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The effect of price tests on trader behavior and market quality: An analysis of Reg SHO ☆

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Abstract

Using data from Regulation SHO's pilot program, we examine how price tests affect trader behavior and market quality, which are areas of interest given by the US Securities and Exchange Commission in evaluating these tests. After comparing sampled matched pairs of pilot and control stocks, we find that the removal of price tests benefit traders by allowing them to trade more aggressively by placing orders that receive quicker execution. Furthermore, concerns about the suspension of price tests leading to a degradation of market quality are unfounded. The evidence therefore suggests unambiguously that such tests should be removed.

JEL classification: D02; G12; G18

Keywords: Short selling; Uptick rule; Bid test; Regulation SHO

1. Introduction

In this paper we use recently published data from a pilot program under the US Securities and Exchange Commission's (SEC) Regulation SHO (Reg SHO) in order to evaluate the effects of *price tests* on trader behavior and market quality on the New York Stock

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Exchange (NYSE) and Nasdaq. These tests apply to short sales and are known individually as the *uptick rule* on the NYSE and the *bid test* on Nasdaq.[1] The SEC had three objectives in mind when it introduced the tests: "(i) allowing relatively unrestricted short selling in an advancing market; (ii) preventing short selling at successively lower prices, thus eliminating short selling as a tool for driving the market down; and (iii) preventing short sellers from accelerating a declining market by exhausting all remaining bids at one price level, causing successively lower prices to be established by long sellers" (SEC, 2004a, pp. 50–51). More simply, the SEC sought to prevent short sellers from participating in market manipulation that forces prices downward, often referred to as bear raids.[2]

The pilot program temporarily suspends price tests for a subset of the stocks that are members of the Russell 3000 index, referred to hereafter as *pilot stocks*. The stated motivation for the suspension was to allow an examination of "the extent to which a price test is necessary to further the objectives of short sale regulation [and] to study the effects of relatively unrestricted short selling on market volatility, price efficiency, and liquidity" and "to monitor trading behavior" (SEC, 2004a, pp. 4, 12; SEC, 2004b, p. 2). Ultimately, such examinations could lead the SEC to either amend or remove price tests.

In this paper we examine how price tests affect trader behavior and market quality as measured by market volatility, price efficiency, and liquidity.[3] We do not examine whether price tests further the stated objective of short sale regulation as this was done previously by Alexander and Peterson (1999) who had the benefit of having order data (such data are not publicly available under Reg SHO). Importantly, their study shows that, relative to similar regular sell orders, short sell orders: (1) take longer to execute, (2) are more frequently cancelled or not filled, and (3) if not executed immediately, frequently become part or all of the inside ask, thereby leading to narrower quoted spreads and greater depth at the ask relative to the bid. Interestingly, the uptick rule was found to impede short selling in an advancing market, thereby failing to achieve the first objective of the rule.

Using Reg SHO data for the NYSE, we find that the costs associated with delays in execution due to the uptick rule come with the benefit of price improvement. More specifically, we find that executed short sales of pilot stocks relative to a matched sample of *control stocks* have lower price locations (i.e., trade prices) relative to the quotes.[4] This is expected since market and marketable limit orders (i.e., limit sell orders with limit prices equal to or less than the bid) of pilot stocks are now more likely to be executed immediately instead of being held up by the uptick rule for possible future execution. Consistent with this observation, we also find that pilot stock short sales that execute below the midpoint have greater price impact, indicating that they contribute more to price discovery when the rule is suspended. Thus, the removal of price tests for all NYSE stocks

[1]These price tests are described more fully later in the paper. While the uptick rule also applies to AMEX-listed stocks, we do not analyze such stocks.

[2]See, e.g., ⟨http://www.investorwords.com/444/bear_raid.html⟩ for a description of bear raids.

[3]Our examination assumes that traders do not shift their short selling from other stocks in order to focus on pilot stocks and that abusive short sellers are not currently avoiding pilot stocks in the belief that SEC is looking closely at these stocks, as pointed out by Larry Harris and Bruce Lehmann, respectively, at a Roundtable on Reg SHO held on September 15, 2006 at the SEC (SEC, 2006b, pp. 94, 108–109).

[4]Less than 2.1% (3.8%) of the short trades of NYSE (Nasdaq) stocks executed at prices below the national best bid. Stoll and Schenzler (2006) examine why trades outside the quotes are observed. Their explanations include: (1) delays in reporting of trades; (2) execution delays because of the use of look-back options by Nasdaq dealers; and (3) large trades that exceed the quoted depth.

will give traders increased freedom to choose how aggressively they want their orders executed since market and marketable limit orders will no longer be treated as de facto limit orders with limit prices above the bid. Furthermore, even quote-improving and at-the-quote limit orders are likely to receive quicker execution since Alexander and Peterson (1999) show that such orders receive slower execution than similar regular sell orders when the uptick rule is in place.

Additional NYSE analysis reveals that, relative to control stocks, the pilot stocks have similar quoted and effective spreads but significantly (1) smaller short trade sizes, (2) more short trades, (3) more short volume, and (4) smaller ask depths. The differences with regard to trade size and number of trades are consistent with an increase in "order splitting" by large informed short sellers of pilot stocks after the removal of the uptick rule as a means of disguising their intentions (Boehmer, Jones and Zhang, 2007).

As a follow-up to the examination of the impact of the uptick rule on effective spreads and depths, we observe a significant increase in the frequency of short orders that are executed below the midpoint for the pilot stocks after the rule is suspended. In contrast, control stocks have more short trades that execute above the spread midpoint and are often equivalent to at-the-quote or quote-improving limit orders. As a result, the typical bid/ask depth ratio is higher after the removal of the uptick rule. Hence, price tests should be viewed as a hindrance to execution that, by delaying execution, distort liquidity.[5]

Further evidence that short selling does not, on balance, hurt liquidity is provided by noting that short trades execute at prices that are, on average, above the quote midpoint even when the uptick rule is suspended, albeit by a smaller amount. Hence, the typical trade involving a short sale is buyer initiated in the sense of Lee and Ready (1991). Lastly, we do not find any evidence that the rule has resulted in pilot stocks having either increased price volatility or decreased price efficiency. Thus, from the evidence regarding volatility, efficiency, and liquidity, we believe that market quality on the NYSE has not been degraded by the suspension of the uptick rule.

With regard to Nasdaq, a previous study by Ferri, Cristophe and Angel (2004) indicates that the bid test has little effect on the execution of short orders. Furthermore, in a June 15, 2006 filing with the SEC (*Federal Register*, June 22, 2006, p. 35965), the National Association of Securities Dealers (NASD) notes that "several exchanges that trade Nasdaq securities do so with no short sale regulation, encouraging market participants to route short sale orders to their markets to avoid any regulatory restriction" Thus, it is not surprising that our analysis of short trades on Nasdaq indicates that the bid test is relatively inconsequential. More specifically, most differences are notably smaller and less frequently significant than those found for the pilot stocks listed on the NYSE. Hence, we believe that the suspension of the bid test has not materially changed trader behavior or degraded market quality on Nasdaq.

Lastly, we also examine those matched pairs where either the pilot or control stock is in the lowest 20% of the Russell 3000 by market capitalization and analyze them during the

[5]For the most part, the results reported in this paper are consistent with the analysis in Alexander and Peterson (2002) who examined the effect of a reduction in tick size on short selling on the NYSE. They find that a smaller tick size resulted in better performance for short sell market orders in that they executed more often and more quickly. The reason behind the improvement in market quality is that a smaller tick size reduces the effectiveness of the uptick rule by making it easier for these orders to step ahead of the inside ask. The same argument applies here, as without the rule it is reasonable to expect that short market orders will execute more often and more quickly.

days in the pre- and post-periods when the Russell 2000 declined in value by at least 1%. The motivation for this analysis is to see if small stocks react differently from the overall sample during periods of market stress.[6] However, since no substantive differences were observed, we conclude that the removal of price tests has had no deleterious effect on trader behavior and has not led to a decrease in market quality as measured by liquidity, price efficiency, or market volatility. The evidence therefore suggests unambiguously that such tests should be removed.

The remainder of the paper is organized as follows. Section 2 reviews the literature and Section 3 provides a description of the data and sample. Section 4 presents empirical analysis of trader behavior, and Section 5 continues the empirical analysis be examining various measures of market quality. Section 6 presents the conclusion.

2. Background information and relevant literature

2.1. Background information

The uptick rule, formally known as Rule 10a-1 since its creation in 1938, applies to exchange listed stocks. It states that short sales cannot be made (i) at a lower price than the previous price, known as a minus tick, or (ii) at the same price as the previous price if the last trade involving a different price involved a higher one, known as a zero-minus tick. For Nasdaq NMS securities the bid test, formally known as NASD Rule 3350 and implemented in 1998, prohibits short sales by NASD members at or below the current best bid when that bid is lower than the previous best bid. As mentioned earlier, these two restrictions on short sale executions are collectively referred to as price tests.

Among other things, Reg SHO establishes a pilot program allowing for the temporary suspension of price tests for pilot stocks.[7] The pilot program began on Monday, May 2, 2005 and initially was scheduled to end on Friday, April 28, 2006. However, the SEC (2006a) recently extended the end date to August 6, 2007. The SEC chose the pilot stocks from the set of Russell 3000 stocks, excluding 32 securities identified on June 24, 2004 that were neither (1) Nasdaq NMS securities, (2) listed on the AMEX, nor (3) listed on the NYSE. Also excluded were issuers whose initial public offerings took place after April 30, 2004. The remaining securities were sorted into three groups by marketplace and then ranked in descending order based on average daily dollar trading volume over the one year prior to the issuance of the order. Finally, every third stock in the ranking for each group was selected to be part of the pilot program where the first stock chosen was the 2nd, followed by the 5th, the 8th, and so on.

2.2. Literature

Macey, Mitchell and Netter (1989) provide a review of the legislative history of the uptick rule and examine it in the context of index arbitrage and portfolio insurance after the 1987

[6]This was mentioned as a source of concern at the Reg SHO Roundtable hosted by the SEC by Paul Irvine, Adam Reed, and others (SEC, 2006b, e.g., pp. 54–55, 74–75, 81–83).

[7]See SEC (2003, 2004a–c, 2006a) for a detailed history of the proposed rule changes and the specification of the pilot program.

crash.[8] They conclude that the execution delay costs of the uptick rule perhaps fall most on index arbitragers who rely on the ability to quickly trade large portfolios of stocks. Barclay (1989) suggests that the uptick rule is relatively impotent since traders will not have to wait very long for an uptick in order to execute trades. Alexander and Peterson (1999) use system order data from the NYSE during 1996 to test the objectives of the uptick rule. While they find that the rule prohibits short selling at the bid for most of the trading day, between 60% and 70% of the short market orders typically are executed. Such orders typically receive execution at a price above the current bid between six and ten minutes after the order is placed. In contrast, only 30%–50% of short limit orders execute, which are notably lower percentages than similar regular sell orders, and that unexecuted short sell orders of all order types typically make up the inside ask either in whole or in part.

Ferri, Christophe and Angel (2004) study the effectiveness of the bid test on Nasdaq stocks. They find that Nasdaq stocks not constrained by the bid test have similar levels of short selling as Nasdaq stocks that are constrained. Furthermore, they find a negative correlation between short selling and volatility, suggesting that the bid test adds no additional protections against abusive short sellers. Indeed, the authors surmise that short selling appears to have a stabilizing effect and the bid test itself appears to prevent stabilizing short selling activities.

Other daily short selling data have become available recently. For example, Boehmer, Jones and Zhang (2007) examine aggregate NYSE daily system order data over a four-year time period and find that 12.9% of the system volume involves a short seller. Stocks with heavy short selling are found to underperform stocks with light short selling. Finding that large-size short sales are the most informative, they suggest that short sellers possess short-term information and cannot afford to be patient in executing their orders by breaking them into smaller sizes and submitting them periodically, i.e., informed short sellers do not engage in "slicing and dicing."

Recently, there have been a number of papers describing an important component of the short selling process—the stock lending market. These papers include D'Avolio (2002), Geczy, Musto and Reed (2002), Duffie, Garleanu and Pedersen (2002), Jones and Lamont (2002), Asquith, Pathak and Ritter (2005), Cohen, Diether and Malloy (2006), and Nagel (2005). Most stock loan papers indicate that shorting is relatively inexpensive. For example, D'Avolio finds that (1) the average cost to borrow stock is only 25 basis points per year, with only 7% of the loan supply actually borrowed, (2) at most, 16% of stocks representing 1% of the value of the market cannot be borrowed, (3) 91% of the stocks, known as general collateral stocks, cost less than 1%/year to borrow, (4) 9% of the stocks are "on special" with average fees of 4.3%/year, (5) less than 1% of stocks on loan become extremely "special", (6) the probability of being on special decreases with size and institutional ownership, and (7) loan recall, at 2% of borrowed stock, is relatively rare.[9]

Nevertheless, Miller (1977) and Figlewski (1981) suggest that in the presence of short selling constraints, prices will be biased upwards because some bearish investors will be restricted from trading.[10] In this view, market prices may be informationally inefficient

[8]Also see SEC (2007) for the legislative history of price tests.

[9]Stocks "on special" are relatively hard to short; "general collateral" stocks are relatively easy to short.

[10]See Jones and Lamont (2002) for an examination of the relationship between short-sale constraints and stock returns.

because of these constraints. Furthermore, Diamond and Verrecchia (1987) show that such constraints imply an asymmetry in the speed of price adjustment to negative versus positive information.

Perhaps the most relevant research are studies by Diether, Lee, and Werner (DLW, 2007), Wu (2007), and the SEC's Office of Economic Analysis (OEA, 2007). All three of them also use Reg SHO data to analyze the effects on market quality associated with the temporary suspension of price tests on the NYSE and Nasdaq except for Wu, who only focuses on the NYSE. On the NYSE, all the studies find an increase in short trading volume for pilot stocks. Furthermore, suspension of the uptick rule is found to result in significantly (1) wider spreads, (2) thinner ask depths, and (3) lower execution prices. Importantly, DLW is the only study that finds evidence of a statistically significant increase in volatility using intraday returns on the NYSE.[11] These results are consistent with those reported in our paper with the exception of the last one. While we also observe an increase in volatility, in our analysis none of our tests indicate that the increase is significant. However, it should be noted that DLW describe their observed increase in volatility as "slight" and conclude that their "evidence does not suggest that Pilot stocks experience an increase in down-side volatility relative to Control stocks" (p. 29). Thus, even though DLW's study differs in research design and methodology from ours (they compare pre- and post-periods for pilot and control stocks but do not use matched pairs), their interpretation of their findings is consistent with our volatility results, leading us to view our results as being robust. Lastly, all studies find that the Nasdaq results are weak and often insignificant relative to those on the NYSE.

3. Sample and data description

3.1. Sample

Our initial analysis involves examining the effect of price tests on one measure of market quality—liquidity. More specifically, we look at short trading volume, number of short trades, short trade size, quoted and effective spreads, inside depths, price location, and price impact. Subsequently we focus on two other measures of market quality—efficiency and volatility. The basic research approach matches pilot stocks with a control group of stocks that are not part of the pilot program. That is, NYSE pilot stocks are matched with NYSE control stocks and Nasdaq pilot stocks are matched with Nasdaq control stocks. The advantage of this approach is that it allows us to compare pilot with control stocks that are similar on certain dimensions. While the disadvantage is that the resulting sample size is smaller since not all pilot stocks can be matched closely with control stocks, this is not severe since our sample of pilot stocks is 50% the size of the pilot stocks used in the DLW study, for example, where matching was not used.

Because a large majority of the stocks in the Russell 3000 are from either the NYSE or Nasdaq, stocks listed on the AMEX are excluded. Trading and microstructure data from four months before the start of the pilot program (January–April 2005) to four months afterwards (May–August 2005) are collected for all four samples of stocks. We omit one week before and after the start of the pilot program (i.e., April 25–29 and May 2–6, 2005)

[11] OEA (2007, p. 53) finds greater volatility only for small cap stocks.

in order to allow for adjustments that traders might make due to the suspension of the price tests.[12]

Matching of the Russell 3000 stocks that are listed on either the NYSE or Nasdaq was performed as follows. First, consider the 1,505 NYSE-listed stocks (1,456 Nasdaq-listed stocks) as of the weekend of April 30–May 1, 2005. The CRSP and Compustat databases were used to obtain for each stock its two-digit SIC industry code, 2004 trading volume, and December 31, 2004 share price, market capitalization, and book-to-market ratio. Removing stocks without these data reduced the number of NYSE-listed stocks to 1,465 (1,343 on Nasdaq). Of these stocks, pilot stocks and potential control stocks were required to have the same 2-digit SIC and have the same option listing status.[13] For those possible pairs sharing the same 2-digit SIC and traded option status, pilot and control stocks were matched (without replacement) by five financial measures: (1) year-end 2004 share price, (2) year-end 2004 market capitalization, (3) fiscal year-end 2004 book-to-market ratio, (4) rate of return for 2004, and (5) consolidated trading volume for 2004. This matching procedure basically follows that of Huang and Stoll (1996) but uses criteria established by Daniel, Grinblatt, Titman, and Wermers (1997) by calculating a Z-score for each pair of stocks ρ involving financial measure i as follows:

$$Z_{\rho i} = \{(F_{pi} - F_{ci})/[(F_{pi} + F_{ci})/2]\}^2 \; i = 1, 2, \ldots, 5, \tag{1}$$

where F_{pi} and F_{ci} are the measure for the pilot and control stocks, respectively. These five Z-scores are summed up to get an aggregate Z-score for the pair of stocks:

$$Z_\rho = Z_{\rho 1} + Z_{\rho 2} + Z_{\rho 3} + Z_{\rho 4} + Z_{\rho 5}. \tag{2}$$

In all, we were able to match 446 of the 488 NYSE-listed pilot stocks (390 of 439 from Nasdaq). From the list of matched pairs, the best 50%, or 223 NYSE matches (195 on Nasdaq) based on aggregate Z-scores are then considered.[14] The final NYSE sample includes 171 pairs of stocks with traded options, and 52 pairs of stocks without traded options, while the final Nasdaq sample includes 130 pairs of stocks with traded options and 65 pairs of stocks without traded options.

Table 1 presents the characteristics of the stocks in the study. For the full sample of 223 pairs of NYSE stocks and 195 pairs of Nasdaq stocks, the average Z-scores defining the quality of match are .65 and .52, respectively. Recalling that the stocks are also paired by 2-digit SIC codes and option trading status, it appears that pilot stocks are reasonably close matches with the control stocks. This can be seen by examining the column entitled 'Difference'. All but one of the differences are insignificant using t-tests or Wilcoxon signed-rank tests.[15] All of the tests in Table 1 were also performed separately for the

[12]We also analyzed just April and May in order to avoid the possible effect that the annual reconstituting of the Russell indices might have on the variables of interest. However, our results were essentially unchanged. See, e.g., Madhavan (2003) and Chen, Noronha, and Singal (2006) for a description and analysis of the reconstitution process.

[13]We match on options status because of the possible use of short selling as part of arbitrage strategies that are designed to capture put-call parity violations; see, e.g., Ofek, Richardson, and Whitelaw (2004). Options listing status was obtained from the Options Clearinghouse Corporation's website.

[14]Qualitatively similar results were obtained when we used the best 25% matches.

[15]The one that is statistically significant (NYSE book/market ratio) has an economically insignificant difference.

Table 1
Matched pair sample description as of December 31, 2004

Variable	Pilot	Control	Difference	*Z*-score full sample	*Z*-score small cap
Panel A. NYSE sample (223 pairs)					
Stock Price	$39.42	$37.57	$1.85	.12	.15
Market Capitalization	$6.27	$6.28	–$.01	.13	.13
Trading Volume	178.21	182.27	–4.06	.14	.20
Annual Stock Return	27.2%	26.9%	.4%	.15	.12
Book/Market Ratio	.47	.44	.02*	.11	.10
Aggregate *Z*-Score				.65	.69
Panel B. Nasdaq sample (195 pairs)					
Stock Price	$22.85	$23.30	–$.45	.09	.09
Market Capitalization	$1.67	$1.58	$.09	.10	.10
Trading Volume	230.49	211.04	19.45	.13	.15
Annual Stock Return	14.3%	13.2%	1.1%	.12	.11
Book/Market Ratio	.39	.40	–.01	.09	.09
Aggregate *Z*-Score				.52	.54

The sample is drawn from NYSE and Nasdaq stocks that were members of the Russell 3000 on May 1, 2005. The pilot stocks were chosen by the SEC. Matches were chosen from the remaining stocks with the requirement that the matches share the same 2-digit SICs (as designated in the CRSP database) and have the same option listing status. Possible pairs were matched by stock price as of December 31, 2004, market capitalization as of December 31, 2004 (measured in $billions), consolidated trading volume for 2004 (measured in millions of shares), book-to-market ratio as of December 31, 2004, and stock return for 2004. The matching procedure follows Huang and Stoll (1996) and the best 50% of matches were considered based on aggregate *Z*-scores. These scores for each variable were calculated as the difference in the values for the pilot and matched control stock, divided by the average of the two values with the resulting number being squared. The aggregate *Z*-score for a matched pair is the sum of the five *Z*-scores. The Small Cap sample is a subset of the matches in the full sample. All stocks in the Russell 3000 were sorted by market and ranked by market capitalization. A matched pair was included in the subsample if either the pilot or the control was in the lowest 20% of market capitalization. Cross-sectional averages are reported. Differences are tested using *t*-tests with **/* indicating significance at 1%/5% level and Wilcoxon signed rank tests with ††/† indicating significance at the 1%/5% level.

options and the no-options sub-samples but the results are not displayed here to save space, as both sub-sample test results were similar.

OEA (2007, p. 61) reports various statistics on the pilot and control stocks involved in Reg SHO in the four months before the start of the pilot. Focusing on pilot stocks, the average price and market capitalization of the NYSE stocks is reported as $38.43 and $7.01 billion, respectively. From Table 1, it can be seen that the matched sample is close in terms of price ($39.42), but has a slightly smaller market capitalization ($6.27 billion).[16] On Nasdaq, OEA reports the pilot stocks have an average price of $23.01 and market capitalization of $1.63 billion. Here the matched sample corresponds even more closely with an average price of $22.85 and market capitalization of $1.67 billion. Overall, the matched samples are representative.

As mentioned earlier, we also separately examined a subset of our sample stocks that focused on the smallest firms. In constructing this subset of stocks, for each market we

[16]Given the concern with smaller stocks, the difference in market capitalizations is not a concern; see footnote 6.

ranked the Russell 3000 stocks by market capitalization. A matched pair was admitted to the subset if either the pilot or the control stock was ranked in the smallest 20% of market capitalization. This subset of stocks, referred to as the Small Cap sample, was examined during the days in the pre- and post-periods when the Russell 2000 declined in value by at least 1% due to concerns that they would be more severely affected relative to larger cap stocks during periods of market stress. Table 1 indicates that the *Z*-scores of the Small Cap sample are similar to the *Z*-scores of the full sample.

3.2. Test approach

The purpose of this study is to see if certain measures of trader behavior and market quality are materially affected by the suspension of short selling price tests that became effective on Monday May 2, 2005. Denote the values of such a variable for pilot stock i during: (1) the *pre-period* of January 3–April 22, 2005 as ${}_iP_{pre}$, and (2) the *post-period* of May 9–August 31, 2005 as ${}_iP_{post}$. A time-series comparison of ${}_iP_{post}$ with ${}_iP_{pre}$ for a sample of pilot stocks is likely to be problematic due to the possibility of a systemic change in the variable between the two periods that would confound analysis of the suspension of the price tests on the variable.[17]

Alternatively, one could undertake a cross-sectional comparison by taking the variable's value for the matching control stock in the post-period, denoted ${}_iC_{post}$, and compare ${}_iP_{post}$ with ${}_iC_{post}$. Since it is impossible to create a large well-matched paired sample that controls for all the variables except for the one under examination, such analysis is also problematic.

However, our use of a sample of matched pairs that involves a combined time-series and cross-sectional comparison where changes between the two periods in the pilot stocks, $({}_iP_{post}-{}_iP_{pre})$, are compared with the changes in the control stocks, $({}_iC_{post}-{}_iC_{pre})$, resolves these difficulties. Hence, our analysis is based on an analysis of sample values of pilot stock changes less control stock changes, i.e., $({}_iP_{post}-{}_iP_{pre})-({}_iC_{post}-{}_iC_{pre})$, which we refer to as the "difference of differences." Note that if this difference of differences is positive, then the sample value for the pilot stocks increased relative to the control stocks after the suspension of price tests. Analogously, a negative difference of differences indicates the pilot stocks had a relative decrease in the sample value.

In our tables when we report a difference of a percent change in a variable, the differences in pilots and controls are winsorized at 1%. This is done because of the potential for extreme outliers due to small values in the denominators when computing percent changes. Significance levels of both *t*-tests and Wilcoxon signed rank tests on differences are reported, and provide remarkably similar results.

3.3. Data description

All short trades of NYSE- and Nasdaq-listed pilot and control stocks that are executed on US markets are initially considered in our analysis. To isolate the effects of the price tests on the normal trading of NYSE stocks, we exclude all trades that occur at or prior to

[17]Analysis of the Russell 3000 indicates that average stock returns were negative in the pre-period and positive in the post-period, indicating a systemic change took place between these two periods.

the opening (as the opening resembles an auction) and after the close. For Nasdaq stocks we exclude trades before 9:30AM and after 4:00PM.

The Reg SHO data include the following fields: market center, stock symbol, trade date, trade time, short type (exempt or non-exempt from the applicable price test), size (number of shares in the trade), price, link indicator (indicator of special conditions), and short size (number of shares sold short). The short type indicator is used so the market maker can recognize which trades are, for whatever reason, exempt from a price test.[18] Unfortunately, in the post-period many trades of pilot stocks continued to be marked as if they were non-exempt from a price test when in fact all trades of these stocks were exempt due to their designation as pilot stocks.[19] For example, in the post-period the pilot stocks in our NYSE sample had a non-exempt short volume of 2.7 billion shares and an exempt short volume of 850 million shares. We cannot determine if the post-period short trades of pilot stocks that were properly marked as exempt would have been exempt had the uptick rule not been suspended, as in early 2005 the SEC issued no-action letters to the Securities Industry Association that gave broker-dealers substantially greater latitude in marking short sales as exempt or non-exempt.[20] Accordingly, during both the pre- and post-periods, all short exempt and non-exempt trades are pooled and are referred to as short sales in our analysis. Importantly, this procedure assumes that the relative frequency of exempt short sales is similar for pilot and control stocks during the pre- and post-periods.[21]

4. Effect of price tests on trader behavior

4.1. Trading activity

We now turn to Table 2 where Panel A shows that the average NYSE pilot stock, relative to the matched control stocks, experienced significant increases of 10.8% in short trading volume and 30.2% in the number of short sale trades (hereafter we will refer to such changes as simply ‘‘relative’’ increases or decreases).[22] In contrast, Panel B shows that Nasdaq pilot stocks had insignificant relative increases of 4.9% in short trading volume and 5.8% in the number of short sale trades.

Given these results, it is not surprising that there is a significant relative decline of 10.1% in the average short trade size on the NYSE but an insignificant 1.2% decline on Nasdaq. This observation is also consistent with the conjecture of Boehmer, Jones, and Zhang

[18]For example, trades that involve ‘‘bona fide arbitrage’’ are exempt from the uptick rule.

[19]We verified this by computing the proportion of NYSE short sales of pilot stocks with short type indicated as non-exempt that executed on a minus tick or a zero minus tick during May. In all, about 40% of such non-exempt short trades executed on a minus tick or zero minus tick.

[20]See Brigagliano (January 3, 2005 and April 15, 2005).

[21]The exempt short sales we refer to here are those that are exempt because they involve, for example, arbitrage, not because they are exempt due to the suspension of price tests. DLW (2007) similarly analyze all short trades; Wu (2007) also analyzes all short trades in her examination of the uptick rule, but when analyzing ‘‘the informedness of shorters’’ (p. 18), differentiates between exempt and non-exempt short sales; OEA (2007) is silent on whether exempt short sales are included or excluded.

[22]It is possible that the significant relative increase in short trading volume of pilot stocks on the NYSE is due to traders shifting at least some of their short selling from control stocks to these stocks. However, it is also possible that the increase is due to the attractiveness of receiving quicker execution of short sell orders involving pilot stocks while short trading in the control stocks is unaffected by Reg SHO. Determining which one of these potential explanations predominates is beyond the scope of this paper. See footnote 3.

Table 2
Short trading volume, number of short trades, average short trade size, and short/long ratio

Variable	Pilot stocks			Control stocks			Difference of differences (%)	Difference of differences (small cap) (%)
	Pre	Post	% Difference	Pre	Post	% Difference		
Panel A: NYSE stocks								
Volume	191	195	6.3*	198	181	–4.5*††	10.8**††	2.0
No. Trades	417	533	32.5**††	418	410	2.3	30.2**††	46.3**††
Trade Size	374	322	–14.6**††	382	367	–4.5**††	–10.1**††	–24.3**††
Short/Long	37	41	14.3**††	37	35	–.8	15.1**††	29.4**††
Panel B: Nasdaq stocks								
Volume	305	270	5.6	290	265	.7	4.9	–17.7
No. Trades	871	816	7.1**	815	755	1.4	5.8	8.7
Trade Size	232	222	–3.3**††	242	232	–2.1*††	–1.2	–15.6**†
Short/Long	66	75	16.4**††	64	64	3.5	12.9**††	–5.3

This table reports the cross-sectional daily average short trading volume, number of short trades, average short trade size, and the short/long ratio for the sample of stocks described in Table 1. *Pre* denotes the pre-period of January 1, 2005 to April 22, 2005; *Post* denotes the post-period of May 9, 2005 to August 31, 2005. All trades of NYSE stocks before the NYSE opening and after the close are excluded. All trades of Nasdaq stocks before 9:30 and after 4:00 are excluded. *Volume* refers to the average consolidated daily short volume in 1,000s; Reg SHO data sources are used to compute the short volume. *No. Trades* refers to the average number of short trades per day. *Short/Long* refers to the short volume divided by the long volume × 100. The values in the column headed by *Difference of Differences (SmallCap)* refer to the 37 NYSE (35 Nasdaq) pairs where both the pilot and control stocks are members of the Russell 2000 using data drawn from days when the Russell 2000 declined by 1% or more. The % differences for pilot and control stocks are winsorized at the 1% level. Differences are tested using *t*-tests with **/* indicating significance at 1%/5% level and Wilcoxon signed rank tests with ††/† indicating significance at the 1%/5% level.

(2007) that large informed NYSE short sellers will tend to split their orders more frequently when they do not have to contend with delayed execution due to the uptick rule.

Lastly, the ratio of short trading volume to long trading volume (i.e., trades that do not involve short selling) had a relative significant increase of 15.1% on the NYSE and 12.9% on Nasdaq. This result is hardly surprising for the NYSE given its relative increase in short trading volume. However, it is surprising for Nasdaq in light of the observation that its short trading volume did not have a relative increase as it suggests that long trading volume declined for Nasdaq pilot stocks in the post period but not for Nasdaq control stocks.

4.2. Spreads and depths

Table 2 indicates that short traders of pilot stocks on the NYSE changed their trading behavior upon the removal of uptick rule restrictions, tending to execute more short trades of smaller size. The marginal short trader of pilot stocks may have also changed order type as well. To test this we examine the changes in the proportionate quoted and effective bid-ask spread from the pre-period to the post-period (hereafter simply referred to as quoted and effective spreads). These two spread measures are calculated as $(Ask\text{–}Bid)/MP$ and $2D(P\text{–}MP)/MP$, respectively, where *Ask* is the national best ask, *Bid* is the national best

Table 3
Spreads and depths

Variable	Pilot stocks			Control stocks			Difference of differences	Difference of differences (small cap)
	Pre	Post	Difference	Pre	Post	Difference		
Panel A: NYSE stocks								
Quoted Spread	13.36	13.83	.47*	13.59	12.85	–.74**††	1.21**††	4.17*††
Effective Spread	8.06	8.05	–.01	8.35	7.65	–.70**††	.69**††	2.10*†
Bid Size	8.45	9.89	–1.3%††	9.04	9.12	1.2%	–2.5%	.0%
Ask Size	11.14	9.09	–22.4%**††	11.66	11.71	1.0%	–23.5%**††	–23.0%**††
BSize/ASize	0.88	1.12	28.8%**††	.89	.90	2.3%*	26.5%**††	30.4%**††
Panel B: Nasdaq stocks								
Quoted Spread	25.77	25.78	.01†	26.48	26.94	0.46	–.46	–2.51
Effective Spread	24.95	20.50	–4.45**††	25.83	21.72	–4.11**††	–.34	–2.28
Bid Size	18.23	19.87	14.1%**††	18.43	23.90	23.7%**††	–9.5%**	7.0%
Ask Size	18.10	18.99	14.1%**††	18.18	22.42	23.2%**††	–9.1%**†	.4%
BSize/ASize	1.14	1.16	2.5%**†	1.13	1.16	3.5%**†	–1.0%	11.8%

This table reports the cross-sectional average time-weighted quoted spreads and depths and cross-sectional average trade-weighted effective spreads for the sample of stocks described in Table 1. *Pre* denotes the pre-period of January 1, 2005 to April 22, 2005; *Post* denotes the post-period of May 9, 2005 to August 31, 2005. *Quoted Spread* is 2(Ask – Bid)/(Ask + Bid), measured in basis points. *Bid Size* and *Ask Size* are depths and are in units of 100 s of shares. *BSize/ASize* refers to the bid size depth divided by the ask size depth. The average *Effective Spread* (= 2 × D × [P – MP]/MP) where D is the trade direction indicator, P is the price, and MP is the quote midpoint, is computed daily, with the daily values averaged by stock, then across stocks. The Effective Spread is reported in basis points. The values in the column headed by *Difference of Differences (SmallCap)* refer to the 37 NYSE (35 Nasdaq) pairs where both the pilot and control stocks are members of the Russell 2000 using data drawn from days when the Russell 2000 declined by 1% or more. The % differences for the *Bid Size*, *Ask Size*, and *BSize/ASize* variables are winsorized at the 1% level. Differences are tested using *t*-tests with **/* indicating significance at 1%/5% level and Wilcoxon signed rank tests with ††/† indicating significance at the 1%/5% level.

bid, D is -1 for seller-initiated trades and $+1$ for buyer-initiated trades as defined by Lee and Ready (1991) without a lag (Peterson and Sirri, 2003), P is the execution price, and $MP = (Ask+Bid)/2$ is the quote midpoint at the time of execution (see, e.g., Bessembinder, 2003).

In Panels A and B of Table 3 we report the cross-sectional average time-weighted quoted spreads and trade-weighted effective spreads for NYSE and Nasdaq, respectively.[23] That is, the average is calculated over the sample period for each stock, and then the averages themselves are averaged. The quoted spread had a relative increase of 1.21 basis points on the NYSE but a relative decrease of .46 basis points on Nasdaq. The effective spread had a relative increase of .69 basis points on the NYSE and decrease of .34 basis points on Nasdaq. While the results on the NYSE are statistically significant, at less than one basis point (or the equivalent of less than one cent for a $100-stock) the magnitudes are so small as to make them economically insignificant.

Quoted and effective spreads are but one dimension of liquidity. To obtain a fuller picture of liquidity the cross-sectional average time-weighted bid and ask depths at the

[23] Trade weighting is used for effective spreads since the significant average trade-size decline from the pre-period to the post-period shown in Table 2 leaves open the possibility that volume weighting is misleading.

NBBO were computed and are also shown in Table 3.[24] Panel A indicates that for pilot stocks on the NYSE, the bid and ask sizes had relative decreases of 2.5% and 23.5%, respectively, with only the latter being significant. It is also interesting to observe that the ask depth is notably greater than the bid depth in the pre-period for NYSE pilot stocks, but in the post-period the bid depth is slightly larger than the ask depth. One possible explanation is that stocks were, in general, falling in price in the pre-period and rising in the post-period. However, the control stocks show the ask depth being notably greater than the bid depth in both periods. It follows that the ratio of bid-to-ask depths increased significantly, with at least part of this increase due to the suspension of the uptick rule.

The results on Nasdaq have two interesting differences from NYSE. First, the bid and ask sizes are of similar size for the pilot stocks in each period, suggesting that the bid test is not resulting in market and marketable limit orders turning into at-the-quote limit orders. Second, the 9.5% relative decline in the bid size is of larger magnitude than the 2.5% relative decline on the NYSE, and is significant. However, the relative 9.1% decline in the ask depth is of similar magnitude to the relative decline in the bid depth. Thus, it is not surprising that the bid-to-ask ratio did not change and that the declines in Nasdaq depths were not due to the suspension of the bid test. In sum, Nasdaq's bid test has much less of an effect on depths than NYSE's uptick rule.

The NYSE results in Table 3 clearly show the implications of the uptick rule. In the presence of the uptick rule, short market and marketable limit orders frequently cannot execute at the bid (Alexander and Peterson, 1999). In turn, these orders become de facto limit orders with the limit price equal to the minimum shortable price (''MSP''), which can be either quote-improving or at-the-quote limit orders. The net result is that the rule delays the execution of these short orders, leading to larger depths at the ask (note that quote-improving limit orders can also become de facto at-the-quote limit orders). Without the uptick rule, these trading distortions are removed, resulting in the bid and ask depths being more symmetric.

4.3. *Effective spreads for trades initiated by short sellers*

Quoted spreads are typically thought of as inferior to effective spreads as a measure of liquidity. Hence, we report the cross-sectional average trade-weighted effective spreads for trades initiated by short sellers, defined in a manner similar to Lee and Ready (1991) as short trades that take place at a price below the quote midpoint, in Panels A and B of Table 4. Here we find that the effective spreads for short sales had a significant relative decrease of .8 basis points on the NYSE but an insignificant increase of 1.3 basis points on Nasdaq. While the Nasdaq results are not surprising given the general ineffectiveness of the bid test, the NYSE results are surprising even though the decrease is not economically significant. This decrease suggests that these trades are taking place slightly closer to the midpoint when the uptick rule is suspended. However, it is reasonable to expect that an increased percentage of pilot stock short trades take place below the midpoint when the uptick rule is suspended even if there is no change in the relative frequencies of the various types of short orders being submitted. Table 4 shows that this is what has happened, as the percentage increased significantly by 16.3% on the NYSE and 1.9% on Nasdaq.

[24]The depth is defined as the aggregate number of shares bid or offered across all markets that are at the National Best Bid and Offer (NBBO).

Table 4
Effective spreads and price locations of trades involving short sales

Variable	Pilot stocks			Control stocks			Difference of differences	Difference of differences (small cap)
	Pre	Post	Difference	Pre	Post	Difference		
Panel A: NYSE stocks								
Effective Spread	10.7	8.7	–2.0**††	10.8	9.6	–1.2**††	–.8*††	–3.9
Trades < Midpoint	24.5	39.8	15.3**††	23.2	22.3	–1.0**††	16.3**††	21.6**††
ES (P < MP)	8.4	8.7	.3	8.5	8.1	–.3††	.6*††	3.8**††
ES (P > MP)	9.4	9.0	–.4**††	9.5	9.1	–.4**††	.0	.5
Price Location	46.1	11.3	–34.8**††	46.9	48.0	1.1**††	–35.9**††	–47.1**††
Pr{P < MP}	22.7	43.6	20.9**††	23.2	23.5	.3	20.5**††	24.7**††
Pr{P > MP}	77.3	56.4	–20.9**††	76.8	76.5	–.3	–20.5**††	–24.7**††
Panel B: Nasdaq stocks								
Effective Spread	38.5	20.7	–17.7**††	41.7	22.7	–19.0**††	1.3	–3.8
Trades < Midpoint	39.6	40.4	.8**††	39.3	38.2	–1.1**††	1.9**††	3.9
ES (P < MP)	58.8	21.3	–37.4**††	56.7	22.7	–34.0**††	–3.4	1.9
ES (P > MP)	65.4	22.8	–42.6**††	56.3	35.3	–21.0††	–21.5	–.8
Price Location	15.4	12.5	–2.9**††	16.1	17.0	.9	–3.9**††	–7.4
Pr{P < MP}	40.9	43.1	2.1**††	41.6	41.8	.2	2.0**††	7.3*†
Pr{P > MP}	59.1	56.9	–2.1**††	58.4	58.2	–.2	–2.0**††	–7.3*†

This table reports the average daily cross-sectional trade-weighted effective spreads and price location for the stocks described in Table 1. *Pre* denotes the pre-period of January 1, 2005 to April 22, 2005; *Post* denotes the post-period of May 9, 2005 to August 31, 2005. *Effective Spread* (= 2 × [MP – P]/MP) is computed for short trades occurring at prices (P) below the quote midpoint (MP) at the time of the trade and is measured in basis points. *ES (P < MP)* and *ES (P > MP)* refer to the estimated effective spread (*ESPREAD*) in basis points for short trades using the following model:

$$\begin{aligned} ESPREAD = {} & \gamma_0 + \gamma_1 D_1 + \gamma_2 D_2 + \gamma_3 D_1 D_2 + \gamma_4 D_3 + \gamma_5 D_1 D_3 + \gamma_6 D_2 D_3 + \gamma_7 D_1 D_2 D_3 \\ & + \gamma_8 TradeSize + \gamma_9 D_1 TradeSize + \gamma_{10} D_2 TradeSize + \gamma_{11} D_1 D_2 TradeSize \\ & + \gamma_{12} D_3 TradeSize + \gamma_{13} D_1 D_3 TradeSize + \gamma_{14} D_2 D_3 TradeSize + \gamma_{15} D_1 D_2 D_3 TradeSize \\ & + \gamma_{16} Return5 + \gamma_{17} D_1 Return5 + \gamma_{18} D_2 Return5 + \gamma_{19} D_1 D_2 Return5 \\ & + \gamma_{20} D_3 Return5 + \gamma_{21} D_1 D_3 Return5 + \gamma_{22} D_2 D_3 Return5 + \gamma_{23} D_1 D_2 D_3 Return5 \\ & + \gamma_{24} DepthRatio + \gamma_{25} D_1 DepthRatio + \gamma_{26} D_2 DepthRatio + \gamma_{27} D_1 D_2 DepthRatio \\ & + \gamma_{28} D_3 DepthRatio + \gamma_{29} D_1 D_3 DepthRatio + \gamma_{30} D_2 D_3 DepthRatio + \gamma_{31} D_1 D_2 D_3 DepthRatio \\ & + \gamma_{32} ShortVol5 + \gamma_{33} D_1 ShortVol5 + \gamma_{34} D_2 ShortVol5 + \gamma_{35} D_1 D_2 ShortVol5 \\ & + \gamma_{36} D_3 ShortVol5 + \gamma_{37} D_1 D_3 ShortVol5 + \gamma_{38} D_2 D_3 ShortVol5 + \gamma_{39} D_1 D_2 D_3 ShortVol5 \\ & + \gamma_{40} QSpread + \gamma_{41} D_1 QSpread + \gamma_{42} D_2 QSpread + \gamma_{43} D_1 D_2 QSpread \\ & + \gamma_{44} D_3 QSpread + \gamma_{45} D_1 D_3 QSpread + \gamma_{46} D_2 D_3 QSpread + \gamma_{47} D_1 D_2 D_3 QSpread + \varepsilon. \end{aligned}$$

This model was estimated applying OLS regression to all short trades that do not execute at the quote midpoint for each match of pilot and control stocks. $D_1 = 1$ if post period, 0 otherwise. $D_2 = 1$ if pilot, 0 otherwise. $D_3 = 1$ if price > midpoint, 0 otherwise. *Return5* is the natural log of the ratio of quote midpoints observed at execution time and five minutes earlier. *DepthRatio* is the ratio of the depth at the bid to the depth at the ask at execution time. *ShortVol5* is the short volume in the previous five minutes. *QSpread* is the NBBO spread at execution time. Using the coefficients from the regressions and setting the variables to their averages, the effective spreads were computed for each match and the average effective spreads are reported and tested in the cross-section. *Trades < Midpoint* is the cross-sectional average percentage of short trades that took place at a price below the quote midpoint and is reported in percent. *Price Location* (= 2 × [P−MP]/[Ask−Bid]) is based on all short trades and is multiplied by 100. Effective spreads and price locations are trade-weighted by day, then averaged across days. Trades with price location greater than 1 are set to 1. Trades with price location less than −1 are set to −1. The probabilities of a short trade executing below (Pr{P < MP}) or above (Pr{P > MP}) the quote midpoint are

Table 4 (*continued*)

estimated using probit regression of the following form:

$$\begin{aligned}PLOC = {} & \gamma_0 + \gamma_1 D_1 + \gamma_2 D_2 + \gamma_3 D_1 D_2 \\ & + \gamma_4 TradeSize + \gamma_5 D_1 TradeSize + \gamma_6 D_2 TradeSize + \gamma_7 D_1 D_2 TradeSize \\ & + \gamma_8 Return5 + \gamma_9 D_1 Return5 + \gamma_{10} D_2 Return5 + \gamma_{11} D_1 D_2 Return5 \\ & + \gamma_{12} DepthRatio + \gamma_{13} D_1 DepthRatio + \gamma_{14} D_2 DepthRatio + \gamma_{15} D_1 D_2 DepthRatio \\ & + \gamma_{16} ShortVol5 + \gamma_{17} D_1 ShortVol5 + \gamma_{18} D_2 DShortVol5 + \gamma_{19} D_1 D_2 ShortVol5 \\ & + \gamma_{20} QSpread + \gamma_{21} D_1 QSpread + \gamma_{22} D_2 QSpread + \gamma_{23} D_1 D_2 QSpread + \varepsilon,\end{aligned}$$

where price location (*PLOC*) is set to one of two values: less than the midpoint or greater than the midpoint. Trades at the midpoint were excluded. The probit was estimated for each match of pilot and control stocks. Using the coefficients from the probits and setting the variables to their averages, the probabilities were computed for each match and the average probabilities are reported and tested in the cross-section. The values in the column headed by *Difference of Differences (SmallCap)* refer to the 37 NYSE (35 Nasdaq) pairs where both the pilot and control stocks are members of the Russell 2000 using data drawn from days when the Russell 2000 declined by 1% or more. Differences are tested using *t*-tests with **/* indicating significance at 1%/5% level and Wilcoxon signed rank tests with ††/† indicating significance at the 1%/5% level.

Since there may be other factors that contribute to effective spreads, such as intraday market movements, OLS regressions that are designed to isolate the effect of removing price tests on effective spreads (*ESPREAD*) of all trades of a given matched pair of stocks that involve a short sale except for those taking place at the quote midpoint were estimated:

$$\begin{aligned}ESPREAD = {} & \gamma_0 + \gamma_1 D_1 + \gamma_2 D_2 + \gamma_3 D_1 D_2 + \gamma_4 D_3 + \gamma_5 D_1 D_3 + \gamma_6 D_2 D_3 + \gamma_7 D_1 D_2 D_3 \\ & + \gamma_8 TradeSize + \gamma_9 D_1 TradeSize + \gamma_{10} D_2 TradeSize \\ & + \gamma_{11} D_1 D_2 TradeSize + \gamma_{12} D_3 TradeSize + \gamma_{13} D_1 D_3 TradeSize \\ & + \gamma_{14} D_2 D_3 TradeSize + \gamma_{15} D_1 D_2 D_3 TradeSize + \gamma_{16} Return5 \\ & + \gamma_{17} D_1 Return5 + \gamma_{18} D_2 Return5 + \gamma_{19} D_1 D_2 Return5 \\ & + \gamma_{20} D_3 Return5 + \gamma_{21} D_1 D_3 Return5 + \gamma_{22} D_2 D_3 Return5 \\ & + \gamma_{23} D_1 D_2 D_3 Return5 + \gamma_{24} DepthRatio + \gamma_{25} D_1 DepthRatio \\ & + \gamma_{26} D_2 DepthRatio + \gamma_{27} D_1 D_2 DepthRatio + \gamma_{28} D_3 DepthRatio \\ & + \gamma_{29} D_1 D_3 DepthRatio + \gamma_{30} D_2 D_3 DepthRatio + \gamma_{31} D_1 D_2 D_3 DepthRatio \\ & + \gamma_{32} ShortVol5 + \gamma_{33} D_1 ShortVol5 + \gamma_{34} D_2 ShortVol5 \\ & + \gamma_{35} D_1 D_2 ShortVol5 + \gamma_{36} D_3 ShortVol5 + \gamma_{37} D_1 D_3 ShortVol5 \\ & + \gamma_{38} D_2 D_3 ShortVol5 + \gamma_{39} D_1 D_2 D_3 ShortVol5 + \gamma_{40} QSpread \\ & + \gamma_{41} D_1 QSpread + \gamma_{42} D_2 QSpread + \gamma_{43} D_1 D_2 QSpread \\ & + \gamma_{44} D_3 QSpread + \gamma_{45} D_1 D_3 QSpread + \gamma_{46} D_2 D_3 QSpread \\ & + \gamma_{47} D_1 D_2 D_3 QSpread + \varepsilon. \end{aligned} \tag{3}$$

Here the dummy variable D_1 was set equal to 1 if the trade was in the post-period and 0 otherwise; dummy variable D_2 was set equal to 1 if the trade involved the pilot stock and 0 if it involved the matching control stock; dummy variable D_3 was set equal to 1 if the trade price is above the quote midpoint that is measured at the time of the trade's execution and 0 if it is below the midpoint. Five control variables are used: *TradeSize* denotes the size of the trade in shares; *Return5* denotes the natural log of the ratio of the quote midpoints observed at execution time and five minutes earlier; *DepthRatio* is the ratio of bid depth to ask depth at execution time; *ShortVol5* is the short trading volume during the five minutes before the execution time; and *QSpread* is the NBBO quoted spread at execution time.

The following chart is useful in identifying the effective spread of a typical trade after allowing for the control variables:

	$P<$Midpoint ($D_3=0$)	$P>$Midpoint ($D_3=1$)
Control Pre ($D_1=0$; $D_2=0$)	γ_0	$\gamma_0+\gamma_4$
Control Post ($D_1=1$; $D_2=0$)	$\gamma_0+\gamma_1$	$\gamma_0+\gamma_1+\gamma_4+\gamma_5$
Pilot Pre ($D_1=0$; $D_2=1$)	$\gamma_0+\gamma_2$	$\gamma_0+\gamma_2+\gamma_4+\gamma_6$
Pilot Post ($D_1=1$; $D_2=1$)	$\gamma_0+\gamma_1+\gamma_2+\gamma_3$	$\gamma_0+\gamma_1+\gamma_2+\gamma_3+\gamma_4+\gamma_5+\gamma_6+\gamma_7$
$\Delta ESPREAD_{control}$	γ_1	$\gamma_1+\gamma_5$
$\Delta ESPREAD_{pilot}$	$\gamma_1+\gamma_3$	$\gamma_1+\gamma_3+\gamma_5+\gamma_7$
$\Delta ESPREAD_{pilot}$–$\Delta ESPREAD_{control}$	γ_3	$\gamma_3+\gamma_7$

As the chart shows, γ_3 and the sum γ_3+γ_7 are of interest in analyzing the effect of the suspension of price tests on effective spreads. In doing so, the averages of the independent variables were used to estimate the marginal effective spreads of each pilot stock in the pre- and post-periods. However, two cases are examined for each period—those involving short sales that are executed below the quote midpoint and those that are executed above the midpoint, as shown in the middle and right-hand column of the above chart. These two cases are analyzed first for pilot stocks and then for control stocks. The difference in these effective spreads was calculated and tested with the results given in the third and fourth lines in Panel A for the NYSE and in Panel B for Nasdaq, followed by tests of the difference in differences between pilot and control stocks, of Table 4.

Consider first short sales that were executed below the quote midpoint. Interestingly, on the NYSE the relative effective spreads for these trades increased on the NYSE by .6 basis points but decreased on Nasdaq by 3.4 basis points. While only the NYSE results are statistically significant, at less than one basis point they again do not appear to be economically significant.

When it comes to short sales that were executed above the quote midpoint, the effective spread did not change for the pilot stocks relative to the control stocks on the NYSE while on Nasdaq the relative spread decreased by an insignificant 21.5 basis points. Thus, the suspension of price tests has not resulted in a substantive change in these measures of liquidity.

4.4. Price location

We next consider the implications of price tests as to the execution prices of all trades involving short sales. Price location is defined as *2(P– MP)/(Ask– Bid)*. Panels A and B of Table 4 reveal that the cross-sectional average trade-weighted price location declined for pilot stocks for both the NYSE and Nasdaq after the price tests were removed, indicating that short trades, on average, took place at lower prices in the post-period. Furthermore, the magnitude of the decline was much larger for stocks on the NYSE (35.9%) than

Nasdaq (3.9%), indicating once again that the NYSE's uptick rule has more of an effect on short trading than Nasdaq's bid test.

Interestingly, even with a lower trade price in the post-period, short sales of stocks in the pilot program continued to have a positive (but smaller) average price location on both markets, 11.3 on NYSE, 12.5 on Nasdaq. This result indicates that short sellers are, on balance, less aggressive relative to other sellers in seeking quick execution even in the absence of price tests, possibly due to a relative increase in the use of quote-improving or at-the-quote limit orders by short sellers after the removal of these tests.

4.5. Probit analysis of price location

In order to isolate the effect of the removal of price tests on price location (*PLOC*), probit regressions were estimated using all short trades that did not take place at the midpoint for each matched pair of stocks:

$$\begin{aligned} PLOC = {} & \gamma_0 + \gamma_1 D_1 + \gamma_2 D_2 + \gamma_3 D_1 D_2 \\ & + \gamma_4 TradeSize + \gamma_5 D_1 TradeSize + \gamma_6 D_2 TradeSize + \gamma_7 D_1 D_2 TradeSize \\ & + \gamma_8 Return5 + \gamma_9 D_1 Return5 + \gamma_{10} D_2 Return5 + \gamma_{11} D_1 D_2 Return5 \\ & + \gamma_{12} DepthRatio + \gamma_{13} D_1 DepthRatio + \gamma_{14} D_2 DepthRatio \\ & + \gamma_{15} D_1 D_2 DepthRatio + \gamma_{16} ShortVol5 + \gamma_{17} D_1 ShortVol5 \\ & + \gamma_{18} D_2 ShortVol5 + \gamma_{19} D_1 D_2 ShortVol5 + \gamma_{20} QSpread \\ & + \gamma_{21} D_1 QSpread + \gamma_{22} D_2 QSpread + \gamma_{23} D_1 D_2 QSpread + \varepsilon. \end{aligned} \tag{4}$$

Specifically, *PLOC* was set equal to one of two values for each trade depending on whether the execution price of the trade was (1) less than the midpoint, or (2) greater than the midpoint, where the midpoint is measured at execution time. The dummy variables D_1 and D_2, along with the five control variables, are defined as in Eq. (3).

Note that the change in the price location for a pilot stock between the two periods is:

$$\begin{aligned} \Delta PLOC_{pilot} = {} & (\gamma_1 + \gamma_3) + (\gamma_5 + \gamma_7) TradeSize + (\gamma_9 + \gamma_{11}) Return5 \\ & + (\gamma_{13} + \gamma_{15}) DepthRatio + (\gamma_{17} + \gamma_{19}) ShortVol5 \\ & + (\gamma_{21} + \gamma_{23}) QSpread + \varepsilon, \end{aligned} \tag{5}$$

indicating that the change in price location, after allowing for control variables, is $(\gamma_1+\gamma_3)$. Similarly, the change in price location for a control stock is

$$\begin{aligned} \Delta PLOC_{control} = {} & \gamma_1 + \gamma_5 TradeSize + \gamma_9 \mathrm{R}eturn5 + \gamma_{13} DepthRatio \\ & + \gamma_{17} ShortVol5 + \gamma_{21} QSpread + \varepsilon, \end{aligned} \tag{6}$$

indicating that the change in price location, after allowing for control variables, is γ_1. Thus, the change in the price location for the pilot, relative to the control, is equal to:

$$\begin{aligned} \Delta PLOC_{pilot} - \Delta PLOC_{control} = {} & \gamma_3 + \gamma_7 TradeSize + \gamma_{11} Return5 + \gamma_{15} DepthRatio \\ & + \gamma_{19} ShortVol5 + \gamma_{23} QSpread + \varepsilon, \end{aligned} \tag{7}$$

indicating that γ_3 is the key coefficient.

However, according to Greene (2003, pp. 736–740), interpreting the marginal effect of the estimated coefficient of a probit regression must be done carefully. Hence, to determine these effects we estimated the probabilities of each price location for four cases involving each matched pair using the averages of the independent variables. The four cases consisted of (1) trades of the pilot stocks in the pre-period, (2) trades of the pilot stocks in the post-period, (3) trades of the control stocks in the pre-period, and (4) trades of the control stocks in the post-period. Next, we subtracted the estimated probabilities of the pre-period pilot stocks from the estimated probabilities of the post-period pilot stocks at each price location and tested the differences for significance. This procedure was repeated for the control stocks, and then for the difference of differences between the pilot and control stocks.

Interestingly, the cross-sectional average predicted probabilities, displayed in the last two lines in each panel of Table 4, are significant for both the NYSE and Nasdaq pilots as well as in the Difference of Differences column. More specifically, they indicate that short sales of stocks in the pilot program tended to execute at lower prices in the post-period relative to the control stocks. That is, the typical short sale of a pilot stock on the NYSE during the pilot program had a 20.9% greater likelihood of executing at or below the midpoint, naturally offset by a 20.9% smaller likelihood of being executed above the midpoint. Each of these differences is significant. For Nasdaq, the typical short sale of a pilot stock during the pilot program had a 2.1% greater likelihood of executing below the midpoint, again offset by a 2.1% smaller likelihood of being executed above the midpoint. These results remain at similar percentages after adjusting for changes in the control sample, and continue to be statistically significant, thus confirming the earlier price location results found in Table 4. While the results on the NYSE are economically significant, they are not so on Nasdaq. In sum, this evidence indicates that short sellers are more aggressive in placing orders that receive quicker execution on the NYSE when the uptick test is suspended even though overall, the average price location of short trades remains above (but closer to) the midpoint.

5. Price impacts, volatility, and efficiency

We now proceed to examine the effect that price tests have on the price impact of short trades, followed by their effect of stock price volatility and market efficiency.

5.1. Price impact

The top row of each panel in Table 5 reports the cross-sectional average trade-weighted price impacts for all trades involving short sales on the NYSE and Nasdaq, respectively. The price impact is estimated as $2D(MP5-MP)/MP$, where $MP5$ is the midpoint measured five minutes following the trade; D is defined as it was earlier for effective spreads. The results indicate that the price impact of short sales had a relatively insignificant decrease for both NYSE and Nasdaq. Thus, short sellers were apparently not moving prices any differently when the price tests were suspended.[25] However, as noted for

[25] An interesting observation is that the price impact on Nasdaq is roughly twice as large as on the NYSE. This can be attributed, at least in part, to the Nasdaq sample involving substantially smaller cap stocks (see Table 1) since Bessembinder (2003, Table 7) shows that price impacts of large-cap NYSE stocks are smaller than those of medium- and small-cap Nasdaq stocks. Furthermore, previously reported results (e.g., Bessembinder, (2003; Boehmer, 2005; Stoll, 2000) that indicate larger price impacts on the NYSE relative to Nasdaq were based on all trades and thus are not informative regarding relative price impacts for trades involving just short sales. The

Table 5
Price impacts of trades involving short sales

Variable	Pilot stocks			Control stocks			Difference of differences	Difference of differences (small cap)
	Pre	Post	Difference	Pre	Post	Difference		
Panel A: NYSE stocks								
Price Impact	4.45	4.18	–.27**††	4.56	4.37	–.20	–.08	1.85
PI ($P<$MP)	–1.02	3.43	4.45**††	–.53	–.80	–.27	4.72**††	11.21**††
PI ($P>$MP)	6.03	5.08	–.95**††	6.06	5.88	–.18	–.77**††	–2.03
Panel B: Nasdaq stocks								
Price Impact	13.40	12.05	–1.35**††	13.67	12.73	–.94*††	–.41	.22
PI ($P<$MP)	13.27	12.02	–1.25	13.72	10.44	–3.28**††	2.03††	–3.11
PI ($P>$MP)	16.35	14.11	–2.24**††	15.97	15.61	–.36	–1.88	–6.43

This table reports the average daily cross-sectional trade-weighted price impact for the stocks described in Table 1. *Pre* denotes the pre-period of January 1, 2005 to April 22, 2005; *Post* denotes the post-period of May 9, 2005 to August 31, 2005. *Price Impact* ($= 2 \times \mathrm{D} \times [\mathrm{MP5}-\mathrm{MP}]/\mathrm{MP}$) is computed for all short trades and D is the trade-side indicator, and is reported in basis points. *PI (P<MP)* and *PI (P>MP)* refer to the estimated price impact (*PIMPACT*) in basis points of short trades using the following model:

$$\begin{aligned}
PIMPACT = {} & \gamma_0 + \gamma_1 D_1 + \gamma_2 D_2 + \gamma_3 D_1 D_2 + \gamma_4 D_3 + \gamma_5 D_1 D_3 + \gamma_6 D_2 D_3 + \gamma_7 D_1 D_2 D_3 \\
& + \gamma_8 TradeSize + \gamma_9 D_1 TradeSize + \gamma_{10} D_2 TradeSize + \gamma_{11} D_1 D_2 TradeSize \\
& + \gamma_{12} D_3 TradeSize + \gamma_{13} D_1 D_3 TradeSize + \gamma_{14} D_2 D_3 TradeSize + \gamma_{15} D_1 D_2 D_3 TradeSize \\
& + \gamma_{16} Return5 + \gamma_{17} D_1 Return5 + \gamma_{18} D_2 Return5 + \gamma_{19} D_1 D_2 Return5 \\
& + \gamma_{20} D_3 Return5 + \gamma_{21} D_1 D_3 Return5 + \gamma_{22} D_2 D_3 Return5 + \gamma_{23} D_1 D_2 D_3 Return5 \\
& + \gamma_{24} DepthRatio + \gamma_{25} D_1 DepthRatio + \gamma_{26} D_2 DepthRatio + \gamma_{27} D_1 D_2 DepthRatio \\
& + \gamma_{28} D_3 DepthRatio + \gamma_{29} D_1 D_3 DepthRatio + \gamma_{30} D_2 D_3 DepthRatio + \gamma_{31} D_1 D_2 D_3 DepthRatio \\
& + \gamma_{32} ShortVol5 + \gamma_{33} D_1 ShortVol5 + \gamma_{34} D_2 ShortVol5 + \gamma_{35} D_1 D_2 ShortVol5 \\
& + \gamma_{36} D_3 ShortVol5 + \gamma_{37} D_1 D_3 ShortVol5 + \gamma_{38} D_2 D_3 ShortVol5 + \gamma_{39} D_1 D_2 D_3 ShortVol5 \\
& + \gamma_{40} QSpread + \gamma_{41} D_1 QSpread + \gamma_{42} D_2 QSpread + \gamma_{43} D_1 D_2 QSpread \\
& + \gamma_{44} D_3 QSpread + \gamma_{45} D_1 D_3 QSpread + \gamma_{46} D_2 D_3 QSpread + \gamma_{47} D_1 D_2 D_3 QSpread + \varepsilon.
\end{aligned}$$

This model was estimated applying OLS regression to all short trades that do not execute at the quote midpoint for each match of pilot and control stocks. $D_1 = 1$ if post period, 0 otherwise. $D_2 = 1$ if pilot, 0 otherwise. $D_3 = 1$ if price > midpoint, 0 otherwise. *Return5* is the natural log of the ratio of quote midpoints observed at execution time and five minutes earlier. *DepthRatio* is the ratio of the depth at the bid to the depth at the ask at execution time. *ShortVol5* is the short volume in the previous five minutes. *QSpread* is the NBBO spread at execution time. Using the coefficients from the regressions and setting the variables to their averages, the price impacts were computed for each match and the average price impacts are reported and tested in the cross-section. The values in the column headed by *Difference of Differences (SmallCap)* refer to the 37 NYSE (35 Nasdaq) pairs where both the pilot and control stocks are members of the Russell 2000 using data drawn from days when the Russell 2000 declined by 1% or more. Differences are tested using *t*-tests with **/* indicating significance at 1%/5% level and Wilcoxon signed rank tests with ††/† indicating significance at the 1%/5% level.

effective spreads, the significant relative increase in the percentage of trades executing at prices below the midpoint for pilot stocks suggests that it is appropriate to examine

(*footnote continued*)
results reported in Table 5 suggest that, unlike regular sales, short sales are more informative on Nasdaq than the NYSE.

separately the price impacts of those trades involving short sales that took place below the midpoint from those that took place above the midpoint. This is done next in a framework identical to that used in equation (3) for analyzing effective spreads.

5.2. Regression analysis of price impact

The difference-of-differences results given in the first line of both panels of Table 5 indicate that short sales of pilot and control stocks have similar price impacts on both the NYSE and Nasdaq while controlling for the matching factors given in Table 1. However, there may be other factors that contribute to price impacts, such as intraday market movements. Accordingly, an OLS regression was run to analyze price impacts (*PIMPACT*) of all trades involving a short sale (except for those taking place at the quote midpoint). For each matched pair of stocks it uses the same independent variables as in Eq. (3) in order to isolate the effect of price tests on the price impact of short trades:

$$\begin{aligned} PIMPACT = {} & \gamma_0 + \gamma_1 D_1 + \gamma_2 D_2 + \gamma_3 D_1 D_2 + \gamma_4 D_3 + \gamma_5 D_1 D_3 + \gamma_6 D_2 D_3 + \gamma_7 D_1 D_2 D_3 \\ & + \gamma_8 TradeSize + \gamma_9 D_1 TradeSize + \gamma_{10} D_2 TradeSize \\ & + \gamma_{11} D_1 D_2 TradeSize + \gamma_{12} D_3 TradeSize + \gamma_{13} D_1 D_3 TradeSize \\ & + \gamma_{14} D_2 D_3 TradeSize + \gamma_{15} D_1 D_2 D_3 TradeSize + \gamma_{16} Return5 \\ & + \gamma_{17} D_1 Return5 + \gamma_{18} D_2 Return5 + \gamma_{19} D_1 D_2 Return5 \\ & + \gamma_{20} D_3 Return5 + \gamma_{21} D_1 D_3 Return5 + \gamma_{22} D_2 D_3 Return5 \\ & + \gamma_{23} D_1 D_2 D_3 Return5 + \gamma_{24} DepthRatio + \gamma_{25} D_1 DepthRatio \\ & + \gamma_{26} D_2 DepthRatio + \gamma_{27} D_1 D_2 DepthRatio + \gamma_{28} D_3 DepthRatio \\ & + \gamma_{29} D_1 D_3 DepthRatio + \gamma_{30} D_2 D_3 DepthRatio + \gamma_{31} D_1 D_2 D_3 DepthRatio \\ & + \gamma_{32} ShortVol5 + \gamma_{33} D_1 ShortVol5 + \gamma_{34} D_2 ShortVol5 \\ & + \gamma_{35} D_1 D_2 ShortVol5 + \gamma_{36} D_3 ShortVol5 + \gamma_{37} D_1 D_3 ShortVol5 \\ & + \gamma_{38} D_2 D_3 ShortVol5 + \gamma_{39} D_1 D_2 D_3 ShortVol5 + \gamma_{40} QSpread \\ & + \gamma_{41} D_1 QSpread + \gamma_{42} D_2 QSpread + \gamma_{43} D_1 D_2 QSpread \\ & + \gamma_{44} D_3 QSpread + \gamma_{45} D_1 D_3 QSpread + \gamma_{46} D_2 D_3 QSpread \\ & + \gamma_{47} D_1 D_2 D_3 QSpread + \varepsilon. \end{aligned} \tag{8}$$

The same chart following equation (3) is useful in identifying the price impact of a typical trade after allowing for the control variables except that the last three rows now represent $\Delta PIMPACT_{control}$, $\Delta PIMPACT_{pilot}$, and $\Delta PIMPACT_{pilot}-\Delta PIMPACT_{control}$, respectively. As the chart shows, γ_3 and the sum $\gamma_3+\gamma_7$ are of interest in analyzing the effect of the suspension of price tests on price impacts.

As with the effective spread equations, the averages of the independent variables were used to estimate the marginal price impacts of each pilot stock in the pre- and post-periods where two cases are examined for each period, first for pilot stocks and then for control stocks. These two cases involve short sales that were executed below the quote midpoint and those executed above the midpoint, with the midpoint being measured at execution time. The difference in these price impacts was calculated and tested, with the results given in the last two lines in Panel A for the NYSE and in Panel B for Nasdaq, followed by tests of the difference of differences between pilot and control stocks, of Table 5.

Consider first short sales that were executed below the quote midpoint. Interestingly, on the NYSE the midpoint was slightly higher five minutes after execution for pilot stocks in the pre-period and for control stocks in both periods, as indicated by their negative values. However, for NYSE pilot stocks in the post-period, the midpoint fell after execution as indicated by its positive value. Furthermore, this fall in price is significantly different than both the rise in the pre-period and the change in the typical control stock, resulting in a net relative fall of 4.72 basis points. This observation suggests that short sale orders seeking relatively faster execution are more informative on the NYSE when price tests are suspended, which is not surprising since market and marketable limit orders (and even quote-improving and at-the quote limit orders) can be executed more quickly without the hindrance of the uptick rule. While slightly less than half as large at 2.03 basis points, a similar observation is apparent on Nasdaq.

When it comes to short orders executed above the quote midpoint, all the pre- and post-period entries are positive on both exchanges, indicating that the midpoint had moved up five minutes after execution. On the NYSE, however, the midpoint moved up by a significantly smaller amount, .77 basis points, in the post-period after adjusting for the control stocks. The results for this case suggest that less aggressive short sale orders that receive relatively slower execution tend to mitigate some of the upward movement in the midpoint associated with buyer-initiated trades and thereby contribute to price discovery when the uptick rule is suspended. However, the economic significance of this observation is quite minor given the percentage change in price impact is less than one basis point. While a similar pattern is observed on Nasdaq, the smaller upward movement in the quote midpoint is insignificant.

5.3. Market efficiency

The previous results suggest that price discovery associated with market and marketable limit short sell orders is improved for such orders that are executed below the midpoint when price tests are suspended. Hence, it can be argued that removing price tests will improve market efficiency since information will be more quickly reflected in security prices. More specifically, negative information held by informed traders might not be reflected in security prices as quickly when price tests are in effect, as short sell orders will receive delayed execution as shown by Alexander and Peterson (1999) relative to regular sell orders of the same type. However, this is an empirical question that we address with two tests.

One method of testing this conjecture is based on Fama's (1976, Chapters 4, 5) suggestion that return autocorrelations can be used to judge market efficiency under the assumption that equilibrium expected returns are constant over the sample period. More specifically, if price tests hinder price discovery, then one might expect that the absolute value of the return autocorrelations will decrease for pilot stocks relative to control stocks.

The average absolute value of the autocorrelation coefficients of returns, computed using quote midpoints over 5-minute intervals excluding overnight returns, is reported in the first row of Panels A and B of Table 6. The difference in the change in autocorrelation coefficients between pilot and control stocks is positive and statistically significant for the NYSE sample. However, the magnitude of the relative increase, .012, is economically insignificant (the numbers displayed in the table have been

Table 6
Market efficiency measures

Variable	Pilot stocks			Control stocks			Difference of differences	Difference of differences (small cap)
	Pre	Post	Difference	Pre	Post	Difference		
Panel A: NYSE stocks								
|Autocorr. (5 min)|	5.7	6.2	.5	6.1	5.4	–.6*†	1.2**††	3.0
|Autocorr. (30 min)|	4.3	4.7	.4	4.4	4.8	.4	.0	–2.4
|Autocorr. (60 min)|	6.1	5.4	–.7	6.1	6.0	–.2	–.6	3.5
Up-Down R^2	–.3	1.3	1.7**††	–.6	1.0	1.6**††	.1	.1
Panel B: Nasdaq stocks								
|Autocorr. (5 min)|	4.7	3.9	–.8**††	4.2	3.7	–.4	–.4	.0
|Autocorr. (30 min)|	4.9	4.4	–.5	5.3	5.1	–.2	–.2	2.6
|Autocorr. (60 min)|	6.1	5.6	–.5	6.6	6.9	.3	–.8	4.8
Up-Down R^2	–.4	.3	.7**††	–.4	.4	.8**††	.0	.6

This table reports cross-sectional average market efficiency measures for the sample of stocks described in Table 1. *Pre* denotes the pre-period of January 1, 2005 to April 22, 2005; *Post* denotes the post-period of May 9, 2005 to August 31, 2005. |*Autocorrelation*| is calculated using quote midpoints to determine intraday returns for a given stock over the pre-period and post-period with a 5/30/60 min sampling frequency, ignoring overnight returns. The results have been multiplied by 100. *Up-Down* R^2 is calculated using two market model regressions for each stock where one regression is estimated when the market return was non-negative, and the other regression is estimated when then market return was negative; the Russell 3000 exchange traded fund was used as the market proxy. Returns are computed at 5 min intervals. The R^2 values were retained and the columns labeled *Pre* and *Post* correspond to the difference between the upside and downside R^2s during the respective time periods. These values have been multiplied by 100. The values in the column headed by *Difference of Differences (SmallCap)* refer to the 37 NYSE (35 Nasdaq) pairs where both the pilot and control stocks are members of the Russell 2000 using data drawn from days when the Russell 2000 declined by 1% or more. Differences are tested using *t*-tests with **/* indicating significance at 1%/5% level and Wilcoxon signed rank tests with ††/† indicating significance at the 1%/5% level.

multiplied by 100). Furthermore, the change on Nasdaq is $-.004$, which is not significant. Thus, the evidence is consistent with price tests not having a material effect on market efficiency.

However, it is possible that a large number of these 5-minute returns are zero due to the quotes not changing. If this is so, it will bias the autocorrelations toward zero, and thus toward having differences of zero. Upon examination, roughly 10% of all NYSE returns used in calculating these autocorrelations were zero on the NYSE, while roughly 20% of the Nasdaq returns were zero. Hence, 30-minute returns were examined, where roughly 3% of NYSE returns and 6% of Nasdaq returns were zero, with the percentages dropping by about an additional 1% on NYSE and 2% on Nasdaq when 60-minute returns were examined. Therefore, we also report the results using returns measured over these longer time intervals as a robustness check, and again find insignificant changes.

The autocorrelation test results, along with the price impact results in Table 6, lend themselves to comparison in the following manner. A stock that experiences an increase in price impact after the suspension of price tests should experience a decline in absolute autocorrelation since price discovery is arguably improved, leading to a lower absolute autocorrelation that is associated with a more efficient market. Accordingly, the correlation of the difference of differences for price impact and absolute autocorrelation was calculated. The resulting Spearman correlation coefficient was, at –.12, marginally significant at the .10 level on the NYSE, providing some evidence that prices are more informative after the suspension of the uptick rule. However, the coefficient for Nasdaq was, at .05, slightly but insignificantly positive.

In an examination of short selling in various countries, Bris, Goetzmann, and Zhu (2007) use an "Up–Down R^2" measure to assess market efficiency. This measure is computed by estimating two market model regressions of individual stock returns. One regression includes only those observations with non-negative market returns and one regression includes only those with negative market returns. The difference in the R^2 values for each stock is the variable of interest. In the presence of price tests, there should be less idiosyncratic risk incorporated into prices conditional on negative information if the tests significantly restrict short sales. That is, price discovery is asymmetric, being quicker for positive information relative to negative information. Therefore the "Down R^2" value should be higher than the "Up R^2" value when short sales are restricted by such tests, resulting in a negative Up–Down R^2 value.

To estimate Up–Down R^2, the Russell 3000 exchange traded fund was used as a proxy for the market return. As above, returns were measured using quote midpoints at five-minute intervals, ignoring overnight returns. Table 6 reports that the Up–Down R^2 values are negative in the pre-period for both NYSE and Nasdaq pilot stocks, as expected. This observation implies that the removal of price tests should result in more idiosyncratic risk being incorporated into prices on the downside, with a post-period value that is closer to zero. However, this is not readily apparent from the tables since the difference-of-differences column indicates statistically insignificant changes on both the NYSE and Nasdaq. Hence, these tests confirm the return autocorrelation tests in that they also provide no evidence that the removal of price tests has had a material effect on market efficiency.[26]

5.4. Volatility

In an effort to examine whether short selling could be responsible for increased volatility of stock prices, we performed tests of five volatility measures. Three of the measures use intraday returns, one measure uses daily price ranges, and another one uses option prices to determine implied volatilities, with the results reported in Table 7. First, the realized volatility, estimated as the sum of squared returns measured at 5-minute intervals, is used to determine if the absence of a price test has a material effect on the volatility.[27] The data

[26]Given the concerns expressed by the SEC about price efficiency (2004a, p. 4) when price tests are absent, we also compared the frequency of downward price runs after a short sale in April with those in May by dividing each trading day into half-hour intervals and identifying the time and price of the first short sale within each time interval. Next, we noted if the next trade was at a price below that of the short sale; if so we proceeded to the subsequent trade price, but if not we stopped and moved to the next half-hour interval. The process was continued for up to five trades after the initial short sale or the end of the half-hour time interval, whichever came first. In general, there was no significant difference between the relative frequency of price runs in the post-period relative to the pre-period for the pilot stocks in comparison to the control stocks on either the NYSE or Nasdaq, consistent with the notion that there is no material change in market efficiency when price tests are removed.

[27]Poon and Granger (2005) note that the most accurate forecasts of future volatility based on realized volatilities involve the use returns based on 5- or 15-minute intervals; also, see Andersen and Bollerslev (1998). Repeating our tests using fifteen-minute returns produced similar results to those reported using 5-minute returns.

Table 7
Stock return volatility

Variable	Pilot stocks			Control stocks			Difference of differences (%')	Difference of differences (small cap) (%)
	Pre	Post	% Difference	Pre	Post	% Difference		
Panel A: NYSE stocks								
Realized σ^2	2.5	2.2	–11.0**††	2.5	2.2	–14.0**††	3.0	5.3
Semi-variance	1.2	1.1	–10.6††	1.2	1.1	–14.3**††	3.7	2.2
MAD	11.3	10.7	–4.8**††	11.1	10.1	–9.0**††	4.2**††	6.0
Price range	2.2	2.0	–7.1**††	2.2	2.0	–8.4**††	1.3	3.9
Implied σ	.26	.24	–7.4**††	.27	.25	–5.1**††	–2.3	–5.9
Panel B: Nasdaq stocks								
Realized σ^2	5.8	4.6	–16.8**††	6.1	4.8	–16.5**††	–.3	0.8
Semi-variance	2.8	2.2	–17.1**††	3.0	2.3	–18.3**††	1.2	7.7
MAD	15.2	13.5	–10.3**††	15.4	13.4	–11.7**††	1.4	3.7
Price range	3.6	3.2	–9.9**††	3.6	3.2	–9.6**††	–.4	2.2
Implied σ	.44	.41	–5.0**††	.45	.42	–3.3†	–1.7	.6

This table reports five cross-sectional average measures of volatility for the stocks described in Table 1. *Pre* denotes the pre-period of January 1, 2005 to April 22, 2005; *Post* denotes the post-period of May 9, 2005 to August 31, 2005. *Realized σ^2* refers to the daily sum of squared returns computed over 5-minute intervals as $100 \times [(P_t)-(P_{t-1})]$, where $P_t = [\ln(Bid_t) + \ln(Ask_t)]/2$; overnight returns are excluded. These values are multiplied by 100. *Semi-variance* is computed over five-minute intervals, similar to the realized volatility, overnight returns are excluded. These values are multiplied by 10^4. *MAD* refers to mean absolute return deviation, and is the average of the absolute values of the five-minute returns; overnight returns are excluded. These values are multiplied by 10^4. *Price Range* is computed daily as [High–Low]/Close; these values are multiplied by 100. *Implied σ* from *IVolatility.com* measured on April 1 and August 5 using nearest maturity (April 16 and August 20) at-the-money call options. The *Difference of Differences* tests are based on 129 (92) pairs for NYSE (Nasdaq) where both members of each pair had trades in the at-the-money call options on both April 1 and August 5. The values in the column headed by *Difference of Differences (SmallCap)* refer to the 37 NYSE (35 Nasdaq) pairs where both the pilot and control stocks are members of the Russell 2000 using data drawn from days when the Russell 2000 declined by 1% or more; these implied volatility tests are based 4 (13) pairs for NYSE (Nasdaq). The % differences for pilot and control stocks are winsorized at the 1% level. Differences are tested using *t*-tests with **/* indicating significance at 1%/5% level and Wilcoxon signed rank tests with ††/† indicating significance at the 1%/5% level.

indicate that realized volatility of pilot stocks did not have a significant change on either the NYSE or Nasdaq.

Second, tests of semi-variance that based on five-minute returns are reported.[28] Like the realized volatility test results, the semi-variance did not have a significant change on either the NYSE or Nasdaq.

Third, tests of the average absolute return deviation (MAD) based on 5-minute returns are reported.[29] Like realized volatility, we assume the average return is zero in calculating MAD. Hence, this measure is simply the average of the absolute value of the 5-minute returns. While it had an insignificant increase of 1.4% on Nasdaq, its increase of 4.2% was

[28]Markowitz (1959, Chapter 11) discusses semi-variance as a measure of risk.

[29]Ederington and Guan (2006) propose using average absolute return deviation as a measure of historical volatility. We do not use the adjustment as suggested since it has no effect in our comparative statistical tests.

statistically significant on the NYSE. However, such a small percentage increase is of questionable economic significance.

Fourth, a test of the average daily relative price range is conducted. The daily relative price range is equal to the high price minus the low price, divided by the closing price.[30] Consistent with the previous results, there was an insignificant relative change on both the NYSE and Nasdaq.

Lastly, the implied volatility for the sub-samples of pilot and control stocks that have traded options was obtained from by *IVolatility.com*.[31] *IVolatility.com* uses the average of the closing NBBO quotes to compute the Black-Scholes implied volatility. We measure implied volatilities of April 16 and August 20 options on April 1 and August 5, i.e., 15 days before the expiration date.[32] Of all the call options, these ones have the shortest time to maturity from April 1 and August 5 since such contracts typically have the heaviest trading volume and thus the most reliable prices when it comes to computing implied volatilities according to Hull (2006, p. 301). August 5 was chosen as it was a day in the post-period of relative market stress as indicated by the 0.9% decline in the Russell 3000 on that day.[33] However, if the option for either the pilot or control stock was not trading on either the pre- or post-period date; the matched pair was deleted from the analysis; the result was a sample of 129 NYSE pairs and 92 Nasdaq pairs. Analysis of the implied volatilities indicates that they had an insignificant relative decrease on both the NYSE and Nasdaq.

In sum, while one statistically significant change in volatilities is observed, no clearly economically significant changes are revealed in our tests. Based on the five volatility measures in Table 7, we find no evidence that the removal of the price tests has an adverse effect on volatility.[34]

6. Conclusion

In this paper we use recently published data from a pilot program under Regulation SHO that temporarily suspends price tests for some stocks to examine how the tests affect (1) trader behavior and (2) market quality, which are areas of interest given by the SEC in deciding whether to permanently remove or amend these tests. After comparing a sample of matched pairs of pilot and control stocks, we conclude that the removal of price tests has had no deleterious effect on trader behavior and has not led to a decrease in market quality.

More specifically, we find that suspension of the uptick rule on the NYSE leads traders to place more executable short orders that are of smaller size. The resulting trades are typically executed at lower prices relative to the quotes, and have slightly greater price

[30] Boehmer (2005) uses daily relative price ranges as a measure of a stock's risk.

[31] Poon and Granger (2005) note that implied volatility measures dominate time-series measures in terms of forecasting accuracy.

[32] We also compared the pre-period April 16 options with those of a second post-period date, May 21, and obtained similar results.

[33] It is also past the time period that index reconstitution might have on stock prices; see Madhavan (2003) and Chen, Noronha, and Signal (2006). The largest decline in the post period was 1.2%; August 5th was the fifth largest decline. Importantly, the Russell 2000 declined by 1.4% on August 5th.

[34] Following Xu and Malkiel (2003), we also compared the squared residuals from regressions of daily returns using the Fama-French three-factor model and CAPM for April with those for May in order to examine firm-specific risks. The results indicated no statistically significant differences between the pilot and control stocks for either model.

impact. All of these results are consistent with an increase after the removal of price tests in "order splitting" by informed large short sellers of pilot stocks who seek to disguise their intentions as suggested by Boehmer, Jones, and Zhang (2007).

We also find significant declines in the ask depth occurring after price tests are removed on the NYSE and, consequently, an increase in the bid/ask depth ratio. Furthermore, the significant decrease in execution price after the removal of the uptick rule on pilot stocks can be attributed to an increased ability to trade more aggressively since short market or marketable limit orders can be executed immediately if they do not exceed the quoted bid depth. In contrast, stocks that trade with the uptick rule in place are traded less aggressively since they tend to execute above the bid more frequently and are often equivalent to de facto quote-improving or at-the-quote limit orders. Thus, the removal of price tests will give traders increased freedom to choose how quickly they want their orders to be executed.

Evidence that short selling without price tests does not, on balance, hurt liquidity is provided by noting that short trades on the NYSE execute at prices that are, on average, above the quote midpoint even after the suspension of price tests, albeit by a smaller amount. Thus, price tests should be viewed as a hindrance to execution that, by removing immediacy, distorts liquidity.

Similar results are observed for Nasdaq pilot stocks except that the differences in the number of short trades, short trade size, and bid/ask ratios are no longer significant. Furthermore, the effect on ask depths, execution prices, and price impacts are notably smaller. Thus, Nasdaq's bid test seems to be relatively inconsequential, a result that is hardly surprising since some exchanges do not enforce the bid test when trading Nasdaq-listed stocks.

As for other measures of market quality, we find no evidence of a significant change in either market volatility or market efficiency after the suspension of price tests on either the NYSE or Nasdaq. These findings are somewhat surprising, given the significant increase in the price impact of short trades that execute below the midpoint after the tests are suspended. However, this could be due to the fact that the increased impact is too subtle since (1) roughly 27% (40%) of the NYSE (Nasdaq) trades analyzed involve a short sale with less than half of them executing below the midpoint and (2) the average price impact of such short sales is only 4.72 (0.77) basis points higher on the NYSE (Nasdaq) when the tests are removed.[35] Importantly, the economic implications of these findings are that removal of price tests are not expected to lead to changes in the equity premium, nor such things as option pricing or the cost of capital.

Concern was expressed at the SEC's Roundtable on Reg SHO that price tests might be most beneficial for small cap stocks during times of market stress. However, replication of all our tests using only the days of greatest market stress (defined as the days when returns on the Russell 2000 dropped at least 1% in value) with just the small cap pilot and control stock pairs does not alter any of the conclusions reached with the full sample on all days.

In sum, the removal of price tests has had no deleterious effect on trader behavior and has not led to a decrease in market quality as measured by liquidity, price efficiency, or market volatility. The evidence therefore suggests unambiguously that such tests should be removed.

[35] OEA (2007, p. 61) similarly report that 27% (37%) of pilot and control trades on the NYSE (Nasdaq) is attributable to short selling during the first four months of 2005.

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