The "15 Days" Debate: The Value of an Early Release of Information (Evidence from 10-K Submissions)

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Abstract

I analyze the effects of a SEC rule that accelerated the deadline for a 10-K submission by 15 days. The results have proven to be in the SEC's favor, on the most part, against the opposing firms. Using regression discontinuity design, I document that investors value the early release of information, as demonstrated by a stronger market reaction around the 10-K release date. I find evidence supporting the position that the stronger market reaction toward the accelerated 10-K filers is due to the Grossman & Stiglitz (1980) model, since fewer traders choose to be informed, when a firms' 10-K is required to be submitted earlier. I do not find that accelerated firms tend to make more mistakes relative to firms with a longer deadline, with the exception of newly large accelerated firms or constrained large accelerated firms.

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1. Introduction

What are the implications of disclosing information earlier? How does private information impact security returns? When would an active trader deem information acquisition to be too costly? These questions have long interested financial economists due to the implication they have on market efficiency and disclosure regulation. The lack of plausible empirical settings has shifted research toward the development of models to attempt answering the questions above. In this paper, I try to shed some light on these questions using an elegant empirical setting.

Grossman & Stiglitz (1980) paradox has opened up a vast venue of research in the economics of information. In this paper, I exploit a unique empirical setting to show that the markets work similarly to Grossman & Stiglitz (1980) paradigm and provide discussion on the broader implications of the findings. Although many academics have accepted Grossman & Stiglitz (1980) paradigm due to its intuition, rarely we observe empirical evidence that supports Grossman & Stiglitz (1980) model. The lack of empirical studies is due to the difficulty in finding a unique setting where we can observe how increasing the cost of information gathering would affect traders' choice in acquiring information. I study an elegant environment where large accelerator filers, treated firms, are required to submit their 10-K, 15 days earlier than accelerated filers, control firms.

I make use of a regulation change that took effect on December 2006, which forced firms with public float above $700 million, treated firms, to submit their 10-K within 60 days rather than 75 days. [1] [2] I provide evidence that firms surrounding the $700 million thresholds do not experience sorting using McCrary Density Test. Failing to reject the null hypothesis of no sorting, I can infer causal effects using regression discontinuity design.

I find that the standardized absolute cumulative abnormal return (SACAR) to be higher for treated firms.[3] The result is economically significant, given that the control firms average SACAR is 2.17% while being treated causes the SACAR to increase by an average of 1.30%, which is more than 50% of the control firms SACAR. A takeaway from this finding that accelerating disclosures makes it costly for traders to acquire private information. As a consequence, less information gets priced into the security before a 10-K is issued producing a higher SACAR, which is a proxy for valuable information to investors.

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[1] The SEC rule analyzed in this paper has Regulatory Identifier Number (RIN): 3235-AJ29. This SEC rule is referred to as the “SEC $700 million rule”, a reference to the $700 million public float threshold, throughout the paper.

[2] Public float refers to the worldwide market value of shares held by "non-affiliates."

[3] SACAR is measured as the CAR of (-1,1) where day 0 is the event day, 10-K submission, and it is standardized over the absolute standard deviation of CAR. More details can be found in Section 6.

I confirm the above result is due to information acquisition channel rather than an omitted channel by measuring the information asymmetry of treated firms relative to control firms using two information asymmetry proxies: bid-ask spread and Amihud illiquidity. I find evidence that information asymmetry is lower for treated firms supporting the argument that it is more costly now for traders to be informed when it comes to treated firms. The average percentage bid-ask spread for the controls is 3.93%, I find treated firms experience 5.5% lower bid-ask spread. The average Amihud illiquidity for control firms is around 0.0067. While treated firms associate a 16.5% decrease in Amihud illiquidity, both results, using bid-ask spread and Amihud illiquidity, are statistically and economically significant.

I confirm the above results are not due to misrepresentation from the firm side. When I analyze the SACAR, I drop firms that issue amendments so that firms' mistakes would not drive my result. In a separate analysis, I find the number of amendments issued between treated and controls is similar — the two occasions where treated firms issue more amendments if I focus only on newly treated firms or if I focus on constrained treated firms. [4] [5] If I focus on constrained treated firms relative to constrained control firms, then there seem to be no difference between the two regarding amendments issued.[6]

Collectively the above empirical findings have broad implications for information economics literature and disclosure policy. First, I provide clear empirical evidence on how private information effects both SACAR and information asymmetry, Grossman & Stiglitz (1980), Hellwig (1980), Verrecchia *(*1982) and Kyle (1985). I empirically document that traders tend to acquire more private information, or more traders tend to obtain private information when the cost is low.[7]

The results documented here support the argument that active traders do have skills, in a sense they make use of any viable opportunity to gather information and in theory should be compensated for it, supporting Berk and Green (2004) model and Baker, Litov, Wachter and Wurgler (2010) findings. We observe that treated firms experience lower information asymmetry which is the outcome of having less active traders choosing to be informed, Easley, Kiefer, O'Hara, and Paperman (1996), because it is too costly. Even though the SACAR for treated firms is 50% higher than controls, indicating there is more benefit to gain from being informed, the empirical evidence that we observe in regards to lower information asymmetry shows the costs associated with being informed must be more substantial.

Furthermore, if we take Berk and Green (2004) assumption that positive net present value opportunities are scarce and active traders face decreasing return to scales, then the regulation should have reduced the

[4] I define "newly treated firms" as firms that changed status relative to the previous year, from being subjected to a 75-day deadline to being subjected to a 60-day deadline.
[5] "Constrained treated firms" are firms that submitted their 10-K, while using all available days permitted, 60 days.
[6] "Constrained control firms" are firms that submitted their 10-K, while using all available days permitted, 75 days.
[7] An implicit assumption would be that the early release of information makes it costly for traders to be informed.

pie of opportunities for active traders. Although it is hard to measure, the results found in this paper would predict that mutual funds have fewer opportunities to make a profit from trading with passive investors post 2006 and specifically in treated firms' securities.

The empirical results found here provide support to Diamond (1985) paradigm. By accelerating disclosure traders are now more homogenous concerning information because they have less incentive to acquire information now that it is costly. This in return improves risk sharing between investors. This conclusion is based on Easley, Kiefer, O'Hara and Paperman (1996) finding that less information asymmetry for a specific firm, proxy for less informed traders following the firm.

The empirical results presented in this paper settles the debate that took place between the SEC and opposing firms regarding the benefits of accelerating disclosures. The SEC argued early information would be valuable to investors. I confirm their reasoning by finding 10-K released by treated firms have more valuable information as proxied by a larger SACAR. Accelerating disclosures by 15 days seems to improve risk sharing such that more traders are now in equal footing due to the fact treated firms have less information asymmetry relative to controls. On the other hand, I find no evidence regarding the opposing firms' argument that treated firms will make more mistakes which will be harmful to investors trust. The number of amendments issued is similar between treated and controls.

Doyle and Magilike (2012) provide initial analyses of the impact of the SEC decision to accelerate disclosures. Although the setting seems similar, there are three vital differences between this papers approach and Doyle and Magilike (2012) which ultimately provide us with diverging overall interpretations and even different outcomes. First, Doyle and Magilike (2012) principal analysis focus on the $75 million thresholds which took place on December 2003. Data limitation forces them to focus on the $75 million threshold. Doyle and Magilike (2012) data span from 2003 to 2007, which means they only have one year of data to study the impact of disclosure ruling that affected firms surrounding the $700 million thresholds. In this paper I make use of a much longer horizon, 1997-2015, to study the impact of the regulation and conduct additional robustness tests during the placebo period.

The $75 million threshold is problematic to infer causality effects because of the documented sorting that occurs around the $75 million threshold, Gao, Wu, and Zimmerman (2009), Iliev (2010) and Nondorf, Singer, and You (2012). Firms below the $75 million thresholds differ from the ones above the threshold in many aspects rather than just accelerating their 10-K by 15 days, raising the concern that confounding factors might drive their results.[8] I escape from these confounding factors when I study the $700 million

[8] Overview of the differences between non-accelerated, firms below $75 million thresholds, and accelerated filers, firms above $75 million thresholds, are highlighted in the Setting section.

thresholds given that the only regulation that differentiates between treated and controls is the 15 days difference in their 10-K deadline.

Second, Doyle and Magilike (2012) definition of treatment is inaccurate. Doyle and Magilike (2012) define treated firms to be ones with market capitalization, and not **public float**, over $75 or $700 based on the threshold they examine. The SEC assigns firms to a specific category based on their public float and not their market capitalization. By using market capitalization rather than the public float, many control firms will be assigned as treated, which would make interpreting the results impossible. For example, a firm could have a market capitalization of $800 million but public float of $600 million. I calculate the ratio of public float to the market capitalization of accelerated firms and find that public float is 75% of market capitalization on average. This means that Doyle and Magilike (2012) should use a market capitalization of $930 to separate treated and controls rather than $700. Even then their treatment will be noisy because not all firms have similar public float ratios. Non-accelerator filers tend to have 56% of their market capitalization as public float, which means that the situation would be even worse. This matter makes Doyle and Magilike (2012) treatment assignment even more inaccurate for the $75 million threshold.

Third, Doyle and Magilike (2012) results are driven by the endogenous behavior of firms rather than making use of an exogenous regulation that effects some firms and not the others. Doyle and Magilike (2012) approach is to compare treated firms with other treated firms using their definition of treatment. Their results are driven by the **decision** that some treated firms **choose** to accelerate their 10-K by 10-17 days relative to last year and compare them with firms that **choose** to submit their 10-K more or less within the same timeframe as they did last year.

Not surprisingly Doyle and Magilike (2012) fail to find that information asymmetry decreases for treated firms. This outcome was expected, given the treated definition they use combines actual treated and actual controls in one bucket. In addition, they compare treated with treated rather than comparing the information asymmetry of actual treated with actual controls. Although, Doyle and Magilike (2012) find that the market reacts stronger to treated firms that accelerated their disclosure by 10-17 days relative to treated firms that did not hasten their 10-K disclosure, the magnitude, 0.71%, is smaller than the one I find, 1.30%. Again their results do not seem to be driven by the information channel I am advocating for, given they failed to detect differences in information asymmetry between their two groups. Doyle and Magilike (2012) find no differences in restatements between the two groups which is expected given both groups face the same deadline. We should not expect restatement to differ especially when their treated firms are endogenously accelerating their 10-Ks rather than facing pressure from regulators.

On a positive note, Doyle and Magilike (2012) rule out that the information asymmetry results found in this paper are merely due to the mechanics of accelerating disclosures. Doyle and Magilike (2012) results indicate that a more fundamental aspect drives the information asymmetry results found in this paper. Sophisticated traders might track beforehand what is the firm category in terms of filer status and **then** make a decision and collect their private information in regards to the "easier targets."[9]

I provide robustness tests to confirm that the results found in this paper is due to the regulation and not due to a confounding factor that has to do with firms' characteristics such as size. For example, all previous statistically significant treatment dummy results disappear when I run the same tests except now I replace the $700 million thresholds with a placebo threshold of $600 or $800 to assign treatments. If the results are not driven by the actual categorization of firms in regards to the $700 million threshold and were in fact due to a confounding factor, then the result should be stronger for either the $600 threshold or the $800 threshold depending on the confounding factor direction.

All statistical significant results in the paper disappear when the actual threshold of $700 is used but in the pre ruling period, 1997-2005, again indicating that the results found in this paper are due solely to the 15 days acceleration and not some confounding factor.

I argue for the external validity of the results even though the main empirical design is a regression discontinuity surrounding the $700 million thresholds. Studying the impact of the law on such large firms can be thought of as a lower bound, given that the majority of firms are much smaller than the firms studied here. Small firms are more likely to exhibit higher asymmetric information relative to large firms, Grant (1980). We would expect an acceleration in disclosure to have a more substantial effect in decreasing information asymmetry for the smaller firms. Unfortunately, it is impossible to conduct the same test for the $75 million threshold due to the confounding factors caused by having multiple different regulations applying to firms above the $75 million threshold but not on firms below the $75 million threshold. Griffin (2003) finds that the most robust market reaction, in absolute terms, to 10-K releases is for small firms, supporting the argument that the magnitude of SACAR of treated acts as a lower bound for the majority of firms.

Accelerated deadlines have affected two-thirds of Compustat firms or the equivalent of 99.7% of the total market capitalization of Compustat firms, Doyle and Magilike (2012). Therefore, understanding the impact of accelerated deadlines from an investor point of view is necessary due to its broad applicability.

[9] "Easier targets" in a sense that benefits is greater than the cost of acquiring the private information.

The paper is organized as follows: Section 2 details the setting. Section 3 provides a literature review. Section 4 highlights the data that is used. Section 5 runs pre-checks to validate the RD design for causal inference. Section 6 describes the results for abnormal returns associated with 10-K release dates. Section 7 describes the results for information asymmetry. Section 8 presents the results for the number of amendments that each filer issues. Section 9 provides some robustness tests, and Section 10 concludes.

2. The Setting

a) SEC legislation:

The SEC passed a final ruling with Regulatory Identifier Number (RIN) 3235-AI33, referred to here as "SEC $75 million rule", on Sept 5th, 2002. The main objective of the rule is to accelerate the 10-K and 10-Q submissions for accelerated filers. [10] The rule was initially intended to be applied in two phases. By the end of December 15, 2003, the 10-K deadline would decrease from 90 days to 75 days. After December 15, 2004, the 10-K deadline *should* fall from 75 days to 60 days. The later phase was postponed and changed after pressure and complaints from opposing firms. On December 21, 2005, the SEC announced that the 60-day deadline for 10-K would be applied only to the newly created category large accelerated filers. [11] Table 1 provides a summary of the final rulings. [12] [13]

(Insert Table 1)

The SEC received 302 comments regarding the SEC $75 million rule, with 282 comments opposing the rule. After announcing the SEC $700 million rule, the SEC received 46 additional letters from opposing firms urging the SEC to repeal the 60-day deadline. The opposing firms tried to justify their demand through several arguments. First, the opposing firms stated that the quality of the annual reports would suffer, especially given the additional requirements they had to perform post-SOX. The opposing firms argued that it did not make sense to increase the workload, while simultaneously decreasing the time frame. Second, the opposing firms argued that the increased use of 8-K filings reduced the value of 10-K content. The SEC defended its position by arguing that large accelerated filers should not have trouble meeting the 60-day deadline, given their size. Besides, the SEC stated that technology had advanced during the previous three decades, which should ease the preparation of a 10-K, while the 90-day deadline stayed constant prior to the December 15th, 2003 period.

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[10] Accelerated filers, "control group" are defined as firms that have a public float of at least $75 million.

[11] Large accelerated filers, the "treated group," are defined as firms with over $700 million in public float. However, firms with a public float less than $700 million can still be categorized as large accelerated filers, as explained in Figure 1.

[12] Table 1 is taken from the SEC rule 33-8644 file.

[13] The SEC starts calculating days, based on the fiscal year end date for each firm.

Unlike Doyle and Magilike (2012), I focus exclusively on the $700 million cutoff rather than the $75 million cutoffs, because it is a better setting for ruling out confounding events when using a regression discontinuity design. The two groups, just below and just above the $700 million thresholds, are similar in terms of their compliance with SOX regulation.[14] For example, all the SOX rules that apply to a firm with a public float of $500 million, the control group, also applies to a firm with a public float of $900 million, the treated group, with the exception of the SEC $700 million rule. This rule accelerated 10-K submissions for firms with public float above $700 million by 15 days. This cannot be done reliably for the two groups surrounding the $75 million cutoffs, since they differ in several aspects. [15]

Figure 1 describes how firms enter (top graph) and exit (bottom graph) the treatment group. Firms enter the treated group, whenever their public float in the second quarter of their fiscal year is above $700 million, effectively starting from December 15, 2006. Once a firm is categorized as treated, they exit this category, only if their public float drops below $500 million. Therefore, firms tend to get "stuck" in the treatment group, when their public float passes the $700 million thresholds. The treated group includes firms subjected to the 60-day deadline and not necessarily firms with public float above $700 million. The mechanics of the SEC $700 million rule makes it possible to observe firms with a public float below $700 million and subject to the 60-day deadline.

(Insert Figure 1)

b) *What happens to late filers?*

If late filers do not discipline, then firms do not have an incentive to submit their 10-K before the deadline. If this is true, then we should not see a difference between the treated and the control group. I argue that firms do submit their 10-K on or before their deadline, due to two disciplinary forces: the market reaction and the SEC imposed penalties. When a firm is unable to submit their 10-K on time, the firm is allowed to submit NT 10-K, which indicates that they would not be able to meet the deadline. The NT 10-K filing must be submitted before the deadline and should explain the cause of the delay. An NT 10-K filing gives an additional grace period of 15 days. Firms dislike filing an NT 10-K form, because it sends a wrong signal and the market reacts negatively as a consequence, Bartov, Defond and Konchitchki (2013). In addition, the SEC imposes other penalties toward late filers, such as the loss of SEC

[14] I dropped firms that had an IPO after December 8, 2011, because they can qualify for Emerging Growth Company (EGC) which creates incentives to stay below the $700 threshold as shown in Alsabah and Moon (2018).

[15] For example, on September 21, 2010, the SEC announced that firms with a public float less than $75 million are not required to provide audit attestation, while accelerated filers are required to do so. In addition, firms with less than $75 million public float could be categorized as non-accelerated filers or smaller reporting companies, which would bias the empirical analysis. Another difference between smaller reporting companies and accelerated filers: smaller reporting companies are not required to 1) file supplemental schedules, 2) describe their financial statement format, 3) provide percentage thresholds for many disclosures or 4) file separate financial statements of significant investees. These tasks are required for accelerated filers.

registration, de-listing from the stock market and other legal consequences.[16] Due to the significant cost associated with being late, I argue and provide empirical evidence in Section 5 that the 10-K deadlines are binding.[17]

3. Literature Review

There have been a few papers that focus on the SEC $75 million rule. However, most of them examine the compliance costs associated with the shorter deadline, rather than looking at the benefits of implementing the rule. To my knowledge, this is the first paper that solely focuses on the SEC $700 million rule, rather than the SEC $75 million rule. In this section, I summarize the findings of papers that dealt with 10-K filings and how the SEC $75 million rule affected firms in general.

Doyle and Magilike (2012) investigate a similar setting, and they reach a different conclusion. Doyle and Magilike (2012) conclude that there is no difference between treated firms and control firms in terms of information asymmetry and provide mixed results in terms of SACAR such that accelerating disclosures for firms surrounding the $75 million provide investor with less valuable information but accelerating disclosures for firms surrounding the $700 million provides investors with more valuable information. Doyle and Magilike (2012) data span from 2003 to 2007, given that they mainly analyze the SEC $75 million rule, which implemented in December 2003. Doyle and Magilike (2012) only have one year, 2007, to analyze the SEC $700 million rule, since the SEC $700 million rule implemented in December 2006. My dataset covers the 1997 to 2015 period. Furthermore, there are three main differences between this papers interpretation and Doyle and Magilike (2012), which were discussed in detail at the Introduction due to their importance.

Griffin (2003) investigates the market reaction due to 10-K and 10-Q submissions. Griffin (2003) documents statistically significant market reaction due to the 10-K and 10-Q release, which supports that these filings are valuable to investors. The sample in Griffin (2003) is between 1996 and 2001, which uses all 10-K, 10-KSB, 10-Q and 10-QSB files in EDGAR. Griffin (2003) finds that SACAR is higher for smaller firms, delayed filers, and firms with fewer institutional investors. He also documents that investor reaction is increasing monotonically with years, which indicates these forms are getting more important with time.

[16] For example, Key Energy Services, an oil company with market value of $1.5 billion at the time, was delisted in 2005, because they missed their 10-K deadline and had not submitted an NT 10-K filing.

[17] Furthermore, SEC red flags any firm with multiple amendments. Therefore, firms will be discouraged from meeting the deadline with *inaccurate* information.

Lambert, Brazel, and Jones (2008) look into whether the SEC $75 million rule has affected the quality of reporting. Lambert, Brazel, and Jones (2008) use discretionary accruals to proxy for earning quality and test if the mandatory reduction in audit time effects quality. Lambert, Brazel, and Jones (2008) document that gaining quality has a negative relationship with audit time reduction. Lambert, Brazel, and Jones (2008) conclude that the SEC $75 million rule had unintended consequences which cause more harm than good.

Similarly, Kutcher, Peng, and Weber (2013) and Doyle and Magilike (2012) look into whether the SEC $75 million rule has affected the quality of reporting. They both use firms' restatements to proxy for the firms' disclosure quality. This paper uses 10-K amendments rather than restatement to proxy for firms' mistakes. Kutcher, Peng, and Weber (2013) data is from 2001 to 2006. They both find that the SEC $75 million rule has caused treated firms to issue more restatements. Although when Doyle and Magilike (2012) look into firms with a market capitalization above $700 million, they fail to find a difference between firms that choose to accelerate their filings and firms that choose not to accelerate their filings.

4. Data Collection and Sample

The public float, accelerator status, date of 10-K submission, fiscal year end date, NT 10-K filings and 10-K amendments filings have been collected from the EDGAR website for each firm. A crawler specifically created to facilitate in downloading all the 10-K filed from EDGAR website. I then scrape the public float data out of the 10-K text files I downloaded. Next, I narrow the sample to firms with a public float between $500 and $900 million and create a code that assigns whether a firm is treated or not, based on the accelerator status found in the 10-K. Similarly, I scrape both the date of 10-K submission and the fiscal year end date from the downloaded 10-K files. I subtract the submission date from the fiscal year end date to calculate the number of days it took a firm to release its 10-K, allowing me to evaluate if the firm submitted its 10-K within the deadline. [18]

The NT 10-K filings were collected using similar techniques. I also create a code to indicate whether a firm has issued an amendment for a given 10-K/year.

The observations breakdown for the sample during the 2007-2015s are as follows: I start with 92,463 observations downloaded from 10-Ks between 1997 and 2015 after removing NT-10K filers and 10-K/A files. Of those 92,463 observations, 24,002 observations dropped because they are either financial firms or

[18] When a firm deadline falls on a weekend or holiday, the filing is due on the next business day (Exchange Act Rule 0-3). This explains why some of my observations submit their 10-K one or two days after their deadline, without issuing an NT-filing.

utility firms. These are firms with a SIC code in the range of 6000 to 6999 and 4900 to 4999 respectively. Of those 68,461 observations, 1,679 are foreign firms. Out of the 66,782 observations, only 5,468 observations have public float between $500 and $900 million. Out of the 5,468 observations, 2,670 file their 10-K after the implementation of the $700 million rule. [19] Out of the 2,670 observations, 145 observations end up lost because accelerator data is missing or they were not categorized as either treated or control group. [20] Control firms with submissions that took over 78 days and treated firms with submissions that took over 63 days were removed, 18 observations are lost, ending up with 2,507 observations. Finally, I drop firms that are eligible for EGC category ending up with 2,423 observations.

The analysis in this paper is conducted on three subsamples. The first sample uses all of the 2,423 observations. I will refer to this sample as **"Sample 1".**

The second sample, **"Sample 2"**, reduces the treated firms' observations to focus on treated firms that are "new" to the 60-day deadline relative to their requirement in the previous year, *newly treated firms*. Treated firms that kept the 60-day deadline for a consecutive year in Sample 2 are dropped for several reasons. First, Sample 2 makes it possible to conduct a more accurate test of whether the SEC ruling is followed.[21] Second, Sample 2 focuses on the *new* treated firms which, in theory, should issue more amendments because they are used to the 75 days deadline last fiscal year. Third, Sample 2 makes it possible to conduct a stricter test on whether firms manipulated their public float. Dropping treated firms that were subjected to the 60-day deadline in their previous year, decreases the subsample to 1,360 observations.

The third subsample, **"Sample 3"**, focuses on treated firms that made use of *all* their 60-day limit, *constrained treated firms*, before submitting their 10-K and control firms that did not make use of all their 75-day limit, *unconstrained control firms*, before submitting their 10-K. Specifically, I restrict the control firms to the ones that submitted their 10-K between 63 and 74-day timeframe. The implicit assumption is that the treated constrained firms would not be constrained, if they had an extra 15 days. Sample 3 has 840 observations.

For the placebo subsample, **"Placebo Sample**," I use observations from the 1997-2005 period.[22] After dropping amendments and NT filers, a total of 2,477 observations remains, which are firms with a public

[19] The focus is on data post-December 2006 because starting in 2006 firms were required to categorize themselves as treated firms. However, they were still not required to follow the new deadline until December 15, 2006.
[20] Firms with a public float between $500 and $900 can be classified as a non-accelerated filer if it has been an Exchange Act reporting company for less than 12 calendar months or never filed an annual report.
[21], Which is shown in Figure 3, Section 5.
[22] I stopped my data collection on the date the SEC announced the SEC $700 million rule on December 21, 2005

float between $500 and $900 million and submitted their 10-K within the SEC timeframe.[23] I assign firms with public float above $700 as "treated" and firms with a public float below $700 million as "control," given that during the 1997-2005 period, there was no official categorization for treated firms.

The period between the rule announcement and rule implementation has 264 observations of firms with a public float between $500 and $900 million.[24] I refer to this subsample as **“Transition Sample."**

I use the Compustat Segment dataset to collect the number of segments each firm has in terms of businesses and geography. I use the Compustat Fundamental Annuals dataset to obtain the number of shareholders each firm has and its industry code (SIC). I use the SIC first two digits for industry fixed effects. I also use the year of the fiscal year end date as a time fixed effect.

The CRSP dataset is used to get price volatility, share price, trading volume, firm size and returns for all observations. All of the data are taken on a monthly basis and then averaged over a year to be used as control variables in the regressions. I also use the highest trade price and lowest trade price from the CRSP dataset to calculate the percentage bid-ask spread. I calculate the percentage bid-ask spread, using 60 days’ of data before the 10-K submission date.

I use the IBES dataset to get the number of analysts and their dispersion for each firm. I focus on the analysts that made predictions within the most recent 30 days, before the actual earnings announcement. I omit any data with less than two analysts.[25]

Finally, the Eventus database is used to collect abnormal return data. I calculate the parameter benchmark using 255 days, starting 15 days before the event, which is the 10-K submission date. I derive the parameters with the Fama and French three-factor model and use the value-weighted index.

Table 2 provides summary statistics for the variables.

(Insert Table 2)

Table 3 provides a covariate balance between treated and control firms.

(Insert Table 3)

Not surprisingly the public float of the treated is higher than the control, given that public float is the running variable. Firms with a public float of $700 million are usually categorized as treated. As a

[23] Only four observations submitted their 10-K after the 93-day timeframe, pre- December 12th, 2003. Only sixteen observations submitted their 10-K after the 78-day timeframe, from December 12th, 2003 to December 21st, 2005.
[24] Rule announcement was on December 21, 2005, and rule enforcement was on December 15, 2006. Post-rule announcement firms were required to categorize themselves accordingly.
[25] At least two analysts are needed to calculate dispersion.

consequence of the categorization mechanism, the treated firms larger in size tend to have a higher number of shares exchanged, have more shareholders and have more analysts following them. Given these differences, it is essential to include public float as a control in all of the analysis.

5. Regression Discontinuity (RD) Design Preliminaries

By studying the impact of the SEC $700 million rule using an RD design, I can focus mainly on the deadline decrease from 75 days to 60 days. The SEC $700 million rule creates a quasi-natural experiment, given that the exact cutoff, $700 million, that the SEC chooses is somewhat arbitrary and does not depend on firm fundamentals.

Before investigating any of these questions, it is necessary to check the RD strategy would produce causal inferences. The primary assumption required to use an RD strategy is that the conditional expectation of the dependent variable for firms is continuous for both treated and untreated firms at the threshold (public float of $700 million). Naturally, we can assume that the conditional expectation of the dependent variable is continuous for any public float value. This assumption is violated if firms can manipulate their public float, which would cause the results to be biased. [26]

Gao, Wu, and Zimmerman (2009), Iliev (2010) and Nondorf, Singer, and You (2012) find that firms near the $75 million benchmark have manipulated their public float to avoid SEC $75 million regulations. Firms within the $75 million thresholds are more likely to manipulate their public float, relative to firms within the $700 million threshold for several reasons. First, these firms are small compared to firms near the $700 million thresholds. Any additional requirements would be more costly for smaller firms relative to their size. Second, the smaller reporting companies, firms below the $75 million thresholds, and accelerated filers, firms above the $75 million thresholds, differ in several aspects other than the 10-K deadline.[27]

Figure 2 provides a histogram of firms in each bin, starting from $500 million and ending at $900 million. I use the Placebo Sample, Transition Sample and Sample 2. It is more likely to find evidence of manipulation with Sample 2 relative to Sample 1. [28] Sample 1 includes treated firms that are "stuck" in this category and, therefore, have less incentive to manipulate their public float.[29] From Figure 2, it is

[26] In my setting, manipulation is monotonic, in the sense that all firms want to manipulate their public float below $700 to avoid the shorter deadline. Given that manipulation is monotonic, I can use the McCrary density test.
[27] Check footnote no. 13
[28] I use the whole post sample, Sample 1, in an omitted figure and get a similar qualitatively histogram.
[29] It is "stuck" in the sense that firms who are already categorized as treated, will have to decrease their public float by 28% at the very least.

clear that the pre-SEC $700 million rule announcement and post-SEC $700 million rule announcement periods have the same trend around the $700 million threshold, which provides a preliminary indication that there is no manipulation.[30] [31]

(Insert Figure 2)

Applying the McCrary (2008) density test, I fail to reject the null hypothesis of no sorting for all years individually and the post-SEC $700 million rule announcement period. I only reject the null for the Placebo Sample, at a significance level of 5%, as shown in Table 4. The results presented in Table 4 indicate that there is no public float manipulation around the $700 million threshold during the years following the SEC $700 million rule announcement. [32]

(Insert Table 4)

Another requirement for a RD design is to have a discontinuity in the independent variable when the running variable passes a threshold. In our setting, we need all firms that have public float over $700 to be categorized as the treated group. Figure 3 shows the compliance when using Sample 2 and Transition Sample. Sample 2 does not include treated firms that had consecutive years of keeping the same status which rules out treated firms with a public float of less than $700 million. The graph titled "Treated Firms" includes all firms categorized as treated in Sample 2 and Transition Sample. The graph titled "Control Firms" includes all firms categorized as controls in Sample 2 and Transition Sample. Control firms should not have public float over $700 million.

(Insert Figure 3)

Figure 3 shows that the majority of firms followed the rule. Only 33 out of 1,623 total observations did not follow the rule, which accounts for 2%. Out of the 33 observations, 11 were supposed to be categorized as the control but were categorized as treated. A total of 5 of these 11 observations occurred in 2006. These observations suggest that the error is due to confusion or being overly cautious about following the rule since the SEC $700 million rule was new in 2006. A total of 22 observations were categorized as control when they were supposed to be categorized as treated. Regarding the new $700 million rule, 19 of these 22 observations occurred in 2006, which suggests confusion.

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[30] The years in all figures refer to the year of the fiscal year end date.

[31] In the omitted figures, I looked at each year separately and found that there is a concern of manipulation in 2000, 2004, 2005 and 2006. The years 2000, 2004 and 2005 were all *before* the SEC announcement regarding the new deadline. It is, therefore, likely that it is merely noise. On the other hand, the concern about 2006 seems troubling, since most firms had heard about the new $700 million rule before calculating their second quarter public float for the 2006 fiscal year.

[32] I conduct the same analysis, using Sample 1 rather than Sample 2 in an omitted table. The results are qualitatively similar.

Since over 98% of the observations correctly categorized themselves into the proper filer status, it was necessary to confirm that the new deadline was actually enforced. Therefore, I checked to see if the treated firms submitted their 10-K within the 60-day deadline rather than the 75-day deadline. If firms fail to follow their deadlines due to lack of punishment, then there would not be any difference between the treated and the control group concerning regulation requirements. In Section 2, it was argued that it is unlikely for firms to violate the new deadline, due to both market and regulatory disciplinary forces. I confirm the hypothesis by comparing the time needed by treated firms and control firms to submit their 10-K in both the 1997-2005 and 2007-2015 periods. The data in the 1997-2005 period is the placebo test, given that firms with public float over \$700 were not obliged to submit their 10-K within 60 days. For the pre-2006 period, I categorize firms with public float over \$700 to be treated, given that no official categorization is available during pre-2006 period. Since the accelerator filer deadline changed on December 15th, 2003 from 90 days to 75 days, I divide the 1997-2005 placebo period into two subperiods: pre-December 15th, 2003 and the subperiod between December 15th, 2003 and December 21st, 2005. I create another subperiod between December 21st, 2005 and December 15th, 2006, since this is the subperiod when the \$700 million rule was announced but was not yet enforced. I use Sample 1, Placebo Sample and Transition Sample for this analysis.

(Insert Table 5)

Table 5 shows that for the subperiods before the creation of treated group on December 15th, 2005, there is no difference between the treated and control firms. After the announcement, December 21st, 2005, of creating the treated group, we start to observe divergences between the two groups, in terms of the days they use before submitting their 10-K. The difference is very clear for the post- December 21st, 2006 period, which is when the 60-day rule was enforced on the treated firms. On average, the treated firms submitted their 10-Ks eight-days before the control firms.

6. 10-K Event Study

I assess whether investors value the 10-K deadline reduction enforced by the SEC on the treated firms. Using an event study strategy in combination with a RD design, I test whether 10-Ks have information that the market values. Two tested hypotheses include:

Hypothesis 1: No market reaction to any 10-K release.

Hypothesis 2: The market will react stronger (in absolute terms) to 10-Ks released by treated firms.

Hypothesis 2 is motivated by the SEC argument:

“While quarterly and annual reports at present generally reflect historical information, a lengthy delay before that information becomes available makes the information less valuable to investors” - SEC (2002)

If this is true, a stronger market reaction should be expected in absolute terms, when the treated firms release their 10-K relative to the control firms. The SEC argument is interpreted through the Grossman and Stiglitz (1980) model, which concludes that prices cannot reflect all of the informed investors’ information. Otherwise, there would be no incentive for any investor to get informed. Based on the Grossman and Stiglitz (1980) model, we should expect a stronger market reaction, in absolute terms, towards the treatment group. Investors in the treated firms have less time to get informed. Therefore, fewer investors will choose to be informed and prices of these securities will reflect less of the informed traders’ information before a 10-K gets released, relative to the securities of the control firms, where investors have more time to get informed before the 10-K is made public.

Hypothesis 1 is motivated by the opposing firms’ argument that 10-K filings do not provide investors with any new information. They argue that all of the 10-K information is historical, and the market has already priced 10-K information into the securities. Griffin (2003) shows that 10-Ks do provide the market with information, but the firms' argument still has a basis. First, Griffin’s (2003) analysis uses data between 1996 and 2001, when the 8-K requirements were lenient, relative to post-SOX. Griffin (2003) finds that the strongest market reaction, in absolute terms, is for small firms. The sample used in this paper contains large firms, and hence the Griffin (2003) finding regarding market reaction might not exist in the sample used.

I omit firms that submitted amendments from this analysis. This would allow me to rule out that the market reaction is due to 10-K mistakes, rather than following Grossman and Stiglitz (1980) model.

I follow Griffin (2003) in calculating abnormal returns. I proxy for investor response using standardized absolute cumulative abnormal return (SACAR). The following is SACAR in a formula form:

$$SACAR_{i,t} = |ACAR_{i,t}|/\sigma|ACAR| \quad (1)$$

The subscripts i and t represent a specific firm and day respectively. ACAR stands for absolute cumulative abnormal return. It is measured as the absolute value of the firm i return on day t, minus market return on day t. The absolute standard deviation of ACAR, $\sigma|ACAR|$, is measured over a short window. More specifically, I use ten days to measure it. These 10 days include days -10 to -6 and 6 to 10 where day 0 is the filing day. Griffin (2003) argues that by taking a very long window to measure standard deviation, we will be including previous filing releases, such as 10-Qs or earnings announcements. Therefore, a long window would not provide us with an accurate measurement for standard deviation.

I test whether investors react to 10-K releases, Hypothesis 1, by subtracting b from e where e and b represent time:

$SACAR_{i,e} - SACAR_{i,b}$ (2)

In omitted tables, I use equation (2) to calculate the difference in abnormal return between the two dates. The market seems to value 10-Ks released by both the treated firms and the control firms in period 2007-2015. The SACAR is largest on the event day, Day 0.[33] The market does not react to 10-K releases during the 1997-2005 period for the placebo control firms and the placebo-treated firms.[34] This is consistent with the notion that 10-K information is becoming more valuable with time, as Griffin (2003) document using the 1996-2001 period. The results support the notion that investors now trust firm filings more post-SOX. The results presented reject Hypothesis 1.

Hypothesis 2 is tested by examining whether the difference between the treated group, t, and the control group, c, SACAR is statistically significant at 10-K release date, e = 0:

$SACAR_{t,e} - SACAR_{c,e}$ (3)

Observing a positive difference between the two groups in Equation (3) would support the SEC assessment regarding Hypothesis 2.

The results in Table 6 provide evidence in support of Hypothesis 2; investors react stronger in absolute terms to a 10-K released by the treated group relative to the control group. Griffin (2003) finds that the SACAR of smaller firms is greater than larger firms. Based on the Griffin (2003) finding, row e = 0 results in Table 6 should have a negative sign; given that control firms are on average, smaller than treated firms. I interpret the results in Table 6 as evidence that the 10-K deadline reduction provides value, as argued by the SEC. There is no difference between the treated and the control group in the placebo period for Day 0, Column "Placebo" in Table 6. This provides further evidence that the results in the first column are due to the 10-K deadline reduction. The results still hold, when I concentrate on firms that submitted their 10-K within the last 10 and 5 days of their deadline, as shown in the second and third columns respectively.

[33] The SACAR difference between day 0 and day 1 for treated firms and control firms are 0.42% and 0.27% respectively. Both are statistically significant at the 1% and 5% level respectively.
[34] The SACAR is statistically insignificant for both placebo treated and placebo control regarding differentiating day 0 SACAR from any day that follows it.

(Insert Table 6)

Table 7 repeats the analysis of Table 6 but in a regression form. I include a flexible functional form of the public float in all regressions, to account for any continuous non-linear effect of public float on the cumulative SACAR. If the difference in public float is properly controlled for and if I successfully convinced you that there is no evidence to imply public float manipulation, then the SACAR should be directly attributed to the 15-day difference between the treated and the control group:

$$SACAR = \alpha_{Year} + \alpha_{Industry} + \beta_1 * LargeAcc + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_4 * dayDummy \quad (3a)$$

SACAR represents the standardized absolute cumulative abnormal return for days (-1, 0, 1), where Day 0 is the event day, 10-K submission. $\alpha_{Industry}$ represents the industry fixed effect, using the first two digits of SIC; α_{Year} represents year fixed effect, using the year in the fiscal year end date; PFL is the public float reported on the firms' 10-K; *dayDummy* is a dummy variable that takes the value of 1, if the 10-K submission day was on a Monday or a Friday. Dellavigna and Pollet (2009) find that the market underreacts to the news on Fridays. $LargeAcc$ is a dummy variable that takes the value of 1 if the firm is categorized as treated.

Table 7 shows that Table 6 results still hold in a regression form. Standard errors are clustered on the firm level.[35] Columns 1 and 2 of Table 7 show that the market reacts stronger, in absolute terms, to 10-Ks issued by treated firms. The results are economically significant, given that the control firms average SACAR is 2.17% while being treated causes the SACAR to increase by an average of 1.30%, which is more than 50% of the control firms SACAR. The results still hold if I limit the observations to firms that submitted their 10-K within the last 10-days of their deadline, Columns 3 and 4, or the last 5 days of the deadline, Columns 5 and 6. Columns 7 and 8 of Table 7 show that the LargeAcc coefficient loses its statistical significance, indicating no difference between the treated and control firms during the placebo period.

(Insert Table 7)

In the next section, I argue that the larger SACAR in the treated firms is due to the Grossman and Stiglitz (1980) setting and is not due to some alternative channel. If treated firms experience less information asymmetry, then this is likely due to fewer traders choosing to be informed, which, in turn, will cause less

[35] In the omitted results, I cluster by date of submission, in addition to firm. Griffin (2003) finds that days where there is high intensity of information, more than 20 filings, the market reacts stronger to the 10-K release. I would then expect that there is a correlation between different firms that release their 10-K on the same day. All results in Table 7 continue to hold.

information to be priced into the security reflecting the larger SACAR, when a treated firm submits its 10-K.

7. Information Asymmetry Analysis

Hypothesis 3: Treated firms will have less information asymmetry relative to control firms.

The intuition behind examining whether the information asymmetry of treated firms is lower relative to control firms is based on Diamond (1985). Investors would find that exerting effort is less appealing when a firm makes their private information public in a shorter time frame. Diamond (1985) compares firms that release their information publicly with firms that do not. Diamond (1985) argues that firms releasing their information rather than multiple informed agents gathering the information is better from a welfare perspective, conditional on the fact that the cost incurred by the firm is less than the total costs incurred by informed agents. Less information asymmetry for a specific firm, would indicate that there are less informed traders following that firm, Easley, Kiefer, O'Hara, and Paperman (1996).

a. Bid-Ask Spread:

Information asymmetry is proxied for with two variables. First, percentage bid-ask spread is used as a proxy, measured as $100 * \frac{ask-bid}{\frac{ask+bid}{2}}$. Bid-ask spread is related to the number of investors holding the asset. Then it also reflects the availability of information about the security, Merton (1987). Glosten and Milgrom (1985) have shown that illiquidity, as measured by bid-ask spread, is correlated with asymmetric information.

The general regression model for this section is estimated as follows:

$$Dep.Var = \alpha_{Year} + \alpha_{Industry} + \beta_1 * LargeAcc + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_4 * NumberOfAnalyst + \beta_5 * AnalystDispersion + \beta_6 * Price + \beta_7 * Size + \beta_8 * Volatility + \beta_9 * LogVolT + \beta_{10} * LogVolS + \beta_{11} * ShareHolders \quad (4)$$

where $NumberOfAnalyst$ represents the number of analysts following a firm; $AnalystDispersion$ is the standard deviation among analysts' forecasts. $Price$ refers to security price, $Size$ refers to firm value in dollars calculated by multiplying shares outstanding by security price, $Volatility$ refers to price volatility, *LogVolT* is the log value of the number of transactions, *LogVolS* is the log value of the dollar value of transactions, and $ShareHolders$ refers to the number of firm's shareholders. All of these variables are calculated on an annual basis for each firm.

All of the controls in Equation (4) have some effect on information asymmetry. Price volatility is positively correlated with information asymmetry, Mclnish, and Wood (1992). Moyer et al. (1989) argue that private information is more valuable for firms with higher price volatility. Mclnish and Wood (1992) document a negative relationship between trading volume and information asymmetry, as proxied by bid-ask spread. Brennan and Hughes (1991) argue that the number of firm analysts is inversely related to stock price, after controlling for size. Firm size is included in (4), even though it is correlated with public float because larger firms usually release more information than smaller firms and more analysts tend to follow larger firms. Finally, the number of shareholders is included as a control, because we expect that the number of analysts following a firm to be positively correlated with a number of shareholders.

Table 8 shows that treated firms experience lower information asymmetry, as proxied by bid-ask spread, confirming Hypothesis 3.

(Insert Table 8)

In the first column, no controls are added with the exception of treatment dummy and public float terms. In the second column, industry and year fixed effects are added. In the third column, five controls: *Price, Size, Volatility, LogVolT,* and *LogVolS* are added. In the fourth column, another control $ShareHolders$ is added. In the fifth column, all controls are added, as shown in Equation (4). The variable of interest is the treatment dummy, $LargeAcc,$ which takes a negative value in all models indicting lower information asymmetry for treated firms, and is statistically significant for Columns three and five. The average percentage bid-ask spread for the controls is 3.93%. Column 5 in Table 8 indicates that treated firms have around a 5.5% lower bid-ask spread.

Given that information asymmetry is not directly observable, Amihud illiquidity is used as another proxy to confirm that the results in Table 8 hold.

b. Illiquidity:

Amihud illiquidity (2002) is used as a proxy for information asymmetry in this subsection. Kyle (1985) shows that illiquidity, as measured by Kyles' lambda, is positively related to information asymmetry. Amihud illiquidity (2002) is measured as follows: The absolute daily value of stock return, divided by its dollar volume, which is then averaged over some period. [36] Amihud illiquidity can be interpreted as the daily price response associated with one dollar of trading volume, serving a rough measure of price impact.

[36] Amihud illiquidity $= 1{,}000{,}000 * \frac{|Return|}{|Price| * Volume}$.

Table 9 uses Amihud illiquidity as the dependent variable in Equation (4). Price, size, and volume are omitted from Equation (4) independent variables because they are used to calculate Amihud illiquidity. Amihud illiquidity is calculated using the preceding 60 days of observations of a 10-K submission. A probit model is used to estimate the coefficients, given that Amihud illiquidity values are always between 0 and 1.[37] Standard errors are clustered at the firm level. In the first column, the controls include public float terms and the treatment dummy, $LargeAcc$. The second column adds volatility as a control. The third column adds the number of shareholders as a control. The last column adds all the controls in Equation (4) with the exception of price, volume, and size. The coefficient of the treatment dummy is significant in all models and always has a negative sign. The negative sign of the treatment dummy indicates that the treated firms experience less information asymmetry, relative to control firms as proxied by illiquidity. [38] The $LargeAcc$ coefficient is hard to interpret when using a probit model. I calculate the partial effects for $LargeAcc$ to be: -.0016, -.0022, -0021 and -0.0011 for Columns 1, 2, 3 and 4 respectively. The average Amihud illiquidity for control firms is around 0.0067. Therefore, treated firms are associated with a 16.5% decrease in Amihud illiquidity when using the full model, Column 4.

(Insert Table 9)

8. Amendment Analysis

Hypothesis 4: Treated firms will make more mistakes in their 10-K.

Hypothesis 4 is motivated by the 46 firms' complaints received by the SEC, in regards to SEC $700 million rule and Prof. Benston's interview in CFO Magazine (2004):

"The real value of financial statements, says George Benston, is that they are a more-thorough, audited version of the numbers. They allow investors to learn things about the company that they might not by relying on earnings releases. By this logic, if rushing the process leads to mistakes, then the rule may do more harm than good."

I estimate the probability of issuing an amendment, when a firm is categorized as treated using Stammann, Heiss, and McFadden (2016) bias-corrected logit (BCL) estimator: $Amendment\ Dummy =$

[37] I do not add any fixed effects due to "Incidental Parameters Problem" in the probit model.

[38] As an auxiliary check (omitted), I run the same test using an OLS approach, which allows me to include both firm and year fixed effects. The results are qualitatively similar to the Table 14 results. Although I cannot say anything about statistical inferences in the OLS approach, I am comforted that the signs are correct and negative, for the treatment dummy.

$\alpha_{Industry} + \beta_1 * LargeAcc + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_5 * numOfBus + \beta_6 * numOfGeo + \varepsilon$ (5)

where $Amendment\ Dummy$ represent a dummy variable equal to one if a firm issued an amendment for a 10-K in a given fiscal year; $numOfBus$ is the number of business segments a firm has, and $numOfGeo$ is the number of geographic segments a firm has. I also include industry fixed effect, $\alpha_{Industry}$. I use the number of segments in both business and geography as controls, due to the Berger and Ofek (1995) finding that diversification implies an average value loss of 13% to 15%. Therefore, these firms might be less focus causing them to make more mistakes.

I use the BCL estimator for several reasons. First, the BCL estimator reduces the incidental parameter bias by analytical bias-correction, Newey and Hahn (2004). Second, the large dummy variable matrix trap is avoided by a special iteratively reweighted least squares demeaning algorithm, Stammann, Heiss, and McFadden (2016). I only include the industry fixed effect, given that the BCL estimator only works with a one-dimensional fixed effect.[39]

The first three columns in Table 10, which uses Sample 1 observations, shows that there is no difference between treated and control firms, regarding amendments issued. Columns 4 to 6 run the test on Sample 2 observations, results show that the newly treated firms tend to issue more amendments, relative to the control firms. Columns 7 to 9 run the test on Sample 3 observations, results show that the constrained treated firms issue more amendments relative to the unconstrained control firms. Interpreting the $LargeAcc$ coefficients in Equation (5) is difficult. I calculate the partial effects of the variable $LargeAcc$ to be: 0.015, 0.024, 0.014, 0.064, 0.075, 0.197, 0.076, 0.119 and 0.157 for Columns 1 to 9 respectively. Newly treated firms are 19.7% more likely to issue an amendment, Column 6, relative to the control firms. Constrained treated firms are 15.7% more likely to issue an amendment, Column 9, relative to the unconstrained control firms.[40] The 15-day deadline reduction applied to treated firms, caused treated firms to commit more mistakes, conditional on constraining the observations to newly treated firms or conditional on constraining the observations to constrained treated firms and unconstrained control firms. Given that there is a basis for the firm's argument when using Samples 2 and 3, I cannot fully reject Hypothesis 4.

[39] I run the same test adding the only year fixed effect, and the results are qualitatively similar. I also run the same test adding both year and industry fixed effects, without correcting for parameter incidental bias and the results are qualitatively similar, except the treatment dummy coefficient is somewhat larger. Running the same test using conditional logit, I get qualitatively identical results to my BIC results. Finally, I run the OLS version and get similar qualitatively results, as my original BIC estimator.

[40] I compare constrained treated firms with constrained control firms and find no difference in amendment issuance between the two groups in an omitted results.

(Insert Table 10)

I check whether the results in Table 10 are conservative, due to NT 10-K filings. For example, this would occur if treated firms are more likely to make mistakes, but treated firms avoid making these mistakes by issuing more NT 10-K filings. In an omitted table, I find that there is no difference between treated and control NT 10-K filings, except for the Sample 3. Treated firms are less likely to submit NT-10K, relative to control firms. This leads to the conclusion that constrained treated firms might reduce the probability of issuing an amendment if they made more use of their NT 10-K filing option.

9. Robustness

The analysis above is repeated with placebo thresholds. Firms with public float above $800 million are categorized as treated and firms below $800 million are categorized as controls, while constraining the sample to firms with a public float between $700 and $900 million.[41] Running this placebo test causes all previous statistically significant treatment dummy results to disappear. Most of the treatment dummy coefficients switch signs, providing further evidence that the results presented in the main analysis are due to the $700 million threshold and not to some unobservable factors.[42] In Tables A, B, C, and D, found in the Appendix, I repeat the analysis of Tables 7, 8, 9, and 10 but I use the placebo threshold of $800, *Placebo800,* instead of the actual treatment assignment the SEC imposes, *LargeAcc.*

In Table A, I no longer get statistically significant results regarding the treatment dummy, *Placebo800.* The *Placebo800* coefficient is now negative, indicating the placebo controls experience higher SACAR. The negative sign in the placebo threshold is consistent with previous literature that smaller firms experience a stronger market reaction when they release public information, Griffin (2003).

In Table B, I no longer get statistically significant results regarding the treatment dummy and the coefficient shown in Column 5 is now positive. This result indicates that the controls in the placebo threshold experience lower information asymmetry, which is opposite of the finding when I use the actual SEC threshold.

In Table C, I no longer get statistically significant results regarding the treatment dummy. The other controls retain their statistical significance and sign, providing evidence that the loss of *Placebo800* statistical significance is not due to a smaller sample.

[41] I concentrate on firms with a public float between $700 and $900 so that the real threshold of $700 million would not influence the results.
[42] In the omitted result, I conduct a robustness test using $600 million as my placebo threshold and focus on firms with a public float between $500 and $700. None of my treatment dummy coefficients are statistically significant.

In Table D, none of the models that includes all of the controls, Columns 3, 6 and 9, are statistically significant and the coefficient is now negative. This result is expected given there is no difference in terms of 10-K deadline between the two groups in this placebo test.

10. Conclusion

I find evidence consistent with the SEC $700 million rule serving its intended purpose. Due to the SEC $700 million rule, the market value 10-K filings released by the treated firms more, confirming Hypothesis 2. The stronger market reaction to 10-Ks issued by treated firms goes against the opposing firms' argument that 10-K filings are no longer informative, rejecting Hypothesis 1. The stronger market reaction to treated firms' 10-Ks is contrary to the opposition firms' argument, that post-2004 firms are required to make more use of 8-K filings which should reduce the value of 10-K filings. Griffin (2003) documents that smaller firms experience a stronger market reaction to public releases, which bias against finding SACAR results in this paper, given that treated firms are on average larger than the control firms. In the Section 6 analysis, I omit observations with amendments issuance, ruling out the stronger market reaction toward treated firms is due to "mistakes" in their 10-K.

I rely on the Grossman & Stiglitz (1980) model to explain the stronger market reaction toward treated firms 10-K submissions. I document that treated firms experience less information asymmetry relative to controls, using two different proxies for information asymmetry, confirming Hypothesis 3. Since treated firms experience less information asymmetry, I can assume that fewer traders are choosing to be informed, which, in turn, will cause less information to be priced into the security. This reflects the stronger market reaction when the treated firms submit their 10-Ks.

Finally, I do find a difference in the number of 10- K amendments issued between treated and control firms when I include all the observations, Sample 1, unless I focus on specific subsamples. Therefore, Hypothesis 4 cannot entirely be rejected. Newly treated firms are more likely to issue an amendment relative to control firms. Constrained treated firms are more likely to issue an amendment relative to unconstrained control firms.

Collectively these empirical evidence provide general and important implication to the literature. First, SEC decision to accelerate disclosure have made the market more efficient in the sense that traders are more homogenous now allowing for a better risk sharing. In addition, the decision made it more difficult for active traders to find positive NPV opportunity in terms of acquiring private information. An interesting future research based on the evidence found here would be whether mutual funds are less likely to hold treated firms relative to control firms after implementing the $700 million rule.

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12. Figures:

Figure 1

Figure 1 describes how firms enter (top graph) and exit (bottom graph) the treatment group. Firms enter the treated group, whenever their public float in the second quarter of their fiscal year is above $700 million, effectively starting from December 15th, 2006. Once a firm is categorized as treated, they exit this category, only if their public float drops below $500 million. Therefore, firms tend to get "stuck" in the treatment group, when their public float passes the $700 million threshold. The treated group includes firms subjected to the 60-day deadline and not necessarily firms with public float above $700 million. The mechanics of the SEC $700 million rule makes it possible to observe firms with public float below $700 million and are subjected to the 60-day deadline.

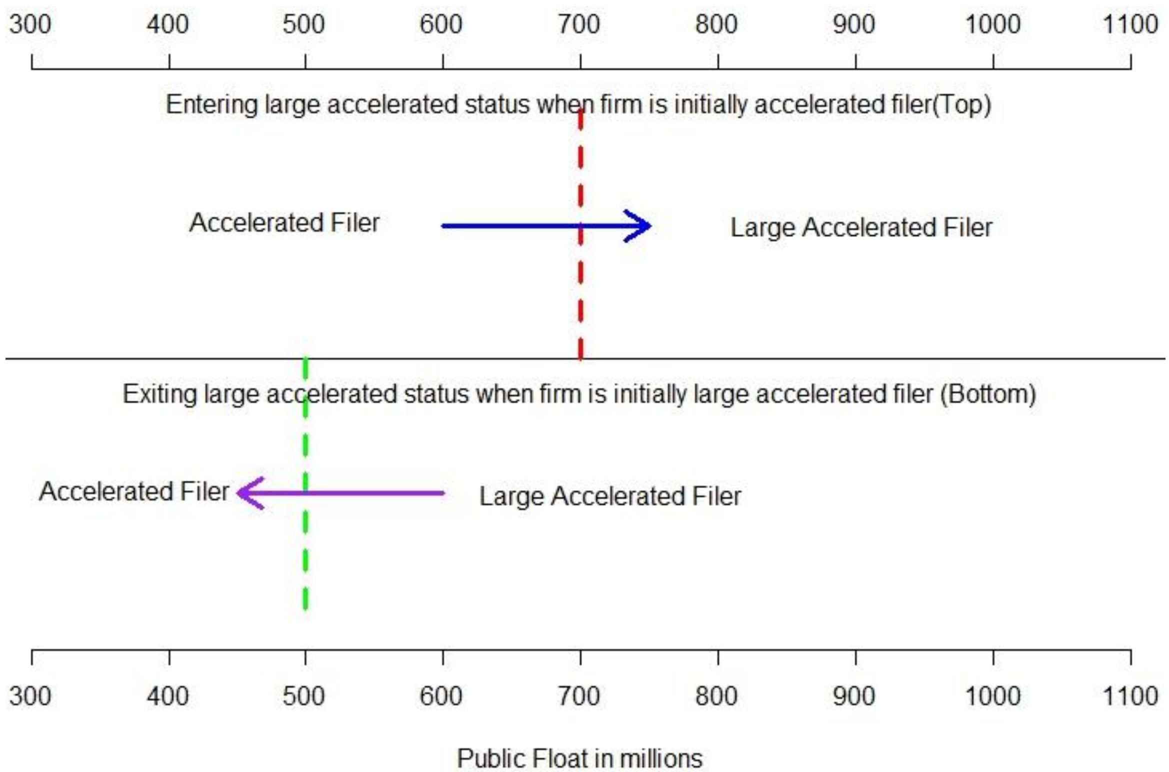


Figure 2

Firms sorting based on $10 million public float bins. Placebo Sample observations is used for the top chart. Sample 2, observations plus the Transition Sample observations are used in the bottom chart.

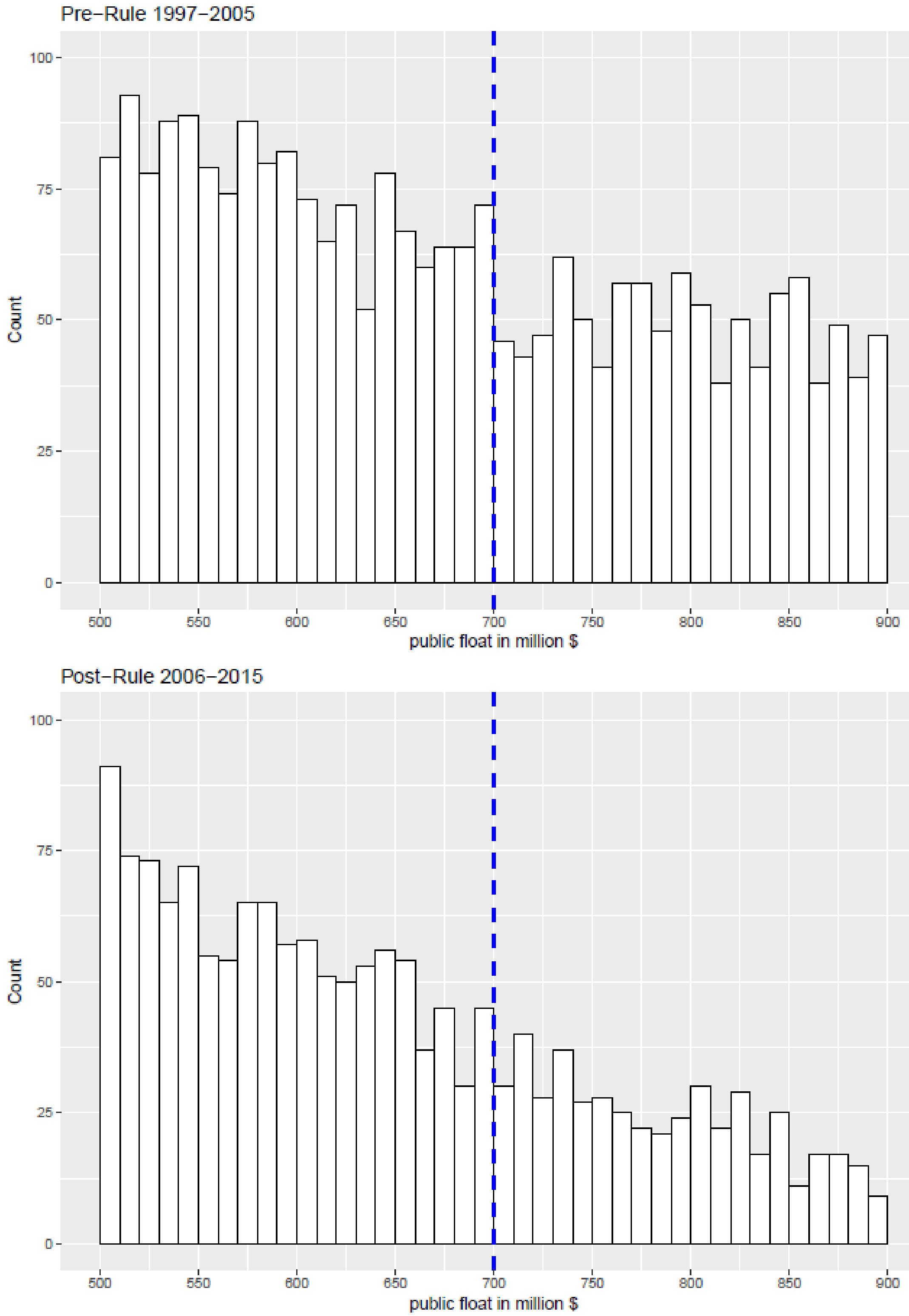
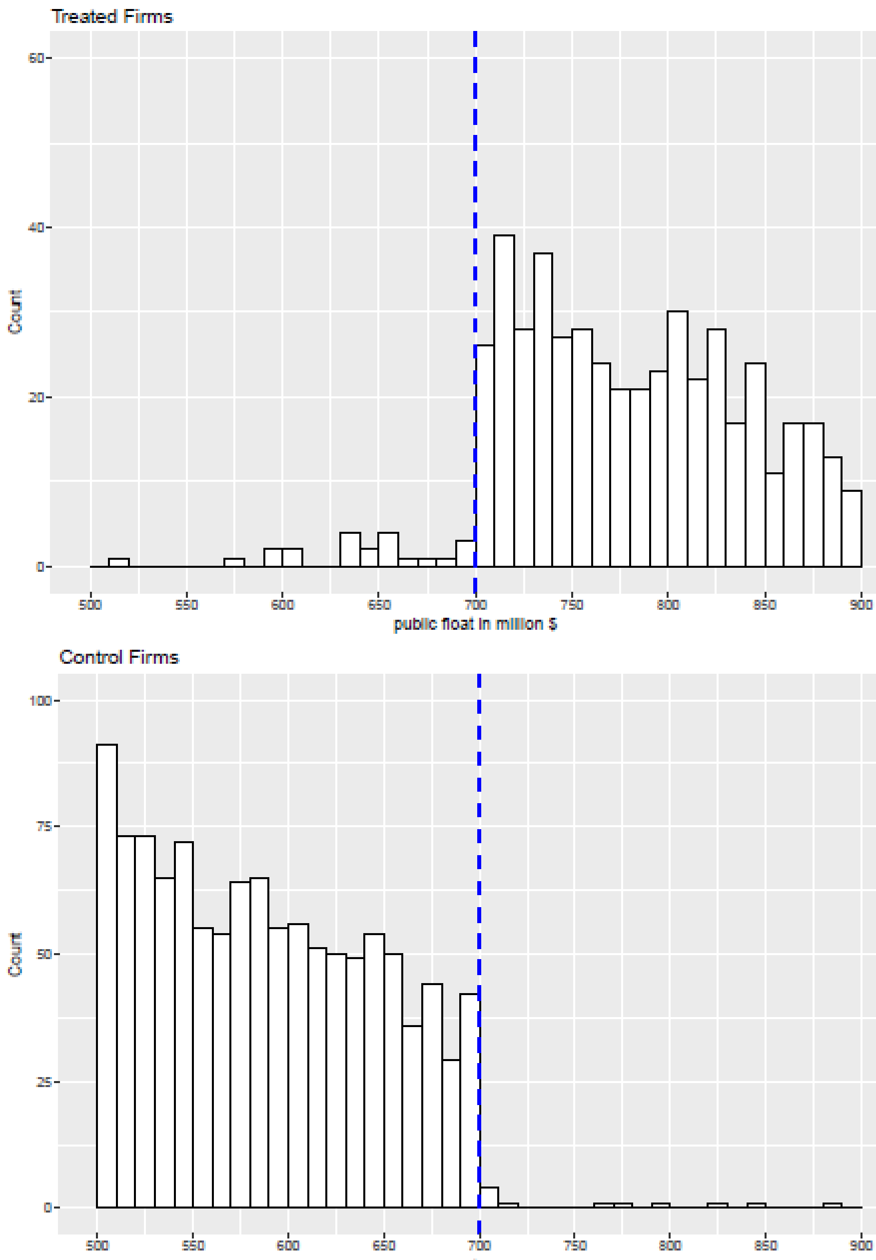


Figure 3

The top graph represent all of observations in Sample 2 and Transition Sample that categorized themselves as treated, "Large Accelerator Filers". The bottom graph represent all of observations in Sample 2 and Transition Sample that categorized themselves as control, "Accelerator Filers".



13. Tables

Table 1: Summary of SEC final rulings regarding 10-K and 10-Q deadline

Large accelerated filers are the treated group while accelerated filers are the control group. The SEC $700 million rule have created large accelerated filer category. Initially, the SEC $75 million rule was going to require accelerator filers to submit their 10-K within 60 days until the SEC made their announcement in regard to the new category of large accelerated filers. Table 1 is taken from the SEC $700 million ruling document (2005).

Category of Filer	Revised Deadlines For Filing Periodic Reports	
	Form 10-K Deadline	Form 10-Q Deadline
Large Accelerated Filer ($700MM or more)	75 days for fiscal years ending before December 15, 2006 and 60 days for fiscal years ending on or after December 15, 2006	40 days
Accelerated Filer ($75MM or more and less than $700MM)	75 days	40 days
Non-accelerated Filer (less than $75MM)	90 days	45 days

Table 2: Summary Statistics

Notes: Sample 1 observations are used here. NT filers' observations are added to Sample 1 when analyzing NT filings. *Days* is the number of days between the fiscal year ending date and the date when the 10-K was submitted to the SEC. *TotalAmendments* represents the total number of amendments a firm have issued for a specific 10-K. *PublicFloat* represents the public float of a firm in millions US dollars. *LargeAcc* is a dummy variable that indicates if the observation is treated or not. *NT 10-K* is a dummy variable that indicates if the observation have filed an NT 10-K. *NumOfBus* is a discrete variable that indicates how many business segments a firm operates in. *NumOfGeo* is a discrete variable that indicates how many geographic segments a firm operates in. Price is the mean stock price for a firm in a given year. *Size* is the total value of the firm calculated as (price)*(number of shares outstanding) in millions US dollars. *Volatility* represents price volatility calculated as sd(return)*sqrt(12). *VolumeT* is the sum of all trades for a given frim for a given year, numbers are in millions. *VolumeS* is the dollar value of *VolumeT* which is calculated as VolumeT*Price, value in million US dollars. *LogVolT* is the log value of *VolumeT*. *LogVolS* is the log value of *VolumeS*. *Shareholders* is the number of firms' shareholders. *NumberOfAnalysts* represents how many analysts are following a firm. *AnalystDispersion* represents the standard deviation between analyst forecast in regards to earnings. *Standarized Abnormal Return* is the abnormal return for a specific firm and is calculated as

$$SACAR_{i,t} = \left|ACAR_{i,t}\right| / \sigma \left|ACAR_{i,t}\right|$$

Where SACAR stands for standardized absolute cumulative abnormal return (SACAR) and proxies for investor response. The subscripts i and t represent a specific firm and day respectively. ACAR stands for absolute cumulative abnormal return and it is measured as the absolute value of firm i return on day t minus market return on day t. The standard deviation of ACAR, σ, is measured over a short window. More specifically, I use ten days to measure it. These 10 days include: days -10 to -6 and 6 to 10 where day 0 is the filing day. Percentage *Bid-Ask Spread* is measured as follows: $100 * \frac{ask-bid}{\frac{ask+bid}{2}}$. *Amihud illiquidity* is measured as follows: $1{,}000{,}000 * \frac{|Return|}{|Price| * Volume}$.

(Table 2 is in the next page)

	# of Observations	min	max	median	mean	std.dev
NT Filers	2540	0	1	0	0.04	0.20
days	2423	0	78	59	59.54	9.37
totalamendments	2423	0	4	0	0.13	0.38
PublicFloat (in millions dollars)	2423	500	900	658.9	671.45	112.62
LargeAcc	2423	0	1	1	0.59	0.49
Percentage Bid-Ask Spread	2036	0.20	17.10	3.40	3.86	1.84
Amihud Illiquidity (multiplied by 100)	2036	0.000049	0.64	0.0028	0.0048	0.02
SACAR	1875	0	73.83	1.26	2.41	3.73
Price	1804	0.88	89.02	21.38	24.17	14.81
Size (in millions dollars)	1804	27.82	2781.10	776.69	850.29	335.95
Volatility	1804	0.09	1.21	0.38	0.43	0.19
VolumeT (in millions)	1804	5.97	3936.92	86.84	166.05	262.48
VolumeS (in millions dollars)	1804	177.04	21892.27	1793.00	2413.62	2071.16
logVolT	1804	6.78	9.60	7.94	7.98	0.43
logVolS	1804	8.25	10.34	9.25	9.27	0.30
ShareHolders	1762	3	36448	574	3347.08	6169.81
NumberOfAnalysts	1468	2	24	6.5	7.29	4.05
AnalystDispersion	1468	0	0.76	0.02	0.05	0.07
NumOfBus	1204	1	10	2	2.49	1.78
NumOfGeo	1204	1	16	3	3.29	2.63

Table 3: Covariate Balance

PublicFloat represents the public float of a firm in millions US dollars. *LargeAcc* is a dummy variable that indicates if the observation is treated or not. *NT 10-K* is a dummy variable that indicates if the observation have filed an NT 10-K. *NumOfBus* is a discrete variable that indicates how many business segments a firm operates in. *NumOfGeo* is a discrete variable that indicates how many geographic segments a firm operates in. Price is the mean stock price for a firm in a given year. Size is the total value of the firm calculated as (price)*(number of shares outstanding) in millions US dollars. *Volatility* represents price volatility calculated as sd(return)*sqrt(12). *VolumeT* is the sum of all trades for a given frim for a given year, numbers are in millions. *VolumeS* is the dollar value of *VolumeT* which is calculated as VolumeT*Price, value in million US dollars. *Shareholders* is the number of firms' shareholders. *NumberOfAnalysts* represents how many analysts are following a firm. *AnalystDispersion* represents the standard deviation between analyst forecast in regards to earnings

Variable (mean)	Treated (obs)	Control (obs)	t.test (p-value)
Price	24.30 (1097)	23.97 (707)	0.65
Size (in millions dollars)	914.79 (1097)	750.21 (707)	<0.01
Volatility	0.43(1097)	0.42 (707)	0.50
VolumeT (in millions)	188.71 (1097)	130.88 (707)	<0.01
VolumeS (in millions dollars)	2771.16 (1097)	1885.84 (707)	<0.01
ShareHolders	3608.14 (1071)	2942.47 (691)	<0.05
NumberOfAnalyst	7.84 (904)	6.42 (564)	<0.01
AnalystDispersion	0.0492 (904)	0.0433 (564)	0.12
NumOfBus	2.51 (720)	2.47 (484)	0.70
NumOfGeo	3.33 (720)	3.23 (484)	0.50
PublicFloat	729.3 (1422)	589.27 (1001)	<0.01

Table 4: McCrary Density Test

This test is based on McCrary (2008). The binwidth is calculated as follows: $2 * sd(Public\ Float) * obs(Public\ Float)^{-0.5}$. Where *sd* refers to standard deviation and *obs* refers to number of observations. The bandwidth is calculated using Fan and Gijbels (1996, Section 4.2).. The SEC $700 million rule was announced on December 2005. The SEC $700 million rule was implemented on December 2006.

Year	P-Value (Obs.)	Null hypothesis of no sorting
1997 (placebo)	0.19 (510)	Fail to reject null
1998 (placebo)	0.35 (263)	Fail to reject null
1999 (placebo)	0.78 (365)	Fail to reject null
2000 (placebo)	0.06 (289)	Fail to reject null
2001 (placebo)	0.17 (306)	Fail to reject null
2002 (placebo)	0.77 (284)	Fail to reject null
2003 (placebo)	0.33 (263)	Fail to reject null
2004 (placebo)	0.06 (310)	Fail to reject null
2005 (placebo)	0.66 (260)	Fail to reject null
2006 (Transition)	0.77 (264)	**Fail to reject null**
2007	0.83 (193)	**Fail to reject null**
2008	0.10 (118)	Fail to reject null
2009	0.57 (75)	Fail to reject null
2010	0.61 (144)	Fail to reject null
2011	0.58 (185)	Fail to reject null
2012	0.89 (105)	Fail to reject null
2013	0.69 (117)	Fail to reject null
2014	0.25 (150)	Fail to reject null
2015	0.51 (110)	Fail to reject null
1997-2005 (placebo)	0.01 (2477)	Reject null
2006-2015	0.59 (1624)	**Fail to reject null**

Table 5: Deadline Enforcement

Test whether the treated firms followed the 60-day deadline and whether the control firms followed the75-day deadline. Post December 15th, 2006 period is when the SEC $700 million rule went into effect. Pre December 15th, 2003 period is when the deadline was 90 days for all firms. Between December 15th, 2003 and December 21st, 2005, the deadline was reduced to 75 days for firms studied in this paper. Between December 21st, 2005 and December 15th, 2006, is when the rule was announced but not yet enforced.

	Large Accelerator (# of obs)	Accelerator (# of obs)
Avg days to release 10-K (Pre 15 December, 2003)	81.459 (751)	81.958 (1143)
Avg days to release 10-K (between 15 December, 2003 and 21 December, 2005)	69.925 (227)	70.744 (356)
Avg days to release 10-K (21 December, 2005 and 15 December, 2006)	67.214 (126)	70.304 (138)
Avg days to release 10-K (Post 15 December, 2006)	56.218 (1456)	64.334 (1402)
Welch Two Sample t-test (Pre 15 December, 2003)	t = -0.88048, df = 1552.1, p-value = 0.3787	
Welch Two Sample t-test (between 15 December, 2003 and 21 December, 2005)	t = -1.1551, df = 393.17, p-value = 0.2488	
Welch Two Sample t-test (21 December, 2005 and 15 December, 2006)	t = -2.9619, df = 211.26, p-value = 0.003408	
Welch Two Sample t-test (Post 15 December, 2006)	t = -22.35, df = 1712, p-value < 2.2e-16	

Table 6: Abnormal Return Difference Between Treated and Control

Test whether the difference between the treated group, t, and the control group, c, standardized absolute cumulative abnormal return (SACAR), where Day 0 is the event day, 10-K submission, is statistically significant at 10-K release date, e = 0. More specifically: $SACAR_{t,e} - SACAR_{c,e}$ (3)

Column "All", applies equation (3) for day 0, e = 0, and the days surrounding it, in each different column, using all observations (1,106 observations for the treated and 769 for the control group). Column "10 days", repeats the analysis in Column "All" except it reduces the observations to the ones that submitted their 10-K within the last 10 days of the deadline. (treated group observations falls to 985, and control group observations falls to 387). Column "5 days", repeats the analysis in Column "All" except it reduces the observations to the ones that submitted their 10-K within the last 5 days of the deadline. (treated group observations falls to 796, and control group observations falls to 251). Finally, Column "Placebo", repeats the analysis in Column "All" except it uses placebo observations (743 observations for the treated and 1,144 for the control group).

Days	All	10 days	5 days	Placebo
-4	0.0589	0.0502	0.0611	0.0029
-3	0.290**	0.221	0.0914	-0.0387
-2	0.0883	0.0434	0.0314	0.0328
-1	0.133	0.0652	0.0557	0.0132
0	0.411**	0.433**	0.542***	0.0413
1	0.245*	0.269*	0.512***	0.1760*
2	-0.0937	-0.193	-0.130	0.0614
3	0.0269	-0.0262	-0.0267	0.0442
4	-0.0111	-0.0593	-0.0833	0.0645

Table 7: RD Approach for 10-K Submission Market Reaction

Repeat the analysis in Table 6 but in an OLS regression form.

$$SACAR = \alpha_{Year} + \alpha_{Industry} + \beta_1 * LargeAcc + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_4 * dayDummy \quad (3a)$$

SACAR represents the standardized absolute cumulative abnormal return for days (-1, 0 , 1) where day 0 is the event day, 10-K submission. $\alpha_{Industry}$ represents industry fixed effect using the first two digits of SIC; α_{Year} represents year fixed effect using the year of fiscal year end date; PFL is the public float reported on the firms' 10-K; $dayDummy$ is a dummy variable that take the value of 1 if the 10-K submission day was on a Monday or a Friday. $LargeAcc$ is a dummy variable that takes the value of 1 if the firm is categorized as treated. The first two columns, uses all the observations. The third and fourth columns, uses observations that submitted their 10-K within the last ten days of the deadline. The fifth and sixth columns, uses observations that submitted their 10-K within the last five days of the deadline. The last two columns use observations in the placebo period of 1997-2005. In columns two, four, six and eight I add industry fixed effects and year fixed effects. Clustered standard errors are reported in brackets. $*$, $**$, and $***$ denote two-sided statistical significance at the 10%, 5%, and 1% levels, respectively. Clustering is done on firm level. There are at least 604 firm clusters (for Columns 5 and 6).

	Dependent variable:							
	SACAR							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LargeAcc	0.855*	1.296**	0.721	1.305**	0.918*	1.424**	0.085	0.132
	(0.515)	(0.574)	(0.534)	(0.622)	(0.545)	(0.625)		(0.576)
dayDummy	-0.616*	-0.632	-0.521	-0.572	-0.817*	-0.907**	-0.100	-0.045
	(0.366)	(0.385)	(0.425)	(0.408)	(0.451)	(0.445)		(0.191)
Constant	100.230**		129.177**		114.575*		51.859	
	(46.829)		(50.170)		(59.829)			
Public Float Terms	X	X	X	X	X	X	X	X
Industry FE		X		X		X		X
Year FE		X		X		X		X
Observations	1,875	1,875	1,372	1,372	1,047	1,047	1,887	1,887
Adjusted R^2	0.006	0.023	0.007	0.013	0.007	0.003	0.001	0.013

Table 8: Bid-Ask Spread Proxy for Information Asymmetry

Results are estimated using the following OLS regression:

$$Bid - Ask\ Spread = \alpha_{Year} + \alpha_{Industry} + \beta_1 * LargeAcc + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_4 * NumberOfAnalysts + \beta_5 * AnalystDispersion + \beta_6 * Price + \beta_7 * Size + \beta_8 * Volatility + \beta_9 * LogVolT + \beta_{10} * LogVolS + \beta_{11} * ShareHolders \quad (4)$$

where $LargeAcc$ is dummy variable that takes the value of 1 if the firm is categorized treated; $\alpha_{Industry}$ represent industry fixed effect; α_{Year} represent year fixed effect; PFL is the public float reported on the firms' 10-K; *NumberOfAnalysts* represent the number of analyst following a firm; $AnalystDispersion$ is the standard deviation among analysts forecasts; $Price$ refers to security price; $Size$ refer to firm value in dollars calculated by multiplying shares outstanding by security price; $Volatility$ refers to price volatility; LogVolT is the log value of the number of transactions; *LogVolS* is the log value of the dollar value of transactions; and $ShareHolders$ refers to number of shareholders a firm has. The dependent variable, percentage $Bid - Ask\ Spread$, is measured as follows $100 * \frac{ask-bid}{\frac{ask+bid}{2}}$. In the first column, no controls are added with the exception of treatment dummy and public float terms. In the second column, industry and year fixed effects are added. In the third column, five controls: *Price*, *Size*, *Volatility*, *LogVolT*, and *LogVolS* are added. In the fourth column, another control $ShareHolders$ is added. In the fifth column, all controls are added, as shown in Equation (4). Clustered standard errors are reported in brackets. $*$, $**$, and $***$ denote two-sided statistical significance at the 10%, 5%, and 1% levels, respectively. Clustering is done on firm level. There are at least 731 firm clusters (for the fifth column).

(Table 8 is in the next page)

Table 8: Cont.

	Dependent variable:				
	Bid-Ask Spread				
	(1)	(2)	(3)	(4)	(5)
LargeAcc	-0.020	-0.132	-0.162^{*}	-0.143	-0.216^{**}
	(0.124)	(0.099)	(0.093)	(0.094)	(0.093)
Price			0.019^{***}	0.015^{**}	0.013^{*}
			(0.006)	(0.006)	(0.008)
Size			-0.000^{***}	-0.000^{***}	-0.000^{***}
			(0.000)	(0.000)	(0.000)
Volatility			3.382^{***}	3.522^{***}	3.682^{***}
			(0.318)	(0.324)	(0.290)
logVolT			1.809^{***}	1.538^{***}	1.295^{***}
			(0.366)	(0.355)	(0.397)
logVolS			-1.488^{***}	-1.196^{***}	-1.220^{***}
			(0.407)	(0.400)	(0.438)
ShareHolders				0.00000	0.00001
				(0.00001)	(0.00001)
NumberOfAnalysts					0.029^{***}
					(0.009)
AnalystDispersion					2.821^{***}
					(0.813)
Constant	-7.643				
	(12.103)				
Public Float Terms	X	X	X	X	X
Industry FE		X	X	X	X
Year FE		X	X	X	X
Observations	2,036	2,036	1,807	1,761	1,467
Adjusted R^2	-0.0004	0.394	0.554	0.557	0.589

Table 9: Amihud Illiquidity Proxy for Information Asymmetry

Results are estimated using the following probit model:

$$\text{Amihud illiquidity} = \beta_0 + \beta_1 * LargeAcc + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_4 * NumberOfAnalysts + \beta_5 * AnalystDispersion + \beta_8 * Volatility + \beta_{11} * ShareHolders \quad (4a)$$

where $LargeAcc$ is dummy variable that takes the value of 1 if a firm is categorized as treated; PFL is the public float reported on the firms' 10-K; $NumberOfAnalyst$ represents the number of analyst following a firm; $AnalystDispersion$ is the standard deviation among analysts forecasts; $Volatility$ refers to price volatility; and $ShareHolders$ refers to number of shareholders a firm has. The dependent variable Amihud illiquidity is measured as follows: $1{,}000{,}000 * \frac{|Return|}{|Price| * Volume}$. In the first column, the controls include public float terms and the treatment dummy, LargeAcc. The second column adds volatility as a control. The third column adds the number of shareholders as a control. The last column adds all the controls in Equation (4a). Clustered standard errors are reported in brackets. $*$, $**$, and $***$ denote two-sided statistical significance at the 10%, 5%, and 1% levels, respectively. Clustering is done on firm level. There are at least 731 firm clusters (for the fourth column).

	Dependent variable:			
	Amihud Illiquidity			
	(1)	(2)	(3)	(4)
LargeAcc	-0.115**	-0.170***	-0.159***	-0.089**
	(0.051)	(0.059)	(0.060)	(0.039)
Volatility		0.796***	0.802***	0.562***
		(0.219)	(0.232)	(0.093)
ShareHolders			-0.00000	0.00000
			(0.00000)	(0.00000)
NumberOfAnalysts				-0.025***
				(0.003)
AnalystDispersion				0.345
				(0.232)
Constant	9.306	8.079	7.164	-1.513
	(8.696)	(9.021)	(9.323)	(2.705)
Public Float Terms	X	X	X	X
Observations	2,092	1,807	1,761	1,467
Log Likelihood	-15.096	-12.389	-12.154	-6.476
Akaike Inf. Crit.	40.192	36.778	38.307	30.952

Table 10: RD Approach for Amendment Dummy

Results are estimated using Stammann, Heiss, and McFadden (2016) bias-corrected logit (BCL) estimator (BIFE function in R): $Amendment\ Dummy = \alpha_{Industry} + \beta_1 * LargeAcc + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_5 * BusSegm + \beta_6 * GeoSegm + \varepsilon$

where $Amendment\ Dummy$ represents a dummy variable equal to one if firm have issued any amendment for a 10-K in a given fiscal year; $LargeAcc$ is dummy variable that takes the value of 1 if the firm is categorized as treated; PFL is the public float reported on the firms' 10-K; $numOfBus$ is the number of business segments a firm operates in; and $numOfGeo$ is the number of geographic segments a firm operates in. Industry fixed effect, $\beta_{Industry}$, using the first two digits of SIC is also added. Columns 1, 2 and 3 uses Sample 1. Columns 4, 5, and 6 uses Sample 2. Columns 7, 8 and 9 uses Sample 3. Column 1, 4 and 7 only adds public float terms. Column 2, 5 and 8 adds fixed effects. Column 3, 6 and 9 adds the controls. Robust standard errors are reported in brackets. $*$, $**$, and $***$ denote two-sided statistical significance at the 10%, 5%, and 1% levels, respectively. The pseudo R-Squared in all columns are calculated as $1 - \frac{1}{1+2*\frac{\log L_1 - \log L_0}{N}}$ where $\log L_1$ denote the maximum loglikelihood value of the model of interest and $\log L_0$ denote the maximum loglikelihood function when all parameters except the intercept are set as zero. Amemiya (1981).

	Amendment Dummy								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
LargeAcc	0.14	0.222	0.132	0.517	0.611	1.372**	0.547**	0.856***	1.176***
	(0.161)	(0.168)	(0.247)	(0.409)	(0.413)	(0.634)	(0.259)	(0.286)	(0.451)
NumOfBus			-0.06			-0.06			-0.09
			(0.058)			(0.081)			(0.107)
NumOfGeo			0.077**			0.094*			0.097
			(0.036)			(0.049)			(0.064)
Constant	12.36			12.13			23.18		
	(17.27)			(30.41)			(29.34)		
Public Float Terms	X	X	X	X	X	X	X	X	X
Industry FE		X	X		X	X		X	X
pseudo R-Squared	0.002	0.037	0.067	0.005	0.061	0.119	0.011	0.096	0.197
Observations	2423	2423	1204	1360	1260	662	840	840	380

Appendix:

Table A: RD Approach for 10-K Submission Market Reaction

Repeat the analysis in Table 7 but uses a placebo threshold of $800 million to categorize firms into treated and control.

$$SACAR = \alpha_{Year} + \alpha_{Industry} + \beta_1 * Placebo800 + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_4 * dayDummy \quad (3a)$$

SACAR represents the standardized cumulative abnormal return for days (-1, 0 , 1) where day 0 is the event day, 10-K submission. $\beta_{Industry}$ represents industry fixed effect; β_{Year} represent year fixed effect; PFL is the public float reported on the firms' 10-K; *dayDummy* is a dummy variable that take the value of 1 if the 10-K submission day was on a Monday or a Friday. $Placebo800$ is a dummy variable that takes the value of 1 if the firm public float is above $800 million. The first two columns uses all observations. The third and fourth columns uses observations that submitted their 10-K within the last ten days of the deadline. The last two columns uses observations that submitted their 10-K within the last five days of the deadline. In columns two, four and six I add industry fixed effects using the first two digits of SIC and year fixed effects. Clustered standard errors are reported in brackets. $*$, $**$, and $***$ denote two-sided statistical significance at the 10%, 5%, and 1% levels, respectively. Clustering is done on firm level. There are at least 377 firm clusters (for Columns 5 and 6).

	Dependent variable:					
	SACAR					
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo800	-3.204	-3.091	-1.881	-2.533	-2.703	-2.964
	(2.287)	(2.071)	(2.129)	(2.241)	(2.302)	(2.588)
dayDummy	-0.347	-0.399	-0.305	-0.579	-0.608	-0.903
	(0.628)	(0.670)	(0.649)	(0.698)	(0.699)	(0.737)
Constant	458.283		14.971		722.210	
	(1,374)		(1,325)		(1,499)	
Public Float Terms	X	X	X	X	X	X
Industry FE		X		X		X
Year FE		X		X		X
Observations	749	749	667	667	539	539
Adjusted R^2	0.011	0.018	0.009	-0.011	0.004	-0.035

Table B: Bid-Ask Spread Proxy for Information Asymmetry

Repeat the analysis in Table 13 but uses a placebo threshold of $800 million to categorize firms into treated and control.

$$Bid - Ask\ Spread = \alpha_{Year} + \alpha_{Industry} + \beta_1 * Placebo800 + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_4 * NumberOfAnalysts + \beta_5 * AnalystDispersion + \beta_6 * Price + \beta_7 * Size + \beta_8 * Volatility + \beta_9 * LogVolT + \beta_{10} * LogVolS + \beta_{11} * ShareHolders \quad (4)$$

where $Placebo800$ is a dummy variable that takes the value of 1 if the firm public float is above $800 million; $\beta_{Industry}$ represents industry fixed effect; β_{Year} represents year fixed effect; PFL is the public float reported on the firms' 10-K; *NumberOfAnalysts* represents the number of analyst following a firm; $AnalystDispersion$ is the standard deviation among analysts forecasts; $Price$ refers to security price; $Size$ refers to firm value in dollars calculated by multiplying shares outstanding by security price; $Volatility$ refers to price volatility*; LogVolT* is the log value of the number of transactions; *LogVolS* is the log value of the dollar value of transactions; and $ShareHolders$ refers to the number of firms'. The percentage bid-ask spread is measured as follows: $100 * \frac{ask-bid}{\frac{ask+bid}{2}}$.

In the first column, I do not add any controls with the exception to large accelerator dummy and public float terms. In the second column, I add industry and year fixed effects. In the third column, I add five controls: Price, Size, Volatility, LogVolT and LogVolS. In the fourth column I add another control $ShareHolders$. In the fifth column I add all controls as shown in equation (4). Clustered standard errors are reported in brackets. $*$, $**$, and $***$ denote two-sided statistical significance at the 10%, 5%, and 1% levels, respectively. Clustering is done on firm level. There is at least 408 firm clusters (for the fifth column).

(Table B is in the next page)

	Dependent variable:				
	Bid-Ask Spread				
	(1)	(2)	(3)	(4)	(5)
Placebo800	0.429	0.135	0.048	0.156	0.210
	(0.323)	(0.267)	(0.238)	(0.226)	(0.249)
Price			0.025**	0.024**	0.029**
			(0.010)	(0.010)	(0.013)
Size			-0.000**	-0.000***	-0.000*
			(0.000)	(0.000)	(0.000)
Volatility			3.656***	3.890***	3.851***
			(0.434)	(0.438)	(0.432)
logVolT			2.336***	2.240***	2.396***
			(0.602)	(0.614)	(0.730)
logVolS			-1.752***	-1.740***	-2.189***
			(0.640)	(0.652)	(0.769)
ShareHolders				0.00000	-0.00000
				(0.00001)	(0.00001)
NumberOfAnalysts					0.023*
					(0.013)
AnalystDispersion					2.665***
					(0.906)
Constant	-36.554				
	(307.338)				
Public Float Terms	X	X	X	X	X
Industry FE		X	X	X	X
Year FE		X	X	X	X
Observations	809	809	729	711	616
Adjusted R^2	0.002	0.371	0.581	0.588	0.612

Table C: Amihud Illiquidity Proxy for Information Asymmetry

Notes: Repeat the analysis in Table 14 but uses a placebo threshold of $800 million to categorize firms into treated and control.

$$\text{Amihud illiquidity} = \beta_0 + \beta_1 * Placebo800 + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_4 * NumberOfAnalysts + \beta_5 * AnalystDispersion + \beta_8 * Volatility + \beta_{11} * ShareHolders \quad (4a)$$

where $Placebo800$ is a dummy variable that takes the value of 1 if the firm public float is above $800 million; PFL is the public float reported on the firms' 10-K; $NumberOfAnalysts$ represents the number of analyst following a firm; $AnalystDispersion$ is the standard deviation among analysts forecasts; $Volatility$ refers to price volatility; and $ShareHolders$ refers to the number of firms' shareholders. The dependent variable Amihud illiquidity is measured as follows: $1{,}000{,}000 * \frac{|Return|}{|Price| * Volume}$. In the first column, the only controls are the public float terms while the main independent variable is the treatment dummy. The second column add volatility as a control. In the third column, I add number of shareholders as a control. In the last column, column four, I add all the controls in equation (4a). Clustered standard errors are reported in brackets. $*$, $**$, and $***$ denote two-sided statistical significance at the 10%, 5%, and 1% levels, respectively. Clustering is done on firm level. There is at least 408 firm clusters (for the fourth column).

	Dependent variable:			
	Amihud Illiquidity			
	(1)	(2)	(3)	(4)
Placebo800	-0.118	-0.045	-0.045	-0.059
	(0.122)	(0.070)	(0.072)	(0.075)
Volatility		0.526***	0.559***	0.635***
		(0.113)	(0.116)	(0.125)
ShareHolders			0.00000	-0.00000
			(0.00000)	(0.00000)
NumberOfAnalysts				-0.025***
				(0.004)
AnalystDispersion				-0.123
				(0.261)
Constant	343.462*	105.133	106.712	102.450
	(207.979)	(79.876)	(81.064)	(90.687)
Public Float Terms	X	X	X	X
Observations	825	729	711	616
Log Likelihood	-3.109	-2.484	-2.452	-2.133
Akaike Inf. Crit.	16.218	16.968	18.904	22.267

Table D: RD Approach for Amendment Dummy

Repeat the analysis in Table 15 but uses a placebo threshold of $800 million to categorize firms into treated and control.

$$\begin{aligned} &\mathit{Amendment\ Dummy} \\ &\quad = \beta_0 + \beta_{Industry} + \beta_1 * Placebo800 + \beta_2 * PFL + \beta_3 * PFL^2 + \beta_4 * PFL^3 + \beta_5 * numOfBus \\ &\quad + \beta_6 * numOGeo + \varepsilon \end{aligned}$$

where $Amendment\ Dummy$ represents a dummy variable equal to one if firm have issued any amendment for a 10-K in a given fiscal year; $Placebo800$ is a dummy variable that takes the value of 1 if the firm public float is above $800 million; PFL is the public float reported on the firms' 10-K; $numOBus$ is the number of business segments a firm operates in; and $numOGeo$ is the number of geographic segments a firm operates in. I also include industry fixed effect, $\beta_{Industry}$, using the first two digits of SIC. Columns 1, 2 and 3 uses Sample 1. Columns 4, 5, and 6 uses Sample 2. Columns 7, 8 and 9 uses Sample 3. Column 1, 4 and7 only adds public float terms. Column 2, 5 and 8 adds fixed effects. Column 3, 6 and 9 adds the controls. Robust standard errors are reported in brackets. $*$, $**$, and $***$ denote two-sided statistical significance at the 10%, 5%, and 1% levels, respectively. The pseudo R-Squared in all columns are calculated as $1 - \frac{1}{1+2*\frac{\log L_1 - logL_0}{N}}$ where $\log L_1$ denote the maximum loglikelihood value of the model of interest and $\log L_0$ denote the maximum loglikelihood function when all parameters except the intercept are set as zero. Amemiya (1981).

	Amendment Dummy								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Placebo800$	-1.06**	-1.06*	-0.64	-1.49***	-1.08	-0.72	-1.86**	-1.81**	-0.00
	(0.528)	(0.551)	(0.810)	(0.788)	(0.881)	(1.837)	(0.785)	(0.892)	(1.566)
numOfBus			-0.02			0.133			-0.09
			(0.09)			(0.166)			(0.171)
numOfGeo			0.101*			0.2*			0.279**
			(0.06)			(0.108)			(0.131)
Constant	939.8			1990			1005		
	(453.8)			(803.8)			(688.3)		
Public Float Terms	X	X	X	X	X	X	X	X	X
Industry FE		X	X		X	X		X	X
pseudo R-Squared	0.535	0.550	0.546	0.675	0.687	0.701	0.552	0.583	0.637
Observations	959	959	494	358	358	178	331	331	146