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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* ." 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* ," 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

M322TJ 0.000299999 Tthf 6.3761 0 0 6.3761 208.6286 251.8111 Tm ( )] TJ /0.000299999 T

on the SUTVA literature [redacted] provide strong evidence t[redacted] spillovers might exist an[redacted]ould be important in the c[redacted] text of changes in the up[redacted] rule in 2005 and 2007.

Our paper is relate[redacted] previous studies on upt[redacted] rule changes, such as Al[redacted]nder and Peterson (2008) a[redacted] Diether et al. (2009). Th[redacted] are two major differences b[redacted] tween our paper and Al[redacted]nder and Peterson (2008) a[redacted] Diether et al. (2009). Fir[redacted] both of these papers focus [redacted] the 2005 partial uptick [redacted]eal, while we mainly exami[redacted] the 2007 full uptick repe[redacted] We also show that the SUT[redacted] assumption might be vi[redacted]ed in fundamentally differe[redacted] ways in 2007 vs. 2005. S[redacted]ond, both of the above pape[redacted] focus on market quality [redacted]asures, such as spreads, pri[redacted] impacts, volume, and vol[redacted]ity measures. We still exami[redacted] these market quality me[redacted]res for completeness, but o[redacted] main focus is on short-s[redacted]ng activity, especially in term[redacted] of aggressiveness. We ch[redacted]e to concentrate on short-sa[redacted] aggressiveness because [redacted] want to identify the specif[redacted] changes in trading beha[redacted]r associated with the regula[redacted]tions, and aggressivenes[redacted]s a direct measure of shor[redacted] selling activity and a str[redacted]gic response to the regulatio[redacted] [redacted]hange.

To summarize, our st[redacted] provides three unique contr[redacted] [redacted]utions. First, we study [redacted]w the 2007 full repeal of th[redacted] [redacted]ptick rule

so [redacted]ort-)] TJ 0 Tc /F2 1 Tf 6.376[redacted] 0 6.3761 282.0687 429.642 Tm [( )] TJ 0.0002 Tc /F1 1 Tf 7.9701 0

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

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. O

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 ($\;\;=1$), for each variable under investigation, we have:

$$E[\;\;(\;\;=1,\psi)] = E\left[\;\;^{O}\;\;|\;\;=1,\psi\right] - E\left[\;\;^{E}|\;\;=1,\phi\right], \tag{4}$$

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

$$E[\;\;(\;\;=0,\psi)] = E\left[\;\;^{O}\;\;|\;\;=0,\psi\right] - E\left[\;\;^{E}|\;\;=0,\phi\right], \tag{5}$$

where the two subtracted terms are the same in expectation due to randomization before treatment begins.

price and the quote midpoint prevailing at the time of the trade, scaled by that quote midpoint, variable), 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).

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 -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) 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 on day . That is,

$$= \sum_{\in} (M - P)/M , \qquad (9)$$

where P is the price at which shares are sold short at time , M is the prevailing quote midpoint at the time of the short sale, and the weight is the size of the short sale, at time , in shares divided by the total number of shares shorted that day in stock . 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 , 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, , is NYSE short-sale volume over NYSE trading volume. Variable is the relative effective spread for short sales only. Variable is the fraction of short-sale orders that are marketable. The relative effective spread, , is the full proportional effective spread. The relative price impact, , is the 5-min price impact. Absolute return persistence, , is computed for each stock-day as the absolute value of the AR(1) coefficient based on 30-min returns. The intraday variance ( ) is computed with 30-min returns. Hasbrouck (1993) price inefficiency measure ( ) 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,	0.374	0.292	0.399	0.380
Relative effective spread for short-sale orders only (bps),	−2.271	−4.667	−1.344	−1.593
Fraction of marketable shorts,	0.339	0.321	0.385	0.377
Relative effective spread (bps),	4.844	4.608	5.381	5.643
Relative price impact (bps),	0.781	0.773	0.892	0.930
Absolute return persistence,	0.215	0.215	0.220	0.221
Intraday variance (bps),	0.090	0.089	0.160	0.171
Hasbrouck price inefficiency,	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

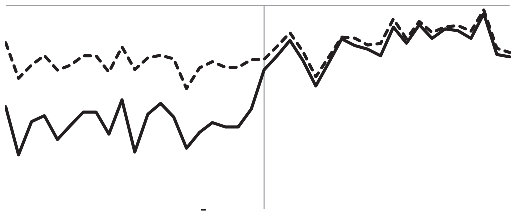


Table 2

Diff-in-diff regressions around July 2007 uptick repeal. [redacted]

In this table, we report coefficients for the following [redacted]on:

$$= \beta_0 + \beta_1 A + \beta_2 + \beta_3 A + \gamma _{-1} + .$$

Event dummy A takes a value of one for dates after [redacted]007, and zero otherwise. The treatment dummy takes a [redacted] one for firms in the pilot program, and zero otherwise. Eac[redacted]sion is estimated for two different dependent variables. The [redacted]pen-dent variable is the relative effective spread fo[redacted]sales only. The second dependent variable is the fracti[redacted]ort-sale orders that are marketable. Panel A reports res[redacted]hout controls. Panel B includes the following market-level c[redacted] $_{-1}$: VIX, average firm-level relative effective spread ([redacted]ver-age firm-level absolute return persistence ( ). All [redacted]level controls are measured from the previous day. The reg[redacted] are estimated over days [−20, +20] around July 6, 2007. [redacted]l ef-fect is measured by β_1, the direct effect is measured [redacted] and the indirect effect is measured by $\beta_1 + \beta_3$. -stats are c[redacted]d us-ing standard errors double-clustered (DC) by date and [redacted]

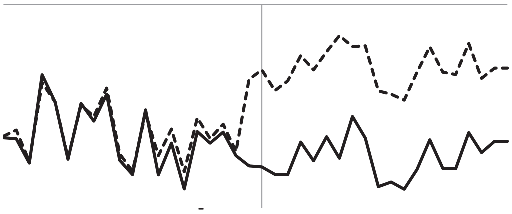
P A:

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

P B: -

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

While the difference-in-difference approac[redacted]entifies an increase in shorting aggressiveness, there [redacted]o evidence of an indirect effect. After the tick test re[redacted] shorting aggressiveness increases even for the pilot [redacted]ks that were already exempt from the tick test and s[redacted]d have been unaffected by the regulatory change. As [redacted]a[redacted]ove, pilot stock shorting receives 3.074 basis poin[redacted]ec-tive bid-ask spread before the repeal and only [redacted]5 [redacted]asis points after repeal, which is 36% less. The fact[redacted]s[redacted]ort-ing in



measures for the 2005 partial uptick repeal in Fig. 2. In Panel A, before May 2005, the time-series for the pilot stocks and non-pilot stocks are very similar. Then one day before the regulation change, the 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 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 pre-scheduled 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

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:

$$= \beta_0 + \beta_1 A + \beta_2 \quad + \beta_3 A \quad + \gamma \quad _{-1} + \quad .$$

Event dummy A takes a value of one for dates after July 6, 2007, and zero otherwise. The treatment dummy 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 is the relative effective spread for short sales only. The second dependent variable is the fraction

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. :

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:

$$= \theta_0 + (\theta_1 + \theta_2 A) \quad + \; , \tag{10}$$

where is the intraday average shorting activity on non-pilot stocks, is the contemporaneous 15-min average shorting activity on pilot stocks, and A 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 ( ) 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 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.

A: C 15- 2007 2005				
	2007 full uptick repeal		2005 partial uptick repeal	
	coef.	(NW)	coef.	(NW)
Pilot	0.785	18.67	0.977	91.98
Pilot*A_t	0.171	8.11	−0.074	−9.73

B: C 15- 2007 2005				
	2007 full uptick repeal		2005 partial uptick repeal	
	coef.	(NW)	coef.	(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 (= 18.67), significantly lower than one. This slope coefficient rises by 0.171 (= 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 (= −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,

$$= \theta_0 + \theta_1 \quad + \; , \tag{11}$$

where the coefficient θ_1 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-

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 ($= -1.64$), 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 -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.
(timing)] TJ 0 Tc /F2 1 Tf 6.3761 0 0 6.3[( ) [( )] TJ 0 Tc /F2 1 Tf 6.3761 0 0 6.3761 21521 0 0 6.3761 8276.7169 50

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:

$$= \beta_0 + \beta_1 A + \beta_2 + \beta_3 A + \gamma \; _{-1} + \; .$$

Event dummy A 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 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 $_{-1}$: VIX, average firm-level relative effective spread ( ), and average firm-level absolute return persistence ( ). All market-level controls are measured from the previous day. The daily measure of shorting activity, , is NYSE short-sale volume over NYSE trading volume. The relative effective spread, , is the full proportional effective spread. The relative price impact, , is the 5-min price impact. Absolute return persistence, , is computed as the absolute value of the AR(1) coefficient for a day of 30-min returns. The intraday variance ( ) is computed with 30-min returns. Hasbrouck (1993) price inefficiency measure ( ) 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. -stats are computed using standard errors double-clustered (DC) by date and firm.

P A: J 2007

		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			+	+	+	+	+		+	+	+	+	+
	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
	(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
D	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
	(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
I	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
	(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

P B: M 2005

		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			+	+	+	+	+		+	+	+	+	+
	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
	(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
D	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
	(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
I	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
	(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. E

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 -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.

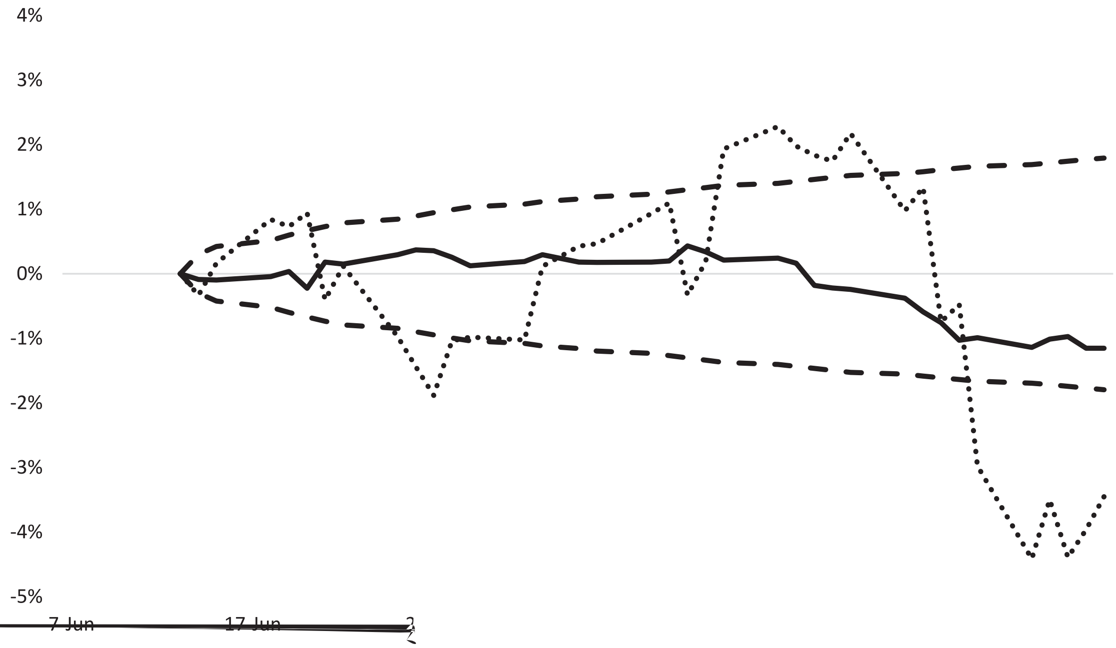
However, the indirect effect is important, as it could indicate that there is more going on than this simple story. Without market controls, the indirect effect coefficient of the 2007 uptick repeal on the effective spread is 0.831 and highly significant; while with market controls, the indirect effect 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. E

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

[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.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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 conclu-

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:

$$= \beta_0 + \beta_1 A + \beta_2 + \beta_3 A + \gamma_{-1} + .$$

Event dummy A takes a value of one for dates after July 6, 2007, and zero otherwise. The treatment dummy 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 is the relative effective spread for short sales only. The second dependent variable 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 $_{-1}$: VIX, average firm-level relative effective spread (

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