

Investment - Cash Flow Sensitivity Is Not Caused by Financial Constraint: New Evidence From a Randomized SEC Experiment

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Abstract

The sensitivity of firm's investment to cash flows is one of the grand unresolved puzzles of corporate finance. This sensitivity could be caused by a firm's financing constraints (and therefore reveal imperfections in capital markets), by agency problems, or by non-problematic factors. Prior papers have not been able to disentangle these causal relationships because the firm's financing constraints, investment, and cash flows are endogenous.

This paper investigates this question using a unique randomized experiment as identification. In 2005-2007, the US Securities and Exchange Commission ran a randomized trial: it randomly selected 1000 firms from the list of Russell 3000 and exempted them from the regulation of short selling, making it easier to short-sell securities of those firms. The exempted firms can be thought of as "treated" (by the experiment), and the non-exempted firms as "control". First, I show that "treated" firms raised significantly more equity during the treatment period than control firms, consistent with the view that short selling improves pricing accuracy and makes outsiders more willing to buy equity. That is, the SEC experiment provides an exogenous random shock to the firm's financial constraint. Next, I ask whether treated firms that were more financially constrained before the treatment raised more equity during the treatment period than less financially constrained treated firms. The answer is no. Third, I ask whether treated firms whose investment was more sensitive to cash flows before treatment raised more equity during treatment than firms whose investment was less sensitive to cash flows. The answer is also no. Finally, I ask whether treated firms that had higher pre-treatment cash flows raised less equity during treatment. The answer is again no – they raised more capital than treated firms with lower pre-treatment cash flows. This evidence is consistent with the view that cash-flow-investment sensitivity is not caused by financial constraints. This result has implications for corporate and securities laws, economic policy, and corporate finance theory.

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Introduction

The sensitivity of firm's investment to cash flows is one of the grand puzzles of corporate finance. The paper that started this literature – Fazzari et al. (1988) generated more than 4,000 citations, and the first response by Kaplan and Zingales generated more than 2,000 citations. This literature continues to grow and contributes to other related fields.

Fazzari et al. (1988) observed that firm's investment is correlated with its cash flows. They hypothesized that in perfect capital markets, there should be no such correlation, as firms would always be able to invest in positive NPV projects, either by using internally-generated cash or by getting outside financing. Observed sensitivity of investment to cash flow must have been caused by capital market imperfections – the firm's inability to raise external capital to finance profitable projects. This analysis claimed to produce a measure of frictions in capital markets, valuable for further theoretical and empirical work. It also had significant policy implications – the government can improve private investment by adopting the policies that reduce costs of capital raising.

To link investment-cash-flow sensitivity to the failures of capital markets, Fazzari et al. (1988) generated a measure of firm's financing constraint and showed that this measure predicts the degree of investment-cash flow sensitivity in cross-section: more financially constrained firms had higher sensitivity. A large number of follow-up studies continued with this approach – to develop firm-level measures of financing constraint and regress them against investment-cash flow sensitivity (Hovakimian and Hovakimian, 2005; Hoshi, Kashyap, Scharfstein, 1991; Whited, 1992; Calomiris and Hubbard, 1995).

Competing hypotheses were developed in Kaplan and Zingales (1997) and expanded in later studies. Kaplan and Zingales (1997) used new data sources and investigated in depth 49 firms from Fazzari et al (1988) sample that had unusually high investment-cash flow sensitivity. They found that those firms have low financing constraint, inconsistent with the Fazzari et al's hypothesis. Kaplan and Zingales' alternative explanation for the higher investment-cash-flow sensitivity for financially unconstrained firms is that the least constrained firms are the most successful ones, and the managers of the most successful firms choose to rely mostly on internal cash flows for investment. Some of the later work also finds no relationship between financing constraint and sensitivity (Rejcie, Rezaul and Qian, 2010) and concluded that agency costs are a better explanation (Pawlina and Rennebog, 2005). Further work has explored whether investment-cash flow sensitivity is linear and made other refinements (Huang, 2002).

Perhaps the most intuitive flavor of the agency-cost hypothesis for the cash-flow-investment sensitivity builds on Jensen's free cash flow theory. In contrast to the financing-frictions hypothesis, which starts with the view that firms sit idly on a pile of profitable investment opportunities that they cannot exploit because of imperfections in capital markets, the agency-cost hypothesis starts with the view that firms do not have a large pool of constantly-available positive NPV projects. When the cash arrives, firm managers start looking for ways to spend it. When good projects dry up, they invest in bad projects, just to avoid returning the cash to investors. This mechanism too would generate the correlation between cash flows and investment. It would also generate the same cross-sectional pattern

found in Fazzari et al: “high-constraint” firms (measured by low dividend rates) are run by more disloyal management (hence low dividends), and more disloyal management is more likely to waste available cash on bad investments instead of distributing it. If this alternative explanation is correct, policy prescriptions should be different: the government interested in improving investment should not concentrate on promoting credit and other forms of capital-raising, but instead should look for ways to reduce firm-level agency costs.

It is also possible that the sensitivity of investment to cash flows does not reflect any problems at all, or at least nothing that needs to be remedied. For example, if cash flows proxy for investment opportunities, then, it’s not surprising that at the time when a firm has higher cash flows, it also invests more. If this is the case, the government’s efforts to influence investment by tinkering with capital markets or by advancing the policies dealing with agency costs would only distort proper levels of investment and its allocation.

Thus, the sensitivity of investment to cash flows is an important puzzle for both finance theory and policy making. However, none of the existing empirical work in this large field has successfully addressed the core identification problem: namely, that financing constraint, investment, and sensitivity are all endogenous. One of them could affect all others; or each of them all could affect each other in complicated series of causal loops; or all of them could be affected by an omitted, perhaps unmeasurable, variable such as management quality. Simply showing, as prior literature did, that all three are correlated, or not correlated, does not tell us much about their causal relationships. None of the prior papers had identification that could disentangle causation.

This paper is the first to propose a research design that can establish reliable causation. I use the one and only true randomized experiment (not a mere “natural experiment”) conducted by the US Securities and Exchange Commission for the specific purpose of studying financial markets. In 2005-2007, the SEC ran a randomized trial: it randomly selected 1000 firms from the list of Russell 3000 and exempted them from the regulation of short selling, making it easier to short-sell securities of those firms. The 1000 exempted firms can be thought of as “treated” by the experiment, and the non-exempted 2000 firms as “control”. The randomization process was conducted transparently, and its results were posted on public government websites, encouraging research by academics and non-academics alike. Neither the affected firms nor other parties had a choice of whether to comply with the treatment protocol. With one significant caveat, the randomization allows us not to worry that “treated” firms are systematically different from “controls” in some unobserved ways.

The significant caveat is that the SEC damaged its own randomization process in the way that I discuss in this paper. To the best of my knowledge, this paper is the first to identify the damage in the SEC’s randomization procedure and suggest econometric solutions for credible causal inference.

In a companion paper (Litvak 2012), I show that treated firms raised more capital during the treatment period than non-treated firms. This is consistent with prior theoretical work in asset pricing, showing that the influx of short-sellers improves the quality of information available to the market and therefore increases investors’ willingness to invest in treated firms.

That is, we can treat the SEC experiment as a random shock to firms' ability to raise capital. This provides us with the first randomized experiment allowing to test the causes of cash-flow-investment sensitivity. In this paper, I ask: did treated firms that were more financially constrained immediately before the treatment raise more equity than treated firms that were less financially constrained, and than non-treated similarly-constrained firms? A positive answer would support the inference that cash-flow-investment sensitivity is caused by financing constraint.

Using the measures of financing constraint developed in prior literature, I find that treated firms that were more financially constrained before treatment did not raise more capital during treatment. This is consistent with the Kaplan-Zingales view that cash-flow-investment sensitivity is not caused by financing constraint.

Further, I ask: did treated firms that had a higher sensitivity of investment to cash flows prior to treatment raise more capital during treatment? This would be consistent with the Fazzari et al. view that the sensitivity is a proxy for financing constraint. I find no such relationship either. Finally, I ask whether treated firms that had higher cash flows prior to treatment raised less capital during the treatment period. The answer is no – to the opposite, they raised significantly more capital during treatment. Overall, this evidence provides support for the Kaplan-Zingales view that investment-cash-flow sensitivity is not caused by financing constraint and does not constitute evidence of capital market imperfections.

This paper seeks to contribute to several bodies of literature. First is the cash-flow-investment sensitivity debate discussed above. Second is the related literature that studies the relationship between agency cost and corporate investment. Suppose a firm receives a large exogenous influx of cash, uncorrelated with changes in investment opportunities. What will it do with it? This is very similar to my setting where treated firms received a large windfall via cheaper external financing, triggered by the random relaxation of short-selling restrictions. Here, I build on a classic study by Blanchard, Lopez-de-Silanes, and Shleifer (1994), who examine eleven firms that experienced a cash windfall and find that managers responded by changing firm assets, capital structures, dividend policy, and executive compensation, in ways broadly consistent with the predictions of the agency theories. This paper is also related to Lamont (1997), who uses a different kind of exogenous shock – changes in oil prices – to identify the investment consequences for conglomerate firms of experiencing a sudden change in the value of their collateral.

Other relevant literatures are the relationship between information and corporate investment, and between capital structure and investment. It builds on the classic Myers and Majluf (1983) model where managers are fully loyal to shareholders, but firms experience rationing on capital markets because outsiders do not know the quality of the firm's investment projects and correspondingly reduce what they are willing to pay for securities. Suppose the information quality improves due to the relaxation of the uptick rule by the SEC's randomized experiment. Will the firms raise more capital in such improved circumstances? If they do, will they then invest it or simply increase their cash holdings?

Finally, this paper adds, albeit more tangentially, to the studies of the impact of short-selling on information quality and capital raising.

The paper proceeds as follows. Part II briefly summarizes the current state of the literature in the investment-cash flow sensitivity debate, as well as the prior literature analyzing the SEC randomized experiment (known as Reg SHO). Part III discusses the SEC experiment that serves as a basis for this study, the problems with the randomization procedure, and the econometric solutions that I use in this paper to adjust for the poorly conducted randomization. Part IV presents the data and defines main variables. In Part V, I present the results of the tests. I discuss limitations and possible extensions in the Conclusion.

Part II: Literature Review

A. The Cash-Flow-Investment Sensitivity Debate and Relevant Literatures

[to be developed further]

In an ideal world, the firm's investment should depend only on the presence of positive net present value projects, and not on the availability of internally-generated cash. Thus, if we believe that in time-series, positive NPV projects arrive to each firm at times uncorrelated with the arrival of cash, then, there should be no correlation between investment and cash flows.

However, investment and cash flows are correlated. Fazzari, Hubbard, and Petersen (1988) hypothesized that firms rely on internal cash for investment when external financing is too expensive, and therefore firms lacking easy access to external capital markets should exhibit stronger correlation of cash flows and investments. They propose a proxy for financing constraint, split firms into groups according to that proxy, and show that more constrained firms exhibit more sensitivity.

A large literature followed, both theoretical and empirical. The core difficulty for the empirical leg of this research was that investment, cash flows, and financing constraints are all endogenous. The fact that all three are correlated might indicate direct causation, reverse causation, causation by an omitted variable, or all of the above simultaneously. Since most of this empirical literature was written before the causal-inference revolution of the recent years, it lacked econometric tools to tackle the endogeneity directly. Instead these studies addressed the difficulties with variable construction and interpretation, and challenged robustness of the cross-sectional relationship found by Fazzari et al. (Kaplan and Zingales 1997; Cleary 1999; Erickson and Whited 2000)

Theoretical research in this area was also affected by endogeneity. For example, there is no reason to think that cash flows and investment opportunities arrive to the firm at random and uncorrelated times; cash flow may simply be a proxy for or a signal of investment opportunities. Thus, cash flows can be correlated with investment even in the absence of any financing frictions (Gomes 2001, Alti 2003). Further work in this area introduced other theoretical complications. Money for investment can come from sources other than internally-generated cash or equity; it can, for example, be borrowed. Thus, Almeida and Campello (2002) investigate the role of credit constraints and

collaterals. They show that the sensitivity of credit-constrained firms increases with available collateral. The next question is what happens when investment is not observable by the market. Povel and Raith (2001) model this situation and find that investment and cash flows have a U-shaped relationship. They also show the role of the information asymmetry: as information asymmetry increases, so does cash-flow-investment sensitivity. Dasgupta, and Sengupta (2002) have a related model, showing non-monotonic relationship between cash flow and investment under unobservable investment. Moyen (2004) investigates the relationship between cash flows and investment in an environment of complete information, and suggest a way to reconcile the results presented by Fazzari et al (1988) and Kaplan and Zingales (1997).

A more recent literature looks at the cash-flow-investment sensitivity across countries. Bond et al. (2003) find that UK firms exhibit higher investment-cash-flow sensitivity than Germany, France, and Belgium. Thus, the newer set of hypotheses asks how the specifics of countries' financial systems affects firms' ability to deal with the problem of asymmetric information, which in turn affects the sensitivity of cash flows to investment (Mizen and Vermeulen, 2005).

B. Prior Work on the SEC's Randomized Experiment

As discussed above, the focus of this paper is the cash-flow-investment sensitivity puzzle, not short selling per se. I use the SEC's randomized experiment only as the source of identification in the study of that question. Thus, most of the literature on the effects of the experiment is not directly relevant to this paper. I nevertheless list the relevant work that puts my identification idea in a broader context.

Several prior or contemporaneous papers investigated the impact of the SEC experiment on treated firms.

The first category of papers studies the effect of the experiment on trading and basic measures of securities markets. In 2007, the SEC's own Office of Economic Analysis released a study investigating the effect of the rule on volume of short sales, option trading, prices, liquidity, volatility, market efficiency, and extreme price changes. They authors find that the abolition of the uptick rule led to higher number of short sale trades, especially for small stocks. Most other measures of short selling intensity were not affected. They also find some evidence of lower liquidity and higher volatility, mostly in small stocks. This study, along with complementary academic studies released contemporaneously, led the SEC to abolish the uptick rule in 2007.

Deither, Lee, and Werner (2006) look at the period around the adoption of the SEC's randomized trial, between February and June of 2005. They ask, first, whether the exemption from the uptick rule affects short selling, liquidity, and volatility, and second, whether the increases in short selling affect market quality. They find mixed results: for companies whose stocks are traded on NYSE, removal of the uptick rule resulted in an increase in short sale trades, but there was no effect on NASDAQ-traded companies. Likewise, liquidity improved for NYSE-traded firms, but not for NASDAQ-

traded. The experiment did not affect most volatility measures, but increased several volatility measures for the NYSE stocks.

In a related study, Alexander and Peterson (2006) measure volume and volatility around the announcement and initiation date of the pilot program. They find no significant impact on short selling for either NYSE or NASDAQ stocks. They do, however, find negative liquidity impact on several measures, primarily for the NYSE stocks. They report no effect on several measures of volatility. They also find no impact on price efficiency, with a few exceptions.

Wu (2006) uses a smaller sample of only NYSE stocks and finds increases in short selling, but only for small firms. The paper reports declines in some liquidity measures, primarily for small stocks; no changes in volatility, and no changes in price efficiency.

Bai (2006) asks a different question. One commonly cited benefit of short selling is its role in allowing market participants to quickly incorporate negative information into stock prices. One commonly studied source of new information is earning announcements. The question, then, is whether the firms exempted from the uptick rule experienced different reactions to negative earning shocks. Bai finds no such differences.

The second category of papers looked at the effect of the experiment on firm behavior. Grullon et al. (2013) ask whether the experiment led to changes in firm share prices and corresponding changes to firms' equity issuances. They find evidence for both – stock prices declined and equity issuances also declined, but only for small firms.

Angelis (2013) investigate whether the relaxation of short-listing rules led to changes in CEO compensation contracts. They find that treated firms increased the convexity of the compensation payoff of their CEOs and other senior managers. Treated firms also adopted new antitakeover provisions.

Fang et al. (2011) ask whether short selling impacts firms' reporting behavior and, through reporting, enhances share price efficiency. The answer is yes. They find that the treated firms (exempted from the uptick rule) have lower discretionary accruals during the treatment period. Conditional on committing financial misconduct, treated firms were more likely to be caught.

Li and Zhang (2014) asked whether the increase in short-selling pressure caused by the SEC's experiment affected firms' voluntary disclosure choices. They find that treated firms reduced the precision of bad news forecasts and also reduced readability of bad news annual reports.

Part III: The SEC Experiment

A. The Intended Experiment

Until July 2007, Rule 10a-1 imposed a restriction on short selling, known as the uptick rule: a listed security may be sold short, either at a price above of the immediately preceding sale (plus tick), or

at the price equal to the last sale price if it was higher than the last different price (zero-plus tick). With few exceptions, short sales on minus ticks or zero-minus ticks were prohibited. The original purpose of the rule, adopted in 1938, was to prevent short sellers from accelerating downwards price spirals. By the early 2000s, the uptick rule was widely considered counterproductive because it prevented negative information from being quickly incorporated into market prices. To investigate whether the uptick rule should be abolished, the SEC implemented its first randomized experiment, known as the Pilot Program, that ran from May 2, 2005 to July 3, 2007. During this period, the SEC's Office of Economic Analysis conducted its own study on the impact of the Pilot Program. The results of this study, together with the results of studies conducted by other parties, led the SEC to abolish the uptick rule in July 2007.

To conduct its experiment, the SEC employed the following procedure. First, it took the list of Russell-3000 firms as of 2004. It sorted the firms in the order of descending volume of trading, and picked every third firm for treatment. This is not, strictly speaking, random selection, but very close to random. Presumably, the SEC chose this procedure over a standard randomization (where each firm is assigned a number, and then, a random-number-generating algorithm selects a third) to create complete transparency in selection of treated and control firms.

Table 1 contains summary statistics for the firms that the SEC listed as treated (Pilot Firms), and all other firms in the Russell-3000 index (control firms). One can see that these firms are very similar in all essential observable characteristics: size, leverage, profitability, trading volume, the number of issued shares, and so on. Only R&D expenditures of control firms are higher than those of treated firms, but this difference is only significant at a 10% level.

Figure A presents a kernel density plot for asset sizes of treated firms (in blue) and firms that were intended to be controls (in red). One can see that the distribution of asset sizes is very similar. I obtain similar pictures when I compare kernel density plots for other firm characteristics. In short, we have every reason to believe that the third of the firms that the SEC selected as treated is in fact randomly selected and very similar in unobserved characteristics as well.

B. The Actual (Busted) Experiment

However, the SEC randomization procedure was compromised. This paper is first to discover and analyze the problem with the SEC's randomization procedure, show its consequences, and correct for the distortions introduced by the SEC's failure to randomize properly.

After completing the original randomization, the SEC took the entire group of original, randomly selected control firms (which I will call "intended-controls"), and move some of those firms into a second category of "partially treated" firms, called "Category B Pilot Securities". These firms were subject to short-sale restrictions during the trading day, but exempt from them from 4:15pm ET until the opening of trading the next day. That is, they were regulated like "controls" during the trading day and like "treated" firms in after-hours trading. The remaining, "true control" firms were designated as "Category C".

In other words, we don't merely have treated and control groups, as prior research assumed. Instead, we have fully treated, partially-treated, and true control firms. The critical question is whether the partially-treated firms were selected randomly from the pool of intended-controls. If so, there should be covariate balance across all possible pairs: fully treated versus true controls; partially treated versus full controls; and fully treated versus partially treated. Table 1B and Figure B answer this question. Fully treated firms are not at all similar to true controls: treated firms are significantly larger than true controls, by every measure. The only measure in which they are not distinct is sales growth.

Table 1C presents the same information differently: it compares the fully treated firms ("Category A") to the partially treated firms ("Category B"). The difference is dramatic. Partially treated firms are much larger than fully treated firms.

Another way to compare fully treated, partially-treated, and true control firms is to compute the propensity of each firm to be fully treated, based on observed firm characteristics. Since size was critical to the SEC's assignment of some intended-control firms to instead be partially treated, I estimated the propensity to be treated based on several different measures of size: asset size, firm market value, and equity issuance (number of issued shares over total number of shares), all as of 2004. Methodology details are discussed in the next section. Figure D presents kernel densities of propensities to be fully treated, separately for the fully treated and true control groups. Figure E presents the densities of the propensity to be partially treated, for the partially treated and true control groups. Figure F presents the densities of the propensity to be fully treated, for the fully treated and partially treated groups. Across all three figures, the densities are very different for each comparison between two groups of firms.

In short, although the SEC intended to conduct a randomized experiment, its later separation of the intended-controls into partially treated firms and true controls was not close to random. While its initial round of randomization (treated versus intended controls) was conducted properly, it appears that the SEC later removed the largest firms from the intended-control category and made them partially treated.

The solutions to this problem are not trivial. We cannot compare treated to true controls because these firms were not selected randomly. But we also cannot compare treated to intended controls (combination of true controls and partially treated firms) because intended controls did not follow the protocol for "controls" – many of them, especially the larger firms, were partially treated.

C. The Solutions to the SEC's "Busted Randomization"

Despite the SEC's busted randomization, we can still estimate an unbiased average treatment effect if the assignment to treatment (removal from the uptick rule) is as good as random, conditional on observed pretreatment covariates. In the causal inference literature this is variously called "conditional independence," "unconfounded assignment" or "selection [only] on observables."

Is it feasible to achieve unconfounded assignment, or come reasonably close? It is impossible to test unconfounded assignment directly because, by definition, we cannot measure unobserved covariates. If the SEC had allowed the intended-control firms chose for themselves whether to be partly

treated, we would worry that the firms chose based on factors known to the managers, but not captured in standard financial variables. Here, however, the choice was made by the SEC legal staff. Amazingly, they did so without telling the economists who designed the randomized experiment. (This information comes from my private correspondence with the decisionmakers involved in the SEC experiment).

However, there is no reason to think that SEC legal staff had access to firm information, beyond usual financial variables. I was able to contact several people who were at the SEC at the relevant time, and should have been knowledgeable about the SEC’s action and the basis for it. Most did not know the randomization had been busted. From those who did know, I was able to determine that the impetus for creating the partially treated group came from traders at investment banks, who wanted to trade these firms’ shares after hours, and were primarily interested in larger firms. These traders were also unlikely to have access to firm information, beyond usual financial variables, trading volume, and the like. The SEC legal staff believed that after-hours trading was a minor matter, and agreed to create Category B. Thus, it is plausible that controls for size and trading volume can largely capture the factors that influenced which intended-control firms became partially treated, and that any unobserved factors are not systematically related to the outcomes of interest for this study.

A second requirement for an unbiased estimate of the average treatment effect is to have overlap between the treated and control groups, so that one is not comparing treated firms to dissimilar control firms or vice versa.

If unconfounded assignment can be achieved, there are a number of approaches, with support in the causal inference literature, for “rebalancing” the treated and control groups, in order to recover an unbiased estimate of the average treatment effect. Below, I pursue one approach which has good robustness properties. My approach begins with “inverse propensity weighting (IPW).” Inverse propensity weighting is similar to probabilistic matching of each treated firm to the most similar control(s) and vice-versa.

(1) Inverse Propensity Weights (IPW)

One way to adjust for the effect of known covariates on whether a firm is treated, and thus recover “as if random” assignment to treatment, is to create inverse propensity weights (IPW) (Rosenbaum and Rubin, 1983 Imbens and Rubin, 2014). IPW performs well in the comparison of methods by Busso, DiNardo and McCrary (2014).

With cross-sectional data, a simple comparison of weighted means for the treated and control firms would provide an unbiased estimate of the average treatment effect. The formula is:

$$\hat{\tau}_{ATE} = \left\{ \frac{1}{\left[\sum_{j: w_j=1} \frac{1}{\hat{p}_j} \right]} * \sum_{i: w_i=1} \frac{y_i}{\hat{p}_i} \right\} - \left\{ \frac{1}{\left[\sum_{j: w_j=0} \frac{1}{(1-\hat{p}_j)} \right]} * \sum_{i: w_i=0} \frac{y_i}{(1-\hat{p}_i)} \right\} \tag{1}$$

Here p is the propensity score (the probability that a firm will be treated, based on available observable covariates). The first term within large brackets is the weighted mean for the treated firms; the weights are normalized to sum to 1. The second term is the weighted mean for the control firms. As is standard in the literature, I compute propensity scores using logit estimation. The predictor variables are $\ln(\text{assets})$, $\ln(\text{cash holdings})$, $\ln(\text{closing price})$, and $\ln(\text{trading volume})$.

This weighted mean will provide an unbiased estimate of the average treatment effect even where treatment effects are heterogenous (i.e., where treatment has a different effect on different types of firms, for example, larger versus smaller firms).

(2) Trimming

After computing propensity scores, I generally trim the sample to a region of “thick support” – to propensity values $p \in [0.1, 0.7]$. By comparison, Crump et al. (2009) suggest trimming to $[0.1, 0.9]$ as a “rule of thumb.” For my sample, I need to trim high propensities more deeply than this because there are essentially no true controls with propensities above around 0.8 (see Figure D). The lack of full control firms with high propensities to be treated is a result of the SEC’s busted randomization. The largest of the intended-control firms were made partially treated, so there are few or no true controls, that one can use as probabilistic matches for the largest treated firms.

If one does not trim the sample, a potential problem with IPW is that the estimate, while unbiased, can be strongly affected by a single treated firm with very low propensity to be treated (and hence very high weight), or a single control firm with very high propensity to be treated (thus, very low propensity to be control), which would also receive a very high weight (Kang and Schaefer, 2007). This is not a concern for this sample, the “common support” region, from the propensity score for the treated unit with the lowest propensity to the score for the control unit with the highest propensity is $[0.055, 0.86]$.

(3) Panel Regression with IPW

I have panel data, rather than cross-sectional data. I therefore combine IPW with panel regression methods, using firm fixed effects (FE) and data for 1990-2011. The FE model can be seen as a “time-demeaned” specification. Let $\mathbf{x}_{i,t}^{dm} = (\mathbf{x}_{i,t} - \bar{\mathbf{x}}_i)$ for the covariates \mathbf{x} , and similar for other variables. The FE model is, with the weights suppressed:

$$y_{it}^{dm} = (\beta_1 \times TR_t) + (\beta_2 \times TR_t \times d_i) + (\beta_3 \times \mathbf{x}_{i,t}^{dm}) + g_t^{dm} + \varepsilon_{i,t}^{dm} \quad (2)$$

Here $y_{i,t}$ is the outcome variable, TR_t is a dummy variable that equals 1 during the treatment period of 2005-2007, 0 otherwise; d_i is a dummy for treated firms, \mathbf{x} is a vector of covariates, g_t are year dummies, and $\varepsilon_{i,t}$ is the error term. The coefficient of principal interest is β_2 , on the interaction between the treatment dummy and the treatment period dummy.

The FE estimator controls for time-invariant firm-specific heterogeneity. The remaining risk, for whether β_2 provides an unbiased estimate of the average annual treatment effect, is the existence of an unobserved time-varying variable that correlates with both the treatment dummy and the outcome.¹

(4) Inverse Propensity Tilting

The IPW plus regression approach should be unbiased in the presence of heterogeneous treatment effects, but is inefficient if the treatment effects are homogeneous. An alternative approach, which has no standard name, is to multiply the standard weights by $p^*(1-p)$. With logit estimation of the propensity score, this creates exact covariate balance at the mean – the weighted mean for the treated firms exactly equals the weighted mean for the controls, for each covariate included in the propensity score estimation. It also reduces standard errors, compared to standard weights. The cost is that the estimate will be unbiased only if one assumes homogeneous treatment effects (see discussion in Black et al., 2014). This approach too can be combined with trimming, where we remove the observations where treated and controls have no common support.

For the initial tests in this paper, investigating the effects of short selling on capital raising, I present the results with all of these approaches (inverse propensity weighting with trimming, inverse propensity weighting without trimming, regular firm FE, inverse propensity tilting, and inverse propensity tilting with trimming), showing their robustness. For the follow-up tests of cash-flow-investment sensitivity, I use the best available procedure -- inverse propensity weighting with trimming. The results are qualitatively similar with the use of tilting and without trimming.

Part III: Data and Variables

I start with the same baseline dataset that the SEC used, the Russell-3000 index. I use all firms that were included in Russell-3000 in 2004, the year when the experiment was announced. I obtain the list of treated and partially-treated firms from the SEC website. As prior studies do, I exclude all foreign firms, regulated utilities, and firms in banking and insurance industries. I also exclude all firms that merged or were delisted during the treatment period.

All firm-level annual data is from Compustat. As is common for the studies of equity issuance, I measure equity issuance as the ratio of "sstk" (sale of common and preferred stock" over total assets (Welch 2011). For debt issuance, I use "dltis" (long-term debt issues) over total assets. The results are robust to other definitions of debt issuances.

¹ With cross-sectional data, IPW plus regression is known as a “doubly robust” method because it will produce unbiased inference if either the propensity model or the regression model is correctly specified (Robins and Rotnitzky, 2001). However, double robustness may not help much if important covariates are unobserved (Kang and Schaefer, 2007).

I use the following firm-level controls. As a measure of firm size, I use a natural logarithm of total assets ("at") +1. I use cash holdings ("chech") to control for firm liquidity and likely immediate need for capital. I use closing price on the last day of the year ("prcc") to account for the fact that stock price predicts the volume of trading and short-selling. I separately control for trading volume, measured in dollars (total number of shares traded, "cshtr_f" multiplied by the closing price, "prcc_f"). In robustness checks (not reported), I use other standard controls -- sales growth, market value of equity, and so on. The results are substantially similar to the ones reported here. I measure cash flow as earnings before extraordinary items (item 18) plus depreciation (item 14). I deflate it by net property, plant, and equipment (item 8).

I use several measures of financing constraints developed in the prior literature. First, as in Fazzari et al. (1988), I divide sample firms into three categories based on their pre-treatment ratio of dividends to income. Firms in "Financial Constraint Group 1" have a dividend-to-income ratio of less than 0.1 for at least 10 year prior to the SEC experiment; these are the kinds of firms that prior literature described as "most constrained." Firms in "Financial Constraint Group 2" are the ones with dividend-to-income ratios between 0.1 and 0.2 (moderately constrained). Firms in "Financial Constraint Group 3" are the ones with dividend-to-income ratios greater than 0.2 (the least constrained).

Other measures of financing constraint are linear: the ratio of total dividends to net income (" dv ")/"ni". An alternative measure, also used, is the ratio of dividends to preferred to net income (" dvp ")/"ni". In robustness checks (not reported), I measured these ratios as a mean of 5 pre-treatment years; mean of 10 pre-treatment years, median of 5 pre-treatment years, and so on; the results were similar.

The research design is a standard difference in differences: I measure the changes in the variable of interest (equity raising or debt raising, either raw or adjusted for PPNT, as is standard in the literature), from before treatment to after treatment, for each firm. I then interact main explanatory variables (pre-treatment cash-flow-investment sensitivity, pre-treatment financing constraints, pre-treatment cash flows) with the treated-firm dummy. The coefficient on this interaction variable indicates the impact of the firm's pre-treatment characteristic on changes in capital raising. Because firms were selected into treatment randomly, and because treatment simplified capital raising of random group of firms, we can be more comfortable in asserting causal relationship between these variables.

Part V: Results

A. The Impact of Short-Selling Restrictions on Capital Raising

Tables 2 through 4 investigate the effect of the removal of short-selling restrictions on capital raising. In Tables 2 and 3, I use the most widely accepted procedures, inverse propensity weighting (Rosenbaum 1998; Rosenbaum and Rubin, 1983). It produces an unbiased result even where treatment

effects are heterogenous. In Table 4, I use a different procedure, inverse propensity tilting, described in Graham (2012). It produces exact covariate balance and is more efficient than inverse propensity weighting, but it also results in biased outcomes where treatment effects are heterogenous. Because I have no reason to think that treatment effects should be homogenous, I do not use this procedure if further tests.

In Column (1) of Table 2 and Table 3, I use inverse propensity weighting with trimming of observations that have no thick support. In Column (2) of Table 2 and Table 3, I use the same inverse propensity weighting, but do not trim the observations on the range without thick support. In Column (3) of Table 2 and Table 3, I neither trim nor use weighting. This specification is equivalent to assuming that the actual controls were assigned to controls randomly.

In all three tables, I use annual observations from 1990 through 2012. All regressions use firm fixed effects, year fixed effects, and firm clusters. The coefficients of interest are those on the interaction variable "treatment period * treated firm".

In all specifications, treated firms raised significantly more equity than controls during the treatment period. The improper specification -- in Column (3), which treats controls without adjustment, as though they were randomly assigned real controls -- produces the coefficient that's twice as large as what we get in the most conservative specification; t-statistics are much larger as well. This indicates that prior work, that failed to adjust for the non-randomness of the SEC treatment-selection procedure, likely produced biased results. (I address this issue in a companion paper in more detail, see Litvak (2013)). This result is consistent with the view that short selling improves accuracy of pricing, which in turn reduces information asymmetry between insiders and outsiders, and induces outsiders to invest.

As Table 3 shows, short selling restrictions do not impact debt raising. This too is consistent with the view that short selling improves information quality. Because debt issuance is less sensitive to short-term fluctuations in available information, most of the value of accurate pricing goes to equity issuances.

In Table 4, I repeat these tests using inverse propensity tilting. The results are similar to those presented in Table 2 and Table 3. The coefficient on the variable of interest (interaction variable between treatment period and treated firm) is about twice as high as in the specification with inverse propensity weighting, and t-statistic is correspondingly higher as well. This is consistent with the fact that inverse propensity tilting assumes homogenous treatment effects and, for some distributions of effects, would inflate coefficients where the true effects are heterogenous.

The results in Tables 2, 3, and 4 are consistent with the view that short-selling improves information environment and helps firms raise equity capital.

B. The Relationship between Pre-Treatment Financing Constraint and Investment-Cash-Flow Sensitivity – Generally (Not Based on the SEC Experiment)

First, I repeat the research design of Fazzari et al. (1988) and verify whether, ignoring the SEC experiment for now, cash flows of constrained sample firms are indeed sensitive to investment during the period of this study. The results are reported in Table 5. As in prior literature, I first divide all firms into three groups, based on their financial constraint. Following Fazzari et al. (1988), I define “financial constraint” as a ratio of dividend to income. Firms in “Financial Constraint Group 1” have a dividend-to-income ratio of less than 0.1 for at least 10 year prior to the SEC experiment; these are the kinds of firms that prior literature described as “most constrained.” Firms in “Financial Constraint Group 2” are the ones with dividend-to-income ratios between 0.1 and 0.2 (moderately constrained). Firms in “Financial Constraint Group 3” are the ones with dividend-to-income ratios greater than 0.2 (the least constrained).

I then ask whether cash flows are sensitive to investment in all three groups in the same way. The answer is no. The coefficient in Column (1) – for most constrained firms – is significant and positive. That is, most constrained firms (as defined by their dividend-to-income ratios) exhibit significant sensitivity of investment to cash flows. Meanwhile, for moderately constrained firms (Column 2), the coefficient is negative and marginally significant; for the least constrained firms (Column 3), the coefficient is negative and strongly significant. That is, financially unconstrained firms exhibit negative investment-cash flow sensitivity. These results are consistent with the findings in Fazzari, Hubbard, Petersen (1988) and other prior research replicating these findings.

Again, the results in Table 5 do not use the randomization generated by the SEC experiment. Their purpose of this table is to verify that the firms in my sample, during the study period, generally behave like firms studied in prior literature.

C. The Impact of Pre-Treatment Investment-Cash-Flow Sensitivity on During-Treatment Capital Raising

In Table 6, I move to the difference-in-difference research design incorporating the SEC randomized experiment. I ask whether the treated firms that exhibited higher investment-cash flow sensitivity before the SEC experiment ended up raising more capital during the treatment period. The coefficient of interest in Table 6 is the one on the interaction variable “Cash-flow-investment sensitivity * Treatment”.

In Column 1, I report the results for equity raising. The coefficient on the interaction variable is small and statistically insignificant. Treated firms with higher pre-treatment cash-flow-investment sensitivity did not raise more capital during the treatment period, when the SEC randomly gave them the opportunity to do so, than treated firms with lower pre-treatment cash-flow-investment sensitivity.

They also did not raise significantly more or less debt (Column 2). In Columns (3) and (4), I repeat these tests, controlling for pre-treatment financing constraint. The results are very similar. This is consistent with the view expressed in Kaplan and Zingales (1997) and not consistent with the view of Fazzari et al.

D. The Impact of Pre-Treatment Financing Constraint on During-Treatment Capital Raising

In Table 7, I ask whether the pre-treatment level of a firm's financial constraint causes the change in capital raising during the treatment. As in Table 6, I say "causes", rather than "predicts", because the SEC randomized experiment allows us to establish causation. I use three measures of financing constraint developed in the prior literature (details to be added), referred to as "Financial Constraint A/B/C". The dependent variable is the firm-level difference between pre-treatment and post-treatment ratios of equity raised over PPENT, as in prior studies. Coefficients of interests are the ones on the interaction variables. Column (1) reports the results for the measure of financing constraint A; Columns (2) and (3) for the measures B and C, respectively. In all columns, I control for firm assets, cash holdings, stock price, and trading volume. In all columns, I use inverse propensity weighting with trimming.

The pattern is consistent across all models. Pre-treatment financing constraint does not cause treated firms to increase their equity raising during the treatment period, as compared to non-treated firms.

E. The Impact of Pre-Treatment Investment-Cash-Flow Sensitivity on During-Treatment Capital Raising

The existing measures of financial constraint might be imperfect. An alternative approach is to ask whether pre-treatment cash flows predict during-treatment equity raising, controlling for standard firm characteristics. The hypothesis is, other things equal, a treated firm that had lower pre-treatment cash flows is more in need of external financing, and therefore should be more eager to use the favorable capital-raising environment created by the treatment to raise capital. In Table 8, I investigate this possibility. Here, the dependent variable, as in Tables 6 and 7, is the firm-level difference between pre-treatment and post-treatment ratios of equity raised over PPENT. The coefficients of interests are the ones on the interaction variables – "Cash Flows in 2004 * Treated Firm Dummy". As in all other tables, I use inverse propensity matching with trimming to adjust for the imperfection of the randomized experiment.

This time, the results are significant and positive. Treated firms with larger pre-treatment cash flows raised significantly more equity and debt during treatment. This is exactly the opposite of what the financial-constraint theories would predict. At the same time, this is consistent with the results in Kaplan and Zingales (1997), who argue that cash flows proxy for investment opportunities.

Conclusions

The sensitivity of firm's investment to cash flows is a classic old puzzle, with significant implications for economic policy and law. If this sensitivity is caused by a firm's inability to quickly and cheaply raise capital on external markets, then, we might be seeing significant distortions in investment, likely cyclical, and likely affected by laws and institutions that regulate capital markets. If this causal relationship is established, the future line of research should concentrate on finding specific regulatory frictions responsible for a firm's inability to tap external markets when needed. Among such frictions one could study the imperfections in the disclosure regime, the problems with the SEC registration procedures, and so on.

On the other hand, if this sensitivity is caused by agency problems, then, a productive new line of research and policy making is to establish connections between a large executive compensation literature, corporate governance, and investment choices.

The main contribution of this paper is that it's the first to suggest a strong identification strategy to disentangle causal relationships among investment, financing constraint, and capital raising. Because my results reject the hypotheses attributing the sensitivity to financing constraint, they indicate that the most fruitful area for both research and regulation is executive compensation and corporate governance, not regulation of financial markets.

Table 1A

Summary Statistics for Intended Randomization.

"Treated group" are the firms in the Russell-3000 that were designated as "Category A Pilot Securities" by the SEC. "Intended control" are all other Russell-3000 firms, that is, a combination of "True Control" firms and "Partially Treated" firms.

Firm Characteristic	Treated Group, Mean	Intended Control (True Controls Plus Partially-Treated)	T-stat Treated v. Intended Control
Number of Firms	580	1168	
Asset Size	2958.95	3245.28	0.67
Cash and Cash Equivalents	19.99	25.15	0.57
Capital Expenditures	152.8	141.14	0.59
Common Shares Issued	110.78	126.56	1.15
Long-Term Debt	694.13	769.53	0.74
Total Liabilities	1772.77	2025.3	0.89
Total Dividends Paid	41.18	40.27	0.12
EBITDA	396.77	388.97	0.16
Number of Employees	10.26	11.31	0.8
PPE Total	1829.85	1835.2	0.09
Sales Growth	267.31	295.38	0.56
R&D Expenditures	54.87	84.15	1.92
Trading Volume, in \$	6.10E+09	6.17E+09	0.1

Table 1B

Summary Statistics for True Randomization

"Treated group" are the firms in the Russell-3000 that were designated as "Category A Pilot Securities" by the SEC. "True Control" are all "Category C Pilot Securities" that were never exempt from the uptick rule.

Firm Characteristic	Treated Group, Mean	True Control Group	T-stat Treated v. True Control
Number of Firms	580	781	
Asset Size	2958.95	719.68	7.36
Cash and Cash Equivalents	19.99	5.47	2.19
Capital Expenditures	152.8	37.3	7.19
Common Shares Issued	110.78	43.42	7.29
Long-Term Debt	694.13	204.83	6.25
Total Liabilities	1772.77	445.25	6.82
Total Dividends Paid	41.18	5.37	6.36
EBITDA	396.77	79.8	8.38
Number of Employees	10.26	3.9	6.47
PPE Total	1829.85	460.66	7.32
Sales Growth	267.31	261.55	0.11
R&D Expenditures	54.87	25.21	3.52
Trading Volume, in \$	6.10E+09	1.67E+09	8.78

Table 1C

Summary Statistics for Full versus Partial Treatment

"Treated group" are the firms in the Russell-3000 that were designated as "Category A Pilot Securities" by the SEC. "Partially Treated " are all "Category B Pilot Securities" that were exempt from the uptick rule after 4pm.

Firm Characteristic	Treated Group, Mean	Partially Treated	T-stat Treated v. Partially Control
Number of Firms	580	387	
Asset Size	2958.95	8303.04	15.84
Cash and Cash Equivalents	19.99	64.62	5.3
Capital Expenditures	152.8	349.37	14.99
Common Shares Issued	110.78	293.06	15.94
Long-Term Debt	694.13	1897.49	14.47
Total Liabilities	1772.77	5185.41	14.39
Total Dividends Paid	41.18	110.18	12.04
EBITDA	396.77	1009.73	17.64
Number of Employees	10.26	26.07	14.9
PPE Total	1829.85	4646.14	14.82
Sales Growth	267.31	362.32	1.56
R&D Expenditures	54.87	214.59	9.74
Trading Volume, in \$	6.10E+09	1.51E+10	18.49

Table 2

Impact of Short-Selling on Equity Issuance, with Inverse Propensity Weighting

The table shows the effects of removal of short selling restrictions on equity issuance by Russell-3000 firms. The treatment period is 2005-2007, the period of the SEC experiment. "Treated firms" are fully treated; "controls" are true controls. Panel from 1990 through 2012. Firm and year fixed effects and firm clusters. Significant results for the coefficients of interest are in bold. T-statistics in parentheses. The adjustment for the imperfection of the SEC randomization procedure are done as follows: in column (2), through inverse propensity weighting, observations with no thick support removed (Rosenbaum 1998; Rosenbaum and Rubin, 1983); in column (2), inverse propensity weighting, no trimming; in column (3) no weighting and no trimming.

	Equity Issuance		
	(1)	(2)	(3)
	With IPW, Trimmed	With IPW, Not Trimmed	No Weights, No Trimming
Treatment Period * Treated Firm	0.0144** (2.11)	0.0184*** (3.05)	0.0288*** (4.43)
Treatment Period	-0.0266*** (-3.523)	-0.0759*** (-5.977)	-0.0367*** (-4.854)
Ln Assets	-0.0887*** (-16.87)	-0.0800*** (-17.77)	-0.0844*** (-19.25)
Ln Cash Holdings	0.0208*** (15.78)	0.0186*** (16.47)	0.0209*** (18.10)
Ln Closing Price	0.0400*** (7.36)	0.0317*** (6.97)	0.0347*** (8.11)
Ln Trading Volume	0.0112*** (3.64)	0.0150*** (5.59)	0.0155*** (6.08)
Constant	0.211*** (3.83)	0.163*** (3.90)	0.115** (2.48)
Fixed Effects	firm, year	firm, year	firm, year
Clusters	firm	firm	firm
Observations	9,563	11,346	12,316
R-squared	0.24	0.207	0.21
Number of Firms	1,231	1,255	1,395

Table 3

Impact of Short-Selling on Debt Issuance, with Inverse Propensity Weighting

The table shows the effects of removal of short selling restrictions on debt issuance by Russell-3000 firms. The treatment period is 2005-2007, the period of the SEC experiment. "Treated firms" are fully treated; "controls" are true controls. Panel from 1990 through 2012. Firm and year fixed effects and firm clusters. Significant results for the coefficients of interest are in bold. T-statistics in parentheses. The adjustment for the imperfection of the SEC randomization procedure are done as follows: in column (2), through inverse propensity weighting, observations with no thick support removed (Rosenbaum 1998; Rosenbaum and Rubin, 1983); in column (2), inverse propensity weighting, no trimming; in column (3) no weighting and no trimming.

	Debt Issuance		
	With weights, Not Censored	With weights, Not Censored	No Weights, Not Censored
Treatment Period * Treated Firm	-0.0042 (-0.450)	-0.00688 (-0.797)	-0.0102 (-1.294)
Treatment Period	-0.0285* (-1.876)	0.00459 (0.33)	-0.00042 (-0.0414)
Ln Assets	0.0317*** (5.78)	0.0217*** (4.69)	0.0213*** (5.10)
Ln Cash Holdings	-0.00438*** (-3.045)	-0.00418*** (-3.093)	-0.00401*** (-3.306)
Ln Closing Price	-0.00584 (-1.084)	-0.00369 (-0.719)	-0.00453 (-1.006)
Ln Trading Volume	-0.00483 (-1.400)	-0.00266 (-0.842)	-0.00272 (-0.986)
Constant	0.0448 (0.95)	0.0325 (0.74)	0.0371 (0.83)
Fixed Effects	firm, year	firm, year	firm, year
Clusters	firm	firm	firm
Observations	9,185	11,008	12,041
R-squared	0.017	0.014	0.013
Number of Firms	1,231	1,255	1,396

Table 4

Impact of Short-Selling on Equity Issuance, with Inverse Propensity Tilting

The table shows the effects of the removal of restrictions on short-selling on equity issuance and debt issuance by Russell-3000 firms. The treatment period is 2005-2007, the period of the SEC experiment. "Treated firms" are fully treated; "controls" are true controls. Panel from 1990 through 2012. Firm and year fixed effects and firm clusters. Significant results for the coefficients of interest are in bold. T-statistics in parentheses. The adjustment for the imperfection of the SEC randomization procedure are done through inverse propensity tilting (Graham et al. 2012).

	Equity Issuance	Debt Issuance
Treatment Period * Treated Firm	0.0285*** (3.33)	-0.0109 (-1.229)
Treatment Period	-0.0911*** (-5.232)	0.013 (0.85)
Ln Assets	-0.0877*** (-16.27)	0.0200*** (3.80)
Ln Cash Holdings	0.0250*** (15.36)	-0.00488*** (-3.331)
Ln Closing Price	0.0417*** (7.28)	-0.0126** (-2.233)
Ln Trading Volume	0.0107*** (3.15)	0.000555 (0.16)
Constant	0.252*** (4.68)	-0.00583 (-0.122)
Fixed Effects	firm, year	firm, year
Clusters	firm	firm
Observations	8,372	8,163
R-sq	0.223	0.016
Firms	948	948

Table 5

Correlation: Financial Constraint versus Cash Flow Investment Sensitivity. No Treatment.

This table replicates prior results reported in Fazzari et al. (1988), showing the sensitivity of investment to cash flow, and the relationship between this sensitivity to financial constraint. All specifications and variable definitions are the same as in Fazzari et al. (1988). No treatment data from the SEC experiment used here. Group 1 contains the firms with dividend-income ratios of less than 0.1 for at least 10 years prior to treatment (most constrained). Group 2 is firms with dividend-income ratios between 0.1 and 0.2. Group 3 is firms with dividend-income ratios greater than 0.2 (least constrained).

	Cash Flow Investment Sensitivity	Cash Flow Investment Sensitivity	Cash Flow Investment Sensitivity
	(1)	(2)	(3)
Financial Constraint Group 1	0.102*** (9.63)		
Financial Constraint Group 2		-0.0362* (1.872)	
Financial Constraint Group 3			-0.109*** (9.312)
Constant	0.213*** -23.96	0.288*** -54.28	0.309*** -56.02
Inverse prop match weighted and trimmed	yes	yes	yes
Obs	863	863	863
R-Squared	0.097	0.004	0.091

Table 6

**Impact of Pre-Treatment Cash-Flow-Investment Sensitivity on Equity Raising,
Cross-Section, Before-After Tests**

This table asks whether treated firms with higher pre-treatment investment-cash-flow sensitivity raised more equity or debt during the treatment period. Cross-sectional data. The coefficients of interest are the ones on the interaction variable. All specifications and variable definitions are the same as in Fazzari et al. (1988). Group 1 contains the firms with dividend-income ratios of less than 0.1 for at least 10 years prior to treatment (most constrained). Group 2 is firms with dividend-income ratios between 0.1 and 0.2. Group 3 is firms with dividend-income ratios greater than 0.2 (least constrained). Controls include firm assets, cash holdings, stock price, and trading volume. All columns use inverse propensity matching with trimming to adjust for the imperfection of the randomized experiment. T-statistics are in parentheses.

	Δ Equity Raising	Δ Debt Raising	Δ Equity Raising	Δ Debt Raising
	(1)	(2)	(3)	(4)
Cash-flow-investment sensitivity * Treatment	-0.106 (-1.180)	-0.0204* (-1.649)	-0.109 (-1.208)	-0.0204* (-1.651)
Fin Constraint Group 1			0.00517 -0.29	-0.001 (-0.412)
Fin Constraint Group 2			0.0215 -0.77	0.00127 -0.35
Cash-flow-investment sensitivity	-0.0362 (-0.532)	-0.0019 (-0.216)	-0.0383 (-0.558)	-0.0015 (-0.162)
Treated	0.0159 -0.54	0.00486 -1.23	0.0154 -0.53	0.00472 -1.19
Assets, Cash Holdings, Closing Price, Trading Volume	yes	yes	yes	yes
Constant	-0.0525 (-0.556)	0.00618 -0.49	-0.0581 (-0.612)	0.00599 -0.47
Observations	486	443	486	443
R-squared	0.052	0.015	0.053	0.016

Table 7

Impact of Pre-Treatment Financial Constraint on Change in Equity Raising, Before-After Tests, Cross-Section

This table asks whether treated firms with higher pre-treatment financial constraint experienced, during the treatment period, a larger change in the ratio of equity raised over PPENT. The dependent variable is the firm-level difference between pre-treatment and post-treatment ratios of equity raised over PPENT. Cross-sectional data. Consistent with the prior literature, I use three different measures of financial constraint (Measures A, B, and C), all linear, measured in 2004, pre-treatment. The coefficients of interest are the ones on the interaction variables (“Financial Constraint A/B/C * Treated Firm Dummy”). Controls include firm assets, cash holdings, stock price, and trading volume. All columns use inverse propensity matching with trimming to adjust for the imperfection of the randomized experiment. T-statistics are in parentheses.

	$\Delta\text{EquityRaised} / \text{PPENT}$	$\Delta\text{EquityRaised} / \text{PPENT}$	$\Delta\text{EquityRaised} / \text{PPENT}$
	(1)	(2)	(3)
Fin Constraint A in 2004 * Treated Firm	0.0121 (0.37)		
Fin Constraint A in 2004	-0.00956 (-0.562)		
Treated Firm Dummy	-0.0167 (-1.097)	-0.0196 (-1.334)	0.00974 (1.05)
Fin Constraint B in 2004 * Treated Firm		0.00909 (0.25)	
Fin Constraint B in 2004		-0.00856 (-0.336)	
Fin Constraint C in 2004 * Treated Firm			-0.0419 (-0.853)
Fin Constraint C in 2004			0.0256 (0.63)
Assets, Cash Holdings, Closing Price, Trading Volume	Yes	Yes	Yes
Inverse Propensity Match Weighted and Trimmed	yes	yes	yes
Constant	0.0125 (0.12)	0.0421 (0.43)	0.0211 (0.27)
Observations	537	529	32
R-squared	0.059	0.062	0.37

Table 8

Impact of Pre-Treatment Cash Flows on During-Treatment Equity Raising, Before-and-After Tests, Cross-Section

This table asks whether treated firms with higher pre-treatment cash flows experienced, during the treatment period, a larger change in the ratio of equity raised over PPENT. The dependent variable is the firm-level difference between pre-treatment and post-treatment ratios of equity raised over PPENT. The coefficients of interest are the ones on the interaction variables (“Cash Flows in 2004 * Treated Firm Dummy”). Controls include firm assets, cash holdings, stock price, and trading volume. All columns use inverse propensity matching with trimming to adjust for the imperfection of the randomized experiment. T-statistics are in parentheses.

	Δ EquityRaised /PPENT	Δ DebtRaised /PPENT
Cash Flows in 2004 * Treated Firm	0.0258*** (9.80)	0.0009*** (2.61)
Treated Firm	-0.021 (-1.560)	-0.00096 (-0.530)
Assets, Cash Holdings, Closing Price, Trading Volume	Yes	Yes
Constant	0.0618 (0.66)	0.0149 (1.18)
Inverse prop match weighted and trimmed	yes	yes
Observations	537	490
R-squared	0.203	0.02

Figure A

Kernel Density of Firm Asset Size for Treated v. Combination of Control and Partially-Treated Firms
(Intended by Randomization)

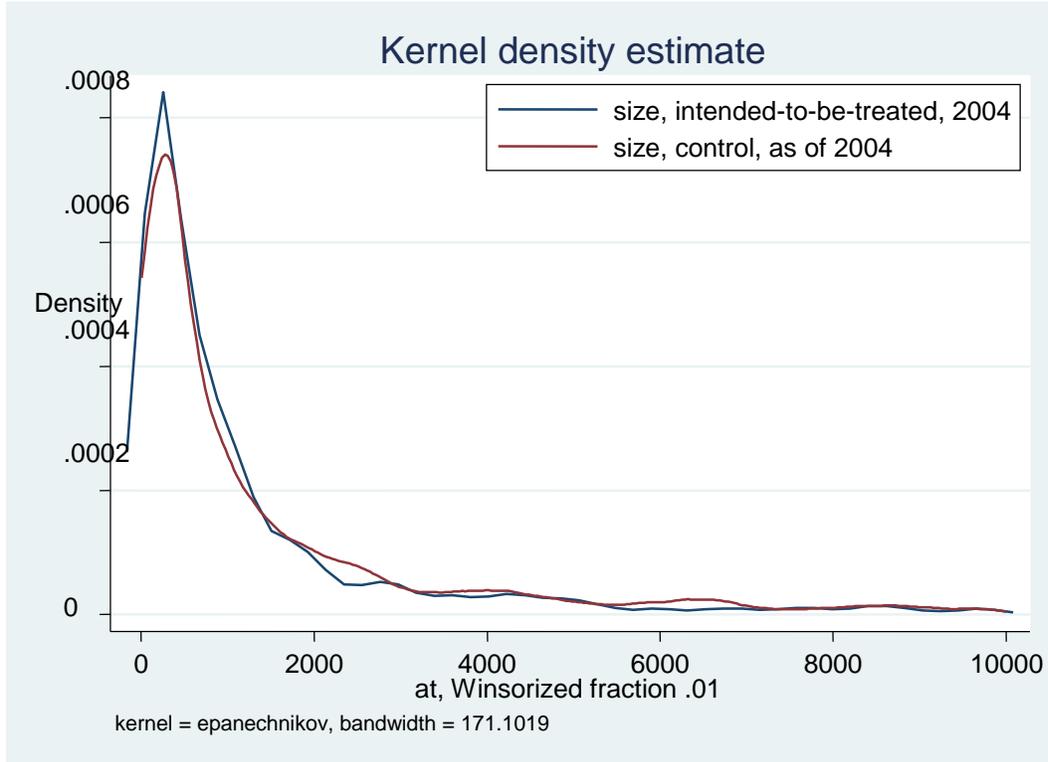


Figure A

Kernel Density of Firm Asset Size in 2004 for Fully Treated v. Original Control Firms

Note: The SEC later separated the original control firms into partially-treated firms and true control firms.

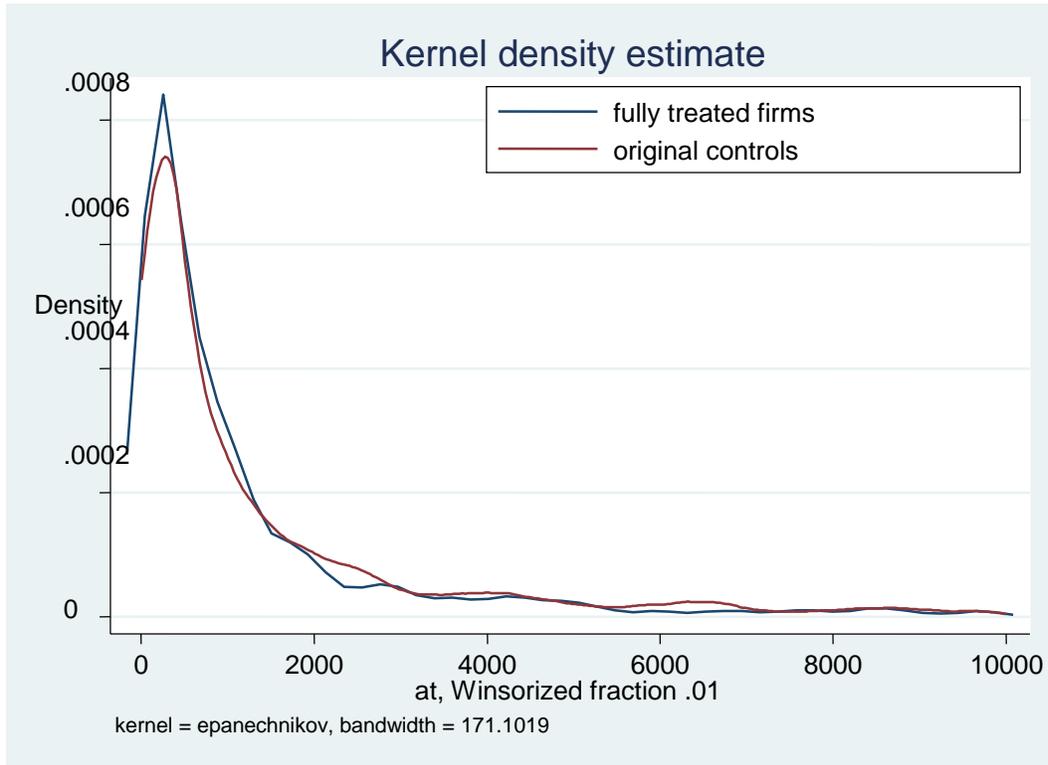


Figure B

Kernel Density of Firm Asset Size in 2004 for Fully Treated v. True Control Firms.

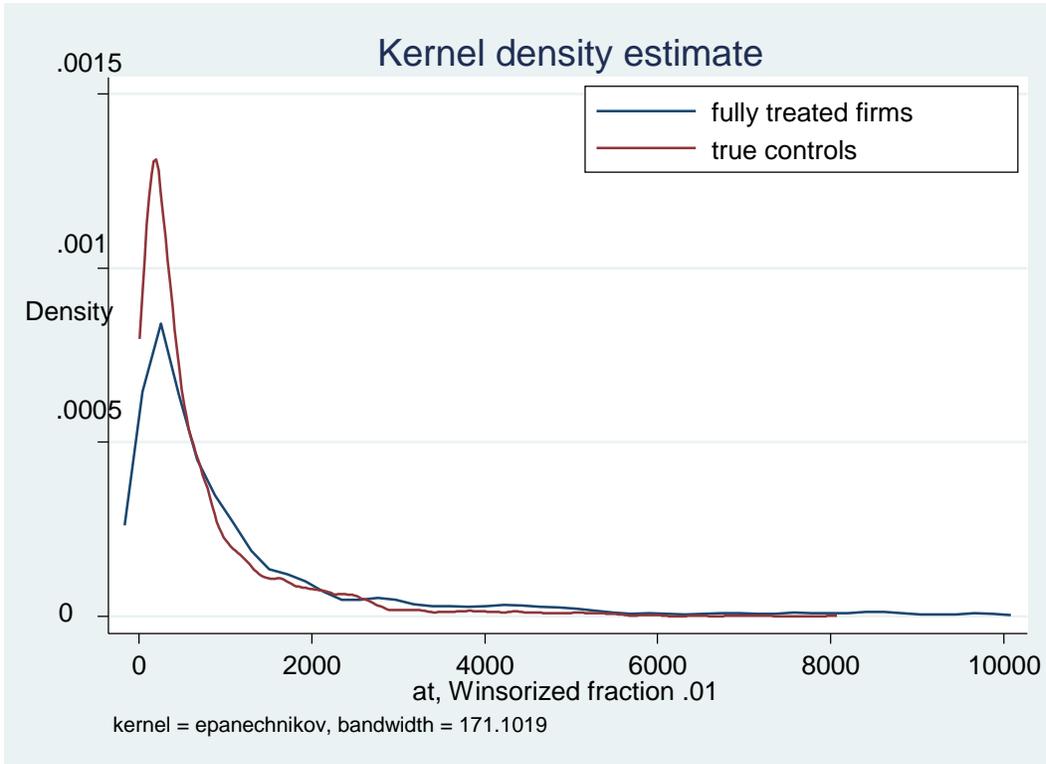


Figure C

Kernel Density of Firm Asset Size in 2004 for Partially Treated v. True Control Firms

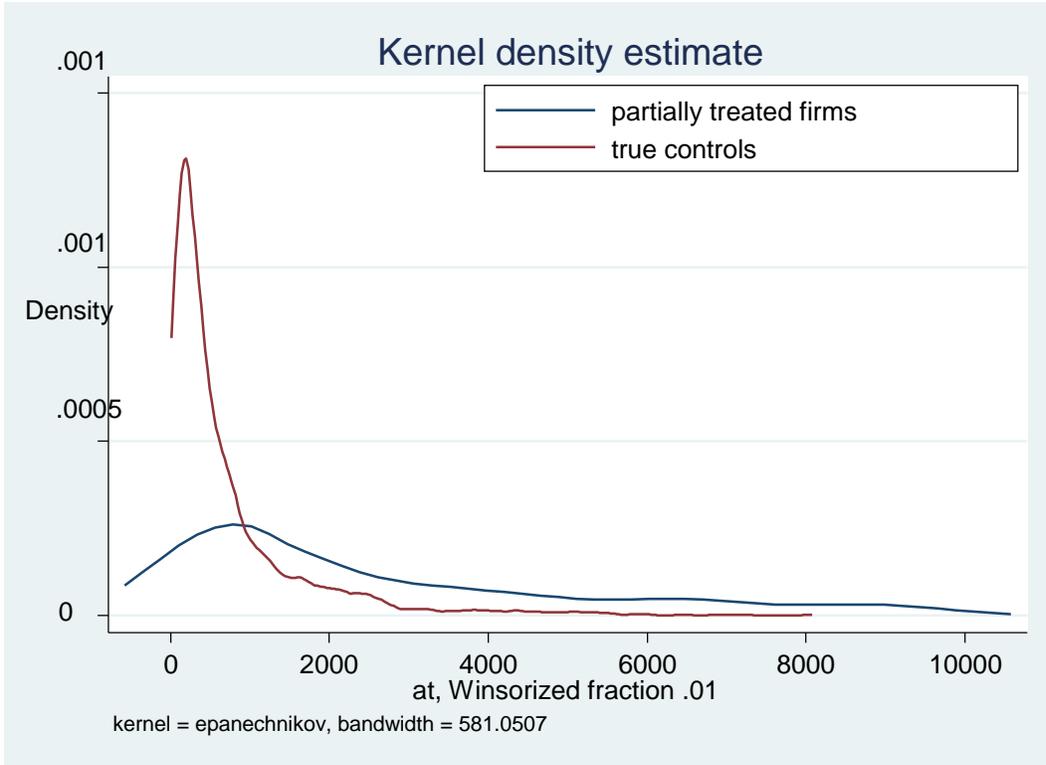


Figure D

Kernel Density of Propensity of Being Treated for Fully Treated v. True Control Firms

Figure shows propensity of fully treated and true control firms to be fully treated, based on logit estimation using data from 2004. Predictor variables are: $\ln(\text{assets})$, $\ln(\text{market value of equity})$, $\ln(\text{trading volume})$, and constant term. Sample excludes partially treated firms.

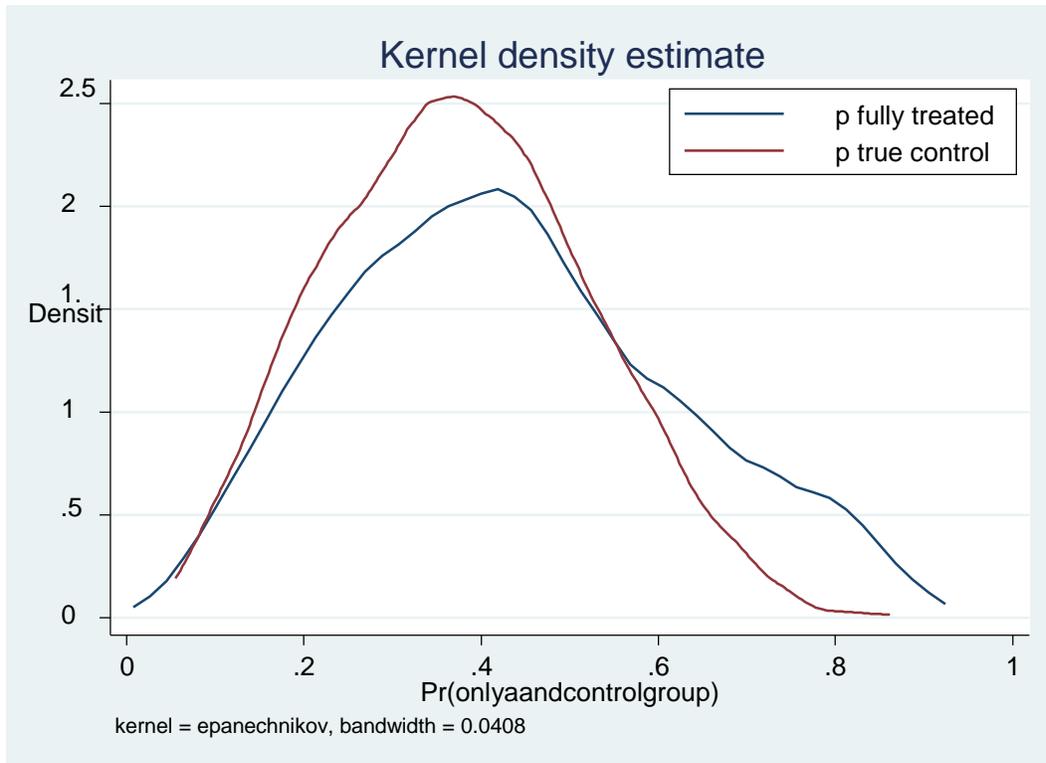


Figure E

Kernel Density of Propensity to be Partially Treated for Partially-Treated v. True Control Firms

Figure shows propensity of partially treated and true control firms to be partially treated, based on logit estimation using data from 2004. Predictor variables are same as in Figure D. Sample excludes fully treated firms.

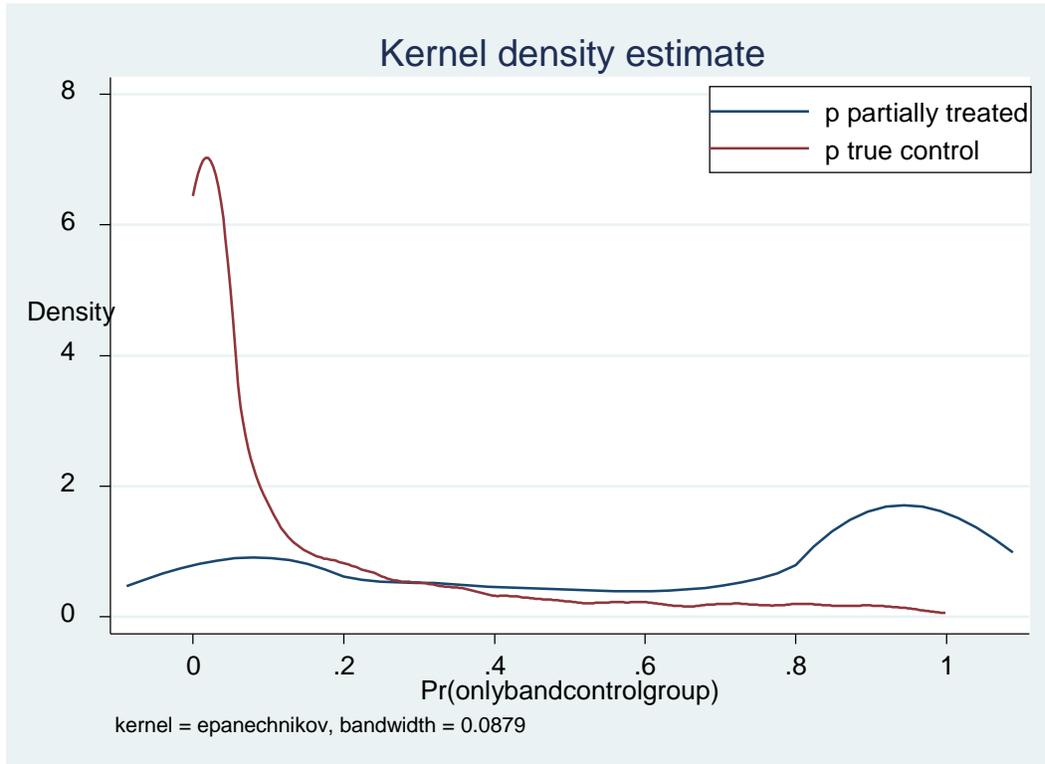
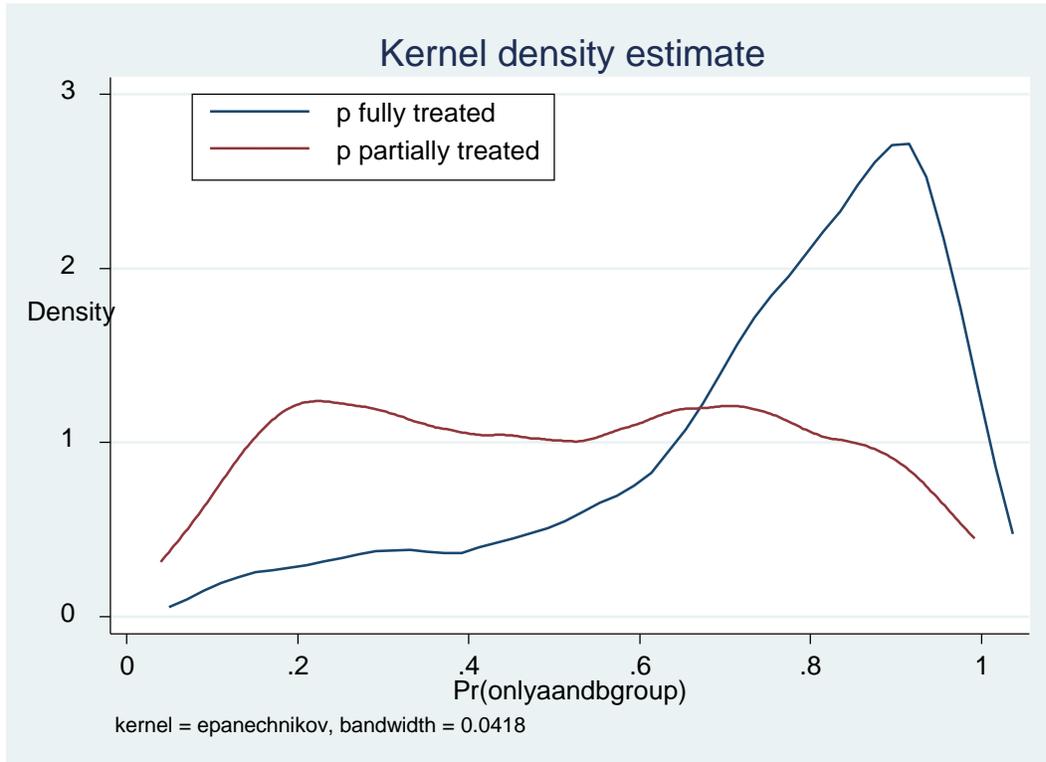


Figure F

Kernel Density of Propensity to Be Fully Treated for Fully Treated v. Partially-Treated Firms

Figure shows propensity of fully treated and partially treated firms to be fully treated, based on logit estimation using data from 2004. Predictor variables are same as in Figure D. Sample excludes true control firms.



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