

**Pre-Analysis Plan for  
The REG SHO Reanalysis Project**

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*This pre-analysis plan can be downloaded without charge from SSRN at:  
<http://ssrn.com/abstract=3415529>*

## Abstract

This is a pre-analysis plan for *The Reg SHO Reanalysis Project*, in which we will reassess primary results from selected recent accounting and finance papers, which exploit the randomized trial conducted by the Securities and Exchange Commission (SEC) from May 2, 2005 to July 6, 2007. As part of this trial, the SEC selected every third stock on the Russell 3000 index, based on trading volume during the year before the experiment was announced, and exempted these firms' shares from the NYSE short-sale uptick rule and the Nasdaq bid test. Early studies of the experiment found minor effects on trading markets (e.g., SEC Office of Economic Analysis (2007), Diether et al (2009)), and led the SEC to rescind these short-sale restrictions. However, more recently, a large body of research has reported evidence for a wide variety of outcomes from the experiment, including increased open short interest, negative returns, reduced investment, reduced earnings management, increased audit fees, and much more for the Pilot firms. We undertook this project because we viewed most of these outcomes as implausible, given that by 2005, the uptick rule and the bid test had little impact on the ability of short sellers (fundamentals-based) to sell short, limited publicity about the experiment (with no opposition from the affected firms that we could find), and evidence from early research that found very minor effects on trading markets. In this proposed project, we will reassess three prominent, recent studies: Fang, Huang and Karpoff (2016), Grullon, Michenaud and Weston (2015), and Hope, Hu, and Zhao (2017). This pre-analysis plan provides details on our sample selection and the specific analyses we plan to carry out for the main results found in these three papers.

## Introduction

This Pre-Analysis Plan was completed and posted to SSRN in July, 2019. It forms the basis for reassessing the principal results from selected articles studying the effects of the short-sale experiment conducted by the Securities and Exchange Commission (SEC) during 2005-2007 under the SEC's rules regulating short sales, known as Regulation SHO.

In July 2004 the SEC announced a pilot program that temporarily suspended the short sale price tests for an effectively random sample of approximately 1000 ("pilot" or "fully treated") firms in the Russell 3000 Index (below, R3000), leaving some but not all of the prior restrictions in place for the remaining firms (original controls).<sup>1</sup> Specifically, the SEC suspended the NYSE uptick rule and the similar Nasdaq bid test for pilot firms during the experiment period (May 2, 2005 to July 6, 2007). Under NYSE Rule 440B, a short sale was permitted only following a plus tick or a zero-plus tick ("uptick rule") and under Nasdaq Rule 3350, short sales in National Market Securities were not allowed at or below the bid when the current bid was at or below the previous bid ("bid test").<sup>2</sup>

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<sup>1</sup> Securities Exchange Act Release 34-50104 (July 28, 2004). The SEC conducted, in effect, a block randomized experiment, in which within each trading market (NYSE, AMEX, and Nasdaq national market), it ranked the R3000 firms by trading volume over June 2003 through May 2004, and chose every third firm (the 2nd, 5th, 8th, etc. on the within-market list) to be fully treated. See OEA (2007).

<sup>2</sup> Below, we generally refer to both the NYSE and Nasdaq rules as the "uptick rule." Where we want to distinguish between the two rules, we call them the "NYSE uptick rule" and the "Nasdaq bid test."

Initial studies of the experiment found little impact of removing short-sale restrictions on returns, the level of open short interest (below, simply “short interest”) or market quality measures. The SEC’s Office of Economic Analysis (OEA, 2007) examined share returns, short interest, short sale volume, and volatility for the pilot and the control firms. The OEA found that relaxing the short-sale constraints did not have a significant impact on returns or short interest for the treatment (pilot) firms but the treated firms did experience an increase in short volume. Diether et al. (2009) also studied the effect of the short-sale experiment on market quality, and found higher short-sale volume, but no significant effect on daily returns or volatility. Overall, the primary takeaway from these early studies is that removing short-sale restrictions did not significantly affect returns, short interest or volatility. The SEC relied on these early studies to remove the short-sale restrictions for all firms when the experiment ended in 2007.

In the years since this experiment however, a large number of finance and accounting papers report evidence that suspension of short sale restrictions had wide ranging direct and indirect effects on pilot firms. For example, these studies report that the pilot program affected share returns, short interest, earnings management, research and development, investments, capital expenditures, acquisition activity, management compensation, audit fees, and more. Appendix 1 lists over 30 papers we know of (as of July 2019) and summarizes their principal findings.

We find many of the outcomes reported in these studies to be implausible. As explained in Diether et al. (2009) and Alexander and Peterson (2009), suspension of the NYSE uptick rule was expected to have a modest impact on short selling. With a tick size of a penny, heavy trading volume, including heavy arbitrage trading (which will often generate upticks unrelated to fundamentals) and alternative ways to short shares (including regional exchanges, electronic exchanges, and options markets), the uptick rule was widely believed to have little remaining effect on valuation based (fundamentals based) short selling, but to provide a real although modest impediment to arbitrage-related trading. For Nasdaq firms, any impact of suspension of short-sale restrictions was expected to be even smaller, because the Nasdaq bid test was less restrictive than the NYSE uptick rule. Our conversations with practitioners and former regulators suggested that by 2004, the NYSE and Nasdaq restrictions tests had no meaningful effect on valuation-based short sellers’ ability to take short positions. The early studies were consistent with the expectation that removing the short-sale restrictions would facilitate arbitrage-related short selling but have few other meaningful economic effects.

There are further reasons to expect limited firm responses. One is the experiment’s limited duration: As announced, the experiment was to last only one year, roughly calendar 2005. The SEC later delayed the start date to give the exchanges time to make software changes to implement it, and then, near the end of the experiment year, extended the experiment for a second year to avoid changing the uptick rule while the SEC was considering repeal.<sup>3</sup> One should ask: Which effects found in recent papers are plausible reactions to a one-year experiment, especially one that was expected to have modest effects on trading markets to begin with?

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<sup>3</sup> The one-year period, to begin January 3, 2005, is stated in Securities Exchange Act Release 34-50104 (July 28, 2004). The experiment, after a delayed launch on May 2, 2005, was set to expire on 28 April 2006, but the SEC instead extended it until mid-2007, in an announcement issued on April 21, 2006, only a week before the initial expiration date. The SEC explained that “the extension will spare markets the expense of changing computer systems to restore restrictions on stocks in the pilot while [the SEC] analyzes results.” SEC to Extend Test on Short Sale Rules, *Wall Street Journal* (April 22, 2006).

A second is the minimal press attention given to the experiment, with no evidence of opposition from the affected firms' managers. The SEC's announcement on June 23, 2004, of its plans to conduct the experiment, and the formal experiment announcement on July 28, 2004, were not covered in any of the standard business news sources. Between announcement and experiment launch in May 2005, we found only a few technical explanations of how the experiment would work. The experiment launch was noted only in a short *Dow Jones News Service* story shortly before launch, with a *Wall Street Journal* summary the next day. As a December 2006 *New York Times* story on the SEC's plans to repeal the short-sale rule explained, "You may not have read of this proposal. It was virtually ignored by the news media, and if any companies are upset about it, they have not made themselves known." Appendix 2 summarizes the news coverage we found.<sup>4</sup>

Third, many recent papers also study indirect effects of the short-sale experiment. However, the longer the causal chain linking relaxation of short-sale restrictions to a firm outcome, and the less plausible it will be that each link in the chain exists.

Given the modest expected experiment effects, and early studies confirming modest direct effects on trading markets, we consider it likely that many of the more recent findings reflect specification searches, and will not be robust, if re-examined. In this project, we propose to reassess the principal results from three prominent recent studies.

Grullon, Gustavo, Sebastien Michenaud, and James Weston (2015), The Real Effects of Short-Selling Constraints, 28 *Review of Financial Studies* 1737-1767 ("GMW").

Finding that the treated firms reduce investment (both capital expenditures and R&D).

Fang, Vivien W., Allen Huang, and Jonathan Karpoff (2016), "Short Selling and Earnings Management: A Controlled Experiment", 71 *Journal of Finance* 1251-1293 ("FHK").

Finding that the treated firms have more negative discretionary accruals during the experiment period.

Hope, Ole-Kristian, Danqi Hu, and Wuyang Zhao (2017), Third-Party Consequences of Short-Selling Threats: The Case of Auditor Behavior, 63 *Journal of Accounting and Economics* 479-498. ("HHZ").

Finding that auditors of the treated firms increase their audit fees.

We chose these studies because they are recent and highly placed, and also because we were skeptical about their findings, based on Desai's discussion of FHK at the 2014 CEAR conference at Georgia State University. (ii) Litvak's discussion of HHZ at the 2016 PCAOB JAR Conference in Washington D.C. and Litvak, Black and Yoo's (2019) examination of short interest and share price reactions for treated versus control firms (also studied by GMW).<sup>5</sup>

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<sup>4</sup> Fang, Huang and Karpoff (2016), at 1255 assert that there was a "huge backlash" against repeal of the short-sale rule. However, support for reinstating short-sale restrictions emerged only in late 2008, during the 2008-2009 financial crisis. We found no evidence of any opposition during the period from experiment announcement to launch, or during the experiment period.

<sup>5</sup> Litvak, Black and Yoo (2019) find no support for, the GMW findings of an increase in open short interest for treated firms during the period from announcement of the experiment to its launch and a decline in share prices for treated firms before the experiment was announced.

Our first goal in this project is to assess the robustness of the findings in these papers. Any one project can examine only a subset of those findings, but the robustness (or lack thereof) of the re-examined findings will be informative as to the likely robustness of dozens of other studies exploiting this experiment.

A second goal is to provide an example of careful (or so we hope) use of a pre-analysis plan in an observational study. Use of such plans is increasing in many disciplines, and was featured in the 2017 *Journal of Accounting Research* conference and the resulting 2018 special issue of that journal. We pre-registered this research design for several reasons. One, we hope to avoid the claim that we engaged in our own specification searches to support our prior skepticism about the results of recent studies of the short-sale experiment. Two, we see large value, in an observational study, of researchers imposing on themselves the discipline of defining in advance their sample, their sample period, their covariates, and the tests they will run, with as much precision as they can muster; and then documenting any later departures from the initial plan. We were repeatedly surprised, in doing so, how hard it can be to do so with precision, and how many choices must be made along the way. Third, we hope this research design can provide guidance on how a randomized trial can be studied, and on the value of simpler designs, which rely on the randomization to ensure that one's estimates are unbiased.

### **Development of Research Design**

The research designs below were developed in the design phase of the project. The research designs were chosen before running any analyses, without using outcome variables (except to assess pre-treatment covariate balance on the outcomes), with the following limited exceptions.

Desai and Yu ran selected analyses of accruals in connection with Desai's comments on Fang, Huang and Karpoff in 2014 at the CEAR Symposium on Corporate Control Mechanisms and Risk at Georgia State University.

Litvak, Black, and Yoo (2019) used a draft version of the 2004 Analysis Sample to assess the effect of the short-sale experiment on short interest and share price reaction.

This research design was developed without returning to those analyses.

### **Research plan elements common to all replications**

#### **Three Separate Experiments, plus a Mixed Experiment.**

As discussed in Litvak, Black, and Yoo (2019), the SEC changed its original experiment, in ways which compromised the original experiment design, but created randomized sub-experiments which can be studied separately. The SEC first randomized the R3000 Index into fully treated firms (one-third of the R3000) and "original controls" (the remaining two-thirds of the R3000). Then, for the "large" firms (those in the Russell 1000 Index, R1000 henceforth), while the SEC suspended the uptick rule for the treated firms both during and after trading hours, it also suspended the uptick rule *after trading hours* for the original control firms. Thus, the control firms in R1000 can be considered "partly treated" as short sale restrictions for these firms remained in place only during trading hours.

For the smaller firms, those in the Russell 2000 Index (below, R2000), the SEC left in place the NYSE and Nasdaq constraints on short-selling for the original control firms (uptick rule and the bid test, respectively). However, the NYSE applied its uptick rule during and after trading hours,

while Nasdaq applied its bid test only during trading hours.<sup>6</sup> In addition, the SEC suspended the uptick rule for all NYSE firms from 8:00 p.m. on each trading day until 8:00 a.m. on the next trading day.<sup>7</sup> This reduced the difference between treated and control firms, at least for valuation-based (as opposed to arbitrage) short-selling, since short-selling can occur either before or after trading hours (Alldredge, Blau, and Brough, 2012).

Taking into account both the differences between the NYSE uptick rule and the Nasdaq bid test (which the experiment left in place), and the difference between the rules that applied to R1000 versus R2000 firms, one can divide the R3000 firms into four sub-experiments, as shown in Table 1.

**Table 1. SEC Experiment Rules: NYSE vs. Nasdaq and R1000 vs. R2000 Firms**

Table 1 indicates which short sale restrictions applied to the original control firms in the SEC experiment, based on (i) NYSE vs. Nasdaq listing; and (ii) membership in R1000 vs. R2000.

Large NYSE Firms (R1000)		Large Nasdaq Firms (R1000)	
<b>Fully Treated</b> no restrictions	<b>Control</b> partly treated (uptick rule during regular trading hours)	<b>Fully Treated</b> no restrictions	<b>Control</b> partly treated (bid test during regular trading hours)
Small NYSE Firms (R2000)		Small Nasdaq Firms (R2000)	
<b>Fully Treated</b> no restrictions	<b>Control</b> full control (uptick rule from 8:00 am-8:00 pm on trading days)	<b>Treated</b> no restrictions	<b>Control</b> partly treated (bid test during regular trading hours)

In effect, for R1000 Firms, the SEC experiment compared “fully treated” large firms (no uptick rule) to partly treated large firms (restrictions during trading hours), with a further division into NYSE and Nasdaq firms. For R2000 NYSE firms, the SEC ran something close to the experiment everyone thought it ran, which compared fully treated NYSE firms to full control NYSE firms; we call this the *Small Firm NYSE Experiment*. For R2000 Nasdaq firms, the SEC effectively ran a different experiment, which compared fully treated Nasdaq firms to partly treated Nasdaq firms (bid test during regular trading hours), which we call the *Small Firm Nasdaq Experiment*. To respect the original randomization, and the basis for causal inference it provides, one should study each of these four sub-experiments separately. To combine them, as most prior studies have done, assumes that there is no difference, from the perspective of valuation-based short-sellers (who seek to profit from a share price decline based on fundamentals), between partly treated and full control firms. In effect, this assumes that after-hours short-selling is infeasible, when we know otherwise. To combine NYSE and Nasdaq firms, as many recent studies have done, assumes there is no difference between the uptick rule and the bid test, though it is known that the bid test was far less restrictive than the uptick rule and that that short sale restrictions were not as effective on Nasdaq due to the fact that electronic exchanges accounted for a significant trading volume for Nasdaq stocks and that these venues did not impose the bid test (Diether et al, 2009).

<sup>6</sup> We combine American Stock Exchange and NYSE firms, because the NYSE and American Stock Exchange used similar uptick rules. We refer to both of them as “NYSE firms” for brevity.

<sup>7</sup> More technically, the SEC suspended the uptick rule for periods when the consolidated transaction reporting “tape” was not operating. At the time of the experiment, this tape operated from 8:00 a.m. to 8:00 p.m. on trading days.

For large Nasdaq firms, there are several additional possibilities, each imperfect. First, one could combine the two large-firm groups into a single *Large Firm Experiment*, at the cost of assuming that the uptick rule (plus additional opportunities to trade without regard to this rule, including trading options and trading on regional exchanges) was similar in practical effect to the Nasdaq bid test (plus opportunities to trade options or trade on alternative electronic exchanges); one could call this the *Large Firm Experiment*.<sup>8</sup> Second, one could combine large and small Nasdaq firms into a single *Nasdaq Experiment*. This would make the implicit assumption, that rule-avoidance opportunities were similar for both groups. Yet, for value-based short selling, it was likely easier to avoid the bid test for larger firms by trading options or trading on electronic exchanges. Third, one could leave the large Nasdaq firms in their own experiment, at the cost of a small sample size and thus limited statistical power.<sup>9</sup>

In the analyses described below, we will study these four sub-experiments separately. However, for greater comparability with prior work, much of which includes all R3000 firms, and ignores the difference between partly treated and full control firms, we will also study all R3000 firms; we call this the *Mixed Experiment*. In robustness checks, we will assess whether any results would change if we combined large NYSE and Nasdaq firms, or combined large and small Nasdaq firms.

### Sample Selection

The SEC announced the experiment on July 28, 2004 and started the experiment on May 2, 2005. Since the composition of R3000 changes every year there were firms in R3000 in July 2004 that were not in R3000 in May 2005 and vice versa. Moreover, GMW conduct some of their analysis around the 2004 announcement of the experiment while FHK and HHZ study only the 2005 the implementation of the experiment. We therefore generate two main samples:

*2004 Analysis Sample:* Sample as of the July 28, 2004 (when the SEC announced the short-sale experiment). Used only to address effects of experiment announcement on short interest and share returns.

*2005 Financial Analysis Sample:* Subsample of the 2004 Analysis Sample, as of May 2, 2005 (when the experiment launched), excludes financial firms and utilities. GMW, FHK, and HHZ also exclude financial firms and utilities.<sup>10</sup>

We rely on the 2005 Financial Analysis Sample unless otherwise specified. Appendix 3 provides details on how we generate these samples. As detailed in this Appendix, we engaged in careful sample selection and matching across datasets, seeking to preserve as much of the original experiment sample as we could.

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<sup>8</sup> Litvak, Black and Yoo (2019) adopt this approach.

<sup>9</sup> The 2004 Analysis Sample (defined below) includes 78 fully treated and 144 partly treated large Nasdaq firms.

<sup>10</sup> GMW also exclude “stocks with prices below \$1, pink sheet and bulletin board stocks, closed-end mutual funds, limited partnerships, royalty trusts, foreign stocks, and American Depositary Receipts (ADRs),” and keep only firms that were in the R3000 in both June 2004 and June 2005. The 2004 Analysis Sample includes only two firms in the R3000 with share prices below \$1; we exclude these firms when studying share returns. HHZ also require firms to be in the R3000 in June 2005 (they lose 387 firms due to this restriction), to have Audit Analytics data (lose 90 firms) and non-missing data for all control variables (lose 199 firms). FHK do not require firms to be in the R3000 in June 2005, but their main analyses are based on the balanced panel sample where they require that firms have data to calculate firm characteristics over the entire sample period (2001-2003 and 2005-2010).

As discussed above, for each of the above two samples, we generate five samples (Mixed Experiment plus four sub-samples): Large-Firm NYSE, Small-Firm NYSE, Small-Firm Nasdaq, Large-Firm Nasdaq and Mixed Experiment. Table 2 shows sample sizes for each subsample, within the 2005 Financial Analysis Sample. The Mixed Experiment sample is slightly larger than the combined sample size of the separate experiments because for each experiment, we enforce common support on  $\ln(\text{market capitalization})$ , and therefore lose a small number of firms from each sub-experiment.

Note that the full control firms are only about one-fourth of all original control (partly treated or full control) firms. If partial treatment is closer to full treatment than to full control (an empirical question), this would provide further reason to expect weak results from the Mixed Experiment.

**Table 2. Sample Sizes within the 2005 Financial Analysis Sample**

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Total (all 4 experiments)	Mixed Experiment
Fully treated (pilot) firms	180	288	176	58	702	702
Partly treated firms	0	602	316	128	1,046	1,054
Full control firms	353	0	0	0	353	359
<b>Subtotal: original control firms</b>					<b>1,399</b>	<b>1,413</b>
<b>Total</b>	<b>533</b>	<b>890</b>	<b>492</b>	<b>186</b>	<b>2,101</b>	<b>2,115</b>

**Balance on Covariates and Outcomes; Parallel Trends.** In Appendix 3, we confirm pre-treatment balance on covariates and outcomes for each experiment though to conserve space we report the statistics for the Mixed Experiment and for the Small-firm NYSE experiment. We also confirm that the pre-treatment trends are reasonably parallel for covariates and outcomes over 1999-2004, for the 2005 Financial Analysis Sample. Balance and parallel trends are expected given the underlying randomization, but should be confirmed as part of good research design.<sup>11</sup>

**Sample Attrition:** In Appendix 3, we check for possible differential attrition and find no evidence that fully treated firms leave the sample at a rate different than partly treated or full control firms.

**Sample sizes:** Our sample sizes are larger than the papers we reassess, sometimes substantially so. This partly reflects the substantial care we took to preserve sample size, but also reflects design choices. Table 3 shows total sample sizes, across all three experiments. There are two primary reasons why our sample is larger.

First, the SEC used the R3000 list as of June 25, 2004, which in turn is based on market capitalization as of May 28, 2004. GMW and HHZ require that firms be in R3000 in both mid-2004 and mid-2005 (the experiment started in early May 2005).<sup>12</sup> The SEC, however, used only the June 2004 list and did not adjust the set of treated and control firms based on continued membership in the R3000 after that. The practical effect of this restriction is to exclude several firms with poor returns between June 2004 and the launch of the experiment in May 2005. Second, all three papers exclude firms with missing data on covariates resulting in a loss of about 200 firms. In contrast, we keep these firms. As discussed below, we prefer regressions without covariates.

<sup>11</sup> Assessing pre-treatment “covariate balance,” often including balance on pre-treatment outcomes, is standard for randomized experiments. Assessing parallel pre-treatment trends is not customary, perhaps because that literature largely concerns cross-sectional results, but is a natural experiment if one will use panel data and a DiD design.

<sup>12</sup> GMW construct their own R3000 lists based on market capitalization at May 28, 2004 and May 31, 2005. HHZ use the R3000 lists as of June 2004 and June 2005.



**Table 3. Comparison of Sample Sizes**

Number of firms in our Mixed Experiment sample and the samples used by FHK, GMW, and HHZ. GMW nominal sample is stated in their Part I. GMW Effective Sample is estimated based on ratio of number of observations reported in regressions in Table 6, to total number of observations stated in Part 1.

Sample	fully treated (pilot)	original controls (partly treated or full control)	total
Our 2004 Analysis Sample	985	1,969	2,954
Our 2005 Financial Analysis Sample	702	1,413	2,115
Our 2001-2010 Balanced Sample	517	947	1,464
FHK (Balanced) Sample	388	709	1,097
GMW Nominal Sample	651	1,279	1,930
GMW Effective Sample	574	1,128	1,702
HHZ Sample	538	1072	1,610

## Research Design and Methodology

Our core research strategy was to assess how *we* would design a study to answer the principal research questions in these papers. In a number of cases this leads us to different, often simpler specifications than the authors used in these papers. For example, our specifications rely more heavily on the initial randomization as a basis for covariate balance. This allows simpler specifications, such as a simple comparison of means, rather than coefficient from a regression with time-varying, firm-level covariates. Though to allow greater comparability, we also run the specifications used in these papers.

### Time periods

**Time periods.** The SEC experiment ran from May 2, 2005-July 6, 2007. It is a judgment call whether to use quarterly or semi-annual data which better matches the experiment timing, or annual data, which is audited and hence more reliable. We choose to use annual data for all outcomes; this is also the choice made in the three papers we reassess.

**Experiment Period.** We treat firm fiscal years which include at least 6 experiment months as *during* the experiment period: this is fiscal years ending October 2005 through December 2007. All others fiscal year-ends are pre- or post-experiment.

**Sample periods.** We use 2001-2010 as our main sample period. We define which firm fiscal years are in the *sample* period using the Compustat convention, under which, if the fiscal year-end month is January-May, the fiscal year is the current calendar year minus 1 year; and if the fiscal year-end month falls in June through December, the fiscal year is the current calendar year. Thus, our sample period includes fiscal year ends from June 2001 through May 2011. Our pre-period thus includes firm fiscal years ending June 2001 through September 2005; our post-period includes firm fiscal years ending January 2008 through May 2011. In robustness checks, for greater comparability to FHK, GMW, and HHZ, we (i) exclude fiscal years ending October 2004 through September 2005 from the pre-period; and (ii) use the FHK pre, during, and post experiment periods. In regressions that use financial covariates, we use the Compustat convention to assign fiscal years to calendar years.

Table 4 compares our definition of the pre-period, the experiment period, and the post-period to those used by FHK, GMW, and HHZ. Appendix 4 provides additional details on these periods.

**Table 4. Definition of Sample Period and Experiment Period**

Table shows fiscal-year month ends for our pre- experiment, experiment, and post periods, FHK periods, and our best guesses as to the periods used by GMW and HHZ. GMW do not directly state what sample period they used, but appear to use an 8-year period, from comparing their number of observations to number of firms, apparently fiscal years 2001-2008. They effectively drop post-experiment observations by setting treatment dummy =1 for all firms in this period. HHZ sample period is fiscal years 2000-2013 (see their Table 1). They refer elsewhere only to “year,” without specifying the correspondence between fiscal years and years. This table assumes that HHZ use the Compustat convention. See Appendix 4 for the specific statements made in each paper about sample and experiment periods.

Period	Pre	Dropped	Experiment	Dropped	Post
Ours	06.2001 – 09.2005	none	10.2005 – 12.2007	None	01.2008 – 05.2011
FHK	01.2001 – 12.2003	01.2004 – 12.2004	01.2005 – 12.2007	None	01.2008 – 12.2010
FHK robustness 1	06.2001 – 06.2003	07.2003 – 04.2005	05.2005 – 06.2007	07.2007- 04.2008	05.2008 – 06.2010
FHK robustness 2	01.2001- 12.2004	none	01.2005 – 12.2007	None	01.2008 – 12.2010
GMW	01.28.2002 – 01.27.2005	None	01.28.2005 – 01.06.2008	01.07.2008- 05.2009	None
HHZ	06.2000 – 05.2004	06.2004 – 05.2005	06.2005 – 05.2008	None	06.2008 – 05.2014
HHZ robustness	06.2000 – 05.2004	06.2004 – 05.2005	06.2005 – 05.2008	None	06.2008 – 05.2011

**Effect timing and DiD specifications.** We are agnostic about whether firm responses to the experiment would occur promptly or with a lag. We will therefore use both a simple DiD model, which assumes a one-time change at the onset of the experiment, and a leads-and-lags specification, which allows an effect to appear gradually during the experiment period.

Any treatment effect should be reversed after the end of the experiment, when the SEC removed short-sale restrictions for all firms. Thus, if an effect is found during the experiment period, we can look for reversal after the experiment period. FHK and HHZ look for evidence of reversal; GMW do not.

### Regression specifications

Unless otherwise specified, all panel regressions include year and firm fixed effects, and standard errors clustered on firm.

**Firm fixed effects (firm FE).** We will use firm FE in all regressions. This is a standard DiD specification. Since we begin with a randomized experiment, omitting firm FE should not cause bias, but adding them should not introduce bias, can address chance imbalances between treated and control firms, and can potentially enhance precision, despite using up a degree of freedom for each firm. GMW and HHZ use firm FE; FHK do not.

We confirmed, in a test regression using an outcome variable that we do not otherwise plan to study (annual % sales growth), that including firm FE produces a modest gain in precision. We therefore consider firm FE to be a preferred specification, but will confirm in robustness checks whether we obtain similar results for the FHK outcomes if we omit firm FE.

**Year fixed effects (year FE).** We include year FE in all regressions, as do GMW. FHK and HHZ use cruder “Pre”, “During”, and “Post” dummy variables; this is a nonstandard choice for panel data (FHK do use year FE in one specification).

**Firm clusters:** Clustering standard errors on firm is standard when using panel data and time-persistent financial variables; GMW and HHZ also do so. FHK report that they use standard errors clustered “by year and firm.” This can be problematic given the short time dimension.<sup>13</sup>

### **FHK: Impact of Short-Sale Restrictions on Earnings Management**

Prior research (Desai et al, 2006, Karpoff and Lou, 2010) have shown that short interest is higher for firms with poor earnings quality. For firms that restate earnings, Desai et al show that short interest increases in the months leading up to the restatement announcement and that the increase in short interest is more pronounced for firms with high operating and total accruals, suggesting that short sellers appear to target firms with poor earnings quality, *ceteris paribus*.

Based on the above, FHK argue that the Pilot Program represents an ideal setting to test the impact of short selling on corporate financial reporting. The Pilot Program reduced short selling constraints for a sample of pilot firms, presumably making it easier to short pilot firms. They test whether pilot firms reduce earnings management relative to control firms during the experiment period, and whether the difference in earnings management disappears after the experiment ends.

FHK use performance matched discretionary accruals (PMDA) as their measure of earnings management. They report that pilot firms have more negative discretionary accruals than control firms during the experiment period. This differential disappears after the experiment ends (when the short-sale restriction is removed for all firms).

We revisit their hypothesis. We use both their PMDA measure and two simpler measures of earnings management. Our first measure is operating accruals. Following Healy (1985) and Sloan (1996), most prior research has focused on operating accruals. However, Richardson et al. (2004) show that investing accruals, from capital investments in physical or intangible assets, are associated with lower earnings persistence and are significantly mispriced. Therefore, we also consider total accruals, which includes both operating and investing accruals. These two measures are defined as follows:

$$TOTACC = (Earnings - CFO - CFI) / LagAssets$$

$$OPACC = (Earnings - CFO) / LagAssets$$

where Earnings is earnings before extraordinary items from the statement of cash flows (Compustat annual data item IBC), and CFO and CFI are cash flow from operations and cash flow from investing activities, from the statement of cash flows (OANCF minus XIDOC, and IVNCF, respectively). LagAssets is the book value of total assets (AT) at the end of the prior year. Following Hribar and Nichols, (2007), we winsorize accruals at 1%/99% to reduce the influence of

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<sup>13</sup> We are not sure what this means. FHK likely used two-way clustering, separately on firm and year. We prefer to cluster only on firm due to the short time dimension of our study. With one-way clustering, clustered standard errors with a small number of clusters are downward biased (e.g., Cameron, Gelbach and Miller, 2008). A similar concern applies to the second clustering dimension. We are aware of only limited research, principally Kezdi (2004), on the importance of this potential bias for the second clustering dimension (here, by year) if one has a large number of clusters for the first clustering dimension (here, by firm).

outliers and address data entry errors in Compustat. All regressions use standard errors clustered on firm.

We prefer these simple measures of accruals for several reasons. First, given the random assignment of firms to pilot versus control, there is no reason to expect systematic differences in the two groups for firm performance or other characteristics prior to the experiment period. Below, we confirm the similarity of pilot and control firms on a broad array of characteristics, including operating and total accruals. Thus, one does not need to use PMDA to address imbalances between pilot and control firms. Second, PMDA has less power than simpler measures to detect earnings management (Dechow, Ge and Schrand, 2010, Dechow, Ge, Larson and Sloan, 2011)). Thus, our simpler measures should have greater power to detect an effect of the short-sale experiment, if one exists. Third, the papers showing that short selling increases prior to restatements, which provide a principal motivation for the FHK study, use these simpler measures.

However, we also study PMDA to allow direct comparison of our results to FHK. To facilitate comparison, we closely follow FHK (except that we add firm fixed effects) and first estimate operating accruals cross-sectionally within each fiscal year and Fama-French 48 industry:<sup>14</sup>

$$\frac{TA_{i,t}}{ASSET_{i,t-1}} = \beta_0 + \beta_1 \frac{1}{ASSET_{i,t-1}} + \beta_2 \frac{\Delta REV_{i,t}}{ASSET_{i,t-1}} + \beta_3 \frac{PPE_{i,t}}{ASSET_{i,t-1}} + \varepsilon_{i,t} \quad (1)$$

where  $i$  indexes firms and  $t$  indexes fiscal years.  $TA_t$  is earnings before extraordinary items and discontinued operations (IBC) minus operating cash flows (OANCF minus XIDOC) for year  $t$ , winsorized at 1%/99%.  $ASSET_{t-1}$  is total assets (AT) at the end of fiscal year  $t-1$ .  $\Delta REV_t$  is the change in sales revenue (REVT) from year  $t-1$  to  $t$ .  $PPE_t$  is gross property, plant and equipment (PPEGT) at the end of year  $t$ . We follow FHK and require at least 10 observations in an industry-year to perform each cross-sectional estimation. Next, following FHK, we use the estimated coefficients from this model to calculate the normal accruals  $NA_{i,t}$ :

$$NA_{i,t} = \widehat{\beta}_0 + \widehat{\beta}_1 \frac{1}{ASSET_{i,t-1}} + \widehat{\beta}_2 \frac{(\Delta REV_{i,t} - \Delta AR_{i,t})}{ASSET_{i,t-1}} + \widehat{\beta}_3 \frac{PPE_{i,t}}{ASSET_{i,t-1}} + \varepsilon_{i,t} \quad (2)$$

where  $\Delta AR_{i,t}$  is the change in accounts receivables (ARC). We then calculate firm-year-specific abnormal accruals as  $AA_{i,t} = TA_{i,t} / ASSET_{i,t-1} - NA_{i,t}$ . Following FHK, we then match each firm-year observation with another observation from the same year and industry which is closest for same-year return on assets ( $ROA_t$  defined as net income [NI] divided by total assets [TA], for year  $t$ ).<sup>15</sup> Finally, we calculate PMDA for each firm-year as the firm-year discretionary accrual minus the discretionary accrual for the matched firm-year.<sup>16</sup>

<sup>14</sup> Following FHK and Kothari et al. (2005), we require a minimum of 10 firms in an industry-year in order to estimate normal accruals. FHK estimate PMDA using the modified Jones model of Dechow et al. (1995). Kothari et al. (2005) criticize use of this model for cross-sectional (as opposed to time series) research, and provide simulation evidence that the unmodified Jones model is a better basis for computing PMDA. In our setting, with good covariate balance between treated and control firms, there is little reason to prefer one specification over the other, so we follow FHK.

<sup>15</sup> While we will follow FHK (and Kothari et al.) in matching on  $ROA_t$ , we note that this is a clear error in a “causal” project. Since accruals affect  $ROA$ , one should match only on pre-treatment values of  $ROA$ . Thus, in our view, matches should be determined in 2004, and not changed after that. If we find significant results for PMDA with matching on contemporaneous  $ROA$ , we will assess robustness to matching instead on pre-treatment values.

<sup>16</sup> Following Hribar and Nichols (2007), we winsorize the variables that enter the regressions (equation 1 and 2) used to estimate the discretionary accruals at 1%/99%, compute PMDA, and winsorize again. FHK apparently winsorize

**DiD specification:** To test whether pilot firms had lower accruals than control firms during the experiment period, we estimate the following DiD model for each of the three accrual measures over 2001-2010.

$$y_{i,t} = \beta_0 + \gamma_t + f_i + \beta_1 \text{Pilot}_i * \text{During}_t + \beta_2 \text{Pilot}_i * \text{Post}_t + \varepsilon_{i,t} \quad (3)$$

Here  $y_{i,t}$  is the accruals measure;  $\text{Pilot}_i = 1$  for fully treated firms and 0 for controls (full controls, partly treated firms, or original controls, depending on which experiment is being studied);  $\text{During} = 1$  during the experiment period (2005-2007), 0 otherwise;  $\text{Post} = 1$  during the first three post-experiment years (2008-2010), after the SEC removed short-sale restrictions for all firms, the  $\gamma_t$  are year fixed effects (FE) and the  $f_i$  are firm FE. A negative coefficient  $\beta_1$  on  $\text{Pilot} * \text{During}$  provides evidence for a treatment effect -- that pilot firms reduce “earnings management,” relative to control firms, during the experiment period. In contrast, the coefficient  $\beta_2$  on  $\text{Pilot} * \text{Post}$  should be zero because the short-sale restrictions were removed for all firms following the experiment. We can also directly test for a sign reversal in the Post period, relative to the experiment period, by replacing  $\text{Pilot} * \text{During}$  with  $(\text{Pilot} * (\text{During or Post}))$  in eqn. (3). With this specification, FHK predict a positive sign for the coefficient on  $\text{Pilot} * \text{Post}$ .

$$y_{i,t} = \beta_0 + \gamma_t + f_i + \beta_1 \text{Pilot}_i * (\text{During or Post})_t + \beta_2 \text{Pilot}_i * \text{Post}_t + \varepsilon_{i,t} \quad (4)$$

**Leads-and-Lags Specification:** Both to assess whether pre-treatment trends are parallel, and to allow for the possibility that the treatment effect emerges gradually during the treatment period, we also use a “leads-and-lags” specification, in which we estimate a separate “treatment effect” for each year, before, during, and after the experiment period, and then plot the annual coefficients and 95% confidence intervals (CIs) in leads-and-lags graphs. The specification is:

$$y_{it} = \gamma_t + f_i + \sum_{k=1999}^{2010} (\beta^k * D_i^k) + \varepsilon_{it} \quad (5)$$

$D_i^k = 1$  for treated firms in year  $k$ , and 0 otherwise. Thus, in contrast to  $\text{Pilot}_i * \text{During}_t$  in eqn. (3), which remains on for treated firms for the entire experiment period, the  $D_i^k$ 's turn on for treated firms only for one specific year, then off again. Therefore,  $\beta^{2005}$  provides the estimated effect for 2005 (first experiment year),  $\beta^{2006}$  provides the effect for 2006, and so on. We adjust the  $\beta^k$  coefficients by subtracting  $\beta^{2002}$  (three years before the experiment starts) from each, so that the reported  $\beta^{2002} = 0$ .<sup>17</sup>

**Firm and year fixed effects:** FHK do not use firm FE, and use year FE only in one specification (in the others, they include *During* and *Post* dummies). We will use firm and year FE in all regressions.

**Covariates:** FHK include the following covariates in their regressions:  $\ln(\text{assets})$ ; ROA; leverage (total debt/assets);<sup>18</sup> and market-to-book ratio, all winsorized at 1%/99%.<sup>19</sup> We prefer a

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the variables that enter the accruals regressions (see their Table 1). They do not state whether they winsorize PMDA, but likely do so because they say that they follow Kothari et al. (2005), who do so.

<sup>17</sup> We will also plot annual univariate mean outcomes, separately for treated and control firms.

<sup>18</sup> Total debt is long-term debt plus the current portion of long-term debt.

<sup>19</sup> The “note” to FHK’s summary statistics table (Table I) states “All variables are winsorized at the 1% and 99% levels. We assume that they also winsorize the covariates that enter regressions, including those used to estimate PMDA. They do not state whether they winsorize PMDA, once computed. We will assume in our replication that

specification without these covariates. Given that we start with a randomized trial, covariates are not needed to address potential omitted variable bias. Including them should have only minor effects on point estimates, and can sometimes affect standard errors if the covariates are powerful predictors of the outcome, which we do not expect here.

Moreover, one should not include covariates that could potentially be affected by the treatment. Doing so can introduce bias, if these covariates also predict the outcome. Given the broad range of outcomes that researchers have argued could be affected by the short-sale experiment, it is hard to see what covariates could be both useful and not also potential outcomes of treatment. In our view, all of the FHK covariates could be affected by treatment. Indeed, GMW argue that the short-sale experiment affected capital raising (and thus assets and leverage) and share price (and thus market-to-book ratio), and FHK themselves argue that the experiment affected abnormal accruals, and thus ROA. Nonetheless, for a closer match to FHK, we will report a specification that includes their covariates, in which we add  $\lambda * \mathbf{x}_{i,t}$  to eqns. (3)-(5), where  $\mathbf{x}_{i,t}$  is a vector of covariates (for each  $i, t$ ) and  $\lambda$  is the corresponding coefficient vector.

As discussed earlier, we calculate discretionary accruals using three different measures, operating accruals, total accruals and PMDA. When we study PMDA, we follow FHK and exclude 2004 from the sample.<sup>20</sup> We also have to decide whether to further limit the sample, beyond the firms in the 2005 Financial Analysis Sample, by requiring a balanced panel of firms, with data throughout the sample period. We prefer to use the full 2005 Financial Analysis Sample. First, we expect, and confirm below, that sample attrition will not be meaningfully affected by the short-sale experiment. Second, the experiment is randomized, so we do not expect there to be substantial treated-vs-control differences during the pre- experiment period. However, it is common when using an unbalanced panel, to assess robustness with a balanced panel. FHK use a balanced panel for their main results.<sup>21</sup> Accordingly, we will report results using both the full 2005 Financial Analysis Sample, and a balanced panel drawn from this sample.

Following Kothari et al. (2005), we will compute PMDA using all firms on Compustat. FHK do not specify which firms they use to compute PMDA, but state that they follow Kothari et al. (2005). They appear to include firms not in the R3000 in computing abnormal accruals; this could explain why they report that both pilot and control firms have negative average abnormal accruals in both the pre and experiment periods (their Figure 2).

**F-score.** FHK also study the effect of the SEC experiment on the F-score, developed by Dechow et al. (2011), as a measure of the likelihood of a future downward restatement of earnings, reported in the SEC Accounting and Auditing Enforcement Releases (AAERs). The model specification will be the same as above, with a different outcome variable. We may lose some firms from the sample due to the need for data on all elements that enter the F-score.

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they did, but also expect that once one winsorizes covariates, also winsorizing PMDA should make little difference.

<sup>20</sup> FHK exclude 2004 from the pre-experiment period in their main results, but include it in robustness checks and report that results are similar either way. Our judgment is not to exclude 2004 from the pre-treatment period, on the grounds that we would be no reason to expect firms, which will become subject to the short-sale experiment in the future, to change their accruals in 2004, even if they would do so in response to the actual experiment.

<sup>21</sup> FHK report descriptive statistics (Table I) and univariate tests (Table II) with both a balanced and unbalanced panel. They report multivariate results only with their balanced sample, but assert that the find similar results with the unbalanced panel (p.1262).

## Toward Exact Replication of Selected FHK Results

After completing the analysis above, we will begin with our results for the Mixed Experiment, and methodically move from our approach toward exact replication of the principal FHK results for earnings management. We will follow the steps specified below, compute results after each step, and report the principal results in either text or appendix. We will come as close as we can to exact replication of the results in their Table I (covariate balance);<sup>22</sup> Table II (summary statistics); Figure 2 (graphical overview of results from Table III); Table III (their core results for PMDA); and Table V (effect of the SEC experiment on F-score). We will produce results for both unbalanced and balanced samples. FHK report results for both unbalanced and balanced samples in their Tables I and II, and state that “Throughout, the results are similar using either sample” (FHK at 1262), but otherwise report only balanced sample results. We expect that the principal limit on exact replication will be that we have a larger sample, and do not have a feasible way to exactly replicate their sample.

The FHK model is similar to our eqn.(3), except that it: (i) includes covariates; (ii) omits firm FE; (iii) uses pre, during and post dummies instead of year FE; and (iv) uses two-way clustering on firm and year, where we use standard errors clustered only on firm.

**Step 1: Define Samples.** As part of the analysis above, we will report results using both balanced and unbalanced samples, with the samples defined separately for our preferred measures of total accruals and operating accruals, and for their preferred measure of PMDA (thus, 6 samples, 3 balanced and 3 unbalanced).

**Step 2: Add Covariates.** We will add the FHK covariates, and limit the unbalanced and balanced samples to firms with data on these covariates (for the balanced sample, we require data for all years). In another setting, limiting the sample to firms with complete data on covariates could introduce bias. This should not be a concern here, because the initial randomization should lead to data on covariates being “missing completely at random, using the terminology of Little and Rubin (2019). As noted above, we are uncomfortable using their covariates, because the covariates could be affected by the treatment. However, if our prior that the SEC experiment should have little effect on firm fundamentals is correct, any bias should be small, apart from the potential for the choice of covariates to reflect an underlying specification search.

**Step 3: Use FHK Sample Periods.** We will switch from our preferred sample periods to the FHK periods, as stated in Table 4 and Appendix 4. Specification search aside, we see no reason why changing the choice of sample period should introduce bias.

**Step 4. Remove Firm FE.** We will remove firm FE. As discussed above, we consider results with firm FE to be more credible, but omitting them should not produce bias, given the underlying randomization.

**Step 5. Remove year FE.** We will replace year FE with the dummy variables that FHK use for the pre-experiment, during, and post-experiment period. Specification search aside, we see no reason why using cruder period dummies should introduce bias.

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<sup>22</sup> We provide a covariate balance table for the 2005 Financial Analysis Sample in Appendix 3. The main change will be to also provide tables for balanced and unbalanced samples, defined consistently with FHK, in which we use their sample period and exclude observations without full data on covariates.

**Step 6. Two way clustering.** We will use two-way clustering of standard errors, by both firm and year. As noted above, given the short time dimension, this has the potential to introduce downward bias in standard errors. If two-way clustering produces smaller standard errors than clustering only on firm, there would be strong reason to prefer one-way clustering.

**Cross-Sectional Assessment:** If we find any significant effect of the short-sale experiment on outcomes for fully-treated firms relative to control firms, for the separate experiments or the Mixed Experiment, we will assess whether cross-sectional variation in those results is consistent with our prior expectations. In particular, any effects of relaxing short-sale restrictions are more likely for the Small Firm NYSE Experiment, which is the only experiment that compares fully treated to full control firms, than for the other experiments, and within the Small Firm NYSE Experiment, for firms without publicly traded options.

We will also assess whether any results are correlated with: (i) firm-specific levels of short interest, which are the presumed drivers of indirect outcomes; and (ii) changes in short interest, relative to the levels at the experiment launch or experiment announcement. We will assess whether the expected reversal of any observed effects, after the experiment ends, is correlated with the observed effect during the experiment period. For example, if as HHZ find, audit fees increase for treated firms relative to controls during the experiment period, is the relative increase in fees different for firms with larger short interest? And if so, do relative fees drop differentially for these firms after the experiment ends?

To take a second example, FHK present evidence in their appendix for a positive association between PMDA and short interest for treated firms during the experiment period. However, there is a general positive association between accruals and short interest (Hirshleifer, Teoh and Yu, 2011). Thus, this association provides evidence for an effect of the short-sale restrictions only if it is stronger for treated than for control firms. A simple test for differences in this correlation would be to examine, during the pre-experiment and experiment period:

$$Accrual_{i,t} = \beta_0 + \gamma_t + f_i + \beta_1 Shintn_{i,t} + \beta_2 Pilot_i * Shintn_{i,t} + \varepsilon_{i,t} \quad (6)$$

Here  $Shintn_{i,t}$  is average short interest during year  $t$ . We expect  $\beta_1$  to be positive based on Hirshleifer, Teoh and Yu (2011). The coefficient of interest is  $\beta_2$ ; a significant positive  $\beta_2$  would provide evidence that this correlation is stronger for treated firms during the experiment period. Analogous tests can be used for other outcomes. One can also replace  $Shint_{i,t}$  in Eqn. (6) with  $\delta Shint_{i,t}$ , defined as  $(Shint_{i,t} - Shint_{i,start})$ , where  $Shint_{i,start}$  is short interest for firm  $i$  at the start of the experiment period (or at experiment announcement). We discuss below the cross-sectional assessments conducted by GMW.

### **GMW: Effect of Short-Sale Rule on Capital Raising and Investment**

GMW report that a measure of “abnormal” short interest rises significantly for smaller (below median) fully treated firms between experiment announcement in July 2004 and launch in May 2005, and that smaller fully treated firms have negative abnormal returns in the second half of July, shortly *before* the short-sale experiment was announced. They further find “that small firms (assets below the sample median) react to these lower prices by reducing equity issues and investment.” They find that small firms have lower capital expenditures (capex), lower R&D, lower assets (consistent with lower capex), and less issuance of equity (but not less issuance of debt). Litvak, Black, and Yoo (2019) find no evidence of an effect on either short interest or share



prices, either before the experiment announcement or at any other time. We will conduct additional tests for a possible effect on short interest or share price (discussed below), but our principal aim is to reassess the other outcomes studied by GMW, which are the principal focus of their article.

As we did for FHK, we first, put aside the GMW specifications, and specify how we would study whether the short-sale restrictions affect capital raising and investment, given the underlying randomized experiment. We then consider additional specifications that provide a closer match to GMW. Because they find results principally for small firms, we will study an additional sample, consisting of the bottom half of the R3000, in addition to our four main samples (Small Firm GMW Experiment).

GMW, unlike FHK and HHZ, do not assess whether any effects they find are reversed during the post-experiment period.<sup>23</sup> We do so in our re-assessment. Including the post-experiment period (2008-2010) and allowing for a separate post-experiment treatment effect, should have minimal effect on the experiment-period estimate.<sup>24</sup>

**Capex and R&D.** For capex and R&D (and the sum of the two), we assess differences in means between treated and control firms, for the four sub-experiments, the Mixed Experiment, and the Small Firm GMW Experiment. The difference in means provides an intent-to-treat estimate, which should be unbiased. We use eqns. (3)-(5), with  $y_{i,t}$  as the outcome (Capex/assets, R&D/sales, or, following GMW ((Capex + R&D)/assets); the other variables are defined above. Studies of R&D typically scale R&D by sales rather than assets; we follow that practice here.<sup>25</sup> GMW do not separately study R&D, scaled by either sales or assets.

As we did for the FHK reassessment, we prefer estimates without covariates. Given the initial randomized experiment, controlling for covariates is not needed for an unbiased estimate. One can control for covariates that are not outcomes of the treatment, and doing so can sometimes produce more precise, but still unbiased estimates. However, we cannot think of any covariates that are both valid (not potential outcomes) and useful. Given the GMW hypothesis that relaxing the short-sale restriction affects growth and investment, we cannot use covariates related to growth or investment, or which are outcomes of growth or investment, as covariates.<sup>26</sup> Nonetheless, for closer comparison to GMW, for these and other outcomes, we will also report a specification that includes their covariates: cash flow, lagged  $\ln(\text{assets})$ , and lagged operating income/assets.

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<sup>23</sup> GMW appear to use a sample period from 2001-2008, based on dividing their number of observations (13,526) by the average number of firms per year (1,690). They treat control firms as treated for fiscal years ended after Jan. 6, 2008.

<sup>24</sup> The estimated treatment period effect  $\beta_1$  in eqn. (3), and the annual leads-and-lags estimates through 2007 from eqn. (4), should be unaffected by adding 2008-2010 as a separate period in eqn. (3) and as separate years in eqn. (4). In regressions with covariates, there will be differences because the coefficients on covariates will be estimated over the entire period, but these differences should be small and adding 2008-2010 should not introduce bias.

<sup>25</sup> One reason for preferring to study R&D/sales rather than R&D/assets is that because R&D is expensed, it directly reduces assets.

<sup>26</sup> GMW control for cash flow, lagged  $\ln(\text{assets})$ , and lagged operating income/assets. But if relaxing the short sale rule affects investment, the change in investment behavior will also affect cash flow and assets, and could affect profitability. Lagging assets and profitability, as they do, does not solve this problem, since they treat firm outcomes during 2005-2007 as the experiment period. For 2006, these outcomes for 2005 could be (and under their hypothesis, are) affected by the experiment, and more so for 2007.

**Asset and Sales Growth.** GMW also study whether the short-sale experiment affected asset growth. For us, a natural specification to assess growth in assets or sales is to use  $\ln(\text{assets})$  or  $\ln(\text{sales})$  as the outcome variable, in a firm fixed effects specification. For closer comparability with GMW we will also study the annual change in  $\ln(\text{assets})$ ; this should be close to their specification, which uses percent change in assets, but will be less influenced by outlier firms which experience rapid growth.

### Capital Raising

**Equity Issuance.** GMW also assess whether the short-sale experiment affects issuance of equity and debt. Compustat provides a measure which captures all share issuances, including small ones (for example, due to exercise of employee stock options).<sup>27</sup> Given this measure, we see equity issuance/(pre-experiment market capitalization) as a natural outcome variable; we measure market capitalization at year-end 2004.<sup>28</sup> GMW instead use equity issuance/lagged assets as the measure of equity issuance. For greater comparability with GMW, we will also examine equity issuance/pre-experiment assets. We may also assess another natural measure for equity issuance: a dummy variable for whether a firm conducts a public offering of shares in a given year.

**Debt Issuance:** As outcome variables for debt issuance, we will use a dummy variable for whether a firm issues long-term debt in a given year, and also debt issuance amount/pre-treatment firm market value (market capitalization plus book value of debt).<sup>29</sup> For greater comparability with GMW will also use debt issuance amount/pre-treatment assets as an alternative outcome variable.

**Total Capital Raised:** If one's hypothesis is that the short-sale rules affect capital raising, it feels natural to us to assess total capital raised, as well as separate measures for equity and debt issuance. We will do so, even though GMW do not, using as outcomes a dummy variable for any capital raising (equity or debt), a categorical variable for the number of times the firm raises equity or debt capital in each year, and total issuance amount/pre-treatment/market value.

**Leverage:** If one's hypothesis is, as GMW's appears to be, that the short-sale rules affect the form in which capital is raised, a natural outcome variable is leverage. We will assess whether there is an effect of treatment on leverage, using as outcomes both book leverage (total debt/total assets) and market leverage (total debt/firm market value).

For each measure, we will again use equations (3) (DiD allowing for post-experiment reversal); and (4) (same with leads-and lags-graphs).

### Cross-Sectional Tests

If any significant results are found, we will conduct cross-sectional tests, similar to those described above for FHK, using equation (5), just with different outcome variables. GMW carry out several additional cross-sectional assessments. They speculate that treated firms with high abnormal accruals, high book-to-market ratio, and high analyst forecast dispersion, in the pre- period, are

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<sup>27</sup> Compustat variable SSTK (sale of common and preferred stock), defined as sale of common and preferred stock, including "exercise of stock options and/or warrants and conversion of preferred stocks and/or debt into common stock. Both we and GMW use this variable.

<sup>28</sup> If one's theory is that the short-sale experiment affected capital raising, pre-experiment market capitalization is a safer denominator than time-varying market capitalization, because it cannot be affected by the treatment. The choice of denominator should not matter much if any effect of the experiment on market capitalization is small, as we expect.

<sup>29</sup> Both GMW and we use the Compustat variable DLTIS (long-term debt issuance) for debt issuance.

more vulnerable to bear raids, and should show higher response to the short-sale experiment. One can implement this idea by extending equation (5) to include an interaction between the treatment dummy and the cross-sectional variable:

$$y_{i,t} = \beta_0 + f_i + \gamma_t + \beta_1 \text{Pilot}_i \times \text{During}_t + \beta_2 * \text{Pilot}_i \times \text{During}_t \times x_{i,2004} + \varepsilon_{i,t} \quad (6)$$

Here  $x_{i,2004}$  is the covariate that one believes may predict vulnerability to bear raids, measured in 2004, prior to experiment launch.

### HHZ: Effect of Relaxing Short-Sale Restrictions on Audit Fees

HHZ study the effect of the short-sale experiment on audit fees. They report evidence for a relative increase in audit fees for treated firms during the experiment period which does not, however, reverse during the post-experiment period. The evidence for higher fees during the experiment period is weak, however. The treatment effect is insignificant in univariate analyses and barely significant ( $t = 1.98$  or  $1.99$  depending on their covariate set) in regression analyses.

As we did for FHK and GMW, we first, put aside the HHZ specifications, and specify how we would study whether the short-sale restrictions affect audit fees, given the underlying randomized experiment. We then consider additional specifications that provide a closer match to HHZ.

Our preferred outcome variable, and theirs, is  $\ln(\text{audit fees})$ . We prefer a regression specification which adds  $\ln(\text{assets})$  as a covariate to eqn. (3). The corresponding leads-and-lags model adds  $\ln(\text{assets})$  to eqn. (5).<sup>30</sup> We know from prior research (and common sense) that larger firms have larger audit fees. Assets is a standard scaling variable in the accounting literature (Simunic 1980). As noted above, controlling for firm size can be problematic if you expect the experiment to affect firm size (as GMW do). Our judgment was that the gain in precision from including this important covariate justifies including it for the HHZ research question, despite the potential for the short-sale experiment to affect firm size. HHZ control for firm size using  $\ln(\text{sales})$  rather than  $\ln(\text{assets})$ . In robustness checks, we will assess what difference it makes whether one controls for  $\ln(\text{assets})$ ,  $\ln(\text{sales})$ , neither, or both.

We use the same sample period as we did for the FHK and GMW re-assessments: 2001-2004 as the pre-treatment period; 2005-2007 as the treatment period; 2008-2010 as the post period, and a robustness check in which we drop 2004. HHZ use 2000-2003 as the Pre Period, exclude 2004, use 2005-2007 as the During period, and use 2008-2013 as the Post Period. We are not sure why they use a Post period that extends until 2013, and prefer to end the post period in 2010. In our view, given the roughly 2-year experiment period, a three year post period should be ample for any treatment-period effects to wash out.

**Covariates.** For the reasons discussed above, we prefer a specification without covariates other than  $\ln(\text{assets})$ . Nonetheless, for closer comparison to HHZ, we will also study a specification that includes the following covariates: Size measured as  $\ln(\text{assets})$ , return on assets, leverage (total debt/assets) and book-to-market ratio (compare their Table 5, model 1). In Table 5 model (2), HHZ use a long list of additional controls including current assets/total assets; quick ratio (current assets other than inventory/current liabilities); (accounts receivable plus inventory/total assets, growth in total assets (not further defined), short interest (% of shares outstanding), Big 4 auditor,

<sup>30</sup> Both we and HHZ obtain audit fees from Audit Analytics, which is the standard source for this measure. They winsorize all variables at 1% and 99%. We winsorize selected variables, where this seemed necessary; see details in the Appendix.

multinational indicator, number of business segments, dummy variable for negative net income, and non-December fiscal year end indicator. But adding these extra covariates does not add much to the explanatory power (the adjusted R-squared increases only by 0.005). One also should not control for short interest, as this could be an outcome of treatment (and should be, if their hypothesis is correct that the short sale experiment affects firm risk, as auditors perceive it, and thus audit fees).

**Cross-Sectional Assessment.** If any significant results are found, we will conduct cross-sectional tests, similar to those described above for HHZ, using eqn. (5), with  $\ln(\text{audit fees})$  as the outcome variable. HHZ also assess whether treated firms with higher risk of financial distress are more likely to pay increased audit fees than other firms. We will assess this hypothesis using the Altman Z-score as a measure of risk of financial distress, using eqn. (5).

### **Further Look at GMW Results for Short interest and Pre-Announcement Returns**

#### **Pre-announcement returns**

GMW report a drop in share prices for small treated firms (below the sample median in assets) in the period *before the experiment was announced*. We focus in this project on their other results because Litvak, Black, and Yoo (2019) assess their finding of a drop in share prices, and find no support for it, using simple raw returns as their returns measure. Raw returns are an appropriate measure, rather than market-adjusted returns because the sample is the whole R3000, so subtracting the market return from all firms will not affect inference. One should not need to use abnormal returns (CARs) because, if one starts with a randomized experiment, the treated and control firms should be balanced on all covariates, including pre-treatment  $\beta$ ; we confirm balance in Appendix 3.

Still, this is not a precise match for GMW, who study abnormal returns for the bottom half of the R3000, while Litvak, Black, and Yoo use raw returns and study somewhat different samples (the Small Firm NYSE Experiment and the Small Firm Nasdaq Experiment, which together comprise the R2000, rather than the bottom half of the R3000). In this project, for greater comparability with GMW, we will assess whether there is any evidence for a pre-announcement share price drop for smaller treated firms (the bottom half of the R3000 for June 2004, based on market capitalization). We will use the 2004 Analysis Sample, but exclude two firms with share price below \$1 as of June 30, 2004. We will measure  $\beta$  relative to the CRSP value-weighted index (VWRETD) (as GMW do), which we measure over the 250 trading days preceding the SEC announcement of the short-sale experiment on July 28, 2004. We will follow their approach of measuring buy-and-hold abnormal returns around the announcement date, and present results in graphical form, similar to their Figure 2, over June-September 2004.

#### **Abnormal Short interest**

GMW also report a gradual increase in short interest, principally for smaller firms, both between experiment announcement and experiment launch, and continuing in the early part of the experiment period. They study both raw short interest and a measure of abnormal short interest. Litvak, Black and Yoo (2019) assess this finding as well, and find no support for it, using raw short interest as their outcome variable. One should be able to use raw short interest, rather than a regression-based measure of “abnormal” short interest, because for a randomized experiment, treated and control firms should be balanced on all covariates. Also, computing abnormal short

interest involves regressing short interest on covariates which could themselves be outcomes of treatment.<sup>31</sup>

Still, this is not a precise match for GMW, who study both raw and abnormal short interest, and also use a somewhat different sample. In this project, for greater comparability with GMW, we will assess whether the evidence for a rise in *abnormal* short interest for smaller firms (the bottom half of the 2004 Analysis Sample based on market capitalization), and measure abnormal short interest using a regression similar to GMW, in which, for each month over the sample period, we regress monthly short interest on month and firm FE and the following covariates: lagged book-to-market ratio; ln(lagged total assets) lagged ROA, and a dummy variable for listing on the NYSE or the American Stock Exchange, compute the monthly residuals, and plot these residuals over the measurement period, in the spirit of their Figure 1. We estimate residuals using:

$$Shint_{i,t} = \beta_0 + \gamma_m + f_i + \lambda x_{i,t} + \varepsilon_{i,t} \quad (7)$$

Here  $\gamma_m$  are the month FE and  $\mathbf{x}$  is the covariate vector. This specification involves two changes from GMW, apart from the longer sample period. First, we do not use trading volume as a regressor, because trading volume is likely to be an outcome of treatment. Second, in their Figure 1, GMW apparently *cumulated* monthly abnormal short interest, rather than plotting monthly *levels* of abnormal short interest. This would be an analytic error, akin to plotting cumulative “abnormal prices” (price minus predicted price, based on covariates), rather than cumulative abnormal *returns*.

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<sup>31</sup> In practice, GMW obtain stronger results for short interest with raw short interest than with abnormal short interest.

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## Appendix 1. Papers Examining the SEC Short-Sale Experiment

The table below summarizes the papers we found in a February 2019 literature search, studying the short-sale experiment. The list includes 5 early papers, 33 recent papers, plus Boehmer, Jones and Zhang (2018), a market microstructure paper which does not fall within either category. For papers which study multiple questions, we exercised judgment in specifying the primary question.

Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
1	Albertus, Bird, Karolyi, and Ruchti (2017), WP	Effect of Reg. SHO exp't on intrafirm capital allocation	Short selling threats discipline managers' capital allocation decisions.	No effect on multinationals' total investment, but 30% higher capital allocation to foreign subsidiaries with strong recent performance; subsidiaries that receive additional capital show no decrease in productivity.	376 multinationals with 5,575 subsidiaries.	All
2	Alexander and Peterson (2008), J Fin. Markets	Effect of Reg. SHO exp't on trader behavior and market quality	Removal of price tests lets short sellers to place orders that receive quicker execution.	For NYSE firms, removal of uptick test lets short sellers trade more aggressively: treated firms have smaller short trade sizes, more short trades, more short volume, and smaller ask depths. No evidence that removal of uptick rule affects liquidity, price volatility or price efficiency. Smaller, usually insignificant effects on Nasdaq. The study quotes prior work (Ferri et al., 2006) to suggest that the bid test was not effective.	223 matched NYSE pairs and 195 matched Nasdaq pairs	NYSE and Nasdaq, studied separately
3	Bai (2008), Rutgers Bus. Law J.	Effect of Reg. SHO exp't on intraday stock prices and short selling after negative earnings surprises	Negative earnings surprises can cause share price changes and higher short sale volume. If uptick rule matters, one should find greater effect on price declines and short sale volume immediately after negative earnings surprises.	No evidence that treated firms experience faster price declines or higher short sale volume than control firms. For NYSE firms, short orders with immediate execution barred by the uptick rule were executed within 15 minutes. Nasdaq bid test was usually not binding.	Neg. earnings surprises: 311 to pilot firms (170 NYSE; 255 distinct firms); 634 to control firms (341 NYSE; 525 distinct firms)	NYSE and Nasdaq, studied separately

Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
4	Bai, Lee, and Zhang (2018), WP	Effect of Reg. SHO exp't on firms' investment in workplace safety (measured by work-related injury and illness rates)	<i>Performance Pressure Hypothesis</i> : an increase in short selling pressure leads managers to be more myopic and shift away from long-term investments such as those in workplace safety.	Work-related injury and illness rates increase significantly for treatment firms after Reg SHO compared to control firms. The effect is stronger for firms that are in more competitive industries, are more financially constrained, have disadvantaged labor force and have poor corporate governance mechanism.	286 pilot and 532 control firms	All
5	Bennett and Wang (2018), WP	Effect of Reg. SHO exp't on forced CEO turnover	Revelation channel: Short selling accelerates price discovery, which makes firms more likely to fire their CEOs based on the new negative information revealed in stock price. Manipulation channel: Price manipulation by uninformed short sellers can mislead CEOs into making poor decisions and in turn lead to worse CEO performance. Meanwhile, boards can be misled by the manipulated lower prices and blame CEO for the lower market valuation.	Increased likelihood of CEO forced turnover for pilot firms. Consistent with revelation channel, effects are stronger when firms have more earnings management, less informative stock prices and less competitive product markets. Consistent with manipulation channel, the effects are also stronger when firms have more growth opportunities, fewer blockholders and less volatile stock.	425 pilot and 756 control firms	All
7	Bhattacharya, Christensen, Li and Ouyang, 2019.	Effect of Reg SHO exp't on pro-forma reporting.	Increased threat of short selling due to suspension of uptick rule/bid test can discipline pro-forma reporting by pilot firms.	Results suggest that increased threat of short selling significantly curbs aggressive non-GAAP pro-forma reporting by Pilot firms.	225 pilot firms and 507 control firms.	All

Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
6	Billett, Liu, and Tian (2018), WP	Effect of Reg SHO exp't on information spillover and cross monitoring between stock market and loan market	Reg. SHO affects information production and monitoring by short sellers in the stock market, which may have a spillover effect to the syndicated loan market and influence loan contracting terms. Conversely, bank monitoring also provides information and/or certification to the stock market	While firms without bank monitors exhibit a significant decline in stock price upon the announcement of SHO, firms with bank monitors do not react. Firms affected by SHO enjoy a 21 basis point lower loan spread that increases to 36 basis points for bank-dependent firms. SHO does not affect non-price loan terms such as maturity, amount, collateral and covenants.	527 pilot and 1012 control firms.	All
8	Boehmer, Jones and Zhang (2018), WP	Effect of 2007 repeal of uptick rule on arbitrage trading.	The 2007 full uptick repeal makes synchronous portfolio trading such as index arbitrage easier and less costly to execute. The 2005 partial uptick repeal incentivize aggressive short sellers to shift toward pilot firms and away from control firms.	Short activity increases substantially for all firms after the 2007 full uptick repeal, even for pilot firms for which the uptick rule was suspended since 2005. Shorting activity on non-pilot firms co-move more with pilot firms after the full repeal. Short selling aggressiveness among the non-pilot firms decreases after 2005 partial repeal; also reduced comovement between shorting activity on pilot and control firms.	NYSE firms: 360 pilot and 728 control	NYSE
9	Cai and Guo (2018), WP	Effect of Reg. SHO exp't on real earnings management	The threat of short selling could discipline managers and constrain real earnings management	Pilot firms experience a reduction of real earnings management after SHO relative to the non-pilot firms. The effect is stronger in firms where managers are more entrenched and firms with lower institutional ownership and higher analyst coverage.	736 pilot and 1503 control firms	All

Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
10	Chang, Huang, Su, and Tseng (2018), WP	Effect of Reg SHO exp't on compensation contracts.	Relaxation of short sale constraint decreases the speculative component in stock prices, making short-termism less attractive to current shareholders, thereby reducing short-term incentives in managerial compensation contracts.	Pilot firms have longer CEO pay duration relative to non-pilot firms during SHO, but this difference becomes insignificant after SHO concludes. This result is concentrated among firms with pre-existing high investor disagreement. Removing short-sale constraints also leads to longer CEO investment horizon and fewer stock repurchases.	254 pilot and 496 control firms	All
11	Chen, Zhu, and Chang (2017), Accounting and Finance.	Effect of Reg. SHO exp't on corporate payouts	Firms adjust corporate payout policies to signal their good quality to counteract intensified short-selling pressures after SHO.	Small pilot firms increase their cash dividends during SHO relative to control firms, and continue to pay higher cash dividends even after the pilot program ended in 2007. However, share repurchase activities of pilot firms remain unchanged before, during and after SHO.	616 pilot and 1275 control firms	All
12	Chen, Cheng, Luo, and Yue (2014), WP	Effect of Reg. SHO exp't on corporate disclosures	Reduction in short sale constraints incentivize managers to disclose good news in a timely manner to boost the confidence of the stakeholders in the firm and to deter short sellers.	Pilot firms are more likely to issue good news management forecasts without changing the issuance of bad news forecasts. Pilot firms are more likely to bundle bad news forecasts with good news earnings announcements, but management forecast optimistic bias does not increase.	768 pilot and 1484 control firms	All
13	Chen and Wu (2019), WP	Effect of Reg. SHO exp't on real activity manipulation	Short selling threat leads pilot firms to reduce real earnings management activities relative to control firms.	Pilot firms reduces real activity manipulation by increasing abnormal cash flows and decreasing abnormal production activities.	487 pilot and 989 control firms	All
14	Cheng and Zhang (2019), WP	Effect of Reg. SHO exp't on credit rating informativeness	Short sellers discipline credit rating agencies and also provide additional information to credit rating agencies.	Credit ratings of pilot firms become more informative than those of control firms during the pilot program.	276 pilot and 510 control firms	All

Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
15	Choi (2018), Finance Research Letters	Effect of Reg SHO exp't on analyst forecast precision.	Reg. SHO reduced SS constraints for Pilot firms. The paper assumes that this increased SS activity for Pilot firms. The increased activity by passive investors in turn causes analysts to expend less effort in producing their forecasts. This is evidenced a greater incidence of rounded forecasts for Pilot firms relative to control firms.	In a regression of ROUNDING on firm characteristics, the coefficient on Pilot*During is positive indicating that analysts' tendency to issue rounded forecasts increased during the period of Reg SHO for Pilot firms relative to Control firms.	497 pilot and 993 control firms.	All
16	Chu, Hirshleifer and Ma (2017)	Effect of Reg. SHO exp't on share return anomalies	Use the Reg. SHO to examine the impact of lower arbitrage constraints on returns to anomalies.	Monthly returns reduce on average by -0.69% for Pilot firms during the test period. The return difference between Pilot and Control firms vanishes after the experiment ends. No effect is observed for Nasdaq firms.	515 pilot and 1025 control firms	NYSE
17	Clinch, Li, and Zhang (2018), WP	Effect of Reg. SHO exp't on bad news disclosure	The paper conjectures that lifting of SS restrictions on pilot firms reduces incentives and potentially increases litigation costs for managers to withhold bad news and hence expects an increase in voluntary bad news forecasts for Pilot firms relative to control firms.	Documents a significant increase in likelihood of bad news management forecasts for Pilot firms relative to control firms during the experiment period. No corresponding result is observed in the Pre or the Post period or for good news management forecasts.	919 pilot and 1888 control firms	All
18	De Angelis, Grullon, and Michenaud (2017), RFS	Effect of Reg. SHO exp't on CEO compensation	Reg SHO increases the threat of bear raids for Pilot firms causing Pilot managers to be more risk averse. The firm can then compensate for the increased risk aversion by increasing the convexity of pay and also by providing more downside protection to managers' pay.	Pilot firms increase convexity of payoffs (Vega) and increase stock option grants relative to share grants	472 pilot and 878 control firms. Balanced sample: 388 pilot and 702 control firms	All

Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
19	Deng, Gao, and Kim (2017), WP	Effect of Reg SHO exp't on stock price crash risk (negative skewness of returns, down to up volatility)	Lifting SS constraints for Pilot firms reduces managers' incentives for withholding bad news which in turn reduces stock price crash risk for pilot firms. The investment efficiency of Pilot firms also improves.	The stock price crash risk decreases for the Pilot firms during the experiment period relative to Control firms.	776 pilot and 1,734 control firms.	All
20	Diether, Lee, and Werner (2009), JF.	Effect of Reg SHO exp't on short-selling activity, daily returns and volatility, and market quality. Outcomes: Share returns, short sale volume, spreads, and volatility		Higher short sale volume. No effect on daily volatility or share returns. Small increase in spreads and intraday volatility for NYSE firms but not NASDAQ firms	NYSE: 448 pilot and 904 control firms Nasdaq: 376 pilot and 757 control firms	NYSE and Nasdaq, studied separately
21	FHK: Fang, Huang, and Karpoff (2016), Journal of Finance	Effect of Reg SHO on earnings management during the experiment period. The measure of EM is Performance matched discretionary accruals	Increased threat of SS causes Pilot firms to decrease EM relative to control firms during the experiment period relative to control firms.	PMDA of Pilot firms are shown to decline during the experiment period relative to control firms. The difference reverses in the Post experiment period.	388 pilot and 709 control firms	All
22	Francis, Samuel, and Wu (2017), WP	Effect of Reg SHO exp't on dividends.	Increased SS can improve price efficiency for Pilot firms which in turn decreases the need for dividend payout. On the other hand, increased SS increases monitoring of managers of Pilot and hence managers become risk averse and increase payout instead of risky investments.	Pilot firms are more likely to increase dividends than control firms.	590 pilot Firms and 1166 control firms. Balanced sample: 448 pilot and 834 control firms.	All

Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
23	GMW: Grullon, Michenaud, and Weston (2015), RFS	Effect of Reg SHO exp't on capital expenditures, R&D, assets, and equity and debt issuance.	For overvalued firms, removing short-selling constraints can lead to lower share prices and reduce overinvestment. Channel: Managers learn from share prices when making investment decisions.	1) Short interest increases for pilot firms after experiment announcement (July 28, 2004) 2) Share prices of pilot firms fall after SEC approves experiment (June 23, 2004) but before list of treated firms is known. 3) Pilot firms invest less during experiment period. (lower CAPEX and asset growth).. 4) Pilot firms reduce equity issuance. Results (2)-(4) larger for small firms.	651 pilot and 1,279 control firms	All (large and small separately)
24	He and Tian (2016), WP	Effect of Reg SHO exp't on managerial myopia.	Tests whether increased threat of SS due to Reg SHO for Pilot firms exacerbates or mitigates managerial myopia. The proxy for myopia is firm innovation measured by the number of patents and patent citations.	Quality, value and originality of patents generated by the Pilot firms improve relative to control firms suggesting that short sellers mitigate myopia.	592 pilot and 1,158 control firms.	All
25	HHZ: Hope, Hu, and Zhao (2016), JAE	Effect of Reg SHO exp't on audit fees.	Auditors of Pilot firms might face higher litigation risk due to higher ex-ante threats of short-selling activities. Thus auditors might increase their audit effort to reduce the probability of making mistakes and ask for higher fees (audit effort channel), or pass the burden of increased risk onto shareholders by charging higher fees without increasing their audit effort (risk-premium channel).	Treated firms incur higher audit fees. The effect is more pronounced for (1) firms with higher loan default risk and (2) firms whose managers have high levels of in-the-money options. Default risk is measured using Bharath and Shumway (2008)'s measure. High level of in-the-money options is relative to SIC 4 digit industry median in at least four out of five years from 2000-2004.	538 pilot firms and 1,072 control firms	All

Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
26	Hu, Li, Liao and Zhang (2019)	Effect of Reg SHO exp't on loan loss provisions and risk taking by bank holding companies	Reduction in short sale constraints for pilot BHCs increases the fear of bank runs and hence incentivizes pilot firms to look better and healthier.	Pilot BHCs are shown to delay loan loss recognition and the effect is concentrated in smaller BHCs and those with lower regulatory capital.	bank holding companies: 72 pilot and 175 control	
27	Kan and Gong (2018), Int'l Review Fin.	Effect of Reg SHO exp't on price informativeness and return synchronicity	Relaxing short sale constraints will lead to improved price discovery and thus higher return synchronicity	Stock return synchronicity is positively associated with price informativeness The price informativeness and the stock return synchronicity of pilot firms increase relative to those of non-pilot firms during the experiment period, but such difference disappeared after the experiment ended.	986 pilot firms, matched to control firms (without replacement)	All
28	Ke, Lo, Sheng, and Zhang (2018), WP	Effect of Reg SHO exp't on quality of analysts' forecast bias and accuracy.	Reg. SHO exp't, leads analysts to expend more effort to uncover bad news; improves price efficiency making it easier for analysts to provide better forecasts	There is a decline forecast bias and in forecast inaccuracy for Pilot firms during the 8 quarters after Reg SHO was implemented.	Balanced sample has 448 Pilot firms and 879 Control firms.	All
29	Kim and Park (2015), WP	Effect of Reg SHO exp't on sensitivity of investment sensitivity to share price	Relaxing short sale constraints leads managers to learn less from share price (why not explained), hence reduces sensitivity of investment decisions to price.	Pilot firms show lower sensitivity of investment to price; lower future operating performance ("theory": pilot firms' managers learn less from their firms' stock prices due to lower PIN)	Balanced panel: 339 pilot and 647 control firms	All
30	Kunzmann and Meier (2018), WP	Effect of Reg SHO exp't on forced CEO turnover	Higher short interest predicts shareholder activism; positive relation between short interest and forced turnover grows stronger with higher activism.	No effect of removing short sale constraints on turnover probability for small firms; higher probability of forced turnover for large firms. Large firm turnover is correlated with short interest.	Small firms: 235 treated; 454 control. Large firms: 252 treated; 493 control	All (large and small separately)
31	Li and Zhang (2015), JAR	Effect of Reg SHO exp't on voluntary disclosure using as proxies management forecast precision and readability of bad news annual reports	Managers of pilot firms respond to increased short-sale threat by reducing disclosure precision, to maintain share price.	Price sensitivity of pilot firms to bad news forecasts increases; no change in sensitivity to good news forecasts.	346 pilot and 711 control firms.	All



Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
32	Lu and Peng (2018), WP	Effect of Reg SHO exp't on measures of corporate tax avoidance	Increased short-sale threat of SS increases monitoring and disciplines managers who respond by reducing corporate tax avoidance.	Pilot firms engage in less tax avoidance and have higher effective tax rate.	579 pilot and 1194 control firms.	All
33	Rusinova, Wernicke, and Bansal (2018), WP	Effect of Reg SHO exp't on corporate social responsibility (CSR), measured by KLD index	To be protected from downward pressure on price by short selling, pilot firms strengthen relations with stakeholders by improving CSR.	1) Pilot firms increase their CSR scores; effect is smaller for firms with higher percentage of transient institutional investors and financially constrained firms	1,682 firms (They do not indicate how many treated firms and controls are among these 1,682 firms)	All
34	Securities and Exchange Commission, Office of Economic Analysis (2007)	Effect of Reg SHO exp't on market quality. Outcomes: Short selling, liquidity (depth and spread), volatility, market efficiency (return reversal and auto-regression), and extreme price changes		1) Pilot firms: higher short sale volume; but no change in short interest or option trading volume 2) Quoted ask depth decreases for NYSE listed pilot firms 3) Pilot firms: higher intraday volatility; 5-minute return reversals and semi-auto-regression, but no change in extreme price movements	NYSE: 504 pilot and 973 control firms. Nasdaq: 439 pilot and 917 control firms	NYSE and Nasdaq, treated separately
35	Sun and Xu (2018), WP	Effect of Reg SHO exp't on readability of 10-K annual reports	Pilot firms reduce readability to reduce short-selling pressure.	Pilot firm 10-K decreases during experiment period; The readability decreases in post exp' when uptick rule was repealed.	630 pilot and 1269 control firms. Balanced sample: 382 pilot and 674 control firms.	All
36	Wang (2018), WP	Effect of Reg SHO exp't on corporate cash holdings	Short-selling pressure reduces share prices to drop and makes external financing more difficult. To avoid being financially constrained, pilot firms hold more cash..	Pilot firms hold more cash; effect more pronounced for firms with more financial constraints, more liquid shares, more short-term investors, and stronger product market competition.	Not stated in the paper	All

Recent	Papers	Research Question	Channels	Summary of Results	Sample size	Exchange
37	Wei and Zhang (2018), WP	Effect of Reg. SHO exp't on SEO underpricing.	Stronger short selling threat exposes firms to greater price downside risk and thus induces lower risk-taking, which mitigates negative market reaction to SEOs and reduces SEO underpricing	1) Pilot firms experience smaller SEO underpricing; effect is stronger for riskier firms and firms with higher executive risk-taking incentives, higher default risk, and lower M/B ratio. 2) Pilot firms have lower discretionary accruals, capital expenditure, leverage, and default risk. 3) Pilot firms are more likely to issue seasoned equity and issue more equity. 4) Control firms have smaller SEO underpricing after experiment ends.	221 pilot and 502 control firms conducting SEOs	All
38	Young (2016), Finance Research Letters	Effect of Reg SHO exp't on conditional conservatism	Pilot firms decrease conditional conservatism by delaying recognition of bad earnings because their share prices become more sensitive to bad news.	Pilot firms decrease conditional conservatism during experiment period.	1,905 firms (number of treated and control firms not stated)	All
39	Zhang (2018), WP	Effect of Reg SHO exp't on mispricing and liquidity.	Relaxing short-sale restrictions can facilitate arbitrage, which could correct mispricing or provide liquidity.. Outcomes: measures of abnormal returns	Pilot firms have reduced abnormal returns and increased liquidity; effect more pronounced for firms more likely to have binding short-sale constraints	742 pilot and 1,482 control firms	All



## Appendix 2. News Coverage of the Short-Sale Experiment

The short-sale experiment attracted very little press attention. We searched the business press for news stories and other information about the experiment, both during the launch period (June 2004 through April 2005) and the experiment period (May 2005-May 2007), and present below the results of that search in. The SEC's announcement on June 23, 2004, of its plans to conduct the experiment, and the formal experiment announcement on July 28, 2004, attracted no news attention whatsoever, at least none that we could find. Most of the comments the SEC received supported the experiment.<sup>32</sup> The very limited news coverage of the experiment between the announcement and the actual launch contained only technical explanations of how the experiment would work.

We found no evidence of concern or political opposition from firm managers. The SEC's July 2004 announcement of the pilot was not covered in any of the standard business news sources (including the Dow Jones News Service (DJNS), Bloomberg, the Wall Street Journal (WSJ), and the New York Times (NYT)).<sup>33</sup> The experiment launch, in May 2005, was noted in a DJNS story a few days earlier, with a WSJ summary the next day. In 2006, the SEC extended the experiment, originally scheduled for one-year, with minimal press attention and no apparent controversy. We found one DNJS story about the extension, with a WSJ summary the next day. Neither WSJ story was long enough to warrant a byline.<sup>34</sup>

The SEC's proposal to repeal the short-sale rule, announced in December 2006, also attracted no apparent opposition that we could find. An NYT story about the repeal explains:<sup>35</sup>

You may not have read of this proposal. It was virtually ignored by the news media, and if any companies are upset about it, they have not made themselves known. A pilot program that exempted some companies from the so-called uptick rule starting in 2005 drew little attention.

This is the *only* NYT story about the experiment we found.

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<sup>32</sup> See SEC Release 34-50103 (July 28, 2004), reprinted in 69 Federal Register (Aug. 6, 2004), at 48,008, 48012.

<sup>33</sup> Appendix Table App-1 lists all of the business press stories we found. The first story was on 30 November 2004, and explained that the SEC was delaying the experiment launch to give the exchanges time to make programming changes so they could implement the experiment. This was not even a separate story about the short sale experiment; instead, this story was appended to a main story about another SEC rule. Judith Burns, SEC Delays Short-Sale Pilot, Seeks NMS Comment, *Dow Jones News Service* (Nov. 30, 2004).

<sup>34</sup> SEC Pilot Program To Halt 'Uptick' Rule, *Wall Street Journal* (April 29, 2005); SEC to Extend Test On Short-Sale Rules, *Wall Street Journal* (April 22, 2006).

<sup>35</sup> Floyd Norris, 70 Years Later, A Scapegoat Gets a Break, *New York Times* (Dec. 8, 2006). The NYT also published an op-ed article in October 2006, supporting repeal of the short-sale rule. Richard Sauer, Bring on the Bears, *New York Times* (Oct. 6, 2006).

The SEC formally approved repeal in June 2007. A *Wall Street Journal* story on the repeal explained that the rule had become “more of an annoyance than a hindrance” to short-sellers, discussed researchers’ view that “the uptick rule's usefulness has disappeared”, and did not mention any opposition to repeal.<sup>36</sup>

### Appendix Table App-1: New Stories about the Experiment

To capture news stories about the SEC Experiment we searched the Factiva database, for the following sources using the following search terms, over July 1, 2004 through year-end 2007: Dow Jones News Service (DJNS); New York Times (NYT); Wall Street Journal (WSJ); Bloomberg; PR Newswire; Forbes; Bloomberg BusinessWeek. We used all combinations of two or more of the following search terms: SEC (also S.E.C.); Regulation (also Reg) SHO; Pilot Program; Short Sale(s). It was infeasible to search solely for “short sale” or for “pilot program” due to a large number of largely irrelevant matches.

Date	Source	Title	Authors	Notes and excerpts
11/30/2004	DJNS	SEC Delays Short-Sale Pilot, Seeks NMS Comment	Judith Burns	Main story is SEC seeking comment on Regulation NMS (for national market system); story notes that “Separately, the SEC announced that an experiment in lifting short-sale restrictions for some stocks, set to begin in January, will be delayed until May. [SEC spokesperson] Nazareth said the additional time will allow exchanges to complete the necessary computer programming modifications. . . .”
04/26/2005	DJNS	SEC Encourages Researchers To Study Short-Sale Data	Judith Burns	“SEC staffers on Tuesday invited outside research on whether market quality would change if short-sale restrictions such as the “bid” and “tick” test were eliminated, and whether a uniform bid test should be extended to smaller stocks. The SEC staff said it has arranged for markets to publicly release monthly short-sale data, and urged researchers to submit studies on the short-sale experiment to the SEC.”
04/28/2005	DJNS	SEC To Suspend 'Uptick' Rule In Reg SHO Pilot Program	Karen Talley	Describes experiment, based on SEC press release: “The suspension of the uptick rule is aimed at allowing the agency to obtain data to help assess whether it should be removed in part or in whole. . . . The pilot program is seen by the SEC as being in the public interest because the uptick rule may actually harm market quality by inhibiting free movement of prices. The rule is about 70 years old and there is some sentiment it may have outlived its purpose, the SEC said.”
04/29/2005	WSJ	SEC Pilot Program To Halt 'Uptick' Rule	none	Summary of DJNS story from prior day.

<sup>36</sup> Spencer Jakab, Short-Sellers May Owe ETFs Some Thanks --- Dropping of 'Uptick' Rule By SEC Comes as Growth Of Stock Baskets Is Soaring, *Wall Street Journal* (June 15, 2007).

<b>Date</b>	<b>Source</b>	<b>Title</b>	<b>Authors</b>	<b>Notes and excerpts</b>
11/17/2005	DJNS Int'l; (based on PR Newswire)	State Regulators Set Forum On Abusive Naked Short-Selling	John Connor	North American Securities Administrators Association NASAA plans meeting to discuss naked short selling. Also, ""the SEC launched a pilot program in May to determine the effectiveness of [Regulation SHO]. 'Since the pilot program has reached the midway point, we believe this forum offers a timely opportunity for a thorough discussion of the effectiveness of Regulation SHO from a variety of perspectives," NASAA president Struck said.
04/21/2006	DJNS	SEC OKs Extending Short-Sale Pilot To August 2007	Judith Burns	Based on SEC Press Release: "The Securities and Exchange Commission announced Friday that it will extend an experiment lifting short-sale restrictions for some stocks. The one-year pilot program, set to end April 28, will be extended until Aug. 6, 2007. . . . The SEC said the extension will spare markets the expense of changing computer systems to restore restrictions on stocks in the pilot while it analyzes results from the experiment."
04/22/2006	WSJ	SEC to Extend Test On Short-Sale Rules	none	Summary of DJNS story from prior day.
06/13/2006	DJNS	SEC To Host Sept. 15 Discussion Of Short-Sale Experiment	Judith Burns	SEC to hold roundtable on Sept. 15 to discuss research on short-sale experiment: "The SEC said the session on its Regulation SHO will discuss results of an experiment that lifted restrictions on short sales of some stocks and hear findings from academic researchers who have studied it."
06/28/2006	WSJ Europe	Lawsuits set focus on short selling	Randall Smith	Story about naked short selling; brief mention of Pilot Program: "Some short sellers say they can't knock down stock prices because of "uptick" rules limiting such sales when prices are falling. However, the SEC has a pilot program exempting about 1,000 stocks from the rules, which also don't apply to some trades off the stocks' exchanges."
09/15/2006	DJNS	SEC Meets To Revisit Short-Sale Restrictions	Judith Burns	Review of research on short-sale rule presented at SEC roundtable; that research persuaded the SEC to rescind the short-sale rule.
10/06/2006	NYT	Bring On the Bears (Editorial)	Richard Sauer	"In an unusual (and laudable) effort to measure whether a long-lived regulation actually works, the Securities and Exchange Commission recently completed a pilot program to suspend the uptick rule for a third of the stocks on the Russell 3000 index and compare their performance to stocks still subject to the rule."
12/08/2006	NYT	70 Years Later, A Scapegoat Gets a Break	Floyd Norris	"You may not have read of that proposal. It was virtually ignored by the news media, and if any companies are upset about it, they have not made themselves known. A pilot program that exempted some companies from the so-called uptick rule starting in 2005 drew little attention."
03/23/2007	DJNS	SEC Seeks To 'Modernize' Short-Selling Regs - Official	Daisy Maxey	Story on remarks by James Brigagliano, associate director in SEC Division of Market Regulation division on short-sale rules, at educational seminar on regulatory issues for hedge fund managers.
06/14/2007	Dow Jones Commodities	Reforms Of Short-Sale Rules Sensible	Spencer Jakab	Discusses SEC approval of rule repeal and research finding limited effect of short-sale rules.

Date	Source	Title	Authors	Notes and excerpts
	Service	(So Far)		
06/15/2007	WSJ	Short-Sellers May Owe ETFs Some Thanks --- Dropping of 'Uptick' Rule By SEC Comes as Growth Of Stock Baskets Is Soaring	Spencer Jakab	Similar to previous day story in Dow Jones Commodities Service.

### First news story:

The first story we found in the business press came four months after the experiment was announced. On 30 November 2004, the *Dow Jones News Service* published a short summary of a delay in launching the experiment, to let the exchanges make programming changes. This was *not* a separate story about the experiment; it was appended to a main story about another SEC rule. The story, in full:

#### Short-Sale Experiment Delay

Separately, the SEC announced that an experiment in lifting short-sale restrictions for some stocks, set to begin in January, will be delayed until May.

[SEC market regulation division director Annette] Nazareth said the additional time will allow exchanges to complete the necessary computer programming modifications. She said the SEC originally planned to have individual brokerage firms assume responsibility for that task, but exchanges volunteered to step in when brokerages balked at the timetable. "You want to make sure the programming is right," said [Securities Industry Association President Marc] Lackritz, who praised the SEC's temporary delay in the experiment.

The SEC approved the short-sale pilot program in July, as part of Regulation SHO, a broader package of reforms involving short sales. The one-year pilot program was to begin Jan. 3, but under an order approved Tuesday, the SEC agreed to delay it until May 2, and have it run through April 2006. All other terms of the pilot project remain the same, and all other provisions of Regulation SHO will take effect Jan. 3, as planned, Nazareth stressed.

Short selling involves sales of borrowed stock. Short sellers must replace the shares at a later date and profit if the stock price declines in the interim. Although short selling is legal, it is subject to restrictions to prevent market manipulation such as "bear raids" that drive stock prices sharply lower.

The SEC's pilot program will suspend short-sale restrictions for about 1,000 stocks in the Russell 3000 Index. Most are listed on the New York Stock Exchange or the Nasdaq Stock Market, with about 2% listed on the American Stock Exchange.

Stocks included in the pilot project include Corning Inc. (GLW), Kohl's Corp. (KSS), Marsh & McLennan Cos. (MMC), Oracle Corp. (ORCL), Peet's Coffee & Tea Inc. (PEET) and Walt Disney Co. (DIS).

Regulators hope the experiment will allow them to study the effect of trading without restrictions on short sales, possibly paving the way to lift short-selling restrictions, in whole or in part, for most actively traded stocks.

### Appendix 3: Sample Selection Details

In this Appendix, we provide sample selection details, plus covariate balance tables for the Mixed Experiment and the four sub-experiments we study: Large NYSE Large Nasdaq, Small NYSE, and Small Nasdaq.

#### Introduction

The SEC adopting release for the short-sale experiment is dated July 28, 2004 (“SEC Adopting Release”).<sup>37</sup> The experiment applies to the Russell 3000 (R3000). The R3000 list changes over time; the SEC Adopting Release states that the SEC used the list as of June 25, 2004. This date because this is when Russell reconstitutes its overall lists (last Friday in June of each year).<sup>38</sup> The R3000 list as of June 25, 2004, in turn, is based on market capitalization as of May 28, 2004.<sup>39</sup>

There are three relevant sets of firms, subject to the original experiment, which we need to recreate, both at the time of announcement (July 28, 2004) and at the experiment start (May 2, 2005):

*Fully treated firms*, which the SEC called “Category A” or “pilot” firms. The SEC published the original list of Category A firms, in July 2004 as Appendix A to the SEC Adopting Release.

Partly treated large firms, within the R1000, which the SEC called Category B firms. Unfortunately, the SEC never published an original list of Category B firms so we need to recreate this list. On April 13, 2005, SEC published updated list of Category A and Category B firms,<sup>40</sup> and a list of updates to the original Category A and Category B lists.

*Other control firms*: To our knowledge, SEC never published a list of full control firms (firms in the R2000 other than Category A firms), so we need to recreate this list as well. Note that, as the SEC explained, the Nasdaq National Market never applied short sale restrictions after trading hours. Thus, all Nasdaq firms were effectively partly treated.<sup>41</sup>

We use two principal samples. First, we construct the original sample of R3000 firms as of the July 28, 2004 announcement of the experiment (“**2004 Analysis Sample**”). This sample is relevant for research on firm reactions to the announcement, and the period between announcement and experiment launch.

For studying share price reactions, it is common to exclude very low priced stocks. We define a 2004 Share Price Reaction Subsample as the 2004 Analysis Sample, less the two firms with shares trading below \$1.

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<sup>37</sup> Source: <https://www.sec.gov/rules/other/34-50104.htm>,

<sup>38</sup> Source: <http://www-ac.northerntrust.com/content/media/attachment/data/newsletter/document/russell102004.pdf>.

<sup>39</sup> This is the trading date that Russell used to rank firms by market cap in order to create the R1000 and R2000 lists as of June 25, 2004. Sources: [https://investorshub.advm.com/boards/read\\_msg.aspx?message\\_id=3322591](https://investorshub.advm.com/boards/read_msg.aspx?message_id=3322591) and <http://boards.fool.com/russell-3000-reconstitution-effect-20862707.aspx?sort=postdate>.

<sup>40</sup> Source for Category A firms: <https://www.sec.gov/spotlight/shopilot/currentpilota41305.txt>. Source for Category B firms: <https://www.sec.gov/spotlight/shopilot/currentpilotb41305.txt>.

<sup>41</sup> See new Question B.8 in the FAQ (updated April 29, 2005), available at <http://www.sec.gov/spotlight/shopilot.htm>.



To study the effects of the experiment once launched, we also use a “**2005 Financial Analysis Sample.**” We derive this sample from the 2004 Analysis Sample, by removing firms which were no longer publicly traded when the experiment started on May 2, 2005; financial firms, utilities, and 3 firms which never filed Form 10-K reports for fiscal 2004. It is common in many finance and accounting analyses to exclude banks and other financial institutions and regulated utilities because standard financial ratios look very different for these firms and measures such as accruals are very different for these firms than firms in other sectors.

Our sample definition and cleaning steps are detailed below.

### **Step 1. 2004 Analysis Sample: Obtain and Cross-check R3000 List.**

We start with the 3000 stocks on the R3000 list as of June 30, 2004, from Bloomberg, which maintains monthly historical lists of R3000 index firms in pdf format.

#### **Cross-check**

- We obtained from Russell, via academic request, a list of the R3000 index firms as of June 30, 2004. This list contains 2998 firms. Relative to the Bloomberg list it excludes two firms (WRC, Westport Resources Corp.; and AMI, Alaris Medical Systems) that were delisted on June 28, 2004 due to M&A activity.<sup>42</sup>

#### **Validity checks:**

- The Bloomberg list of R3000 stocks as of June 30, 2004 includes all 986 original Category A pilot stocks announced by the SEC in July 2004.
- This list also includes all of the 78 original Category A and Category B stocks that SEC deleted in its April 13, 2005 summary of updates to Category A and Category B stocks.
- Earlier (May 2004) and later (July 2004) lists of R3000 firms from Bloomberg match less well to the initial Category A and Category B lists.

### **Step 2: 2004 Analysis Sample: Match R3000/SEC list to CRSP**

Merge the R3000 list from Step 1 with the Center for Research in Security Prices (CRSP) monthly stock file for June 2004, matching Russell ticker with CRSP historical trading ticker symbol (TSYMBOL). We successfully match all 3,000 firms. We associate each stock with CRSP permanent identifiers PERMNO and PERMCO.

### **Step 3: 2004 Analysis Sample: Match R3000/CRSP list to SEC Initial List**

*Fully treated (Category A) firms:* We merge the R3000/CRSP list with the 986 original Category A firms announced by the SEC in July 2004. The matching succeeds for all 986 firms. We code these firms as “SHO\_PILOT = 1”, and the remaining 2014 firms as “SHO\_PILOT = 0”. The SEC’s original list of Category A firms included only 986 firms, rather than 1,000, because the SEC excluded, prior to randomization, firms that were either not listed on the Nasdaq National

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<sup>42</sup> WRC was initially listed by the SEC as a Category A firm, but the SEC later excluded it from the short sale experiment (presumably due to delisting). The SEC picked the list of Category A (fully treated) firms from the R3000 index on **June 25, 2004**, this can explain why it included WRC in its original Category A list.

Market (Nasdaq), the New York Stock Exchange (NYSE), or the American Stock Exchange (AMEX), and thus were not subject to short sale restrictions to begin with, or which became public after April 30, 2004 (through initial public offering or spinoff). We exclude WRC (delisted on June 28, 2004) from this list, resulting in 985 Category A firms.

*Original control (Category B and original control) firms:* For these firms, we have no SEC list to rely on. Following the SEC's statement of its exclusion rules, we exclude firms that were not listed on the Nasdaq National Market, NYSE or AMEX (all traded on the Nasdaq SmallCap market). We identify these firms using historical exchange code (EXCHCD) and Nasdaq National Market Indicator (NMSIND) from the CRSP monthly stock file. Again following the SEC's statement of its exclusion rules, we also exclude firms which began trading after April 30, 2004. We identify these firms using the BEGDAT variable on CRSP.<sup>43</sup> Below, we refer to both NYSE and AMEX firms as NYSE firms.

This leaves a Mixed Experiment sample of **2,954** firms:

985 fully treated firms:

333 within R1000

652 within R2000

1,969 original control firms:

#### **Step 4: 2004 Analysis Sample: Match R3000/SEC/CRSP list to Compustat**

Merge the R3000/SEC/CRSP list from Step 3 with COMPUSTAT, using the historical PERMNO-GVKEY link around June 30, 2004 in the CRSP- COMPUSTAT Linking Table. This associates each of the 2,954 sample firms with a GVKEY, the unique firm identifier used by COMPUSTAT; no firms are lost in this step.

#### **Step 5. 2004 Analysis Sample: Create Large-Firm; Small-Firm NYSE, and Small-Firm Nasdaq subsamples**

At this point, we have a complete Mixed Experiment sample. However, additional work is needed to create the samples for the four sub-experiments.

*Identifying firms in the R1000 (for the Large Firm Experiment):* The SEC did not provide a list of Category B (partly treated firms, in the R1000) firms in its July 2004 release, but likely used the same initial list of R3000 firms as of June 25, 2004 that it used to determine the Category A firms. The first available SEC list of Category B firms is contained in an SEC spreadsheet from April 13, 2005. We sought to confirm directly from Russell the R1000 membership at May 28 or June 25, 2004; they did not respond to repeated inquiries.

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<sup>43</sup> We also exclude AMI, which was delisted prior to the experiment announcement (see above). We exclude three spinoffs after April 30, 2004, which were *included* in the SEC list of Category B firms. From examining the SEC's original list of Category A firms, and the Recreated SEC Category B List (defined below), the SEC excluded from Category A spinoffs which began trading after April 30, 2004, but *included* them in Category B. These three firms are (1) Liberty Media International (LBTYA: spinoff on 6/7/2004 from Liberty Media; (2) Hospira (HSP; spinoff on 5/3/2004 from Abbott Laboratories; (3) Genworth Financial (GNW; spinoff in May 2004 from GE).

We generate a list of “Presumed SEC Category B Firms,” as of June 25, 2004, as follows. We rank all 3,000 firms in the R3000 by market capitalization as of May 28, 2004, using data from the CRSP daily file. We treat the largest 1,000 firms by market capitalization as “Presumed R1000 Firms.” There are 333 Category A firms on this list; which we consider to be fully treated firms for the Large Firm Experiment.

We then remove these fully treated firms, as well as AMI and WRC (delisted before June 28, 2004; see above) from the Presumed R1000 Firms. This leaves 665 “Presumed SEC Category B Firms.” These logically should be the original Category B firms.

Alas, matters are not this simple. We also use the SEC’s April 13, 2005 update of the Category A and Category B lists to attempt to recreate the original SEC list of Category B shares, in a different way. This produces a somewhat different list of 666 firms (after excluding AMI).<sup>44</sup> This “Recreated SEC Category B List”:

includes 653 firms that are also within the Presumed SEC Category B Firms (651 after excluding the GNW and HSP spinoffs, see discussion above);

also includes 13 firms that are not among the Presumed SEC Category B Firms;<sup>45</sup> and

omits 11 large firms (9 NYSE; 2 Nasdaq) that are among the Presumed SEC Category B Firms.<sup>46</sup>

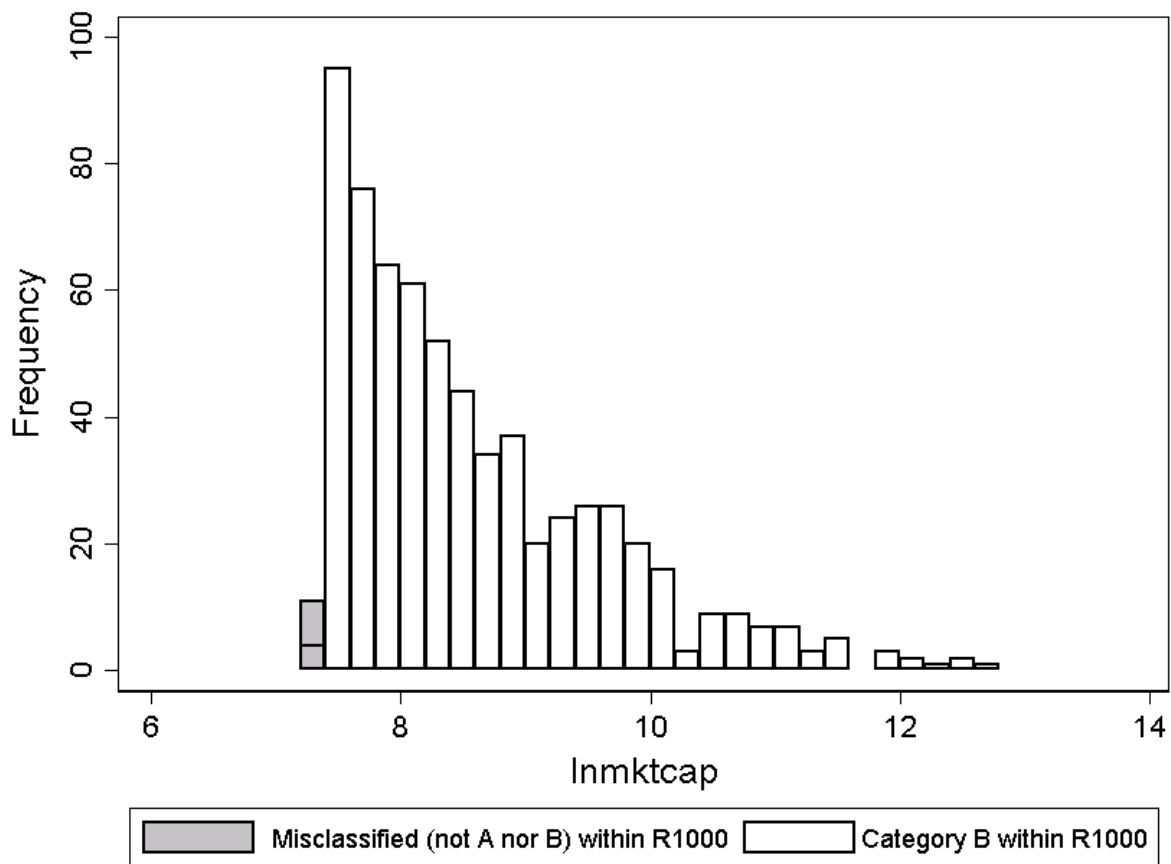
All R1000 firms with  $\ln(\text{market cap})$  of 7.37 or higher as of May 28, 2004, are either Category A firms, or else appear in *both* the Presumed SEC Category B Firms and the Recreated SEC Category B List. The discrepancies are for slightly smaller firms, near the size boundary between the R1000 and R2000. The 11 Presumed SEC Category B Firms that are not in the presumed R1000 Firms all have  $\ln(\text{market cap})$  between 7.2 and 7.37 as of May 28, 2004. See histogram below:

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<sup>44</sup> To recreate the presumed original SEC list of Category B firms, we start with the SEC’s updated list of 652 Category B stocks as of April 13, 2005, add back 22 firms that the SEC deleted in its April 13, 2005, summary of updates, and exclude 7 firms that SEC added in its summary of updates. This results in 667 Category B firms (666 after excluding AMI) as of July 28, 2004.

<sup>45</sup> These 13 firms are : BBI (Blockbuster Inc.; NYSE); BLK (Blackrock Inc.; NYSE); CXR (Cox Radio Inc.; NYSE); DYN (Dynegy Inc.; NYSE); HEW (Hewitt Associates Inc.; NYSE); HTV (Hearst Argyle Television Inc.; NYSE); IDT (IDT Corp.; NYSE); JNC (Nuveen Investment Inc.; NYSE); MNI (McClatchy Co.; NYSE); RGC (Regal Entertainment Group; NYSE); ISCA (Int’l Speedway Corp.; Nasdaq); ROIAK (Radio One Inc.; Nasdaq); VRTS (Veritas Software Corp.; Nasdaq).

<sup>46</sup> These 11 firms are: ARG (Airgas Inc.; NYSE); CPO (Corn Products Int’l; NYSE); CRE (CarrAmerica Realty Corp; NYSE); IEX (Idex Corp; NYSE); NUS (NU Skin Enterprises; NYSE); O (Realty Income Corp.; NYSE); PRK (Park National Corp.; NYSE); STE (Steris Corp.; NYSE); TTN (Titan Corp.; NYSE); FMBI (First Midwest Bancorp DE; Nasdaq); TECH (Techne Corp., Nasdaq).



Based on this assessment, we initially define “Partly Treated Large Firms” (for the Large Firm Experiment) using the 651 firms included in both the Recreated SEC Category B List and the Presumed SEC Category B Firms.

For the R2000 firms in the Mixed Experiment, we know which are Category A, and for all firms, we know which exchange they trade on. We have 258 small, Category A NYSE firms and 394 small, Category A Nasdaq firms. Among the controls within the R2000, there are 506 NYSE and 788 Nasdaq firms. This produces the following initial division of the 2,954 firms in the Mixed Experiment among the three sub-experiments. In the table below, we have shaded the cells containing (i) the 13 firms (10 NYSE, 3 Nasdaq) that are on the Recreated SEC Category B List are too small to be within the Presumed R1000 Firms); and the 11 firms (9 NYSE, 2 Nasdaq) that are in the Presumed R1000 Firms but which the SEC does not list as either Category A or Category B. We examine these firms further in Step 6.

### Initial Division of 2004 Analysis Sample into Subsamples

Gray shading indicates apparently misclassified firms: Either listed by SEC as within category B but too small to be within the Presumed R1000 Firms; or within the Presumed R1000 Firms but not listed by the SEC as within either Category A or Category B.

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Mixed Experiment
Fully treated: Category A	258	394	255	78	985
Partly treated: Category B	10	2	501	151	664
Full control: Not A nor B - NYSE	506	0	9	0	515
Partly treated: Not A nor B - Nasdaq	0	788	0	2	790
<b>Total</b>	<b>774</b>	<b>1,184</b>	<b>765</b>	<b>231</b>	<b>2,954</b>

### Step 6. 2004 Analysis Sample: Enforce Common Support on Market Capitalization

Next, we have to decide how to handle the apparently misclassified firms in the table above, for purposes of the four sub-experiments. Consider first the 10 NYSE firms that the SEC included in Category B, even though they are smaller than any other R1000 firms based on market capitalization at May 28, 2004. These firms have  $\ln(\text{market cap})$  between 6.23 and 7.25.<sup>47</sup> But the next smallest firm in the Large Firm NYSE Experiment has  $\ln(\text{market cap}) = 7.37$ . We do not want to include these 10 firms in the Large Firm NYSE Experiment, because the sample would then lack common support on market capitalization at May 28, 2004. These firms also do not fit the Small-Firm NYSE Experiment, because they are partly treated, while the other control firms in this experiment are full controls. We therefore exclude them from both the NYSE sub-experiments, but retain them in the Mixed Experiment sample.

We exclude the 9 large NYSE firms within the Presumed R1000 Firms from the Large Firm NYSE Experiment, but not on the Recreated SEC Category B list, because they are full controls rather than partly treated. We also cannot include them in the Small Firm NYSE Experiment due to their size. These two exclusions from the NYSE sub-experiments reduce the combined sample for all four sub-experiments to 2,935, versus 2,954 for the Mixed Experiment.

We include two small Nasdaq firms, listed in the SEC's Category B, but smaller than other R1000 firms, in the Small Firm Nasdaq Experiment. Finally, we include the two large Nasdaq firms within the Presumed R1000 Firms, but not on the Recreated SEC Category B list, in the Large-Firm Experiment, since they are partly treated by virtue of being Nasdaq firms, even though the SEC did not list them as Category B firms.

These choices produce a "2004 Analysis Sample," as of July 28, 2004, when the SEC announced the short sale experiment. Using the same categories as the table above:

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Total (all 4 Expt's)	Mixed Experiment
Fully treated: Category A	258	394	255	78	985	985
Partly treated: Category B	0	2	501	151	654	664
Full control: Not A nor B - NYSE	506	0	0	0	506	515
Partly treated: Not A nor B - Nasdaq	0	788	0	2	790	790
<b>Total</b>	<b>764</b>	<b>1,184</b>	<b>756</b>	<b>231</b>	<b>2,935</b>	<b>2,954</b>

<sup>47</sup> Among these 10 firms, Nuveen Investments Inc. (JNC) is the smallest with  $\ln(\text{market cap}) = 6.230$ , and McClatchy Co. (MNI) is the largest with  $\ln(\text{market cap}) = 7.247$ .

Reorganizing and simplifying this table into three rows, instead of four, based on whether a firm is fully treated, partly treated, or full control, the 2004 Analysis Sample is:

### 2004 Analysis Sample

Size range in US \$ millions, as of May 28, 2004.

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Total (all 4 Expt's)	Mixed Experiment
Fully treated (Category A)	258	394	255	78	985	985
Partly treated (Category B or Nasdaq)	0	790	501	153	1,444	1,454
Full control: NYSE	506	0	0	0	506	515
<b>Subtotal: original control firms</b>					<b>1,950</b>	<b>1,969</b>
<b>Size range (<i>ln</i>(market cap))</b>	4.33-7.36	4.51-7.36	7.37-12.76	7.37-12.55	4.33-12.76	4.33-12.76
<b>Total</b>	<b>764</b>	<b>1,184</b>	<b>756</b>	<b>231</b>	<b>2,935</b>	<b>2,954</b>

### 2004 Share Price Reaction Sample

To study share price reaction to the 2004 announcement of the experiment, we remove from the 2004 Analysis Sample two small firms, both NYSE firms, with share prices under \$1 as of June 30, 2004. There are only two such firms because the R3000 is based on market capitalization.<sup>48</sup> This produces a 2004 Share Price Reaction Sample, summarized in the next table.

### 2004 Share Price Reaction Sample

Size range in US \$ millions, as of May 28, 2004.

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Total (all 4 Expt's)	Mixed Experiment
Fully treated (Category A)	258	394	255	78	985	985
Partly treated (Category B or Nasdaq)	0	790	501	153	1,444	1,454
Full control: NYSE	504	0	0	0	504	513
<b>Size range (<i>ln</i>(market cap))</b>	4.33-7.36	4.51-7.36	7.37-12.76	7.37-12.55	4.33-12.76	4.33-12.76
<b>Total</b>	<b>762</b>	<b>1,184</b>	<b>756</b>	<b>231</b>	<b>2,933</b>	<b>2,952</b>

## Step 7. Creating the 2005 Financial Analysis Sample at the start of the Experiment (May 2, 2005)

### 7.1. SEC Exclusions and Changes

The short-sale experiment began on May 2, 2005. As noted above, the SEC published an updated list of Category A and Category B firms on April 13, 2005, which reflected mergers, bankruptcies, and other reasons why firms were excluded from Categories A and B, and also name changes. The SEC never, however, posted either, an original Category B list, nor any list of other control firms. To develop an updated sample as of the experiment onset on May 2, 2005, we update the 2004 Analysis Sample as follows:

<sup>48</sup> These firms are Eagle Broadband (EAG) (small NYSE firms with share price of 99 cents) and Terremark Worldwide (TWW) (small NYSE firm with share price of 85 cents). In the 2004 Analysis Sample, 98 firms have share price < \$5 at June 30, 2004.

**Category A (fully treated):**

- (i) Following the SEC, we exclude 38 firms due to mergers or acquisitions (8 large; 8 small NYSE; 22 small Nasdaq); and move one Nasdaq firm (ARBA, Ariba Inc.) from partly treated (small Nasdaq firm) to Category A (fully treated)<sup>49</sup>;
- (ii) We also exclude 5 firms (one small NYSE; four small Nasdaq) that ceased trading as of May 2, 2005 (no share prices on CRSP);

**Category B (partly treated):**

- (iii) Following the SEC, we exclude the 15 firms excluded by the SEC, all due to mergers or acquisitions;<sup>50</sup>
- (iv) We also exclude 5 large firms that ceased trading as of May 2, 2005;

**Small Nasdaq (partly treated):**

- (v) Following the SEC, we move ARBA from partly treated to fully treated;
- (vi) We also exclude 33 small Nasdaq firms that ceased trading as of May 2, 2005;

**Small NYSE (full control)**

- (vii) We exclude 24 small NYSE firms that ceased trading as of May 2, 2005.

These steps lead to the following preliminary 2005 sample, updated to the date of experiment, but without yet excluding financial firms and utilities:

**2005 Preliminary Sample (Before Excluding Financial Firms and Utilities)**

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Total (all 4 Expt's)	Mixed Experiment
Fully treated (Category A)	249	369	251	74	943	943
Partly treated (Category B or Nasdaq)	0	756	484	150	1,390	1,400
Full control	482	0	0	0	482	491
<b>Total</b>	<b>731</b>	<b>1,125</b>	<b>735</b>	<b>224</b>	<b>2,815</b>	<b>2,834</b>

**7.2. Firms that Change Exchange**

In defining the 2005 Financial Analysis Sample, we also need to address the nine firms in this sample which change exchange, from Nasdaq to NYSE (including Amex) or vice versa, between experiment announcement and experiment launch. The table below lists these firms, their change date, their status (Category A, Category B, or neither), and how we handle these switches. These switches do not affect the category a firm falls into (A, B, or neither), go in both directions, and are not concentrated in the period just before experiment launch. This provides comfort that treatment assignment is unlikely to have caused the switch. We ignore exchange switches after the experiment launch.

<sup>49</sup> The SEC did not explain this move, but it was likely because ARBA acquired a Category A firm, FMKT, as of July 1, 2004. See <http://edgar.secdatabase.com/1967/119312504118896/filing-main.htm>.

<sup>50</sup> One Nasdaq firm in the R1000 is not on the SEC's Category A or Category B lists: UTSI (UTStarcom), perhaps because its ticker changes to UTSIE as of April 7, 2005. We treat this firm as large and partially treated.

Firm	Old Ticker	Switch from/to	Switch Date	SEC category	Pre-Switch Expt	Post-Switch Expt
Modine Manufacturing	MODI	Nasdaq to NYSE	Oct. 14, 2004	A	Small Nasdaq	Small NYSE
Knight Transportation	KNGT	Nasdaq to NYSE	Dec. 30, 2004	A	Small Nasdaq	Small NYSE
Ptek Holdings	PTEK	Nasdaq to NYSE	Jan. 3, 2005	none	Small Nasdaq	Small NYSE
Technical Olympic USA	TOUS	Nasdaq to NYSE	Nov. 9, 2004	none	Small Nasdaq	Small NYSE
Headwaters Inc.	HDWR	Nasdaq to NYSE	April 6, 2005	none	Small Nasdaq	Small NYSE
Kindred Healthcare	KIND	Nasdaq to NYSE	Oct. 26, 2004	none	Small Nasdaq	Small NYSE
E Z EM	EZM	NYSE to Nasdaq	April 12, 2005	none	Small NYSE	Small Nasdaq
Bioenvision	BIV	NYSE to Nasdaq	Aug. 20, 2004	none	Small NYSE	Small Nasdaq
Sears Roebuck	S	NYSE to Nasdaq	March 28, 2005	B	Large Firm	Large Firm

Adjusting for these changes produces a revised preliminary 2005 Sample:

### Preliminary 2005 Sample (Before Excluding Financial Firms and Utilities)

Size range is in US \$ millions, as of May 28, 2004.

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Total (all 4 Expt's)	Mixed Experiment
Fully treated (Category A)	252	366	252	73	943	943
Partly treated (Category B or Nasdaq)	0	754	483	151	1,388	1,398
Full control	484	0	0	0	484	493
<b>Size range (<i>ln</i>(market cap))</b>	4.33-7.36	4.51-7.36	7.37-12.76	7.37-12.55	4.33-12.76	4.33-12.76
<b>Total</b>	<b>736</b>	<b>1,120</b>	<b>735</b>	<b>224</b>	<b>2,815</b>	<b>2,834</b>

It would be also possible to combine the Large Firm and Small Firm Nasdaq experiments, since both compare fully treated to partly treated firms. The practical value of doing so is unclear, since we expect that if any effects are found in our reexamination, they will be most likely for the Small Firm NYSE Experiment. Our judgment, at the design stage, is not to create and study this additional subsample.

### 7.3. Excluding Financial Firms and Utilities

We next exclude firms financial and utility firms using historical SIC codes from Compustat for the 2004 fiscal year. We also exclude three firms that did not file annual reports on Form 10-K for their 2004 fiscal years and were delisted during 2005.<sup>51</sup> The table below shows how many firms were removed for the three sub-experiments taken together (numbers of firms would be slightly larger for the Mixed Experiment).

Category	No. of firms
<b>Preliminary 2005 Sample, less</b>	<b>2,815</b>
financial firms (SIC 6000-6999)	606
utility firms (SIC 4900-4999)	105
Firms that did not file 10-K for fiscal 2004	3
<b>2005 Financial Analysis Sample</b>	<b>2,101</b>

<sup>51</sup> The three firms are Orthodontic Centers America (OCA; Category A, small NYSE); Standard Commercial Corp (STW; small NYSE firm, full control) and 3) Collins and Aikman Corp (CKC; small NYSE firm, full control).



Note that the R3000, by construction, does not contain non-U.S. firms. The next table summarizes the final 2005 Financial Analysis Sample.

### 2005 Financial Analysis Sample

Size range in US \$ millions, as of May 28, 2004.

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Total (all 4 Expt's)	Mixed Experiment
Fully treated (Category A)	180	288	176	58	702	702
Partly treated (Cat. B or Nasdaq)	0	602	316	128	1,046	1,054
Full control	353	0	0	0	353	359
<b>Subtotal: original control firms</b>					<b>1,399</b>	<b>1,413</b>
<b>Size range (<math>\ln(\text{market cap})</math>)</b>	4.33-7.36	4.51-7.36	7.37-12.76	7.37-12.55	4.33-12.76	4.33-12.76
<b>Total</b>	<b>533</b>	<b>890</b>	<b>492</b>	<b>186</b>	<b>2,101</b>	<b>2,115</b>

The 2005 Financial Analysis could potentially have drifted away from common support on market capitalization, if a few firms in the treated or control group grew or shrank unusually rapidly. In fact, based on market capitalization at May 2, 2005, all large treated firms were within the support of the large control firms; all small Nasdaq treated firms were within the support of the small partly treated Nasdaq firms, and only one small treated NYSE firm was outside within the support of the small control NYSE firms.<sup>52</sup> We chose not to trim the subsamples again to common support (which would have eliminated only this one treated firm).

## 8. Covariate Balance

We next need to confirm whether we have covariate balance for each of these experiments, for the 2004 and 2005 Financial Analysis Sample.<sup>53</sup> In order to conserve space, this analysis is reported only for the Mixed Experiment and Small-firm NYSE experiment but was conducted for all the sub-samples for the 2004 and the 2005 Financial Analysis samples. The results are consistent with balance between treated and control firms. For example, for the 2005 Financial Analysis sample only one variable out of 25 (EBIT/Assets) is significantly different between pilot and control firms at the 5% level. The covariate balance tables are presented at the end of this document.

## 9. Assessing Attrition

A threat to validity for any causal design is differential attrition, which could potentially be caused by the treatment. It seems unlikely that being fully treated is a factor powerful enough to affect attrition. We can confirm this by measuring how many firms in the 2005 Financial Analysis Sample are still publicly traded when the experiment ends. The experiment ends on July 6, 2007; we measure the remaining sample at June 29, 2007 (last trading day in June). We find no evidence for differential attrition of fully treated firms during the experiment period.

<sup>52</sup> That firm, Boyd Gaming Corp (BYD) had  $\ln(\text{market cap})$  at May 2, 2005 of 8.44; the largest full control NYSE firm, Quicksilver Resources Inc. (KWK) had  $\ln(\text{market cap})$  of 7.92.

<sup>53</sup> We did not separately assess covariate balance for the Share Price Reaction Sample because it is only slightly smaller than the 2004 Analysis Sample, from which it is drawn.

### Firms from 2005 Financial Analysis Sample Remaining at June 29, 2007

Table shows number of firms in the 2005 Financial Analysis Sample, Large Firm, Small Firm NYSE, and Small Firm Nasdaq experiments at June 29, 2007 (experiment end). Size range is in US \$ millions, as of May 28, 2004. Changes in principal exchange from that in 2005 Analysis Sample are ignored.

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Mixed Experiments
Fully treated (Category A)	159	243	155	50	607
Partly treated (Category B or Nasdaq)	0	496	288	112	904
Full control	296	0	0	0	301
<b>Total</b>	<b>455</b>	<b>739</b>	<b>443</b>	<b>162</b>	<b>1,812</b>

### Fractional Attrition of 2005 Financial Analysis Sample During Experiment Period

Table shows fractional attrition of firms in 2005 Financial Analysis Sample, Large Firm, Small Firm NYSE, and Small Firm Nasdaq experiments, from May 5, 2005 through June 29, 2007. Last row provides z-statistic for test of two-sample difference in proportions. z-statistic for mixed experiment is for fully treated vs. original controls.

Experiment	Small NYSE	Small Nasdaq	Large NYSE	Large Nasdaq	Mixed Experiments
Fully treated (Category A)	0.117	0.156	0.119	0.138	0.135
Partly treated (Category B or Nasdaq)	-	0.176	0.089	0.125	0.142
Full control	0.161	-	-	-	0.162
<b>Average for all firms</b>	<b>0.146</b>	<b>0.170</b>	<b>0.100</b>	<b>0.013</b>	<b>0.143</b>
<b>z-statistic</b>	<b>1.38</b>	<b>0.74</b>	<b>1.09</b>	<b>0.24</b>	<b>0.73</b>

## Pretreatment “Covariate Balance” Tables

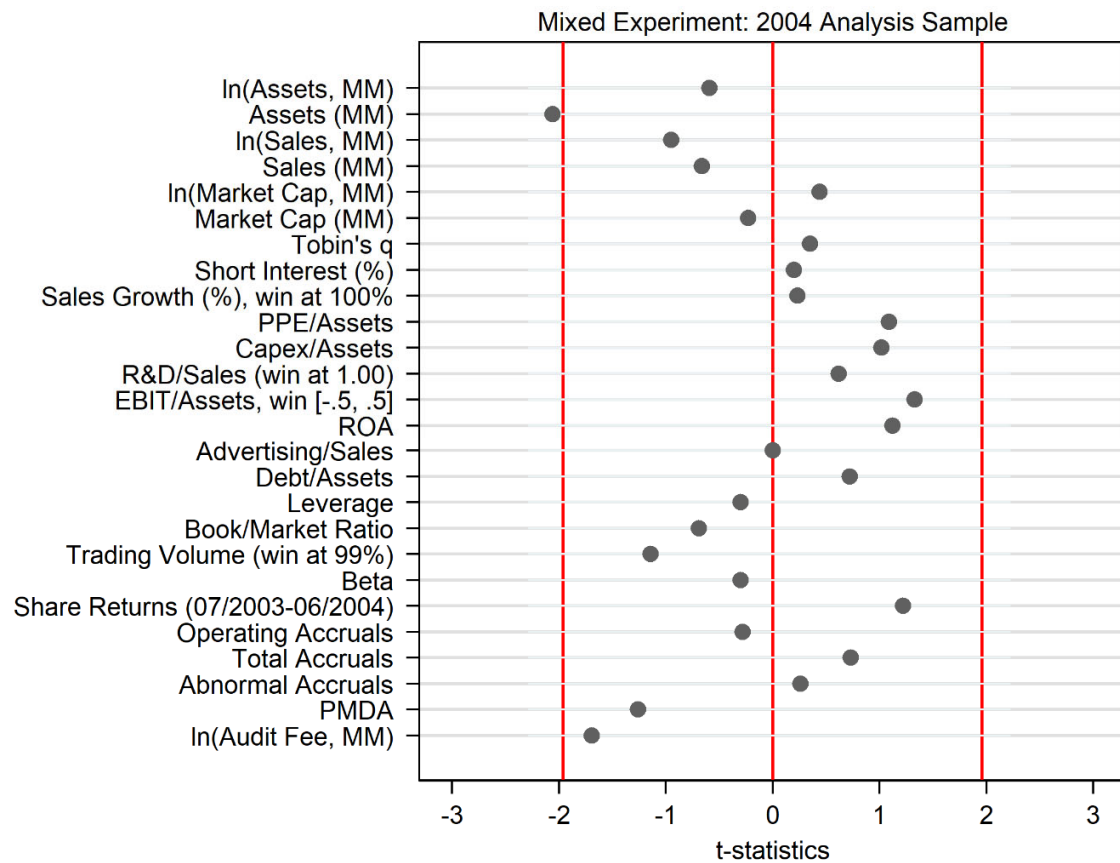
See below for detailed variable definitions. Except as stated in those definitions, we assess balance for the 2004 Analysis Sample using the most recent Compustat “datadate” before July 2004, and assess balance for the 2005 Financial Analysis sample using the most recent Compustat datadate before May 2005. These tables show, for selected outcome and financial variables, mean and median for treated and control firms, together with a two-sample *t*-statistic for difference in means, and a z-statistic from a rank-sum test for difference in medians. Sample for most variables is indicated in the top row of each table; sample is slightly smaller for some variables due to missing data on Compustat. \*, \*\*, \*\*\* indicates statistical significance at the 10%, 5%, and 1% levels; significant results at 5% or better are in **boldface**. Financial data is from Compustat; trading volume and share returns are from CRSP.

### 2004 Analysis Sample

#### Covariate Balance Table for Mixed Experiment

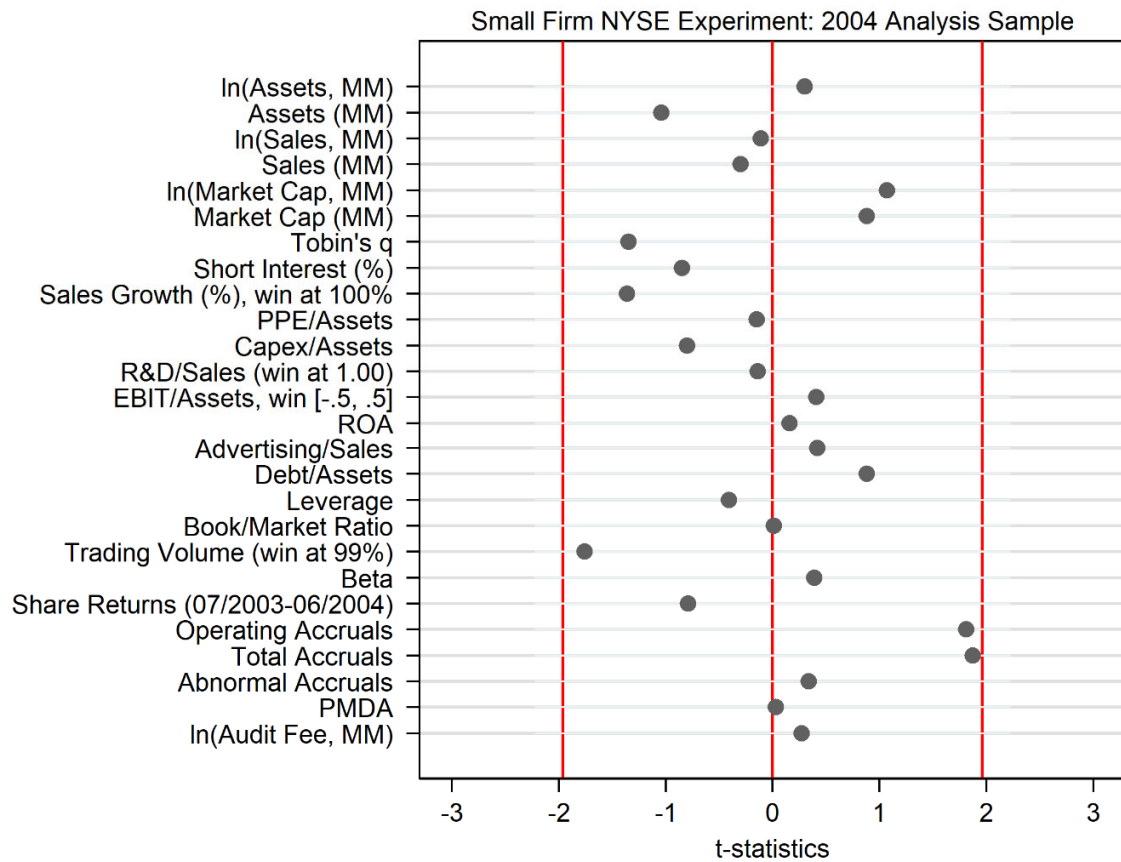
Number of firms	Original Controls		Fully Treated		Norm. Diff.	<i>t</i> -test for Means	Rank-sum test
	1,969		985				
	Mean	Median	Mean	Median			
<i>ln</i> (Assets, \$M)	7.024	6.900	6.983	6.832	-0.023	-0.59	-0.19
Assets, \$M	9,818	992	5,724	927	-0.089	<b>-2.06**</b>	-0.19
<i>ln</i> (Sales, \$M)	6.352	6.320	6.283	6.283	-0.037	-0.95	-0.84
Sales, \$M	3,068	548	2,786	532	-0.026	-0.66	-0.77
<i>ln</i> (Market Cap, \$M)	7.022	6.738	7.046	6.757	0.017	0.44	0.75
Market Cap, \$M	4,534	844	4,377	860	-0.009	-0.23	0.75
Tobin's <i>q</i>	2.180	1.630	2.203	1.643	0.014	0.35	0.12
Short Interest (%) (07/2003-06/2004)	3.820	2.194	3.859	2.224	0.008	0.20	0.29
Sales Growth (%), win at 100%	12.902	9.511	13.325	8.886	0.010	0.23	-1.05
PPE/Assets	0.217	0.138	0.227	0.147	0.043	1.09	1.34
Capex/Assets	0.040	0.026	0.042	0.028	0.041	1.02	1.29
R&D/Sales (win at 1.00)	0.076	0.000	0.081	0.000	0.024	0.62	-0.75
EBIT/assets, win [-.5, .5]	0.051	0.060	0.057	0.064	0.053	1.33	1.27
ROA	0.084	0.095	0.091	0.098	0.045	1.12	0.94
Advertising/Sales	0.012	0.000	0.012	0.000	0.000	0.00	-0.74
Debt/Assets	0.224	0.187	0.231	0.198	0.028	0.72	0.84
Leverage	0.359	0.321	0.354	0.323	-0.011	-0.30	-0.21
Book/Market Ratio	0.509	0.417	0.439	0.422	-0.031	-0.69	0.41
Trading Volume (win at 99%)	0.174	0.126	0.167	0.126	-0.045	-1.14	-0.89
Beta	1.306	1.197	1.299	1.201	-0.012	-0.30	0.02
Share Returns (07/2003-06/2004)	0.433	0.301	0.468	0.300	0.045	1.22	0.29
Operating Accruals	-0.063	-0.054	-0.064	-0.051	-0.011	-0.28	0.24
Total Accruals	0.060	0.009	0.074	0.015	0.030	0.73	1.39
Abnormal Accruals (AA)	0.063	0.057	0.065	0.063	0.011	0.26	0.34
PMDA	-0.011	-0.009	-0.028	-0.009	-0.055	-1.26	-0.39
<i>ln</i> (Audit Fees, \$M)	13.321	13.179	13.245	13.118	-0.068	-1.69*	-1.60

The figure below summarizes balance for this sample in graphical form. Vertical lines show t-statistics of -1.96, 0, and +1.96. Similar figures accompany other balance tables below.



### Covariate Balance Table for Small Firm NYSE Experiment

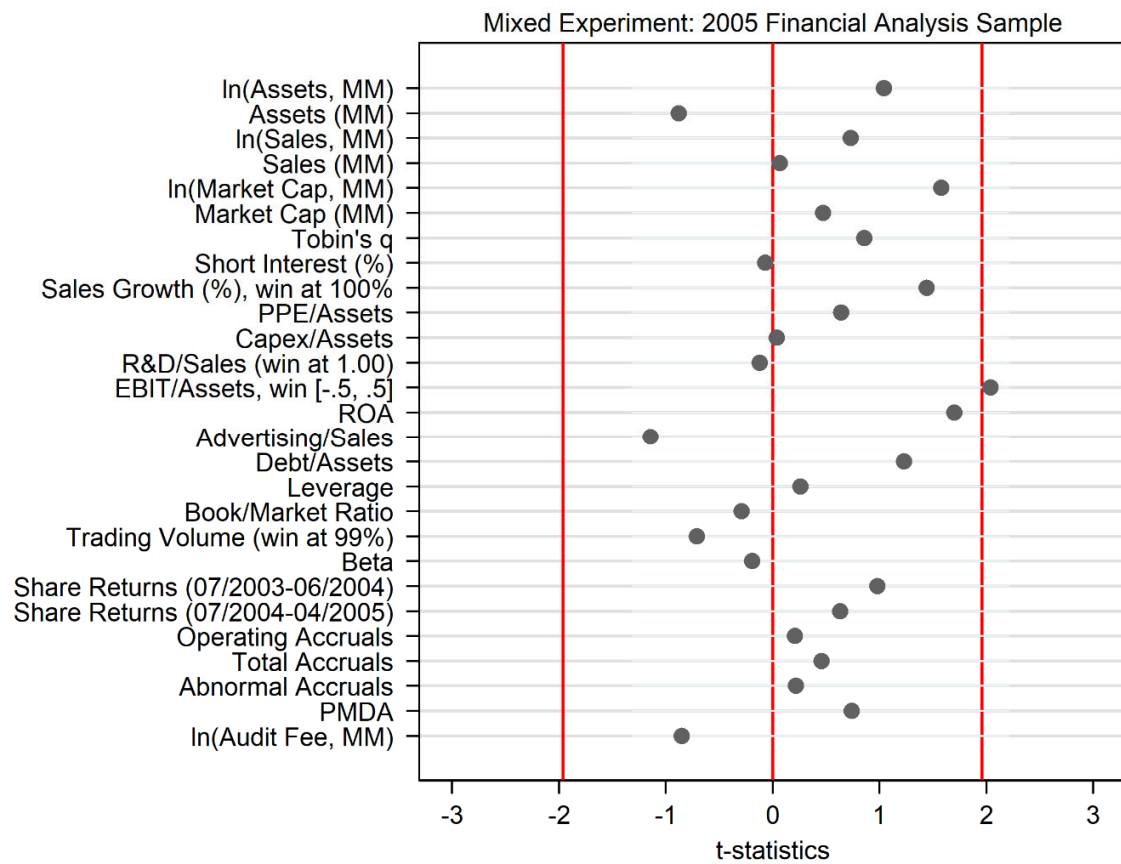
	Full Controls		Fully Treated		Norm. Diff.	t-test for Means	Rank-sum test
	506		258				
Number of firms	Mean	Median	Mean	Median			
<i>ln</i> (Assets, \$M)	6.717	6.679	6.741	6.734	0.024	0.30	0.50
Assets, \$M	1,596	796	1,385	841	-0.085	-1.04	0.50
<i>ln</i> (Sales, \$M)	6.222	6.334	6.210	6.363	-0.009	-0.11	0.07
Sales, \$M	998	562	962	580	-0.023	-0.30	0.11
<i>ln</i> (Market Cap, \$M)	6.426	6.489	6.477	6.534	0.082	1.07	1.06
Market Cap, \$M	740	658	768	688	0.067	0.88	1.06
Tobin's q	1.679	1.323	1.544	1.309	-0.114	-1.35	-0.40
Short Interest (%) (07/2003-06/2004)	3.917	2.104	3.569	1.948	-0.070	-0.85	-1.06
Sales Growth (%), win at 100%	12.623	7.982	9.976	5.217	-0.106	-1.36	<b>-2.35**</b>
PPE/Assets	0.286	0.222	0.283	0.220	-0.012	-0.15	-0.06
Capex/Assets	0.043	0.029	0.040	0.026	-0.063	-0.80	-0.49
R&D/Sales (win at 1.00)	0.018	0.000	0.017	0.000	-0.010	-0.14	-0.78
EBIT/assets, win [-.5, .5]	0.065	0.066	0.068	0.064	0.033	0.41	0.00
ROA	0.102	0.104	0.103	0.096	0.013	0.16	-0.41
Advertising/Sales	0.012	0.000	0.013	0.000	0.029	0.42	-0.35
Debt/Assets	0.286	0.265	0.301	0.266	0.067	0.88	0.59
Leverage	0.436	0.403	0.420	0.411	-0.034	-0.41	0.09
Book/Market Ratio	0.534	0.520	0.534	0.547	0.001	0.01	0.50
Trading Volume (win at 99%)	0.137	0.105	0.122	0.103	-0.140	-1.76*	-1.29
Beta	1.207	1.173	1.222	1.149	0.029	0.39	-0.12
Share Returns (07/2003-06/2004)	0.447	0.328	0.413	0.294	-0.061	-0.79	-0.73
Operating Accruals	-0.063	-0.054	-0.049	-0.048	0.148	1.81*	1.36
Total Accruals	0.028	0.003	0.094	0.004	0.125	1.87*	0.28
Abnormal Accruals (AA)	0.052	0.045	0.057	0.052	0.030	0.34	0.27
PMDA	-0.007	-0.011	-0.007	-0.002	0.003	0.03	0.21
<i>ln</i> (Audit Fees, \$M)	13.159	13.138	13.177	13.111	0.021	0.27	-0.06



## 2005 Financial Analysis Sample

### Covariate Balance Table for Mixed Experiment

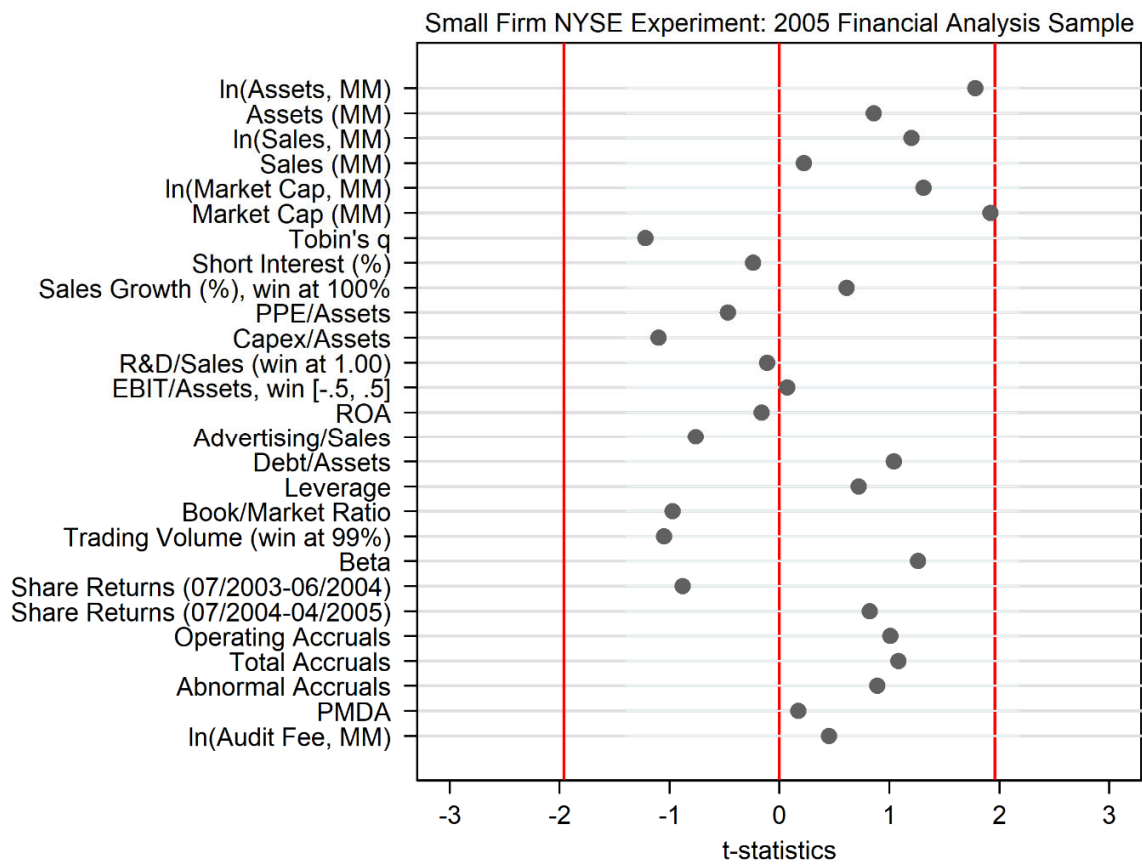
Number of firms	Original Controls		Fully Treated		Norm. Diff.	t-test for Means	Rank-sum test
	1,413		702				
	Mean	Median	Mean	Median			
<i>ln</i> (Assets, \$M)	6.724	6.546	6.799	6.662	0.048	1.04	1.28
Assets, \$M	4,514	697	3,571	782	-0.045	-0.88	1.28
<i>ln</i> (Sales, \$M)	6.474	6.491	6.539	6.598	0.034	0.73	0.70
Sales, \$M	3,539	657	3,582	731	0.003	0.07	0.77
<i>ln</i> (Market Cap, \$M)	6.890	6.666	6.999	6.792	0.073	1.58	1.78*
Market Cap, \$M	4,467	785	4,871	890	0.022	0.47	1.78*
Tobin's q	2.307	1.825	2.369	1.849	0.039	0.86	0.30
Short Interest (%) (05/2004-04/2005)	4.870	3.404	4.852	3.146	-0.003	-0.07	-0.20
Sales Growth (%), win at 100%	18.938	13.798	20.706	14.654	0.067	1.44	1.28
PPE/Assets	0.238	0.164	0.244	0.177	0.029	0.64	0.99
Capex/Assets	0.049	0.031	0.049	0.033	0.002	0.04	0.42
R&D/Sales (win at 1.00)	0.100	0.009	0.099	0.005	-0.005	-0.12	-1.51
EBIT/assets, win [-.5, .5]	0.062	0.082	0.075	0.087	0.095	<b>2.04**</b>	1.84*
ROA	0.100	0.120	0.113	0.128	0.080	1.70*	1.48
Advertising/Sales	0.018	0.001	0.013	0.002	-0.060	-1.14	-0.03
Debt/Assets	0.195	0.162	0.207	0.173	0.056	1.23	1.20
Leverage	0.284	0.232	0.288	0.240	0.012	0.26	0.82
Book/Market Ratio	0.390	0.361	0.384	0.382	-0.012	-0.29	0.30
Trading Volume (win at 99%)	0.198	0.158	0.193	0.155	-0.033	-0.71	-0.58
Beta	1.446	1.358	1.442	1.399	-0.009	-0.19	-0.01
Share Returns (07/2003-06/2004)	0.487	0.338	0.524	0.336	0.043	0.98	0.09
Share Returns (07/2004-04/2005)	-0.030	-0.036	-0.020	-0.041	0.029	0.63	0.07
Operating Accruals	-0.059	-0.052	-0.058	-0.050	0.010	0.21	0.86
Total Accruals	0.077	0.026	0.083	0.030	0.021	0.46	0.53
Abnormal Accruals (AA)	0.051	0.026	0.053	0.023	0.010	0.22	-0.20
PMDA	-0.044	-0.020	-0.030	-0.012	0.037	0.74	0.89
<i>ln</i> (Audit Fees, \$M)	13.907	13.826	13.864	13.769	-0.040	-0.85	-0.84





### Covariate Balance Table for Small Firm NYSE Experiment

	Full Controls		Fully Treated		Norm. Diff.	<i>t</i> -test for Means	Rank-sum test
	Mean	Median	Mean	Median			
Number of firms	353		180				
<i>ln</i> (Assets, \$M)	6.573	6.613	6.722	6.749	0.165	1.78*	1.75*
Assets, \$M	1,100	745	1,223	854	0.078	0.86	1.75*
<i>ln</i> (Sales, \$M)	6.512	6.548	6.650	6.717	0.113	1.20	1.39
Sales, \$M	1,268	698	1,306	827	0.021	0.22	1.39
<i>ln</i> (Market Cap, \$M)	6.414	6.504	6.507	6.667	0.119	1.31	1.51
Market Cap, \$M	778	668	874	786	0.171	1.92*	1.51
Tobin's <i>q</i>	1.849	1.511	1.722	1.442	-0.116	-1.22	-0.89
Short Interest (%) (05/2004-04/2005)	4.954	3.280	4.823	2.870	-0.024	-0.24	-0.34
Sales Growth (%), win at 100%	16.529	12.256	17.847	11.794	0.054	0.61	0.23
PPE/Assets	0.307	0.240	0.297	0.235	-0.043	-0.47	-0.39
Capex/Assets	0.057	0.033	0.050	0.033	-0.107	-1.10	0.08
R&D/Sales (win at 1.00)	0.024	0.000	0.023	0.000	-0.010	-0.11	-1.06
EBIT/assets, win [-.5, .5]	0.085	0.080	0.085	0.080	0.007	0.07	-0.41
ROA	0.128	0.118	0.126	0.115	-0.015	-0.16	-0.63
Advertising/Sales	0.015	0.000	0.012	0.000	-0.076	-0.76	0.85
Debt/Assets	0.241	0.227	0.260	0.242	0.094	1.04	0.72
Leverage	0.355	0.335	0.377	0.351	0.067	0.72	0.79
Book/Market Ratio	0.461	0.468	0.395	0.467	-0.077	-0.97	0.33
Trading Volume (win at 99%)	0.162	0.134	0.151	0.122	-0.100	-1.05	-0.59
Beta	1.408	1.339	1.459	1.429	0.114	1.26	1.42
Share Returns (07/2003-06/2004)	0.519	0.377	0.471	0.357	-0.082	-0.88	-0.45
Share Returns (07/2004-04/2005)	0.030	0.022	0.057	0.020	0.074	0.82	0.52
Operating Accruals	-0.055	-0.047	-0.044	-0.042	0.100	1.01	1.33
Total Accruals	0.062	0.015	0.087	0.024	0.096	1.08	0.81
Abnormal Accruals (AA)	0.040	0.019	0.058	0.024	0.083	0.89	0.80
PMDA	-0.007	-0.005	0.002	0.001	0.017	0.17	1.14
<i>ln</i> (Audit Fees, \$M)	13.839	13.853	13.877	13.814	0.043	0.45	0.31



## Variable Definitions

Balance sheet and income statement values are from Compustat Annual. Except as specified below, we use the most recent data before July 2004 for the 2004 Analysis Sample, and the most recent data before May 2005 for the 2005 Analysis Sample. Compustat variable names are indicated below.

Variables	Definitions
Assets (\$M)	Total Assets (AT)
Sales (\$M)	Total Sales (REVT). Negative Total Sales are replaced with 0.
Market Cap (\$M)	Market Capitalization ( $PRC \times SHROUT/1000$ ). Based on price and shares outstanding from CRSP as of June 30, 2004 for 2004 Analysis Sample, and as of April 29, 2005 for 2005 Analysis Sample.
Tobin's $q$	Tobin's $q$ , defined as total assets (AT) minus book value of common equity (CEQ) and deferred tax (TXDB), plus market capitalization, scaled by total assets. $(AT - CEQ - TXDB + (PRCC\_C \times CSHO)/AT)$
Short Interest (% of Shares Outstanding)	Average % <i>monthly open short interest</i> during 12 months from July 2003 to June 2004 (for 2004 Analysis Sample) or from May 2004 to April 2005 (for 2005 Analysis Sample), defined as monthly short interest reported on 15th of each month (from Compustat) scaled by shares outstanding at the start of the month (from CRSP) $\times 100$ $(100 \times SHORTINT/(SHROUT \times 1000))$ .
Sales Growth (%)	Sales Growth $(100 \times ((REVT_t/REVT_{t-1}) - 1))$ . Winsorized at 100%.
PPE/Assets	Property, Plant, and Equipment scaled by Total Assets ( $PPENT/AT$ )
Capex/Assets	Capital Expenditures scaled by Total Assets ( $CAPX/AT$ )
R&D/Sales	R&D scaled by Total Sales ( $XRD/REVT$ ). Missing R&D is replaced with 0 and negative Total Sales are treated as missing. Winsorized at 1.
EBIT/Assets	Earnings before Interest and Taxes scaled by Total Assets ( $EBIT/AT$ ). Winsorized at -0.5 and 0.5.
ROA	Return on Assets, defined as operating income before depreciation scaled by total assets ( $OIBDP/AT$ ).
Advertising/Sales	Advertising Expense scaled by Total Sales ( $XAD/REVT$ ). Missing Advertising Expense is replaced with 0.
Debt/Assets	Debt-to-Asset Ratio, defined as short-term debt plus long-term debt scaled by total assets $((DLC + DLTT)/AT)$ . Missing DLC and DLTT are replaced with 0.
Leverage	Total Debt/Total Assets $((DLC + DLTT)/(DLC + DLTT + SEQ))$
Book/Market Ratio	Book-to-Market Ratio ( $CEQ/(CSHO \times PRCC\_F)$ )
Trading Volume	Average <i>fractional trading volume</i> during 12 months from July 2003 to June 2004 (for 2004 Analysis Sample) or from May 2004 to April 2005 (for 2005 Analysis Sample), defined as monthly trading volume (from CRSP) scaled by shares outstanding at the end of the month (from CRSP) $(100 \times VOL/(SHROUT \times 1000))$ . Winsorized at 99%.
Beta	Beta from regression of daily return (RET) on market value weighted return from CRSP (VWRETD) over 250 trading days preceding July 28, 2004 (for 2004 Analysis Sample) or May 2, 2005 (for 2005 Analysis Sample)
Share Returns	$\prod_i (1 + RET_i) - 1$ , where $i$ includes 12 months from July 2003 to June 2004 (for 12-month pre-announcement period) or 10 months from July 2004 to April 2005 (for 10 month period between experiment announcement and experiment launch)
Operating Accruals	Operating Accruals, defined as Earnings Before Extraordinary items and discontinued operations on the cash flow statement (IBC), minus operating cash flows (OANCF) before extraordinary items and discontinued operations (XIDOC), scaled by beginning-of-the-year total assets. $((IBC - (OANCF - XIDOC))/AT_{t-1})$
Total Accruals	Total Accruals, defined as Earnings Before Extraordinary items and discontinued operations on the cash flow statement (IBC), minus operating cash flows (OANCF) before extraordinary items and discontinued operations (XIDOC), minus investing cash flow (IVNCF), scaled by beginning-of-the-year total assets. $((IBC - (OANCF - XIDOC) - IVNCF)/AT_{t-1})$
Abnormal Accruals (AA)	Measured using the modified Jones model, as described in the test. Estimated using Compustat fyear 2003 for the 2004 Analysis Sample, and fyear 2004 for the 2005 Financial Analysis Sample
PMDA	Performance-matched discretionary accruals, measured as described in the text. Measurement periods are same as for AA.
Auditing fees	Auditing fees in \$ millions, from Audit Analytics.

#### Appendix 4. Sample Periods Used by FHK, GMW, and HHZ

**FHK:** “The sample period in our main analysis consists of nine calendar years, 2001 to 2003 (inclusive) and 2005 to 2010 (inclusive). We construct three variables to indicate three subperiods: *PRE* equals one if a firm-year’s fiscal end falls between January 1, 2001 and December 31, 2003 and zero otherwise; *DURING* equals one if a firm-year’s fiscal end falls between January 1, 2005 and December 31, 2007 and zero otherwise; and *POST* equals one if a firm-year’s fiscal end falls between January 1, 2008 and December 31, 2010 and zero otherwise.” (pp. 1262)

“Table IA.III of the Internet Appendix reports tests that yield similar results if we instead define the three subperiods as May 2001 to June 2003, May 2005 to June 2007, and May 2008 to June 2010, thus restricting the *DURING* period more closely to the actual start and end dates of the program.” (pp. 1262)

Although not stated in the FHK paper, FHK Appendix Table IA.IV also reports results if they include 2004 in their Pre period.

**GMW:** “*Before* is the period that extends three years before the announcement of the pilot program (July 28, 2004), while *After* is the three-year 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 six months during the period considered to be included in the sample.” (pp. 1751, Table 5 notes)

“*Treatment* is a dummy variable equal to one if the company is in the pilot group of Reg SHO and the fiscal year includes at least six months of activity after the announcement of Reg SHO (July 28, 2004) and equal to zero otherwise.” (pp. 1753, Table 6 notes)

**HHZ:** “Our sample period is from 2000 to 2013.” (pp. 483)

“*During* is one for observations from year 2005 to 2007 and zero otherwise; *Post* is one for observations from year 2008 to 2013 and zero otherwise. As the pilot firm list was announced on July 28, 2004 but the price tests were not removed for pilot firms until May 2, 2005, it is unclear ex ante whether auditors reacted in year 2004. Therefore, we follow prior literature and exclude all observations in year 2004.” (pp. 484)

On page 484, Table 1 has a column for “Fiscal year” and lists 2000-2003 and 2005-2013.

On page 483, footnote 11 states that “If we instead end the sample period in 2010 (as in Fang et al. (2016)), no inferences are affected.”