The Real Effects of Stock Market Prices

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Abstract

We use a natural experiment to test whether an exogenous change in equity value drives real decisions within the firm. Examining the effects of an SEC rule that relaxed short-selling constraints on a random sample of US stocks, we find evidence that an exogenous increase in short-selling activity negatively affects stock market prices. More importantly, we find that firms that are more likely to be financially constrained react to this valuation correction by reducing equity issues and investment. Our results suggest that the distortion in stock prices induced by short sale constraints has a direct effect on both corporate financing and investment decisions.

We identify a causal effect of equity mispricing on corporate behavior using a natural experiment that affected stock prices for a randomly selected set of stocks. Our identification strategy relies on a random event and a well-defined experimental sample period which provide an ideal setting to test whether stock price distortions affect real corporate decisions. Specifically, we examine a regulatory change (regulation SHO) that caused an exogenous increase in short selling activity, but only for an *ex-ante* randomly selected treatment group. Our main finding is that the stock prices of firms in the treatment group fell, and that this exogenous change in prices leads to an economically meaningful change in corporate policy.

Theory predicts that if capital market frictions (e.g., short-selling constraints, noise trader risk, and transaction costs) are large enough, then prices can drift away from fundamentals for long enough to have real effects on corporate decisions. Indeed, several papers argue that stock market mispricing has a significant effect on corporate investment decisions. For example, Chirinko and Schaller (2001) and Goyal and Yamada (2004) find that investment in Japan is sensitive to bubbles in equity prices. Further, Campello and Graham (2007) find similar results around the mispricing of technology firms during the internet boom of the late 1990s. Apart from periods of speculative bubbles, Polk and Sapienza (2009) find a relationship between investment and mispricing, Edmans, Goldstein and Jiang (2011) show a shock to stock prices increases takeover probability and Campello, Ribas, and Wang (2010) find that trading reforms have an effect on corporate decisions. Furthermore, Gilchrist, Himmelberg, and Huberman (2005) develop a theoretical framework for how short sales constraints and dispersion of beliefs can drive

distortions in share issuance and real investment. The recurring theme in all these studies is that random variation in equity prices appears to distort investment flows.

On the other hand, there are also studies that point to a limited role of equity prices on real investment decisions. For example, Morck, Shleifer, and Vishny (1990) show that the stock market is mostly a "sideshow" for corporate investment decisions after controlling for cash flows. Pastor and Veronesi (2006) argue that the rise and fall of Nasdaq prices and investment by technology companies in the late 1990s simply reflects changes in expected investment opportunities that turned out to be wrong. In a more general setting, Bakke and Whited (2010) find no evidence that mispricing affects corporate investment decisions when focusing on the component of stock prices directly affecting investment.

A big challenge in this literature is finding a reasonable measure of mispricing. Unfortunately, many of the most popular proxies for mispricing or stock bubbles could also serve as proxies for growth opportunities (e.g., market-to-book ratio, discretionary accruals, periods of rising stock prices). As a result, there is no clear consensus in the literature on how to measure mispricing or whether it affects real decisions.

We measure mispricing with an identification strategy that relies on a natural experiment. In 2005, the SEC implemented Regulation SHO which removed restrictions on short sales. Previously, short sales could not be placed when stock prices were declining, a regulation commonly referred to as the "uptick rule".[1] This constraint was binding. For example, Alexander and Peterson (1999) find that the uptick rule not only

[1]Rule 10a-1imposed the uptick rule on the NYSE in 1938, and Rule 3350 imposed the bid price test on NASDAQ in 1994. On the NYSE, short sales can only be made on plus ticks or zero plus ticks based on the last sale. The bid test applicable to Nasdaq securities prohibits sales below the bid if the last bid was a down bid.

impacts the short sale execution quality in declining markets, but also impacts short sales prices and execution in advancing markets. However, the SEC only lifted this restriction for a randomly selected sample of one third of the Russell 3000 stocks (the pilot group). Two years later the SEC removed the restriction for all stocks.

This regulatory change was not welcomed by the exchanges or listed firms. In public comments, NYSE officials, specialists, and member firms all expressed support for short sale restrictions and opposed any change that affected some listed stocks but not others. There appears to have been a general fear that bear raids on stocks in the pilot group could have adverse effects on investment and security issuance. [2]

Such fears have theoretical support. In traditional models of trading with short-selling constraints (e.g., Miller (1977), Diamond and Verrechia (1987), Arnold, et al (2005)), negative information may not be fully reflected in equilibrium stock prices, which could lead to stock overvaluation (Jones and Lamont (2002)). From a theoretical perspective, there are several channels through which short-selling constraints can affect firms' real activities. On the one hand, short-selling constraints can lead to rational overvaluation (e.g., stock bubbles) that managers exploit to issue overvalued equity and invest in real assets (Gilchrist, Himmelberg, and Huberman (2005)). On the other hand, short-selling constraints can lead to overinvestment as managers receive overoptimistic signals from the stock market about their firms' prospects (Chen, Goldstein, and Jiang

[2] For example, see the open letter to the SEC by Darla C. Stuckey, Corporate Secretary, New York Stock Exchange March 1, 2004. Inc. A similar letter to the SEC by David Humphreville, President, The Specialist Association dated February 12, ,2004, states that firms "…fear, legitimately, that removal of Rule 10a-1's price constraints on short selling of their companies' stocks for two years will undermine proper pricing, tend to discourage new decisions to invest, and weaken the resolve of current holders of their companies' stocks to refrain from selling them in times of market stress."

(2007)). In general, existing theoretical models predict that a removal of short-selling constraints should lead to a decline in corporate investment.

A number of recent studies have looked at the effect of repealing the uptick rule. Diether, Lee, and Werner (2009), Boehmer, Jones, and Zhang (2008), Alexander and Peterson (2008), and the SEC's Office of Economic Analysis (OEA, 2007) all find that short sales increase after the repeal but that there is little change in volatility and virtually no effect on prices. How then, could there be any real effect on corporate decisions, when the relaxation of the short sale restriction appears to have had little effect on prices?

We reexamine some of these findings using a wider event window and find different results. We find strong evidence of abnormal stock returns over different time horizons, but the biggest effect on prices happens up to 30 days before the SEC announcement date (but after the SEC board approval date). The negative effect on prices is especially large for small firms. Moreover, consistent with the idea that short-selling constraints sideline pessimistic investors from the market, we find that small firms in the pilot group exhibit more negative returns on down-market days, and become more sensitive to negative earnings news. Taken together, our results show firms in the pilot group experienced a negative price reaction and continued to come under heavy selling pressure throughout the implementation period – even though they were randomly chosen to participate in the Reg SHO's pilot program. Further, it suggests that information about the list of firms participating in the pilot program leaked into the market before the official announcement date.

We then examine whether the effect of the pilot study on prices translates into the real economic activities of those firms. Following the logic in Gilchrist, Himmelberg,

and Huberman (2005), we argue that if short-selling constraints generate stock overvaluation, then equity-dependent pilot firms should reduce investment as their cost of equity capital increases after the adoption of Reg SHO. Because recent evidence suggests that the most relevant variable to measure financial constraints is firm size (Hadlock and Pierce (2010)), we examine whether small firms in the pilot group are more likely to reduce investment after Reg SHO than small firms in the control group. We also examine whether these effects are more pronounced for firms that are potentially overvalued, firms that are experiencing high short-selling activity during the experiment, firms that either experienced the largest negative market reaction or the highest trading volume around the announcement of the pilot program, and firms that were more sensitive to negative market news before the start of the experiment.

Using a difference-in-differences approach, the evidence supports our main predictions. Specifically, we find that firms in the pilot program reduced investment in fixed assets by 8% to 13% relative to the control group firms after controlling for firm characteristics such as cash flows, size, and past profitability as well as firm- and year-fixed effects. The effect is stronger for small firms, growth firms, firms with high discretionary accruals, firms with high dispersion in analysts' forecasts, firms experiencing the largest negative market reaction or the highest trading volume around the disclosure of the pilot list, and firms with high downside beta. Firms in the pilot group with a big increase in short sales activity exhibit larger reductions in investment. Moreover, consistent with the argument that firms were financing their investment with mispriced equity, we find that small firms in the pilot program decreased their equity issues during the experiment.

We perform a number of robustness checks to ensure that no other factors are driving our main results. Given the experimental setting, endogeneity is not a concern in the sense that there is no corporate action or firm characteristic that could have *caused* a firm to be included in the pilot program. However, because we are dealing with a single regulatory event, the outcomes of the experiment may have been the result of pure chance. To gauge this, we randomize the selection process of firms in the Pilot and Control Group and bootstrap an empirical distribution of results. Out of 5,000 simulations, we do not find a single example of statistically significant (at the 10% level) combined reductions in *Capital Expenditures*, *Changes in Total Assets*, *R&D expenses* and negative abnormal returns. In addition, we test whether the experiment may have been contaminated by some exogenous shock that was concentrated in firms in the pilot group for some unobserved reason. In particular, firms in the pilot group may have had a negative profitability shock that could explain the simultaneous increased short sales activity and the drop in investment. Our results are not consistent with this story. It seems unlikely that the results we document are generated by methodology choices or sample selection.

We also consider some alternative explanations for our results. In particular, we test whether managers learn about investment opportunities by looking at their stock prices; the evidence indicates that managerial learning is not the main driver of our results. We also consider whether short sellers act as external monitors of corporate governance to mitigate the overinvestment problem; we find no evidence supporting this hypothesis either. The paper is organized as follows. Section I briefly discusses the empirical strategies, the data and the variables used in the tests. Section II presents the

main empirical results, while Section III discusses alternative explanations, robustness checks and additional tests. Section IV concludes.

I. Sample, Data, and Variable Definitions

The SEC disclosed on July 28, 2004 a list of 968 firms to be included in the pilot group of Reg SHO, though the SEC adopted the list a month earlier on June 23, 2004. The SEC selected firms from the Russell 3000 index listed on NYSE, NASDAQ and AMEX and ranked them independently for each stock exchange by average daily traded volume. Every third firm on these lists was then included in the pilot group. The objective of the pilot study was to test the impact of restrictions on short sales on the market volatility, price efficiency, and liquidity.[3,4]

This process was chosen so that the pilot group would be representative of the average trading volume in each market. The selection process used by the SEC was quasi-random in the sense that the draw of firms was not purely random. It was dictated by the objective of ensuring representativeness of the three stock markets, and representativeness of the average trading volume.

We construct the main dataset from the Center for Research on Security Prices (CRSP). We build the Russell 3000 index based on market capitalization on May 28, 2004 and May 31, 2005. Consistent with the definition of the Russell 3000 at the reconstitution date, we exclude stocks with prices below $1, pink sheet and bulletin board

[3] A first announcement was made on October 28, 2003 (Securities Exchange Act Release No 48709) on the intention to carry out the experiment. External comments were requested. The final design of the experiment, the list of all firms in the pilot group, the group of firms for which all price tests were suspended, and the control group, were announced on July 28, 2004 (Securities Exchange Act Release No 50104).

[4] The pilot program (rule 202T) was part of a broader rule (Reg SHO), which was announced on the same day as Reg SHO and adopted on August 6, 2004, (Release No 34-50103). It included provisions concerning location and delivery of short-sales (rule 203) aimed at reducing naked short-selling, and new marking requirements for equity sales (rule 200 and 201).

stocks, closed-end mutual funds, limited partnerships, royalty trusts, foreign stocks and American Depositary Receipts (ADRs). In line with Diether, Lee and Werner (2009) we keep firms that were in the Russell 3000 index in 2004 and 2005 and eliminate firms added to the index between June 2004 and June 2005, or firms that are deleted from the index due to acquisitions, mergers or bankruptcies during the year. We merge this list with the list of pilot securities announced on July 28, 2004 by the SEC. Out of the 968 pilot securities in the initial list, 946 pilot securities remain in the sample after the first filter. Merging with Compustat and excluding utilities and financials leaves 1,930 firms (1,279 control / 651 pilot).[5] We do not require firms to remain in the sample over the entire sample period to avoid survivorship bias. Our final sample is an unbalanced panel of 13,526 firm-year observations with 8,919 firm-year observations in the control group and 4,607 firm-year observations in the pilot group (an average of 1,690 firms per year, 576 of which are in the pilot group).

We use *Capital Expenditures*, *Changes in Total Assets*, and *Capital Expenditures plus R&D* as measures of corporate investment. *Capital Expenditures* is equal to investment in fixed assets (Compustat item *CAPX*) scaled by the beginning-of-the-year total assets (Compustat item *AT*). *Changes in Total Assets* is equal to the percent change in total assets. R&D is equal to research and development expenses (Compustat item *XRD*) scaled by the beginning-of-the-year total assets.

We also construct *Equity Issues* and *Debt Issues* to measure the external financing activities of the firms during the Reg SHO experiment. *Equity Issues* is computed as the sale of common and preferred stock (Compustat item *SSTK*) scaled by beginning-of-the-

[5] Details of the breakout between control and pilot firms in each step of the database construction are presented in Appendix 1.

year total assets. *Debt Issues* is computed as the long-term debt issues (Compustat item DLTIS) scaled by beginning-of-the-year total assets. We multiply all numbers by 100. Appendix 2 provides more detail on all of our variable definitions.

While Panel A of Table 1 contains summary statistics for all the firms in the sample, Panel B reports the same information for small firms (firms with total assets below the sample median).[6] Consistent with the quasi-random selection of the firms in the pilot and control group, we find very little difference between the two groups. For example, both groups of firms have roughly the same size, corporate spending, payout, and capital structure. None of the differences in characteristics are statistically significant[7]. We also estimate probit and logit models that predict inclusion in the pilot group based on a comprehensive set of firm characteristics (not reported in a table). We find that none of the variables predict inclusion in the pilot program. In short, our filtering process has not created any obvious sample selection bias for the firms in the pilot group relative to the quasi-random sample created by the SEC, and the data support the hypothesis that our pilot group firms represent a random draw from the population.

II. Short Interest and Prices

Before reporting the analysis of short sales on corporate finance decisions, we re-examine the impact of short sales constraints on short interest levels and prices. Our focus in this section is primarily on prices because our results differ from the ones in previous studies of the Reg SHO experiment. Following the methodological approach in Diether, Lee, and Werner (2009), we examine the effect of Reg SHO on short selling

[6] To help mitigate the impact of outliers and measurement errors in the data, we winsorize or trim variables at the first and 99th percentile.

[7] In addition to differences in mean, we also test for differences in the median for these variables using the two-sample Wilcoxon rank sum test. The results are similar qualitatively: we do not find any statistically significant difference between the two groups.

activity and stock prices around both the announcement date and the effective date of the pilot program.

We examine the effect of Reg SHO around the announcement date because under rational expectations, investors should incorporate the future impact of the regulatory change at the time of the announcement. Even though there are still short-selling constraints at this time, investors should be more willing to short sell the pilot stocks in upticks as they expect these stocks to converge quickly to their intrinsic value in the future. Furthermore, the future removal of short-selling constraints eliminates the option to resell the stock to more optimistic investors in the future (Scheinkman and Xiong (2003)), which reduces the current price that investors are willing to pay for the pilot stocks. Finally, Reg SHO could have an adverse effect on prices at the time of the announcement by increasing the incentives of bear raiders to manipulate the value of those firms more susceptible to short selling activity (Goldstein and Gumbel (2008)).

A. Short Interest

Previous studies have shown that short sales increased after the start of the pilot program on May 2, 2005 (e.g. Diether, Lee, and Werner (2009), Alexander and Peterson (2008), The SEC's Office of Economic Analysis (OEA, 2007)). In this sub-section we test whether *Short Interest* increases after the announcement of the pilot program. As explained above, short sellers who anticipate a real effect of the suspension of price tests on firms in the pilot group should increase their short-selling activity on these firms following the disclosure, even before the actual suspension of the price tests. We construct a monthly time-series of *Short Interest* from the monthly short interest reported by NASDAQ and NYSE. Monthly short interest is the number of all open short positions

on the last business day on or before the 15th of each calendar month as a percentage of previous calendar month shares outstanding (from CRSP). We collect open short positions from NASDAQ and NYSE over the period 2002-2009.[8]

Table 2 presents the average monthly short interest three years before and three years after the announcement of the pilot test on July 28, 2004 for the two groups of firms in the experiment (pilot and control firms). The results are presented for all firms and for small firms in the sample. *Monthly Short Interest* increases more for firms in the pilot group. To test whether the uptick rule had an effect, we construct a difference-in-difference test by comparing the difference in short interest between the pilot and control groups and difference from before to after the announcement. The difference-in-difference is +0.36%. This abnormal change is statistically significant and represents a relative increase of about 9% of the average monthly short interest. The difference in *Short Interest* was not significant in the three years preceding Reg SHO, but became significant after. *Monthly Short Interest* for the small-firms in the pilot group also increased more, though the standard errors are somewhat larger.

We also construct a measure of abnormal short interest as the residuals from two-way fixed effects regressions of *Monthly Short Interest* on firm size, market-to-book, profitability, and other firm characteristics. Our results (not reported in a table) are qualitatively similar to those reported in Table 2. Consistent with previous studies, we find that short-selling increased after the repeal of the uptick rule. It is perhaps surprising that such a seemingly mild constraint was binding and affected short selling behaviour.

[8] NASDAQ brokers-dealers are required to report their short positions as of settlement on the 15th of each month or the preceding business day. It takes 3 business days to settle trades, therefore the short interest number includes short sales that occurred 3 business days prior to the 15th.

B. The Suspension of Price Tests, Firm Valuation, and Stock Returns

If the uptick rule constrains short-selling activity, then some stocks may be overvalued.[9] As a result, prices should go down when the constraint is removed. In this section, we test whether there is such a price reaction in our sample of pilot stocks.

Determining the exact timing of expected price reaction is problematic because it relies on loose definitions of information flow and event dates. For example, the stock price reaction could either occur immediately after the approval (June 23, 2004) if information leaked out, or prices could react after the formal announcement of the pilot experiment (July 28, 2004), or after the implementation of the experiment when the uptick rule is actually removed (May 2, 2005). We focus on the approval and announcement dates since these incorporate any change in expectations that might precede implementation. Nevertheless, we test a wide variety of definitions and event windows to present a comprehensive review of price reactions.

First, we construct event study excess returns with various windows around the announcement and approval dates. Next, we also construct long-run abnormal returns for the sample period around the entire experiment (up to two years). Finally, we look at the daily returns in bearish stock market days and around negative earnings announcements.

B.1. Event Study CARs

Our first tests present simple event study cumulative abnormal returns around July 28, 2004, the date when the SEC publicly disclosed the list of firms included in the pilot. We first compute excess returns as the difference between the daily returns and the CRSP value weighted returns. We then regress these returns on a dummy variable for the

[9] For example, see Miller (1977), Harrison and Kreps (1978), Morris (1996), Duffie, Garleanu, Pedersen (2002), and Scheinkman and Xiong (2003).

inclusion in the pilot, adjusting standard errors for heteroskedasticity and clustering standard errors at the firm level. This gives us the average daily excess returns around the announcement date and the difference in average daily excess returns between the two groups of firms.

Table 3 presents our analysis for four different event windows around the announcement date. In the first two columns, we report CARs for a 3-day event window (-1,1) and the event day itself (0) around the official announcement day of July 28, 2004. Consistent with past studies (e.g., Diether, Lee, and Werner (2009)), we find little movement in prices around the official announcement date. Interestingly, Table 3 shows that the results change dramatically when we expand the event window back toward the date that the SEC approved (but did not yet announced) the list of firms included in the pilot. Going back 10 trading days before the announcement, there is a significant decline in the prices of the pilot stocks. On average, pilot stocks decline by 0.14% while the control stocks show little change. The differences are significant at the 1% level. When we expand the window back to the approval date of June 23, 2004 (26 trading days before the announcement), we find similar results. Pilot firm prices decline significantly relative to the control group. The results are strong for both the full sample (Table 3, Panel A) and for small firms (Table 3, Panel B) and are economically significant. For example, an average daily price decline of 0.14% translates to a cumulative abnormal return of -1.5% over a period of 11 days.

A closer look at the difference in CARs around the approval and announcement dates helps to explain the discrepancy between our results and past studies. Figure 1 plots the time series of the difference in CARs between the pilot and control group

around a window of 40 days before and after the announcement date. For both all firms, and small firms, most of the abnormal returns occurred well before the official announcement date. For both samples, it appears that the bulk of the negative abnormal performance occurred at least two weeks before the announcement date. The small firm sample exhibits a negative reaction starting on July 16, 2004, but the relative negative performance persists more or less one week after the official announcement date up to -3% around the first two weeks of August 2004.[10]

B.2. Long-run returns

We also test whether the negative price reaction persists in the long-run. We construct abnormal returns six months, one year, and two years after the announcement of the pilot program. We form two portfolios for the pilot and control groups, and compute the value-weighted return using monthly CRSP returns.[11] We then estimate regressions of excess returns against a market factor (CRSP value-weighted index returns) and collect the alphas to tests whether long-run returns underperform the market. Table 4 reports these results.

Although we do not find any abnormal returns using all firms in the sample (Panel A), small firms in the pilot group significantly underperform small firms in the control group. (Panel B). , For small firms, the difference in abnormal returns diminishes as the holding period following the announcement of the pilot experiment increases. The difference in abnormal monthly returns ranges from a negative 0.82% to a negative

[10] We have been unable to identify patterns of insider trading that could explain this early stock market reaction for the pilot firms using CEOs' stock transactions reported to the SEC in form 144. These results point towards information leakage about the list of firms included in the pilot group.

[11] We adjust the monthly returns from CRSP in July 2004 and August 2004 using daily returns from CRSP to only take into account returns up to June 23, 2004 for the period before the approval date of Reg SHO, and after June 23, 2004 for the period after the announcement date.

0.71% when the holding period is six months or one year. Both the economic magnitude and the statistical significance of the difference in abnormal returns are large.

Since there is no systematic difference between the characteristics of the pilot and control group at the time of portfolio formation, there is no reason to expect any systematic differences in exposure to risk factors between the two groups. Nevertheless, we also compute abnormal returns using the Fama and French (1992) three-factor model and test for differences in abnormal returns between the pilot and control group. Our results (not reported in a table) are qualitatively similar.

B.3. Downside risk and sensitivity to news announcements

In addition to the event study and long-run price behavior, we also test whether firms in the pilot group are more sensitive to bad news. If short-selling activity is boosted after the announcement of Reg SHO, then firms in the pilot should react more negatively than before Reg SHO to negative news. Because small firms were more sensitive to the change in short-sales regulation, we expect this result to be more prominent in the subset of small firms.

Our first set of tests relate to bad stock market news. To tests whether firms in the pilot group are more sensitive to systematic bad news, we sort daily market returns into quintiles and test whether the raw returns of firms in the pilot group are more negative in bad market days (quintile 1) after the announcement of the pilot program than before relative to the control group. This simple difference-in-difference test should reveal whether pilot firms are more sensitive to bad news than control firms after the repeal of the uptick rule, relative to before Reg SHO.

Table 5 presents the results from our difference-in-difference tests. The two groups of firms do not have different returns on bad market days before the announcement of Reg SHO. However, firms in the pilot group do have more negative returns than the control firms after the announcement. The difference-in-difference is statistically significant at the 1% level and the effect is larger for small firms.

We also test whether there are changes in the sensitivity to firm specific news. We test for differences between pilot and control group reactions to earnings news using standardized unexpected earnings measures as in Bernard and Thomas (1989). The results from this analysis are reported in Table 6. On average, small firms in the pilot group show large negative CARs relative to the control group on negative earnings news. This is consistent with the hypothesis that increased short selling puts more downward pressure on stocks with bad news by investors who may have been sidelined before the repeal of the uptick rule. All of our results point to a tangible downside price effect on firms in the pilot group.

C. The Real Effects of the Suspension of Price Tests

In the previous section, we show that Reg SHO results in more short-selling activity, and that this had a negative effect on prices for firms in the pilot group. Due to the randomness of the selection process, the negative shock to equity values should be independent of any changes in investment opportunities of the pilot firms. In this section, we test whether the "correction" to equity values around Reg SHO has any real effect on corporate policy.

Overvalued firms invest more than they should. Baker, Stein and Wurgler (2003) argue that financially constrained firms' equity issuance and investment policy should

respond more to changes in stock prices. Gilchrist, Himmelberg, and Huberman (2005) also show that short-selling constraints can lead to rational overvaluation that managers can strategically exploit to issue overvalued equity and invest in real assets. Unlike previous studies, we rely on an exogenous negative shock to stock prices and test whether firms reduce investment and equity issuance.

C.1. Univariate tests

Table 7 presents a univariate test of whether pilot group firms reduce *Capital Expenditures*,*Total Assets*, and Capital Expenditures plus R&D relative to control group firms during the experiment relative to before. There is no difference in the investment behavior of these two groups of firms prior to the pilot experiment. However, firms in the pilot group invest significantly less during the experiment. The effects are stronger for small firms. For example, the difference-in-difference in *Capital Expenditures* is -0.60% of total assets for all the firms in the sample and -0.97% for the small firms in the sample. The latter result corresponds to a reduction of about 17% relative to the mean *Capital Expenditures* for the small firms in the sample. This represents approximately 14% of the yearly standard deviation of total investment for the small firms in the sample. Although large in magnitude, it is still a relatively small fraction of the yearly variability in investment.

The difference-in-difference in *Changes in Total Assets* is -3.16% for all the firms in the sample and -6.12% for the small firms in the sample. The latter result corresponds approximately to a reduction of 21% relative to the mean *Changes in Total Assets* for the small firms in the sample. Although this reduction is economically large, it is only 12% of the yearly standard deviation of *Changes in Total Assets*. The difference-in-difference

of *Capital Expenditures plus R&D expenses* is also negative and statistically significant for small firms.

We also test whether firms change their financing after Reg SHO. . Consistent with the idea that equity-dependent firms (e.g., small firms) are more likely to reduce equity issue after Reg SHO, we find a negative and significant decrease in *Equity Issues* for small firms (Panel B) We also find evidence that the reduction in equity values affects the ability of firms to issue debt. Panel A shows that *Debt Issues* are negatively affected by Reg SHO.

C.2. Multivariate tests

While the results in Section C.1 provide an overview of mean differences in corporate behavior between our treatment and control samples, there are some reasons to be cautious about the results. First, estimates are formed over four years of annual data, which may induce some confounding effects if the randomness of the pilot group diminishes over time as corporate actions change. Second, because some variable like corporate spending and equity issues are highly skewed, simple difference-in-differences may mask expected variation in these variables that are driven by firm characteristics.

In this section, we account for these potential concerns by testing whether inclusion in the pilot program had an effect on corporate actions after accounting for firm fixed effects, and variations in firm size, cash-flows, profitability, etc. In this regression setting, our identification strategy relies on the exogenous shock to prices. We measure the effects of Reg SHO with a dummy variable equal to 1 if the firm was announced to be in the pilot group or has had price tests suspended on its stock for at least 6 months

during the fiscal year end-date and equal to zero otherwise[12]. The SEC announced the list of firms in the SHO pilot group on July 28, 2004, and suspended price tests on May 2, 2005 for firms in the pilot group, while it suspended price tests for all firms on July 7, 2007.

Panel A of Table 8 presents our regression results. The first set of regressions show that *Capital expenditures* are reduced by the introduction of Reg SHO and the effect is concentrated in small firms. The economic magnitude of the effect is again large: the effect of Reg SHO on Capital Expenditures is a reduction between 12% and 15% of mean *Capital Expenditures* (10% and 13% of the yearly standard deviation of *Capital Expenditures*). There is also evidence that Reg SHO adversely affected c*hanges in Total Assets* and *Capital Expenditures plus R&D Expenses.* These effects are primarily concentrated in the smallest firms in the sample.

Our assumption is that firms that are financially constrained are the most likely to be affected by Reg SHO. This should translate into a measureable decrease in the financing activity of the small firms in the sample. We test this assumption using *Equity Issues* and *Debt Issues* as the dependent variables. We control for the firms' cash-flows, size, profitability, lagged leverage, and lagged cash holdings. The results are presented in Panel B of Table 8.

Small firms that are subject to the suspension of price tests decrease equity issuance activity but do not decrease debt issuance. The coefficient on the dummy *SHO* variable or the interacted *Small Firms* and *SHO* variable is negative and significant at the

[12] This specification is equivalent to interacting a dummy for the inclusion in the Pilot group with a dummy for the time period over which the experiment is conducted given that we include firm-fixed effects. The only difference is that we allow firms in the control group that are later subject to the repeal of the uptick rule from July 7, 2007 to be considered as part of the experiment.

5% or 1% level in all specifications. The economic magnitude of the reduction is large: from 19% to 34% of mean *Equity Issues* but only 10% of the yearly standard deviation. The point estimate on *Debt Issues* is negative but statistically insignificant fro small firms, but significant for all firms. We conclude that Reg SHO caused a reduction in financing activity, especially in the equity issuance activity of small firms.

Our results are consistent with an increased cost of equity issuance owed to a negative shock on stock prices. This cost is also potentially increased as the pilot stocks become more sensitivity to both market-wide and firm-specific negative news (see section II.B.). It is also well known that firms react negatively to public announcements of SEOs (e.g. Asquith and Mullins, 1986). Therefore, rational managers that anticipate a stock price decline may be more reluctant to issue equity.

C.3. Interaction Effects

Finally, we present a series of tests that identify the source of the impact on corporate behavior. We find that the real effects of Reg SHO are concentrated among the firms most likely to be impacted by Reg SHO around the announcement date of the experiment or based on *a priori* characteristics. We expect firms in the pilot program that were overvalued before the start of Reg SHO to be more likely to be impacted by Reg SHO. We test this hypothesis using *High Total Accruals*, *High Market-to-Book*, and *High Analyst Dispersion* as proxies for stock price overvaluation. *High-Total Accruals* is a dummy variable equal to one if the *Total Accruals* for the firm fell into the top decile before the announcement of Reg SHO. Polk and Sapienza (2009) and Hirshleifer, et.al (2011) argue that accruals are a proxy for overvaluation as managers use accruals to inflate the non-cash component of their earnings. *Total Accruals* represent the stock of

accumulated accruals. The higher the total accruals, the more likely managers have used accruals management to inflate earnings and stock prices in the past. *High-Market-to-Book* is also a common proxy for overvaluation used in the finance literature (e.g. Baker, Stein and Wurgler (2003)). We use a dummy variable equal to 1 if the firm's market-to-book ratio falls into the top quintile before the Reg SHO announcement date and equal to 0 otherwise.

Analyst's disagreement has been used as a proxy for investors' dispersion of beliefs. Diether, Malloy, and Scherbina (2002) find that analysts' EPS forecast dispersion predict negative subsequent returns. In our tests, we use the dispersion of analyst recommendations to proxy for investors' disagreement. We use a dummy variable equal to 1 if the analysts' standard deviation of recommendations falls in the top decile before the Reg SHO announcement date and equal to 0 otherwise.

We show in section II.B. that small pilot firms during Reg SHO became more sensitive to negative market-wide news. We therefore expect firms that are more sensitive to bad market news before the start of the experiment to be most affected in their investment and financing decisions. We use the downside beta (Ang, Chen, and Xing, (2006)) as a proxy for the sensitivity to negative news. *High Downside Beta* is a dummy variable equal to 1 if the firm's average downside beta (the beta of the firm conditional on market returns being below the average yearly returns) falls in the top decile before the announcement of Reg SHO and equal to 0 otherwise.

We expect firms that exhibited the most negative CARs and the highest trading volume around the announcement date to be most impacted by the experiment if the stock market sees through the real effects of the experiment or if the exogenous drop in stock

prices affected the firms most in their equity issuance and their investment activity or both. We use the logarithm of (one plus) the cumulative abnormal returns during the 11 trading days around the announcement date (July 14, 2004 to July 29, 2004) as the proxy for *Announcement CARs*. Trading Volume is the sum of the common stock daily traded volume (from CRSP) divided by 100 over the 12 trading days around the announcement date of the Pilot program.

Finally, we use short interest to examine whether the effects of Reg SHO on real decisions are driven by the actions of short sellers. We expect that firms experiencing *ex-ante* high short selling activity should be more sensitive to the effects of Reg SHO. To test this conjecture, we interact the Reg SHO dummy with High Short Interest, which is a dummy variable equal to 1 if the average reported monthly short interest scaled by the total number of shares outstanding during the fiscal year falls in the top quintile, and is equal to 0 otherwise.

The results of the cross-sectional regressions are presented in Table 9. Consistent with our predictions, Panel A shows that firms that are likely to be overvalued before the announcement date of Reg SHO decrease more their *Capital Expenditures* and *Capital Expenditures plus R&D Expenses* using high total accruals and high market-to-book ratio as measures of overvaluation. Firms that are likely to be overvalued before the announcement date of Reg SHO decrease more their *Changes in Total Assets* using high analyst dispersion as a proxy for overvaluation. Seemingly-overvalued firms decrease more their *Equity Issues* using all three measures of overvaluation. We do not find any effects of overvaluation on *Debt Issues*.

Panel B shows that firms reduce more their *Capital Expenditures*, *Changes in Total Assets, Capital Expenditures plus R&D Expenses* , *Equity Issues,* and *Debt Issues* when they are more sensitive to bad news before the announcement date of Reg SHO (e.g., high downside beta). This panel also shows that firms that exhibit the lowest CARs around the announcement date experience the largest decrease in investment (as measured by *Changes in Total Assets*), *Equity Issues* and *Debt Issues*. We also find that firms that exhibit the highest trading activity around the announcement date experience the largest decrease in investment and equity issues, while the firms that exhibit increased short selling activity after the announcement and during the experiment decrease more their investment and equity and debt issues. These results suggest that the mispricing correction through the relaxation of the short-selling constraint drives the reduction in investment and equity issuance activity. This mispricing correction primarily affects overvalued firms before the announcement of Reg SHO. Firms that were sensitive to bad news before the announcement of Reg SHO were also more likely to reduce equity issuance and corporate investment. This suggests that the managers of these firms become even more reluctant to issue equity because they anticipated that Reg SHO would increase their firms' stock price sensitivity to negative news such as an SEO announcement.

III. Robustness

We perform a number of robustness checks and investigate alternative explanations for the results presented so far. First, we investigate whether the standard errors and coefficients that we get from our regressions could have been the result of pure chance. This is an important issue because Bertrand, Duflo, and Mullainathan (2004) suggest that Difference-in-differences statistics may underestimate standard errors. To account for this, we perform a Monte Carlo analysis where we randomize the selection

process in the Pilot and the Control Group and perform several analyses presented earlier to test our main hypothesis. We repeat the randomization process 5,000 times and obtain key regression coefficients, standard errors and t-stats from the fixed-effects capital investment regressions (using *Capital Expenditures*, *Changes in Total Assets* and *Capital Expenditures plus R&D* as the dependent variables), and the long-run abnormal returns following the announcement date.

All t-stats and coefficients are consistent with error rates of 5% in two-tailed tests. In particular, it should be noted that we only get three randomized samples in which we find a simultaneous reduction in Capital Expenditures, Changes in Total Assets and Capital Expenditures plus R&D in the pseudo pilot group at the 10% level. We cannot find a single randomized sample in which a firm simultaneously reduces its corporate spending measures, and long-run abnormal returns at the 10% level. We report the statistical distribution of the t-stats for these tests in Table 10. All t-stats correspond to the expected rejection values at the 1, 5, 10, 90, 95 and 99 percentiles. The probability that our results are purely driven by chance is less than about 0.02% (or 1/5000).

In addition to these simulations, we also bootstrap an empirical distribution of results for the CARs of firms in the pilot group. First, we randomized which firms are included in the treatment and tested whether there were abnormal CARs around the 12-day announcement window detailed in Table 3. In 5,000 simulations, we find only seven cases (0.14%) of negative CARs with a T-stat as large as we find in the data. Second, we took the actual sample of pilot firms and randomized the event window. That is, we took the pilot stocks and formed event windows based on a random start date between January

2001 to June 1st 2004. In these simulations, we do not find a single example of negative CARs with a T-statistic as large as we find in the data.

We also investigate whether short sellers discipline managers by monitoring their actions through short selling behavior. By shorting stocks of mismanaged firms, short-sellers may put pressure on managers who care about stock prices to correct mismanagement and alter their corporate financial policies in a way that is consistent with good governance firms. The free cash flow theory (Jensen, 1986) suggests that firms with strong shareholders rights should reduce wasteful investment (e.g. diversification acquisitions), reduce cash holdings, increase leverage and increase payout through dividend payments and share repurchases. Admati and Pfleiderer (2009) argue that the threat by large shareholders of selling shares may be credible and may discipline managers. Short selling could be thought of as an extension of this threat to sell stocks by existing large shareholders. However, this hypothesis predicts an increase in value following the short-selling activity that induces better investment. Our asset pricing results do not confirm this alternative explanation.

We also examine whether managerial learning can explain our empirical results. Chen, Goldstein, and Jiang (2007) document that managers use information impounded into stock prices to improve corporate investment decisions. Therefore, an alternative explanation to our results is that managers learn in a sample where prices go down on average, and therefore reduce investment. To test this hypothesis, we re-estimate Chen, Goldstein, and Jiang (2007)'s corporate investment specification using the Reg SHO experiment as an instrument for increased stock price informativeness. However, we do not find that firm's investment becomes more sensitive to stock prices after the

experiment, which suggests that managerial learning may not be the primary channel in our sample.

IV. Conclusion

In this paper we test whether short-selling constraints distort investment and financing decisions. Using a natural experiment, we find that firms that were included in a randomly selected pilot group for the SECs regulation SHO experienced more short sales and a continued increase in selling pressure relative to a control group of stocks.

Inclusion in this treatment group had a real effect on corporate decisions. Using difference-in-difference tests around the pilot program, we find that investment and equity issues declined for firms that were subject to an increase in short selling. The results are stronger for small firms and for firms that appear more sensitive to overvaluation prior to the regulatory change.

Our results have a number of implications. First, we find that even a subtle regulation like the uptick rule can have a significant effect on the equity prices of small firms and appear to be a binding constraint on the equilibrium level of short selling. Second, we find that corporate investment and security issues are sensitive to changes in equity prices, apparently driven by overvaluation that stems from the restriction on short selling activity. Our results are directly related to Gilchrist, Himmelberg, and Huberman (2005) who posit that short sales constraints can drive distortions in share issuance and real investment. Our findings support their prediction and suggest that random variation in equity prices appears to distort corporate investment flows.

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Appendix 1

Construction of the sample of Pilot and Control group firms

The various steps in the sample selection process and the remaining firms in the sample are detailed in the table below:

Selection process	Total # Firms left after selection	# Firms in Control Group	# Firms in Pilot Group
Russell 3000 on May 31, 2004	3,000		
Only firms listed on Nasdaq national market securities market, (NNM), AMEX and NYSE	2,968		
Russell 3000 in 2004 and 2005	2,747	1,801	946
Compustat merge	2,565	1,685	883
Banks and financial services firms are excluded	2,040	1,349	691
Utility firms are excluded (Final Sample)	1,930	1,279	651

Appendix 2 – Definition of Main Variables

CAPX	Capital expenditures (Compustat *CAPX*) scaled by start-of-year total assets (*AT*) x 100
CAPXR&D	Capital expenditures (*CAPX*) plus Research and Development Expenses (*XRD*) if non-missing scaled by start-of-year total assets (*AT*) x 100
Cash flow	Net income before extraordinary Items (IB) + depreciation and amortization expenses (*DP*) scaled by start-of-year total assets x 100
Cash Holdings	Cash and Short Term Investment (*CHE*) scaled by start-of-year total assets (*AT*) x 100
Δ *Total Assets*	Total Assets (*AT*) divided by start-of-year Total Assets minus one x 100
Debt Issues	Long-term debt Issues (*DLTIS*) scaled by start-of-year Total Assets (*AT*) x 100
Dividends	Common Shares Dividends (*DVC*) plus Preferred Shares Dividends (*DVP*) scaled by start-of-year total assets (*AT*) x100
Equity Issues	Sale of Common and Preferred Shares (*SSTK*) scaled by start-of-year Total Assets (*AT*) x 100
Leverage	Long term debt (*DLTT*) plus debt in current liabilities (*DLC*) scaled by the sum of long term debt, debt in current liabilities, and total stockholders' equity (*SEQ*) x 100
Market-to-Book ratio	Market value of equity (*PRCC* x *CSHO*) plus book value of assets minus book value of equity minus deferred taxes (when available) (*AT-CEQ-TXDB*), scaled by book value of total assets (*AT*). Variable is lagged one year
Monthly Short Interest	Monthly short interest reported to NASDAQ or NYSE on the 15th of each calendar month scaled by the total number of shares outstanding (from CRSP) at the start of the month.
Past profitability	Ratio of operating income before depreciation and amortization (*OIBDP*) to start-of-year total assets (AT) x 100. Variable is lagged one year
SHO	Dummy variable equal to 1 if the company is in the Pilot Group of REG SHO and the fiscal year includes at least 6 months of activity after the announcement of Reg SHO (July 28, 2004) and equal to 0 otherwise. The dummy variable is equal to 0 for firms in the control group before the suspension of all prices for all firms in the US stock markets and equal to 1 after the firms' fiscal year includes at least 6 months after the suspension of all prices for all firms in the US stock markets (July 6, 2007).
Short Interest	Average reported monthly short interest during the fiscal year, where monthly short interest reported to NASDAQ or NYSE is scaled by the total number of shares outstanding (from CRSP). Variable is lagged one year
Total assets	Start-of-year total assets (*AT*) (in million USD)

Figure 1

Cumulative Abnormal Returns

This graph presents the equally weighted cumulative abnormal return for all the firms and small firms in the pilot group minus the cumulative abnormal return for the firms in the control group around the approval date (June 23, 2004) and the announcement date (July 28, 2004) of the pilot program.

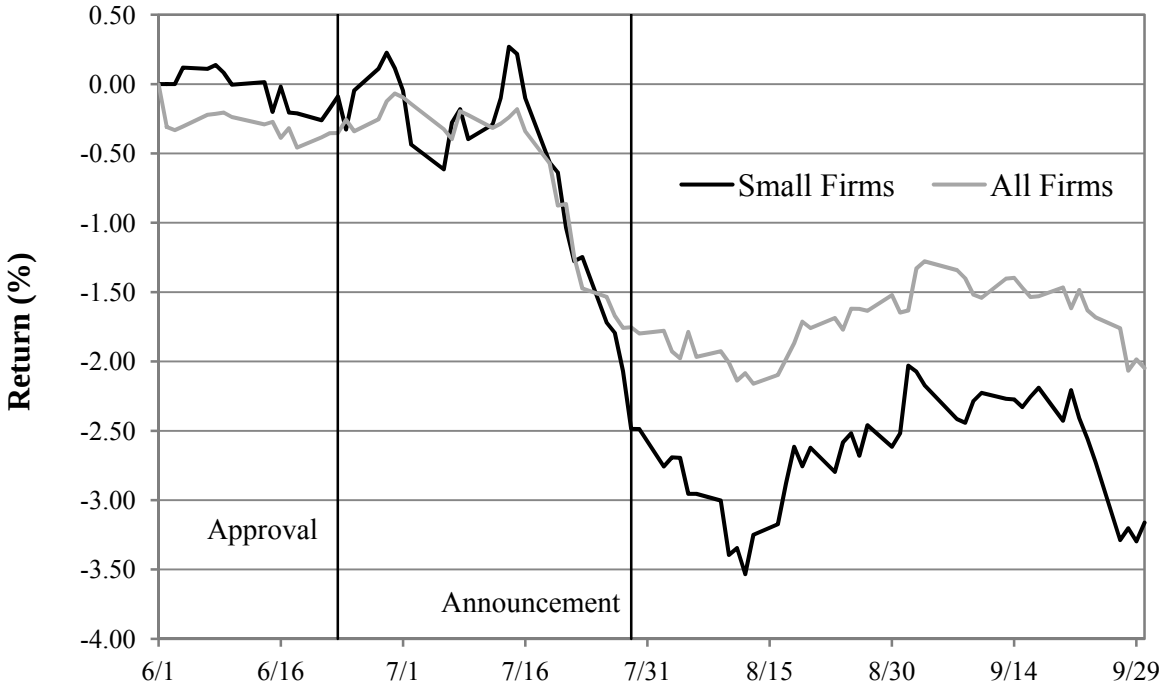

Table 1

Summary Statistics

Data are collected from the merged CRSP/Compustat Industrial database in the fiscal year that is the closest to July 28, 2004, the announcement date of the SHO pilot test. We exclude firms that are not in the Russell 3000 index in 2004 and 2005, financial services firms (SIC code 6000-6999), regulated utilities (SIC code 4900). Small Firms are firms that have a value of Total Assets below the sample median. T-stat is the t-statistic of a two sample t-test. All variables are described in Appendix 2.

Panel A: Entire Sample

	Pilot group				Control group					
	N	Mean	Median	Std.Dev	N	Mean	Median	Std.Dev	Diff.	T-stat
Total assets	651	3,448	735	11,276	1,270	3,779	696	12,459	-331	-0.57
Market-to-Book ratio	651	2.31	1.77	1.68	1,270	2.24	1.70	1.65	0.07	0.90
CAPX	643	5.82	3.78	6.60	1,249	5.59	3.45	6.40	0.23	0.73
Δ *Total Assets*	635	21.55	10.66	39.08	1,235	19.33	10.81	33.89	2.21	1.27
CAPXR&D	634	11.00	7.66	10.35	1,239	11.12	7.88	10.37	-0.12	-0.23
Cash flow	641	9.15	10.22	14.81	1,250	8.59	10.23	16.86	0.56	0.72
Leverage	649	27.27	23.35	27.23	1,263	27.30	22.99	27.78	-0.03	-0.02
Equity Issues	625	5.62	1.42	14.17	1,202	5.52	1.40	13.87	0.10	0.15
Debt Issues	612	12.14	0.01	23.93	1,186	10.74	0.00	22.56	1.40	1.22
Dividends	642	0.90	0.00	1.80	1,248	0.87	0.00	1.78	0.03	0.35
Cash Holdings	643	26.87	16.35	31.49	1,250	27.47	15.62	31.00	-0.60	-0.40
Past profitability	640	10.48	12.29	13.37	1,245	9.86	11.58	14.65	0.63	0.91

Panel B: Small Firms

	Pilot group				Control group					
	N	Mean	Median	Std.Dev	N	Mean	Median	Std.Dev	Diff.	T-stat
Total assets	316	329	248	172	634	316	236	169	13	0.93
Market-to-Book ratio	316	2.76	2.07	1.93	634	2.60	1.98	1.87	0.16	1.26
CAPX	313	5.85	3.60	6.98	629	5.73	3.31	7.00	0.12	0.25
Δ *Total Assets*	306	29.24	13.73	49.25	622	25.78	13.93	41.67	3.47	1.12
CAPXR&D	310	13.66	10.27	12.25	626	14.02	10.47	12.26	-0.36	-0.42
Cash flow	313	7.36	10.18	19.54	630	7.08	10.70	21.82	0.28	0.19
Leverage	316	18.62	5.01	27.34	630	17.64	4.96	25.33	0.98	0.55
Equity Issues	302	9.06	2.04	19.34	606	8.77	2.01	18.56	0.29	0.22
Debt Issues	296	11.52	0.00	24.49	603	9.99	0.00	23.86	1.52	0.89
Dividends	313	0.65	0.00	1.86	629	0.70	0.00	1.87	-0.05	-0.44
Cash Holdings	313	39.65	28.79	37.86	630	39.00	30.56	36.20	0.65	0.26
Past profitability	312	7.63	10.88	16.94	629	6.92	10.36	18.57	0.70	0.56

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Table 2

SHO Pilot and Short Interest

This table presents mean values of Monthly Short Interest, for firms that were part of the pilot and group control group for three years pre- and post-SHO around the announcement date (July 28, 2004). Monthly Short Interest is the monthly mean ratio of net short positions outstanding reported on the 15th of each month to shares outstanding at the start of the month. Averages are computed for all firms that are in the Pilot Group and in the Control Group, and for the subset of firms that have Total Assets below the median value of Total Assets (Small firms). T-statistics are constructed with Newey-West standard errors (2 lags). Panel A and B present the results for the period three years before and after the announcement of the Pilot Program (July 28, 2004). c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

All firms	Before	After	Difference	T-statistic
Pilot Group	3.93	6.94	3.01[a]	(26.84)
Control Group	3.98	6.67	2.68[a]	(34.71)
Difference	-0.06	0.28[b]		
T-statistic	(-0.83)	(2.34)		
Difference-in-difference			0.33[b]	(2.44)
Small firms				
Pilot Group	4.04	7.37	3.33[a]	(22.88)
Control Group	4.07	7.09	3.01[a]	(29.60)
Difference	-0.03	0.29[b]		
T-statistic	(-0.28)	(1.98)		
Difference-in-difference			0.32[c]	(1.78)

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Table 3

Announcement Day Abnormal Returns

Panel A and B present the daily abnormal returns of firms around the announcement date of the list of Pilot firms (July 28, 2004) for all firms in the sample and for the small firms in the sample (below median Total Assets). We use 4 different time windows around the announcement date: (-1,1) (from July 27 to July 29, 2004), (0,0) (July 28, 2004), (-10,1) (from July 14 to July 29, 2004), and (-26,1) (June 23 to July 29, 2004). The daily abnormal returns are the difference between the firms' daily returns and the market's value weighted daily returns from CRSP. T-stats are displayed within brackets next to the relevant coefficient. Standard errors are adjusted for heteroskedasticity and are clustered at the firm level.

Panel A: All Firms

Event Window	(-1,1)		(0,0)		(-10,1)		(-26,1)	
	Abnormal Returns	T-stat	Abnormal Returns	T-stat	Abnormal Returns	T-stat	Abnormal Returns	T-stat
Pilot	0.44[a]	(5.84)	-0.65[a]	(-5.78)	-0.14[a]	(-4.41)	-0.10[a]	(-5.26)
Control	0.49[a]	(9.23)	-0.68[a]	(-7.53)	-0.01	(-0.36)	-0.05[a]	(-3.57)
Difference	-0.06	(-0.59)	0.04	(0.27)	-0.13[a]	(-3.44)	-0.06[b]	(-2.27)

Panel B: Small Firms

Event Window	(-1,1)		(0,0)		(-10,1)		(-26,1)	
	Abnormal Returns	T-stat	Abnormal Returns	T-stat	Abnormal Returns	T-stat	Abnormal Returns	T-stat
Pilot	0.49[a]	(4.09)	-1.02[a]	(-5.38)	-0.24[a]	(-4.63)	-0.18[a]	(-5.50)
Control	0.80[a]	(9.58)	-0.81[a]	(-5.59)	-0.04	(-0.96)	-0.09[a]	(-3.98)
Difference	-0.31[b]	(-2.09)	-0.21	(-0.88)	-0.20[a]	(-3.20)	-0.09[b]	(-2.23)

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Table 4

Long-horizon Market-adjusted Abnormal Returns

Panel A and B present the abnormal value-weighted monthly alphas of portfolios computed using the market model. The alphas are computed for all firms in the Pilot Group and in the Control Group (Panel A) and firms in the Pilot Group and in the Control Group that have Total Assets below the median value of Total Assets (Small firms in Panel B). We compute alphas over six-month, one year, and two year periods after June 23 2004, the date of the SEC approval of Reg SHO prior to the announcement of the Pilot Program on July 28, 2004. Standard errors are adjusted for heteroskedasticity.

Panel A: All Firms

Event Window	Six Months		One Year		Two Years	
	Abnormal Returns	T-stat	Abnormal Returns	T-stat	Abnormal Returns	T-stat
Pilot	-0.14[c]	(-2.56)	-0.65	(-1.71)	-0.52b	(-2.48)
Control	-0.00	(0.02)	-0.11	(-0.47)	-0.15c	(-1.15)
Difference	-0.14	(-1.50)	-0.54	(-1.04)	-0.37	(-1.29)

Panel B: Small Firms

Event Window	Six Months		One Year		Two Years	
	Abnormal Returns	T-stat	Abnormal Returns	T-stat	Abnormal Returns	T-stat
Pilot	-1.74[c]	(-2.38)	-1.28[a]	(-3.16)	-0.84[a]	(-2.78)
Control	-0.92	(-1.18)	-0.57	(-1.20)	-0.49[c]	(-1.75)
Difference	-0.82[a]	(-6.49)	-0.71[a]	(-4.08)	-0.35[b]	(-2.08)

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Table 5

Sensitivity to Negative Daily Market Returns

Panel A and Panel B present mean daily raw returns for all firms in the sample (Panel A) and the small firms in the sample (below median Total Assets, Panel B) that were part of the pilot experiment, and firms that were part of the control group. We sort the observations based on the quintile of market daily returns. Quintile 1 of market daily returns is the lowest quintile of market daily returns while quintile 5 is the largest. Daily returns are the daily returns from CRSP. The difference-in-difference measures the change in mean daily returns after the announcement of the Pilot (versus before the announcement of the Pilot) for the pilot group relative to the control group. The t-statistic is the coefficient of the variable that interacts a dummy for the period with a dummy variable for firms in the Pilot program in an OLS regression where the daily returns are regressed on a dummy for firms in the Pilot, a dummy variable equal to 1 after the period is started (i.e. After the announcement of the Pilot) and the interaction term of these two variables. Before announcement of the Pilot is the one year period before July 27, 2004. After the announcement of the Pilot is the one-year period after July 28, 2004. Standard errors adjust for heteroskedasticity and are clustered at the firm level.

Panel A: All firms

	Before				After					
Quintile	Pilot	Control	Diff.	T-stat	Pilot	Control	Diff.	T-stat	Diff.-in-Diff.	T-stat
1	-1.50	-1.51	0.01	(0.87)	-1.29	-1.24	-0.05[b]	(-2.94)	-0.06[a]	(-2.94)
2	-0.27	-0.25	-0.01	(-0.67)	-0.41	-0.38	-0.03[b]	(-2.08)	-0.02	(-0.92)
3	0.20	0.19	0.01	0.46	0.11	0.12	-0.01	(-0.89)	-0.02	(-0.93)
4	0.75	0.78	0.03	(-1.82)	0.56	0.54	0.02	(1.10)	0.05[b]	(2.08)
5	1.64	1.63	0.01	(0.70)	1.33	1.31	0.02	(1.12)	0.00	(0.18)

Panel B: Small firms

	Before				After					
Quintile	Pilot	Control	Diff.	T-stat	Pilot	Control	Diff.	T-stat	Diff.-in-Diff.	T-stat
1	-1.65	-1.70	0.05[c]	(1.72)	-1.40	-1.35	-0.06[b]	(-2.11)	-0.11[a]	(-2.72)
2	-0.26	-0.26	0.00	(0.12)	-0.45	-0.41	-0.05[c]	(-1.75)	-0.05	(-1.26)
3	0.21	0.21	0.00	(-0.06)	0.09	0.12	-0.03	(-1.24)	-0.03	(-0.78)
4	0.86	0.90	-0.04	(-1.60)	0.59	0.58	0.00	(0.21)	0.05	(1.38)
5	1.82	1.81	0.01	(0.43)	1.42	1.43	0.00	(0.14)	0.03	(0.69)

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Table 6

Abnormal Return after Negative Earnings News

Panel A (Panel B) presents the cumulative abnormal returns computed one day before up to one day after the date of announcement of the negative earnings news for all firms in the Pilot and the control Group (Small Firms in the Pilot and the control Group - small firms being below median Total Assets). Before announcement of the Pilot is the one year period before July 27, 2004. After the announcement of the Pilot is the one year period after July 28, 2004. The negative earnings news is defined as quarterly earnings that fall below expected quarterly earnings using a first order autoregressive estimation adjusted for seasonality effects. Standard errors are adjusted for heteroskedasticity.

Panel A: All firms	Before	After	Difference	T-statistic
Pilot Group	0.04	-0.68	-0.72	(-3.28)
Control Group	-0.27[c]	-0.54	-0.27	(-1.56)
Difference	0.31	-0.14		
T-statistic	(1.31)	(-0.92)		
Difference-in-difference			-0.45	(-1.60)
Panel B: Small firms				
Pilot Group	-0.18	-1.16[a]	-0.97[a]	(-2.72)
Control Group	-0.53	-0.58[a]	-0.05	(-0.17)
Difference	0.35	-0.58[b]		
T-statistic	(0.95)	(-2.28)		
Difference-in-difference			-0.93[b]	(-2.07)

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Table 7

Real Effects of the SHO Pilot Program: Univariate Results

This table presents the mean values of Capital Expenditures, Changes in Total Assets, CAPX & R&D, Equity Issues, and Debt Issues before and during the Pilot program for all firms in the sample (Panel A) and for Small firms (firms that have Total Assets below the median value of Total Assets – Panel B). The difference-in-difference measures the change in the mean value after the announcement and during the pilot (versus before the announcement) for firms in the pilot group relative to firms in the control group. The t-statistic is the coefficient of the variable that interacts a dummy for the period with a dummy variable for firms in the Pilot program in an OLS regression where investment is regressed on a dummy for firms in the Pilot, a dummy variable equal to 1 after the Pilot is started and the interaction term of these two variables. Before is the period that extends 3 years before the announcement of the Pilot program (July 28, 2004) while After is the 3 years period of the Pilot Program before the repeal of the uptick rule (July 28, 2004 to July 6, 2007). We require the fiscal year to overlap at least 6 months during the period considered to be included in the sample. All mean values appear in bold while t-stats are displayed within brackets next to the relevant coefficient. Standard errors are adjusted for heteroskedasticity and clustered at the firm level. All variables are detailed in Appendix 2.

Panel A: All Firms

	CAPX		Δ Total Assets		CAPXR&D		Equity Issues		Debt Issues	
	Before	After	Before	After	Before	After	Before	After	Before	After
Pilot	5.63	5.82	14.90	12.68	10.85	10.77	4.68	3.45	11.00	10.54
Control	5.37	6.16	13.20	14.14	10.93	11.40	4.70	3.99	9.79	10.71
Difference (Pilot-Control)	0.26	-0.34	1.71	-1.45	-0.09	-0.63	-0.03	-0.54	1.21	0.17
	(0.94)	(-1.07)	(1.19)	(-1.50)	(-0.17)	(-1.35)	(-0.06)	(-1.46)	(1.49)	(-0.67)
Difference-in-Difference		-0.60[a]		-3.16[b]		-0.54		-0.52		-1.38[c]
		(-2.85)		(-2.28)		(-1.62)		(-1.10)		(-1.67)

Panel B: Small Firms

	CAPX		Δ Total Assets		CAPXR&D		Equity Issues		Debt Issues	
	Before	After	Before	After	Before	After	Before	After	Before	After
Pilot	5.46	5.40	18.25	14.07	13.49	12.56	7.59	4.69	8.69	8.31
Control	5.31	6.21	15.31	17.25	13.77	13.88	7.21	5.92	8.34	9.74
Difference (Pilot-Control)	0.15	-0.82[c]	2.93	-3.18[b]	-0.28	-1.33[c]	0.38	-1.23[c]	0.35	-1.46
	(0.37)	(-1.71)	(1.50)	(-2.04)	(-0.55)	(-1.76)	(0.43)	(-1.84)	(0.31)	(-1.22)
Difference-in-Difference		-0.97[a]		-6.12[b]		-1.05[c]		-1.61[c]		-1.80
		(-2.88)		(-2.55)		(-1.81)		(-1.82)		(-1.52)

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Table 8

Corporate Investment, Financing and the SHO Pilot Program: Multivariate Results

This table presents the results of OLS regressions with firm-fixed effects and year-fixed effects for the subset of Small firms (firms with Total Assets below the sample median) (columns 1, 3, 5) and all firms in the sample (columns 2, 4, 6). The dependent variables are CAPX (columns 1 and 2), Changes in Total Assets (columns 3 and 4), and CAPXR&D (columns 5 and 6). Coefficient estimates appear in bold while t-statistics are displayed within brackets under each coefficient. Standard errors adjust for heteroskedasticity and within correlation clustered by firm. All variables are defined in Appendix 2.

Panel A – Corporate Investment

	CAPX		Δ Total Assets		CAPXR&D	
	1	2	3	4	5	6
SHO	-0.54[b]	-0.01	-4.10[b]	-2.34[c]	-0.85[c]	-0.10
	(-2.00)	(-0.06)	(-2.13)	(-1.93)	(-1.91)	(-0.40)
SHO x Small Firms		-0.62[a]		0.02		-0.76[b]
		(-2.63)		(0.01)		(-2.05)
Cash Flow	0.03[a]	0.03[a]	0.58[a]	0.57[a]	-0.07[a]	-0.07[a]
	(3.48)	(4.83)	(6.91)	(8.24)	(-3.80)	(-4.38)
Log(Lagged Total Assets)	-0.88[b]	-0.96[a]	-36.15[a]	-34.68[a]	-5.82[a]	-5.06[a]
	(-2.35)	(-3.74)	(-14.70)	(-19.91)	(-7.49)	(-9.86)
Past Profitability	3.68[a]	5.23[a]	20.32[c]	28.20[a]	0.07	3.03
	(3.80)	(6.39)	(2.33)	(3.83)	(0.04)	(1.92)
Sample	Small	All	Small	All	Small	All
Firm-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N obs	5,807	11,929	5,812	11,942	5,807	11,929
Adj. R2	0.70	0.74	0.31	0.30	0.78	0.79

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

This table presents the results of OLS regressions with firm-fixed effects and year-fixed effects for the subset of Small firms (firms with Total Assets below the sample median) (columns 1, 3) and all firms in the sample (columns 2, 4). The dependent variables are Equity Issues (columns 7 and 8), and Debt Issues (columns 9 and 10). Coefficient estimates appear in bold while t-statistics are displayed within brackets under each coefficient. Standard errors adjust for heteroskedasticity and within correlation clustered by firm. All variables are defined in Appendix 2.

Panel B - Financing

	Equity Issues		Debt Issues	
	1	2	3	4
SHO	-1.43[b]	0.73[b]	-1.09	-1.98[b]
	(-1.97)	(1.96)	(-0.91)	(-2.21)
SHO x Small Firms		-2.45[a]		1.42
		(-4.62)		(1.27)
Cash Flow	-0.07[c]	-0.04	-0.02	-0.03
	(-1.79)	(-1.57)	(-0.53)	(-0.95)
Log(Lagged Total Assets)	-10.34[a]	-7.66[a]	-3.93[a]	-5.86[a]
	(-9.71)	(-11.07)	(-3.46)	(-6.08)
Past Profitability	2.92	-0.23	4.09	9.13[b]
	(0.68)	(-0.06)	(0.90)	(2.33)
Lagged Leverage	0.08[a]	0.05[a]	-0.07[b]	-0.09[a]
	(4.09)	(3.65)	(-2.48)	(-4.47)
Sample	Small	All	Small	All
Firm-fixed effects	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes
N obs	5,560	11,340	5,363	11,099
Adj. R2	0.42	0.42	0.44	0.48

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Table 9

Interaction Effects: Multivariate Results for Small Firms

This table presents the results of OLS regressions with firm-fixed effects and year-fixed effects for small firms in the sample (firms with Total Assets below the median). The dependent variables are CAPX, Changes in Total Assets, CAPXR&D, Equity Issues, and Debt Issues. Controls are identical to those used in the main specification of the multivariate analysis (Table 8 Panel A and B). Panel A presents the results for the ex-ante overvaluation proxies. High Total Accruals is a dummy variable equal to 1 if Total Accruals before the announcement of the SHO Pilot Program falls in the top decile, and is equal to 0 otherwise. High Market-to-Book is a dummy variable equal to 1 if the Market-to-Book before the announcement of the SHO Pilot Program falls in the top quintile, and is equal to 0 otherwise. High Analyst Dispersion is a dummy variable equal to 1 if the standard deviation of Analyst Recommendation before the announcement of the SHO Pilot Program falls above the median and is equal to 0 otherwise. Panel B presents the results for the stock price characteristics. High Downside Beta is a dummy variable equal to 1 if the average Downside beta (the beta of the firm conditional on market returns being below the average yearly returns of the firm (as defined in Ang, Chen, and Xing, 2006), computed on a monthly basis over one year before the announcement of the SHO Pilot Program falls in the top decile, and is equal to 0 otherwise. Low CAR is a dummy variable equal to 1 if the announcement CAR of the firm 12 trading days around the announcement date of the Pilot program (July 14, 2004 to July 29, 2004) falls in the bottom quintile and equal to 0 otherwise. Trading Volume is the sum of the common stock daily traded volume (from CRSP) divided by 100 over the 12 trading days around the announcement date of the Pilot program (July 14, 2004 to July 29, 2004). High Short Interest is a dummy variable equal to 1 if the average reported monthly short interest scaled by the total number of shares outstanding during the fiscal year falls in the top quintile, and is equal to 0 otherwise. The variable is lagged one year. We only report the coefficient estimates of the SHO dummy variable and the interaction term for expositional brevity but we include the main effect in the regression when relevant, which is only the case for High Short Interest as other variables are time invariant. Coefficient estimates appear in bold while t-statistics are displayed within brackets under each coefficient. Standard errors adjust for heteroskedasticity and within correlation clustered by firm. Standard errors are adjusted for heteroskedasticity and clustered at the firm level. All variables are defined in Appendix 2.

Panel A – Ex-ante Overvaluation Proxies

	High Total Accruals		High Market-to-book		High Analyst Disp.	
	SHO Dummy	Interaction	SHO Dummy	Interaction	SHO Dummy	Interaction
CAPX	-0.38	-1.18[a]	0.01	-1.41[a]	-0.35	-0.52
	(-1.34)	(-2.12)	(0.04)	(-3.81)	(-1.23)	(-1.20)
Δ Total Assets	-3.27	-5.97	-2.73	-4.28	-1.26	-7.88[a]
	(-1.62)	(-1.55)	(-1.32)	(-1.40)	(-0.58)	(-2.80)
CAPXR&D	-0.53	-2.32[b]	-0.03	-2.59[a]	-0.54	-0.85
	(-1.16)	(-2.43)	(-0.06)	(-3.33)	(-1.23)	(-1.14)
Equity Issues	-0.14	-1.99[b]	-0.71	-2.27[c]	-0.48	-2.61[b]
	(-0.34)	(-2.09)	(-1.03)	(-1.70)	(-0.66)	(-2.28)
Debt Issues	-0.98	-1.38	-1.42	1.09	-0.69	-1.09
	(-0.76)	(-0.56)	(-1.00)	(0.59)	(-0.54)	(-0.59)

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Panel B – Stock Market Characteristics

	High Downside Beta		Low CAR		Trading Volume		High Short Interest	
	SHO Dummy	Interaction	SHO Dummy	Interaction	SHO Dummy	Interaction	SHO Dummy	Interaction
CAPX	-0.04 (-0.12)	-0.82[b] (-2.02)	-0.43 (-1.58)	-0.37 (-0.80)	-0.06 (-0.24)	-0.32[a] (-5.49)	-0.23 (-0.83)	-1.64[a] (-2.59)
Δ Total Assets	1.82 -0.71	-9.15[a] (-3.39)	-1.62 (-0.78)	-7.13[b] (-2.40)	-1.88 (-0.96)	-1.28[a] (-4.04)	-2.52 (-1.27)	-6.41[c] (-1.83)
CAPXR&D	0.2 -0.34	-1.55[b] (-2.38)	-0.49 (-1.09)	-0.82 (-1.07)	-0.17 (-0.38)	-0.37[a] (-2.62)	-0.28 (-0.65)	-2.39[b] (-2.43)
Equity Issues	-0.15 (-0.17)	-2.22[b] (-2.33)	-0.63 (-0.85)	-2.59[b] (-2.14)	-0.67 (-0.90)	-0.49[b] (-2.22)	-0.86 (-1.20)	-2.83[b] (-2.03)
Debt Issues	1.6 -0.79	-4.24[b] (-2.13)	-0.2 (-0.15)	-2.77[c] (-1.65)	-0.82 (-0.67)	-0.12 (-0.73)	-0.38 (-0.31)	-3.36[c] (-1.74)

c, b, a indicate a significance level of less than 10%, 5%, and 1% respectively.

Table 10

Bootstrapped Distribution of T-statistics for Randomized Samples

This table presents the distribution of t-stats of the OLS regressions with firm-fixed effects for the subset of small firms (firms with Total Assets below the sample median) when we randomize the selection of firms in the Pilot and Control Group using 5,000 simulations. The t-stats correspond to either the Pilot Group dummy variable in the long horizon abnormal returns analysis (column 1) as presented in Table 4 for the small firms, or the coefficient of the SHO dummy variables for the real variables analysis as presented in Table 8.

	Long-Run CAR one year	Capital Exp.	Changes in Total Assets	R&D	Equity Issues	Debt Issues
Percentiles	1	2	3	4	5	6
1%	-2.57	-2.25	-2.38	-2.20	-2.52	-1.93
5%	-1.78	-1.56	-1.71	-1.59	-1.93	-1.40
10%	-1.36	-1.23	-1.33	-1.27	-1.60	-1.09
50%	-0.02	-0.07	-0.14	-0.09	-0.40	0.06
90%	1.28	1.10	1.09	1.07	0.68	1.24
95%	1.67	1.43	1.39	1.41	0.99	1.58
99%	2.34	1.95	1.98	1.94	1.67	2.22
Coefficient	Diff.-in-Diff.	SHO Pilot Dummy	SHO Pilot Dummy	SHO Pilot Dummy	SHO Pilot Dummy	SHO Pilot Dummy
Location	Table 4	Table 8.1	Table 8.3	Table 8.5	Table 8.7	Table 8.9
Reported T-stat	(-4.08)	(-2.25)	(-1.73)	(-1.72)	(-3.03)	(-1.34)
Significance level	1%	1%	5%	5%	1%	10%