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Potential pilot problems: Treatment spillovers in financial regulatory experiments

Ekkehart Boehmer[a], Charles M. Jones[b], Xiaoyan Zhang[c,*]

[a] Lee Kong Chian School of Business, Singapore Management University, 50 Stamford Road, Singapore
[b] Columbia Business School, Columbia University, 3022 Broadway, New York, NY 10027, USA
[c] PBC School of Finance, Tsinghua University, 43 Chengfu Road, Beijing, China

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ABSTRACT

The total effects of a regulatory change consist of direct effects and indirect effects (spillovers), but the standard difference-in-difference approach mostly ignores potential indirect effects. During the 2007 full repeal of the uptick rule, short-sellers become much more aggressive across the board, even in control stocks where the uptick rule is already suspended. This finding is consistent with positive and significant indirect effects on control stocks, likely driven by aggressive broad list-based shorting. In contract, the indirect effect coefficients on shorting aggressiveness are negative for the 2005 partial uptick repeal, possibly due to substitutions between control and treatment stocks.

1. Introduction

Many financial regulatory policy changes are hard to study. For example, new rules are typically imposed on all firms at once. To gauge the effects of a new regime, a particularly useful approach for the regulator is to test out a new policy by conducting a randomized experiment. Randomized controlled trials are considered to be the gold standard for drawing unambiguous statistical conclusions about the effects of a rule change. By dividing firms or individuals into treatment and control groups at random, it becomes possible to isolate the average effects of the regulatory change by comparing average outcomes for the two groups.

In this paper, we highlight some of the pitfalls associated with even well-designed, essentially random pilot programs. We show that the total effect of a regulatory change can be decomposed into a direct effect and an indirect effect, or a so-called "spillover." Standard econometric techniques, such as difference-in-difference approaches, measure only the direct effect and could lead to the wrong conclusions. Indirect effects are due to externalities of some sort, where treatments have indirect effects on the control group. For instance, Miguel and Kremer (2004) face this issue in conducting randomized trials of various treatments to reduce worm infections in humans. The treated individual benefits from the direct effect of taking the drug, but treated individuals are also less likely to infect others in the same village. Thus, untreated individuals in the same Kenyan village (the control group) also benefit

* Corresponding author.
E-mail addresses: eboehmer@smu.edu.sg (E. Boehmer), cj88@gsb.columbia.edu (C.M. Jones), zhangxiaoyan@pbcsf.tsinghua.edu.cn (X. Zhang).

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from the treatment. This is an indirect effect or a spillover. Standard econometric techniques normally assume that the control group is unaffected, so a different econometric approach is required to assess these indirect effects.

Our subject for studying the indirect and direct effects of randomized regulation is the Regulation SHO pilot program conducted by the U.S. Securities and Exchange Commission (SEC) from 2005 to 2007. On the NYSE, short-sale price tests are also known as the "uptick rule." [1] The uptick rule requires short sales to take place on a strict uptick (at a price strictly higher than the last sale price) or on a zero-plus tick (where the price is equal to the last sale price but the most recent price change is positive).[2] The uptick rule was designed to limit shorting in declining markets, but after the minimum tick was narrowed to a penny in 2001, the uptick rule became a much smaller impediment to shorting. Also, as trading volumes exploded in the increasingly decentralized U.S. equity markets, it became more difficult for trading venues to ensure that a given short sale in fact took place on an uptick.

On July 28, 2004, as part of the adoption of Regulation SHO, a number of changes to short-sale regulations were announced, including a pilot program to suspend short-sale price tests in 1000 essentially randomly chosen stocks, namely, every third stock in the Russell 3000 index ranked by volume. The pilot program took effect in May 2005, and was expressly designed to allow the commission to study the effectiveness of the rule. We refer to this 2005 event as the "*2005 partial uptick repeal.*" Alexander and Peterson (2008) and Diether et al. (2009) study the 2005 partial uptick repeal and conclude that suspending the uptick rule has modest effects on bid-ask spreads and other measures of market quality. Both papers also predict that short-sellers would be more aggressive after the uptick rule is removed, but due to data limitations, these papers can provide only supportive rather than direct evidence.

On June 13, 2007, the SEC announced plans to eliminate all short-sale price tests, effective July 6, 2007. We refer to this event as the "*2007 full uptick repeal*," and it is the focus of our paper. We have access to detailed quote and order submission data, which allows us to construct direct measures of shorting aggressiveness. The uptick rule directly limits the aggressiveness of short-sellers, and thus shorting aggressiveness is the cleanest laboratory for our methodological investigation into direct and indirect effects of the rule change. Unconditionally, the full repeal frees short-sellers from the requirement to trade passively as liquidity providers, so we expect shorting aggressiveness to increase after the full repeal. Starting with standard econometric techniques, we find that the short-sellers become significantly more aggressive in non-pilot stocks (the treatment stocks) after the 2007 uptick repeal, as expected. More interestingly, we find that short-sellers become much more aggressive, and shorting activity increases substantially, for all stocks, even for the pilot stocks (the control stocks) where the uptick rule had been suspended since 2005. This is consistent with a positive spillover or a positive indirect effect.

One potential explanation for the 2007 increase in shorting aggressiveness among pilot stocks is that the full uptick repeal might enable aggressive, liquidity-demanding short sales of broad stock portfolios, such as index arbitrage. Broad stock portfolios, such as stock market indexes, normally include both pilot and non-pilot stocks. The removal of the uptick rule for all stocks in 2007 makes synchronous portfolio trading much easier and less costly to execute, and that is likely why we observe increases in short-selling aggressiveness for both pilot and non-pilot stocks. Using intraday data on stock returns and shorting activity measures, we directly examine the comovements between pilot and non-pilot stocks for 2007. We find that shorting activity and returns on non-pilot stocks co-move significantly more with pilot stocks right after the 2007 full uptick repeal, which supports the broad-portfolio trading hypothesis.

What happens for the full repeal in 2007 is in sharp contrast to what happens for the partial repeal in 2005. When the shorting restrictions disappear for one-third of stocks (the pilot stocks) in the Russell 3000 index in 2005, broad market short-selling remains relatively expensive to execute. With lower shorting cost on the pilot stocks than non-pilot stocks, we expect aggressive short-sellers to shift toward pilot stocks and away from non-pilot stocks. This substitution might increase the short aggressiveness towards treatment stocks (the pilot stocks), and reduce the short aggressiveness towards control stocks (the non-pilot stocks), which is consistent with a negative indirect effect.[3] We find substantial decreases in short-selling aggressiveness among the non-pilot stocks after the 2005 repeal, which indicates a negative indirect effect. By examining the intraday comovement in shorting and returns between pilot and non-pilot stocks after the 2005 partial repeal, we find that the shorting activity and return on non-pilot stocks co-move significantly less with pilot stocks after the partial repeal, which supports the substitution hypothesis that short-sellers favor pilot stocks over non-pilot stocks after the repeal.

Our paper is built on the literature studying the "stable unit treatment value assumption" (SUTVA). The SUTVA assumption is also referred to as the non-interference assumption, equivalent to assuming an indirect effect of zero. Many textbooks, such as Wooldridge (2010), discuss this issue. The importance of assuming "non-interference" (or SUTVA) when interpreting randomized experiments goes back to at least Rubin (1974) and has since been discussed in many specific settings, such as Angrist et al. (1996), Heckman et al. (1998), and Miguel and Kremer (2004). However, the SUTVA assumption is often overlooked by researchers in finance. In this paper, we build

[1] Short-sale price tests of a different form were also present in Nasdaq-traded stocks. But as noted by Diether et al. (2009), the price tests for Nasdaq-traded stocks could be easily circumvented. For this reason, we focus on the uptick rule as it applies to NYSE-listed stocks.

[2] See Jones (2012) for more details and an analysis of the introduction of the uptick rule and other U.S. short-sale regulatory changes that took place in the 1930s.

[3] Bessembinder et al. (2006) find a similar spillover around the initiation of the Trade Reporting and Compliance Engine (TRACE) system.

on the SUTVA literature and provide strong evidence that spillovers might exist and could be important in the context of changes in the uptick rule in 2005 and 2007.

Our paper is related to previous studies on uptick rule changes, such as Alexander and Peterson (2008) and Diether et al. (2009). There are two major differences between our paper and Alexander and Peterson (2008) and Diether et al. (2009). First, both of these papers focus on the 2005 partial uptick repeal, while we mainly examine the 2007 full uptick repeal. We also show that the SUTVA assumption might be violated in fundamentally different ways in 2007 vs. 2005. Second, both of the above papers focus on market quality measures, such as spreads, price impacts, volume, and volatility measures. We still examine these market quality measures for completeness, but our main focus is on short-selling activity, especially in terms of aggressiveness. We choose to concentrate on short-sale aggressiveness because we want to identify the specific changes in trading behavior associated with the regulations, and aggressiveness is a direct measure of short-selling activity and a strategic response to the regulation change.

To summarize, our study provides three unique contributions. First, we study how the 2007 full repeal of the uptick rule affected short-sale aggressiveness, and we provide evidence of a positive and significant indirect effect on pilot stocks. This means that a standard difference-in-difference analysis will understate the association between the tick rule and shorting aggressiveness. Second, we provide supportive evidence on the source of the positive indirect effect by examining the comovement of intraday shorting activity and returns. We find that the 2007 full repeal is associated with more comovement in shorting activity and returns between pilot and non-pilot stocks, consistent with more broad market list-based trading after the repeal. Third, we also investigate the 2005 partial repeal, and we provide evidence of a negative and significant indirect effect on the non-pilot stocks, which can be possibly attributed to substitution between treatment and control stocks. In this case, a standard difference-in-difference approach would overstate the effect of the rule change.

Our study has two limitations. First, we compute direct and indirect effects using our own specifications of what affects short aggressiveness around the regulation events. By doing so, there is a risk of misspecification. For instance, there might be confounding factors that are not included in our specification and they might affect the key measures before and after the events.[4] Moreover, there might be pretrends in the data that we could mislabel as treatment effects. To alleviate these concerns, we construct robustness checks with a selected set of market condition variables, and our main results stay similar. We also examine our data for pretrends, and find no evidence in this direction. The second limitation of our study is that even though we provide supportive evidence for the mechanisms of the positive and negative indirect effects in 2007 and 2005, we can never be completely sure about the exact mechanisms that are associated with the changes in the short aggressiveness, and we are open to criticisms. The purpose of the study is to show that there might be indirect effects in randomized pilot programs, and we believe that we achieve this goal.

The paper is organized as follows. Section 2 discusses estimation methodology, and Section 3 introduces the data. We present the empirical results on shorting aggressiveness in Section 4. Section 5 reports empirical results on market quality measures and returns. Section 6 concludes with some advice for those designing regulatory experiments in the future.

[4] We thank the referee for pointing out that the source of the confounding factor concern is that the indirect effect estimated in this paper boils down to a pre- vs. post-event comparison of outcomes for untreated stocks. This, in turn, could be confounded by any other factor related to shorting aggressiveness that happens to be changing around that time. The referee suggests that the ideal solution to this concern would be to find a more convincing counterfactual. In this setting, this would involve identifying a set of stocks that do not experience any indirect effect. Unfortunately, the two stages of the Reg SHO program affect all stocks traded in the U.S., and there is no plausible control group for estimating the indirect effect.

2. Estimation methodology

2.1. Defining direct and indirect effects

We adopt the potential outcomes framework of Rubin (1974), and we most closely follow the notation of Hudgens and Halloran (2008). Assume that there are N firms, and let $Y_i(T_i, \psi)$ be a random variable reflecting the potential outcome for firm i given its own treatment T_i. In our case, the treatment is binary, with $T_i = 1$ if firm i is subject to the regulatory change, and $T_i = 0$ otherwise. The fraction of firms treated in the randomized treatment assignment strategy is denoted by ψ. Taking expectations of the outcome variable, the individual direct treatment effect can then be defined as:

$$DE_i(\psi) = E[Y_i(T_i = 1, \psi) - Y_i(T_i = 0, \psi)]. \tag{1}$$

Recall that a single firm is either treated or not, so we observe only one of these quantities. The other is an unobserved counterfactual. Nevertheless, these potential outcomes can be averaged across the N firms to give the average direct treatment effect:

$$\begin{aligned} DE(\psi) &= \sum_{i=1}^{N} DE_i(\psi) \\ &= \sum_{i=1}^{N} E[Y_i(T_i = 1, \psi) - Y_i(T_i = 0, \psi)], \end{aligned} \tag{2}$$

for a given treatment strategy ψ. If firms are chosen randomly for treatment, the direct treatment effect can be estimated as the average outcome for treated firms less the average outcome for untreated firms.

Note that a treatment strategy ψ is often compared to an alternative treatment, ϕ. To compare two treatment strategies ψ and ϕ, we seek to measure the overall treatment effect:

$$\begin{aligned} TE(\psi,\phi) &= \sum_{i=1}^{N} E[Y_i(T_i=1,\psi)-Y_i(T_i=0,\phi)] \\ &= \sum_{i=1}^{N} E\{[Y_i(T_i=1,\psi)-Y_i(T_i=0,\psi)] \\ &\quad +[Y_i(T_i=0,\psi)-Y_i(T_i=0,\phi)]\} \\ &= DE(\psi,\phi)+IE_i(\psi,\phi). \end{aligned} \tag{3}$$

The first difference in the summation should be familiar as the direct treatment effect, and we can define the second difference in the summation to be the indirect treatment effect. This has the natural interpretation as the indirect effect or spillover on an untreated firm from changing the overall treatment strategy from ϕ to ψ. In economics, the indirect effect is sometimes called a treatment externality or general equilibrium effect, while in statistics, this effect is often referred to as interference. If the SUTVA holds, that is, if a unit's outcomes are unaffected by another unit's treatment assignment, then the indirect effect should be zero. But if the SUTVA assumption is violated, then the indirect effect might be nonzero, and inference based only on the direct effect might be biased.

2.2. Our model specification

Estimation of direct and indirect effects is the easiest when there are many different groups of subjects, with only within-group spillovers. Identification of direct and indirect treatment effects is then obtained by varying the fraction treated across groups. The problem in financial regulatory settings is that there is usually only one group or one financial market. This makes it more difficult (but not impossible) to identify direct or indirect effects. In the case of the Reg SHO pilot, we obtain identification using observations immediately before and after changes in the treatment policy along with control variables.

Given random assignment, each term of the direct effect can be consistently estimated using the mean time-series difference for the firms assigned to that group. That is, for the treated group ($T_i=1$), for each variable Y_i under investigation, we have:

$$E[Y_i(T_i=1,\psi)]=E\left[Y_i^{POST}|T_i=1,\psi\right]-E\left[Y_i^{PRE}|T_i=1,\phi\right], \tag{4}$$

and similarly for the untreated group ($T_i=0$):

$$E[Y_i(T_i=0,\psi)]=E\left[Y_i^{POST}|T_i=0,\psi\right]-E\left[Y_i^{PRE}|T_i=0,\phi\right], \tag{5}$$

where the two subtracted terms are the same in expectation due to randomization before treatment begins. Thus, in a randomized setting such as the Reg SHO pilot, an estimator for the direct effect is the standard difference-in-difference estimator.

Now consider the indirect effect. If ψ corresponds to the post-event treatment situation and ϕ is pre-event, then this can be estimated as the average change in the outcome variable for control stocks:

$$\begin{aligned} E[Y_i(T_i=0,\psi)-Y_i(T_i=0,\phi)] &= E\left[Y_i^{POST}|\ T_i=\ 0,\psi\right] \\ &\quad -E\left[Y_i^{PRE}|\ T_i=0,\phi\right]. \end{aligned} \tag{6}$$

At the beginning of the pilot in 2005, one-third of Russell 3000 stocks are selected for the regulatory treatment, essentially at random ($\psi=1/3$). Before the partial repeal begins, we measure outcome variables for pilot and non-pilot stocks, and at this time no firms are being treated, so $\phi=0$. After the partial repeal starts, we again measure the average outcome variables for both pilot and non-pilot stocks with $\psi=1/3$. For the full repeal of the uptick rule in 2007, before the event, 1/3 of the firms are treated, and after the repeal, all firms are treated. That is, $\phi=1/3$ and $\psi=1$. For comparison purposes, even though our main results are about the full uptick repeal in 2007, we also study the partial uptick repeal in 2005. To avoid confusion between the two settings, we always define the 1/3 pilot firms to have $T_i=1$, and the 2/3 non-pilot firms to have $T_i=0$.

We estimate our direct and indirect effect coefficients through the standard difference-in-difference specification:

$$Y_{it}=\beta_0+\beta_1A_t+\beta_2T_i+\beta_3A_tT_i+\varepsilon_{it}. \tag{7}$$

Variable A_t is an indicator variable that equals one if and only if the randomized treatment has occurred. The interaction term β_3 measures the direct treatment effect, and the coefficient β_1 measures the indirect treatment effect. Equivalently, the indirect effect coefficient is the average change in control firm outcome moving from the old to the new treatment strategy. Most studies using difference-in-difference focus on the direct effect coefficient, β_3, while our paper emphasizes the importance of the indirect effect coefficient, β_1.

For the 2005 partial repeal of the uptick rule, our specification is defined as in Eq. (7). For the 2007 full repeal of the uptick rule, we conduct a simple linear transformation of Eq. (7), because we set $T_i=1$ for pilot firms, which now become the control group, and $T_i=0$ for non-pilot firms, which now are the treatment group. In this case, the direct effect coefficient on the treatment group becomes $-\beta_3$, and the indirect effect coefficient becomes $\beta_1+\beta_3$.

Unfortunately, if anything else comes along during the experiment and affects all firms at the same time, such as changes in proverbial "market conditions," this would confound estimates of the indirect effect coefficient. There is no panacea for the issue of confounding factors. A common approach is to augment the difference-in-difference specification with a vector of control variables X_{it} that captures the changes in these market conditions:

$$Y_{it}=\beta_0+\beta_1A_t+\beta_2T_i+\beta_3A_tT_i+\gamma X_{it}+\varepsilon_{it}. \tag{8}$$

These controls can usually be reduced from firm-level information X_{it} to market-level information X_t, based on the treatment randomization. Since our focus is on measures of short-seller aggressiveness as well as market quality measures, we include three market-wide variables from the previous day to define market conditions. These three variables are the previous day's VIX (an implied volatility index based on option prices), market-wide liquidity measured using the previous day's cross-sectional average effective spread (twice the distance between the trade price and the quote midpoint prevailing at the time of the trade, scaled by that quote midpoint, variable *mktres*), and a market-wide price efficiency measure calculated as the previous day's cross-sectional average AR1 coefficient (the absolute value of the AR(1) coefficient in a daily time-series regression of 30-min quote midpoint returns, variable *mktar*).

The purpose of including control variables is to use them as proxies for potential confounding factors, while the choices can be subjective. The usual arguments against the control variable approach include concerns on exogeneity (whether the controls are really exogenous in the regression), appropriateness (whether the controls are really relevant for the dependent variable), and completeness (whether we exhaust all the important confounding factors). Since there is no theoretical guidance on identifying the "confounding factors," there is no perfect solution to this issue. Here, we consider including three commonly used market-wide variables that affect short aggressiveness as a reasonable, but possibly imperfect solution.[5]

Finally, as discussed in Bertrand et al. (2004), the standard errors in difference-in-difference regressions can be biased. Thus, all *t*-statistics for the panel regressions are double-clustered by date and firm. Because double clustering does not guarantee positive definiteness of the variance-covariance matrix, when the corresponding double-clustered standard error is not available, we conduct inference using the standard errors clustered by firm.[6]

3. Data

For the 2007 full repeal of the uptick rule, our main sample includes the period from 20 trading days before to 20 days after the uptick repeal became effective on July 6, 2007. We specifically choose a short 20-day window around the event to minimize potential impact from the August 2007 Quant Meltdown, as discussed in Khandani and Lo (2011). To further account for any market-wide changes in that period, we rely on difference-in-difference regressions with market condition controls.

In addition to the standard data sources, such as the Trade and Quote (TAQ) and the Center for Research in Security Prices (CRSP), we have all NYSE system order data records related to short sales for this period. Because we have data on all short-sale orders placed, not just executed short sales, we can measure order aggressiveness based on the placement of short-sale orders relative to the existing bid and ask prices. We match firms with CRSP and retain only NYSE-listed common stocks, which means that we exclude securities such as foreign stocks, warrants, preferred shares, American Depositary Receipts, closed-end funds, real estate investment trusts (REITs), and other certificates. We limit the sample to firms that were in the Russell 3000 index during 2004–2005 and were thus eligible for the SEC pilot program. This leaves us with 1088 NYSE-listed common stocks in the sample, of which 360 are pilot stocks and 728 are non-pilot stocks.

Table 1 compares pilot and non-pilot stocks along several dimensions, including market capitalization, book-to-market, trading volume, shorting activity, and market quality measures. We report the 20-day average of the cross-sectional median for both pilot and non-pilot stocks 20 days before the 2007 full repeal in the left panel, and post-repeal medians in the right panel. The two groups (pilot and non-pilot) are very similar in terms of stock characteristics, which is not surprising given the original assignment algorithm for the SEC pilot program. For example, before the event, the average median market capitalization is \$2.928 billion for pilot stocks and \$3.189 billion for non-pilot stocks. Median daily trading volume is just under 400,000 shares for pilot stocks vs. about 422,000 shares for non-pilot stocks. However, characteristics for shorting are significantly different between pilot and non-pilot stocks in both half panels. We measure daily shorting flow (variable *relss*) as the fraction of NYSE trading volume executed in a given stock on a given day that involves a system short seller. Before the full repeal, 37.4% of share volume involves a short-seller for the average pilot stock, while the comparable figure is only 29.2% for non-pilot stocks, indicating that the partial repeal for pilot stocks did in fact remove a significant impediment to shorting. After the full repeal, 39.9% of pilot-stock share volume involves a short, and the comparable figure is 38% for non-pilot stocks, indicating that the shorting activity quickly picked up for non-pilot stocks after the full uptick repeal.

Our key variable in this study is shorting aggressiveness, measured two different ways. Our first measure is based on the average relative effective (half) spread paid by short-sellers in stock i on day t. That is,

$$shortres_{it} = \sum_{s \in t} w_{is}(M_{is} - P_{is})/M_{is}, \tag{9}$$

where P_{is} is the price at which shares are sold short at time s, M_{is} is the prevailing quote midpoint at the time of the short sale, and the weight w_{is} is the size of the short sale, at time s, in shares divided by the total number of shares shorted that day in stock i. We scale the dollar spread by the prevailing midpoint to generate a proportional effective spread. This measure is negative if short-sellers provide liquidity on average, and positive if they demand liquidity on average. When short-sellers become more aggressive, the effective spread increases.

The second proxy for shorting aggressiveness is based on the pricing of the order relative to the existing quote. Specifically, we calculate the fraction of submitted short-sale orders that are marketable, variable *fmkt*, based on the existing bid price. These orders could be either market orders or limit orders to sell short where the limit price is below the existing bid, making them marketable. In either case, these orders are virtually certain to be executed. Unlike the effective spread measure, which is computed after the trades are executed, the fraction of marketable orders is computed after the orders are submitted, so there is a slight difference between the two. But the intuition is

[5] Confounding factors can also show up in the form of pretrends, and we examine the existence of pretrends in later discussions. We find no evidence of pretrends in these supplemental tests.

[6] We thank the referee for this suggestion. For all numbers presented in Tables 2–4, double-clustered standard errors can always be computed. For results in Table 7, there are four out of 72 cases where the double-clustered errors were unable to be computed, and for these four cases, we use the standard errors clustered by firm instead.

Table 1
Summary statistics.
This table reports the time-series mean of the cross-sectional median of daily firm characteristics for our sample of NYSE-listed common stocks, over the 20 trading days before and after the uptick rule repeal on July 6, 2007. The daily share volume is the NYSE volume. The daily measure of shorting activity, *relss*, is NYSE short-sale volume over NYSE trading volume. Variable *shortres* is the relative effective spread for short sales only. Variable *fmkt* is the fraction of short-sale orders that are marketable. The relative effective spread, *res*, is the full proportional effective spread. The relative price impact, *rpi*, is the 5-min price impact. Absolute return persistence, *ar*, is computed for each stock-day as the absolute value of the AR(1) coefficient based on 30-min returns. The intraday variance (*intrav*) is computed with 30-min returns. Hasbrouck (1993) price inefficiency measure (*hasb*) is the volatility of noise over volatility of price. For each measure, we report statistics for RegSHO non-pilot stocks and pilot stocks. The pilot stocks are the sample stocks from Russell 3000 Index that were subject to the RegSHO pilot program in 2005, and the non-pilot stocks are the rest of the Russell 3000 Index stocks in our sample.

	Before July 6th, 2007		After July 6th, 2007	
	Pilot	Non-pilot	Pilot	Non-pilot
Number of firms	360	728	359	725
Market cap ($billions)	2.928	3.189	2.812	3.172
Book-to-market	0.424	0.423	0.438	0.431
Daily share volume (millions)	0.399	0.422	0.469	0.503
Shorts share volume/ total share volume, *relss*	0.374	0.292	0.399	0.380
Relative effective spread for short-sale orders only (bps), *shortres*	−2.271	−4.667	−1.344	−1.593
Fraction of marketable shorts, *fmkt*	0.339	0.321	0.385	0.377
Relative effective spread (bps), *res*	4.844	4.608	5.381	5.643
Relative price impact (bps), *rpi*	0.781	0.773	0.892	0.930
Absolute return persistence, *ar*	0.215	0.215	0.220	0.221
Intraday variance (bps), *intrav*	0.090	0.089	0.160	0.171
Hasbrouck price inefficiency, *hasb*	0.058	0.061	0.053	0.055

similar: higher percentages of marketable orders indicate more aggressive shorting.[7]

From the left panel of Table 1, the relative effective spread for shorts before the full uptick repeal is on average −2.271 basis points (bps) for pilot stocks, and −4.667 basis points for non-pilot stocks. The negative sign indicates that short-sellers in our sample period on average provide liquidity to the market, and more so for non-pilot stocks. From the right panel of Table 1, the relative effective spread for shorts after the full repeal is on average −1.344 for pilot stocks, and −1.593 basis points for non-pilot stocks. After the full repeal, the relative effective spread increases for both pilot and non-pilot stocks, indicating that short-sellers become more aggressive towards all stocks. Before the full repeal, on average, 33.9% of shorts are marketable for pilot stocks vs. 32.1% for non-pilot stocks, indicating short-sellers are slightly more aggressive towards pilot stocks before the full repeal. After the full repeal, these two measures become 38.5% and 37.7%, respectively, indicating that short-sellers become more aggressive towards both pilot and non-pilot stocks.

To compare the measures before and after the uptick repeal event day by day, we present time-series of the cross-sectional mean of the short-sale flow and both shorting aggressiveness measures in Fig. 1. For the *shortres* measure in Panel A, it is clear that, compared to non-pilot stocks, short-sellers are more aggressive in pilot stocks before the 2007 full uptick repeal, and the difference quickly disappears after the full uptick repeal. Interestingly, the *shortres* measure seems to increase for both pilot and non-pilot stocks after the full uptick repeal. For the *fmkt* variable, the difference between pilot and non-pilot stocks is not as obvious before or after the full repeal, but the percentage of marketable orders seems to increase for both pilot and non-pilot stocks after the uptick repeal. The timing of the changes closely coincides with the exact day of the rule change, and is not supportive of the hypothesis of a pretrend.

Other than the shorting aggressiveness measures, we also report summary statistics on a few market quality measures, including the full proportional effective spread, the 5-min price impact, the absolute return persistence (the absolute value of the AR(1) coefficient based on 30-min returns), the intraday variance of 30-min returns, and the Hasbrouck (1993) price inefficiency measure (the volatility of noise over volatility of price). All these measures have similar magnitudes as documented in earlier studies.

4. Empirical results on short-sale aggressiveness

Clearly, the uptick rule constrains the trading behavior of many if not most short-sellers. While the rule is in place, short-sellers cannot hit an existing bid if the resulting trade price would violate the uptick rule. Given this constraint, some short-sellers might comply by submitting less aggressive limit orders for firms with the uptick constraint. Others might choose not to trade at all. Thus, when the uptick rule is repealed, we expect to see short-sellers trade more aggressively on firms without the uptick constraint. Comparing the start of the partial repeal pilot program in 2005 to the full repeal in 2007, the implications for short-sellers' aggressiveness could be quite different. For the partial repeal in 2005, we expect to observe

[7] The above two measures reflect different aspects of shorting aggressiveness. The variable *shortres* provides information on the average bid-ask spread, while *fmkt* reveals more about order distribution. We present results on both measures for completeness in capturing short-sellers' behaviors.

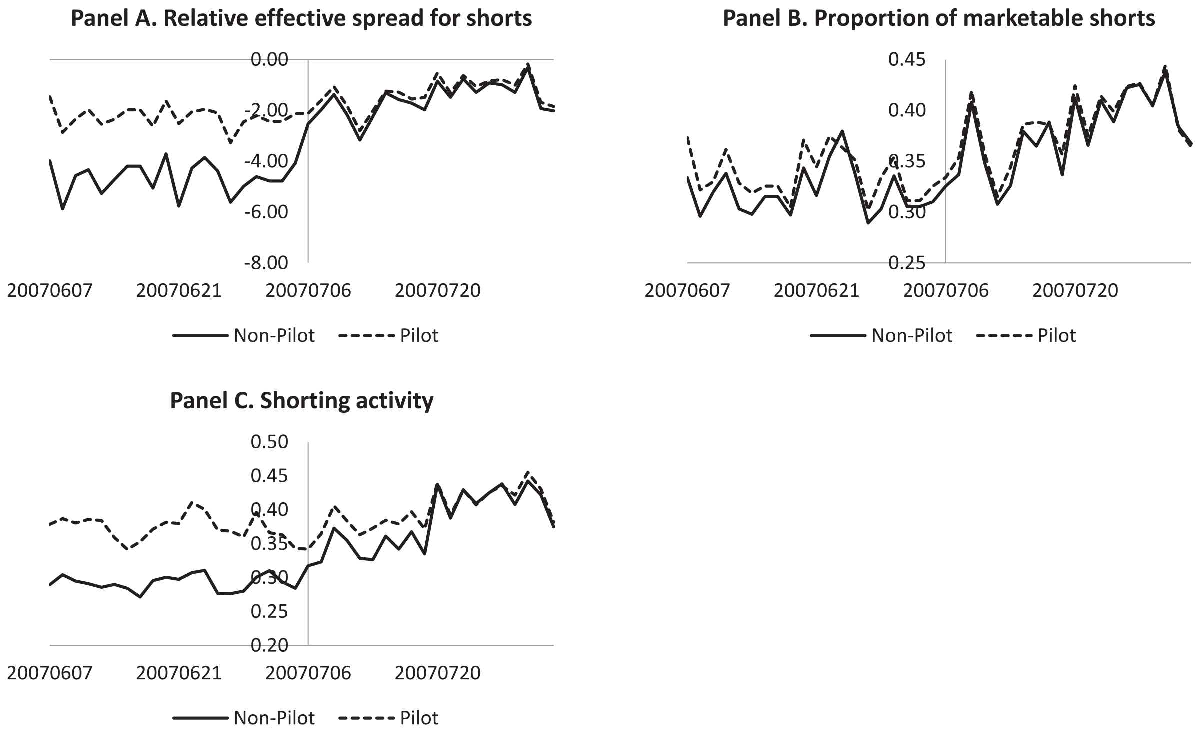

Fig. 1. We present the time-series of three key variables over days [−20, +20] around the tick test repeal on July 6, 2007. The *y*-axis crosses at the event day of July 6, 2007, so to the left, we report pre-repeal, and to the right, post-repeal. Variable *shortres* is the relative effective spread for shorts only. Variable *fmkt* is the fraction of short-sale orders that is marketable at the time of submission. Shorting activity, *relss*, is measured each day as the fraction of NYSE daily share volume. Cross-sectional medians are reported for both pilot and non-pilot stocks.

more aggressive shorting in pilot firms, but not in non-pilot firms, because the uptick rule would still be in place for these firms. For the full repeal in 2007, we might observe short-sellers becoming more aggressive in the non-pilot firms.

We start Section 4.1 with an analysis of changes in shorting aggressiveness around the full uptick repeal in 2007 to identify potential indirect effect. Section 4.2 examines shorting aggressiveness in 2005 with partial repeal of the uptick rule for indirect effect. Section 4.3 reports robustness checks on indirect effect coefficients with subgroups of firms. Section 4.4 discusses the changes in the intraday comovement of stock returns and shorting activity, and provides insights on likely mechanisms for the indirect effect in 2005 and 2007.

4.1. Effects of 2007 full uptick repeal on shorting aggressiveness

The uptick rule directly limits the aggressiveness of short-sellers, and thus shorting aggressiveness is the cleanest laboratory for our methodological investigation into direct and indirect effects of the rule change. As discussed above, once the uptick constraint is removed, short-sellers are free to demand or supply liquidity as they see fit. As a result of this shift away from supplying liquidity, we expect to see short-sellers earn the bid-ask spread less often and pay the bid-ask spread more often. On average, then, the bid-ask spread paid by short-sellers should increase, and we would expect to see shorts use more marketable orders compared to limit orders.

Table 2 provides details on shorting aggressiveness in both pilot and non-pilot stocks, before and after the July 6, 2007 repeal of the tick test. Estimation results with and without the market-level controls are reported in Panels A and B, respectively. The first column of Panel A contains a simple difference-in-difference specification for the average effective spread paid by short-sellers. This specification shows that before repeal, short-sellers pay $-5.923+2.849=-3.074$ basis points in pilot stocks, while they pay -5.923 basis points (that is, they earn 5.923 basis points of spread) for non-pilot stocks, where the tick test remains in effect. The difference of 2.849 basis points is strongly statistically significant ($t=9.81$). This shows that short-sellers are constrained by the tick test to supply rather than demand liquidity. Once the tick test is repealed on July 6, the cross-sectional differences quickly disappear. Over the 20-day post-repeal period, short sales pay an average effective spread of $-5.923+3.818=-2.105$ basis points for non-pilot stocks vs. $-5.923+3.818+2.849$ – $2.710=-1.965$ basis points for pilot stocks. A standard difference-in-difference test would conclude that the July 2007 repeal of the tick test is associated with more aggressive shorting in the affected non-pilot stocks. Based on the results in Table 2 Panel A, the uptick repeal causes short-sellers to pay 2.710 basis points more in effective spread, which is about half the 5.923 basis points that they were previously receiving ($t=13.25$).

Table 2
Diff-in-diff regressions around July 2007 uptick repeal.

In this table, we report coefficients for the following regression:

$$y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}.$$

Event dummy A_t takes a value of one for dates after July 6, 2007, and zero otherwise. The treatment dummy T_i takes a value of one for firms in the pilot program, and zero otherwise. Each regression is estimated for two different dependent variables. The first dependent variable *shortres* is the relative effective spread for short-sales only. The second dependent variable *fmkt* is the fraction of short-sale orders that are marketable. Panel A reports results without controls. Panel B includes the following market-level controls X_{t-1}: VIX, average firm-level relative effective spread (*mktres*), and average firm-level absolute return persistence (*mktar*). All market-level controls are measured from the previous day. The regressions are estimated over days [−20, +20] around July 6, 2007. The total effect is measured by β_1, the direct effect is measured by $-\beta_3$, and the indirect effect is measured by $\beta_1 + \beta_3$. *T*-stats are computed using standard errors double-clustered (DC) by date and firm.

Panel A: Without control variables

Dep. var.	shortres		fmkt	
	coef.	t(DC)	coef.	t(DC)
β_0	−5.923	−18.55	0.335	55.78
β_1	3.818	9.67	0.044	4.35
β_2	2.849	9.81	0.010	2.74
β_3	−2.710	−13.25	−0.004	−1.03
R-square	0.04		0.04	
# obs.	41,785		41,395	
Total effect	3.818	9.67	0.044	4.35
Direct effect	2.710	13.25	0.004	1.03
Indirect effect	1.108	3.64	0.040	4.23

Panel B: With market-level control variables

Dep. var.	shortres		fmkt	
	coef.	t(DC)	coef.	t(DC)
β_0	−8.153	−6.45	0.264	7.54
β_1	3.522	8.85	0.030	2.90
β_2	2.850	9.81	0.010	2.74
β_3	−2.710	−13.24	−0.004	−1.04
VIX	−0.323	−3.74	−0.007	−3.01
mktres	1.027	3.77	0.029	4.27
mktar	1.693	0.83	−0.060	−1.01
R-square	0.04		0.06	
# obs.	41,785		41,395	
Total effect	3.522	8.85	0.030	2.90
Direct effect	2.710	13.24	0.004	1.04
Indirect effect	0.812	2.43	0.026	2.67

While the difference-in-difference approach identifies an increase in shorting aggressiveness, there is also evidence of an indirect effect. After the tick test repeal, shorting aggressiveness increases even for the pilot stocks that were already exempt from the tick test and should have been unaffected by the regulatory change. As noted above, pilot stock shorting receives 3.074 basis points of effective bid-ask spread before the repeal and only 1.965 basis points after repeal, which is 36% less. The fact that shorting in unaffected control stocks becomes more aggressive is consistent with positive and significant spillover associated with the uptick repeal. At the bottom of Panel A, we report our estimates of the total effect, along with the direct and indirect treatment effect coefficients of the uptick repeal. Traditional difference-in-difference approaches pick up only the direct effect coefficient, which we estimate to be 2.710 basis points. The indirect effect coefficient from treatment spillovers contributes an additional 1.108 basis points of shorting aggressiveness, as measured by effective spread, for a total treatment effect of 3.818 basis points. All three estimates use double-clustered standard errors and are statistically significant. That is, if we ignore the indirect effect coefficient, we substantially understate the increase in shorting aggressiveness associated with uptick repeal by 29%.

Of course, there could be other explanations for the increase in pilot stock shorting aggressiveness after July 6. Perhaps the aggressiveness of shorting activity depends on market conditions such as returns and volatility, and perhaps market conditions were different post-repeal. To investigate this possibility, we augment the difference-in-difference specification with market-level control variables, and the results are reported in Table 2 Panel B. The results are quite similar with these controls in place, and the indirect effect coefficient is slightly smaller at 0.812 basis point with a significant *t*-statistic of 2.43.

Our other measure of aggressiveness, the fraction of submitted short-sale orders that are marketable, shows similar differences between pilot and non-pilot stocks. Note that short-sellers as a group are still relatively passive traders. Even after uptick repeal, only about 38% of their submitted orders are marketable, and on average they continue to earn rather than pay the spread. From Table 2 Panel A, the total effect coefficient on *fmkt* is 0.044 ($t = 4.35$): the direct effect coefficient is 0.004 ($t = 1.03$), and the indirect effect coefficient on pilot stocks is 0.040 ($t = 4.23$). This indicates that overall, short-sellers become more aggressive for both pilot and non-pilot stocks. Interestingly, the direct effect coefficient on non-pilot stocks is insignificant, while the indirect effect coefficient on pilot stocks accounts for most of the total effect and is positive and highly significant. The pattern clearly indicates that short-sellers become more aggressive by using more marketable orders (for both pilot and non-pilot stocks) after the 2007 full repeal of the uptick rule. If instead we rely on the standard difference-in-difference approach and focus only on the direct effect coefficient, we are likely to make the inference that short-sale aggressiveness, measured by *fmkt*, is not significantly affected by the 2007 full repeal. When we add in market controls in Panel B, the empirical results are qualitatively similar, with *t*-statistics that are smaller but still statistically significant.[8]

[8] To check the reliability of the double-clustered *t*-statistics, we conduct a placebo test, as suggested by the referee. For this placebo test, we choose a sample close to our main sample period that has no uptick repeal event. The placebo sample period is January 1, 2007 to June 30, 2007. For our main estimations in Table 2, we estimate a difference-in-difference regression using a 40-day window around the event. For the placebo test, we estimate our difference-in-difference regression for each 40-day window (without the event) within the placebo sample period. We obtain empirical distributions for the total, direct, and indirect effect coefficients, by either directly ranking the estimated coefficients from the placebo sample or by randomly resampling the coefficients 1000 times. We compute the 95th percentile from both methods and compare with our sample estimates. For the 12 cases of the total, direct, and indirect effect coefficients in Table 2, the significances of the original double-clustered *t*-statistics are mostly confirmed with the placebo tests. The two exceptions are the indirect effect coefficients on *shortres* and *fmkt* with market controls, where the resampling methodology confirms significance

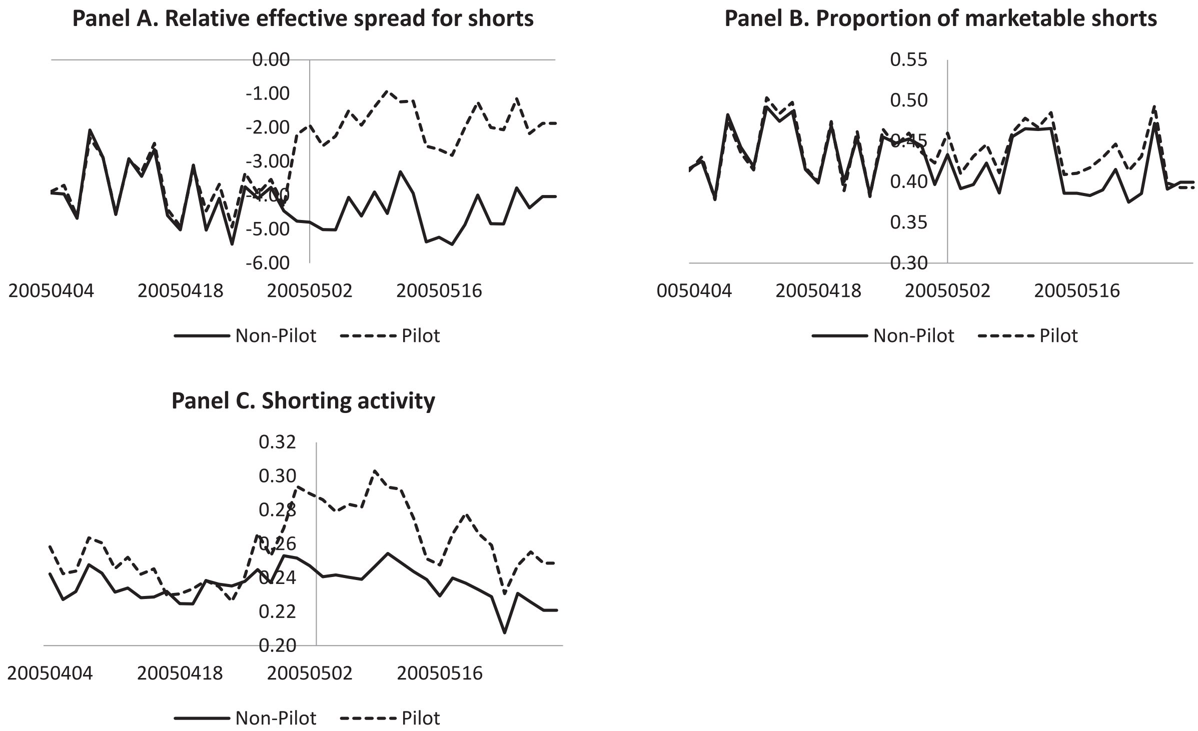

Fig. 2. We present the time-series of three key variables over days [−20, +20] around the tick test repeal on May 2, 2005. The *y*-axis crosses at the event day of May 2, 2005, so to the left, we report pre-repeal, and to the right, post-repeal. Variable *shortres* is the relative effective spread for shorts only. Variable *fmkt* is the fraction of short-sale orders that is marketable at the time of submission. Shorting activity, *relss*, is measured each day as the fraction of NYSE daily share volume. Cross-sectional medians are reported for both pilot and non-pilot stocks.

Why would short-sellers become more aggressive in the unaffected pilot stocks? A possible explanation is that the uptick repeal made it easier to implement "list-based shorting," a shorting strategy that demands liquidity and involves multiple stocks. One typical example for "list-based shorting" is index arbitrage. If stock index futures are cheap relative to cash market prices, an index arbitrageur would like to buy futures and immediately short all of the underlying stocks. The arbitrageur can observe the bid and ask prices for all stocks. If the trade is profitable at the existing quotes, the arbitrageur would like to hit all of the bids in the underlying stocks simultaneously, thereby locking in a profit. Thus, she would like to submit simultaneous marketable short-sale orders for a list of stocks. However, if the uptick rule is binding in some of the stocks, the index arbitrageur cannot demand liquidity in those stocks, but instead is forced to either supply liquidity or abstain from shorting those stocks.[9] As a result, the arbitrage strategy is subject to considerable execution risk or tracking error in the presence of the uptick rule, so the index arbitrageur may not be able to implement this strategy as effectively when the uptick rule is in place. Once the uptick rule is repealed, aggressive trading activity associated with an index arbitrage strategy may increase markedly, and we would expect to see more aggressive short sales in all of the underlying stocks, including pilot stocks that were already exempt from the uptick rule. Similar arguments would apply for any broad list-based portfolio short-selling strategy where some of the stocks are freed from the uptick rule. We consider this explanation in more detail in Section 4.4.

4.2. *Effect of 2005 partial uptick repeal on shorting aggressiveness*

If aggressive list-based short-sellers require full or near-full uptick repeal to implement their trading strategies, the 2005 partial uptick repeal could yield different results on shorting aggressiveness. At that time, tick tests were suspended for only one-third of Russell 3000 stocks, which probably inhibits most such portfolio trading strategies. On the other hand, given that shorting constraints are eased for all pilot stocks, but not for non-pilot stocks, we might observe more aggressive shorting in pilot stocks, but not in non-pilot stocks. To investigate this hypothesis, we first present the time-series patterns of the shorting aggressiveness measures, then we estimate the difference-in-difference specifications on a similarly constructed sample that extends from 20 trading days before to 20 trading days after the 2005 partial uptick repeal.

We report the time-series of the cross-sectional mean of the short-sale flow and both shorting aggressiveness

and the direct ranking of the parameters does not. That is to say, the inference from the original double-clustered standard errors is mostly consistent with what we see in the placebo tests. Similar placebo tests are also conducted for the 2005 event, and we reach similar conclusions.

[9] Index arbitrage by registered broker-dealers is exempt from the uptick rule, see Macey et al. (1989). However, index arbitrage by others is subject to the uptick rule.

measures for the 2005 partial uptick repeal in Fig. 2. In Panel A, before May 2005, the *shortres* time-series for the pilot stocks and non-pilot stocks are very similar. Then one day before the regulation change, the *shortres* time-series quickly diverge, with short-sellers being much more aggressive towards the pilot stocks than the non-pilot stocks. The same patterns also exist for the *fmkt* measure, in the sense that the difference between pilot and non-pilot stocks is not as obvious before the partial repeal, but the percentage of marketable orders seems to increase for pilot stocks, starting from one day before the partial repeal. Notice that the exact event date, May 2 of 2015, is prescheduled and public news. The finding that the shorting aggressiveness diverges one day before the event date indicates that some market participants start to trade accordingly already one day before the event. The time-series patterns in Fig. 2 indicate that the timing of the changes almost coincides with the exact event date, and there does not seem to be pretrends.

The estimation results for the difference-in-difference specification are presented in Table 3. Panel A provides results from the simple differences-in-differences specification, and Panel B reports results after including market-level controls. Based on the specification without market condition controls in Panel A, for the 20 trading days after the pilot starts, short-sellers pay an average effective spread of $-4.287 - 1.579 = -5.866$ basis points in non-pilot stocks vs. $-4.287 - 1.579 + 0.174 + 3.517 = -2.175$ basis points for pilot stocks. The estimates show that suspension of the uptick rule causes short-sellers to reduce the effective spread that they receive by 3.517 basis points ($t = 8.74$) on pilot stocks, a reduction of about 60% from pre-pilot levels. As before, the standard difference-in-difference test would conclude that the 2005 partial uptick repeal is associated with more aggressive shorting in treated pilot stocks. Results in Panel B are quite similar.

What about the indirect effect coefficient on the non-pilot stocks? The indirect effect coefficient is estimated at an economically and statistically significant -1.579 basis points ($t = -2.93$) in Panel A, and -1.832 basis points ($t = -3.58$) in Panel B. Clearly, regardless of the specification, the indirect effect coefficient is statistically significant, with a magnitude close to 50% of the direct effect coefficient. That is to say, short-sellers are significantly less aggressive towards non-pilot stocks after the 2005 partial uptick repeal.

More interestingly, the indirect effect coefficient in 2005 is negative, which is the opposite to what we observe in 2007. Based on the direct effect coefficient, short-sellers are more aggressive for pilot stocks. Yet, the indirect effect coefficient, coming from non-pilot stocks, is significant and negative. This pattern clearly differs from the list-based trading hypothesis we proposed for 2007, and is consistent with a possible substitution effect. That is, short-sellers take advantage of the eased shorting restrictions on pilot stocks, and at least partially substitute pilot stocks for non-pilot stocks in their short portfolios after the partial repeal, which is associated with higher aggressiveness towards pilot stocks and less aggressiveness towards non-pilot stocks.

Table 3
Difference-in-difference regressions around May 2005 pilot start.
In this table, we report coefficients for the following regression:

$$y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}.$$

Event dummy A_t takes a value of one for dates after May 2, 2005, and zero otherwise. The treatment dummy T_i takes a value of one for firms in the pilot program, and zero otherwise. Each regression is estimated for two different dependent variables. The first dependent variable *shortres* is the relative effective spread for short sales only. The second dependent variable *fmkt* is the fraction of short-sale orders that are marketable. Panel A reports results without control variables. Panel B includes the following market-level controls X_{t-1}: VIX, average firm-level relative effective spread (*mktres*), and average firm-level absolute return persistence (*mktar*). All market-level controls are measured from the previous day. The regressions are estimated over days [−20, +20] around May 2, 2005. The total effect is measured by $\beta_1 + \beta_3$, the direct effect is measured by β_3, and the indirect effect is measured by β_1. *T*-stats are computed using standard errors double-clustered (DC) by date and firm.

Panel A: Without control variables

Dep. var.	shortres		fmkt	
	coef.	t(DC)	coef.	t(DC)
β_0	−4.287	−8.92	0.452	56.53
β_1	−1.579	−2.93	−0.028	−2.52
β_2	0.174	0.46	0.001	0.18
β_3	3.517	8.74	0.018	3.73
R-square	0.01		0.01	
# obs.	42,881		42,910	
Total effect	1.939	3.27	−0.010	−0.97
Direct effect	3.517	8.74	0.018	3.73
Indirect effect	−1.579	−2.93	−0.028	−2.52

Panel B: With market-level control variables

Dep. var.	shortres		fmkt	
	coef.	t(DC)	coef.	t(DC)
β_0	−1.583	−0.40	0.403	4.46
β_1	−1.832	−3.58	−0.034	−2.84
β_2	0.175	0.46	0.001	0.18
β_3	3.516	8.74	0.018	3.72
VIX	−0.577	−2.64	−0.009	−1.83
mktres	0.501	1.07	0.016	1.45
mktar	2.200	0.33	0.069	0.44
R-square	0.01		0.01	
# obs.	42,881		42,910	
Total effect	1.684	2.79	−0.016	−1.43
Direct effect	3.516	8.74	0.018	3.72
Indirect effect	−1.832	−3.58	−0.034	−2.84

4.3. Robustness check on the indirect effect coefficients: subgroup results

Before we look into the potential causes of the positive indirect effect coefficient in 2007 and the negative indirect effect coefficient in 2005, we briefly examine the robustness of these indirect effect estimates using subsamples. We consider two sets of subsamples. We first separate firms into three market capitalization buckets (small, medium, and large), with each bucket containing an approximately equal number of these NYSE-listed stocks. The small-cap category has a median market cap of about \$0.8 billion, the mid-cap category has a median market cap of about \$3 billion, and the large-cap category has a median market capitalization around \$14.3 billion.

Table 4
Total, direct and indirect effects of the July 2007 uptick repeal, subgroup analysis.
In this table, we report the total, direct and indirect effects coefficients for the following regression:

$$y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}.$$

Event dummy A_t takes a value of one for dates after July 6, 2007, and zero otherwise. The treatment dummy T_i takes a value of one for firms in the pilot program, and zero otherwise. Each regression is estimated for two different dependent variables. The first dependent variable *shortres* is the relative effective spread for short sales only. The second dependent variable *fmkt* is the fraction of short-sale orders that are marketable. The left half panel reports results without controls. The right half panel includes the following market-level controls X_{t-1}: VIX, average firm-level relative effective spread (*mktres*), and average firm-level absolute return persistence (*mktar)*. All market-level controls are measured from the previous day. Panel A divides the NYSE-listed sample into three market-cap terciles; Panel B partitions based on membership in the S&P 500. The regressions are estimated over days [−20, +20] around July 6, 2007. The total effect is measured by β_1, the direct effect is measured by $-\beta_3$, and the indirect effect is measured by $\beta_1 + \beta_3$. *T*-stats are computed using standard errors double-clustered (DC) by date and firm.

Panel A: Size groups

		Regression using dummy variables only						Regression using dummy variables and market controls					
		shortres small	shortres medium	shortres large	fmkt small	fmkt medium	fmkt large	shortres small	shortres medium	shortres large	fmkt small	fmkt medium	fmkt large
Total effect	coef.	6.015	2.959	2.438	0.016	0.031	0.083	5.591	2.620	2.311	0.006	0.016	0.068
	t(DC)	7.36	11.56	12.82	1.03	3.48	9.37	6.47	10.52	13.51	0.36	1.85	7.76
Direct effect	coef.	4.471	1.944	1.619	−0.020	0.006	0.025	4.473	1.945	1.619	−0.020	0.006	0.025
	t(DC)	8.14	12.71	13.00	−2.87	1.11	4.59	8.12	12.67	12.98	−2.85	1.11	4.57
Indirect effect	coef.	1.544	1.015	0.819	0.036	0.025	0.059	1.118	0.676	0.692	0.026	0.010	0.043
	t(DC)	2.43	4.07	4.96	2.74	2.70	6.85	1.50	3.00	4.29	1.76	1.11	5.13

Panel B: Effects by S&P500 membership

		Regression using dummy variables only				Regression using dummy variables and market controls			
		shortres member	shortres no	fmkt member	fmkt no	shortres member	shortres no	fmkt member	fmkt no
Total effect	coef.	2.575	4.320	0.086	0.026	2.430	3.962	0.071	0.014
	t(DC)	12.51	8.82	9.30	2.36	12.96	7.86	7.68	1.16
Direct effect	coef.	1.666	3.132	0.025	−0.004	1.666	3.133	0.025	−0.004
	t(DC)	16.11	11.07	4.30	−1.00	16.04	11.06	4.28	−0.98
Indirect effect	coef.	0.909	1.188	0.060	0.031	0.764	0.829	0.045	0.018
	t(DC)	5.26	3.10	7.08	2.94	4.89	1.92	5.33	1.61

Total, direct, and indirect effect coefficients are summarized by market-cap tercile in Table 4 Panel A for *shortres* and *fmkt*. Our focus is on the indirect effect coefficients presented at the bottom of Panel A. The 2007 full uptick repeal in general is positively associated with short-sellers' aggressiveness, indicating that full uptick repeal is associated with increases in short-sellers' aggressiveness in all firms. For the *shortres* measure, the increase ranges between 0.676 bps and 1.544 bps, with five out of the six *t*-statistics being significant at the 5% level. In terms of magnitude, the increase in aggressiveness is higher for smaller firms. But if we compare with the magnitude of the total effect coefficient, the proportion of indirect effect coefficient relative to the total effect coefficient ranges between 20% (for small-cap firms) and 34% (for mid-cap firms), so it is not clear that the indirect effect is less important for mid- and large-cap firms. For the *fmkt* measure, we also observe large and positive indirect effect coefficients. When there are no market-level controls, the indirect effect coefficient on *fmkt* is always statistically significant. With market-level controls, the indirect effect coefficient on *fmkt* remains highly significant for large-cap firms, but becomes marginally significant for small-cap firms, and insignificant for mid-cap firms. In terms of magnitude and significance, the indirect effect seems much stronger for large-cap firms vs. small-cap firms. As mentioned earlier, the *shortres* measure and the *fmkt* measure reveal different aspects of short-sellers' aggressiveness, and we are not surprised that there might be differences in results using the two measures. Overall, we find positive indirect effect coefficients for all size subgroups.

Given our discussions of index arbitrage and the fact that many institutional investors explicitly or implicitly track the Standard & Poor's (S&P) 500, another useful way to partition these stocks is based on their membership in the S&P 500 index. In terms of the membership in the S&P 500 index, 371 of our sample firms are in the S&P 500, while 771 are not. Market cap, book-to-market, trading volume, short sale, and market quality statistics for the S&P 500 subsample are quite similar to those for the large-cap subsample, indicating that the non-S&P 500 firms are more similar to our small- and mid-cap firms.

Table 4 Panel B reports the total, direct, and indirect effect coefficients for S&P 500 vs. non-S&P stocks. From the bottom of Panel B, the indirect effect coefficient is positive and large for all stocks. For the *shortres* measure, the indirect effect coefficient is always statistically significant except for non-S&P firms after we include the market-level controls. The magnitude of the indirect effect coefficient is slightly larger for non-S&P firms, while as a proportion of the total effect coefficient, the S&P 500 firms have larger spillovers. For the *fmkt* measure, the indirect effect coefficient is always larger and significant for the S&P firms. The larger indirect effect coefficient for S&P 500 firms is consistent with the list-based trading hypothesis.

Overall, we show positive indirect effect coefficients for the 2007 full uptick repeal for various subgroups in this subsection. We would like to caution that "list-based trading" can be based on indices such as the S&P500, but could also potentially include trades based on industries, factors, or other indices. Therefore, we do not expect the subsample results to necessarily exhibit patterns among different subgroups. Instead, we use these results to provide more details and robustness of the indirect effect finding. We provide results for the 2005 partial uptick repeal in Appendix Table A1, and results are similar but in the opposite direction.

4.4. Source of indirect effects around uptick repeal: comovements

Perhaps it is not too surprising that when a rule that limits traders' aggressiveness in a specific group of stocks is repealed, those traders become more aggressive in these stocks. But it is intriguing to find that the rule is associated with significant changes on control stocks, which are not directly affected by the rule change. For the increase in pilot stocks' shorting aggressiveness around the 2007 full uptick repeal, our hypothesis is that traders are now better able to simultaneously short a portfolio of stocks. For the decrease in non-pilot stocks' shorting aggressiveness around the 2005 uptick repeal, our hypothesis is that this results from a substitution effect. In this section, we look for direct evidence of the indirect effects by examining comovement in intraday shorting activity. If there is a substitution effect, we would observe less comovement in shorting activity, and if the list-based trading complementarity dominates, we would observe more comovement in shorting activity.

We take all sample firms and partition them into pilot and non-pilot stocks. For non-pilot and pilot stocks, respectively, we compute a cross-sectional average using firm-level intraday 15-min shorting activity, measured as NYSE short-sale shares divided by overall NYSE trading volume during that 15-min interval. Based on the resulting time-series that extends from 20 trading days before the uptick repeal to 20 trading days after, we regress average non-pilot shorting activity on contemporaneous pilot stock shorting activity, allowing a different slope coefficient after the uptick repeal. That is, we estimate the following regression:

$$relss_t^{nonpilot} = \theta_0 + (\theta_1 + \theta_2 A_t) relss_t^{pilot} + u_t, \tag{10}$$

where $relss_t^{nonpilot}$ is the intraday average shorting activity on non-pilot stocks, $relss_t^{pilot}$ is the contemporaneous 15-min average shorting activity on pilot stocks, and A_t is an indicator variable that equals one if and only if the uptick rule has been repealed. Given that stocks have been assigned essentially randomly to pilot and non-pilot groups, if the uptick repeal has no spillover between pilot and non-pilot stocks in terms of shorting activity, we expect the coefficient θ_2 to be zero. If the 2007 full uptick repeal is associated with more list-based shorting activity across the board with positive indirect effects, we expect θ_2 to be positive. If the 2005 partial uptick repeal is associated with substitution between pilot and non-pilot shorting, we expect θ_2 to be negative. For time-series regressions as in Eq. (10), the standard errors are computed using Newey–West standard errors with five lags.

Table 5
Comovements among 15-min shorts and returns around uptick repeals.

This table reports the comovement of returns and shorting activity (*relss*) before and after the uptick rule repeal in July 2007 and May 2005. We regress average non-pilot firms' shorts (returns) on the average pilot firms' shorts (returns), interacting with the event dummy A_t which takes the value of one after the event date, and zero otherwise. Panel A reports the comovement results on shorting activity, and Panel B reports the comovement results on returns. We report Newey–West (NW) standard errors with five lags. Each regression has 2080 observations.

Panel A: Comovement of 15-minshorting activities 2007 and 2005

	2007 full uptick repeal		2005 partial uptick repeal	
	coef.	*t*(NW)	coef.	*t*(NW)
Pilot	0.785	18.67	0.977	91.98
Pilot*A_t	0.171	8.11	−0.074	−9.73

Panel B: Comovement of 15-min returns in 2007 and 2005

	2007 full uptick repeal		2005 partial uptick repeal	
	coef.	*t*(NW)	coef.	*t*(NW)
Pilot	0.947	66.19	0.971	89.39
Pilot*A_t	0.077	4.88	−0.030	−1.64

The results on the 2007 full uptick repeal are reported on the left half of Table 5 Panel A. Before the full repeal in July 2007, non-pilot and pilot shorting do not co-move one-for-one, with an estimated slope coefficient of only 0.785 ($t = 18.67$), significantly lower than one. This slope coefficient rises by 0.171 ($t = 8.11$) after the July full uptick repeal. The new slope coefficient becomes $0.785 + 0.171 = 0.956$. The increase in shorting activity comovement is consistent with the list-based trading hypothesis with a strong positive indirect effect. When the full repeal is in place, pilot and non-pilot stocks then experience very similar time-series variation in shorting activity.

In the right half of Table 5 Panel A, we present results on the 2005 partial uptick repeal. It is striking to observe that the results are opposite to those in Panel A. Before May 2005, the comovement between pilot and non-pilot stock shorting activity is 0.977, quite close to one, indicating synchronous shorting when the uptick rule is applied to all stocks. However, the comovement in shorting activity significantly drops by 0.074 ($t = -9.73$), after the partial uptick repeal in May 2005. The lower comovement in shorting is consistent with a substitution effect.

To better understand the timing of the comovement dynamics and to examine for pretrends, for each day, we regress intraday non-pilot shorting on pilot shorting, day by day,

$$relss_t^{nonpilot} = \theta_{0t} + \theta_{1t} relss_t^{pilot} + u_t, \tag{11}$$

where the coefficient θ_{1t} reflects day-by-day dynamics of the comovement. We present the time-series of the daily coefficients in Fig. 3. For ease of comparison, we add in each panel the pre- and post-event average of the estimated coefficients in the time-series plot. Panel A re-

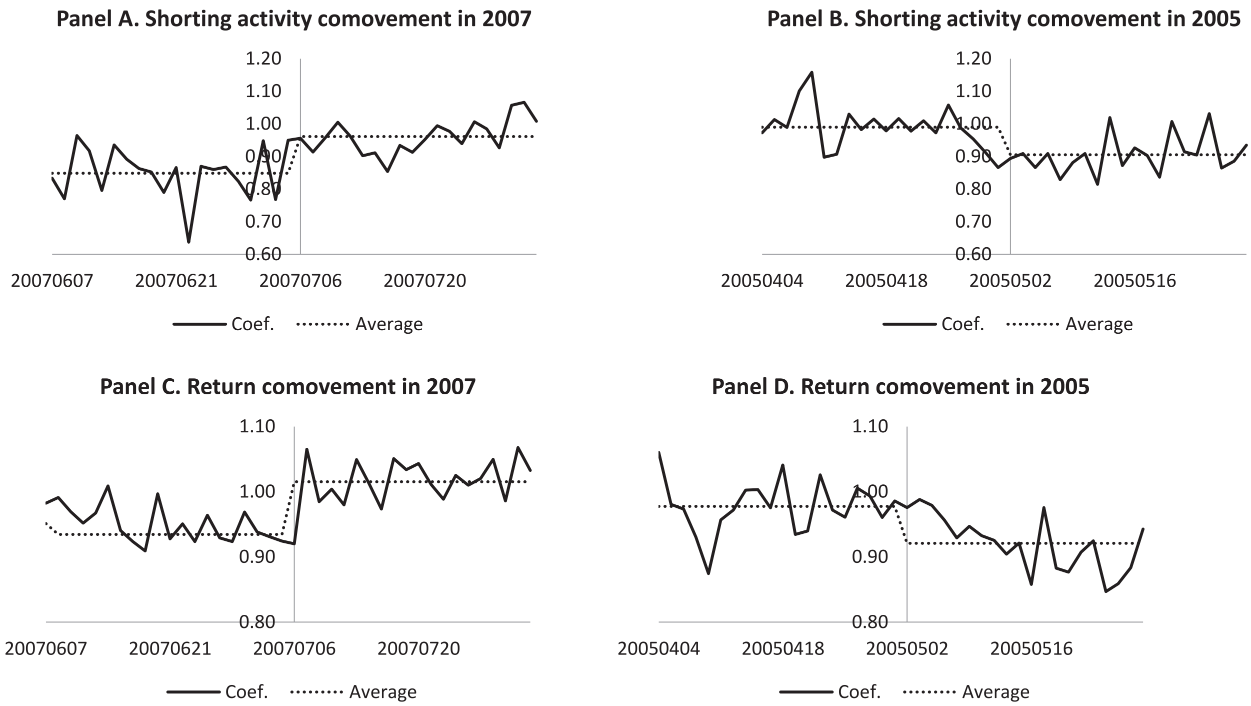

Fig. 3. This figure reports the comovement of returns and shorting activity (*relss*) before and after the uptick rule repeal in July 2007 and May 2005. We regress average non-pilot firms' shorts (returns) on the average pilot firms' shorts (returns), day by day, and plot the coefficients. Panel A reports the comovement results on shorting activity, and Panel B reports the comovement results on returns. Each daily regression has on average 26 intraday observations.

ports the daily comovement coefficients for the shorting activity comovement before and after the 2007 full uptick repeal. Before July 6, 2007, the daily coefficients mostly range between 0.7 and 0.9, and afterwards, the daily coefficients are most between 0.9 and 1.0. There is a clear increase in the shorting comovement coefficients after July 6, 2007. We report the daily shorting comovement coefficients for 2005 partial repeal in Panel B. Before May 2, 2005, the coefficients are mostly between 0.9 and 1.1, while afterwards, the coefficients drop to between 0.8 and 1.0. Again, there seems to be clear decreases in the shorting comovement after May 2, 2005. These patterns indicate that the timing of the comovement pattern change coincides with the event, and thus there does not seem to be pretrends.

Shorting is an important channel for price discovery. Once tick tests disappear, we expect prices to incorporate negative information more quickly via short sales. Furthermore, if our list-based hypothesis is correct and uptick repeal encourages more list-based trading activity, we should see evidence of this trading activity in share price comovements. Next, we examine intraday share prices and returns to see whether the pilot and non-pilot stocks incorporate common information at the same time. Parallel to the shorting activity comovement regression, we estimate the following regression for 15-min returns:

$$ret_t^{nonpilot} = \theta_0 + (\theta_1 + \theta_2 A_t) ret_t^{pilot} + u_t, \tag{12}$$

where $ret_t^{nonpilot}$ is the intraday equal-weighted return on non-pilot stocks using quote midpoints, ret_t^{pilot} is the contemporaneous 15-min return on pilot stocks, and A_t is an indicator variable that equals one if and only if the uptick rule has been repealed. If information is incorporated into pilot and non-pilot stocks at the same rate, we would expect a slope coefficient of one in this regression, given that stocks have been assigned randomly to these two groups. If the partial uptick rule slows down information incorporation for non-pilot stocks relative to pilot stocks, we would expect θ_1 to be below one. After the full repeal, we expect θ_2 to be positive and the comovement among pilot and non-pilot stocks to increase. On the other hand, in the case of the 2005 partial repeal, we expect θ_1 to be around one when the uptick rule is applied to all stocks, and θ_2 to be negative when the partial uptick rule hinders shorting in non-pilot stocks, reducing the comovement between pilot and non-pilot stocks.

The results are reported in Panel B of Table 5. For the 2007 uptick repeal, when the partial uptick rule is in effect, the estimated slope coefficient θ_1 is 0.947, significantly below one, indicating that the partial uptick rule reduces price synchronicity. After the uptick rule is fully repealed, the slope coefficient increases by 0.077 ($t = 4.88$), and the total slope becomes 1.024, which is statistically indistinguishable from unity. This implies that after the uptick rule is fully repealed, there is a significant increase in the comovement in prices of both pilot and non-pilot stocks, which is consistent with our list-based trading hypothesis. The coefficient θ_2 itself, which directly measures the association between the uptick repeal and the comovement, is positive and significant.

The pattern of the 2005 partial uptick repeal is in opposition to the above findings. When all stocks are subject to the uptick rule, before May 2005, the coefficient $\theta_1 = 0.971$, is close to one. After the partial uptick repeal, the slope coefficient decreases by 0.030 ($t = -1.64$), indicating that the existence of the partial uptick rule actually reduces the comovement between the pilot and non-pilot stocks, which is more consistent with the substitution hypothesis. The *t*-statistics in Table 5 Panel B for return comovements are in general smaller than those in Table 5 Panel A for short-selling comovements. This might not be surprising, because the uptick rule directly affects short-selling, and it is easier to observe significant changes in shorts around the rule changes, while returns can be affected by many other factors beyond short-selling, making it more difficult to identify significance.

In Fig. 3 Panels C and D, we plot the day-by-day return comovement coefficients, with coefficients estimated by regressing the non-pilot intraday returns on the pilot intraday returns each day, similar to specification in Eq. (11). Panel C reports day-by-day coefficients for return comovement for 2007, and Panel D reports parallel coefficients for 2005. The time-series of the daily coefficients for return comovements are more volatile than those of the shorting activity in Panels A and B, but they share similar patterns. That is, after the full repeal in 2007, there is a large increase in the return comovement, while after the partial repeal in 2005, there seems to be a large decrease in the return comovement. The timing of the changes coincides with the event date, and a pre-existing trend appears unlikely.

In Table 6, we further investigate the comovement pattern among subgroups. Suppose we take the 2007 full uptick repeal as an example. In Panel A, the coefficient θ_1 is 0.646, 0.784, and 0.819 for small-, mid- and large-cap firms, and the coefficient θ_2 is 0.222, 0.133, and 0.167 for these three groups of firms. All coefficients are highly significant. The comovement for small firms is lower than the large firms to start with, and after the full uptick repeal, the comovement between pilot and non-pilot stocks is much closer to one, indicating the full uptick repeal increases synchronicity more for the small firms. Between the S&P 500 member firms and non-member firms, the S&P firms behave similarly to the large firms, and the non-S&P firms are similar to the mid-cap and small-cap firms. In Panel B, similar patterns are observed for the comovement in returns. As mentioned earlier, returns are driven by more factors than just short-selling regulation changes, therefore the *t*-statistics are generally lower than those in Panel A. Results for the 2005 partial uptick repeal are qualitatively similar to what we observe for 2007 but in the opposite direction.

The subgroup results on comovement have two implications. First, the increases (decreases) in the comovement in 2007 (2005) further support that the positive (negative) indirect effect might exist for all subgroups. Second, the increases in comovements in 2007 are consistent with the list-based trading hypothesis for all subgroups, and the decreases in comovements in 2005 are consistent with the substitution hypothesis for all subgroups.

Table 6
Comovements among 15-min shorts and returns in 2007 and 2005, subgroups

This table reports the comovement of returns and shorting activity (*relss*) before and after the Reg SHO start in May 2005. Sample stocks are partitioned into market-cap terciles. We regress average non-pilot firms' shorts (returns) on the average pilot firms' shorts (returns), interacting with the event dummy A_t which takes the value of one after May 3, 2005, and zero otherwise. Panel A reports the comovement results on shorting activity, and Panel B reports the comovement results on returns. We report Newey–West standard errors with 5 lags. Each regression has 2080 observations.

Panel A: Comovement of 15-min shorting activities 2007 and 2005

		2007 full uptick repeal		2005 partial uptick repeal	
		coef.	*t*(NW)	coef.	*t*(NW)
Small	Pilot	0.646	8.05	0.970	53.94
	Pilot*A_t	0.222	5.11	−0.050	−3.80
Medium	Pilot	0.784	57.18	0.921	59.87
	Pilot*A_t	0.133	12.52	−0.098	−8.80
Large	Pilot	0.819	25.74	0.964	72.62
	Pilot*A_t	0.167	8.84	−0.053	−5.41
S&P	Pilot	0.852	48.61	0.944	65.64
	Pilot*A_t	0.155	12.14	−0.062	−6.28
Non S&P	Pilot	0.745	13.73	0.975	74.26
	Pilot*A_t	0.176	6.84	−0.075	−8.09

Panel B: Comovement of 15-min return 2007 and 2005

		2007 full uptick repeal		2005 partial uptick repeal	
		coef.	*t*(NW)	coef.	*t*(NW)
Small	Pilot	0.879	40.67	0.917	58.70
	Pilot*A_t	0.134	5.10	−0.076	−3.03
Medium	Pilot	0.939	94.92	0.980	97.03
	Pilot*A_t	0.042	3.18	−0.053	−2.64
Large	Pilot	0.990	118.26	0.971	89.39
	Pilot*A_t	0.028	2.18	−0.030	−1.64
SP	Pilot	0.973	117.16	0.990	85.59
	Pilot*A_t	0.005	0.41	−0.029	−1.67
Non SP	Pilot	0.926	40.30	0.963	96.57
	Pilot*A_t	0.104	4.26	−0.058	−3.19

5. Other related results

In the previous section, we study whether the repeal of the uptick rule has direct and indirect effects on shorting aggressiveness. In this section, we examine the direct and indirect effects of the uptick repeal on other important variables, such as shorting volume, market quality and liquidity measures, and stock price.

5.1. Effects of uptick repeal on shorting activity

We measure shorting activity in a given stock using *relss*, the NYSE short-sale volume over total NYSE trading volume in that stock, which has been used in several papers, including Boehmer et al. (2008). We first present the time-series of cross-sectional mean of *relss* in Panel C of Fig. 1. Before the 2007 full uptick repeal, pilot stocks have more shorting activity than non-pilot stocks. The difference in activity quickly narrows after the full uptick repeal. Interestingly, shorting activity for both the pilot and non-pilot stocks increases after the uptick repeal.

Table 7
Total, direct and indirect effects for market quality measures.
In this table, we report the total, direct and indirect effects coefficients for the following regression:

$$y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}.$$

Event dummy A_t takes a value of one for dates after the events and zero otherwise. For Panel A, the event date is July 6, 2007; for Panel B, the event date is May 2, 2005. The treatment dummy T_i takes a value of one for firms in the pilot program, and zero otherwise. The left half panel reports results without controls. The right half panel includes the following market-level controls $X_{t\text{-}1}$: VIX, average firm-level relative effective spread (*mktres*), and average firm-level absolute return persistence (*mktar*). All market-level controls are measured from the previous day. The daily measure of shorting activity, *relss*, is NYSE short-sale volume over NYSE trading volume. The relative effective spread, *res*, is the full proportional effective spread. The relative price impact, *rpi*, is the 5-min price impact. Absolute return persistence, *ar*, is computed as the absolute value of the AR(1) coefficient for a day of 30-min returns. The intraday variance (*intrav*) is computed with 30-min returns. Hasbrouck (1993) price inefficiency measure (*hasb*) is the volatility of noise over volatility of price. In Panel A, the total effect is measured by β_1, the direct effect is measured by $-\beta_3$, and the indirect effect is measured by $\beta_1 + \beta_3$. In Panel B, the total effect is measured by $\beta_1 + \beta_3$, the direct effect is measured by β_3, and the indirect effect is measured by β_1. *T*-stats are computed using standard errors double-clustered (DC) by date and firm.

Panel A: July 2007 uptick repeal

		Regression using dummy variables only						Regression using dummy variables and market controls					
		Relss	Res	Rpi	Ar	Intrav	Hasb	Relss	Res	Rpi	Ar	Intrav	Hasb
Expected sign for worse market quality			+	+	+	+	+		+	+	+	+	+
Total	coef.	0.087	1.422	0.377	0.007	0.141	−0.011	0.067	0.552	0.136	0.004	0.000	0.000
	t(DC)	8.75	5.23	4.06	0.57	2.76	−2.14	6.60	4.93	1.89	0.28	0.02	−0.11
Direct	coef.	0.063	0.591	0.151	0.002	0.032	−0.002	0.063	0.592	0.152	0.002	0.032	−0.002
	t(DC)	12.38	5.17	2.78	0.43	2.17	−1.07	12.36	5.18	2.78	0.43	2.15	−1.06
Indirect	coef.	0.023	0.831	0.225	0.006	0.109	−0.009	0.003	−0.040	−0.016	0.002	−0.032	0.001
	t(DC)	2.66	3.09	2.46	0.43	2.56	−1.85	0.35	−0.35	−0.25	0.17	−1.53	0.33

Panel B: May 2005 pilot start

		Regression using dummy variables only						Regression using dummy variables and market controls					
		Relss	Res	Rpi	Ar	Intrav	Hasb	Relss	Res	Rpi	Ar	Intrav	Hasb
Expected sign for worse market quality			+	+	+	+	+		+	+	+	+	+
Total	coef.	0.026	0.766	−0.059	−0.005	−0.049	0.007	0.025	0.764	−0.061	−0.011	−0.058	0.008
	t(DC)	4.64	4.71	−0.57	−0.51	−2.25	1.79	4.40	6.02	−0.68	−1.05	−3.08	2.13
Direct	coef.	0.026	1.053	0.104	0.002	0.015	0.000	0.026	1.054	0.105	0.002	0.015	0.000
	t(DC)	6.19	5.08	2.13	0.86	2.30	0.21	6.19	5.08	2.14	0.85	2.30	0.20
Indirect	coef.	0.000	−0.287	−0.163	−0.007	−0.065	0.007	0.007	−0.193	−0.066	0.000	−0.010	−0.008
	t(DC)	−0.07	−1.62	−1.69	−0.79	−2.90	1.68	1.66	−0.31	−0.50	0.08	−0.70	−1.90

Table 7 Panel A summarizes the direct and indirect effect coefficients both with and without market-level controls for the 2007 uptick repeal. Based on the specifications without controls, uptick repeal is associated with an 8.7% increase in shorting, relative to total trading volume. The standard difference-in-difference test would uncover only the direct effect coefficient, which we estimate at 6.3%, leaving an indirect effect coefficient of 2.3%. However, when we estimate the model with market-level controls, the indirect effect coefficient is no longer statistically discernible.[10]

In comparison, Table 7 Panel B estimates the direct and indirect effect coefficients of the May 2005 start of the Reg SHO pilot on shorting activity. In that case, the indirect effect coefficient is indistinguishable from zero both with or without control variables included. The direct effect coefficient is also much smaller at 0.026. Perhaps the effect is smaller simply because there is considerably less shorting in 2005.

5.2. Effects of uptick repeal on market quality measures

What should we expect in terms of liquidity and volatility, both of which are essential market quality measures? If the uptick rule forces some short-sellers to supply liquidity rather than demand it, the uptick rule might be mechanically associated with more liquid markets, as measured by bid-ask spreads or depths. If short-sellers are differentially informed and the uptick rule causes a change in the amount of shorting, this could also affect liquidity. For volatility, with less trading constraint, the trader might

[10] The specification with market controls has its own caveat. From unreported coefficients, the important control variable appears to be the previous day's market-wide effective spread. The amount of shorting is positively related to spreads, and this seems to account for the increase in shorting activity in control stocks. However, unlike the randomized grouping of stocks into pilot vs. non-pilot, variation in market-wide liquidity is endogenous, and in fact it is possible that the change in liquidity is caused by the final repeal of the uptick rule. Some commentators, including the CNBC commentator Jim Cramer, argue that uptick repeal is in fact responsible for some of the observed post-repeal decline in market quality. In that case, these control variables would be undesirable, as using them would mean throwing out some or all of the indirect effect baby with the bath water. Should we include the controls or not? Ultimately, we do not attempt to give a definitive answer, nor do we draw a conclusion as to whether uptick repeal causes spillover effects in terms of the amount of shorting. The discussion here is simply intended to highlight the issues and difficulties associated with measuring indirect effects.

choose to trade more aggressively, so we expect the volatility to increase. Diether et al. (2009) find that the 2005 pilot program to suspend price tests in the U.S. slightly worsens some measures of market quality.[11]

Here, we briefly examine a few market quality measures to see if the results from the full uptick repeal in 2007 match the results from the partial uptick repeal in 2005. For each NYSE common stock each day, we calculate several market quality measures, such as the effective spread (twice the distance between the trade price and the quote midpoint prevailing at the time of the trade, scaled by the prevailing quote midpoint), price impacts (the change in the quote midpoint in basis points five minutes after each signed trade), absolute return persistence (the absolute value of the AR(1) coefficient in a daily time-series regression of 30-min quote midpoint returns), the intraday variance (variance of 30-min quote midpoint returns), and a price inefficiency measure [the variance of the temporary component divided by the total price variance as in Hasbrouck (1993)]. According to Diether et al. (2009), the 2005 partial repeal worsens some of the market quality measures. If our results are consistent with the earlier finding, we expect that the direct liquidity measures, such as effective spread, price impact, and AR coefficient to increase, as well as direct volatility measures, such as intraday volatility and Hasbrouck measure. Given the diversity of our liquidity and volatility measures, we insert a row in Table 7 to show the expected signs of each coefficient for worse market quality for clarity.

Take the effective spread in Panel A of Table 7 as an example. Since the uptick rule is in place for only the non-pilot stocks in 2007, we expect non-pilot stocks subject to the rule to have narrower effective spreads than pilot stocks before the full repeal, all else equal. Once the uptick rule is fully repealed, we expect to see a widening of non-pilot stock effective spreads so as to match the pilot stock effective spreads. Without market controls in the left half panel of Panel A, we find the direct effect coefficient of the 2007 uptick repeal on the effective spread is 0.591, with a significant *t*-statistic. With market control in the right half panel, the direct effect coefficient becomes 0.592, still significant. Regardless of the specification chosen, the direct effect on liquidity is clear: repeal of the uptick rule somewhat worsens market liquidity, as measured by widening effective spreads. This matches the findings of other researchers from the start of the pilot in 2005, and the interpretation is fairly straightforward. In some situations, the uptick rule impedes liquidity demand by short-sellers and forces them to supply liquidity if they want to trade. Repealing the uptick rule reverses this artificial liquidity supply.

However, the indirect effect is important, as it could indicate that there is more going on than this simple story. Without market controls, the indirect effect coefficient of the 2007 uptick repeal on the effective spread is 0.831 and highly significant; while with market controls, the indirect effect coefficient becomes −0.040 and insignificant. Given different results with and without the market condition controls, we want to be cautious about our interpretation. Among the market condition controls, the lagged market-wide effective spread is correlated with the dependent variable, the effective spread, because of time-series persistence, which gives a reason to prefer the results without those controls. If so, results without the market condition controls reveal a large, positive, and significant indirect effect coefficient, indicting worsening market liquidity. As before, our main purpose is to highlight the existence of these indirect effects and discuss the methodological issues associated with their estimation.

Similar findings exist for the price impact measure and the intraday volatility measure, indicating worsening market liquidity and larger market volatility. The results on autoregressive coefficients and Hasbrouck measures are mostly insignificant.

5.3. Effects of uptick repeal on stock prices

In terms of share price levels and returns, theoretical models with differences in beliefs predict that stock prices should be higher when there are constraints on short sales. In these models, shorting restrictions mean that pessimists are shut out of the market, and optimists do not take into account the absence of pessimists in setting prices. If the truth is somewhere in between the optimists and pessimists, prices are too high. Examples of such models include Miller (1977), Harrison and Kreps (1978), and Duffie et al. (2002).[12] When short-sellers' information is not incorporated into prices because shorting is costly, difficult, or prohibited, the evidence indicates that stocks can get overvalued.[13] Looking at the imposition or removal of short-sale price tests, Rhee (2003) finds some evidence of price effects in Japan following the imposition of an uptick rule there. Diether et al. (2009) find during the 2005 pilot program, returns and volatility at the daily level are unaffected. On the other hand, Grullon et al. (2015) find a price effect in the weeks before the list of pilot stocks is announced on July 28, 2004.[14] As implied by Miller (1977), stock price effects should appear on the effective date of the new regulatory regime. In this case, the SEC announces on June 13, 2007 that short-sale price tests would be prohibited, with an effective date of July 6, 2007. And of course, if agents have completely rational expectations and common valuations or if the uptick rule does not im-

[11] Beber and Pagano (2013) and Boehmer et al. (2013) show that short-sale bans strongly degrade equity market quality such as liquidity and volatility, but bans impose much more severe restrictions on shorting compared to price tests. In particular, shorting bans may limit market-making, thereby worsening liquidity.

[12] In contrast, if all agents have rational expectations, as in Diamond and Verrecchia (1987), they do not agree to disagree, and shorting prohibitions do not cause stock prices to be biased on average.

[13] See, for example, Lamont and Thaler (2003) and Mitchell et al. (2002) for evidence of overvaluation around spinoffs. Pontiff (1996) provides similar evidence for closed-end funds. Jones and Lamont (2002) show that in the 1920s and 1930s, stocks that were expensive to short had abnormally low future returns, even after accounting for shorting costs.

[14] See also Danielsen and Sorescu (2001), who show that the introduction of listed options on a given stock eases shorting constraints and reduces share prices slightly. Chang et al. (2007) find price effects in Hong Kong when specific stocks are designated as eligible for shorting.

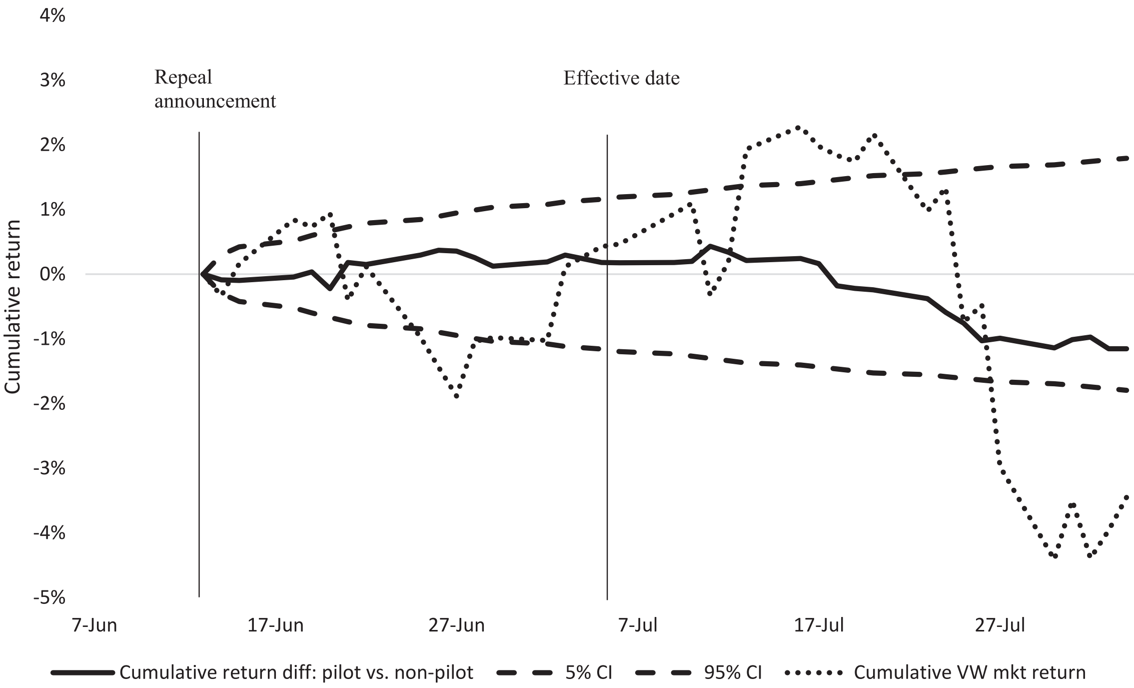

Fig. 4. This figure shows cumulative returns on the overall stock market (dotted line) over the uptick repeal period as well as the differential return on pilot vs. non-pilot stocks (solid line). We also report the confidence bounds (dashed lines) for the return differentials, two standard errors in either direction, using a daily standard deviation of the pilot vs. non-pilot value-weighted (VW) portfolio return difference of 0.15% based on returns up to that date in 2007.

pede short-sellers, repeal of the uptick rule should have no effect on share prices.

Fig. 4 shows the cumulative returns on the overall stock market over this time period as well as the differential return on pilot vs. non-pilot stocks. If the shorting constraint models are correct, non-pilot stocks should fall on the news of the uptick rule repeal, at least relative to the control group of unaffected pilot stocks. The figure shows the cumulative return of pilot less non-pilot stocks. This return should be positive if the shorting constraint models are correct, the uptick rule actually restricts informed short-sellers, and the announcement of the repeal is unanticipated. The confidence bounds are approximately two standard errors in either direction, using a daily standard deviation of the pilot vs. non-pilot value-weighted portfolio return difference of 0.15% based on returns up to that date in 2007. On announcement, the pilot vs. non-pilot return difference is virtually zero, and in fact non-pilot stocks slightly outperform over a longer holding period through the end of August 2007. Similarly, little happens immediately around the effective date of July 6, 2007. The pilot vs. non-pilot return difference is again indistinguishable from zero.[15]

6. Conclusions

In this paper, we discuss potential treatment spillovers in Reg SHO pilot programs and other financial regulatory experiments. The upshot is that randomization into treatment and control firms does not yield ideal results if the regulatory treatment results in externalities, behavioral responses, or general equilibrium effects that alter outcomes for control stocks. If control stocks are affected by the regulatory pilot, then different econometric techniques are required to discern the various effects caused by the regulatory change.

In particular, we study the July 6, 2007 full repeal of the uptick rule that limited short sales on the NYSE. Some stocks were already exempt from the uptick rule due to an SEC pilot program begun in 2005 (the partial uptick repeal). We use these pilot stocks as a control group, presumably unaffected by the regulatory change. The remaining stocks were affected by the repeal, and we use these non-pilot stocks as the treatment group. When the full repeal takes effect, short-sale orders on average become more aggressive in both affected and unaffected stocks, which indicates a positive indirect effect. It is possible that when shorting impediments, the uptick rule, are eased for all stocks, it facilitates more list-based shorting in both pilot and non-pilot stocks, which is associated with the

[15] These results differ from those found by Grullon et al. (2015) at the start of the pilot. It could be that this action was not really news to the market. Most observers expected the repeal of the uptick rule at some point, though the exact timing remained uncertain. It could also be that while the uptick rule might affect liquidity providers, quant funds, and other short-term traders, it has little effect on long-term fundamentals-based shorting strategies. In fact, at a 2006 roundtable hosted by the SEC, one fundamentals-based hedge fund manager characterized the uptick rule as only a “minor nuisance” in taking short positions.

positive indirect effect. We provide supporting evidence that the comovement in shorting activities and returns between pilot and non-pilot stocks becomes significantly higher after the full uptick repeal.

In comparison, we also apply our methodology to the partial uptick repeal in 2005, and we find an opposite, significantly negative indirect effect coefficient. Possibly when partial repeal removes a shorting impediment for pilot stocks, short-sellers would favor these stocks over the non-pilot stocks, and the negative indirect effect coefficient is likely driven by substitution between pilot and non-pilot stocks. We find that the comovement between pilot and non-pilot stocks is significantly lower after the partial uptick repeal, which supports the substitution hypothesis.

Fortunately, these indirect effects do not sharply degrade market quality in the 2007 full uptick repeal. Overall, uptick repeal causes market liquidity to worsen slightly, and prices incorporate common factor information more quickly.

The possibility of treatment spillovers provides a cautionary tale for those designing regulatory experiments. We do not mean to dissuade regulators and other policymakers from pursuing regulatory experiments. Randomized pilot programs remain the cleanest way to evaluate the effects of rule changes, and we hope the current trend toward more such trials continues. However, pilot planners should think carefully about how a pilot might affect control stocks or firms. Designers probably should look for potential externalities, behavioral responses by investors in control stocks or management of control firms, or other general equilibrium effects.

For example, the SEC has embarked on a pilot program that changes the minimum tick and related rules for a subset of small-cap stocks, all in an effort to identify market structure alterations that might improve liquidity in this notoriously illiquid sector of the market. To be eligible for the pilot, firms must have a market cap of at most $3 billion, a share price of at least $2, and average daily trading volume of at most one million shares. Approximately 1200 stocks are included in the pilot, divided into three test groups. One test group is quoted in minimum increments of $0.05. A second test group also places restrictions on trade prices and requires internalizers of retail order flow to provide a minimum price improvement of $0.005. A third group would also impose a so-called "trade-at" rule, requiring off-exchange trades to provide significant price or size improvement. There is also a control group of about 1400 stocks. Unlike the Reg SHO pilot, portfolio trading effects are most likely not particularly important for this particular regulatory experiment. But there could be important substitution effects. For example, some investors might move their trading activities from one group to the other, either from control stocks to treatment stocks, or from treatment stocks to control stocks. Alternatively, traders and investors might move into or out of the entire illiquid small-cap sector due to the pilot. Pilot designers and researchers should take these possibilities into account; otherwise, it may prove difficult to draw conclusions from the resulting data.

In addition, our approach has wide applicability in finance research, largely due to the prominence of the difference-in-difference methodology. In fact, we find 122 papers in the top three finance journals between 2006 and 2015 that apply some sort of difference-in-difference methodology. To what extent are our concerns about potential spillovers and estimation approach relevant for these studies? For illustration, we pick two types of regulation changes as examples. The first type includes tax rate changes, such as the tax cut in the U.S. dividend tax rate (Brown et al., 2007), tax rate changes for capital gains (Morellec and Schurhoff, 2010), and other tax changes around the world (Becker et al., 2013). For example, the dividend tax directly affects firms paying dividends. These firms could be expected to change their payout policies. However, firms that do not pay dividends may also alter their payout policies, i.e., begin to pay dividends. Such an indirect effect could arise if, for example, more investors prefer dividends after the tax cut. Changes to trading rules could also have spillovers. One example is the 2008 shorting ban on financial firms, which directly restricted shorting of financial stocks. In this case, the regulation change could also affect trading behavior in nonfinancial firms. For example, Boehmer et al. (2013) show that the ban has a significant impact on overall liquidity, trading volume, and volatility, suggesting that the ban indeed has an indirect effect on nonfinancial firms.

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Appendix

Table A1
Total, direct and indirect effects of the May 2005 partial uptick repeal, subgroup analysis.
In this table, we report the total, direct and indirect effects coefficients for the following regression:

$y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}$.

Event dummy A_t takes a value of one for dates after July 6, 2007, and zero otherwise. The treatment dummy T_i takes a value of one for firms in the pilot program, and zero otherwise. Each regression is estimated for two different dependent variables. The first dependent variable *shortres* is the relative effective spread for short sales only. The second dependent variable *fmkt* is the fraction of short-sale orders that are marketable. The left half panel reports results without controls. The right half panel includes the following market-level controls X_{t-1}: VIX, average firm-level relative effective spread (*mktres*), and average firm-level absolute return persistence (*mktar)*. All market-level controls are measured from the previous day. Panel A divides the NYSE-listed sample into three market-cap terciles; Panel B partitions based on membership in the S&P 500. The regressions are estimated over days [−20, +20] around May 2, 2005. The total effect is measured by $\beta_1 + \beta_3$, the direct effect is measured by β_3, and the indirect effect is measured by β_1. T-stats are computed using standard errors double-clustered by date and firm.

Panel A: Size groups

		Regression using dummy variables only						Regression using dummy variables and market controls					
		shortres small	shortres medium	shortres large	fmkt small	fmkt medium	fmkt large	shortres small	shortres medium	shortres large	fmkt small	fmkt medium	fmkt large
Total effect	coef.	3.836	1.174	0.948	−0.045	−0.004	0.017	3.342	1.072	0.777	−0.053	−0.009	0.013
	t(DC)	2.63	3.48	4.04	−2.44	−0.44	2.25	2.22	3.34	3.31	−2.63	−0.93	1.73
Direct effect	coef.	6.714	2.475	1.486	−0.007	0.026	0.031	6.711	2.474	1.486	−0.007	0.026	0.031
	t(DC)	6.10	9.03	8.26	−0.76	3.77	5.03	6.09	9.02	8.25	−0.78	3.76	5.03
Indirect effect	coef.	−2.878	−1.300	−0.539	−0.038	−0.030	−0.014	−3.369	−1.402	−0.709	−0.046	−0.035	−0.018
	t(DC)	−2.55	−3.53	−2.50	−1.87	−3.11	−2.40	−3.05	−4.18	−3.34	−2.05	−3.58	−3.08

Panel B: Effects by S&P500 membership

		Regression using dummy variables only				Regression using dummy variables and market controls			
		shortres member	shortres no	fmkt member	fmkt no	shortres member	shortres no	fmkt member	fmkt no
Total effect	coef.	0.708	2.377	0.527	2.096	0.009	−0.017	0.004	−0.023
	t(DC)	3.02	3.12	2.23	2.70	1.13	−1.36	0.53	−1.71
Direct effect	coef.	1.232	4.312	1.232	4.310	0.024	0.015	0.024	0.015
	t(DC)	7.36	8.08	7.34	8.07	3.43	2.61	3.42	2.60
Indirect effect	coef.	−0.525	−1.935	−0.706	−2.214	−0.015	−0.032	−0.020	−0.038
	t(DC)	−2.32	−2.94	−3.23	−3.52	−2.24	−2.41	−2.98	−2.65

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