25.888	-26.190	-26.112	-26.367
	(82.055)	(77.843)	(78.084)	(77.834)
Event x Day = 3	108.19	107.89	107.77	107.97
	(74.889)	(76.418)	(76.422)	(76.401)
Event x Day = 4	-105.37	-105.67	-105.79	-105.38
	(64.497)	(64.916)	(64.921)	(64.885)
Event x Day = 5	-66.123	-66.425	-66.551	-65.836
	(52.474)	(52.747)	(52.748)	(52.808)
Controls				
Amount of Events	No	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	28188	28024	27971	28024
Num. of Groups	348	348	348	348
R-Squared (%)	6.6038	9.1872	9.1904	9.1914

This table presents the results for the DiD estimates of the Rule 201 ban effect on returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$Ret_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} + \\ \eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.21 - Bans and Abnormal Returns - Placebo (-18%)

	(1)	(2)	(3)	(4)	(5)	(6)
Event x Day = 0	10.582	-19.680	2.440	10.732	-19.677	2.442
	(64.277)	(63.352)	(62.870)	(64.258)	(63.346)	(62.876)
Event x Day = 1	-39.588	-52.661	-26.081	-39.126	-52.612	-26.260
	(60.814)	(59.787)	(58.639)	(60.789)	(59.768)	(58.595)
Event x Day = 2	-45.524	-27.261	-8.035	-45.752	-27.743	-8.351
	(71.528)	(70.119)	(72.350)	(71.320)	(69.882)	(72.096)
Event x Day = 3	101.26	104.19	$113.81^{\dagger}$	101.37	104.19	$113.85^{\dagger}$
	(66.969)	(66.097)	(65.555)	(66.971)	(66.113)	(65.591)
Event x Day = 4	$-110.64^{\dagger}$	$-107.31^{\dagger}$	$-101.41^{\dagger}$	$-110.40^{\dagger}$	$-107.29^{\dagger}$	$-101.46^{\dagger}$
	(62.551)	(60.353)	(59.545)	(62.527)	(60.345)	(59.534)
Event x Day = 5	$-82.496^{\dagger}$	$-76.135^{\dagger}$	-98.174^{*}	$-82.053^{\dagger}$	$-76.085^{\dagger}$	-98.335^{*}
	(45.313)	(45.103)	(49.316)	(45.362)	(45.148)	(49.369)
Controls						
NTRG	Yes	Yes	Yes	Yes	Yes	Yes
LIQ	Amihud	Amihud	Amihud	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Standard Error	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Model	CAPM	FF3	Carhart	CAPM	FF3	Carhart
Num. of Obs.	27820	27820	27820	27873	27873	27873
Num. of Groups	348	348	348	348	348	348
R-Squared (%)	4.1329	4.0158	3.8817	4.1313	4.0129	3.8789

This table presents the results for the DiD estimates of the Rule 201 ban effect on abnormal returns measured in basis points. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$AbRet_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} + \\ \eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Abnormal returns *AbRet* are defined as the difference between the observed return and the estimated one based on an out-of-sample estimation of either capital asset pricing model (CAPM), Fama and French (1993) three factor (FF3) or Carhart (1997) four factor model. We estimate each of these three models for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. Columns (1), (2) and (3) estimates the effect on abnormal returns controlling for liquidity via the Amihud and Mendelson (1986) illiquidity measure. Columns (4), (5) and (6) estimates the model controlling for liquidity via the bid-ask spread. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.22 - Standardized Effects - Placebo (-18%)

	(1)	(2)	(3)	(4)
Event x Day = 0	0.005	0.248	0.172	0.262
	(0.184)	(0.324)	(0.344)	(0.355)
Event x Day = 1	-0.115	-0.111	-0.178	-0.123
	(0.161)	(0.246)	(0.256)	(0.262)
Event x Day = 2	-0.083	-0.274	-0.197	-0.122
	(0.141)	(0.238)	(0.240)	(0.254)
Event x Day = 3	0.048	0.241	0.295	0.346
	(0.139)	(0.212)	(0.211)	(0.212)
Event x Day = 4	-0.124	-0.369*	-0.368*	-0.396*
	(0.117)	(0.183)	(0.184)	(0.189)
Event x Day = 5	-0.117	-0.182	-0.141	-0.139
	(0.101)	(0.145)	(0.158)	(0.172)
Controls				
NTRG	Yes	Yes	Yes	Yes
LIQ	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	28024	27873	27873	27873
Num. of Groups	348	348	348	348
R-Squared (%)	12.0089	5.3043	5.3257	5.2453

This table presents the results for the DiD estimates of the Rule 201 ban effect on the standardized measures of overpricing. The unit of measure are deviations from the standard deviation of either returns or abnormal returns. The base model is that of expression 2.4. The specific model tested is that of (2.4) with parameters $\omega = 0$ and $T = 5$:

$$StOP_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5} \{\gamma_\tau \cdot Day^{\tau}_{i,t} + \beta_\tau \cdot (Day^{\tau}_{i,t} \cdot Event_i)\} + \eta \cdot LIQ_{i,t} \xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Our variable of interest is our measure for standardized overpricing as defined in Expression (2.3). The table reports the standardized return in Column (1) whereas standardized abnormal returns estimated from CAPM, FF3 and Carhart, are reported in Columns (2) to (4), respectively. Return volatility is obtained from the out-of-sample return distribution between days -80 to -41. For the market models, we estimate each of them for every event and control stock during the period -80 to -41 in terms of relative days with respect to the stock. All estimations include the bid-ask spread to control for liquidity as well as the total number of records (NTRG). Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively.

Table 2.23 - Bans and Price Delay: Boehmer and Wu (2012) - Placebo (-18%)

	(1)	(2)	(3)	(4)
Event x Day = 0	-0.002	-0.002	-0.002	-0.002
	(0.026)	(0.026)	(0.026)	(0.026)
Event x Day = 1	-0.005	-0.005	-0.005	-0.006
	(0.025)	(0.025)	(0.025)	(0.025)
Event x Day = 2	-0.031	-0.031	-0.031	-0.031
	(0.023)	(0.023)	(0.023)	(0.023)
Event x Day = 3	-0.019	-0.019	-0.020	-0.020
	(0.022)	(0.022)	(0.022)	(0.022)
Event x Day = 4	-0.029	-0.029	-0.029	-0.029
	(0.023)	(0.023)	(0.023)	(0.023)
Event x Day = 5	-0.013	-0.013	-0.013	-0.014
	(0.022)	(0.022)	(0.022)	(0.022)
Controls				
Amount of Events	Yes	Yes	Yes	Yes
Liquidity	No	No	Amihud	Bid-Ask Sp.
Stock FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
St. Errors	Cluster	Cluster	Cluster	Cluster
Num. of Obs.	7308	7288	7275	7288
Num. of Groups	348	348	348	348
R-Squared (%)	0.1371	0.1483	0.1703	0.1959

This table presents the results for the DiD estimates of the Rule 201 ban effect on Boehmer and Wu (2012) price delay measure. (2.8). The base model is that of expression 2.4. The specific model tested is that of (2.9) with parameters $\omega = 0$ and $T = 5$:

$$PriceDelay_{i,t} = \alpha + \phi \cdot Event_i + \sum_{\tau=0}^{5}\{\gamma_\tau \cdot Day_{i,t}^{\tau} + \beta_\tau \cdot (Day_{i,t}^{\tau} \cdot Event_i)\} + \\ \eta \cdot LIQ_{i,t}\xi \cdot NTRG_t + \lambda_t + \nu_i + \epsilon_{i,t}$$

Column (1) estimates the model without controlling for liquidity nor the amount of triggers. Column (2) includes the control for the total number of Rule 201 triggers ($NTRG_t$) recorded at each day t. Columns (4) and (5) estimate the full model described in (2.4) with the liquidity control of Amihud (2002) illiquidity measure and the closing bid-ask spread, respectively. Two way clustered by stock-date standard errors are presented in parentheses. One, two or three stars represent statistical significance at the 5%, 1% and 0.1% respectively. A † symbol represents weak statistical significance (the estimate has a p-value below 0.1 but larger than 0.05).

Chapter 3

The Activation of Short Selling Restrictions: Intraday Effects of the SEC Rule 201

3.1 Introduction

Short sale restrictions have a long history and are a significant point of contention in popular debates over stock market regulation, specially at times of stock market price declines. The U.S. has experienced several rounds of regulation of short sales, and we are interested in exploring the dynamic effects of the current regulation at a granular intraday level. The current regulation is significantly different from the type of short sale restrictions (bans) that have been imposed in the past. In particular, the current regulation imposes a short sale ban only on aggressive short sales and comes into effect only after a stock suffers a significant price drop (10% from the previous day's closing price). The short selling restrictions affect only that stock, and for the rest of the day and the subsequent 24 hours. This temporary, endogenously triggered regulation does not ban all short-sales, but only "aggressive" short sales, those at (or below) the current bid price, and applies to all but a handful of exempt types of short sales. In contrast, the most commonly observed bans in the past have been bans on all short sales, imposed for a prolonged period of time, and on a significant fraction, if not all, traded stocks.

Our analysis splits the effect into an overall and a local effect. With the local effect we look at the effects within a window of five minutes before and after the minute in which the restrictions are activated, while the overall effect captures what happens during the remaining time of day in which the restrictions are in place. We find that the observed market conditions are consistent with a situation in which the stock is going through a strong downward price pressure, with strong order imbalance driven by sales of the asset, which results in a price drop that is partly reversed by trading over the remainder of the day after the restrictions are introduced. The restrictions appear to be accompanied by a partial shift to competition in liquidity provision using hidden orders, an increase in off-exchange trading, and off-exchange exempt short sales.

In particular, following the introduction of the restrictions we find that volume continues to be greater than before the restrictions, and it continues to reflect an order imbalance driven by sales. This is also accompanied by increased off-exchange short sales, where non-exempt short sales continue at pre-restrictions level, and the volume of exempt short sales increases. We observe that there is a displacement towards off-exchange trading, but not from the NASDAQ exchange, which retains its pre-restrictions market share. On NASDAQ, liquidity, as measured in terms of the quoted spread and depth (particularly on the bid side), is lower than before, but the effective spread improves. We explain this from an observed increase in hidden volume that suggests that after the restrictions are introduced, aggressive short-sellers convert their executable sales into aggressively priced hidden orders inside the spread. Liquidity demanders facing higher quoted spreads can thus obtain lower effective spreads by executing against these hidden orders inside the spread. This increased competition via hidden liquidity is paired with a reduction in algorithmic activity (measured in terms of trade-to-order ratio) on the ask side of the order book, and an increase on the bid side. For market makers we observe lower realized spreads, consistent with lower visible liquidity, and with a slowly rebounding of the price, as well as potentially substantial profits for long-term liquidity providers willing to hold on to the asset over longer time horizons.

We also observe that the introduction of the restrictions leads to unusual activity in the immediate vicinity of the transition to active short sale restrictions. We find that the introduction of the short sale restrictions appears to act as a catalyst for price pressure to turn into a price drop (albeit only in the short-term) in a way that reminds us of the bubbles bursting in the coordination model of Brunnermeier and Oehmke (2013). This is accompanied by unusually high volume, larger sell order imbalance, larger volatility (wider ranges of price movement), and wider effective and quoted spreads. We also observe a temporary increase in buy volume on NASDAQ, and away from other (off-exchange) venues but only in the minute of the event. This coincides with a shift in algorithmic trading on the bid side of the order book, and higher realized spreads.

3.1.1 Related Literature

Our analysis complements existing research in H. Florindo (2020b) that provides an extensive study of the ban at the daily level over most of the period since the ban was introduced in 2011, focusing on price efficiency. The authors find that the ban causes an artificial price stabilization for affected stocks that is reverted at the removal of the ban. They also find that this is accompanied by a decrease in price efficiency. Other papers that study this regulation at the daily level are Dixon (2021), and Jain et al. (2012). Jain et al. (2012) includes additional information on short selling volume at the intraday level, while Switzer and Yue (2019) and Davis et al. (2017) also look at the intraday with a focus on price informativeness and price clustering. The closest paper to this one is Barardehi et al. (2019) and their methodology is significantly different from ours as well as some of their conclusions on market quality.

Barardehi et al. (2019) analyze the impact of Rule 201 on different characteristics of stocks. They find that there is a positive effect on returns. Also, they find a spillover effect of short-selling pressure to peer assets once Rule 201 applies. Regarding volatility, they find a decrease in spot volatility indicating that Rule 201 stabilizes prices. The effects of Rule 201 on liquidity are positive: spreads are lower and depth is bigger. In general, their results support the Rule 201 policy.

Dixon (2021) analyzes the role of information around the 2008 short selling ban. Their main idea is that assets' owners are the informed investors. Once the ban is active and short sellers are not in the market, the adverse selection cost of liquidity should increase on the sell side. The empirical evidence of the 2008 ban is consistent with the theoretical prediction of his model.

Up to date, research on the effects of Rule 201 is scarce. Jain et al. (2012) analyze the ban if it had been applied under the flash crash of 2010 and during the most volatile trading dates of the 2008 crisis, documenting its ineffectiveness at reducing price declines. Halmrast (2015) finds no significant effect of the ban on stock prices. However, both research designs overweight the retrospective analysis, with limited contributions to the analysis of actual events. In Jain et al. (2012) only two months are included after February, 2011 (the compliance date) and Halmrast (2015) excludes part of 2012, precisely the most volatile months in which assessing the effects of the ban are more interesting for market participants and regulators.

Davis et al. (2017) find evidence of price clustering, a sign of price inefficiency while Switzer and Yue (2019) document no effect on the main metrics of market quality. Both studies do not go beyond 2012 in their analysis and their results arise from differences-in-differences analyses that compare the Rule 201 affected stocks with a before treatment situation (before February, 2011) that allows no comparison with contemporaneous counterparts. Given the significant informational component we could expect from a price decline of 10% (or more) we believe there is potential for a better identification of the effects when the units of analysis share informational sets, which cannot be the case when the treated unit and the control unit are selected with time gaps.[1]

Diether et al. (2009a) study the effects on stock's market quality of the suspension of short-sale pilot program on 2005. First, the effect is not homogeneous to NYSE or NASDAQ listed stocks. Permitting short sales increase the bid depth and quoted and effective spreads. Moreover, it does not change significantly in downsize volatility.

Another important paper is Diether et al. (2009b). First, these authors highlight the huge amount of short selling in the US Market, that is around 31% and 24% of share volume on Nasdaq and the NYSE, respectively. They identify some regularities on short selling activity. On one side, short sellers increase their activity after periods of positive returns, on days with significant buying pressure, and on days with high levels of asymmetric information. Their results are consistent with short sellers

[1]As later discussed in Section 3, we show how the Rule 201 likelihood of treatment assignation is not purely random and uniformly distributed, but rather closely related to both idiosyncratic factors of the asset and also specific situations in the markets and specific days. A misinterpretation of the covariates influencing the likelihood of the treatment and the lack of controlling for them could severely bias the results (Rosenbaum, 2002). If the likelihood of the treatment is time variant and dependent on the specific day-market situations, non-contemporaneous control units should not be considered as candidates for the control. We later provide evidence on this issue surrounding the Rule 201 circuit breakers and how they are associated with both idiosyncratic and market-wide factors.

trading on short-term overreaction of stock prices.

Shkilko et al. (2012) do not analyze Rule 201. They study events where there is an important price decline. Although before the price drop, short sellers are demanding liquidity and they carry some features of predatory trading. Their results show that the main driver of such drops are long sellers seeking for liquidity and not short-sellers. They highlight that short sellers are active but their influence in price declines is secondary.

Comerton-Forde et al. (2016) distinguish between short sellers that provide liquidity, and the ones that demand it. Short sellers providing liquidity trade when spreads are unusually wide and the contrary for short sellers that demand liquidity. They use 2008 data just before short selling ban. Their results support the 201 type regulation where the regulation differentiate short sellers that provides liquidity (the good) from the ones that demand it (the bad). However, their evidence show that both types are important because the good ones are an important ingredient of a competitive, and liquid market, while liquidity-demanding short sellers are important to price discovery.

Engelberg et al. (2018) show the additional risk that short sellers face. More specifically, Engelberg et al. (2018) analyze the role of loan recalls and the risk of changing loan fees. Overall, the authors show that short-selling risk is associated with more mispricing and less short selling, especially for trades with longer holding periods.

Hu et al. (2021) analyze the information content of shorts considering the time of the short during the day and the horizon of the measured cross-sectional return predictability. Their main result is that off-exchange short sales before 10am provides a strong prediction of the the next-day return in the expected direction, but this effect is no longer than the next day. Moreover, institutional short sales contain information that is similarly strong and much longer-lived.

Félez-Viñas (2019) show the effects of short selling bans on resiliency and commonality in liquidity analyzing the short selling bans on financial assets in some countries in Europe on 2011. The author find that the ban has a negative effect on the speed of recovery of the tightness and depth dimensions. Interestingly, Félez-Viñas (2019) look at the recovery speed at deeper levels of depth finding similar results. Moreover, the author shows a decrease in commonality in liquidity for the banned stocks following the implementation of the ban.

Chakrabarty et al. (2012) look at the quality of Lee-Ready trade classification algorithm for short and long sales. Their results show that the algorith is correct identifying transactions at the daily level and is not as good at the interdaily classification.

Brogaard et al. (2017) show that the role of HFT and non-HFT short sellers is the opposite [2008 ban]. On one side, non-HFTs' short sellers improve liquidity when measured by bid-ask spreads. On the other, HFTs' short sellers has the opposite effect by adversely selecting limit orders. The results highlight that some HFTs' activities are harmful to liquidity during the extremely volatile short-sale ban period. Also, the authors show that HFTs' ability to time liquidity is the main driver of the liquidity decrease and the increase in the cost of liquidity providers activity.

3.2 Institutional Setting

Under Rule 201, a bid test comes into effect for non-exempt short sales in a given symbol if its share price on a given day has fallen by 10% or more since the previous close. This bid test requires market centers to implement policies and procedures to prevent the execution or display of a short sale order at a price equal to or below the current national best bid. This price test remains in effect for the rest of the trading day and all of the following trading day. Most short sales are non-exempt. Exempt short sales are normally part of a hedged trading strategy involving two highly correlated securities, such as different classes of a single company's common equity, two ETF's that track the same index, and so on.

The alternative uptick rule is a short selling restriction measure adopted by the Securities and Exchange Commission on February 26, 2010. Hereinafter, we will refer to it as the **Rule 201**, but to be precise, this new price test is the result of a series of amendments to Rule 201 Regulation SHO. Originally, this directive from the SEC removed all the previous price tests after the conclusions on the Pilot study that deemed short-selling bans as ineffective.

The Rule 201 ban prohibits the short selling of any security at or below the national best bid if that security's price has fallen below a threshold of 10% relative to the last closing price. Once the trigger condition is met, short sale orders at or below the best bid are immediately prohibited for the asset for the remainder of the current trading day and the whole of the next one. The rule does allow for the possibility of an activation on consecutive days. If this happens, the ban extends for an additional trading day after the last trigger. Trading centers are required to comply with the new regulation since February 28, 2011.[2]

[2] Division of Trading and Markets: Responses to Frequently Asked Questions Concerning Rule 201 of Regulation SHO. Accessed: Sep 28, 2017.

This alternative uptick rule represents an innovation with respect to previous short sale restrictions. In contrast with previous bans, the trigger condition is endogenously determined. Whether the prohibition is imposed or not depends on the behavior of the stock's price in the market. Previous research was based upon regulations that arbitrarily forbade (2008 emergency ban) or allowed (Russell 3000 pilot program) short-selling for a list of stocks. Furthermore, Rule 201 acts as a temporary correction mechanism, that is automatically reverted shortly after its application, which contrasts with previous bans which were in force for much longer time periods. The effects of this new short lived constraint and whether they differ from those observed with previous regulation is still an open question.

3.3 Data & Methodology

We collect the data on Rule 201 bans from the Philadelphia Stock Exchange website, which publishes the list of stocks that trigger the circuit breaker on a daily basis. Our period of study covers all observations from January until December, 2017.[3]

We combine data from a number of sources: CRSP, TAQ quotes and trades, Total-View-ITCH, and FINRA short sales. We match CRSP and TAQ ticker symbols. We retain only common stocks (those with a CRSP share code equal to 10 or 11) and exclude securities such as warrants, preferred shares, American Depositary Receipts, closed-end funds, and REITs. We require a minimum share price of $2 for a stock-day to be included in our sample.

We use trade and quote level data between 9:30AM–4:00PM EST from Daily TAQ. 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

[3] https://www.phlx.com

Consolidated Trades Tape. We match each transaction to the mid-point of the prevailing best bid and offer prices at the end of the previous millisecond. We construct best national bid and offer prices at the millisecond frequency using the Consolidated Quotes Tape and NBBO files from the Daily TAQ data base. We also drop stocks whose identifying information does not allow a merge with both CRSP and Daily TAQ. Transactions are classified into buyer- or seller-initiated using the Lee-Ready (1991) algorithm, based on the midpoint of national best quoted prices at the end of the millisecond prior to each transaction.

Short sale information is obtained from the FINRA monthly files which include all short sale transactions that are executed off-exchange and reported to the consolidated tape. All of the transactions are reported via a FINRA Trade Reporting Facility (TRF). [4]

We use Total-View-ITCH which is publicly available data from NASDAQ. The data are time-stamped to the millisecond and contain every message to post, or cancel a limit order, and messages that indicate the execution (partial or total) of a displayed or non-displayed order. Although non-displayed orders are not visible in the data when they are submitted to the limit order book, one can see them (ex-post) when they execute against a marketable order.

Some of our variables are constructed using only NASDAQ data. This ensures the reliability of trade direction and allows us to study in detail the microstructure conditions around the application of Rule 201 restrictions. NASDAQ is only one of the several exchanges that are open for trade in US cash equities. Although NASDAQ has gradually lost market share it remains as one of the dominant venues for trade and, in 2017, had an estimated market share of 20%. Combining data from

[4] FINRA posts the data on its website a week or so after the end of each month. It includes the ticker symbol, trade price, size, and other sale conditions, along with a time stamp to the nearest second. One additional field in this dataset is a condition code on whether the short sale is exempt from price tests.

several sources allows us to provide a general overview of the effects of the Rule 201 restrictions while also providing additional analysis of market conditions for a key venue for which we have additional detailed information.

One of the main challenges in analyzing the impact of Rule 201 is that it is triggered by a very unusual event, a 10% price drop relative to the previous day's close (the "*event*"). Under these circumstances, the choice of a reference group to serve as counterfactual, as well as the choice of control variables is mired with problems.

Our approach is to limit our analysis to the day of the event and use the same stock's circumstances as comparison with post-event ones. To account for the transition in market conditions (such as magnet effects for example) we separate the immediate times surrounding the time of transition to short-selling restrictions (the *event window*) from the rest of the trading day. We also limit our comparison in terms of within stock variation by standardizing our variables, so that our analysis measures variation in terms of standard deviations from the mean for each stock-day.[5] To isolate the effects of interest as much as possible, and to avoid endogeneity in the controls, we only include time-of-day fixed effects (in the form of half-hourly dummies) and fixed effects for the direction and magnitude of price changes.[6]

Our analysis pools the resulting variables in a joint panel OLS estimation with clustered standard errors. We exclude the first and last 10 minutes of trading to avoid contamination from the opening and closing auction. We also drop stock-days when the restrictions are activated within the first 15 minutes.

[5]Moments are computed using the in-sample means and standard deviations. The use of the in-sample means is standard practice in all panels with fixed-effects, and the use of the in-sample standard deviation will, if anything, bias against finding significant differences, given that the sample stock-days have unusually high intraday volatility.

[6]The fixed effects for price changes is obtained by separating the range of possible returns into 22 bins for the intervals: $(-\infty, -10\%]$, $(-10, -9\%]$, $\ldots$, $(9, 10\%]$, $(10\%, \infty)$, and using dummies for each bin.

We implement the analysis by running the following panel data regression:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t} \in [R_{j-1}, R_j]} + \varepsilon_{i,t} \quad (3.1)$$

Where β captures the overall effect while δ_j capture the transition in market conditions for the minutes around the event. The variables H_j and $Dum_{r_{i,t} \in [R_{j-1}, R_j]}$ represent the time and return size fixed effects.

3.4 Results

We split the analysis of our estimation results into two parts: The first focuses on the overall comparison of intraday market conditions before and after the introduction of short-selling restrictions; The second looks at market conditions immediately surrounding the transition from no restrictions to the restrictions imposed by Rule 201.

3.4.1 Overall effect

After the event that triggers the short selling restrictions we find that on average returns tend to be positive for the remainder of the trading day (Table 3.2). This is consistent with the evidence in Jain et al. (2012) or Barardehi et al. (2019) that price drops of the magnitude needed to trigger the short sale restrictions tend to be followed by a price 'rebound'.[7] This price change is also accompanied by an increase in the volatility of the asset, as measured in terms of its 1-minute price range (also on Table 3.2). This is consistent with the results in Boehmer et al. (2008) and Jain

[7]Naturally, the return regression does not include return size dummies, only time fixed effects. The results are not affected if we exclude the time fixed effects. Also, this regression is computed with the actual returns and the variable is not standardized.

et al. (2012) using a similar measure.[8]

Predictability in price direction is inconsistent with market efficiency under perfect capital markets. Thus, we need to consider friction-based models, such as limited capital available to provide short-term liquidity (Grossman and Miller, 1988), and predatory trading (Brunnermeier and Oehmke, 2013). These models allow for a temporary predictable price deviation from 'fundamentals' that is due to a temporary price pressure that is not fully absorbed by existing liquidity providers. These explanations imply negative immediate expected profits for liquidity providers, as they provide liquidity in response to strong sell pressure, followed by positive expected profits in the 'long-term', once the initial sell pressure subsides, and the price rebounds.

In this context, the introduction of Rule 201 short sale constraints implies that (a) liquidity provision on the bid side continues to be subject to short-term losses as market makers are buying from long sellers while the price is falling, (b) the selling pressure from short-sellers will continue at prices above the bid, or (c) shift to other, less transparent venues. These effects will continue until the price pressure subsides, at which point prices will gradually move up. Consistent with this we find (Table 3.3) that overall volume, across all markets, is greater, and driven by increased volume on the ask side (buys), while the sell side volume is not significantly different to that prior to the event. We also find that off-exchange short selling activity increases, primarily due to increases in exempt short-sales (Table 3.4). This suggests that short-selling continues to take place in less transparent venues, and it increases through channels permitted via Rule 201 exemptions. Furthermore, we observe on Table 3.5 that the overall volume stays the same in the NASDAQ exchange, though the share of off-exchange volume grows significantly (at the expense of other non-

[8] Barardehi et al. (2019) obtains different results using realized volatility over 65 minutes intervals as opposed to the 1-minute ranges we use in this study.

NASDAQ venues).

When we look at the activity on NASDAQ specifically, we find that volume increases (consistent with the overall increased volume and its constant market share). This increase in volume does not occur in visible trading (trading against limit orders that are visible to market participants) but rather it takes the form of increased trading against hidden volume (Table 3.6).[9] However, we do observe changes in the pattern of visible volume, where we observe an increase in volume on the ask side (buys) and a decrease in volume on the bid (sells), consistent with a reduction in selling pressure and a price rebound. The increase in hidden orders complements the increase in short-sale volume off-exchange reported on Table 3.4 as a way for traders to adapt to the new short-selling restrictions. This increase is consistent with the specifics of the Rule 201 restrictions that allow aggressive short sell orders to take place as long as they do so above the bid, which encourages switching from executable to passive limit orders. These passive limit orders, when priced aggressively are more likely to be executed quickly, and when posted as hidden are less likely to provoke competition from existing liquidity providers (see for example Chakrabarty et al. (2020)).

However, following the trigger event, visible liquidity provision in terms of measured visible depth (Table 3.7), as well as immediate execution costs (quoted spread, Table 3.8) is diminished. The quoted spread increases, and depth decreases. The decrease in depth is primarily driven by a decrease in depth on the bid side, consistent with a depletion of capital during the process of absorbing unusual selling pressure, while visible depth on the ask side is not for the most part, significantly different than before the event. But, this decrease in liquidity provision does not translate into higher immediate effective trading costs, as the increase in aggressively priced

[9] In unreported analysis we also find that hidden volume increases as a percentage of overall volume.

hidden limit orders reduces actual trading costs to those demanding liquidity on the exchange. At the same time we observe an increase in the quoted spread. The wider spread reduces the cost of aggressively pricing hidden short sale limit orders and explains how we can simultaneously observe an increase in the quoted spread and a decrease in the effective one.

In terms of message activity, we find that it is lower though primarily because activity on the ask side is lower, while that on the bid remains the same (Table 3.9). This is accompanied by a decrease in HFT activity on the ask side, whether measured using PC100 or the Trade-to-order ratio (T2O).[10] On the bid side we also observe lower PC100 but the T2O also decreases (which indicates fewer trades per messages and is indicative of greater algorithmic activity). The net effect is an overall reduction in measures of algorithmic activity (lower PC100 and greater T2O).

In order to evaluate the observed changes in activity we consider existing theories. A number of them associate HFT activity with anticipatory trading that exploits speed advantages (Cartea and Penalva (2012), Foucault et al. (2017)) and leads to speed races (Aquilina et al., 2020). These strategies are more likely to take place under high volatility conditions, however we observe the opposite: high volatility (Table 3.2) and lower HFT activity (lower PC100 and greater T2O, Table 3.9). Nevertheless, we have just argued that competition for liquidity is now taking place inside the spread via aggressively posted sell limit orders. Algo strategies that hold very low inventories (Kirilenko et al. (2017)) need to ensure that their orders posted on the bid side are only executed after they have a matching order executed on the ask. This leads to greater algo activity on the bid, that is not reflected in PC100 as volume is also lower. Moreover, increased competition from hidden sell orders inside the spreads leads to lower overall HFT activity on the ask, both in terms of

[10]This measures are used, amongst others, in Cartea et al. (2019) and Menkveld (2013)

T2O and PC100 (despite the increase in visible aggressive buy activity).

Other theories emphasize the role of algorithmic traders as liquidity providers. From the market-makers' point of view we observe a decrease in realized spreads (and associated expected profits) across our three horizons of analysis (100ms, one, and five minutes, Table 3.10). This lowered short-term expected profits are consistent with the observed decrease in our two measures of algo activity, PC100 and T2O. The observed changes in realized spreads are not, for the most part, accompanied by increased toxicity, as measured by price impact (Table 3.11), except at the one-minute horizon, which matches the effect of the price rebound observed on Table 3.2 and benefits those that have been providing liquidity to sellers until the rebound.

3.4.2 The immediate change in market conditions

We have seen the overall changes in the market that occur before and after the introduction of short sale restrictions. In the second part of our analysis we consider whether we observe unusual behavior surrounding the specific event triggering the implementation of Rule 201 restrictions. This is motivated by existing results in the literature that find that regulation triggered by market conditions, such as volatility halts or trading pauses, may be accompanied by specific market reactions, such as for example the magnet-effect (Abad and Pascual (2013), Abad and Pascual (2007), Goldstein and Kavajecz (2004), Sifat and Mohamad (2020)).

To study whether the Rule 201 triggers specific local effects, we define an event window that includes the minute containing the event that triggers Rule 201, and the five minutes before and after this minute. In particular, in our regressions describing the introduction of Rule 201 restrictions we include dummies for the minute in which the event takes place ($Trigger$) as well as for each of the previous five minutes ($T(-1)$ to $T(-5)$) and the subsequent five minutes ($T(1)$ to $T(5)$). We refer to these eleven minutes under study as the event window.

If there are no specific local effects, the overall pattern we should observe would be consistent with the pattern previously described: strong short-selling pressure followed by an easing and price rebound. However, this is not the case, and on Table 3.2 we observe the opposite effect. Instead of a price drop followed by a price rebound, we observe that in the early minutes of the event window the stocks display a short term appreciation, which is followed by two minutes of relative stability, where returns are not on average significantly different from zero, and in the minute of the event the price drops significantly and continues to fall until the end of the event window, with the magnitude of the price drop decreasing over time. This pattern suggests that the price pressure accumulates in the seconds around the time of the trigger (we cannot identify whether it is immediately before, during, or after the event) and the price pressure continues to drive the price down for the remaining time in the event window.

Although prior to the event we see no obvious evidence of price pressure on the average returns, during the pre-trigger minutes we observe prices moving over unusually wide price ranges, and the price movements widen and peak at the trigger minute, with a price range over 1.5 standard deviations away from the mean for the current-stock day. Following the introduction of the short trading restrictions, although we have seen that prices fall, the range of variation is narrower than in the pre-trigger minutes and becomes smaller as the end of the event window is reached. Even in the last minute of the event window the price range is significantly above the mean, but the magnitude is the lowest in the entire event window, 13% of one standard deviation. Thus, it appears that the restrictions are accompanied by a reduction in price volatility while at the same time the price pressure turns into price declines.

In terms of overall volume, on Table 3.3, we observe evidence consistent with a significant downward price pressure: (i) Unusually large volume, (ii) unusually large aggressive selling activity, and (iii) below average aggressive buy volume. The net effect is consistent with downward pressure on the price that is present throughout the event window, and is accompanied by a price drop from the event minute onward.

When discussing off-exchange short selling behavior above, we found that during the remainder of the day off-exchange short sales increased, and one of the arguments was that short selling pressure shifts to less transparent venues. However, what we find is that the local effects on off-exchange short sales are consistent with a more straight-forward explanation: an increase in short-selling restrictions leads to a reduction in off-exchange short sales (Table 3.4). Off-exchange non-exempt short selling activity is lower but not significantly different from zero, during most of the event window. The exemptions are the event minute (32% of a standard deviation below the mean) and in the minute immediately following and the one preceding the event (7 and 10%, respectively). Short-sale exempt trades however are significantly less frequent during the entire event window, specially at the event minute.

The activity surrounding the event is hardly unusual from the rest of the day in terms of market shares for most of the event window (Table 3.5). Fluctuations in the overall market shares of NASDAQ and off-exchange (FINRA) volume are mostly not significantly different from the mean prior to the event, with one marked exception. Overall NASDAQ market share is significantly greater in the minutes immediately before and after the trigger event, due to a greater share of sell volume. After this, NASDAQ market share returns to its usual level. Off-exchange market share drops slightly at the event, but it becomes positive in subsequent minutes driven by an increase in both sell and buy volume market share, though the increase is larger for buy volume.

Focusing now on NASDAQ trading, we find that there is significantly more volume than usual throughout the entire event window, both on the bid and ask side, as well as in terms of hidden volume (Table 3.6). This is consistent with the generic increase in volume associated with the general unusual sell pressure we observe in the days in our sample and the roughly constant market share that NASDAQ retains throughout the event window.

Volume displays a tent shape, increasing to a peak at the event time and decreasing thereafter. In terms of visible trading, i.e. aggressive orders trading with passive orders at the best bid and offer, we observe similar though slightly greater volume on the ask (aggressive buying) than on the bid (aggressive selling) up to the event. In the minute of the event this pattern changes where we observe a very significant peak of trading that is much greater on the bid than on the ask side (1.4 on the bid as compared with 0.8 standard deviations above the mean on the ask side). For the rest of the event window, visible trading is larger on the bid than on the ask side. The pattern observed for (unsigned) trading against hidden orders mimics volume on the ask side up to the event time and switches to mimic the visible volume on the bid side at and after the event (with a peak at 1.4 standard deviations above the mean at the time of the event).

On Table 3.8 we observe that the unusually large ranges for price movements and volume observed during the event window are accompanied by wider spreads, both quoted and effective. Both types of spreads start at above average levels and increase to a peak, declining to a positive but lower level than when they entered the event window. Effective spreads are highest for the minute of the trigger while quoted spreads are highest for the last minute before the event. Under normal circumstances we observe this pattern of high volume accompanied by high volatility and spreads at the beginning of the trading day, when there is significant uncertainty about the price of the asset combining both divergence of opinions and adverse selection. In our sample, and specially around the trigger, we find the same pattern with the caveat that there is (by the choice of sample) a significant downward price pressure that crystallizes into price declines at the time of the event, and which continues for the remaining duration of the event window. Thus, despite the unusual downward pressure in our sample, the context in terms of volume, uncertainty, and spreads, appears to be one of unusually high uncertainty and asymmetric information, that peaks at the time of the event. In terms of the relationship between spreads and hidden volume we interpret them in the context of the results on short-seller behavior reported in Comerton-Forde et al. (2016). The authors find that passive short sellers are more willing to supply liquidity when effective spreads are wider. In the 201 event we find that spreads are wider and hidden volume (on NASDAQ) is greater, suggesting that passive short sellers are entering the market to provide liquidity via hidden orders, and this increased hidden liquidity is driving the gradual decline in effective spreads (and not in quoted spreads) we observe after the trigger.

If we look at depths, on Table 3.7, we observe that the directional pressure is reflected in the asymmetric circumstances on the bid and ask sides of the order book. On the ask side what we observe are mostly negative or insignificant changes during the event window. Depth at the best offer is mostly unchanged with small increases at and closely following the event. At deeper levels, depth 5 cents above the ask is significantly lower in the five minutes before the event, and at the event and the following two minutes depth is insignificantly different from zero (the mean for the stock-day). Towards the end of the event window the depth becomes negative at a lower level than it was at the beginning of the event window. At 10 cents above the best offer depth is significantly below its mean during the whole of the event window, though we do see a significant reduction in the magnitude of the difference from the mean at and following the event. Thus, overall, the shift of buying activity towards NASDAQ we have observed is not sufficient to attract significantly large liquidity on the ask side, specially at deeper levels, where we observe that liquidity is drained in times of significant selling pressure.

On the bid side, depth is around zero for the entire event window. We only observe a significant sharp increase in depth at the best bid at the time of the event. Thus, both in the bid and the ask side we find that the lower liquidity observed in terms of spreads is partly reflected in terms of lower or no change in depth levels, with the exception of the time of peak activity at the time of the event.

However, lower liquidity on the order book does not mean lower activity. On Table 3.9 we observe significant increases in the measures of activity, messages and PC100, both at the bid and ask sides of the order book which is associated with the increase in trading activity. As in the case of volume, we also observe a tent shape with (very economically significant) peaks at the event time. This peak activity at the time of the event is between 2 and 3 standard deviations above the mean for all measures of activity: messages, and PC100, both on the bid and ask sides of the order book, while outside the event times, activity levels do not exceed a one standard deviation rise above mean levels. And this stands in sharp contrast with the much lower levels of activity observed outside the event window, specially when the restrictions are in place. However, when we look at T2O, which looks at activity after controlling for volume we find that most of the change that goes with the restrictions is concentrated on the bid side, starting with greater activity in the minute immediately preceding the event, and followed by a significant drop in algo activity during and after the restrictions are put in place. This again stands in stark contrast with the effect outside the event window, where we saw that algo activity is higher on the bid and lower on the ask side.

In this context, we observe on Table 3.10 that the lower liquidity in terms of spreads and depth is accompanied by positive realized spreads, specially at the shortest horizon. These realized spreads shrink with the negative returns that follow the implementation of trading restrictions at the 1 and 5 minute horizons, indicating that the lower liquidity is justified for those market-makers that expect to hold their positions over these time horizons, and whose pre-event extra profits are eliminated by the post-event price drop.

Table 3.11 displays the patterns for price impact. What we find is that the price impact is only significantly different from (and greater than) the mean prior to the event at the shortest time horizon (100ms). In the early uncertain times, with jittery liquidity providers, trades move prices more than usual only over a very short time horizon. But then, as the triggering event unfolds, price impact returns to normal at the shortest time horizon, while it increases substantially at longer horizons. It appears as if, following the introduction of short selling restrictions, liquidity providers yield to the selling pressure following the triggering of the event, and allow prices to move downward.

3.5 Conclusions

Our paper provides a detailed analysis of the stock-days in which the short sale restrictions of Rule 201 are put into effect. With this, we aim to provide additional evidence on the ongoing discussion over the positive or negative role played by the short sales restrictions in general, and the current US Rule 201 in particular. We focus on the effects of the Rule 201's ban of aggressive short sellers on market quality, the level and distribution of traded volume (visible, hidden, and short-sales), returns and volatilities, algorithmic activity, and other dimensions of market microstructure during the day the short-selling restrictions are activated.

The first key take-away from our analysis is that off-exchange short selling activity increases, primarily due to increases in exempt short-sales, while at the same time hidden volume in NASDAQ also increases. The combination of these suggests that short-selling continues to take place in less transparent venues through the channels permitted via Rule 201 exemptions, or is modified to continue to be aggressive but not immediately executable in the form of aggressively posted hidden limit orders inside the spread.

Second, in terms of market quality, the effect of the ban has a negative impact when we look at depth or quoted spreads. However, the presence of aggressively posted hidden limit orders leads to an improvement in the effective spread, so that the effective cost of transactions decreases (at least on NASDAQ). The transformation of aggressive short-sales into aggressive limit orders is consistent with the general evidence in Comerton-Forde et al. (2016) that short sellers become liquidity providers when the quoted spread is wider than usual.

The possibility of increased competition for liquidity from hidden orders is also consistent with the observed decrease in algorithmic activity on the ask, both in terms of the trade-to-order (T2O) ratio and ultra-fast activity (PC100), despite an observed increase in the arrival of visible aggressive buy orders. Furthermore, realized spreads (and associated expected profits) across our three horizons of analysis (100ms, one, and five minutes) are lower which further justifies the observed wider quoted spreads and lower depth.

We also find evidence consistent with previous studies on the dynamics of the price, such as Jain et al. (2012) or Barardehi et al. (2019). As expected, returns are negative in the minutes after the ban and we observe a 'rebound' during the rest of the day. Accompanying the price movements, we find that general market conditions around the time of the activation of the restrictions are similar to those in early trading, where high volume is accompanied by wider quoted spreads and higher volatility. However, we find that overall trading volume, although constant in NASDAQ, shifts from other exchange to off-exchange venues.

Our results are obtained using standardized variables and trade direction in TAQ data and hidden volume is obtained signed using the Lee-Ready algorithm. Given these caveats (and the limitation of the algorithm as identified in Chakrabarty et al. (2012)), the paper sheds some light on the immediate consequences of the short selling restrictions under Rule 201, and provides direct evidence of the impact and possible costs and benefits of this particular form of regulating short sales.

Tables

Table 3.1 - Descriptive statistics

	N (,000)	mean	sd	min	max
Shares SS	1659.82	1012.08	6642.01	0	1619325
Shares SS (Exempt)	1659.82	41.09	659.79	0	271475
Shares SS (Non-Exempt)	1659.82	970.99	6445.03	0	1619325
Mess Total	1659.82	67.63	221.52	0	31612
PC100 Total	1658.71	10.15	51.5	0	15170
ToR Total	1199.19	4.02	9.71	0	100
QS (bps)	1659.82	225.47	416.13	0	19910.31
ES (bps)	565.19	49.96	102.55	0	9489.05
RS 1min	565.18	2.03	63.64	-165.22	4342.11
PI 1min	565.18	22.84	59.14	-19.9	6543.49
Log Total Volume Vis	1659.82	2.58	4.09	0	16.92
Log Total Volume Hidden	1659.82	1.5	3.21	0	16.32
Log Depth Total ($)	1659.82	8.53	1.31	1.83	15.21
Log Depth Bid ($)	1659.82	7.57	1.61	0.69	15.03
Log Depth Ask ($)	1659.82	7.61	1.5	0.96	14.95
Range (volatility)	1200.14	0.38	0.92	0	344.39

Table 3.2 - Returns and Volatilities

PRICE MOVNT	Range	Return (%)
Ban	0.0408***	0.0510***
T (-5)	0.313***	0.0391*
T (-4)	0.329***	0.0682***
T (-3)	0.416***	0.0652***
T (-2)	0.477***	0.0227
T (-1)	0.692***	0.0190
Trigger	1.653***	-1.330***
T (+1)	0.370***	-0.427***
T (+2)	0.219***	-0.246***
T (+3)	0.161***	-0.160***
T (+4)	0.157***	-0.124***
T (+5)	0.134***	-0.118***
Observations	1,163,911	1,614,960
R-squared	0.314	0.018
# Events	4,486	4,486

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t} \in [R_{j-1}, R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. The variables of interest are $Return_{i,t}$ measured as one-minute asset return for asset i in minute t and $Range_{i,t}$ calculated as the difference between the highest minus the lowest midprice during the minute, normalized by the average of the two. Ban takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. $Trigger$ takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t} \in [R_{j-1}, R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, ..., $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Table 3.3 - TAQ Volumes

VOLUME (All markets)	Sell (Bid)	Buy (Ask)	Log Volume (Total)
Ban	-0.0128	0.119***	0.0791***
T (-5)	0.373***	-0.0295	0.294***
T (-4)	0.388***	-0.0380*	0.304***
T (-3)	0.471***	-0.0748***	0.365***
T (-2)	0.515***	-0.111***	0.386***
T (-1)	0.635***	-0.121***	0.485***
Trigger	1.368***	0.125***	1.106***
T (+1)	0.546***	-0.0398*	0.461***
T (+2)	0.444***	-0.0491**	0.363***
T (+3)	0.405***	-0.0704***	0.313***
T (+4)	0.367***	-0.0482**	0.307***
T (+5)	0.325***	-0.0532**	0.259***
Observations	1,582,113	1,573,138	1,588,216
R-squared	0.103	0.035	0.107
# Events	4,407	4,382	4,424

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t} \in [R_{j-1}, R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. The variables of interest is $TAQVolume_{i,t}$ calculated as the (log) total dollar volume obtained by aggregating all (regular) trades in the TAQ dataset for asset i in minute t. Orders are classified as aggressive buy and sell using Lee and Ready (1991). *Ban* takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. *Trigger* takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t} \in [R_{j-1}, R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, ..., $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Table 3.4 - Short Selling Activity

SHORT SALES	Exempt	Non-Exempt	Total
Ban	0.0550***	-0.00138	0.0174**
T (-5)	-0.170***	-0.0104	0.00384
T (-4)	-0.185***	-0.00955	-0.00341
T (-3)	-0.192***	-0.0342	-0.0197
T (-2)	-0.204***	-0.00938	0.00191
T (-1)	-0.305***	-0.0985***	-0.0741***
Trigger	-1.015***	-0.320***	-0.298***
T (+1)	-0.403***	-0.0725***	-0.0787***
T (+2)	-0.247***	-0.0171	-0.0133
T (+3)	-0.199***	0.000833	0.000873
T (+4)	-0.210***	-0.0269	-0.0200
T (+5)	-0.160***	0.0133	0.0106
Observations	1,610,640	1,614,240	1,614,240
R-squared	0.097	0.029	0.026
# Events	4,474	4,484	4,484

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t} \in [R_{j-1}, R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. The variables of interest are Short-Selling ones. Off-exchange short selling activity for asset i in minute t is measured as the log dollar total volume of trades reported as (off-exchange) short sales by FINRA on their website. Short sales are reported as $Exempt$, $Non-Exempt$, and $Total$ (the sum of the two). Ban takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. $Trigger$ takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t} \in [R_{j-1}, R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, ..., $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Table 3.5 - Share Volumes

MARKET		NASDAQ			FINRA	
SHARE	(Bid)	(Ask)	(Total)	(Bid)	(Ask)	(Total)
Ban	0.00730	0.0134	-0.0145*	0.0443***	0.0745***	0.0965***
1-1 T (-5)	-0.0233	0.0375	0.0157	0.0914***	0.0308	0.0494**
T (-4)	-0.0181	0.0326	0.0372*	0.0917***	0.0330	0.0472**
T (-3)	-0.0284	0.0234	0.0139	0.0667*	0.0280	0.0372*
T (-2)	0.0265	0.0323	0.0345*	0.0653*	0.0279	0.0242
T (-1)	0.0561*	0.0689***	0.0739***	-0.00485	0.0119	-0.0154
Trigger	-0.0102	0.0464***	0.0617***	0.0408	0.0176	-0.0322**
T (+1)	-0.0227	0.0626***	0.0364*	0.124***	0.0167	0.0306
T (+2)	-0.0326	0.0362*	0.00874	0.132***	0.0501**	0.0692***
T (+3)	-0.0375	-0.0123	-0.0235	0.133***	0.0677***	0.0938***
T (+4)	-0.0600*	0.0370*	0.0237	0.0841**	0.0497**	0.0473**
T (+5)	-0.0376	-0.00865	-0.0288	0.148***	0.0638***	0.0777***
1-1 Obs.	524,718	762,292	950,513	525,623	762,457	950,662
R^2	0.011	0.015	0.017	0.010	0.016	0.018
# Evs.	4,219	4,348	4,383	4,288	4,360	4,395

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t}\in[R_{j-1},R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. The variables of interest are $NASDAQ_{i,t}$ and $FINRA_{i,t}$. The market share of total volume traded on the NASDAQ exchange are reported in the TAQ dataset for asset i in minute t as a percentage of total volume. The FINRA market share of total volume traded outside official exchanges as reported in the TAQ dataset under the FINRA moniker for asset i in minute t as a percentage of total volume. Ban takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. $Trigger$ takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t}\in[R_{j-1},R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, …, $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Table 3.6 - NASDAQ Volume

VOLUME	Visible (Bid)	Visible (Ask)	Visible (Total)	Hidden (Total)
Ban	-0.0804***	0.0916***	0.00951	0.0868***
T (-5)	0.163***	0.229***	0.221***	0.247***
T (-4)	0.168***	0.176***	0.203***	0.257***
T (-3)	0.175***	0.289***	0.259***	0.316***
T (-2)	0.185***	0.337***	0.278***	0.341***
T (-1)	0.297***	0.440***	0.393***	0.538***
Trigger	1.417***	0.813***	1.228***	1.392***
T (+1)	0.537***	0.143***	0.453***	0.329***
T (+2)	0.338***	0.110***	0.287***	0.219***
T (+3)	0.251***	0.0808***	0.207***	0.189***
T (+4)	0.253***	0.0994***	0.224***	0.198***
T (+5)	0.167***	0.0739***	0.154***	0.138***
Observations	1,597,320	1,563,480	1,605,960	1,593,360
R-squared	0.211	0.072	0.142	0.062
# Events	4,437	4,343	4,461	4,426

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t} \in [R_{j-1}, R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. $NASDAQVolume_{i,t}$ is the (log) dollar volume obtained by aggregating trades in the ITCH dataset for asset i in minute t. Orders are separated into *visible* and *hidden* depending on whether the trade-initiating order executes against a visible (*visible*) or non-visible (*hidden*) standing order. Visible trades are classified as buy or sell orders according to the reported side of the order book of the matching limit order. Ban takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. $Trigger$ takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t} \in [R_{j-1}, R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, ..., $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Table 3.7 - Depth

DEPTH	Ask	Ask+5c	Ask+10c	Bid	Bid-5c	Bid-10c
Ban	0.0202	-0.0423*	-0.0209	-0.0475**	-0.211***	-0.233***
T (-5)	-0.0395*	-0.120***	-0.184***	-0.0117	-0.0440*	-0.0537**
T (-4)	-0.0165	-0.120***	-0.177***	-0.0186	-0.0320	-0.0461*
T (-3)	-0.00887	-0.122***	-0.186***	-0.00350	-0.0123	-0.0330
T (-2)	-0.0117	-0.131***	-0.209***	0.00661	0.00408	-0.0129
T (-1)	0.00712	-0.107***	-0.206***	0.0526**	0.0224	0.00636
Trigger	0.0416*	-0.00237	-0.0885***	0.214***	0.224***	0.200***
T (+1)	0.0363*	-0.0326	-0.0949***	0.0638***	-0.00736	-0.0472*
T (+2)	0.0466**	-0.0149	-0.0676***	0.0466*	-0.00731	-0.0527**
T (+3)	0.0338	-0.0315	-0.0714***	0.0472*	-0.00115	-0.0483*
T (+4)	0.0175	-0.0486**	-0.0740***	0.0567**	0.0148	-0.0353
T (+5)	0.0160	-0.0495**	-0.0714***	0.0339	0.00924	-0.0462*
Obs.	1,614,960	1,614,960	1,614,960	1,614,960	1,614,960	1,614,960
R^2	0.011	0.016	0.014	0.006	0.010	0.011
# Evs.	4,486	4,486	4,486	4,486	4,486	4,486

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t} \in [R_{j-1}, R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. $DX_{i,t}$ for asset i is calculated as the sum of the total US dollar value resting on the LOB within $X \in \{0, 5, 10\}$ cents away from the best bid and ask, time-weighted over minute t. Ban takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. $Trigger$ takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t} \in [R_{j-1}, R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, ..., $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Table 3.8 - ES and QS

SPREADS	Effective	Quoted
Ban	-0.107***	0.127***
T (-5)	0.221***	0.362***
T (-4)	0.220***	0.398***
T (-3)	0.290***	0.466***
T (-2)	0.298***	0.504***
T (-1)	0.422***	0.612***
Trigger	0.559***	0.360***
T (+1)	0.132***	0.198***
T (+2)	0.102***	0.168***
T (+3)	0.0769**	0.175***
T (+4)	0.117***	0.183***
T (+5)	0.0740**	0.153***
Observations	545,724	1,614,960
R-squared	0.104	0.092
# events	4,438	4,486

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t} \in [R_{j-1}, R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. The variables of interest are $EffSp$, the volume-weighted effective spread, and QSp the time-weighted quoted spread. Ban takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. $Trigger$ takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t} \in [R_{j-1}, R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, ..., $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Table 3.9 - Algorithm Activity

ACT'Y	Messages (Bid)	Messages (Ask)	PC100 (Bid)	PC100 (Ask)	T2O (Bid)	T2O (Ask)
Ban	0.00230	-0.0138*	-0.0400***	-0.0483***	-0.0664***	0.111***
T (-5)	0.298***	0.282***	0.174***	0.165***	0.0223	0.00539
T (-4)	0.304***	0.346***	0.167***	0.216***	0.0246	-0.0223
T (-3)	0.405***	0.425***	0.234***	0.241***	-0.00995	0.0288
T (-2)	0.476***	0.534***	0.272***	0.282***	-0.0280*	0.0341*
T (-1)	0.844***	0.916***	0.522***	0.462***	-0.0557***	0.0397*
Trigger	2.693***	2.849***	2.030***	2.350***	0.201***	0.00733
T (+1)	0.538***	0.605***	0.230***	0.352***	0.270***	-0.0102
T (+2)	0.309***	0.365***	0.167***	0.192***	0.169***	-0.00953
T (+3)	0.222***	0.285***	0.103***	0.172***	0.179***	-0.0154
T (+4)	0.217***	0.235***	0.127***	0.114***	0.137***	0.00540
T (+5)	0.135***	0.183***	0.0416*	0.105***	0.0567**	0.00155
Obs.	1,614,960	1,614,960	1,574,280	1,583,640	991,372	1,011,607
R^2	0.219	0.257	0.089	0.120	0.096	0.045
# Evs.	4,486	4,486	4,373	4,399	4,436	4,343

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t}\in[R_{j-1},R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. $Messages_{i,t}$ is the number of messages for asset i during minute t including posting, canceling, and execution of visible limit orders on the corresponding side of the order book (bid and ask). $PC100_{i,t}$ is number of limit orders that are posted and subsequently canceled within 100ms for asset i during minute t. $T2O_{i,t}$ is the trade-to-order ratio computed as the number of executed visible limit orders as a percentage of messages for asset i during minute t. Ban takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. $Trigger$ takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t}\in[R_{j-1},R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, ..., $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Table 3.10 - Realized Spreads in bps

REALIZED SPREAD	100ms	1min	5min
Ban	-0.0542***	-0.0672***	-0.0173**
T (-5)	0.0561*	0.0147	-0.0127
T (-4)	0.0665**	0.0413	0.0251
T (-3)	0.0956***	0.0776**	0.0184
T (-2)	0.0730**	0.111***	0.0725*
T (-1)	0.139***	0.113***	0.0848**
Trigger	0.0949***	0.121***	0.221***
T (+1)	0.135***	-0.804***	-0.486***
T (+2)	0.0770**	-0.176***	-0.607***
T (+3)	0.0535*	-0.108***	-0.656***
T (+4)	0.0909***	-0.0540*	-0.663***
T (+5)	0.0399	-0.0135	-0.499***
Observations	545,726	545,726	545,721
R-squared	0.021	0.065	0.026
# Events	4,439	4,439	4,438

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t} \in [R_{j-1}, R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. The realized spread for asset i, $RS_{i,t}$ is the intra-minute volume weighted average realized spread. The realized spread for the transaction at time t' is computed as $D_{t'}(p_{t'} - m_{t'+\Delta})/m_{t'+\Delta}$, where $D_{t'}$ is the direction indicator for the trade at t' ($+1$ for an aggressive buy and -1 for a sale), $p_{t'}$ is the trade price and $m_{t'+\Delta}$ the prevailing midquote at time $t + \Delta$, where Δ is a pre-specified period of time. We consider three values for Δ, namely 100ms, 1 minute, and 5 minutes. Trade directions for visible traders are available from the ITCH dataset. *Ban* takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. *Trigger* takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t} \in [R_{j-1}, R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, ..., $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Table 3.11 - Price Impact in bps

PRICE IMPACT	100ms	1min	5min
Ban	-0.0179	0.0302***	0.000954
T (-5)	0.117***	0.0770**	0.0620*
T (-4)	0.0944***	0.0412	0.0164
T (-3)	0.107***	0.0326	0.0418
T (-2)	0.147***	0.00674	-0.00962
T (-1)	0.146***	0.0519	0.0111
Trigger	0.305***	0.113***	-0.0760***
T (+1)	-0.0288	0.839***	0.505***
T (+2)	0.00381	0.212***	0.611***
T (+3)	0.00271	0.125***	0.656***
T (+4)	-0.00236	0.0938***	0.679***
T (+5)	0.0212	0.0326	0.502***
Observations	545,671	545,717	545,723
R-squared	0.097	0.113	0.041
# Events	4,429	4,436	4,438

The table reports the coefficients from the estimation of the equation:

$$L_{i,t} = \alpha_i + \beta\, Ban + \sum_{j=-5}^{5} \delta_j T_{j,t} + \sum_{j=1}^{13} \kappa_j H_j + \sum_{j=-10}^{11} \gamma_j Dum_{r_{i,t}\in[R_{j-1},R_j]} + \varepsilon_{i,t},$$

where $L_{i,t}$ is the variable of interest for event (asset-day) i during minute t on asset-days in which Rule 201 comes into effect. The sample period is January to December 2017. Price Impact for asset i, $PI_{i,t}$, is the intra-minute volume weighted average price impact. The price impact for the transaction at time t' is computed as $D_{t'}(m_{t'+\Delta} - m_{t'})/m_{t'+\Delta}$, where $D_{t'}$ is the direction indicator for the trade at t' (+1 for an aggressive buy and -1 for a sale), $m_{t'}$ is the prevailing midquote at time t', and $m_{t'+\Delta}$ the prevailing midquote at time $t+\Delta$, where Δ is a pre-specified period of time. We consider three values for Δ, namely 100ms, 1 minute, and 5 minutes. Trade directions for visible traders are available from the ITCH dataset. *Ban* takes the value +1 for the minutes in which short sale restrictions are in effect, and 0 otherwise. *Trigger* takes the value +1 for the minute that includes the moment in which the short sale restrictions are activated and 0 otherwise. $T(k)$ takes the value +1 for the minute k minutes before (-) or after (+) the moment the short sale restrictions are activated and 0 otherwise. Omitted from the table are the coefficients for the time fixed-effects, H_j, which takes value +1 for the jth half-hour of the trading day (so that H_1 corresponds to the period 9:30-10:00, ...), and fixed effects for the size of the asset's net price movement, $Dum_{r_{i,t}\in[R_{j-1},R_j]}$, which takes value +1 if the mid-point return between the start and the end of the minute falls in the interval $[R_{j-1}, R_j]$, where $j = -10, \ldots, 0, \ldots, 10$ and $R_{-11} = -\infty$, $R_{-10} = -10\%$, ..., $R_{10} = 10\%$, and $R_{11} = \infty$, as well as the individual event (asset-day) fixed effects. * (**)(***) next to the coefficient indicates that the coefficients are statistically different from zero at the 5% (1%)(0.1%) significance level using standard errors clustered by stock.

Appendix

A. Variable Definitions

Our variables are defined as follows:

- $Return_{i,t}$. One-minute asset return for asset i in minute t is calculated as the log difference between the midprice at the end of minute t and the beginning of minute t.
- $Range_{i,t}$. The range of price movement for asset i during minute t is calculated as the difference between the highest minus the lowest midprice during the minute, normalized by the average of the two.[11]
- $TAQVolume_{i,t}$. The (log) total dollar volume obtained by aggregating all (regular) trades in the TAQ dataset for asset i in minute t. Orders are classified as aggressive buy and sell using Lee and Ready (1991).[12]
- Short-Selling. Off-exchange short selling activity for asset i in minute t measured as the log dollar total volume of trades reported as (off-exchange) short sales by FINRA on their website. Short sales are reported as $Exempt$, $Non-Exempt$, and $Total$ (the sum of the two).
- $NASDAQ_{i,t}$. The market share of total volume traded on the NASDAQ exchange are reported in the TAQ dataset for asset i in minute t as a percentage of total volume.
- $FINRA_{i,t}$. The market share of total volume traded outside official exchanges as reported in the TAQ dataset under the FINRA moniker for asset i in minute t as a percentage of total volume.

[11] This variable is normalized in different ways in the literature. As we are working with intervals containing substantial price drops we use the arithmetic average of the two to avoid biasing the measure in any direction.

[12] For more details on the effectiveness of the Lee-Ready algorithm see Chakrabarty et al. (2012).

- $NASDAQVolume_{i,t}$. The (log) dollar volume obtained by aggregating trades in the ITCH dataset for asset i in minute t. Orders are separated into *visible* and *hidden* depending on whether the trade-initiating order executes against a visible (*visible*) or non-visible (*hidden*) standing order. Visible trades are classified as buy or sell orders according to the reported side of the order book of the matching limit order.

- $DX_{i,t}$. Depth for asset i is calculated as the sum of the total US dollar value resting on the LOB within $X \in \{0, 5, 10\}$ cents away from the best bid and ask, time-weighted over minute t.

- $QS_{i,t}$. Quoted spread for asset i is the time-weighted (by millisecond) average, over minute t, of $(a_{t'} - b_{t'})/m_{t'}$ where $a_{t'}$ is the best ask, $b_{t'}$ the best bid, $m_{t'}$ the midprice, and t' indexes observations within a minute.

- $ES_{i,t}$. Effective spread for asset i is the intra-minute volume weighted average effective spread. The effective spread for the transaction at time t' is computed as $2\, D_{t'}(p_{t'} - m_{t'})/m_{t'}$, where $D_{t'}$ is the direction indicator for the trade at t' (+1 for an aggressive buy and −1 for a sale), $p_{t'}$ is the trade price and $m_{t'}$ the prevailing midquote (prior to an execution). Trade directions for visible traders are available from the ITCH dataset and do not need to be estimated. Hidden trades are classified using Lee-Ready.

- $Messages_{i,t}$. Number of messages for asset i during minute t. These include posting, canceling, and execution of visible limit orders on the corresponding side of the order book (bid and ask).

- $PC100_{i,t}$. Number of limit orders that are posted and subsequently canceled within 100ms for asset i during minute t.

- $T2O_{i,t}$. Trade-to-order ratio computed as the number of executed visible limit orders as a percentage of messages for asset i during minute t.

- $RS_{i,t}$. Realized spread for asset i is the intra-minute volume weighted average realized spread. The realized spread for the transaction at time t' is computed as $D_{t'}(p_{t'} - m_{t'+\Delta})/m_{t'+\Delta}$, where $D_{t'}$ is the direction indicator for the trade at t' (+1 for an aggressive buy and −1 for a sale), $p_{t'}$ is the trade price and $m_{t'+\Delta}$ the prevailing midquote at time $t + \Delta$, where Δ is a pre-specified period of time. We consider three values for Δ, namely 100ms, 1 minute, and 5 minutes. Trade directions for visible traders are available from the ITCH dataset and do not need to be estimated. Hidden trades are classified using Lee-Ready.

- $PI_{i,t}$. Price Impact for asset i is the intra-minute volume weighted average price impact. The price impact for the transaction at time t' is computed as $D_{t'}(m_{t'+\Delta} - m_{t'})/m_{t'+\Delta}$, where $D_{t'}$ is the direction indicator for the trade at t' (+1 for an aggressive buy and −1 for a sale), $m_{t'}$ is the prevailing midquote at time t', and $m_{t'+\Delta}$ the prevailing midquote at time $t + \Delta$, where Δ is a pre-specified period of time. We consider three values for Δ, namely 100ms, 1 minute, and 5 minutes. Trade directions for visible traders are available from the ITCH dataset and do not need to be estimated. Hidden trades are classified using Lee-Ready.

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