The Effect of Mandatory Information Disclosure on Financial Constraints

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ABSTRACT

This paper studies the effect of mandatory disclosure on firms' financial constraints and investment policies. Using a difference-in-difference estimation, I analyze a quasi-natural experiment that exogenously changed voluntarily reported information into mandatory disclosure. I find that firms became more equity-constrained but less debt-constrained. Firms also invested more in physical capital but kept expenditure in R&D unchanged. The effect on equity constraints was stronger when investors had little information about the firm and when the firm had difficulties to differentiate from other firms. Alternatively, the effect on debt constraints was stronger when the ability to guarantee a permanent disclosure policy was more relevant. The findings suggest that mandatory disclosure benefits debt by reducing the agency cost (Jensen and Meckling (1976)) but harms equity by shutting down the signaling mechanism inherent to voluntary disclosure (Myers and Majluf (1984)).

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I Introduction

Asymmetric information is one of the main theoretical explanations for why financial constraints exist. For example, in the presence of adverse selection problems, firms with profitable investment opportunities might find it difficult to raise external funding if they cannot convince investors about the quality of their projects. Additionally, regardless of whether firms can prove that their projects have positive net present value, external finance might be difficult to obtain due to agency problems. Indeed, if investors cannot monitor firms' actions after the funding is provided, firms might circumvent the intended terms and conditions of the project.

Regulators invested significant effort into providing channels through which firms can reduce asymmetries of information and thus reduce the external cost of finance. However, market reactions to these regulations are very complex and difficult to predict. A critical question that regulators have to decide is whether to force firms to comply with specific information requirements or to allow firms to decide how much information to report voluntarily. Theoretically, there exist reasons why either could be optimal. Empirical testing is complicated by endogeneity issues and a scarcity of clean laboratories for separating the signaling channel from alternatives such as changes in the type or in the amount of information disclosed.

When disclosure is voluntary, there should exist a market solution to adverse selection problems because highly profitable firms might have incentives to disclose. Several theoretical models suggest that firms use voluntary disclosure to signal their type. If the authority forces disclosure, it eliminates this mechanism, and thus it might increase adverse selection problems. As suggested by recent works (e.g., Goldstein and Yang (2018)), more information may not be better if it reduces the informativeness of the currently available information. By shutting down the signaling mechanism, a mandatory provision of public information may decrease -and not increase- the overall quality of information available to investors.

On the other hand, mandating disclosure could have benefits as the authority increases the amount of information available in the market and simultaneously reduces the uncertainty of that disclosure. As a consequence, investors may more easily monitor firms, and the agency cost of external finance might decrease. The consequences of mandating disclosure are, therefore, an empirical question.

The existing empirical research tells us surprisingly little about the contrast between voluntary and mandatory disclosure of the same piece of information. In this paper, I empirically investigate how moving from a voluntary to a mandatory regime affects how financially constrained firms are and if there is any change in their investment policies. I also examine possible mechanisms behind the results and specific theoretical predictions regarding types of firms that are more affected by the shifts in the disclosure regime.

I use the discontinuation of a streamlined version of form 10K that small businesses could file before 2008, as a quasi-natural experiment that transformed information that was voluntarily disclosed by some firms into mandatory disclosure. In 2008 the Securities and Exchange Commission (SEC) eliminated the form 10KSB that allowed companies with less than $25 million in public float to have more lenient mandatory disclosure requirements. Thus, after 2008 the SEC mandated every firm to use the long form 10K. Supporting the economic relevance of this regulation, I find a body of evidence indicating that the impact of being forced to file a 10K versus a 10KSB is economically large. For example, I find that firms switch to more costly and reputable auditors, the document increases in length and the firms receive higher levels of scrutiny from investors.

Furthermore, before 2008, some firms permitted to use 10KSB voluntarily reported regular 10Ks. Those firms were voluntarily disclosing more and better information than what was required. However, that information became mandatory for every firm after 2008. I use this change in SEC rules to conduct a difference-in-difference analysis in which the more informative forms that some firms were voluntarily reporting became mandatory. This regulation guaranteed a permanent use of 10K, but at the same time it shut down the signaling mechanism associated with the voluntary use of that form because both strong and weak type firms had to disclose, thus forcing a pooling equilibrium.

The treated group consists of all firms with a public float below $25 million that did not use the streamlined 10KSB form the year before the rule changed, even though they were allowed to. The critical identifying features of this sample is that neither the amount nor the quality of the information reported changed for those firms. The only thing that changed was the fact that portions of the extra information that they previously disclosed voluntarily became an obligation[1]. Because the disclosure of these treated firms did not change, this experiment provides a unique and rare empirical opportunity to test the effect of the *mandatory* condition of the disclosure isolated from a change in the amount of information disclosed[2]. This ability to separate shocks to signaling from shocks to disclosure quantity is the focus of my study, and is the reason why I do not focus on the firms that switched from 10KSB prior to 10K after. Those firms are treated in two ways: they lost their signaling ability but also they were forced to disclose more information. Theoretical predictions for these two effects go in opposite directions[3].

The control group consists of firms that report 10K before and after the regulation and did not have the option of using 10KSB. Thus, nothing changed for the control group in 2008, neither the information they disclosed nor the conditions -mandatory or voluntary- in which they disclosed it. These firms had to mandatorily file form 10K before and after the rule change without interruption. Figure 1 depicts the identification strategy.

Given the design of the quasi-natural experiment, treated firms are small. Thus comparing them to much larger control firms might not be accurate. For that reason, I restrict the control firms to those with a public float between $25 and $50 million[4]. As an extra caution, I also balance the sample using entropy balancing, following Hainmueller (2012). As Hennessy and Whited (2007), Beck, Demirgüç-Kunt, and Maksimovic (2008) and Hadlock and Pierce (2010) show, small and young firms usually operate in environments with high asymmetry of information, which can increase the difficulty of raising external

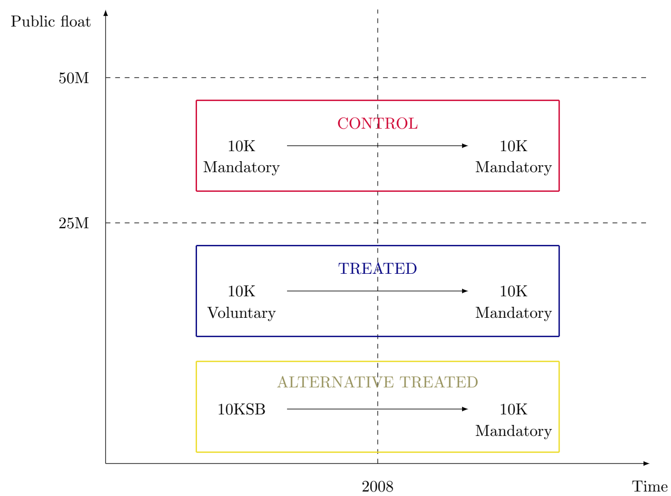
[1]As a validation test of treated firms not disclosing more information, I find that the size of the 10K forms reported by these treated firms did not change after the new rules were implemented, whereas the size of those firms that changed from 10KSB to 10K increased significantly. I find similar results regarding auditing costs and level of scrutiny.

[2]The previous empirical literature of mandatory disclosure (e.g., Leuz and Verrecchia (2000), Daske, Hail, Leuz, and Verdi (2008), and Christensen, Hail, and Leuz (2013)), analyzes the mandatory adoption of reporting regimes that involve an increase in the information reported.

[3]In fact, I do not find significant effects for these firms, which I discuss in sections IV.A and V.E.4.

[4]I restrict it to $50 million to keep symmetry and a similar number of firms in the treated and control groups.

Figure 1: Identification Strategy



financing. They typically engage a higher cost of external financing, and their investment is very sensitive to their ability to procure funding. Therefore, by focusing on small firms, the experiment provides an ideal setting for testing the implications of information disclosure on external financing.

I use Hoberg and Maksimovic (2015) text-based measures of financial constraints as the main variables in my study. These measures distinguish debt from equity constraints and measure constraints directly for each firm. The latter is particularly important considering a main criticism faced by other indexes of financial constraints (see Farre-Mensa and Ljungqvist (2016) and Buehlmaier and Whited (2018)). I also examine debt and equity issuance and different measures of investment.

I find evidence of firms becoming more equity constrained, but less debt constrained when moving from a voluntary disclosing regime to a mandatory regime. Relative to the control group, on average the treated firms increase their equity constraint index by 20% and reduce their debt constraint index by 16% of a standard deviation. I also find that treated firms are 14% more likely to issue debt, with this effect being especially significant for long-term debt. I do not see a significant impact on equity issuance[5]. Regarding investment policies, I find that firms invest more in property, plant, and equipment, but keep expenditure in R&D unchanged. When analyzing different subsamples, I find that changes in long-term debt issuance and changes in investment appear in the same subsamples in which firms become less debt-constrained.

To interpret these results, I consider the primary attributes of each disclosure regime. The extra information disclosed by firms using 10K instead of 10KSB has value by itself -investors know more about the firm- but also acts as a signal: high-quality firms are more likely to disclose extra information (Grossman and Hart (1980) and Grossman (1981)). Under a mandatory regime, all firms must report regular 10K and therefore there is no signaling value. Under a voluntary regime, there exists signaling but the value of the information by itself is less relevant as the voluntary nature of the disclosure indicates that

[5] This lack of significance is not surprising since equity issuances are rare events (as documented in DeAngelo, DeAngelo, and Stulz (2010)).

the firm cannot guarantee disclosure for future periods. Consequently investors' uncertainty increases, leading to higher financial costs (Diamond (1985) and Ben-Porath, Dekel, and Lipman (2017)).

The signaling theory predicts that a voluntary regime offers a stronger device against adverse selection. On the other hand, a mandatory regime might be better to fight moral hazard since it gives investors permanent access to information about firms' actions. Therefore, the empirical results of firms being less equity constrained when voluntarily disclosing information and less debt constrained when mandatorily disclosing suggest that equity-holders might be more concerned about adverse selection, whereas debt-holders might be more concerned about moral hazard.

This interpretation can be explained by the fact that debt-holders are usually more concerned about exercising their control rights when necessary and about preventing managers from taking excessive risks that could lead them to bankruptcy. By guaranteeing future disclosure, firms make it easier for debt-holders to monitor managers' behavior and thus reduce the agency cost of debt (Jensen and Meckling (1976)). Nevertheless, equity-holders might be more concerned about purchasing overvalued claims and thus prefer a setting in which firms can signal their type (Myers and Majluf (1984)).

Investigating further, I find that the effect on equity constraints is only significant in situations where signaling capacity is expected to be more critical. First, the impact on equity constraints is considerably stronger when information asymmetry is likely to be more extreme. Particularly, the effect is 2.5 times larger when the investor private information contained in stock prices -as measured by Chen, Goldstein, and Jiang (2006)- is low. The main idea of this analysis is that when investors have little information about the firm, the signal can more strongly revise investor priors. Second, the effect on equity is also 24% larger for firms located in states with trade secrets protected by the Uniform Trade Secrets Act (UTSA). Following Verrecchia (1983), a lower cost of disclosing proprietary information[6] makes the signal less noisy and thus more effective[7]. Information asymmetry should also be larger for these firms as they are less likely to disclose their innovation due to its higher protection if kept secret. Third, I also find that the effect on equity constraints is 57% higher if firms were facing higher levels of product-market similarity at the moment of the shock. It is plausible to think that the ability to signal is more valuable when competitors look similar.

Alternatively, I find that the effect on debt constraints is stronger in situations where the ability to commit to future disclosures is expected to be more relevant. First, the effects of the mandatory regime are only significant for long-term debt and not for short-term debt. Second, the impact on debt constraints is more than three times stronger in states where trade secrets are not protected by UTSA. A higher cost of disclosure decreases the scenarios where voluntary disclosure is optimal and consequently makes future disclosures less likely. In that sense, the ability to credibly commit to disclose is expected to be more valuable when future disclosure is more costly and thus less credible. Third, as Hoberg and Maksimovic (2015) highlight, the debt-constrained index is closely related to discussions about covenant

[6] Think of a cost due to the disclosure of proprietary information such as trade secrets, methods used in production, innovation processes, business and marketing plans, salary structure, customer lists, contracts, details of its computer systems, etc.

[7] Indeed, if there is a proprietary cost of disclosure, the signal provided by the firm when withholding information becomes noisier, given that traders are not sure if the information was withheld due to "bad news" or due to a cost that was too high. The noise of the signal increases even more if the proprietary cost is higher for high-type firms.

issues. This correlation supports the idea of mandatory disclosure helping firms' financing by making covenants easier to monitor and enforce.

Finally, I also examine what types of firms are more affected by a change in the disclosure regime. Specifically, I find that firms that rely more on equity tend to be more negatively affected when losing the voluntary regime, whereas firms that rely more on debt are more positively affected when moving to a mandatory regime. In particular, and in line with Brown, Fazzari, and Petersen (2009), I find that firms that report the possession of proprietary information and firms with high levels of investment in R&D face more substantial increases in equity constraints. Innovative firms typically do not have large amounts of collateral, and therefore heavily rely on strong signals to convince investors about the quality of their projects.

I run several placebo tests and robustness checks to reduce the risk of selection bias. I restrict the control firms to those that always voluntarily disclosed 10K when they were below $25 million in years previous to 2007, but were above the threshold in 2007. Those are firms that behaved similarly to the treated group when they had the chance to voluntarily disclosed more information but are not treated because they were already above $25 million at the moment of the shock. The results hold. The results also hold if I drop all firms that became too big after the shock or were too big before the shock. This test helps to rule out the idea that the results may be explained by firms growing or shrinking at the moment of the treatment. The results also hold after entropy balancing the control group based on observable characteristics the year before the shock. The entropy balance gives a higher weight to firms in the control group that would have more likely been treated, given their observable characteristics. Importantly, I also provide evidence against the hypothesis that the financial crisis derive the results (see section VI.C for more details).

I make several contributions to the empirical literature on the effects of information disclosure on external finance. By exploiting a quasi-natural experiment that does not alter the information firms provide to investors, I am able to isolate the impact of mandatory disclosure from an increase in information. To the best of my knowledge, this is the first paper showing effects of mandatory disclosure on both financial constraints and investment, contributing to the scarce empirical evidence about real effects of mandatory disclosure (see survey by Leuz and Wysocki (2016)). I also provide evidence of firms using voluntary disclosure as a signaling mechanism. Even though the theoretical literature has largely accepted the latter result, empirical research has made little traction, especially given the severe endogeneity issues present in most settings. Finally, I provide evidence that mandatory disclosure has different effects when the firm relies more on equity or debt financing. This distinction has significant implications for public policies and regulatory decisions.

The remainder of the paper is organized as follows. Section II describes the previous literature and the main characteristics of a voluntary and a mandatory disclosure regime. Section III gives institutional details of the quasi-natural experiment. Section IV explains the methodology, the identification strategy, and describes the data. Section V discusses the main results and extends the analysis to different subsamples. Section VI provides placebo tests, robustness checks and address the main limitations of the experiment. Finally, section VII concludes.

II Literature Review

There exists an extended theoretical literature supporting the idea that the level of information disclosed by firms has significant implications for their ability to raise funding. Jensen and Meckling (1976) show that a lack of information about managers' decisions makes equity and debt more expensive due to agency problems. In their model, debt creates incentives for risk shifting whereas outside equity induces the manager to consume excessive perks because he does not bear the full cost of it. Myers and Majluf (1984) show that asymmetric information between managers and investors makes capital issuance more costly since investors are concerned about a possible "lemons" problem (Akerlof (1978)). Krasker (1986) shows that asymmetric information can lead firms with the best investment opportunities to face financial constraints, forcing them to under-invest. Even though this effect has been difficult to test empirically[8], there exists some evidence supporting these theoretical predictions. Whited (1992) estimates the Euler equation of an optimizing model of investment and presents evidence of debt constrained firms not being able to optimally allocate investment through time. The constraints seem to be driven by informational asymmetries[9]. Similarly, Hoberg and Maksimovic (2015) find that firms that mention the existence of trade secrets and proprietary information in their 10-K are more likely to be equity market-constrained.

Theoretically, by disclosing more information firms can alleviate problems of moral hazard and adverse selection and thus reduce the external cost of finance. Hölmstrom (1979) points out that any measurement that contains incremental information about an agent's hidden actions adds value by making contracts more efficient. In this line, regulators have invested significant effort in providing channels through which firms can reduce asymmetries of information. However, whether firms should be forced to comply with specific information requirements or if regulators should let them voluntarily decide how much information to disclose is still an open question.

Grossman and Hart (1980) and Grossman (1981) show that when information disclosure is costless and verifiable, there is no need to force firms to disclose: full and truthful disclosure always takes place, since withholding information is interpreted by the market as negative information. However, Verrecchia (1983) shows that when there is a cost of disclosing information, the equilibrium is not to always disclose. Managers will make public their private information only when the effect of the information is sufficiently high to overcome the cost associated with its disclosure and thus investors cannot interpret the lack of disclosure as bad news. Similarly, Dye (1985) and Jung and Kwon (1988) show that full disclosure is not an equilibrium if managers' endowment of information is unknown by the market. Jovanovic (1982) considers the case where the firm does not know her type, but instead receives a signal and decides whether to disclose it or not. The main result of the model is that there exists an equilibrium such that only firms with high signals disclose. Song Shin (2003) develops a multi-period model with different projects that can either fail or succeed. He shows that there is a sequential equilibrium in which the

[8] The empirical analysis has been especially challenging due to many difficulties and lengthy debates on how to measure financial constraints and also, because of the scarcity of exogenous changes in information environments.

[9] In her estimations, constrained firms face a wedge between the marginal cost of investment today versus investing tomorrow. This wedge corresponds to the effect of financial factors on the discount rate. The one-period discount rate responds more strongly to the financial variables for a group of firms with higher information asymmetry (firms without bond ratings).

manager only discloses successes. Guttman, Kremer, and Skrzypacz (2014) develop a dynamic model of voluntary disclosure of multiple pieces of private information. Their model shows that managers sometimes voluntarily disclose part of their private information while concealing another part of their information. Overall, regardless the different settings and assumptions, all voluntary disclosure models agree on the existence of a signaling mechanism: under high levels of information asymmetry, some firms might find it optimal to voluntarily disclose more information to separate from other firms that do not disclose.

However, under a voluntary disclosing setting, firms cannot ex-ante credibly commit to a long-term disclosure policy, and that can increase financial costs by increasing investors' uncertainty. There exist different papers that model this effect of information disclosure commitment on the cost of investment. Diamond (1985) suggests that given that under a voluntary setting full disclosure is not guaranteed in every scenario, firms might be better off if they can commit ex-ante to disclose information in both good and bad scenarios. In multi-period debt contract models (like Bolton and Scharfstein (1990) and Inderst and Müller (2003)) investors' need to incentivize the firm to reveal true cash-flows prevents them from full financing firms' investment projects. In those models, financial constraints come from the fact that the manager cannot credibly commit to revealing true cash-flows. Ben-Porath, Dekel, and Lipman (2017) develop a model in which an agent decides the projects to invest on and also how much information to disclose about the outcomes of those projects. They show that the agent would be better off if she has no control over information disclosure. The main idea is that when the equilibrium is not to always disclose (think of a setting like Verrecchia (1983) or Dye (1985)), the manager can (at least to some extent for some time) hide bad outcomes and disclose only good results. As a consequence, the manager may favor riskier projects even with lower expected returns. The reason is that by only disclosing positive outcomes, she might have an incentive to choose projects that are more likely to give her an opportunity to disclose information that makes her look good[10]. Because the market can anticipate this opportunistic behavior and given that the manager cannot credibly commit to not incur in it, the firm ends up being worse off.

Surprisingly, there is little evidence about the effects of mandatory disclosure on firm performance in general and on external finance in particular. Most of the papers trying to answer these questions have focused on international evidence. Leuz and Verrecchia (2000) study German firms that switched from the German to an international reporting regime (IAS or U.S. GAAP), showing that firms committing to increased levels of disclosure (international reporting strategy) are associated with lower bid-ask spreads and higher share turnover. The magnitude of this relation depends on the strength of the mandatory disclosure system. Daske, Hail, Leuz, and Verdi (2008) study the adoption of mandatory International Financial Reporting Standards (IFRS) reporting around the world. They find that on average, market liquidity increases and firms' cost of capital decreases around the time of the introduction of IFRS. Interestingly, capital-market benefits occur only in countries where firms have incentives to be transparent and where legal enforcement is strong. Similarly, Christensen, Hail, and Leuz (2013) report that countries where IFRS became mandatory experienced an increase in liquidity. However, this effect was limited to

[10] One way of interpreting this incentive to "look good" is the manager having career concerns. She might want to disclose information to the market to form beliefs about her abilities (see Chen (2015)). Another interpretation is to consider that the manager might have incentives to manage earnings. Earning management is especially relevant in cases of debt covenants.

five European countries that concurrently made substantive changes in reporting enforcement. In fact, they find similar liquidity effects for firms that experience enforcement changes but do not concurrently switch to IFRS.

All these studies suggest that the effects of mandatory disclosure are more related to enforcement than the information by itself. That evidence is in line with the idea of mandatory disclosure allowing firms to commit to a disclosure policy credibly.

Regarding the effects of voluntary disclosure, the empirical analysis has proved to be very challenging too. Balakrishnan, Billings, Kelly, and Ljungqvist (2014) are probably the first ones providing evidence that firms can affect the liquidity of their shares substantially through voluntary disclosure. Using the closure of 43 brokers between 2000 and 2008, they show that firms that voluntarily disclose more information can reduce the negative effect on liquidity after an analyst coverage loss.

Overall, the effects of information disclosure regimes -voluntary or mandatory- are not clear. Studies tend to agree that more disclosure should improve liquidity and reduce the cost of capital. However, there is no clear evidence if it is convenient to force that disclosure. Most importantly, there is no definite evidence about its effect on the capacity of the firm to raise external capital or to affect real investment decisions. The highly endogenous nature of information disclosure and the lack of reliable natural experiments[11] have made especially difficult to find convincing evidence of any effect. In addition, studies usually focus on the effects of extra information but not on whether the same information was voluntarily or mandatorily disclosed. In this paper, I address most of these issues.

III Quasi-natural Experiment

On February 4th of 2008, the SEC adopted a new set of disclosure rules for smaller companies filing periodic 10K reports and registration statements[12]. The new rules eliminated the ability of "small business issuers" to use streamlined filing requirements. Specifically, the SEC eliminated all "small business" forms (Form 10KSB, Form 10QSB, Form SB1, Form SB2, and Form 10SB), thus mandating the smaller reporting companies to use regular forms (Form 10K, Form 10Q, Form S1).

The regulation SB, particularly the Form 10KSB, was implemented by the SEC as a result of historical efforts not to subject smaller companies to unduly burdensome regulations. 10KSB forms did not only provide reduced disclosure requirements[13], but they also created a different disclosure system for small business[14]. As a consequence, the elimination of the form 10KSB increased the amount of information required, but most importantly it simplified the way that information was disclosed, reducing complexity and forcing small companies to move to a more streamlined system. The SEC expressly pointed out this

[11]Finding the right control group has been especially tricky since regulations usually affect the whole universe of firms.

[12]The SEC proposed the modifications on May 23, 2007. The final rule was adopted on November 15 of that same year and became effective on February 4th of 2008.

[13]In comparison to regular forms, 10KSB forms did not require the following items: segment information and disclosure about foreign operations; a plan of operations for the next 12 months and market risk disclosures; executive compensation related to benefits paid or accrued under pension or retirement plans; stock performance graph; compensation committee report; and interlocking directorship. 10KSB forms also allowed firms to disclose business development activities for only three years instead of five.

[14]For more details about the regulation SB, see Carmichael and Graham (2003).

intention when releasing the new rules:

"Eliminating the 'SB' forms will mitigate any perceived notion that smaller companies are currently reporting under a completely different and inferior disclosure framework."[15]

Firms using 10KSB have smaller reports, receive less attention from the public, spend less money on auditing and have lower quality auditors than firms using regular 10K. Figure 2 presents these differences graphically[16]. Similarly, the empirical evidence presented in section V.A shows that by moving from 10KSB to regular 10K, firms increased the level of disclosure significantly. In addition to that, the quality of the information and the level of scrutiny also increased considerably. The increase in the amount and quality of information is mechanically explained since 10KSB implies less disclosure and less auditing than regular 10K. The increase in scrutiny is probably explained by the fact that regular 10K provides a much more meaningful presentation of information, as practitioners and investors need to focus on only one disclosure framework. Also, analysts can more easily compare reports between small and non-small companies if all of them are using the same system. Uniformity in disclosure format and comparability of reports make regular 10K more attractive for analysts and investors.

The form 10KSB forms was available to firms with less than $25 million in public float. Interestingly, some firms with a public float below $25 million and hence allowed to use 10KSB, reported regular 10K. These firms were voluntarily disclosing more and better information than it was required. However, after 2008 that voluntary disclosure became mandatory. These firms reported the same form before and after the rule change, with the only difference that previous 2008 the disclosure was voluntary and after 2008 it was mandatory.

By design, this experiment focuses on small firms. This attribute makes the experiment especially appropriate to study the effect of information disclosure on the cost of external finance. Indeed, small firms usually have little cash-flows and thus rely heavily on the external market to finance their investment. At the same time, their stocks most often trade as "penny stocks" and, consequently, they do not receive significant coverage from analysts, becoming very opaque to investors. As a consequence, they face important financial constraints. As Hennessy and Whited (2007), Beck, Demirgüç-Kunt, and Maksimovic (2008) and Hadlock and Pierce (2010) show, small firms usually face a higher cost of external financing, and their investment is very sensitive to it. The experiment is also particularly appropriate because the information reported in 10K forms is verifiable -false information is considered fraud- and all theoretical models presented in section II rely on this assumption.

[15] Securities and Exchange Commission, 17 CFR Parts 210, 228 et al. Smaller Reporting Company Regulatory Relief and Simplification; Final Rule.

[16] The graph related to audit fees only consider small firms with a public float below $100 million, since large firms usually face higher costs of auditing. The difference is five times larger if I include all firms.

IV Empirical Analysis

A Methodology

I use the change in the regulation SB as an exogenous shock that changed voluntarily disclosed information into mandatory disclosure, and conduct a difference-in-difference analysis. The treated firms are all firms with a public float below $25 million that did not use 10KSB in 2007 (the year before the rule changed). Those are the firms that disclosed more information than what was required and for whom that extra information became mandatory in 2008.

In the treated group I only consider the small firms that voluntarily reported 10K in 2007 and that did not report 10KSB in years before 2007. In other words, I drop from the treated group any firm that reported 10KSB at some point of its life. By doing this I only focus on firms that were always taking advantage of the voluntary disclosing setting and therefore more clearly identify the change in the disclosing regime. This refinement is important because the SEC announced the new 10K rules in 2007 and some firms might have decided to report 10K that year with the only purpose of adapting to the new rules. I do not include those firms in the treated group.

An essential characteristic of this experiment is the fact that treated firms did not change the amount of information disclosed before and after the rules changed. The only difference for them is that before the reform the disclosure was voluntary and after it was mandatory. Because the information disclosed by these firms DOES NOT CHANGE, this experiment provides a unique and rare empirical opportunity to test the effect of the *mandatory* condition of the disclosure isolated from a change in the amount of information disclosed.

The control group consists of firms that changed neither the disclosing regime nor the amount of information disclosed. In that line, I consider firms that were obligatorily reporting regular 10K before and after 2008. That is, firms above the $25 million of public float threshold. I also exclude from the control group any firm that reported 10KSB at some point of its life. By doing this, I do not consider firms that behave differently than the treated firms when they had the chance to do it.

These two conditions -treated firms only changing voluntary disclosure and control firms not changing anything- allow to focus only on the *disclosing regime* effect isolated from the change in the amount of information. This ability to separate shocks to signaling from shocks to disclosure quantity is the focus of my study, and is the reason why I do not focus on the firms that switched from 10KSB prior to 10K after ("Alternative Treated" in Figure 1). Those firms are treated in two ways: they lost their signaling ability but also they were forced to disclose extra information that they were voluntarily withholding. I can use them to test the treatment effect regarding a shock forcing firms to disclose more than previously, but I cannot use them to test the signaling mechanism that a voluntary disclosure regime provides. I briefly analyze the effect of the regulation on these firms in section V.E.4. However, they are not the objective of this paper.

Figure 1 depicts the identification strategy. The blue box corresponds to the treated group: firms that always used 10K but had the chance to report 10KSB before 2008. The red box corresponds to the control group: firms that always reported 10K as well, but did not have the chance to report the 10KSB before 2008 because they had a public float above \$25 million. Control firms mandatorily disclosed 10K both previous and after 2008, whereas treated firms voluntarily reported 10K previous 2008 and mandatorily after.

The treated firms are small and relatively young, and therefore a comparison using all firms above \$25 million might not be very accurate. To select an adequate control group, I restrict the sample to those very close to the threshold. I use symmetry as the driving criterion and thus restrict the control group to firms with a public float between \$25 and \$50 million. Also, in this setting, the number of control firms is similar to the number of treated firms.

To make the control and treated group even more comparable, I follow Hainmueller (2012) and balance first and second moments of six observable characteristics: the log of sales, the log of firm age, profitability, tangibility, market to book ratio and leverage. Each observation is weighted in order to maximize the probability of being treated. The balancing is based on those observable characteristics in 2007. The purpose of the entropy balancing is to make the treated and the control group as similar as possible in the probability of being treated, regarding mean and variance of size, age, profitability, the ratio of tangible assets, growth opportunities and the capital structure they had just before the shock.

The difference-in-difference analysis will be estimated as follows:

$$\text{DEP VAR}_{it} = \alpha_0 + \beta_1 \text{ TREATEDxPOST2008}_{it} + \delta_1 \text{ CONTROLS}_{it-1} + \mu_i + \gamma_t + \epsilon_{it} \tag{1}$$

where TREATED equals one if the firm is treated and zero if the firm belongs to the control group; POST2008 is a dummy that equals one for years after 2008 (2008 included), and TREATEDxPOST2008 is the interaction between TREATED and POST2008. Firm-level controls lagged in one period are included, as well as firm and year fixed effects. Standard errors are clustered by firm. The coefficient of interest is β_1. I will analyze three types of dependent variables: financial constraints, investment and issuance.

By including firm fixed effects, the results only consider within firm variations and thus control for time-invariant unobservable firm characteristics. At the same time, year fixed effects control for possible shocks that affect all firms in a given year. The inclusion of year fixed effects is particularly important considering the experiment is conducted during the Subprime financial crisis. Section VI.C provides a more detailed discussion of this issue.

Because the Sarbanes-Oxley Act of 2002 made significant changes to disclosure rules, I decide to restrict the pre-treatment period to years after 2003. I restrict the post-treatment period to years before 2012 to maintain symmetry in samples between pre-treatment and post-treatment.

B Data Description

I use Hoberg and Maksimovic (2015) text-based measures of financial constraints (HM) as the main dependent variables[17]. By analyzing discussions in 10K statements in which management summarizes

[17] I thank Jerry Hoberg and Vojislav Maksimovic for posting this dataset on their website.

their firm's inability to obtain financing for planned investments, Hoberg and Maksimovic (2015) construct a broad index of financial constraints[18]. The index is based on cosine similarities to the managers' discussion of firms known to be financially constrained. It has a mean value that is close to zero, and the interpretation is that a higher value indicates a higher level of likely constraints.

There are three important reasons why HM measures are especially convenient for my setting. First, they can distinguish between equity and debt constraints[19]. Second, they are available for the entire COMPUSTAT panel. Finally, they measure constraints directly to each firm, instead of extending measures from other surveys[20]. The latter is especially important considering that the experiment focuses on small firms and they usually behave very different than large firms regarding external financing.

I use the three main constraint variables included in the database:

1. Total constraints: firms with higher values are more similar to a set of firms known to be at risk of delaying their investments due to issues with liquidity.

2. Equity constraints: firms with higher values are more similar to a set of firms that (A) are at risk of delaying their investments due to liquidity issues and (B) that indicate plans to issue equity (presumably to address their liquidity challenges).

3. Debt constraints: analogous to the above, but the firm indicates plans to issue debt (presumably to solve their liquidity problems).

I obtain firm-level data from CRSP/COMPUSTAT. I get the observed choice to file using the small business version of the 10K (10KSB or 10KSB40) or the standard version of the 10K from a textual analysis of 10K[21]. I link each 10K observation to the CRSP/COMPUSTAT database using the central index key (CIK) obtained from the 10K, and the link table provided in the WRDS SEC Analytics package.

I also study debt and equity issuance. For that purpose, I create two dummy variables: *DEBT ISSUANCE* and *EQUITY ISSUANCE*. The former equals one if the firm issues debt an the latter equals one if the firm issues equity. For a deeper analysis, I also create similar variables for long-term and short-term debt. Following the extant literature, and specifically Leary and Roberts (2005), an issuance is defined as having occurred in a given year if the net change in equity (number of common shares) or debt, normalized by the book value of assets at the end of the previous period, is greater than 5%.

To analyze the implications of the results and to better understand the underlying mechanisms, I extend the analysis to investment variables. Particularly I consider capital expenditure, expenditure on property, plants, and equipment and R&D expenses, all of them scaled by total sales. I set R&D to zero when missing[22].

[18]The analysis is based on the Management's Discussion and Analysis (MD&A) section and focus on the mandated disclosures regarding each firm's liquidity.

[19]HM takes advantage of the mandated disclosure of the sources of capital each firm intends to use in addressing its financing needs.

[20]Previous constraint measures rely on potentially unstable reduced-form predictive models estimated on small samples using accounting ratios, which are then applied out of sample to materially different populations of firms.

[21]I thank Jerry Hoberg for sharing this data with me.

[22]I obtained similar results when setting missing R&D to the industry mean, as Koh and Reeb (2015) suggests.

I follow the extant literature and control for a vector of firm characteristics that are traditionally associated with financial constraints and investment: logarithm of total sales, the log of firm age, market to book ratio, profitability, and tangibility. With these control variables, I seek to control for size, age and growth opportunities. Firm age is a listing vintage computed as the first year the firm first appears in the CRSP/COMPUSTAT merged database; profitability is operating income before depreciation (OIBDP) divided by assets; and tangibility is total property, plant, and equipment (PPENT) divided by assets. Tangibility is set to zero when PPENT is missing.

Following existing studies in corporate finance, I exclude financial firms and regulated utilities (SIC 6000-6999 and 4900-4999, respectively) from the sample. I also exclude observations for which the control variables are missing (sales, firm age, market to book ratio, profitability, and tangibility). All variables are winsorized at 1% and 99% level.

C Summary statistics

The treated group corresponds to 140 firms that have public float below $25 million and report standard 10K in 2007. Panel A of Table I reports summary statistics on the treated group in 2007. Just before the reform, treated firms had an average public float of $14 million, total sales of $146 million and were 15 years old, on average. A quarter of them had net losses, while the other 75% had positive profitability. Market to book ratio was close to one for most of them, while it increases up to five for the 90th percentile. Investment in R&D varies a lot among them: half of the treated group invested less than 15% of their sales, while 5% invested more than 30 times their sales of that year. On average, they financed 22% of their assets with debt.

Panel B reports summary statistics on the control group in 2007. It includes all 144 firms with a reported public float between $25 and $50 million the year before the regulatory change. Regarding public float, these firms are the closest to the treated group. As expected, they are slightly larger -on average they have a public float of $37 million- and less profitable. They are of similar age (15 years old on average), have similar levels of leverage and of tangible assets. Panel C reports summary statistics for the same firms, but after weighing them with Hainmueller (2012) entropy balancing. It is worth noticing that by construction, after the balance control firms on average are similar in terms of size, age, profitability, tangibility and market to book ratio. However, they are weighted differently according to the expected likelihood of have been treated if they had had the chance.

Treated firms are distributed in 29 industries according to their 2-digit sic code and 57 industries according to their 3-digit sic code. Even though control firms are not distributed in exactly the same industries, 73% of treated firms share industries with the control group, according to their 2-digit sic codes. This percentage goes up to 97% if we consider the 2-digit sic code +/- 1. The most relevant industries for both treated and control groups are Chemicals & Allied Products (sic 2800), Electronic & Other Electrical Equipment & Components (sic 3600), Measuring, Photographic, Medical, & Optical Goods, & Clocks (sic 3800), and Business Services (sic 7300). Figure 3 displays the distribution of firms for each 4-digit sic code. Each blue triangle represents a treated firm, while each red circle represents a control firm. We can see that treated and control firms are distributed in a similar way across the spectrum of industries[23].

Table II presents the correlation matrix of dependent variables and control variables. Panel A focus on the financial constraints variables, while Panel B on investment. As expected, total constraints are negatively correlated with size, age, profitability, and tangibility, but positively correlated with the market to book ratio. The latter correlation reveals that in this sample, firms with better growth opportunities are more likely to delay projects due to liquidity issues. Also, before the regulation, equity and debt constraints are little but positively correlated, implying that firms were facing both types of constraints at the same time. This correlation is not surprising since the sample consists of small and young firms. Concerning standard firm characteristics, usual correlations are found: larger firms tend to be older and more profitable, but with lower growth rates than smaller firms. Concerning investment, correlations are as usual: smaller and younger firms tend to invest more, while profitable firms tend to invest less.

V Results

A Validity of the Quasi-Natural Experiment

There are two crucial identifying assumptions in this experiment. The first one is that the treated firms were disclosing more information than what was required before the reform. The second one is that treated firms did not change the amount of information disclosed after the reform. To test these assumptions, I run several estimations.

First, I test if forms 10KSB disclose less information than regular 10K. For that purpose, I regress the size of the 10K form[24] on a dummy variable that equals one if the firm reports 10KSB and zero if it reports regular 10K. The results are presented in column (1) of Panel A of Table III and show that the size of the 10K is 24.5% smaller when firms report 10KSB form. In column (2) I apply the same analysis to the size of item 1 instead of the whole 10K and get similar results. Some readers might think that item 1 is more appropriate to measure information disclosure because it is the section of the 10K that describes the business of the company in detail[25].

Then I test if firms that moved from 10KSB to regular 10K disclosed more information. To do that I estimate a regression similar to equation 1, but with a different treated group and using the size of 10K and item 1 as the dependent variable. As opposed to the specification used in the rest of the paper, in this case, treated firms are those firms with a public float below $25 million that reported 10KSB (the "Alternative Treated" of Figure 1). The control group is the same described in the previous section (firms with a public float between $25 and $50 million). Columns (3) and (4) of Panel A report the results of

[23] As a robustness check, I added some industry-level characteristics (the mean of profitability, tangibility, and market to book ratio at the 3-digit SIC code) to the covariates used in the entropy balancing and I re-estimated the main regressions of the paper. All the results hold.

[24] It is measured as the logarithm of the number of paragraphs.

[25] It includes a description of recent events, competition, regulations and labor issues, special operating costs, seasonal factors, or insurance matters, as well as anything that could go wrong, likely external effects, possible future failures to meet obligations, and other risks disclosed to adequately warn investors and potential investors.

this regression. As expected, firms reported forms that are significantly larger when moving from 10KSB to 10K, using the size of both whole 10K and Item 1.

Evidence so far supports the assumption that, by reporting 10K instead of 10KSB, treated firms voluntarily opted to disclose more information. However, the experiment also relies on the fact that the treated firms always disclosed the same amount and type of information. By construction, they reported regular 10K before and after the reform, so the type of information is the same. However, they could have changed the amount of information reported within the 10K. To be sure that this is not the case, I estimate equation 1 using the size of 10K as the dependent variable. The treated and control groups are the ones described in the previous section. That is, firms that reported 10K when having the chance to report 10KSB as the treated group, and firms between $25 and $50 million as the control. The results are presented in columns (5) and (6) and support the assumption. The size of both 10K and Item 1 of firms that moved from a voluntary to a mandatory setting is not significantly different from the size of 10Ks reported by firms that were always under a mandatory setting.

The primary purpose of the regulation was to unify the disclosure system of small firms with the system of all the rest. Therefore, the treatment is not only about the amount of information becoming mandatory but also the type of information. The main consequence of a unified system is that investors more easily analyze information, and thus firms moving to the regular system should receive more attention and scrutiny from the public. Thus I test if firms that were reporting 10KSB before 2008 received more attention from the market when they started to report standard 10K. As a measure of scrutiny, I use the number of EDGAR filings downloaded in a given year[26]. The results are presented in Panel B. Column (1) shows that firms that mandatorily moved from 10KSB to 10K faced a significant increase in the level of scrutiny, increasing their filing download in around 13%. On the other hand, firms that were already reporting regular 10K did not experience a significant change (column (2)).

I also examine whether the elimination of 10KSB implied an increase in auditing cost, measured as the log of audit fees[27]. Panel C presents the results of this analysis. Column (1) shows that the cost of auditing is 48% lower for firms reporting 10KSB than firms reporting 10K. In column (2) we can see that firms that were reporting 10KSB incurred in significantly higher costs of auditing when moving to regular 10K in 2008. On the other hand, firms that were already reporting 10K did not experience any change (column (3)).

I also find that firms that moved from 10KSB to 10K are 11.4% more likely to switch to a more prestigious auditor. To measure this effect, I create a dummy that equals one if the auditor is one of the so-called "Big Four" (Ernst & Young, Deloitte, KPMG or PricewaterhouseCoopers). In contrast, firms that were voluntarily reporting 10K before the regulation did not switch auditors. The results are presented in columns (1) and (2) of Panel D. Columns (4) and (5) show similar results when using the log of a ranking of auditors (lagged in one period) as the dependent variable. This ranking is based on the number of clients of each auditor, with lower ranking meaning more clients. Overall, these results show that regular 10K forces firms to improve the quality of their auditors in comparison to form 10KSB.

[26] I thank Bruce Li for sharing these data with me. The cleaning process of the data is similar to Loughran and McDonald (2017) and Lee, Ma, and Wang (2015). For more details, see Li (2018).

[27] The dataset comes from Audit Analytics.

This effect might be a consequence of firms needing better auditing, but also a response to the increase in scrutiny. In any case, it is evidence of regular 10K increasing not only the amount but also the quality of the information.

There is one more feature of the new regulation that has to be addressed. Under the new rules, companies that have less than $75 million in public float are eligible to use scaled disclosure within the 10K (forms S-K). They still have to disclose regular 10K, so there is no difference in the disclosure system, but they have the chance to provide less detailed descriptions in the items that 10KSB did not have to comply with[28]. Companies that opt for scaled 10K must check the "smaller reporting company" box on the registration statement of the 10K.

To examine the relevance of the scaled reporting possibility, I run several tests and find strong evidence showing that scaled forms do not reduce the impact of form 10KSB regarding the amount and quality of disclosure. In columns (7) and (8) of Panel A of Table III, I regress the size of the 10K and of item 1 on a dummy variable that equals one if the firm reports 10KSB and zero if it reports scaled 10K (S-K). The size of 10KSB is significantly smaller than scaled 10K. Form 10KSB is also significantly smaller in terms of item 1. In columns (9) and (10), the dummy variable equals one if the firm reports scaled 10K (S-K) and zero if it reports regular 10K. Interestingly, there is no significant difference between the sizes of scaled 10K and 10K, neither in terms of the whole form nor of item 1.

In Panel B, column (3) shows that firms using 10KSB have 31% fewer filings downloaded from EDGAR than firms using scaled 10K. On the other hand, there is no difference in downloads between scaled 10K and regular 10K. Like before, in column (5) of Panel B, I apply equation 1 to firms that were disclosing 10KSB before 2008, although in this case I only consider firms that used scaled forms after 2008 (both in the treated and in the control group). The results show that the number of downloads significantly increased when firms moved from 10KSB to scaled 10K.

Also, column (4) of Panel C shows that firms using 10KSB have 40.5% lower costs of auditing than those reporting scaled 10K. Similarly, column (5) of Panel C and columns (3) and (6) of Panel D, show that firms that mandatorily moved from 10KSB to scaled 10K increase the cost of auditing (in 18%) and are more likely to switch to a more prestigious auditor. Finally, in section VI.B.3 I show that the results do not change if I exclude from the experiments firms that were allowed to report scaled 10K after 2008.

All these tests point to the fact that the experiment is based on a highly material shock, as the difference between 10KSB and regular 10K is economically large. This is true in terms of amount of information disclosed, attention to the disclosure and quality of the information. These tests also help to validate the exclusion requirement since the treated group has disclosure that does not change any of the characteristics just mentioned. A sterile environment like this is critical to testing the signaling theory specifically, as we have an experiment where the treated firms are not forced to take any actions,

[28]There are some requirements that scaled 10K cannot avoid. Scaled forms still have to report two years of audited financial statement instead of one under SB, with all the footnotes and explanatory notes. 10KSB has lower requirements related to audited information in case of acquisitions and IPOs. Under S-K, disclosure related to bankruptcy must be provided pertaining to any petitions filed under the Federal bankruptcy laws or any state insolvency laws filed by or against a director or officer of the company. Under 10KSB the information was only disclosed if the person was an executive of the company during the bankruptcy event. Finally, under S-K, the SEC stressed the importance of consistent disclosures that allow period-to-period comparisons. In that line, the rules require the disclosure of any additional information necessary to avoid a misleading disclosure.

but rather the value of the signal is uniquely shocked.

B Financial Constraints

When companies do not have enough internal resources to finance their growth opportunities, they must rely on the external capital market. However, external finance is not always guaranteed. When firms find troubles getting external funding, we say they are financially constrained. External finance can be obtained from two sources: equity and debt. Thus, financially constrained firms can face difficulties raising equity, debt or both. As described in section II, the ability to raise external finance is highly sensitive to the amount of information disclosed by the firm, regardless of the source used. In this section, I show that external finance does not only depend on the amount of information, but also on how that information was disclosed. Specifically, if it was voluntarily or mandatorily disclosed. And I show that this effect is different depending on the source of external funding.

Table IV presents the estimation of equation 1 using HM index of total constraint as the dependent variable. Column (1) displays results without controlling for firm characteristics, column (2) controls for firm characteristics, and column (3) uses the control group after entropy balancing. Column (1) provides evidence of firms becoming more constrained when moving from a voluntary disclosure setting to a mandatory setting. Firms that changed voluntary disclosure to mandatory disclosure, on average increased their financial constraint index by 21% of a standard deviation, in comparison to firms that were always under a mandatory setting. Column (2) shows that this result is robust to including firms observable characteristics and column (3) that it has a similar magnitude, whether the control group is entropy balanced or not.

I then examine the effects on financial constraints separating the two sources of external finance: equity and debt. Specifically, I analyze how HM indexes for each of these constraints change when firms move from a voluntary disclosure regime to a mandatory regime. I also examine if COMPUSTAT measures of equity and debt issuance are affected as well.

Figures 4 and 5 display a graphical representation of these effects. Each figure consists on a time-series graph of the fitted residuals from the regression of the index on the log of total sales, the log of firm age, market to book ratio, profitability, and tangibility, including firm and year fixed effects. The blue line represents the fitted residuals of the treated firms and the red line the fitted residuals of the control firms. The control group corresponds to all firms with a public float between \$25 and \$50 million after entropy balance. The vertical line points the year 2007. In both figures we can see that after controlling for the usual firm characteristics, financial constraints indexes of both groups move together during the pre-treatment period but go in different directions after 2008, creating a gap between them. Specifically, treated firms became more equity constrained but less debt constrained than control firms.

In a more accurate analysis, Table V reports results for equation 1 with specifications similar to the ones presented in Table IV but using equity constraints and debt constraints as the dependent variable. The first three columns report results for equity constraints and the last three columns report results for debt constraints. Interestingly, I find that after losing the voluntary regime, even though firms are more constrained as a whole (see Table IV), they become more equity constrained but less debt-constrained. These opposite effects are robust to the three specifications (both in statistic significance and economic magnitude), with results slightly stronger when using the balanced sample. Indeed, I find that firms increase their equity financial constraints index in something between 18% and 21% of a standard deviation, depending on the specification used. On the other hand, they reduce their debt constraint index in something between 13% and 18% of a standard deviation.

C Issuance

Similar to Table V, Table VI reports results for equity and debt issuance. I use three measures of debt: total debt (columns 4, 5 and 6), long-term debt (columns 7, 8 and 9) and short-term debt (columns 10, 11 and 12). In line with firms becoming less debt constrained, I find that treated firms are 14% more likely to issue debt when moving to a mandatory setting. Interestingly, this effect is explained by increases only in long-term debt. Indeed, after the shock, firms are 12.4% more likely to issue long-term debt, whereas short-term debt is not significantly affected.

On the contrary, I do not find significant effects on equity issuance. This result is not surprising since equity issuances are rare events (as documented in DeAngelo, DeAngelo, and Stulz (2010)).

D Investment

In this section, I analyze if by affecting financial constraints, the change in information disclosure regime also affects firms' level of investment. I consider three measures of investment: total capital expenditure; expenditure on property, plants and equipment, and R&D expenses. Each of them is scaled by total sales. Table VII reports the estimation of equation 1 for each of these measures.

From columns (1), (2) and (3) we can observe that capital expenditure expressed as a percentage of total sales increases in approximately 4% (8.5% of a standard deviation) when the firm moves from a voluntary disclosure regime to a mandatory disclosure regime. This effect goes up to 8% (17% of a standard deviation) when using the entropy balanced control group. I find similar results for investment in property, plants, and equipment (see columns (4), (5) and (6)).

Given that debt constraints decreases and debt issuance increases, it seems that debt is driving the increase in investment. These results also suggest that investment in property, plants, and equipment is more sensitive to debt than to equity. This interpretation is in line with the fact that physical assets can be used as collateral, helping the firm to get a lower interest rate.

Columns (7), (8) and (9) show that investment in R&D does not change[29]. These results are in line with the results on equity issuance. Even though it is difficult to make strong conclusions out of non-significant results, at least this can be interpreted as R&D not being sensitive to debt. More evidence about this point is provided in section V.F.

[29]I find the same results when setting the missing XRD to the industry mean instead of zero, as Koh and Reeb (2015) suggest.

E Interpretation of the Results

To interpret the results, first consider the main attributes of each disclosing regime. Generally, voluntary disclosure models agree on the existence of a signaling mechanism: under high levels of information asymmetry, some firms might find it optimal to voluntarily disclose more information to separate from other firms that do not disclose (Grossman and Hart (1980) and Grossman (1981)). In this particular setting, the signal consists of reporting a more informative 10K. Because of the higher informativeness, investors can get a more accurate estimate of the future value of the firm. Considering that high-value firms are better off if the estimation is closer to the real value than low-type firms, they should be more willing to send the signal. As a consequence, investors can use the voluntary report of 10K to identify likely high-type firms and thus reduce the adverse selection problem inherent to the external financial market.

However, the extra information that is voluntarily disclosed not only has value as a signal, but also has informational value as investors know more about the firm. When the information becomes mandatory, the signaling value disappears (every firm reports the same 10K); however, the value of the information by itself becomes more relevant, since it is guaranteed for future periods. Indeed, a mandatory setting does not permit firms to signal their type but allows them to credibly commit to a long-term disclosure policy. More and better information allows investors to monitor the firm and thus reduce the classic moral hazard problem inherent to financial agreements.

As a consequence, we can expect that after form 10KSB is eliminated the risk of adverse selection increases since it is harder to differentiate “good” from “bad” firms, whereas the risk of moral hazard decreases since it is easier for investors to monitor the firm’s activities. Even though both equity and debt have both agency costs and adverse selection problems, the results described in sections V.B and V.C suggest that debt is more sensitive to agency problems, whereas equity is more sensitive to adverse selection.

Based on Myers and Majluf (1984) and Krasker (1986), it is plausible to expect equity funding to be very sensitive to adverse selection and therefore to the ability of firms to signal their type. A voluntary setting, in which “good” types can disclose more information to distinguish themselves from other firms, strengthens this ability. The result of firms becoming more equity constrained when losing a voluntary disclosure setting is consistent with them losing some signaling ability.

Alternatively, debt contracts usually imply longer relationships and rely more on future disclosures. Jensen and Meckling (1976) describe how debt financing generates risk-shifting incentives for the firm, creating higher financial costs. To attenuate this agency cost of debt, firms look for ways to credibly commit to debt-holders and thus reduce moral hazard problems. Information disclosure commitment is one of those ways, and therefore debt funding -and especially long-term debt- is expected to be more sensitive to the ability to commit. In that sense, the result of firms becoming less debt constrained after moving into a mandatory setting can be interpreted as firms gaining the ability to commit. This interpretation is also supported by the fact that long-term debt completely drives the effect.

In the next sections, I stress test mechanisms by analyzing situations where the signaling ability is expected to be more important and others where commitment to future disclosure seems to be more relevant.

E.1 Private Information in Stock Price

The value of signaling should be higher when investors have little information about the firm. When the informational gap is large, the adverse selection problem is higher, and a signal can be of great value to revise investor priors about the quality of the firm.

One way of measuring how much information investors have about the firm is by looking at stock prices. Market prices reflect both public information and information privately produced by traders (Grossman and Stiglitz (1980), Glosten and Milgrom (1985), and Kyle (1985)). There is a large body of literature that studies the private information that investors have about the firm contained in stock prices (e.g., Chen, Goldstein, and Jiang (2006) and Goldstein and Yang (2018)). Following Chen, Goldstein, and Jiang (2006) I use price nonsynchronicity as a measure of investors' private information in the stock price. Particularly, I measure it as $1-R^2$, where R^2 is the R-square from a regression of firm's daily stock returns in a given year on a constant, the CRSP value-weighted market return, and the return of the three-digit SIC industry portfolio (equally weighted)[30]. The idea behind this measure is that if a firm's stock return is strongly correlated with the market and industry returns, then the firm's stock price is less likely to convey firm-specific information. Low private information in stock prices means that investors gather little information on individual firms, and thus prices reflect less firm-specific information. Therefore, the prediction is that the effect of signaling should be more important when private information in prices is low, that is when investors have little information about the firm.

To test this prediction, I split the sample between firms that had private information in stock prices above and below the median of the total sample in 2007. Panel A of Table VIII shows the estimation of equation 1 for each subsample. I find that the effect on equity constraints is considerably stronger when private information in prices is low. Indeed, the coefficient $\hat{\beta_1}$ is only significant when investors have low private information and, more importantly, it is 2.5 times larger than the coefficient for the whole sample. This result suggests that equity is more sensitive to voluntary disclosure when the signal of voluntary disclosure is expected to have a greater impact on investors beliefs.

E.2 Product Market Similarity

In this section, I analyze how the results change if firms face a higher level of product market similarity. The motivation behind this analysis is the idea that the ability to signal should be more valuable when firms look similar.

To test this idea, I use Hoberg and Phillips (2016) measure of product market similarity (PMS). Based on each firm's unique product market vocabulary, this variable measures how similar are its competitors[31].

[30] I obtain firms' stock price and return information from Center for Research in Security Prices (CRSP).

[31] They calculate a firm-by-firm pairwise similarity scores by parsing the product descriptions from the firm 10Ks and forming word vectors for each firm to compute continuous measures of product similarity for every pair of firms in the whole CRSP/COMPUSTAT sample in each year (a pairwise similarity matrix). This is done using the cosine similarity method, which is applied after basic screens to eliminate common words are applied. For any two firms i and j, they thus have a

According to this variable, I split the sample between firms that had PMS above and below the median of the whole CRSP/COMPUSTAT sample in 2007.

Panel B of Table VIII displays the results of equation 1 for each subsample. I find that after moving from the voluntary to the mandatory regime, firms only become significantly more equity constrained if they were facing a high level of PMS at the moment of the change. This result is consistent with the idea of equity being more sensitive to a voluntary disclosure regime when signaling is more important. At the same time, the effect on debt constraints is similar in both subsamples, implying that the effect of PMS is only affecting financial constraints through equity.

E.3 Cost of Information Disclosure

In this section, I examine how the results change when firms face lower proprietary costs of disclosure. The analysis here relies on the idea that disclosing information might imply a cost to the firm if the information disclosed is related to proprietary information such as trade secrets, methods used in production, innovation processes, business and marketing plans, salary structure, customer lists, contracts, details of its computer systems, etc. An increase in the cost of disclosure makes the signal stronger only if that increase is lower for high-types. However, if high-type firms are more likely to have trade secrets and proprietary information, an increase in cost of disclosure due to the risk of losing them will affect high-types more. As a consequence, the signal becomes noisier since high-types are less willing to disclose and low-type can more easily mimic them (Verrecchia (1983) and Bhattacharya and Ritter (1980))[32]. In that way, a decrease in the proprietary cost of disclosure should increase the value of signaling. Thus, if equity is more sensitive to the signaling ability, I would expect to find stronger results in equity constraints when the proprietary cost of disclosure is lower.

On the contrary, a higher cost of disclosure makes future disclosure less credible, for the simple reason that it decreases the scenarios where voluntary disclosure is optimal. In that sense, I would expect the ability to credibly commit to disclose information to be more valuable when the cost of disclosure is high.

To test this idea, I use differences in the level of the legal protection of trade secret among states as a variation in the cost of disclosure that is exogenous to the firm. The intuition is that the more protected trade secrets are, the lower the risk of making them public and therefore the lower the proprietary cost of disclosing information. To justify the validity of this analysis, it is important to highlight that, as Hoberg and Maksimovic (2015) show, many firms do mention the existence of trade secrets and proprietary information in their 10K, and the description of their business and the discussion of possible risks (provided in item 1) usually include extensive references to them.

I thus compare the effect of moving from a voluntary setting to a mandatory setting on firms in states where trade secrets are strongly protected to states where they are weakly protected. In 1979, the National Conference of Commissioners on Uniform State Laws published and recommended the Uniform

product similarity, which is a real number in the interval [0,1] describing how similar the words used by firms i and j are.

[32] More specifically, Verrecchia (1983) shows that when there is a proprietary cost of disclosing information, the equilibrium is not always to disclose. The main idea is that, given the cost of disclosing, a firm will make public what she observes when the effect of the information is sufficiently high to overcome the cost associated with its disclosure. Therefore, if the information is withheld, traders are unsure whether it was withheld because the value observed was low, or because it was high but not sufficiently high to overcome the cost.

Trade Secrets Act (UTSA). UTSA strengthened the protection of trade secrets by providing a more comprehensive definition of trade secret and specifying procedures and remedies[33], and interestingly it was not implemented by all the states. Thus, I exploit UTSA variation among states to test the effects of different level of cost of disclosing proprietary information on the results discussed so far.

Table IX displays the results of this analysis. In all specifications the control group is entropy balanced[34]. Odd columns report results for firms operating in states with trade secrets protected by the Uniform Trade Secrets Act (UTSA). Even columns report the same estimations but for the states that did not implement UTSA.

Columns (3) and (4) show that firms become considerably less debt constrained when voluntary disclosure turns into mandatory if trade secrets are not protected (absence of UTSA). The magnitude of the effect is more than three times larger in the absence of UTSA. This evidence is in line with the commitment effect being more valuable when the cost of disclosure is higher. In line with firms becoming less debt constrained, I also find that they are more likely to issue debt when trade secrets are not protected. Interestingly this difference in the effect is considerably stronger when we only analyze long-term debt (see column (10)).

In contrast, from columns (1) and (2) we can see that the effect of firms becoming more equity constrained is only statistically significant when trade secrets are protected. These results support the idea that signaling is more valuable when the proprietary cost of disclosure is lower[35].

E.4 Firms that report 10KSB

Althouth the alternative treated group of firms cannot be used to separate signaling from the quantity of information disclosed, it is natural to additionally examine if the elimination of form 10KSB had any impact on firms that were reporting 10KSB. However, any results for this treatment group must be interpreted with care. Regarding predictions, the elimination of the signaling should have a positive effect on financial constraints since these firms can now pool with the firms that voluntarily reported regular 10K. However, that prediction will be combined with those stemming from the fact that these alternative treated firms also have to report more information after the rule change. Because they voluntarily chose not to disclose that extra information, we would expect the newly disclosed information to be negative. For the same reason, even though they gain an ability to commit to future disclosure, it might not be good for a firm to commit to disclose information if that information is expected to be negative.

Table X shows the estimation of equation 1 using the alternative treated firms (i.e., those that reported 10KSB). The control group is the same described in the previous sections (firms with a public float between \$25 and \$50 million). I do not find any significant changes on any type of financial constraints. I conclude

[33] Generally speaking, this act provides a broader definition of aspects like whether a trade secret must be in continuous business use, whether mere acquisition of the secret is misappropriation, the limitation on the time for the owner to take legal action for misappropriation, whether an injunction is limited to eliminating the advantage from misappropriation and the multiple of actual damages available in punitive damages. For more institutional details about UTSA, see Png (2017).

[34] Results are similar when using the non-balanced sample. The reason I prefer the entropy-balanced specification is that I believe it is more accurate since it reduces potential selection bias in the treatment.

[35] A caveat to consider when interpreting these results is that I cannot isolate the effect of a lower signaling power (higher noise) from the effect of a higher cost of disclosure harming the firm and making it less attractive for investors.

that any positive effect that those firms gained due to the elimination of the signaling mechanism is almost fully offset by the increase of information.

F Innovative Firms

Firms with activities that highly rely on innovation usually face higher challenges when dealing with external investors. One reason is that they tend to be more constrained at the moment of disclosing information due to the need to protect trade secrets and proprietary information in general. Another reason is that their future outcomes tend to be more uncertain. To exploit this idea, I examine how the results differ when extending the analysis to firms that are more opaque due to innovation reasons and to firms that invest more in R&D.

To distinguish opaque from non-opaque firms I examine whether firms report the possession of proprietary information and trade secrets in their 10K. Specifically, I use the measure used by Hoberg and Maksimovic (2015) consistent of a binary variable that is one if the firm has at least one paragraph mentioning trade secrets or proprietary information in the form 10K[36]. Under this measure, 49.6% of firms in the sample are opaque in 2007.

To distinguish between high and low innovative firms, I cut the sample between firms that had R&D over sales above and below the median of the whole CRSP/COMPUSTAT sample in 2007.

Table XI presents the results. Panel A reports the estimation of equation 1 for firms with and without proprietary information in 2007, and panel B reports the estimations for firms with high and low R&D (scaled by sales) in 2007. From columns (1) and (2) of panel A, we can note that firms become more equity constrained when they move from a voluntary to a mandatory disclosure mechanism if they have proprietary information. In fact, the effect is only significant for those firms. This can be interpreted as evidence of firms with proprietary information -and hence higher information asymmetry- tending to be more sensitive to signaling mechanisms. If the informational gap is large, the signal very strongly revises investor priors that the firm is a good firm.

Columns (3) and (4) show that the reduction in debt constraints is only relevant for firms without proprietary information. One interpretation of this result is that even though mandatory 10K allows them to commit to future disclosures, those disclosures are less relevant if the firm has much information that cannot be monitored, like proprietary information.

Panel B shows similar results for high and low R&D firms. The increase of equity constraints is larger and more significant for firms with high rates of R&D investment, while the decrease in debt constraints is larger for firms with low rates of R&D investment. This similarity with the results of the proprietary information subsamples is not surprising, as highly innovative firms are more likely to generate trade secrets and proprietary information. These results are also consistent with existent literature suggesting that innovation is primarily financed with equity rather than debt (Brown, Fazzari, and Petersen (2009)). Innovative firms typically do not have large amounts of collateral, and therefore heavily rely on strong signals to convince investors about the quality of their projects.

[36] Again, I thank Gerard Hoberg for sharing these data.

In general, I find that the commitment effect is only relevant for firms that are not very innovative and have low asymmetric information. On the other hand, the signaling mechanism seems to be more relevant for highly innovative and secretive firms. These findings suggest that regarding their ability to raise external finance, a change in the disclosure regime from voluntary to mandatory would benefit low innovative firms but it would harm highly innovative firms.

VI Placebo Tests, Robustness Checks, and Limitations

A Placebo Tests

I implement two types of placebo test. First, I analyze how results are affected when changing the year of treatment. Second, I modify the treated group, applying the same analysis to firms that were not treated by the regulatory change. If the identification strategy is valid, we should not find significant results under either of these new settings.

Table XII presents results from placebo regressions in which I repeat the Diff-in-Diff analysis but changing the year in which treatment starts. As in the principal analysis, I use four years of pre-treatment and four years of post-treatment. For these estimations, the same specification is used, as well as the same control group (firms with a public float between \$25 and \$50 million after entropy balance), and I control for the same firm characteristics. The effects on equity constraints are only significant when the initial year of the treatment is the actual year in which the new rules were implemented, 2008. In the case of debt, some effects are seen in 2007. One possible explanation of this result is that because the new rules were announced in 2007, investors knew that information was going to be mandatory and therefore firms became able to commit future disclosures already in 2007 credibly. Equity does not experience this anticipation-effect because firms could still voluntarily disclose information in 2007. Debt also presents significant effects if the treatment starts in 2009. One explanation for these delayed effects can be just that there is a slow adjustment, mainly because debt contracts imply long-term relationships and therefore effects on debt might last longer.

I also run placebo tests in the control group. First, I randomly assign half of the control group in 2007 as treated and the other half as a control. Then I repeat the same analysis: balance the sample between treated and control using entropy balancing and estimate the same diff-in-diff. Panel A of Table XIII reports the results of this placebo test. As expected, there is no significant change in any type of constraints.

I run one more placebo test. I use the control group as the treated group and compare it to a new control group defined as all firms with a public float between \$50 and \$75 million. This new setting is similar to the original estimation, although moving the public float threshold such that the originally treated firms are removed from the sample and replaced by the original control group. The results are reported in Panel B of Table XIII and are not significant, discarding the presence of the original results in the control group.

B Robustness Checks and Limitations

B.1 Control firms that voluntarily reported regular 10K before 2007

In the identification strategy, the main condition to be treated is to report 10K instead of 10KSB. Indeed, the treated group only considers firms below the $25 million threshold that voluntarily decided to report more information. The control group also only includes firms that always used regular 10K and thus it is very similar to the treated group. However, there are some firms that never had the chance to use 10KSB and thus there is room for selection bias in the estimations. It might be the case the effects that I find are explained by comparing firms that are essentially different. The difference-in-difference estimation partly accounts for this as it controls for the firm-specific fixed effect in the difference. The entropy balance also helps to alleviate this problem by giving a higher weight to firms in the control group that would have been more likely treated given their observable characteristics. Therefore, the concern is limited to time-varying unobserved characteristics after matching on observed characteristics.

In this section I try to mitigate the concerns about this selection bias even more. For that purpose, I re-estimate equation 1 but only including in the control group firms above $25 million in 2007 that voluntarily reported regular 10K when they were below $25 million. Those are firms that behaved similarly to the treated firms when they had the chance to voluntarily disclose more information but were not treated because at the moment of the shock they were already above $25 million. The results are presented in Table XIV. Panel A (Panel B) presents the results before (after) entropy balancing. To keep a similar number of observations in each group, I expand the size of control firms up to $160 millions. Also, because the difference in size was too big, in order to allow the entropy balance method to converge, I only balanced the mean and not the variance.

As evidence supporting the identification strategy, I find that the results hold. In fact, they have the same magnitude and statistic significance. The estimations are a bit noisier when using entropy balancing, but this is not surprising given that the balancing is less accurate because it does not consider the second moment.

B.2 Firms that became too big after the shock

The second condition to be treated is the size of the firm. Thus, another concern about the identification strategy is that some firms might have been small enough to be treated or to be in the control group at the moment of the shock only because of "bad timing": the experiment happened to occur just when they were starting to grow. They were in the right moment at the right time, but actually, they might be essentially different from the rest of the sample. The same argument can apply the other way around: some firms might had been shrinking at the moment of the shock.

To test if this is a valid concern, I drop from the sample any firm that had a public float of more than $500 million[37] at any time during the sample period with public float available[38]. The results of this analysis are presented in Table XV and are similar to the ones obtained with the baseline specification,

[37] $500 million is the smallest threshold that keeps the sample size of control and treated groups balanced.

[38] Between 1997 and 2013

both in magnitude and significance. Thus, we should not be concern about the *growing and shrinking* hypothesis.

B.3 Excluding firms between 25 and 75 million

Considering that under the new rules, companies that have less than $75 million in public float were eligible to use scaled disclosure within the regular 10K, in this section, I test if the results are affected by this possibility. In section V.A, I presented evidence showing that scaled 10K do not replace SB forms in terms of informativeness, auditing and scrutiny. However, after the reform, firms in the control group gained the possibility of reporting scaled 10K and this option might be affecting the results through the control group instead of the treated group. To be sure that this effect on the control group is not driving the results, I run the same analysis, but using firms between $75 and $110 million as the control group. Those firms do not have the possibility of scaled 10K, and therefore this specification takes away any possibility of the control group being treated. I use $110 million as the upper bound only to get the same number of firms of the treated group.

Table XVI presents the results of this new estimation. The same specification and entropy balance are applied. Column (1) shows the results for total constraints, column (2) for equity constraints and column (3) for debt constraints. As we can observe, the results are the same than those obtained using the original control group of firms between $25 and $50 million. These results, combined with the results provided in Table III, helps to rule out concerns about the effects being driven by changes in the control group.

C Sub-prime Crisis

Having the treatment year be at the same time as the Sub-prime financial crisis may raise some valid concerns about the experiment. The inclusion of year fixed effects helps to control for macroeconomic conditions that equally affected all firms in the experiment. However, if the crisis affected the control and the treated group differently, year fixed effects would not be a sufficient control. This possibility cannot be completely ruled out, although there are some reasons to believe that a differentiated effect of the crisis on treated and control groups should not be a big concern.

First, even though treated firms are smaller than control firms, all of them are quite small. In fact, they correspond to the 15% percentile of public float distribution. It is reasonable to believe that both groups were affected similarly. However, if there were a differentiated effect, I would expect the treated group to be more affected by the sub-prime crisis, given that they are smaller. Yet I find the opposite: treated firms increased investment and became less debt-constrained.

Besides their size, the only distinguishing characteristic between treated and control is that the treated group excludes firms that voluntarily disclosed less information, whereas the control group does not exclude that type of firms. Maybe the financial crisis is more strongly affecting the control group through those firms. However, the results presented in Table XIV and described in section VI.B.1 show that this is not the case. In fact, the results do not change if I only include in the control group firms that voluntarily disclosed more information when they had the chance before the treatment.

Finally, other empirical studies suggest that the Sub-prime crisis mainly affected the market for short-term debt (see Duchin, Ozbas, and Sensoy (2010)). In opposition to that evidence, I do not find any significant effect on short-term debt. In fact, the main effects I find are related to long-term debt.

In this way, the results of the experiment are unlikely to be reflecting the effects of the crisis. However, I cannot the possibility that the financial crisis made the effects of mandatory disclosure stronger and thus easier to identify with the experiment. However, this possibility does not affect the validity of the results, but it implies the need to be cautious when extending them to other periods of time.

VII Conclusion

I studied a quasi-natural experiment that changed voluntarily disclosed information into mandatory disclosure, and analyzed how this change affected firms' financial constraints. I found that firms became more equity-constrained but less debt-constrained. Furthermore, I found that the effect on equity constraints was stronger when the ability to signal is more relevant. Specifically, the increase in equity constraints was larger when investors had little information about the firm and when the firm has proprietary information. This effect was also larger in states where trade secrets were protected and thus the cost of disclosing proprietary information was lower.

On the other hand, the effect on debt constraints was stronger in situations where the ability to guaranteed future disclosures was more relevant. Further supporting this conclusion, the effect was mainly explained by the issuance of long-term debt rather than short-term debt. Also, firms became less debt constrained when their trade secrets were not protected and when they used the debt to finance investment in physical capital.

These findings, taking together, provide evidence supporting the hypothesis that a voluntary disclosing regime creates a signaling mechanism for firms, whereas a mandatory regime provides a mechanisms to credibly commit to investors to keep disclosing information in the future. A plausible interpretation of these results is that equity holders are more sensitive to adverse selection problems, whereas debt-holders care primarily about moral hazard issues.

The results also suggest that firms that rely on the equity market -which tend to be innovative and opaque firms- and firms that face intense product market competition should benefit more from a system in where official information is voluntarily disclosed. On the other hand, firms that rely more on the debt market should prefer a setting in which information disclosure is mandatory.

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Table I: Summary Statistics

This table reports summary statistics for the treated and control firms in 2007, the year before the regulatory change. Financial firms and utilities are excluded. Financial constraints correspond to the text-based financial constraint data from Hoberg and Maksimovic (2015). Capital expenditure, expenditure in property, plant, and equipment, R&D expenditure, sales, firm age, market to book ratio, profitability and tangibility are financial indicators from Compustat and CRSP. Firm age is computed using the first effective date of the current link. R&D was set to zero when missing. Profitability is the operating income before depreciation (oibdp) over assets, and tangibility is total property, plant and equipment (ppent) over assets. Public float was obtained from each firm's 10K form using textual analysis. All non-binary variables are yearly winsorized at the 1-99% level.

Variable	Obs	Mean	Std. Dev.	Min	Max	P50
Panel A: treated firms						
Log Sales	140	3.594	1.777	-3.124	8.475	3.833
Log Firm age	140	2.411	.998	0	3.829	2.565
Profitability	140	-.045	.26	-1.015	.446	.018
Tangibility	140	.183	.221	0	.911	.085
Market to book ratio	140	1.088	.815	.145	5.374	.885
Public float	140	14.077	6.56	.508	24.948	13.465
Delay due to constraints	114	-.035	.092	-.202	.209	-.041
Delay due to equity constraints	114	-.034	.101	-.248	.23	-.044
Delay due to debt constraints	114	.001	.057	-.102	.151	-.008
Capital expenditure/sales	140	.101	.47	0	3.847	.013
Capital Expend Property, Plant and Equipment/sales	140	.105	.502	0	4.132	.013
R&D expense/sales	140	.47	2.389	0	16.307	0
Total debt / total assets	140	.229	.253	0	.973	.14
Common shares	140	13.425	18.462	.527	151.229	7.152
Panel B: control group before entropy balance						
Log Sales	144	3.945	1.629	-2.386	6.936	4.206
Log Firm age	144	2.334	1.012	0	3.761	2.485
Profitability	144	-.078	.318	-1.015	.423	.044
Tangibility	144	.179	.207	.001	.911	.101
Market to book ratio	144	1.213	.934	.145	5.966	.966
Public float	144	37.125	7.554	23.6	49.744	37.096
Delay due to constraints	134	-.013	.096	-.184	.274	-.021
Delay due to equity constraints	134	-.014	.102	-.188	.289	-.022
Delay due to debt constraints	134	.007	.053	-.11	.118	.011
Capital expenditure/sales	144	.094	.415	0	3.847	.018
Capital Expend Property, Plant and Equipment/sales	144	.096	.433	0	4.132	.018
R&D expense/sales	144	.882	3.16	0	16.307	.01
Total debt / total assets	144	.182	.21	0	.973	.117
Common shares	144	16.992	17.443	1.519	145.645	12.335
Panel C: control group after entropy balance						
Log Sales	144	3.592	1.799	-2.386	6.936	4.017
Log Firm age	144	2.432	1.011	0	3.761	2.565
Profitability	144	-.05	.266	-1.015	.423	.051
Tangibility	144	.196	.228	.001	.911	.103
Market to book ratio	144	1.104	.824	.145	5.966	.864
Public float	144	36.388	7.764	23.6	49.744	36.193
Delay due to constraints	134	-.019	.092	-.184	.274	-.022
Delay due to equity constraints	134	-.021	.097	-.188	.289	-.022
Delay due to debt constraints	134	.007	.053	-.11	.118	.008
Capital expenditure/sales	144	.209	.671	0	3.847	.02
Capital Expend Property, Plant and Equipment/sales	144	.214	.702	0	4.132	.02
R&D expense/sales	144	1.299	3.976	0	16.307	.011
Total debt / total assets	144	.22	.246	0	.973	.17
Common shares	144	16.867	19.896	1.519	145.645	8.795

Table II: Correlations

This table displays the correlation matrix of dependent variables and control variables (sales, firm age, market to book ratio, profitability, and tangibility) in 2007. Panel A includes the text-based index of financial constraints and panel B includes investment variables.

Panel A: Financial Constraints

Variables	Total constraints	Equity constraints	Debt constraints	Log Sales	Log Firm age	Profitability	Tangibility	Market to book	Public float
Total constraints	1.000								
Equity constraints	0.937	1.000							
Debt constraints	0.139	0.016	1.000						
Log Sales	-0.200	-0.327	0.336	1.000					
Log Firm age	-0.300	-0.336	-0.054	0.226	1.000				
Profitability	-0.334	-0.441	0.249	0.696	0.239	1.000			
Tangibility	-0.107	-0.147	0.123	0.106	0.100	0.166	1.000		
Market to book ratio	0.158	0.236	-0.094	-0.223	-0.140	-0.131	-0.164	1.000	
Public float	0.098	0.078	0.065	0.131	0.015	-0.025	-0.032	0.095	1.000

Panel B: Investment

Variables	CAPX/sales	CAPXV/sales	R&D/sales	Log Sales	Log Firm age	Profitability	Tangibility	Market to book	Public float
Capital expenditure/sales	1.000								
Prop, plant, equip/sales	1.000	1.000							
R&D/sales	0.656	0.655	1.000						
Log Sales	-0.459	-0.453	-0.644	1.000					
Log Firm age	-0.124	-0.123	-0.072	0.226	1.000				
Profitability	-0.268	-0.263	-0.481	0.696	0.239	1.000			
Tangibility	0.207	0.207	-0.139	0.106	0.100	0.166	1.000		
Market to book	0.037	0.039	0.192	-0.223	-0.140	-0.131	-0.164	1.000	
Public float	-0.024	-0.025	-0.001	0.131	0.015	-0.025	-0.032	0.095	1.000

Table III: Differences between SB forms, regular K and scaled regular K (S-K)

This table displays the results of different estimations using different dependent variables. In Panel A, the dependent variable is the size of either 10K or Item 1 (measured as the log of the number of paragraphs). Columns (1) and (2) report the results of regressing size of 10K (Item 1) on a dummy variable equal to one if the firm reports 10KSB and zero if it reports regular 10K. Column (7) and (8) are similar to (1) and (2), but the dummy variable equals zero if the firm reports scaled 10K (S-K). In columns (9) and (10), the dummy variable equals one if the firm reports scaled 10K (S-K) and zero if it reports regular 10K. Columns (3) and (4) report results of equation 1 using firms that moved from 10KSB to standard 10K as the treated group and those with a public float between $25 and $50 million as the control group. Columns (5) and (6) report results of equation 1 using firms below $25 million that always disclosed standard 10K.

Panel B uses the log of the number of forms downloaded from SEC webpage in a given year. Columns (1) and (5) report results of equation 1 using firms that moved from 10KSB to standard 10K as the treated group and those with a public float between $25 and $50 million as the control group. In column (5) only firms reporting scaled 10K after 2008 were included. Column (2) reports results of equation 1 using firms below $25 million that always disclosed standard 10K. Column (3) reports the results of regressing number of downloads on a dummy variable equal to one if the firm reports 10KSB and zero if it reports scaled 10K (S-K). In column (4) the dummy variable equals one if the firm reports scaled 10K (S-K) and zero if it reports regular 10K.

Panel C uses the log of audit fees as the dependent variable. Column (1) reports the results of regressing log of audit fees on a dummy variable equal to one if the firm reports 10KSB and zero if it reports regular 10K. Columns (2) and (5) report results of equation 1 using firms that moved from 10KSB to standard 10K as the treated group and those with a public float between $25 and $50 million as the control group. In column (5) only firms reporting scaled 10K after 2008 were included. Column (3) reports results of equation 1 using firms below $25 million that always disclosed standard 10K. Column (4) similar to (1), but the dummy variable equals zero if the firm reports scaled 10K (S-K).

In Panel D, columns (1), (2) and (3) use the likelihood of switching to a "Big Four" auditor as the dependent variable. Columns (4), (5) and (6) use the log of a ranking of auditors (lagged in one period) as dependent variable. The ranking is based on the number of clients of each auditor, with lower ranking meaning more clients. Columns (1), (3), (4) and (6) report results of equation 1 using firms that moved from 10KSB to standard 10K as the treated group and those with a public float between $25 and $50 million as the control group. In columns (3) and (6) only firms reporting scaled 10K after 2008 were included. Columns (2) and (5) report results of equation 1 using firms below $25 million that always disclosed standard 10K.

Panel A: Size of 10K

VARIABLES	(1) Size 10-K	(2) Size Item 1	(3) Size 10-K	(4) Size Item 1	(5) Size 10-K	(6) Size Item 1	(7) Size 10-K	(8) Size Item 1	(9) Size 10-K	(10) Size Item 1
1 if SB, 0 if K	-0.280***	-0.419***								
	(0.000)	(0.000)								
post2008xAlternativeTreated			0.156**	0.243*						
			(0.035)	(0.052)						
post2008xTreated					0.074	0.034				
					(0.145)	(0.665)				
1 if SB, 0 if S-K							-0.669***	-0.283***		
							(0.000)	(0.002)		
1 if S-K, 0 if K									-0.055	0.081
									(0.167)	(0.338)
Constant	5.745***	4.054***	5.804***	4.030***	5.881***	4.041***	5.977***	3.866***	6.299***	4.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	8,616	8,616	1,001	1,001	1,201	1,201	4,225	4,225	6,222	6,222
R-squared	0.109	0.014	0.031	0.027	0.034	0.027	0.157	0.017	0.007	0.011
Number of gvkey	2,844	2,844	174	174	175	175	1,338	1,338	2,066	2,066
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Panel B: Scrutiny

VARIABLES	(1) Downloads	(2) Downloads	(3) Downloads	(4) Downloads	(5) Downloads (S-K)
post2008xAlternativeTreated	0.123**				0.129**
	(0.027)				(0.046)
post2008xTreated		0.093			
		(0.131)			
1 if SB, 0 if S-K			-0.366**		
			(0.024)		
1 if S-K, 0 if K				-0.019	
				(0.757)	
Constant	5.833***	5.885***	5.997***	7.323***	5.661***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2,182	1,839	1,906	6,250	1,388
R-squared	0.665	0.667	0.680	0.625	0.696
Number of gvkey	324	271	552	2,068	174
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Panel C: Audit cost

VARIABLES	(1) Audit Fees	(2) Audit Fees	(3) Audit Fees	(4) Audit Fees	(5) Audit Fees (S-K)
1 if SB, 0 if K	-0.724***				
	(0.000)				
post2008xAlternativeTreated		0.198***			0.169***
		(0.000)			(0.002)
post2008xTreated			0.062		
			(0.242)		
1 if SB, 0 if S-K				-0.519***	
				(0.001)	
Constant	12.828***	11.884***	12.222***	11.524***	11.826***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	6,961	1,956	1,627	2,581	1,238
R-squared	0.345	0.126	0.076	0.376	0.171
Number of gvkey	2,292	323	269	761	174
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Panel D: Likelihood of switching to a more prestigious auditor

VARIABLES	(1) Switch to B4	(2) Switch to B4	(3) Switch to B4 (S-K)	(4) Ranking	(5) Ranking	(6) Ranking (S-K)
post2008xAlternativeTreated	0.114***		0.120***	-0.294**		-0.311*
	(0.001)		(0.003)	(0.048)		(0.065)
post2008xTreated		0.029			0.127	
		(0.526)			(0.469)	
Constant	0.405***	0.532***	0.412***	2.140***	1.472***	1.993***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,961	1,216	1,238	1,857	1,567	1,212
R-squared	0.161	0.197	0.159	0.140	0.195	0.149
Number of gvkey	324	177	174	318	264	174
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Table IV: Effects of mandatory disclosure on total constraints

This table displays the results of OLS panel regression of equation 1 when the dependent variable is total constraints. Column (1) displays results without including firm characteristics as control variables and using all firms with a public float between $25 and $50 million as the control group. Column (2) is similar to (1) but controlling for the following firm characteristics: size, age, market to book ratio, profitability, and tangibility. Sales and age are in logarithms. The size of the 10K is also included in the control variables, as the log of the number of paragraphs in the 10K. Column (3) is similar to (2) but using the entropy balanced control group. In all regressions, firm and year fixed effects are included and standard errors are clustered by firm. All variables are winsorized at the 1-99% level and non-binary right-hand variables are standardized. P-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

VARIABLES	(1)	(2)	(3)
post2008xTreated	0.021**	0.023**	0.021**
	(0.024)	(0.014)	(0.035)
Log Sales in t-1		0.006	0.014
		(0.427)	(0.110)
Log Firm age in t-1		-0.018*	-0.027***
		(0.057)	(0.001)
Profitability in t-1		-0.008**	-0.007**
		(0.017)	(0.047)
Tangibility in t-1		0.012	0.011
		(0.422)	(0.443)
Market to book ratio in t-1		0.002	-0.001
		(0.472)	(0.727)
Size of 10-K		0.004	0.003
		(0.156)	(0.402)
Constant	-0.025***	-0.036***	-0.023
	(0.000)	(0.006)	(0.188)
Observations	1,650	1,586	1,555
R-squared	0.027	0.045	0.055
Number of gvkey	266	262	251
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Entropy Balancing	NO	NO	YES

Table V: Effects of mandatory disclosure on equity and debt constraints

This table displays the results of OLS panel regression of equation 1 when the dependent variables are equity constraints and debt constraints. Columns (1), (2) and (3) displays results for equity constraints, while columns (4), (5) and (6) for debt constraints. Columns (1) and (4) don't include firm characteristics as control variables and use all firms with a public float between $25 and $50 million as the control group. Column (2) and (5) are similar to (1) and (4) but controlling for firm characteristics such as size, age, market to book ratio, profitability, and tangibility. Sales and age are in logarithms. The size of the 10K is also included in the control variables, as the log of the number of paragraphs in the 10K. Column (3) and (6) are similar to (2) and (5) but using the entropy balanced control group. In all regressions, firm and year fixed effects are included and standard errors are clustered by firm. All variables are winsorized at the 1-99% level and non-binary right-hand variables are standardized. p-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

	Equity Constraints			Debt Constraints		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
post2008xTreated	0.019**	0.021**	0.018*	-0.013**	-0.016**	-0.018**
	(0.037)	(0.023)	(0.065)	(0.050)	(0.022)	(0.017)
Log Sales in t-1		0.005	0.015*		0.001	0.006
		(0.604)	(0.060)		(0.771)	(0.348)
Log Firm age in t-1		-0.013	-0.024***		0.004	0.004
		(0.245)	(0.003)		(0.416)	(0.670)
Profitability in t-1		-0.007**	-0.009**		-0.000	0.001
		(0.035)	(0.020)		(0.888)	(0.670)
Tangibility in t-1		0.008	0.008		0.012**	0.012**
		(0.504)	(0.546)		(0.015)	(0.025)
Market to book ratio in t-1		0.003	0.000		0.008***	0.005*
		(0.279)	(0.992)		(0.001)	(0.070)
Size of 10-K		0.004	0.004		0.000	-0.001
		(0.129)	(0.194)		(0.851)	(0.760)
Constant	-0.025***	-0.037***	-0.025*	-0.000	0.005	0.003
	(0.000)	(0.005)	(0.081)	(0.970)	(0.589)	(0.821)
Observations	1,650	1,586	1,555	1,650	1,586	1,555
R-squared	0.028	0.042	0.055	0.036	0.054	0.067
Number of gvkey	266	262	251	266	262	251
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Entropy Balancing	NO	NO	YES	NO	NO	YES

Table VI: Effects of mandatory disclosure on debt and equity issuance

This table displays the results of OLS panel regression of equation 1 when the dependent variable are different measures of issuance. Columns (1), (2) and (3) displays results for a dummy variable equals one if the number of shares scaled by total assets increased by more than 5% respect to the previous year. Columns (4), (5) and (6) for a dummy variable equals one if total debt scaled by total assets increased by more than 5% respect to the previous year. Columns (7), (8) and (9) for a dummy variable equals one if long-term debt scaled by total assets increased by more than 5% respect to the previous year. Columns (10), (11) and (12) for a dummy variable equals one if short-term debt scaled by total assets increased by more than 5% respect to the previous year. Columns (1), (4), (7) and (10) don't include firm characteristics as control variables and use all firms with a public float between $25 and $50 million as the control group. Column (2), (5), (8) and (11) are similar but controlling for firm characteristics such as size, age, market to book ratio, profitability, and tangibility. Sales and age are in logarithms. The size of the 10K is also included in the control variables, as the log of the number of paragraphs in the 10K. Column (3), (6), (9) and (12) are similar to (2), (5), (8) and (11) but using the entropy balanced control group. In all regressions, firm and year fixed effects are included and standard errors are clustered by firm. All variables are winsorized at the 1-99% level and non-binary right-hand variables are standardized. P-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

	Equity issuance			Total debt issuance			Long-term debt issuance			Short-term debt issuance		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
post2008xTreated	0.025	0.022	0.042	0.130***	0.130***	0.140***	0.113***	0.117***	0.121**	0.030	0.033	0.027
	(0.436)	(0.457)	(0.180)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.011)	(0.444)	(0.404)	(0.580)
Log Sales in t-1		-0.061	-0.017		-0.095*	-0.137***		-0.075	-0.105**		-0.088**	-0.102**
		(0.214)	(0.731)		(0.093)	(0.008)		(0.140)	(0.018)		(0.032)	(0.018)
Log Firm age in t-1		0.033	-0.003		0.014	0.020		-0.014	-0.053		0.002	0.018
		(0.277)	(0.950)		(0.742)	(0.688)		(0.695)	(0.291)		(0.956)	(0.675)
Profitability in t-1		-0.046**	-0.055**		-0.016	0.000		0.004	0.020		-0.022	-0.009
		(0.014)	(0.022)		(0.432)	(0.983)		(0.843)	(0.350)		(0.205)	(0.578)
Tangibility in t-1		0.038	0.055**		0.057	0.057		0.123**	0.124**		0.044	0.056*
		(0.136)	(0.024)		(0.354)	(0.356)		(0.012)	(0.021)		(0.116)	(0.077)
Market to book ratio in t-1		0.078**	0.073**		0.014	0.013		-0.002	-0.012		0.020	0.023
		(0.011)	(0.045)		(0.534)	(0.581)		(0.923)	(0.554)		(0.291)	(0.213)
Constant	0.094***	0.081*	0.105**	0.133***	0.074	0.029	0.100***	0.061	0.024	0.094***	0.040	0.028
	(0.000)	(0.079)	(0.019)	(0.000)	(0.107)	(0.543)	(0.000)	(0.104)	(0.491)	(0.000)	(0.260)	(0.495)
Observations	1,769	1,767	1,738	1,769	1,767	1,738	1,769	1,767	1,738	1,771	1,769	1,740
R-squared	0.018	0.054	0.054	0.016	0.024	0.026	0.014	0.027	0.031	0.005	0.015	0.017
Number of gvkey	265	265	256	265	265	256	265	265	256	265	265	256
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Entropy Balancing	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES

Table VII: Effects of mandatory disclosure on investment

This table displays the results of OLS panel regression of equation 1 when the dependent variable are different measures of investment. Columns (1), (2) and (3) displays results for capital expenditure over sales, columns (4), (5) and (6) for expenditure on property plant and equipment over sales, and columns (7), (8) and (9) for R&D expenses over sales. Columns (1), (4) and (7) don't include firm characteristics as control variables and use all firms with a public float between $25 and $50 million as the control group. Column (2), (5) and (8) are similar to (1), (4) and (7) but controlling for firm characteristics such as size, age, market to book ratio, profitability, and tangibility. Sales and age are in logarithms. The size of the 10K is also included in the control variables, as the log of the number of paragraphs in the 10K. Column (3), (6) and (9) are similar to (2), (5) and (8) but using the entropy balanced control group. In all regressions, firm and year fixed effects are included and standard errors are clustered by firm. All variables are winsorized at the 1-99% level and non-binary right-hand variables are standardized. p-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

	Capital expenditure			Property, plant & equipment			R&D expenses		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
post2008xTreated	0.044*	0.039*	0.077*	0.047*	0.042**	0.086*	0.065	0.063	0.146
	(0.059)	(0.052)	(0.053)	(0.058)	(0.050)	(0.055)	(0.653)	(0.676)	(0.659)
Log Sales in t-1		-0.031	-0.094		-0.030	-0.103		-0.167	-0.777
		(0.512)	(0.282)		(0.542)	(0.276)		(0.621)	(0.194)
Log Firm age in t-1		-0.008	-0.102		-0.012	-0.128		-0.267	-0.514
		(0.821)	(0.352)		(0.763)	(0.346)		(0.138)	(0.125)
Profitability in t-1		0.013	0.030		0.013	0.031		-0.078	-0.212
		(0.563)	(0.374)		(0.592)	(0.371)		(0.351)	(0.115)
Tangibility in t-1		0.012	0.030		0.012	0.029		-0.122	0.059
		(0.607)	(0.313)		(0.632)	(0.338)		(0.281)	(0.729)
Market to book ratio in t-1		0.013	-0.012		0.013	-0.017		0.112	0.159
		(0.426)	(0.576)		(0.470)	(0.506)		(0.274)	(0.366)
Constant	0.074***	0.039	-0.018	0.077***	0.039	-0.025	0.349***	0.069	-0.366
	(0.000)	(0.141)	(0.768)	(0.000)	(0.162)	(0.701)	(0.000)	(0.777)	(0.431)
Observations	1,843	1,780	1,744	1,843	1,780	1,744	1,843	1,780	1,744
R-squared	0.004	0.010	0.037	0.005	0.010	0.039	0.012	0.024	0.071
Number of gvkey	271	267	256	271	267	256	271	267	256
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Entropy Balancing	NO	NO	YES	NO	NO	YES	NO	NO	YES

Table VIII: Effects of mandatory disclosure when private information in stock prices is low and when firms face high product-market similarity

This table displays the results of OLS panel regression of equation 1 when the dependent variables are the financial constraints indices. In panel A, the sample is divided between firms with private information in stock price above and below the median of sample used in the experiment (treated group plus control group). In panel B, the sample is divided between firms with product market similarity above and below the median of all public firms in 2007. Firm characteristics such as size, age, market to book ratio, profitability, tangibility, and size of 10K are included as controls, as well as firm and year fixed effects. The control group corresponds to all firms with a public float between $25 and $50 million after entropy balance. All variables are winsorized at the 1-99% level and non-binary right-hand variables are standardized. Standard errors are clustered by firm and p-values are reported in parenthesis. Significance levels are indicated: *=10%, **=5%, ***=1%.

Panel A: Private information in stock prices

	Equity Constraints		Debt Constraints	
	(1)	(2)	(3)	(4)
VARIABLES	High Info	Low Info	High Info	Low Info
post2008xTreated	-0.021	0.044***	-0.018	-0.023**
	(0.149)	(0.000)	(0.116)	(0.022)
Constant	0.012	-0.073	-0.010	0.018
	(0.824)	(0.175)	(0.539)	(0.189)
Observations	709	847	709	847
R-squared	0.073	0.107	0.079	0.067
Number of gvkey	119	132	119	132
Controls	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Entropy Balancing	YES	YES	YES	YES

Panel B: Product market similarity

	Equity Constraints		Debt Constraints	
	(1)	(2)	(3)	(4)
VARIABLES	High similarity	Low similarity	High similarity	Low similarity
post2008xTreated	0.022*	0.014	-0.018*	-0.020*
	(0.099)	(0.240)	(0.075)	(0.063)
Constant	0.055	-0.065	0.005	0.007
	(0.447)	(0.124)	(0.775)	(0.594)
Observations	557	990	557	990
R-squared	0.113	0.042	0.080	0.075
Number of gvkey	92	158	92	158
Controls	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Entropy Balancing	YES	YES	YES	YES

Table IX: Effects of mandatory disclosure when trade secrets are legally protected

This table displays the results of OLS panel regression of equation 1 when the dependent variables are the financial constraints indices and the issuance dummy variables. The sample is divided between states in where trade secrets are protected by the Uniform Trade Secrets Act (UTSA) and states in where they are not. Firm characteristics such as size, age, market to book ratio, profitability, tangibility, and size of 10K are included as controls, as well as firm and year fixed effects. The control group corresponds to all firms with a public float between $25 and $50 million after entropy balance. All variables are winsorized at the 1-99% level and non-binary right-hand variables are standardized. Standard errors are clustered by firm and p-values are reported in parenthesis. Significance levels are indicated: *=10%, **=5%, ***=1%.

	Equity Constraints		Debt Constraints		Equity issuance		Total debt issuance		Long-term debt issuance		Short-term debt issuance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	UTSA=1	UTSA=0	UTSA=1	UTSA=0	UTSA=1	UTSA=0	UTSA=1	UTSA=0	UTSA=1	UTSA=0	UTSA=1	UTSA=0
post2008xTreated	0.021*	0.017	-0.010	-0.033***	0.008	0.053	0.079	0.119	0.062	0.195***	0.067	-0.015
	(0.084)	(0.281)	(0.291)	(0.004)	(0.851)	(0.166)	(0.169)	(0.145)	(0.219)	(0.007)	(0.180)	(0.820)
Constant	-0.028*	-0.018	-0.012	-0.000	0.075	-0.046	0.243**	0.037	0.118	0.038	-0.063	-0.171
	(0.091)	(0.560)	(0.354)	(0.987)	(0.517)	(0.717)	(0.027)	(0.834)	(0.223)	(0.825)	(0.524)	(0.311)
Observations	1,039	517	1,039	517	1,153	600	1,153	599	1,153	599	1,155	601
R-squared	0.079	0.058	0.089	0.127	0.073	0.063	0.075	0.102	0.031	0.099	0.139	0.108
Number of gvkey	166	88	166	88	174	94	174	94	174	94	174	94
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Entropy Balancing	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Table X: Effects of mandatory disclosure on firms that report 10KSB (Alternative Treated)

This table displays the results of OLS panel regression of equation 1 when the dependent variable are total constraints, equity constraints and debt constraints. The control group includes all firms with a public float between $25 and $50. Sales and age are in logarithms. The size of the 10K is also included in the control variables, as the log of the number of paragraphs in the 10K. In all regressions, firm and year fixed effects are included and standard errors are clustered by firm. All variables are winsorized at the 1-99% level and non-binary right-hand variables are standardized. P-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

VARIABLES	(1) Total Constraints	(2) Equity Constraints	(3) Debt Constraints
post2008xAlternativeTreated	-0.002	0.006	-0.003
	(0.865)	(0.585)	(0.746)
Log Sales in t-1	-0.010	-0.011	-0.002
	(0.315)	(0.318)	(0.553)
Log Firm age in t-1	-0.018**	-0.015	0.003
	(0.037)	(0.128)	(0.535)
Profitability in t-1	-0.009***	-0.011***	0.003
	(0.006)	(0.002)	(0.139)
Tangibility in t-1	-0.003	-0.007	0.013***
	(0.723)	(0.329)	(0.007)
Market to book ratio in t-1	0.000	-0.000	0.006***
	(0.926)	(0.892)	(0.007)
Size of 10-K	0.004	0.003	0.002
	(0.133)	(0.233)	(0.454)
Constant	-0.050***	-0.048***	-0.012
	(0.002)	(0.006)	(0.247)
Observations	1,572	1,572	1,572
R-squared	0.044	0.055	0.070
Number of gvkey	293	293	293
Firm FE	YES	YES	YES
Year FE	YES	YES	YES

Table XI: Effects of mandatory disclosure on innovative firms

This table displays the results of OLS panel regression of equation 1 when the dependent variables are equity constraints and debt constraints. Firm characteristics such as size, age, market to book ratio, profitability, tangibility, and size of 10K are included as controls, as well as firm and year fixed effects. The control group corresponds to all firms with a public float between $25 and $50 million after entropy balance. Panel A reports the estimation of equation 1 for firms with and without proprietary information in 2007. Panel B reports estimation for firms with high and low R&D (scaled by sales) in 2007. All variables are winsorized at the 1-99% level and non-binary right-hand variables are standardized. Standard errors are clustered by firm and p-values are reported in parenthesis. Significance levels are indicated: *=10%, **=5%, ***=1%.

Panel A: Proprietary information

	Equity Constraints		Debt Constraints	
	(1)	(2)	(3)	(4)
VARIABLES	prop info=1	prop info=0	prop info=1	prop info=0
post2008xTreated	0.032**	0.010	0.001	-0.034***
	(0.027)	(0.439)	(0.916)	(0.001)
Constant	-0.009	-0.041**	0.008	0.000
	(0.695)	(0.049)	(0.616)	(0.975)
Observations	724	832	724	832
R-squared	0.121	0.044	0.042	0.103
Number of gvkey	125	126	125	126
Controls	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Entropy Balancing	YES	YES	YES	YES

Panel B: R&D over sales

	Equity Constraints		Debt Constraints	
	(1)	(2)	(3)	(4)
VARIABLES	High R&D	Low R&D	High R&D	Low R&D
post2008xTreated	0.024*	0.015	-0.010	-0.022**
	(0.081)	(0.197)	(0.241)	(0.031)
Constant	-0.037*	-0.034*	-0.010	0.014
	(0.052)	(0.050)	(0.416)	(0.272)
Observations	792	764	792	764
R-squared	0.050	0.062	0.096	0.052
Number of gvkey	128	123	128	123
Controls	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Entropy Balancing	YES	YES	YES	YES

Table XII: Placebo test: change in the year of treatment

This table displays the results of equation 1 using different years of treatment. Each estimation considered four years of pre-treatment and four years of post-treatment. Only the interaction TREATEDxPOST is reported (p-values are reported in parenthesis). Firm characteristics such as size, age, market to book ratio, profitability, tangibility, and size of 10K are included as controls, as well as firm and year fixed effects. The control group corresponds to all firms a with public float between $25 and $50 million after entropy balance. Columns (1), (2) and (3) show results for total constraints, equity constraints, and debt constraints, respectively. All variables are winsorized at the 1-99% level and standard errors are clustered by firm. p-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

Post treatment beginning after:	(1) Total constraints	(2) Equity constraints	(3) Debt constraints
2004	-0.009	-0.005	0.008
	(0.420)	(0.688)	(0.196)
2005	-0.005	0.001	0.001
	(0.623)	(0.951)	(0.851)
2006	0.004	0.006	-0.007
	(0.647)	(0.517)	(0.304)
2007	0.011	0.010	-0.015**
	(0.274)	(0.298)	(0.012)
2008	0.021**	0.018*	-0.018**
	(0.035)	(0.065)	(0.017)
2009	0.016	0.011	-0.023***
	(0.111)	(0.310)	(0.008)
2010	-0.002	-0.001	-0.017*
	(0.808)	(0.924)	(0.085)
2011	-0.008	0.000	-0.011
	(0.485)	(0.975)	(0.309)
2012	-0.007	0.006	-0.006
	(0.242)	(0.354)	(0.302)

Table XIII: Placebo test: treating the control group

This table displays the results of equation 1 using the original control group as treated. In Panel A, half of the original control firms were randomly assigned as treated and the other half as the control. Entropy balance was then implemented between the new treated firms and the rest of the control group. Panel B uses the original control group as the treated group and compares it to a new control group defined as all firms with a public float between $50 and $75 million. Columns (1), (2) and (3) show results for total constraints, equity constraints, and debt constraints, respectively. Firm characteristics such as size, age, market to book ratio, profitability, tangibility, and size of 10K are included as controls, as well as firm and year fixed effects. All variables are winsorized at the 1-99% level and standard errors are clustered by firm. p-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

Panel A

	(1)	(2)	(3)
VARIABLES	Total Constraints	Equity Constraints	Debt Constraints
post2008xTreated	0.000	-0.004	-0.009
	(0.986)	(0.677)	(0.287)
Constant	-0.041**	-0.033*	-0.006
	(0.017)	(0.051)	(0.621)
Observations	1,085	1,085	1,085
R-squared	0.056	0.057	0.077
Number of gvkey	169	169	169
Controls	YES	YES	YES
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Entropy Balancing	YES	YES	YES

Panel B

VARIABLES	Total Constraints	Equity Constraints	Debt Constraints
post2008xTreated	-0.008	-0.011	0.010
	(0.390)	(0.235)	(0.186)
Constant	-0.056***	-0.051**	0.012
	(0.003)	(0.015)	(0.516)
Observations	1,253	1,253	1,253
R-squared	0.035	0.048	0.051
Number of gvkey	260	260	260
Controls	YES	YES	YES
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Entropy Balancing	YES	YES	YES

Table XIV: Robustness check: control firms that voluntarily reported regular 10K before 2007

This table displays the results of equation 1 including in the control group only firms that always voluntarily disclosed 10K when were below $25 million, but they were not treated because in 2007 were already above the threshold. Same specification and entropy balance are applied. Column (1) presents the results for total constraints, column (2) for equity constraints and column (3) for debt constraints. Firm characteristics such as size, age, market to book ratio, profitability, tangibility, and size of 10K are included as controls, as well as firm and year fixed effects. Panel A presents the results before entropy balance and Panel B after. All variables are winsorized at the 1-99% level and standard errors are clustered by firm. p-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

Panel A: without entropy balance

VARIABLES	(1) Total Constraints	(2) Equity Constraints	(3) Debt Constraints
post2008xTreated	0.021**	0.017*	-0.020***
	(0.028)	(0.069)	(0.007)
Log Sales in t-1	-0.000	-0.006	0.009
	(0.995)	(0.606)	(0.212)
Log Firm age in t-1	-0.016	-0.008	-0.002
	(0.199)	(0.433)	(0.665)
Profitability in t-1	-0.008**	-0.007*	-0.003
	(0.039)	(0.088)	(0.323)
Tangibility in t-1	0.008	0.003	0.005
	(0.634)	(0.819)	(0.299)
Market to book ratio in t-1	-0.001	-0.001	0.003
	(0.520)	(0.585)	(0.216)
Size of 10-K	0.004	0.005*	-0.000
	(0.156)	(0.062)	(0.901)
Constant	-0.050***	-0.060***	0.007
	(0.000)	(0.000)	(0.540)
Observations	1,267	1,267	1,267
R-squared	0.038	0.032	0.066
Number of gvkey	194	194	194
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Entropy Balancing	NO	NO	NO

Panel B: after entropy balance

VARIABLES	(1) Total Constraints	(2) Equity Constraints	(3) Debt Constraints
post2008xTreated	0.025**	0.020*	-0.016**
	(0.021)	(0.074)	(0.050)
Log Sales in t-1	-0.008	-0.022	0.020**
	(0.345)	(0.132)	(0.043)
Log Firm age in t-1	-0.018	-0.011	-0.005
	(0.233)	(0.432)	(0.444)
Profitability in t-1	-0.007	-0.004	-0.002
	(0.102)	(0.392)	(0.514)
Tangibility in t-1	0.001	-0.001	0.007
	(0.954)	(0.970)	(0.236)
Market to book ratio in t-1	-0.003	-0.004	0.002
	(0.221)	(0.146)	(0.215)
Size of 10-K	-0.001	0.001	-0.002
	(0.853)	(0.948)	(0.787)
Constant	-0.017	-0.042	0.028
	(0.712)	(0.403)	(0.447)
Observations	1,267	1,267	1,267
R-squared	0.057	0.057	0.061
Number of gvkey	194	194	194
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Entropy Balancing	YES	YES	YES

Table XV: Robustness check: excluding firms that became (were) too big after (before) the shock

This table displays the results of equation 1 using firms with a public float between $25 and $50 million as the control group. Firms that reached a public float above $500 million at any time are removed from the sample. Same specification and entropy balance are applied. Column (1) presents the results for total constraints, column (2) for equity constraints and column (3) for debt constraints. Firm characteristics such as size, age, market to book ratio, profitability, tangibility, and size of 10K are included as controls, as well as firm and year fixed effects. All variables are winsorized at the 1-99% level and standard errors are clustered by firm. p-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

Panel A: without entropy balance

VARIABLES	(1) Total Constraints	(2) Equity Constraints	(3) Debt Constraints
post2008xTreated	0.020*	0.017*	-0.022***
	(0.051)	(0.086)	(0.004)
Log Sales in t-1	0.005	0.007	0.005
	(0.527)	(0.399)	(0.427)
Log Firm age in t-1	-0.027***	-0.025***	0.005
	(0.002)	(0.004)	(0.601)
Profitability in t-1	-0.007**	-0.009**	-0.000
	(0.030)	(0.018)	(0.943)
Tangibility in t-1	0.010	0.006	0.014***
	(0.528)	(0.643)	(0.009)
Market to book ratio in t-1	-0.001	0.000	0.003**
	(0.749)	(0.866)	(0.048)
Size of 10-K	0.003	0.004	-0.001
	(0.427)	(0.190)	(0.495)
Constant	-0.034*	-0.036**	0.006
	(0.066)	(0.013)	(0.571)
Observations	1,525	1,525	1,525
R-squared	0.053	0.054	0.070
Number of gvkey	247	247	247
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Entropy Balancing	NO	NO	NO

Panel B: after entropy balance

VARIABLES	(1) Total Constraints	(2) Equity Constraints	(3) Debt Constraints
post2008xTreated	0.023**	0.020**	-0.016**
	(0.015)	(0.031)	(0.020)
Log Sales in t-1	0.004	0.003	0.002
	(0.589)	(0.775)	(0.707)
Log Firm age in t-1	-0.018*	-0.013	0.003
	(0.069)	(0.265)	(0.526)
Profitability in t-1	-0.008**	-0.008**	-0.000
	(0.012)	(0.029)	(0.890)
Tangibility in t-1	0.011	0.008	0.012**
	(0.448)	(0.523)	(0.016)
Market to book ratio in t-1	0.001	0.002	0.005***
	(0.489)	(0.290)	(0.001)
Size of 10-K	0.004	0.004	0.000
	(0.163)	(0.129)	(0.904)
Constant	-0.040***	-0.043***	-0.002
	(0.002)	(0.002)	(0.835)
Observations	1,551	1,551	1,551
R-squared	0.045	0.041	0.056
Number of gvkey	256	256	256
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Entropy Balancing	YES	YES	YES

Table XVI: Robustness check: a control group with public float between $75 and $110 million.

This table displays the results of equation 1 using firms with a public float between $75 and $110 million as the control group. Same specification and entropy balance are applied. Column (1) presents the results for total constraints, column (2) for equity constraints and column (3) for debt constraints. Firm characteristics such as size, age, market to book ratio, profitability, tangibility, and size of 10K are included as controls, as well as firm and year fixed effects. All variables are winsorized at the 1-99% level and standard errors are clustered by firm. p-values are reported in parenthesis and significance levels are indicated: *=10%, **=5%, ***=1%.

VARIABLES	(1) Total Constraints	(2) Equity Constraints	(3) Debt Constraints
post2008xTreated	0.017	0.022*	-0.019**
	(0.196)	(0.081)	(0.041)
Log Sales in t-1	0.026**	0.017	-0.012
	(0.028)	(0.159)	(0.158)
Log Firm age in t-1	-0.030*	-0.012	-0.011
	(0.064)	(0.388)	(0.162)
Profitability in t-1	-0.004	-0.004	0.004
	(0.568)	(0.507)	(0.333)
Tangibility in t-1	0.013	-0.011	0.003
	(0.261)	(0.320)	(0.646)
Market to book ratio in t-1	-0.001	0.000	0.004*
	(0.832)	(0.883)	(0.053)
Size of 10-K	0.004	0.000	0.001
	(0.180)	(0.877)	(0.524)
Constant	-0.019	-0.017	-0.010
	(0.114)	(0.124)	(0.330)
Observations	1,468	1,468	1,468
R-squared	0.062	0.035	0.066
Number of gvkey	245	245	245
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Entropy Balancing	YES	YES	YES

Figure 2: 10KSB vs regular 10K

The first plot shows the average number of paragraph of 10KSB and 10K during the sample period (2004-2011). The second plot shows the average number of filling downloaded from EDGAR per firm. The third plot shows the average total fees payed to auditors (in this plot, only firms with public float below $100 million are considered). The fourth plot shows the ratio of firms audited by a "Big Four" auditor.

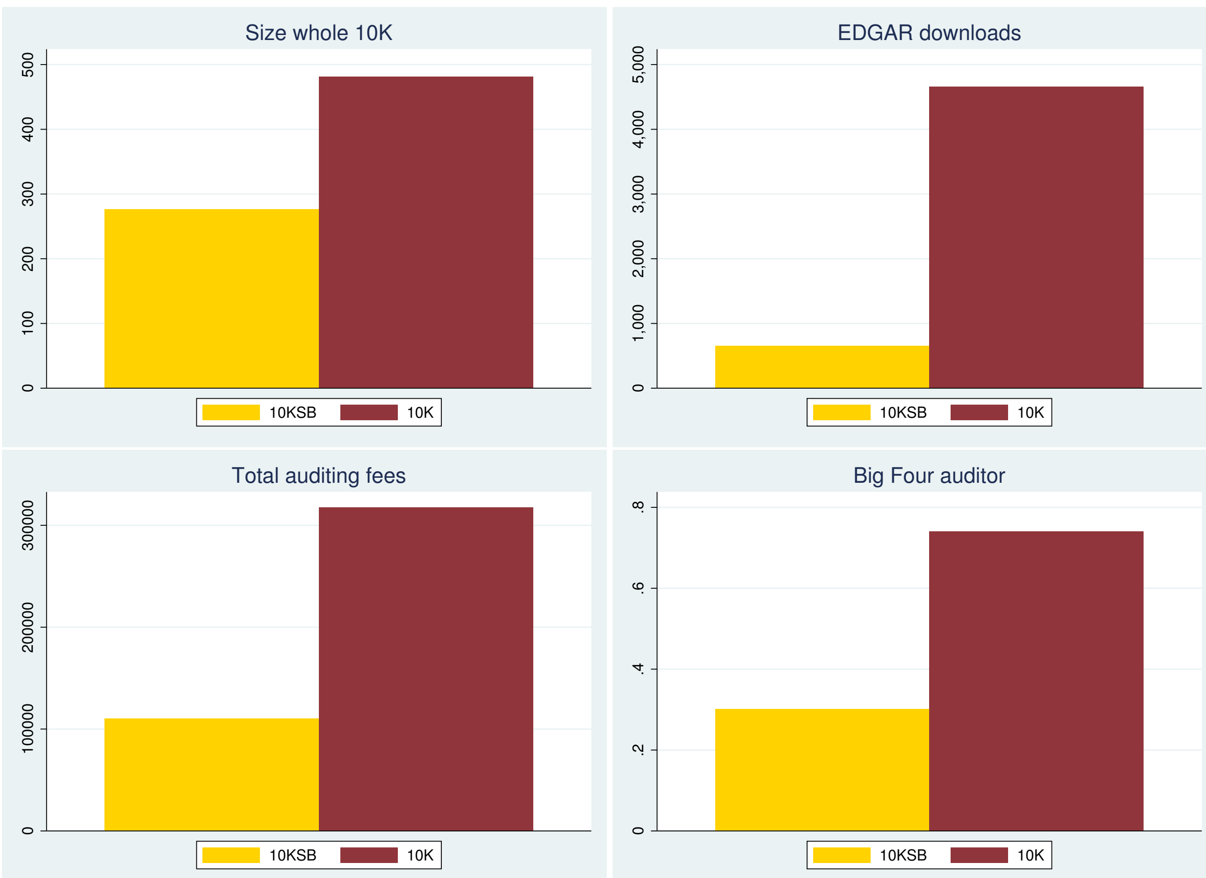

Figure 3: Distribution of firm's industries

The figure shows firms scattered throughout the 4-digit sic codes. Each blue triangle represents a treated firm, while each red circle represents control firm.

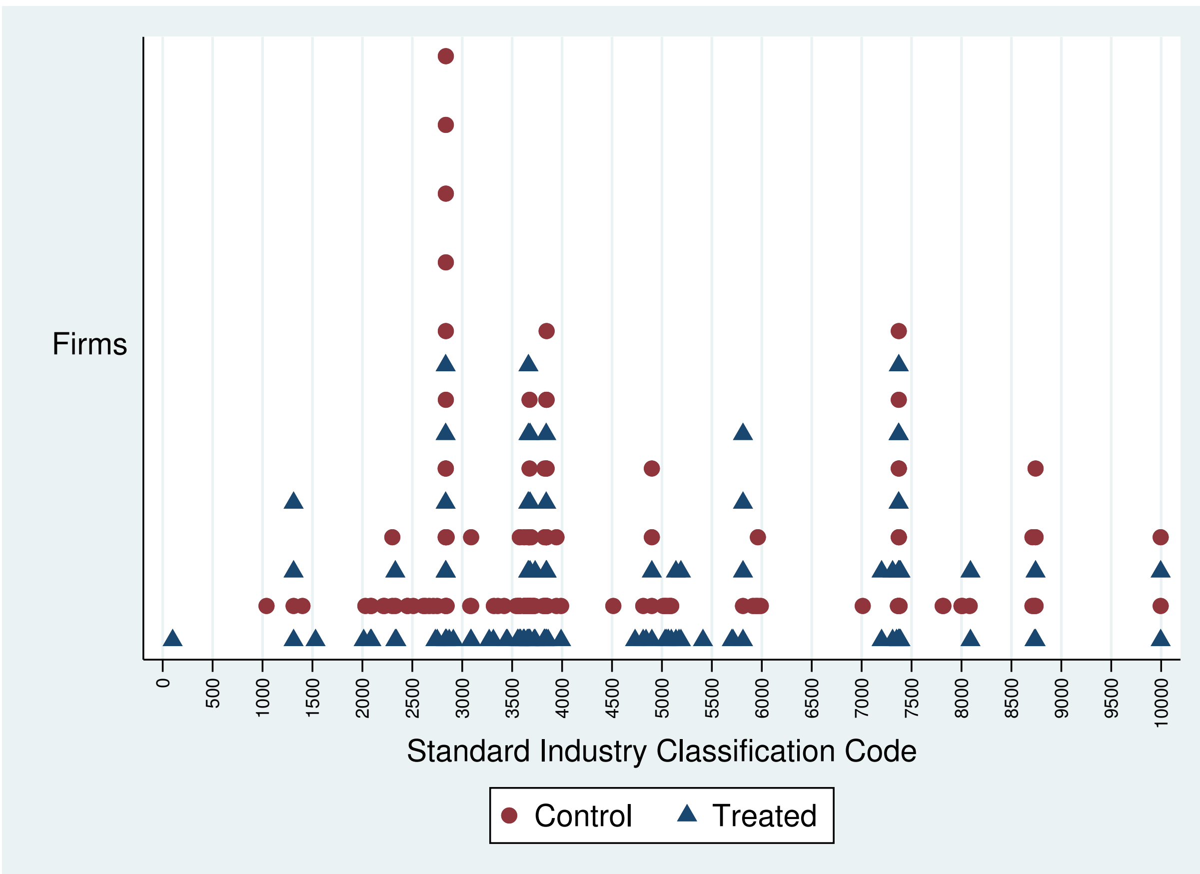

Figure 4: Debt Constraints

The figure shows the fitted residuals from the regression of debt constraints on all control variables used in equation 1 -including fixed effects-, except for the interaction post2008xVOLtreat. The blue line represents the fitted residuals of the treated firms and the red line the fitted residuals of the control firms. The control group corresponds to all firms with public a float between \$25 and \$50 million after entropy balance. The vertical line corresponds to the year 2007.

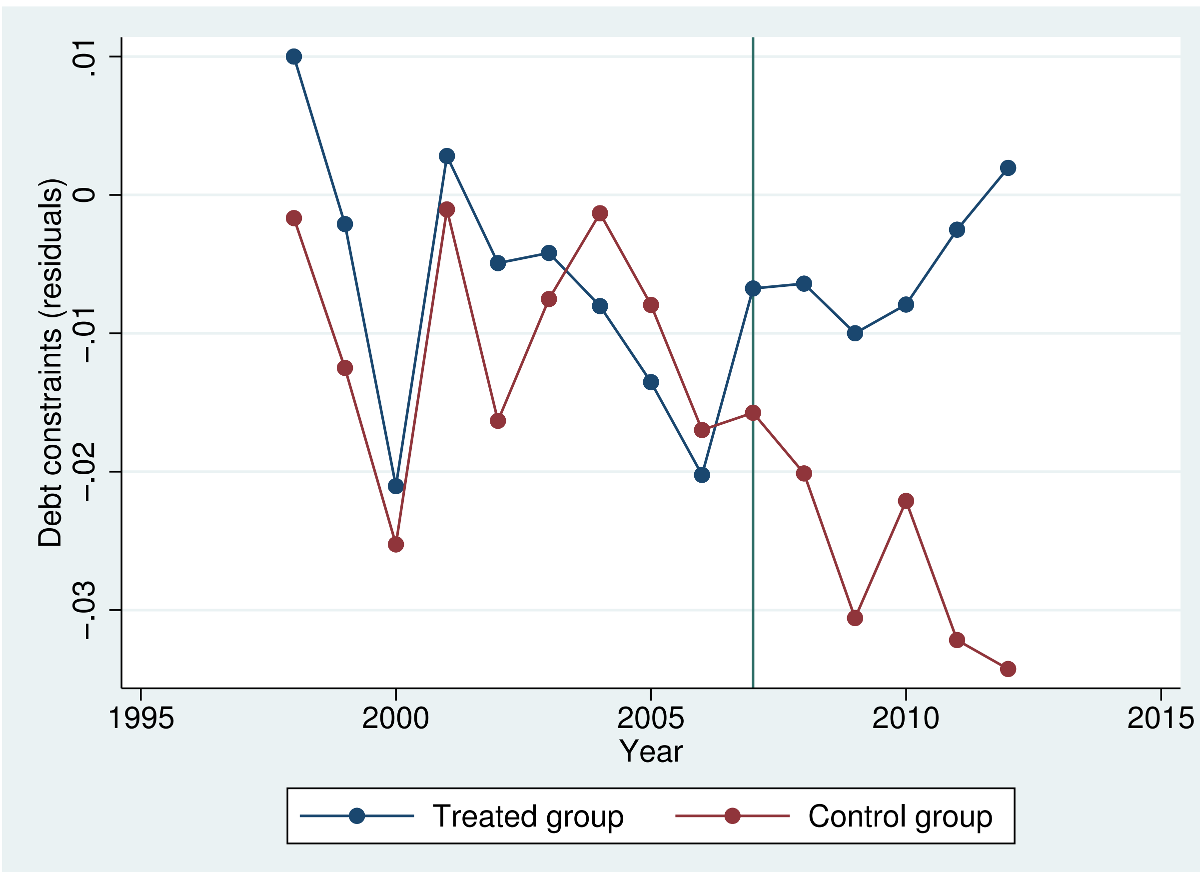

Figure 5: Equity Constraints

The figure shows the fitted residuals from the regression of equity constraints on all control variables used in equation 1 -including fixed effects-, except for the interaction post2008xVOLtreat. The blue line represents the fitted residuals of the treated firms and the red line the fitted residuals of the control firms. The control group corresponds to all firms with a public float between $25 and $50 million after entropy balance. The vertical line corresponds to the year 2007.