POTENTIAL PILOT PROBLEMS: TREATMENT SPILLOVERS IN FINANCIAL REGULATORY EXPERIMENTS

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ABSTRACT

Generally, the total effect of a regulatory change consists of direct and indirect effects. The standard difference-in-difference approach measures only direct effects, assuming the control group is unaffected. We examine the 2007 SEC repeal of the uptick rule and find that the indirect effects are substantial. Unlike the 2005 partial repeal, total repeal enables aggressive portfolio shorting. Short sellers become much more aggressive across the board, and shorting activity increases, even in control stocks where the uptick rule was already suspended. We conclude that regulatory pilot designers should carefully consider potential spillovers.

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1. Introduction

Many financial regulatory policy changes are hard to study. For example, new rules are typically imposed on all firms all at once. If it is important 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 the 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. Standard econometric techniques, such as a difference-in-difference approach, measure only the direct effect and may lead to the wrong conclusions. Indirect effects are due to externalities of some sort, where treatments have spillover 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. Obviously, the treated individual benefits from the drug, and that is considered a direct effect. In addition, treated individuals are less likely to infect others in the same village. Thus, untreated individuals in the same Kenyan village (the control group) also benefit from the treatment, and this is an indirect or spillover effect. Standard econometric techniques assume that the control group is actually 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 US Securities and Exchange Commission from 2005 to 2007. In 2005, exactly 1,000 of the Russell 3000 stocks (every third stock based on market capitalization) are essentially randomly assigned to the pilot program suspending price tests that limit short selling. In 2007, the SEC repealed price tests for the remaining two-thirds of Russell 3000 stocks.

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 narrowed to a penny in 2001, the uptick rule became a much smaller impediment to shorting. Also, as trading volume 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 Regulation SHO, which made a number of changes to short-sale regulations, the SEC announced a pilot program to suspend short sale price tests in 1,000 different stocks. The pilot program took effect in May 2005 and was expressly designed to allow the commission to study the effectiveness of the rule. Diether, Lee, and Werner (2009) and Alexander and Peterson (2008) conclude that suspending the uptick rule has only modest effects on bid-ask spreads and other measures of market quality.

On June 13, 2007, the SEC announced that it was eliminating all short sale price tests, effective July 6, 2007, and in this paper we focus on this final repeal of the uptick rule. We take advantage of the random assignment and, starting with standard econometric techniques, we examine the direct effects of uptick repeal on the aggressiveness of short sellers, shorting activity, share price behavior, and various liquidity measures. We provide strong evidence that uptick repeal has spillover effects. In particular, we find that short sellers become much more aggressive across the board, and shorting activity increases, even in control stocks where the uptick rule was already suspended.

We find evidence that final repeal of the uptick rule has a heretofore unexpected indirect effect: it enables aggressive, liquidity-demanding short sales of broad stock portfolios, such as index arbitrage. This trading behavior does not occur at the start of the pilot in 2005, because the shorting restrictions remain in place for two-thirds of stocks. Only when the tick test is completely repealed does this trading behavior start to manifest itself.

[1] Short sale price tests of a different form were also present in Nasdaq-listed stocks. As noted by Diether, Lee, and Werner (2009), the price tests for Nasdaq-listed 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 for an analysis of the introduction of the uptick rule and other U.S. short sale regulatory changes that took place in the 1930's.

Our main contribution is twofold. First, we propose an estimation procedure to measure the indirect and direct effects of randomized regulatory changes. Second, we apply our estimation procedure to a specific event, the repeal of the uptick rule, and show that the indirect effect can be significant and substantial.

The difference-in-difference approach is an important methodology for finance research, especially for event studies. We searched the top 3 finance journal publication for 2006 to 2015. In total, 122 papers applied the methodology. Are our concern and estimation relevant for other difference-in-difference studies? W For illustration, we pick two regulation changes as examples. The first is the 2003 dividend tax cut act, which directly affects firms paying dividends. But faced with changes in the payout policies of firms that are directly affected by the act, firms that do not pay dividends may also alter their payout policy, i. e., begin to pay dividends. Such an indirect effect could arise if, for example, more investors prefer dividends after the Act than before. The second regulation change of interest is the 2008 shorting ban on financial firms, which directly shuts down shorting of financial stocks. In this case, the regulation change can easily affect also the trading behavior in non-financial firms. For example, Boehmer, Jones, and Zhang (2013) show that the ban has significant impact on overall liquidity, trading volume, and volatility. These effects are caused by the ban and create an indirect effect of the ban for non-financial firms.

The paper is organized as follows. Section 2 discusses the econometrics of regulatory experiments, and we establish testable hypotheses in Section 3. Section 4 discusses the data. We present the empirical results in Section 5, and Section 6 concludes with some advice for those designing regulatory experiments in the future.

2. The econometrics of regulatory experiments

We adopt the potential outcomes framework of Rubin (1974), most closely following the notation of Hudgens and Halloran (2008). Assume that there are n firms, and let $Y_i(z_i,\psi)$ be a random variable reflecting the potential outcome for firm i given its own treatment z_i. In our case, the treatment is binary, and $z_i = 1$ if firm i is subject to the regulatory change, and $z_i = 0$ 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:

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

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 firms to give the average direct treatment effect:

$$DE(\psi) = \sum_{i=1}^{N} DE_i(\psi) = \sum_{i=1}^{N} E[Y_i(1,\psi) - Y_i(0,\psi)],$$

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. The case of no treatment, $\phi = 0$, corresponds to the situation before a regulatory pilot program begins. If the pilot is then extended to all firms, one can think of the treatment strategy changing from the original pilot fraction ϕ to $\psi = 1$. There are also cases where a researcher would naturally compare two non-degenerate treatment assignment strategies. For example, especially in light of recent measles outbreaks in the United States, one might compare the results of vaccinating, say, 75% of the population to vaccinating 50% of the population.

To compare two treatment strategies ψ and ϕ, we seek to measure the overall treatment effect:

$$TE(\psi,\phi) = \sum_{i=1}^{N} E[Y_i(1,\psi) - Y_i(0,\phi)].$$

This can be rewritten as:

$$TE(\psi,\phi) = \sum_{i=1}^{N} E\{[Y_i(1,\psi) - Y_i(0,\psi)] + [Y_i(0,\psi) - Y_i(0,\phi)]\}.$$

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:

$$IE(\psi,\phi) = \sum_{i=1}^{N} E[Y_i(0,\psi) - Y_i(0,\phi)].$$

This has the natural interpretation as the spillover effect on an untreated firm from changing the overall treatment strategy from ϕ to ψ. In economics, the indirect effect is also sometimes called a treatment externality or general equilibrium effect, while in statistics settings, this effect is often referred to as interference. Mathematically, the overall treatment effect is simply the sum of direct and indirect treatment effects:

$$TE(\psi, \phi) = DE(\psi) + IE(\psi, \phi).$$

So far, our decomposition says nothing about experimental design, estimation, or statistical inference. Estimation of direct and indirect effects is easiest when there are many different groups of subjects, with only within-group spillovers. For example, the spillover effect in Miguel and Kremer (2004) is that individual treatments for worms benefit others in the same village, because treated individuals are less likely to infect others.[3] In designing their experiments, the key observation is that villages are separated, and worm treatments in one village are assumed to have zero effects in other villages. Identification of direct and indirect treatment effects is then obtained by varying the fraction treated across villages.

The problem in financial regulatory settings is that there is usually only one village. This makes it difficult to identify indirect or spillover effects. In the case of the Reg SHO pilot, one-third of Russell 3000 stocks are selected for the regulatory treatment, essentially at random (ψ = 1/3). At the beginning of the pilot, we argue that there are essentially four different observations. Before the pilot 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 pilot starts, we again measure average outcome variables for both pilot and non-pilot stocks with $\psi = 1/3$. Recall that the direct effect is defined as:

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

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) we have:

$$E[\overline{Y}_i^{POST} \mid \psi, T_i = 1] - E[\overline{Y}_i^{PRE} \mid \phi = 0, T_i = 1] = E[Y_i(1, \psi)],$$

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

$$E[\overline{Y}_i^{POST} \mid \psi, T_i = 0] - E[\overline{Y}_i^{PRE} \mid \phi = 0, T_i = 0] = E[Y_i(0, \psi)],$$

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

[3] Other examples in the economics literature include Heckman, Lochner, and Taber (1998) and Angelucci and Di Maro (2010).

direct effect is the standard difference-in-difference estimator. In a difference-in-difference regression setting:

$$Y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \varepsilon_{it},$$

where 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. Now consider the indirect effect, which is given by:

$$IE_i(\psi, \phi) = E[Y_i(0, \psi) - Y_i(0, \phi)].$$

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:

$$E[\overline{Y}_i^{POST} \mid \psi, T_i = 0] - E[\overline{Y}_i^{PRE} \mid \phi, T_i = 1] = E[Y_i(0, \psi) - Y_i(0, \phi)].$$

In the difference-in-difference regression specification, the coefficient β_1 measures the indirect treatment effect. Equivalently, the spillover effect is the average change in control firm outcome moving from the old to the new treatment strategy.

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 will confound estimates of the indirect effect. There is no panacea in this case. The difference-in-difference specification must simply be augmented with a vector of control variables X_{it} that captures the changes in these market conditions:

$$Y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{it} + \varepsilon_{it}.$$

These controls can usually be reduced to X_t based on the treatment randomization. Still, the choices of these control variables typically engender the usual arguments about exogeneity, appropriateness, and completeness, and unless the financial regulatory experiment can be applied to several distinct, non-interacting villages (or perhaps countries), there is no escaping these arguments.

As discussed in Bertrand et al (2004), the standard errors in difference-in-difference regressions can be biased. Thus, all t-statistics in the paper are double-clustered by date and firm. Because double clustering does not guarantee positive definiteness of the variance-covariance matrix, we conduct inference using the heteroskedasticity-consistent standard error if it is larger than the corresponding double-clustered standard error.

3. Hypothesis development and literature review

Clearly, the uptick rule constrains the trading behavior of 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. Others might choose not to trade at all. Thus, when the uptick rule is repealed, we expect to see short sellers trade more aggressively. They should use more marketable orders vs. limit orders, for example.

With the restriction lifted, it is also natural to expect shorting activity to increase for the non-pilot firms. Alexander and Peterson (2008) and Diether, Lee, and Werner (2009) find evidence of this for the pilot firms at the beginning of the Reg SHO pilot in 2005. Price tests are reimposed in 2010 on stocks that fall by more than 10% in a day, and Jain, Jain and McInish (2012) find that these price tests affect short sale aggressiveness and the amount of shorting.

What should we expect in terms of liquidity? There are at least two channels of interest. First, if the uptick rule forces some short sellers to supply liquidity rather than demand it, the uptick rule might be rather 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. This observation goes back to Diamond and Verrecchia (1987), who point out that since short sellers do not have the use of the sale proceeds, shorting never takes place for liquidity reasons, and one might expect more information content in short sales. Many researchers have found that short sellers are well-informed about fundamentals (see, for example, Dechow et al. (2001), Desai, Krishnamurthy, and Venkataraman (2006), Cohen, Diether, and Malloy (2007), and Boehmer, Jones and Zhang (2008, 2015), suggesting that limits on shorting could affect the information revealed through trading. Diether, Werner, and Lee (2009) find that the 2005 pilot program to suspend price tests in the U.S. slightly worsens some measures of market quality. [4]

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

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

between the optimists and pessimists, prices are too high. Examples of such models include Miller (1977), Harrison and Kreps (1978), and Duffie, Garleanu and Pedersen (2002).[5] When short sellers' information is not incorporated into prices because shorting is costly, difficult, or prohibited, the evidence indicates that stocks can get overvalued.[6]

Looking at the imposition or removal of short sale price tests, Rhee (2003) finds some evidence of price effects in Japan following imposition of an uptick rule there. Diether, Lee, and Werner (2009) make use of the standard difference-in-difference approach. They find that the 2005 pilot program increases shorting activity for the pilot stocks and worsens some measures of market quality, while returns and volatility at the daily level are unaffected. On the other hand, Grullon, Michenaud, and Weston (2015) find a price effect in the weeks before the list of pilot stocks is announced on July 28, 2004.[7]

4. Data

The uptick repeal sample extends for 20 trading days before and after uptick repeal became effective on July 6, 2007. In addition to standard data sources, such as TAQ and 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 against CRSP and retain only NYSE-listed common stocks, which means that we exclude securities such as warrants, preferred shares, American Depositary Receipts, closed-end funds, and REITs. 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 1,088 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 cap,

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[5] 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.

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

[7] 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, Cheng, and Yu (2007) find price effects in Hong Kong when specific stocks are designated as eligible for shorting.

book-to-market, trading volume, shorting activity, and market quality measures. The two groups (pilot and non-pilot) are very similar, which is not surprising given the original assignment algorithm for the SEC pilot program. For example, the 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. To revisit other researchers' findings for the initial imposition of the pilot, we also use the same approach to create a sample of NYSE-listed stocks for the 20 trading days surrounding the start of the Reg SHO pilot in May 2005.

In some of our tests, we look for heterogeneous effects by examining subsets of stocks. Table 1 also provides summary statistics with the sample partitioned into three market-cap terciles. The small-cap category has a median market cap of about $0.8 billion, and the mid-cap category has a median market cap of about $3 billion. The median large-cap pilot stock has a market capitalization of $14.3 billion, while the median large-cap non-pilot stock has a market cap of $16.5 billion. We also partition on membership in the S&P 500 index, and Table 1 also contains summary statistics for this partition. Of our sample firms, 317 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.

For parts of the analysis, we do not need order-level short-sale data, and in that case it is more convenient to work with daily shorting data for each stock. We measure daily shorting flow as the fraction of NYSE trading volume executed in a given stock on a given day that involves a system short seller.

5. Empirical results: direct vs. indirect effects of uptick repeal

This section contains our main results. We start in section 5.1 with an analysis of changes in shorting aggressiveness, including the identification of indirect effects. Section 5.2 discusses the changes in the intraday co-movement of stock returns and shorting activity, providing evidence that list-based shorting is behind the identified indirect effects. Section 5.3 examines the changes in overall shorting activity associated with uptick repeal. Section 5.4 measures liquidity effects using effective spreads and price impacts. Section 5.5 looks at various subsets of stocks and finds some cross-sectional differences in the causal effects of the uptick rule.

5.1 Effects of uptick repeal on shorting aggressiveness

The uptick rule directly limits the aggressiveness of short sellers, and thus shorting aggressiveness is the clearest laboratory for our methodological investigation into direct and indirect effects of the rule change. As discussed above, once the constraint is removed, short sellers are free to demand or supply liquidity as they see fit, and we would expect to see shorts use more marketable orders vs. limit orders. As a result of this shift away from supplying liquidity, we should 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.

Following this logic, we define aggressiveness based on the average effective (half) spread in cents paid by short sellers in stock *i* on day *d*. This is defined as:

$$F_{id} = \sum_{t \in d} w_{it}(M_{it} - P_{it}),$$

where P_{it} is the price at which shares are sold short at time *t*, M_{it} is the prevailing quote midpoint at the time of the short sale, and the weight w_{it} is the size of the time *t* short sale in shares divided by the total number of shares shorted that day in stock *i*. This measure is negative if short-sellers provide liquidity on average and positive if they demand liquidity on average.

Another proxy for shorting aggressiveness is the pricing of the order relative to the existing quote. Specifically, we calculate the fraction of submitted short-sale orders that are marketable 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. Higher percentages of marketable orders indicate more aggressive shorts.

Figure 1 and Table 2 provide details on shorting aggressiveness in both pilot and non-pilot stocks, before and after the July 6, 2007 repeal of the tick test. The first column of Panel A contains a simple differences-in-differences 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 until 2007. The difference is strongly statistically significant (*t*-stat = 9.81). This shows that short sellers are constrained by the tick test to supply rather than demand liquidity in some situations. This result is not driven by differences in overall liquidity between pilot and non-pilot stocks: the pre-repeal average effective spread for non-pilot stocks is 6.464 basis points, which is statistically indistinguishable (t = 0.27) from the 6.566 basis point effective spread for pilot stocks. Our other measure of aggressiveness, the fraction of submitted short sale orders that is marketable, shows similar differences between pilot and non-pilot stocks.

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 differences-in-difference test would conclude that the July 2007 repeal of the tick test leads to more aggressive shorting in the affected non-pilot stocks. Based on the results in Table 2 Panel A, 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).

While the difference-in-difference approach identifies an increase in shorting aggressiveness, there is also evidence of an indirect or spillover effect. After 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 earns 3.074 basis points of average effective spread before the repeal and only 1.965 basis points after repeal. 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.

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, we augment the difference-in-difference specification with market-level control variables, including expected volatility measured by 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 , variable mktres), and a market-wide price efficiency measure 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-minute quote midpoint returns, variable mktar). The results are given in Table 2 Panel B. The results are virtually the same with these controls in place.

The fact that shorting in unaffected control stocks becomes more aggressive is evidence of a spillover effect associated with uptick repeal. Table 3 reports our estimates of the direct and indirect treatment effects of uptick repeal. As noted above, these are simply linear combinations of the difference-in-difference coefficients, and we report these treatment effects for specifications with and without market-level controls.

To save space, we focus our discussion on the results with market-level controls in the right panel, and the results without market-level controls in the left panel are quite similar. Overall the numbers indicate that the indirect effect is qualitatively important. Traditional difference-in-difference approaches pick up only the direct effect, which we estimate to be 2.710 basis points. The indirect effect from treatment spillovers contributes an additional 0.812 basis points of shorting aggressiveness, as measured by effective spread, for a total treatment effect of 3.522 basis points. All three estimates use double clustered standard errors and are statistically significant. . That is, if we ignore the spillover effect, we substantially understate the increase in shorting aggressiveness associated with uptick repeal.

Why would short sellers become more aggressive in unaffected pilot stocks? A logical explanation is that the tick test repeal made it easier to implement a shorting strategy that demands liquidity and involves multiple stocks. For example, 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 marketable short-sale orders in 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 supply liquidity. As a result, the arbitrage strategy is subject to considerable execution risk in the presence of the uptick rule, so the index arbitrageur may not be able to implement this strategy as often 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 become free of the uptick rule.

5.1.1 Shorting aggressiveness in 2005 at the start of the Reg SHO pilot

If aggressive list-based short sellers require complete or near-complete uptick repeal to implement their trading strategies, the 2005 initiation of the Reg SHO pilot may yield very different results on shorting aggressiveness. At that time, tick tests are suspended for only one-third of Russell 3000 stocks, which is probably not enough to enable most such trading strategies, and we would expect to see little effect on shorting aggressiveness around the May 2005 start of the tick test pilot. To investigate this, we estimate the same difference-in-difference specification with the same control variables on a similarly constructed sample that extends from 20 trading days before to 20 trading days after the pilot begins.

The results are in Table 4. Panel A reports the simple differences-in-differences specification; Panel B includes market-level controls. Based on the specification without controls, 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 = -2.174 basis points for pilot stocks. As before, the standard differences-in-difference test would conclude that the Reg SHO pilot leads to more aggressive shorting in treated pilot stocks. Without controls, this test estimates 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), a reduction of about 80% from pre-pilot levels. The results are qualitatively similar when we include market controls in Panel B of Table 4.

Table 5 estimates the indirect effects associated with introduction of the pilot. In 2005, the direct effect for pilot firms, as mentioned above, is 3.517 basis points. However, there is a significant negative spillover effect on shorting aggressiveness. The indirect effect is estimated at an economically and statistically significant -1.579 basis points (t = -2.93) when there are no market control variables. The indirect effect becomes slightly larger when we have market control variables. Regardless of the specification, the indirect effect magnitude is close to 50% of the direct effect, and it is in the opposite direction. We find this pattern intriguing. Based on the direct effect, short-sellers are more aggressive for the pilot stocks. Yet, the indirect effect, coming from the non-pilot stocks, is significant and negative, which indicates that short-sellers are less aggressive towards the non-pilot stocks after the 2005 regulatory change. This pattern is consistent with a substitution effect. That is, once the pilot gets underway in 2005, short-sellers become more aggressive in pilot stocks and less aggressive in non-pilot stocks, possibly because they take advantage of the eased restrictions and substitute the non-pilot stocks with pilot stocks in their portfolios.

5.2. Effects of uptick repeal on stock comovements

Ultimately, it is not too surprising to discover that when a rule is repealed that limits some traders' aggressiveness, those traders become more aggressive. Perhaps more interesting is whether this aggressive trading behavior has any positive or negative effects on price discovery or liquidity. In this section, we explore the effect of uptick repeal on the behavior of stock prices. 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. To investigate this empirically, we approach the problem from two basic angles. First, we examine intraday share prices and returns to see if the affected stocks incorporate common factor information more quickly. Second, we examine intraday shorting activity, to see if the observed change in share price behavior is associated with a change in shorting behavior.

We take all sample firms and partition them into pilot and non-pilot stocks. For each group, we calculate equal-weighted intraday 15-minute returns based on quote midpoints. Based on the resulting time-series that extends from 20 trading days before uptick repeal to 20 trading days afterward, we estimate the following regression:

$$R_{nt} = \alpha + (\delta + \theta A_t)R_{pt} + u_t,$$

where R_{nt} is the intraday return on non-pilot stocks, R_{pt} is the contemporaneous 15-minute 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. However, if the uptick rule slows down information incorporation for non-pilot stocks relative to pilot stocks, we would expect a coefficient below one.

The results are in Table 6 Panel A. While the uptick rule is in effect, the estimated slope coefficient δ is only 0.947, consistent with the hypothesis that the uptick rule reduces price efficiency, and some information is only incorporated into prices with a lag.[8] After the uptick rule is repealed, the slope coefficient $(\delta + \theta)$ is equal to 1.024, which is statistically indistinguishable from unity. This implies that after the uptick rule is repealed, common-factor information is incorporated more quickly into non-pilot stocks. The coefficient θ itself, which directly measures the impact of the uptick repeal on the comovement, is positive and significant.

To see whether this information incorporation is associated with shorting, a similar exercise can be performed on the time-series of intraday shorting activity. For the non-pilot and pilot stocks, respectively, we cross-sectionally average intraday 15-minute shorting activity, measured as NYSE short sales divided by overall NYSE trading volume during that 15-minute interval. We then regress average non-pilot shorting activity on contemporaneous pilot stock shorting activity, allowing a different slope coefficient after uptick repeal. The results are in Table 6 Panel B, and they indicate the same pattern found in the returns. While the uptick rule is in effect, non-pilot and pilot shorting do not move one-for-one, with an estimated slope coefficient of only 0.785. This slope coefficient rises to 0.785 + 0.171 = 0.956 once the uptick rule is repealed. This indicates that without tick tests, pilot and non-pilot stocks experience the same time-series variation in shorting activity, indicating that short sellers are responsible for at least some of the increased timeliness of common factor information incorporation.

Another way to see this effect is via principal components. For all of our sample stocks (pilot and non-pilot together), we take the panel of 15-minute shorting activity and calculate the first three principal components. Table 6 Panel D has the results. When the uptick rule is in effect, the first three principal components explain 33.1% of the overall variance in shorting activity. After repeal, the first three principal components of shorting activity explain 48.15% of the overall variance, a much higher fraction. Most of the increase is driven by the first principal component, which increases from 22.5% to 39.4% in terms of overall variance explained. This is exactly what we would expect to see if uptick repeal makes it easier to execute a trading strategy that calls for simultaneous shorting in all stocks at once. In fact, when we limit the sample to stocks in the S&P 500 index, uptick repeal is associated with an even larger increase in

[8] When we add lagged pilot stock returns, we find as expected that the sum of the slope coefficients on contemporaneous and lagged pilot stock returns is always statistically indistinguishable from one.

variance explained. For example, the first principal component of shorting activity explains 27.7% of shorting variance pre-repeal, and this almost doubles to 50.5% post-repeal.

Interestingly, we do not get the same results when we look at the first three principal components of intraday 15-minute quote midpoint returns. As reported in Table 6 Panel C, for all of the stocks in our sample, the first principal component accounts for 10.3% of returns while the uptick rule is in place, but only 7.8% of returns after the uptick rule is repealed. Fortunately, there is a good explanation. The repeal of the uptick rule facilitates quicker incorporation of both systematic and firm-specific information. The results here simply indicate that quicker incorporation of firm-specific information is quantitatively more important.

5.3. Effects of uptick repeal on shorting activity

In the previous section, we established that the repeal of the uptick rule has both direct and indirect effects on shorting aggressiveness. In addition to changes in aggressiveness, short sellers might adjust their level of activity, and we expect to see more shorting once the uptick rule is repealed. There could be direct effects on affected stocks, and if list-based shorting becomes more common, there could also be indirect spillover effects, with more shorting even in ostensibly unaffected stocks.

We investigate this using the same suite of tests used to investigate shorting aggressiveness. Figure 1 and Table 2 provide details, and Table 3 summarizes the direct and indirect effects both with and without market-level controls. In Table 2, short sales average 30.3% of trading volume pre-repeal for stocks where the tick test is still in effect, and based on the specifications without controls, uptick repeal causes shorting to increase by a total of 8.7 percentage points relative to total trading volume. The standard difference-in-difference test would uncover only the direct effect, which we estimate at 6.3 percentage points, leaving an indirect or spillover effect of 2.3 percentage points. That is, the standard difference-in-difference approach would understate the effects on shorting volume.

However, when we estimate the model with market-level controls, the indirect effect is no longer present. The estimate of the direct effect remains 6.3 percentage points and is unaffected because the controls are market-wide and only vary over time. The estimated spillover effect is only 0.3 percentage points (t = 0.35).[9]

From Panel B of Table 2, 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 in fact 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 or spillover effects.

5.4. Effects of uptick repeal on liquidity measures

Diether, Lee, and Werner (2009) and Alexander and Peterson (2008) find that the pilot program suspending the uptick rule widens spreads slightly. As discussed in the previous section, depending on the current bid and ask prices relative to the last transaction price, short sellers subject to the uptick rule may be limited in their ability to demand liquidity. In this case, the uptick rule forces some short sellers to either not place a short-sale order or to supply liquidity instead via a limit order that complies with the uptick rule.

Here we briefly investigate market quality to see if the results from the end of the pilot and the complete repeal of the uptick rule in July 2007 match the results from the start of the pilot in 2005. For each NYSE common stock each day, we calculate trade-weighted proportional

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[9] Table 5 estimates the direct and indirect effects of the May 2005 start of the Reg SHO pilot on shorting activity. In that case, the indirect effect is not distinguishable from zero either with or without control variables included (IE = 0.0 percentage points, t = 0.07 without controls; IE = 0.7 percentage points, t = 1.66 with controls). The direct effect is also much smaller at 2.6 percentage points. Perhaps the effect is smaller simply because there is considerably less shorting in 2005; the results in Table 4 Panel A indicate that shorting is about 25% of overall selling activity right before the pilot took effect. Another possibility is that there are treatment interactions among affected stocks. This would be captured in the direct effect, and the bigger direct effect in 2007 might reflect the fact that there are about twice as many treatment stocks in the 2007 event compared to the 2005 pilot initiation.

round-trip effective spreads. The effective spread is defined as twice the distance between the trade price and the quote midpoint prevailing at the time of the trade. We use effective spreads because specialists and floor brokers are sometimes willing to trade at prices within the quoted bid and ask prices. The wider the effective spread, the less liquid is the stock. We consider pilot and non-pilot stocks separately. For each group of stocks, we calculate a cross-sectional average effective spread for each trading day.

While the uptick rule is in place, we would expect non-pilot stocks that are subject to the rule to have narrower effective spreads than pilot stocks, all else equal. Once the uptick rule is repealed, we would expect to see a widening of non-pilot stock effective spreads so as to match the pilot stock effective spreads. Spreads depend on various market conditions, notably volatility, but if market conditions do not change after the repeal, we would expect to see no change in effective spreads on the control group pilot stocks.

The results are graphed in Figure 1 and detailed in Table 2. Before the repeal of the uptick rule, non-pilot stocks and pilot stocks exhibit similar liquidity, based on the effective spread measures. Table 2 Panel A shows that in the pre-event period, the average effective spread for pilot stocks is 6.566 basis points vs. 6.464 basis points for non-pilot stocks, and the difference of 0.1 basis point is statistically insignificant (t = 0.27).

What happens once the uptick rule is repealed? Spreads widen significantly for both pilot and non-pilot stocks post-repeal. Table 3 summarizes the estimated direct and indirect effects. Repeal of the tick test causes a direct effect, with effective spreads widening by 0.59 basis points. Again, the indirect effect depends on whether market-level controls are included. When these control variables are excluded, the estimated indirect effect is even bigger than the direct effect, at 0.83 basis points (t = 3.09). However, when the market-wide controls are included, the indirect effect completely disappears (IE = -0.04, t = -0.35).

A similar analysis can be performed for price impacts. Price impacts are measured in basis points five minutes after each signed trade, and for each stock each day an average price impact is calculated by averaging equally across all transactions. The direct and indirect effects of uptick repeal are again summarized in Table 3. The direct effect, which can be estimated via the difference-in-difference approach, is an increase in price impact of 0.151 basis points (t = 2.78). As with the effective spread results, the indirect effects of uptick repeal on price impact depend on whether control variables are included. With control variables, there is no discernible indirect effect. Without control variables, however, the indirect effect is estimated at 0.225 basis points, which is statistically significant (t = 2.46) and is bigger than the estimated direct effect.

For completeness, we also report results for other measures of market quality. The absolute return persistence (*ar*) is computed for each stock each day as the absolute value of the AR(1) coefficient in a daily time-series regression of 30-minute quote midpoint returns. The intraday variance (*intrav*) is also calculated for each stock each day using 30-minute quote midpoint returns. Finally, we use 15-minute quote returns to calculate a price inefficiency measure (*hasb*) for each stock each day based on the vector autoregression approach of Hasbrouck (1993). The inefficiency measure is defined as the variance of the temporary component divided by the total price variance. There is little evidence that these quantities are affected by uptick repeal.

Regardless of the model chosen, the total effect on liquidity is clear: the repeal of the uptick rule somewhat worsens market quality, as measured by effective spreads and price impacts. This matches the findings of other researchers from the start of the pilot, 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 then. Repealing the uptick rule undoes this artificial liquidity supply.

However, the indirect effect is important, as it could indicate that there is more going on than this simple story. In this particular case, the market-wide effective spread variable is correlated with the LHS liquidity variables by construction, which gives a reason to prefer the results without those controls; but these reveal a large and significant indirect effect. As before, however, our main purpose is to highlight the possibility of these spillovers and discuss the methodological issues associated with their estimation.

5.5. Subgroup results

When we divide stocks into different market-cap groupings, there are some interesting cross-sectional differences in the impact of uptick repeal. As discussed earlier, the three market capitalization buckets (small, medium, and big) contain approximately equal numbers of these NYSE-listed stocks. Table 7 Panel A provides the regression results for shorting aggressiveness in each of these subgroups. Interestingly, the biggest proportional effects on shorting aggressiveness are in small stocks. Before uptick repeal, for example, short sellers earn an average of 9.374 basis points for small non-pilot stocks (the intercept in Regression I), and this quantity is reduced by almost half based on the 4.471 basis point estimated direct effect. In contrast, the effects of repeal on shorting aggressiveness are relatively smaller for large-cap stocks.

Given our discussions of index arbitrage and the fact that many institutional investors explicitly or implicitly track the S&P 500, another useful way to partition these stocks is based on their membership in the S&P 500 index. Table 7 Panel B has the regression results for these two subgroups. The results for S&P 500 components closely track the results for the large-cap group, and the results for stocks that are not part of the S&P 500 appear to be somewhere between the medium-cap and small-cap groups.

Total effects, direct effects, and indirect effects are summarized by market-cap tercile in Table 8 Panel A for all of our outcome variables. Table 8 Panel B does the same for S&P 500 members vs. other stocks. With or without the market level controls, shorting aggressiveness direct effects are large and strongly significant for all subgroups. The indirect effect on shorting aggressiveness is large and significant when the market controls are included, while when control variables are included, the indirect effects on shorting aggressiveness are more reliable in the mid-cap, large-cap and S&P500 subgroups. There is also statistical evidence of an indirect effect of uptick repeal on shorting activity for S&P500 stocks, with an estimated indirect effect of 1.7 percentage points ($t = 2.23$) when market-wide control variables are included.

Subgroups also reveal some interesting heterogeneity in price efficiency effects. When we consider the full sample and include control variables, Table 3 reveals that uptick repeal causes no discernible change in the Hasbrouck price inefficiency measure, which is the standard deviation of the temporary or noise component divided by the total standard deviation of 30-minute returns. Table 5 shows that this is also true for the 2005 start of the tick test pilot program. However, Table 8 Panel A shows that uptick repeal causes small-cap stock prices to be more efficient, as the estimated direct effect on the Hasbrouck price inefficiency measure is a decline of 0.011 ($t = 4.01$). Economically, this is a reasonably large change, given that small non-pilot stocks start with a Hasbrouck measure of 0.101.

5.6. Effects of uptick repeal on stock prices

As discussed earlier, shorting constraints can cause stocks to be overvalued relative to fundamentals if investors disagree on valuations. If these disagreement models are right and the uptick rule imposes a binding constraint on fundamentals-based short sellers, we should see a price effect upon repeal. The timing of the pricing effect differs across models. If agents have different valuations but are otherwise rational, the price should react when the market learns that uptick rule is to be repealed. On the other hand, if optimists and pessimists rely only on their own valuations, 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 impede short sellers, repeal of the uptick rule should have no effect on share prices. Figure 2 shows 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 constraints models are correct, non-pilot stocks should fall on the news of the uptick rule's 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 constraints 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.

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 has an impact on 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.

6. Conclusions

In this paper, we address treatment spillovers in 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 elimination of the uptick rule that had limited short sales on the New York Stock Exchange. Some stocks were already exempt from the uptick rule due to an SEC pilot program begun in 2005, and we use these pilot stocks as a control group ostensibly 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 repeal takes effect, short-sale orders on average become more aggressive in both affected and unaffected stocks. These indirect effects indicate the presence of treatment spillovers, and we find evidence on the source of the spillovers: total repeal of the uptick rule made it much easier to aggressively short portfolios of stocks, and this aggressive shorting shows up in ostensibly unaffected control stocks. This indirect effect was not present at the 2005 start of the regulatory pilot, and as a result, studies of the start of the pilot underestimate the overall impact of uptick repeal.

Fortunately, these indirect effects do not sharply degrade market quality in this particular case. Overall, uptick repeal causes market liquidity to worsen slightly, and prices incorporate common factor information more quickly. However, the identification of treatment spillovers in this case 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 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 is currently considering a pilot program that will change 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. In order to be eligible for the pilot, the SEC has proposed that firms must have a market cap of at most \$5 billion, a share price of at least \$2, and average daily trading volume of at most one million shares. Approximately 1,200 stocks are likely to be eligible for the pilot and would be divided into four equal-sized groups: one control group and three test groups. One test group would be quoted in minimum increments of \$0.05. A second test group would also place restrictions on trade prices and require 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. Unlike the Reg SHO pilot, portfolio trading effects are probably 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 should take these possibilities into account in advance of the pilot; otherwise, it may prove difficult to draw conclusions from the resulting data.

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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 20 trading days before the uptick rule repeal on July 6, 2007. The daily share volume is the NYSE 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 main daily measure of shorting activity, *valln*, is NYSE short sale volume over NYSE trading volume. The relative effective spread, *res*, is the full effective spread scaled by VWAP. The relative price impact, *rpi*, is the 5-minute price impact scaled by VWAP. Absolute return persistence, *ar*, is computed as the absolute value of the AR(1) coefficient for a day of 30-minute returns. The intraday variance (*intrav*) is computed with 30-minute returns. The Hasbrouck price inefficiency measure (*hasb*) is the volatility of noise over volatility of price. For each measure, we report statistics for all firms, size groups (as of July 5, 2007), and S&P 500 firms. We also separate firms by whether they are Reg SHO pilot or non-pilot firms.

	All firms		Small		Medium		Big		S&P 500 firms		Non S&P 500 firms	
	non pilot	pilot	non pilot	pilot	non pilot	pilot	non pilot	pilot	non pilot	pilot	non pilot	pilot
Number of firms	728	360	244	111	235	128	246	118	211	106	517	254
Market cap ($billions)	3.189	2.928	0.798	0.849	3.226	2.897	14.277	16.453	13.894	16.052	1.750	1.895
Book-to-market	0.423	0.424	0.594	0.556	0.387	0.414	0.333	0.317	0.338	0.346	0.451	0.472
Daily share volume (millions)	0.422	0.399	0.160	0.155	0.374	0.362	1.264	1.202	1.261	1.245	0.265	0.267
shortres, relative effective spread for short sale orders only (bps)	-4.667	-2.271	-7.549	-3.408	-4.732	-2.310	-3.495	-1.901	-3.576	-1.920	-5.367	-2.585
fmkt, fraction shorts marketable	0.321	0.339	0.340	0.330	0.307	0.333	0.316	0.351	0.318	0.355	0.323	0.331
valln, shorts / volume	0.292	0.374	0.329	0.413	0.293	0.384	0.262	0.336	0.260	0.341	0.307	0.390
res, relative effective spread (bps)	4.608	4.844	8.502	8.509	4.412	4.822	2.912	3.073	2.895	3.064	5.493	5.880
rpi, relative price impact (bps)	0.773	0.781	1.647	1.793	0.775	0.836	0.418	0.412	0.435	0.412	1.041	1.088
ar, absolute return persistence	0.215	0.215	0.208	0.210	0.213	0.206	0.224	0.232	0.221	0.231	0.212	0.210
intrav, intraday variance (bps)	0.089	0.090	0.137	0.133	0.077	0.087	0.071	0.068	0.067	0.068	0.103	0.102
hasb, price inefficiency measure	0.061	0.058	0.101	0.093	0.061	0.058	0.042	0.042	0.043	0.042	0.073	0.070

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

In this table, we report coefficients for regressions of the form: $y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}$. Event dummy A_t takes a value of 1 for dates after July 6, 2007, and 0 otherwise. The treatment dummy T_i takes a value of 1 for firms in the pilot program, and 0 otherwise. Panel A reports results without controls. Panel B includes the following market-level controls X_{t-1}: VIX, average firm level relative effective spread, and average firm level coefficient *ar*, all measured the previous day. Each regression is estimated for 8 different dependent variables; see Table 1 for variable definitions. The regressions are estimated over [-20, +20] around July 6, 2007. T-stats are based on double clustered standard errors.

Panel A. Without control variables

dep. Var	shortres		fmkt		valln		res		rpi		ar		intrav		hasb	
Reg	I		II		III		IV		V		VI		VII		VIII	
	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)
Intercept	-5.923	-18.55	0.335	55.78	0.303	73.35	6.464	26.87	1.190	19.80	0.246	21.42	0.142	18.38	0.092	20.96
β_1	3.818	9.67	0.044	4.35	0.087	8.75	1.422	5.23	0.377	4.06	0.007	0.57	0.141	2.76	-0.011	-2.14
β_2	2.849	9.81	0.010	2.74	0.081	13.43	0.102	0.27	0.043	0.51	0.000	0.13	-0.005	-0.52	-0.005	-1.14
β_3	-2.710	-13.25	-0.004	-1.03	-0.063	-12.38	-0.591	-4.21	-0.151	-2.78	-0.002	-0.43	-0.032	-2.17	0.002	1.07
R-square	0.04		0.04		0.09		0.01		0.00		0.00		0.01		0.00	

Panel B. With market level control variables

dep. Var	shortres		fmkt		valln		res		rpi		ar		intrav		hasb	
Reg	I		II		III		IV		V		VI		VII		VIII	
	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)
Intercept	-8.153	-6.45	0.264	7.54	0.183	5.21	1.509	1.63	-0.241	-0.82	0.199	3.97	-0.766	-2.89	0.143	9.88
β_1	3.522	8.85	0.030	2.90	0.067	6.60	0.552	4.93	0.136	1.89	0.004	0.28	0.000	0.02	0.000	-0.11
β_2	2.850	9.81	0.010	2.74	0.081	13.42	0.102	0.27	0.043	0.51	0.000	0.13	-0.005	-0.53	-0.005	-1.13
β_3	-2.710	-13.24	-0.004	-1.04	-0.063	-12.36	-0.592	-4.24	-0.152	-2.78	-0.002	-0.43	-0.032	-2.15	0.002	1.06
vix	-0.323	-3.74	-0.007	-3.01	-0.002	-0.73	0.065	0.80	0.054	2.48	-0.001	-0.15	0.017	0.80	-0.002	-1.17
mktres	1.027	3.77	0.029	4.27	0.022	3.52	0.654	3.13	0.094	1.27	0.004	0.36	0.086	2.30	-0.006	-1.45
mktar	1.693	0.83	-0.060	-1.01	0.000	-0.01	-1.275	-0.68	-0.037	-0.07	0.126	1.06	0.315	0.57	0.049	1.79
R-square	0.04		0.06		0.10		0.02		0.01		0.00		0.04		0.01	

Table 3. Total, direct and indirect effects of the July 2007 uptick repeal

Based on regressions of the form: $y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}$.

Variable definitions can be found in previous table legends. 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 double clustered standard errors.

		regression with only dummy variables			regression with dummy variables and market controls		
		total	direct	indirect	total	direct	indirect
shortres	coef	3.818	2.710	1.108	3.522	2.710	0.812
	t(DC)	9.67	13.25	3.64	8.85	13.24	2.43
fmkt	coef	0.044	0.004	0.040	0.030	0.004	0.026
	t(DC)	4.35	1.03	4.23	2.90	1.04	2.67
valln	coef	0.087	0.063	0.023	0.067	0.063	0.003
	t(DC)	8.75	12.38	2.66	6.60	12.36	0.35
res	coef	1.42	0.59	0.83	0.55	0.59	-0.04
	t(DC)	5.23	-4.21	3.09	4.93	-4.24	-0.35
rpi	coef	0.377	0.151	0.225	0.136	0.152	-0.016
	t(DC)	4.06	2.78	2.46	1.89	2.78	-0.25
ar	coef	0.007	0.002	0.006	0.004	0.002	0.002
	t(DC)	0.57	0.43	0.43	0.28	0.43	0.17
intrav	coef	0.141	0.032	0.109	0.000	0.032	-0.032
	t(DC)	2.76	2.17	2.56	0.02	2.15	-1.53
hasb	coef	-0.011	-0.002	-0.009	0.000	-0.002	0.001
	t(DC)	-2.14	-1.07	-1.85	-0.11	-1.06	0.33

Table 4. Diff-in-diff regressions around May 2005 pilot start

In this table, we report coefficients for regressions of the form: $y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}$. Event dummy A_t takes a value of 1 for dates after May 2, 2005, and 0 otherwise. The treatment dummy T_i takes a value of 1 for firms in the pilot program, and 0 otherwise. 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, and average firm level coefficient *ar*, all measured the previous day. Each regression is estimated for 8 different dependent variables; see Table 1 for variable definitions. The regressions are estimated over [-20, +20] around May 2, 2005. T-stats are based on double clustered standard errors.

Panel A. Without control variables

dep. Var	shortres		fmkt		valln		res		rpi		ar		intrav		hasb	
Reg	I		II		III		IV		V		VI		VII		VIII	
	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)
Intercept	-4.287	-8.92	0.452	56.53	0.251	84.93	10.251	26.16	2.900	26.88	0.254	34.94	0.251	12.26	0.092	24.52
β_1	-1.579	-2.93	-0.028	-2.52	0.000	-0.07	-0.287	-1.62	-0.163	-1.69	-0.007	-0.79	-0.065	-2.90	0.007	1.68
β_2	0.174	0.46	0.001	0.18	0.007	1.66	-0.192	-0.30	-0.065	-0.49	0.000	0.08	-0.010	-0.69	-0.008	-1.91
β_3	3.517	8.74	0.018	3.73	0.026	6.19	1.053	4.48	0.104	2.13	0.002	0.86	0.015	2.30	0.000	0.21
R-square	0.01		0.01		0.01		0.00		0.00		0.00		0.00		0.00	

Panel B. With market level control variables

dep. Var	shortres		fmkt		valln		res		rpi		ar		intrav		hasb	
Reg	I		II		III		IV		V		VI		VII		VIII	
	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)
Intercept	-1.583	-0.40	0.403	4.46	0.203	5.01	3.949	4.20	-0.461	-0.78	0.248	3.25	-0.536	-4.34	0.171	5.43
β_1	-1.832	-3.58	-0.034	-2.84	-0.001	-0.28	-0.290	-2.07	-0.166	-2.06	-0.013	-1.30	-0.073	-3.96	0.008	2.19
β_2	0.175	0.46	0.001	0.18	0.007	1.66	-0.193	-0.31	-0.066	-0.50	0.000	0.08	-0.010	-0.70	-0.008	-1.90
β_3	3.516	8.74	0.018	3.72	0.026	6.19	1.054	4.49	0.105	2.14	0.002	0.85	0.015	2.30	0.000	0.20
vix	-0.577	-2.64	-0.009	-1.83	-0.001	-0.38	0.176	4.03	0.092	2.70	-0.008	-1.90	0.013	1.79	-0.002	-1.04
mktres	0.501	1.07	0.016	1.45	0.004	1.03	0.289	2.81	0.158	2.39	0.013	1.65	0.055	3.73	-0.006	-1.75
mktar	2.200	0.33	0.069	0.44	0.052	0.90	3.396	2.42	1.771	1.79	-0.068	-0.47	0.215	0.76	0.031	0.64
R-square	0.01		0.01		0.01		0.00		0.00		0.00		0.01		0.00	

Table 5. Total, direct and indirect effects of the May 2005 pilot start

Based on regressions of the form: $y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}$.

Variable definitions can be found in Table 1 and Table 4 legends. The indirect effect is measured by β_1, the direct effect is measured by β_3, and the total effect is measured by $\beta_1 + \beta_3$. T-stats are computed using double clustered standard errors.

		regression with only dummy variables			regression with dummy variables and market controls		
		total	direct	indirect	total	direct	indirect
shortres	coef	1.939	3.517	-1.579	1.684	3.516	-1.832
	t(DC)	3.27	8.74	-2.93	2.79	8.74	-3.58
fmkt	coef	-0.010	0.018	-0.028	-0.016	0.018	-0.034
	t(DC)	-0.97	3.73	-2.52	-1.43	3.72	-2.84
valln	coef	0.026	0.026	0.000	0.025	0.026	-0.001
	t(DC)	4.64	6.19	-0.07	4.40	6.19	-0.28
res	coef	0.766	1.053	-0.287	0.764	1.054	-0.290
	t(DC)	4.71	4.48	-1.62	6.02	4.49	-2.07
rpi	coef	-0.059	0.104	-0.163	-0.061	0.105	-0.166
	t(DC)	-0.57	2.13	-1.69	-0.68	2.14	-2.06
ar	coef	-0.005	0.002	-0.007	-0.011	0.002	-0.013
	t(DC)	-0.51	0.86	-0.79	-1.05	0.85	-1.30
intrav	coef	-0.049	0.015	-0.065	-0.058	0.015	-0.073
	t(DC)	-2.25	2.30	-2.90	-3.08	2.30	-3.96
hasb	coef	0.007	0.000	0.007	0.008	0.000	-0.008
	t(DC)	1.79	0.21	1.68	2.13	0.20	-1.90

Table 6. Co-movements among shorts and returns around the July 2007 uptick repeal

This table reports the co-movement of returns and shorting activity (*valln*) before and after the uptick rule repeal. In Panel A (B), we regress average non-pilot firms' returns (shorts) on the average pilot firms' returns (shorts), interacting with the event dummy A_t which takes the value of 1 after July 6, 2007, and 0 otherwise. In Panel C and Panel D, we conduct principal component analysis on the panel of returns and shorting activity, respectively. We report the percentage of variance explained by the first 3 PCs. All numbers for before uptick repeal are estimated over 20 days before July 6, 2007, and all numbers for after repeal are estimated over 20 days after July 6, 2007.

Panel A. Co-movement of 15-minute returns

REG	all stocks		small		medium		large	
	coef	t(OLS)	coef	t(OLS)	coef	t(OLS)	coef	t(OLS)
Intercept	0.000	-1.41	0.000	-2.56	0.000	-1.65	0.000	0.73
Pilot return	0.947	112.20	0.879	63.07	0.939	110.67	0.990	111.61
pilotret*A_t	0.077	7.06	0.134	7.48	0.042	3.89	0.028	2.43

Panel B. Co-movement of 15-minute shorting activity

REG	all stocks		small		medium		large	
	coef	t(OLS)	coef	t(OLS)	coef	t(OLS)	coef	t(OLS)
Intercept	0.000	0.74	0.003	15.37	0.000	0.62	-0.001	-6.26
Pilot short	0.785	147.24	0.646	83.56	0.784	128.67	0.819	124.68
pilotshort*A_t	0.171	33.22	0.222	22.12	0.133	23.95	0.167	29.06

Panel C. Principal component analysis for intra-day and daily returns

	before 1 PC	after 1 PC	diff	before 2 PC	after 2 PC	diff	before 3 PC	after 3 PC	diff
Intraday returns									
Full sample	10.3%	7.8%	-2.5%	18.7%	14.7%	-4.1%	26.3%	21.2%	-5.1%
S&P 500 firms	11.0%	8.1%	-2.9%	19.6%	15.0%	-4.5%	26.7%	21.8%	-5.0%
Daily returns									
Full sample	12.0%	9.9%	-2.1%	19.3%	17.3%	-2.0%	26.1%	24.5%	-1.7%
S&P 500 firms	14.5%	10.5%	-4.1%	22.4%	19.6%	-2.8%	29.9%	27.9%	-2.0%

Panel D. Principal component analysis for intra-day and daily shorting activity

	before 1 PC	after 1 PC	diff	before 2 PC	after 2 PC	diff	before 3 PC	after 3 PC	diff
Intraday shorting									
Full sample	22.5%	39.4%	16.9%	28.0%	44.0%	16.0%	33.1%	48.1%	15.1%
S&P 500 firms	27.7%	50.5%	22.8%	33.3%	54.6%	21.3%	38.3%	58.3%	20.0%
Daily shorting									
Full sample	44.6%	48.8%	4.2%	54.0%	56.9%	2.9%	59.7%	62.8%	3.0%
S&P 500 firms	49.7%	49.5%	-0.2%	56.3%	58.2%	1.9%	62.3%	63.1%	0.9%

Table 7. Diff-in-diff regressions around July 2007 uptick repeal, subgroup analysis

In this table, we report coefficients for regressions of the form: $y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}$. Event dummy A_t takes a value of 1 for dates after July 6, 2007, and 0 otherwise. The treatment dummy T_i takes a value of 1 for firms in the pilot program, and 0 otherwise. Regression I reports results without control variables X_{t-1}; Regression II includes them. Panel A divides the NYSE-listed sample into three market-cap terciles; Panel B partitions based on membership in the S&P 500. The dependent variable is shorting relative effective spread (shortres); see Table 1 for variable definitions. The regressions are estimated over [-20, +20] around July 6, 2007. T-stats are based on double clustered standard errors.

Panel A. Shorting aggressiveness measured as relative effective spread of short orders, for three size groups

Size group	small		medium		big		small		medium		big	
Regression	I		II		III		IV		V		VI	
	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)
Intercept	-9.374	-13.32	-4.719	-23.13	-3.592	-22.34	-12.618	-4.98	-7.188	-8.63	-4.613	-7.54
β_1	6.015	7.36	2.959	11.56	2.438	12.82	5.591	6.47	2.620	10.52	2.311	13.51
β_2	4.479	5.98	2.173	8.34	1.662	13.05	4.479	5.98	2.173	8.33	1.662	13.04
β_3	-4.471	-8.14	-1.944	-12.71	-1.619	-13.00	-4.473	-8.12	-1.945	-12.67	-1.619	-12.98
vix							-0.592	-3.29	-0.218	-3.42	-0.159	-4.99
mktres							1.777	3.20	0.823	4.04	0.487	5.02
mktar							2.566	0.61	1.640	1.11	1.024	1.08
R-square	0.04		0.09		0.15		0.04		0.09		0.15	

Panel B. Shorting aggressiveness measured as relative effective spread of short orders, for S&P 500 members and non-members

S&P 500	member		non-member		member		non-member	
Reg	I		II		III		IV	
	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)	coef.	t(DC)
Intercept	-3.765	-20.55	-6.798	-16.88	-4.916	-7.40	-9.463	-6.06
β_1	2.575	12.51	4.320	8.82	2.430	12.96	3.962	7.86
β_2	1.789	10.21	3.263	8.35	1.789	10.20	3.264	8.35
β_3	-1.666	-16.11	-3.132	-11.07	-1.666	-16.04	-3.133	-11.06
vix					-0.189	-5.37	-0.378	-3.37
mktres					0.573	5.39	1.214	3.40
mktar					1.098	1.29	1.918	0.74
R-square	0.14		0.04		0.149		0.040	

Table 8. Total, direct and indirect effects of the July 2007 uptick repeal, subgroup analysis

Based on regressions of the form: $y_{it} = \beta_0 + \beta_1 A_t + \beta_2 T_i + \beta_3 A_t T_i + \gamma X_{t-1} + u_{it}$. Variable definitions can be found in legends for Tables 1 and 2. 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 double clustered standard errors.

Panel A. Size groups

		Regression using dummy variables only						Regression using dummy variables and market controls					
		total		direct		indirect		total		direct		indirect	
		coef	t(DC)	coef	t(DC)	coef	t(DC)	coef	t(DC)	coef	t(DC)	coef	t(DC)
shortres	small	6.015	7.36	4.471	8.14	1.544	2.43	5.591	6.47	4.473	8.12	1.118	1.50
	medium	2.959	11.56	1.944	12.71	1.015	4.07	2.620	10.52	1.945	12.67	0.676	3.00
	big	2.438	12.82	1.619	13.00	0.819	4.96	2.311	13.51	1.619	12.98	0.692	4.29
fmkt	small	0.016	1.03	-0.020	-2.87	0.036	2.74	0.006	0.36	-0.020	-2.85	0.026	1.76
	medium	0.031	3.48	0.006	1.11	0.025	2.70	0.016	1.85	0.006	1.11	0.010	1.11
	big	0.083	9.37	0.025	4.59	0.059	6.85	0.068	7.76	0.025	4.57	0.043	5.13
valln	small	0.092	6.49	0.074	9.02	0.018	1.29	0.073	4.51	0.074	9.00	-0.001	-0.08
	medium	0.077	8.02	0.056	7.17	0.021	2.20	0.057	6.19	0.056	7.15	0.001	0.09
	big	0.091	10.00	0.062	9.19	0.029	3.62	0.070	8.36	0.062	9.16	0.008	1.04
res	small	2.421	5.90	0.829	2.42	1.592	3.67	1.053	10.31	0.832	2.44	0.221	1.12
	medium	1.091	4.42	0.606	5.45	0.485	2.16	0.354	2.78	0.608	5.56	-0.254	-2.70
	big	0.670	4.24	0.214	47.49	0.455	2.67	0.164	2.21	0.215	13.87	-0.051	-0.65
rpi	small	0.848	4.29	0.303	2.15	0.545	2.51	0.324	2.02	0.304	2.14	0.020	0.13
	medium	0.197	3.15	0.098	2.58	0.100	1.79	0.057	1.09	0.098	2.57	-0.041	-0.96
	big	0.056	1.93	0.017	0.66	0.039	1.37	-0.003	-0.14	0.017	0.66	-0.020	-0.73
ar	small	0.015	1.29	-0.002	-0.34	0.017	1.33	0.014	1.41	-0.002	-0.34	0.016	1.37
	medium	0.009	0.67	-0.003	-0.49	0.012	0.88	0.006	0.40	-0.003	-0.49	0.009	0.65
	big	-0.001	-0.04	0.009	1.49	-0.010	-0.49	-0.007	-0.31	0.009	1.49	-0.016	-0.69
intrav	small	0.195	2.50	0.076	1.79	0.118	2.24	-0.006	-0.19	0.077	1.78	-0.082	-2.05
	medium	0.130	2.86	0.022	1.75	0.108	2.45	0.004	0.21	0.022	1.71	-0.018	-0.96
	big	0.096	2.91	-0.004	-0.43	0.099	2.80	0.000	-0.02	-0.004	-0.40	0.003	0.23
hasb	small	-0.018	-2.27	-0.011	-4.01	-0.006	-0.79	0.000	-0.04	-0.011	-3.92	0.011	1.51
	medium	-0.008	-1.62	-0.001	-0.35	-0.007	-1.67	0.002	0.48	-0.001	-0.36	0.003	0.72
	big	-0.006	-1.77	0.000	0.08	-0.006	-2.42	-0.002	-0.63	0.000	0.07	-0.002	-0.86

Panel B. Effects by S&P500 membership

		regression using dummy variables only						regression using dummy variables and market controls					
		total		direct		indirect		total		direct		indirect	
		coef	t(DC)	coef	t(DC)	coef	t(DC)	coef	t(DC)	coef	t(DC)	coef	t(DC)
shortres	member	2.575	12.51	1.666	16.11	0.909	5.26	2.430	12.96	1.666	16.04	0.764	4.89
	no	4.320	8.82	3.132	11.07	1.188	3.10	3.962	7.86	3.133	11.06	0.829	1.92
fmkt	member	0.086	9.30	0.025	4.30	0.060	7.08	0.071	7.68	0.025	4.28	0.045	5.33
	no	0.026	2.36	-0.004	-1.00	0.031	2.94	0.014	1.16	-0.004	-0.98	0.018	1.61
valln	member	0.096	10.76	0.059	8.43	0.037	4.98	0.076	9.26	0.059	8.41	0.017	2.23
	no	0.083	7.55	0.065	11.05	0.017	1.65	0.063	5.35	0.065	11.02	-0.002	-0.21
res	member	0.631	4.12	0.226	2.72	0.405	2.91	0.150	2.22	0.226	2.76	-0.076	-2.87
	no	1.750	5.45	0.724	3.91	1.026	3.17	0.716	5.68	0.726	3.94	-0.010	-0.07
rpi	member	0.033	1.26	0.005	0.21	0.028	1.11	-0.017	-0.84	0.005	0.21	-0.022	-1.05
	no	0.518	4.23	0.206	2.83	0.312	2.54	0.197	2.04	0.206	2.83	-0.009	-0.10
ar	member	0.002	0.11	0.012	2.01	-0.009	-0.48	-0.005	-0.19	0.012	2.01	-0.016	-0.70
	no	0.010	0.83	-0.003	-0.65	0.012	1.01	0.008	0.66	-0.003	-0.64	0.010	0.88
intrav	member	0.097	2.95	-0.003	-0.24	0.100	2.69	0.003	0.23	-0.003	-0.23	0.006	0.43
	no	0.159	2.69	0.046	2.06	0.113	2.46	-0.001	-0.05	0.046	2.04	-0.047	-1.84
hasb	member	-0.007	-1.93	-0.001	-0.83	-0.005	-2.06	-0.002	-0.74	-0.001	-0.83	-0.001	-0.41
	no	-0.013	-2.09	-0.002	-0.95	-0.010	-1.74	0.000	0.07	-0.002	-0.94	0.003	0.49

Figure 1. Key variables before and after tick test repeal.

We present the time-series of 4 key variables over [-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, the faction of market order, is the fraction of market order and market limit order out of all shorts. Shorting activity, valln, is measured each day as the fraction of NYSE/Arca daily share volume. Variable res is the relative effective spread, the full effective spread scaled by VWAP. Cross-sectional means are reported for both pilot and non-pilot stocks.

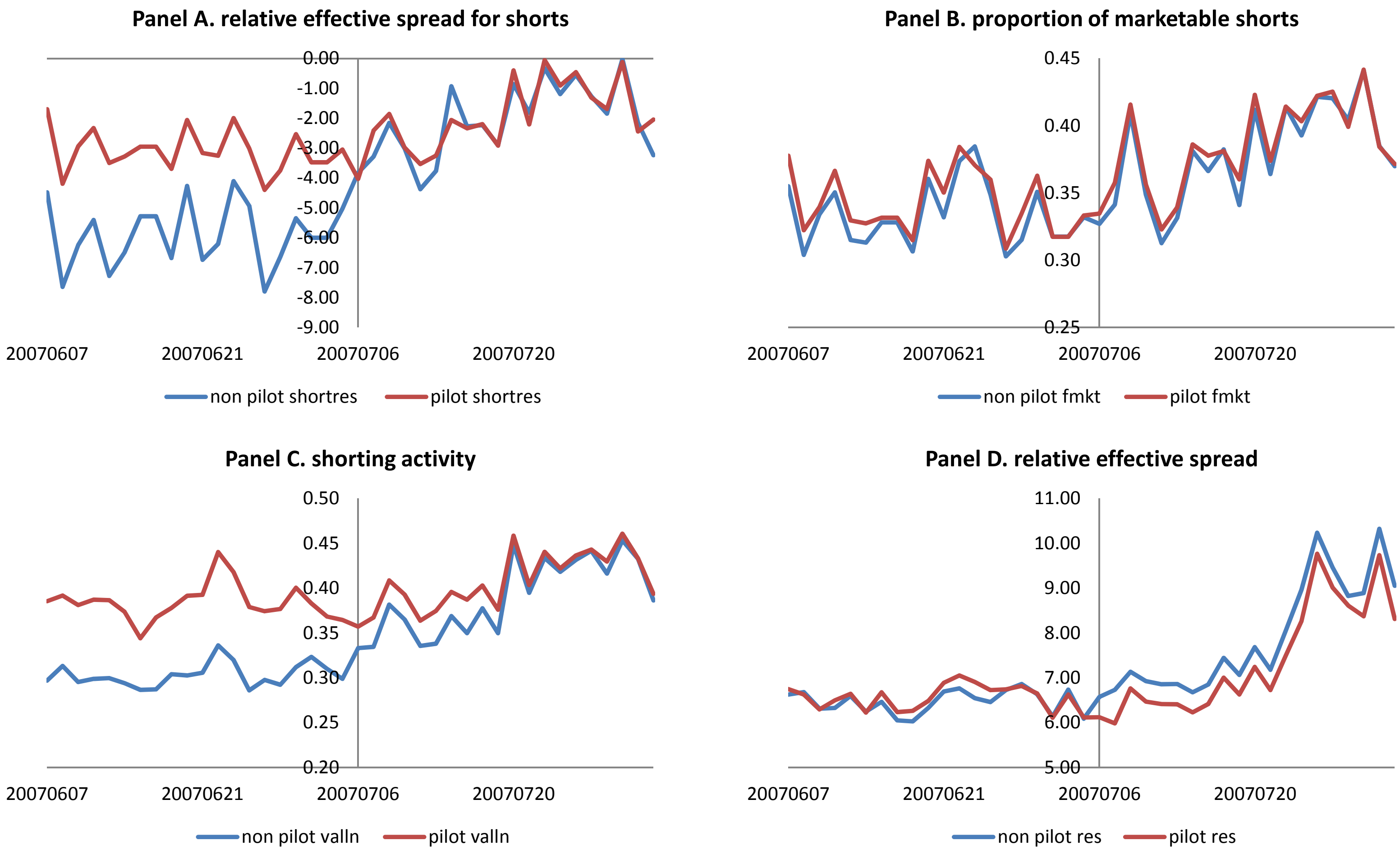

Figure 2. Return response after tick test repeal.
This figure shows cumulative returns on the overall stock market (red line) over the uptick repeal period as well as the differential return on pilot vs. non-pilot stocks (blue line). We also report the confidence bounds (orange lines) for the return differentials, 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.