Offering Speed and Private Placements: Evidence from a Quasi-Natural Experiment

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Abstract

We empirically show that the time delay for raising debt in public markets has a pronounced impact on firms switching to private placement. We exploit the 2005 Security Offerings Reform as a quasi-natural experiment where for a subset of large U.S. firms the regulatory delay (1-1.5 month) associated with raising public debt is eliminated. Difference-in-difference estimates suggest a direct link between regulatory delay on public markets and the share of private placements. Following the Reform, affected firms have reduced their share of private market placements by 25-30% in comparison to unaffected control-group firms. The empirical findings lead to the conclusion that the Reform has been a success as the SEC has achieved its purpose to contain the growth of private placements and to strengthen public markets.

Keywords: Securities Offering Reform, corporate regulation, financing, time delay

JEL Classification: D23; D92; G32; G38; K22

1 Introduction

On June 29, 2005, the Securities Exchange Commission (henceforth SEC) changed the public shelf registration procedure for debt and equity issues from a transaction- and securities-based system to a company based system under which firms, once they underwent the general registration procedure, can issue securities without delay. The SEC started contemplating such a move as early as 1996 (Securities and Exchange Commission, 1996; Bethel and Sirri, 1998), reacting to the unexpected and partly unintended growth of private placements at the cost of public placements.

By adopting Rule 144A in 1990 and allowing for private placements to be freely traded among qualified institutional buyers (henceforth QIBs), the SEC offered an alternative to the Rule 415 public shelf registration procedure which on average caused a regulatory delay of 1-1.5 month from filing to effectiveness (Kulak, 2011; Huang and Ramirez, 2010; Gustafson, 2012). In doing so the SEC hoped to create a more liquid class of private placements in order to attract new and predominantly foreign issuers which were previously discouraged by the illiquidity premia in traditional private placement markets as well as by the registration requirements of the public shelf market (Carey, Prowse, Rea, and Udell, 1993). And indeed foreign bond issuance in the U.S. has been on the rise after 1990. But as the increase is robust in both, public shelf and private Rule 144A markets, this growth cannot be attributed to the adoption of Rule 144A.

While it seems unlikely that it was Rule 144A that triggered the intended growth of the U.S. market for foreign bonds, we can unambiguously attribute the less intended migration of domestic debt issues from public markets to private markets to the adoption of Rule 144A. Especially speculative-grade firms flocked to the Rule 144A market. Following its inception in April 1990, the Rule 144A market already in 1997 captured more than 80% of domestic junk bond offerings (Fenn, 2000; Livingston and Zhou, 2002; Chaplinsky and Ramchand, 1998) as well as 83% of the domestic convertible debt issues in 2004 (Huang and Ramirez, 2010) and analysts anticipated that Rule

144A would eventually capture the entire sub-prime market.

The existing literature provides a number of explanations for the increased use of private debt registration under Rule 144A by U.S. firms.

Arguably the absence of registration leads to less disclosure while speedier issuance leaves less time for a thorough due diligence. But there is little evidence that this was a big concern for investors. Press accounts document mixed investor sentiment on the significance of disclosure and due diligence. Further, if private issuance under Rule 144A exposed investors to information asymmetries, this should be reflected in the premia paid on Rule 144A debt vis-a-vis publicly registered debt.

While Chaplinsky and Ramchand (1998) find that investors require significant premia for debt issues on the Rule 144A market and Sengupta (1998) shows that disclosure quality generally impacts premia paid on debt, there is contradicting evidence from studies in the 1980s, suggesting that shelf registration reduced bond yields (Kidwell, Marr, and Thompson, 1984; Rogowski and Sorensen, 1985). But Gao and Ritter (2010) and Allen, Lamy, and Thompson (1990) demonstrate that firms which self-select to take advantage of accelerated equity offerings differ significantly from those that chose to opt for traditional non-accelerated offerings. Fenn (2000), corrects for self-selection by controlling for credit rating, and finds that premia on 144A debt disappeared shortly after the inception of the 144A market.

While for debt issuance the importance of disclosure as well as on having sufficient time for a due diligence seems to be limited, Gustafson (2012) shows that time for disclosure and a thorough due diligence do matter for equity issues. He shows that following the 2005 Security Offering Reform, Well Known Seasoned Issuers' (henceforth WKSIs) reduced regulatory delay leads to a 27% increase in SEO underpricing, which is only partly offset by avoiding the pre-issuance stock price decline associated with the information the issuer conveys by announcing to rely on external financing. Issuers' opting for accelerated issuance in spite of the cost of exacerbate underpricing clearly illustrates their appreciation of flexibility and swift access to capital.

In a similar vain, there exists a vast literature emphasizing the importance of uncertainty and delayed access to capital markets for cash holdings (Baskin, 1987; Bates, Kahle, and Stulz, 2009; Opler, Pinkowitz, Stulz, and Williamson, 1999; Hugonnier, Malamud, and Morellec, 2010; Kulak, 2011). But US firms were already afforded flexibility and swift access to capital under Rule 415[1]. Shelf registration allows firms to register a large portfolio of securities ex-ante, so that in case of need registered securities could be taken "off the shelf" without any delay at all. But in spite of eliminating regulatory delay, shelf registration enjoyed only lukewarm popularity, accounting for about 50% of underwritten offers of debt securities and only 20% of all potential equity issues (Securities and Exchange Commission, 1996; Bethel and Krigman, 2008). Further, shelf registration was used by only a few predominantly large firms (Securities and Exchange Commission, 1996), representing about 14% of all eligible firms (Kulak, 2011). The reluctant usage of shelf-registration, especially by small firms, is frequently blamed on "market overhang" and firms difficulty in predicting future capital needs. Most saliently, managers are reluctant to pre-registering securities, as conveying future need for external financing is viewed as a negative signal to investors (Bhagat, Marr, and Thompson, 1985; Allen, Lamy, and Thompson, 1990; Denis, 1991, 1993; Bethel and Krigman, 2008). The relatively lower share of equity issues in public shelf registrations is consistent with the pecking order theory of capital, which predicts investors viewing debt issues as a less concerning signal then equity issues (Scholes, 1972; Leland and Pyle, 1977).

The SEC enacted the 2005 Securities Offering Reform, educated by the believe that firms' placement choice is chiefly contingent on avoiding regulatory delay without having to reveal future capital need. This view would explain why only few firms exploited the possibility to eliminate regulatory delay through shelf registration, but

[1]Shelf registration under Rule 415 allows firms pre-registering securities. While a regulatory delay from filing to effectiveness of 1-1.5 months is common, firms can "take securities off the shelf" for up to two years post effectiveness. Shelf registration potentially allows firms to sort out regulatory issues in advance and thus eliminate the time delay before offerings. It does, however, require firms to anticipate and reveal future capital needs. Rule 415 was permanently adopted in 1983, though it has been discussed from the 1940s onwards (Treadway Jr, 1983).

gave up public placement in favour of private placement. It further would predict that firms affected by the Reform should resort to public market placement.

The paper makes two main contributions to the literature. First, we evaluate the impact of the 2005 Securities Offering Reform. We find that the SEC has succeeded in containing the growth of private placements for debt- and convertible debt issues. Further, we cannot link the Reform to an increase of public placements for equity issues, which is consistent with Gustafson (2012)'s finding that for common equity issues a decrease in regulatory delay is paid for with additional underpricing, while there a decrease in regulatory delay does not lead to higher premia in debt issues (Fenn, 2000).

Second, our analysis highlights the importance of the frictions and time delays firms encounter in raising external capital. The theoretical literature has evolved from frictionless markets allowing for instantaneous adjustments to the capital structure (Modigliani and Miller, 1958) to theoretical models accounting for frictions and their impact on firms' financing choices (Baskin, 1987; Hugonnier, Malamud, and Morellec, 2010). A number of recent empirical studies show that urgent liquidity needs and strategic implications arising from regulatory delay can inform firms' choice of security type as well as placement channel (Fenn, 2000; Chaplinsky and Haushalter, 2010; Gao and Ritter, 2010; Kulak, 2011). Our paper adds to these studies by exploiting a quasi-experimental setting with exogenous variation in regulatory time delay to show that firms' choice of issuance channel is informed by delay considerations.

The remainder of the paper is organized as follows: Section 2 provides an overview of the institutional background of the 2005 Securities Offering Reform. Section 3 derives the research design and its econometric implementation, section 4 describes the sample construction, and section 5 presents the results. Section 6 concludes.

2 Institutional Background

Following the passage of the Securities Act of 1933, public firms in the U.S. need to undergo a review process with the SEC before receiving the permission to issue new securities. Firms first filed a preliminary registration statement, waited for SEC clearance and then issued securities. SEC clearance in case of a preliminary review takes in the region of one month, while average waiting times are closer to three month in case of a detailed review. But more importantly, figure 1 illustrates that firms always faced a risk of waiting as much as half a year in extreme cases (Masulis and Korwar, 1986; Mikkelson and Partch, 1986; Securities and Exchange Commission, 1996; Bethel and Krigman, 2008; Bortolotti, Megginson, and Smart, 2008). This waiting time creates strong incentives for firms to sidestep public placement in favour of private placements.

[INSERT TABLE 1 HERE]

But the 2005 Security Offerings Reform has eliminated this regulatory delay for a subset of firms with a public float $\geq \$700M$, by assigning them WKSI status and granting them access to the much swifter "Automatic Shelf Registration" (henceforth ASR). Under ASR, WKSIs still need to file their capital issues, but their filings are immediately effective. For a group of firms, the Reform therefore eliminated the time delay in public markets and with it an important reason to deviate to private markets. For the other group, non-WKSIs, nothing changed.

The assignment of WKSI status is mainly hinging on a public float test which assigns WKSI status to firms with a public float of at least \$700M [2]. Using a dataset compiled by Kulak (2011), which contains public float information collected from U.S. firms' 10-K statements, we find significant and economically relevant evidence for a causal link between regulatory time delay on public markets and firms' decision to opt

[2]Public float is defined as the market value of a firms' voting and non-voting common equity, which is held by non-affiliates of the firm. Affiliates are defined as large block holders as well as directors and officers.

for private markets. Difference-in-Difference estimates suggest that the Reform has led to a 25-30% reduction in WKSI firms issuing debt on private markets, down from 60% before the Reform.

As the assignment to treated (WKSI) and control firms is not random but driven by public float or more generally size, we take a series of measures to defuse concerns that there exist systematic differences between treatment and control firms. Most notably the forcing variable public float (size) exhibits a strong negative correlation with firms' likelihood to opt for private placements, most notably because information asymmetries in large firms are less severe than in small firms. We repeat the difference-in-difference estimation of the treatment effect with different configurations of control variables which are discussed in the literature as proxies for information asymmetries. Further, the model's robustness to varying specifications is tested, by skipping industry fixed effects as well as introducing control variables as interaction terms into the main difference-in-difference specification. We also re-estimate the treatment effect with six month, one year, 18 month, and two year time interval lengths before and after the Reform's inception. Finally, we conduct a placebo analysis and try to reproduce the regression results for seven adjacent time intervals with imaginary pre- and post-reform cut-offs, and find that all estimates of the treatment effect become statistically insignificant and small in size. The coefficients and t-scores of the remaining covariates remain largely unchanged.

3 Test of the Time Delay Hypothesis

With its two-tier structure, the 2005 Securities Offerings Reform constitutes a quasi-natural experiment, allowing inferences as to whether delays in the capital raising process cause firms to opt for private placement of securities. If firms preferred to issue securities on private markets because of the time delay they face on public markets, then those firms affected by the Reform should respond by subsequently reducing security placements on private markets relative to firms unaffected by the Reform.

Of cause shelf registration already afforded WKSIs the option to issue securities without time delay before the Reform. If firms with WKSI status nonetheless respond to the Reform by decreasing their rate of private placements relative to non-WKSI firms, that also hints at shortcomings of the shelf registration procedure, such as imperfect foresight, deterrent registration costs, or the negative signalling effect of indicating future need for external financing.

In addition to reducing the regulatory delay for WKSIs, the Reform has relaxed WKSIs' disclosure requirements. While this might theoretically have reduced issuance cost, the SEC justified the relaxation of disclosure by stating that WKSIs' disclosure environment would not be materially affected (Gustafson, 2012). Further, Shroff, Sun, White, and Zhang (2011) find no differences in disclosure changes surrounding the inception of the Reform.

It is well documented that industries differ in terms of cash flow volatility or severity of information asymmetries. Although a good part of industry specific variation should be captured by introducing control variables such as leverage, cash holdings, or intangibles, industry specific intercepts are still likely to be correlated with covariates. We control for this by introducing fixed effects on industry level. Industries are clustered by the first two SIC code digits.

Examining the Reform's effect on issuers' probability to use private markets implies using a dummy as dependent variable. In a setting where we aim to predict individuals' responses, which by definition would take values of zero and one, we should use a logit regression. As our analysis aims at estimating an average effect on an entire class of issuers, rather than an individual issuer's reaction, we do not require the marginal properties of a logit regression near the [0,1] boundaries. Further, the linear interpretation of OLS coefficients is much more intuitive than the marginal interpretation of logit coefficients. We nonetheless discuss the methodology of logit difference-in-difference estimators in appendix A.1. We further discuss and compare logit difference-in-difference estimates in appendix A.2.

As the 2005 Security Offerings Reform assigns WKSI status to a clearly defined

set of firms with a public float $\geq \$700M$, Regression Discontinuity Design (RDD) would in principle be a feasible choice of empirical procedure. But as we already limit our analysis to observations directly before and after the Reform, additionally confining our analysis to issues of firms which lie in a narrow margin around the $\$700M$ public float cut-off would excessively restrict our sample size. Further, by applying RDD we would only evaluate the local impact of the Reform by focusing on changes in the dependent variable of firms with a public float in a narrow interval around the $\$700M$ cut-off. We instead opt for implementing difference-in-difference estimators which allow us to evaluate the Reform's impact on all affected firms with a public float $\geq \$700M$. The inevitable drawback of difference-in-difference estimation in comparison with RDD estimation is, that firm size is for a number of reasons associated with a lower likelihood to opt for private placements. The apparent advantage of RDD is then that it allows controlling for size directly. To diffuse concerns that this might systematically affect our results, we carefully chose control variables which capture all the underlying reasons why large firms are less likely to opt for private placement [3]. Further, we interact covariates, which differ very significantly across control and treatment sample, with the difference-in-difference main specification. As such we allow for the possibility that the effects of treatment might vary across units. Finally, we subject our estimates to various robustness checks.

3.1 Research Design and Identification

To measure the effect of the Reform on the prevalence of private placements, we estimate difference-in-difference estimators based on an industry fixed effects model [4].

[3] The literature argues that large firms are likely to have longer histories of ratings, are more likely to be diversified, are more likely to enjoy analyst coverage; all of which are factors mitigating information asymmetries from which public and small investors would suffer over proportionately.

[4] Our data constitutes a pseudo panel of i instances of debt issues, conducted by firms from j different industries. The observations are either treated or control - depending on the issuers public float - and either fall into the post reform or into the pre reform period. As such, each coefficient obtains the subscripts i and j.

$$
\begin{aligned}
Y_{i,j} &= \mu + \alpha_j + \gamma D_{i,j} + \lambda A_{i,j} + \delta D_{i,j} A_{i,j} + \tilde{x}\prime_{i,j}\Theta + \epsilon_{i,j} \\
&= \mu + \alpha_j + \gamma 1_{[Treat],i,j} + \lambda 1_{[Post],i,j} + \delta \left(1_{[Post],i,j} \times 1_{[Treat],i,j}\right) + \tilde{x}\prime_{i,j}\Theta + \epsilon_{i,j} (1)
\end{aligned}
$$

where $Y_{i,j}$ is an issue's conditional probability to be conducted as a private placement. μ is the average intercept, and α_j are industry specific intercepts. The difference-in-difference specification is given by the dummy variables $D_{i,j}$,$A_{i,j}$, and $DA_{i,j}$. $D_{i,j}$ is an indicator variable which defines whether an issuing firm was affected by the Reform (discussed below in more detail) and $A_{i,j}$ is an indicator variable defining whether a security placement took place in the period after the Reform. $DA_{i,j}$ is an indicator variable that defines placements which are conducted by treated firms after the Reform. Finally, $X_{i,j}$ is a vector of control variables. δ is the salient coefficient of equation (1) as it represents the average treatment effect of the treated (ATET) of the Reform. It compares pre- and post-reform changes in the share of private placements across firms that were affected by the Reform (treated) and those firms what were not (control). As a clean identification hinges on the assumption that changes in the share of private placements between the periods are only depending on their being effected by the Reform and not by other factors which happen to be correlated with treatment status. We therefore control for contemporaneous changes in control variables. Informed by the believe that time delays on public markets lead to a higher rate of public placements, we expect a negative treatment effect δ.

To define the treatment indicator $D_{i,j}$ in equation (1), we consider an issuers public float in the fiscal quarter preceding the capital issue[5]. We define the indicator to be one if the issuing firm surpassed the SEC's public float threshold at the fiscal quarter

[5]As a second avenue for firms which do not qualify for WKSI status based on their public float, the SEC offers the option to obtain WKSI status based on offering volume. The SEC's Office of Economic Analysis found that few firms staying below the public float cut-off would meet the offering volume requirement. Further, false treated firms would in principal lead to a downward bias of the treatment effect. As such, we disregard this alternative channel in the empirical specification.

end before issuance so that

$$D_{i,j} = \begin{cases} 1 & \text{if } Float_{i,j} \geq \$700M \\ 0 & \text{if } Float_{i,j} \leq \$700M \end{cases}$$

were issues with public floats clustered around the cut-off are not excluded[6].

3.2 Confounding effects and robustness checks

The identifying assumption of the difference-in-difference estimator in equation (1) is that conditional on the control variables $\Theta_{i,j}$, the error term $\epsilon_{i,j}$ is uncorrelated with $D_{i,j}$, the dummy variable indicating treatment. That implies that in a counter factual world without reform, and after controlling for the covariates Θ, the relative frequency of private placements should evolve in the same way for treated and control firms. That means that as far as their change in private placement frequency is concerned, firms should be as good as randomly assigned to treatment and control group.

The assignment of issues to the treatment and control group is of cause not random, but based on public float. While large firms may be less likely to opt for private placements in absolute terms, it is absolutely imperative that the *change* of private placement frequencies neither depends on firm size, nor on the value of any other covariate [7]. But assuming that the treatment effect does not vary across units, is a rather restrictive assumption.

While it is not possible to test this assumption, there are a number of measures

[6]Firms which obtain WKSI status keep this status for the upcoming year, even if their public float subsequently falls below the \$700M cut-off. But arguing that the main advantage of WKSI status is avoiding time delay in issuing capital, it is not plausible that firms with a public float just above \$700M who expect to loose WKSI status in the future, would issue more capital then they need to.

[7]It is crucial not to control for issuers' public float in regressions. By construction, the public float values of treated issues and control issues do not overlap. If, as conjectured, WKSI firms (treated) where to reduce their frequency of public placements in response to the Reform, this effect would be completely captured by a public float variable, thus downward biasing the estimate for the treatment effect. Following the same line of reasoning, the inclusion of book assets requires special attention. Public float (close to market capitalization) and book value of assets differ significantly as firms differ in leverage and market-to-book ratios. As such both variables have a correlation of 49%. Not controlling for controlling for book assets leads to a significant upwards bias of the treatment effect and an overstatement of the significance of its coefficient.

we employ to corroborate it. Fenn (2000) documents that following the inception of Rule 141 in 1990, the rate of private placements for small firms skyrocketed while it remained constant for large firm. This size effect largely disappeared after controlling for speculative ratings, thus illustrating the importance of including appropriate control variables. We consult the literature for alternative theories explaining private placements, and construct control variables to reduce their influence. The choice of control variables is discussed in more depth in section 3.3. We further experiment by varying the length of the time interval around the Reform and try to falsify the assumption of parallel trends of treated and control issuers by conducting a placebo analysis.

Finally, we relax the assumption that the treatment effects do not vary across units. This flexible specification is achieved by interacting control variables, for which we suspect a changing treatment effect, with the main specification of the difference-in-difference estimators, so that the model becomes:

$$\begin{aligned} Y_{i,j} &= \mu + \alpha_j + \gamma D_{i,j} + \lambda A_{i,j} + \delta D_{i,j} A_{i,j} \\ &+ \tilde{x\prime}_{i,j}\Theta + D_{i,j}\tilde{x\prime}_{i,j}\gamma_1 + A_{i,j}\tilde{x\prime}_{i,j}\lambda_1 + D_{i,j}A_{i,j}\tilde{x\prime}_{i,j}\delta_1 + \epsilon_{i,j} \end{aligned} \tag{2}$$

Where $\tilde{x'}_{i,j}$ is a rescaled regressor expressing the deviation from the treated group mean in the treatment period, such that:

$$\tilde{x}_{i,j} = x_{i,j} - E(x_{i,j}|D_{i,j} = 1, A_{i,j} = 1) \tag{3}$$

This yields unbiased estimates δ of the ATET, even when the treatment effect varies across units.

3.3 Endogenous Choice of Security Class

The focus of this paper lies on the effect of the 2005 Security Offering Reform on debt issues. However, firms do not face the decision whether to issue debt on a private or public market, or whether to issue equity on a public or private market, but enjoy all these options at the same time. Therefore, the choice of security type is endogenous and influenced by the Reform. We discuss a number of arguments diffusing such endogeneity concerns.

Equity to Debt (Private) & Debt to Equity (Private) From Fenn (2000) we know that for debt issues a reduction in regulatory delay does not lead to higher yields. While Gustafson (2012) shows that SEO underpricing increases with the elimination of regulatory delay, firms are still afforded the option to delay SEOs to avoid additional underpricing. We can therefore conclude that within security classes, the Reform has unequivocally rendered public issues more attractive. Responding to treatment by either changing from issuing equity to issuing private debt or changing from issuing debt to issuing private equity is therefore implausible.

Debt to Equity (Public) Investors perceive equity issues as much poorer signals than debt issues. This is largely due to the much severer information asymmetries equity issues entail. As such, shorter regulatory delay in equity issues leaves investors with less opportunities to conduct due diligence to mitigate the information asymmetries inherent to equity issues, so that issuers pay for the swifter access to capital by additional underpricing (Gustafson, 2012). In contrast, adverse selection is much less severe for debt issues, so that the reduction in regulatory delay, after controlling for rating quality, does not lead to higher yields (Fenn, 2000). Therefore, the Reform has unequivocally made public debt issues more attractive in comparison to private debt issues. For equity issues, however, the Reform's impact is ambiguous. This view is consistent with our finding that the 2005 Reform caused the share of private debt- & convertible debt issues to decline by approximately 25%, while the shares of private

and public equity issues remained unchanged. It is therefore not likely that firms would respond to treatment by switching from debt to public equity.

Equity to Debt (Public) It is not strictly implausible that firms would respond to the treatment by changing from issuing equity to issuing public debt. It is, however, not very likely as debt and equity issues differ in very salient aspects, such as signalling, severity of adverse selection and the way their values respond to changes in volatility or uncertainty. Most importantly, they drive leverage in opposite directions [8]. Responding to treatment by changing from issuing equity to issuing public debt, therefore requires incentives strong enough to justify increasing the deviation from the target leverage.

Based on the above arguments, we expect at worst a very small upwards bias of the estimates of the treatment effect for debt issues.

4 Data

4.1 Sample Construction

We use data collected on all debt- and equity issues conducted by publicly listed US firms during the period from December 1, 2002 until November 30, 2008. In order to avoid the impact of corporate events such as mergers or corporate restructuring, we continue to eliminate firm-years with negative or missing sales, cash, assets, with cash-to-asset ratios greater than one or with sales or asset growth larger than 100% (Almeida, Campello, and Weisbach, 2004; Acharya, Almeida, and Campello, 2007; Fresard, 2010). We further set negative book equity values equal to zero.

Shelf registration, while allowing firms to issue capital almost without time delay,

[8] Firms' are assumed to have an individually optimal leverage level which they track by adjusting their capital structure. Hugonnier, Malamud, and Morellec (2010) show that in the presence of transaction cost, firms will not instantaneously issue or buy back capital to keep their leverage constant at its optimal level. Instead, adjustments take place only when the deviation from the target leverage exceeds a certain threshold. Any firm issuing capital therefore has exactly one choice of security type that will bring it closer to its optimal leverage while the alternative choice will cause an even greater deviation from target leverage.

still forces them to signal to the public that they anticipate having to raise new capital in the foreseeable future. One of the most salient reasons for firms to issue in private markets rather than to use shelf registration under Rule 415, is to avoid sending this signal and thus potentially revealing negative inside information on future cash flows. Accordingly, we eliminate observations for which this argument would not hold, such as debt issues by credit agencies (Fannie Mae, Freddie Mac, or Farmer Mac), issues of structured products, equity issues by university trusts, IPOs and pure secondary offerings.

The SDC dataset contains registrations as well as actual issues, thus potentially double counting issues. In order to only take into account actual issues, we only keep those observations for which the SDC variable marketplace is U.S. Public or U.S. Private and eliminate all observations which only constitute registrations as well as those observations for which no marketplace information is available.

Now that we have identified actual issues which were conducted by suitable issuers, we proceed with constructing a reliable shelf indicator in order to sort the observations into three categories; public non-shelf, public shelf, and private placement. Private placements are indicated by the marketplace being U.S. private. Public observations are categorized as public shelf if they were issued under Rule 415 whereas they are categorized under public non-shelf if they have not been issued under Rule 415. Further, we use the variable *masterdealtype* to categorize issues into specific asset classes. For debt issues, the asset classes are debt and preferred shares; for equity issues we categorize into convertible debt, convertible preferred debt, and common stock. To avoid inconsistencies, we drop observations which *masterdealtype* identifies as ADRs, ADSs, beneficial institutions, capital shares, limited liability institutions, partnerships, warrants, income dependent securities, or equity secured units. Finally we use credit rating information by S&P and Moody's to categorize debt issues into three types; no rating, investment grade, and speculative. Observations with a fiscal year ending in 2005 or earlier are defined as pre-reform while observations with a fiscal year ending in 2006 or later are defined as "post-reform".

4.2 Public Float Data

For the remaining sample firms, we collect public float data. As public float does not take into account shares held by mutual funds or large shareholders, using data on shares outstanding or market capitalization is unfortunately not feasible. But the Sarbanes-Oxley Act of 2002, albeit leaving some leeway with regards to the definition of what a large shareholder is, requires firms to report public float data in the header of their annual 10-K filings (Iliev, 2010; Gao, Wu, and Zimmerman, 2009). Firms by convention report their public float on the last trading day of the 2nd fiscal quarter and we follow Kulak (2011) in calculating firms' public float throughout the fiscal year by scaling this value by changes in share prices [9].

To collect public float data from firms' 10-K, we link firm-year observations from the Compustat dataset to the corresponding electronic 10-K filings which we download from the SEC's EDGAR database. We subsequently retrieve the public flat information, using an automated search algorithm which follows a two stage approach identifying the relevant paragraph in the 10-K filing and subsequently extracts the public float value from within that paragraph [10]. Following Iliev (2010) and Kulak (2011), we find that some firms report public float values that do not correspond to the fiscal end of the 2nd quarter. This does not inhibit a consistent estimation of year round public float values but we still eliminate these observations from the sample to avoid confusing public float and market capitalization date, which often are reported in the same paragraph and in proximity to the fiscal quarter end [11].

[9] This approximation is exactly precise as long as no SEOs take place and large shareholders do not change.

[10] The search algorithm (Kulak, 2011) identifies the relevant paragraph within the header of firms' 10-K filings, running a search query for "aggregate market value" and "non-affiliates" as well as common variations of respective values (i.e. missing spaces, missing dash, or missing words). The algorithm extracts all dollar and date values from the identified public float paragraph and in case of multiple dollar values matches each dollar values with the corresponding date based on order and distance in the paragraph. We manually retrieved missing values or values for which the search algorithm could not unequivocally match date and dollar value.

[11] Restricting the sample to public float values from the second fiscal quarter is inevitable because some firms also report their market capitalization within the same 10-K paragraph, typically as of a date close to their fiscal year end. As firms' market capitalization is by definition larger than or equal to the public float, the restriction to observations from the second fiscal quarter thus avoids falsely classifying firms that ought to be in the control group into the treated group. Due to the freedom in

We can locate 10-K filings for 94% of the firms in the post 2002 Compustat sample. Out of respective filings, the text search algorithm successfully identifies public float values for 91% of all firm years, 83% of which refer to fiscal 2nd quarter ends. While the data loss attributed to the search of public float values is significant, it is justified considering that public float of the average (median) firm is 81%(88%) of its market capitalization with a standard deviation of respective ratio of 25%. Most striking, the lowest quartile of firms in the sample has a public float of less than 66% of its market capitalization. As such, it is crucial to attribute firms to treatment- and control-group based on public float rather than market capitalization (or a fraction thereof) as otherwise we would falsely classify a number of control group firms into the treatment group and hitherto dilute the estimate of the effect of the Reform.

4.3 Control Variables

Our sample comprises of three major security classes; Common Equity (1200 observations), Convertible Debt (665 observations), and Straight Debt (1111 observations). The application of the afore described sample selection criteria led to a notable reduction in sample size. To cleanly identify the effect of the Reform, we further prefer to look at a tight time window around the Reform's inception date.

As debt and equity issues differ in aspects as salient as the way their values respond to changes in volatility or uncertainty, we must suspect that the coefficients of control variables such as LEVERAGE differ significantly across both security types, so that coefficients of a joint regression would likely be a meaningless average.

We therefore opt to separately test the Reform's effect on private placements of equity. We jointly test the Reform's causal effect on debt and convertible debt issues, as they exhibit a sufficient level of similarity. To allow for some variation, we introduce

wording and ordering on content in 10-K filings, this is the only way to avoid misclassification without manually compiling the 10-K filings in question. we consider float values to belong to a firm's second fiscal quarter if the stated calculation date lies within 30 calendar days of the theoretical fiscal end of the 2nd quarter or, in case no date is reported, if the firm explicitly states that the value refers to its second fiscal quarter end. Float values are considered data errors if they deviate from a firm's Compustat fiscal year end market capitalization by a factor of 30 or more. This eliminates between 6 and 10 observations per calendar year

the dummy variable CONVERTIBLE.

Even though we estimate the treatment effect of the Reform on debt- and equity issues, we focus our analysis on the Reform's effect on debt issues for two reasons. First, it was the migration of debt and convertible debt to private markets that spurred the SEC to enact the 2005 Security Offerings Reform. Second, empirical evidence suggests that due to the information asymmetries inherent to equity, a reduction in regulatory delay comes at the cost of additional underpricing (Gustafson, 2012) while there are no additional costs for debt issues (Fenn, 2000).

In this paper we argue that time delay is the main factor which induces firms to issue on private markets rather than on public markets. Theoretical and empirical papers show that time delay in issuing capital is associated with firms holding larger reserves of cash and short-term investments (Baskin, 1987; Hugonnier, Malamud, and Morellec, 2010; Kulak, 2011). Conversing the argument, firms in command of large liquidity reserves are less likely to care about time delay of issuance. We therefore control for firms' liquidity by determining their cash and short-term investments as a fraction of book assets (CASH).

The treatment group dummy variable D_{Treat} constitutes an imprecise measure of size, as the divergence between public float and market capitalization may vary depending on ownership structure. However, large firms typically enjoy facilitated access to capital markets for a number of reason such as lower cash flow volatility, lower transaction cost and less information asymmetries due to better analyst coverage. As such we control for size by using the logarithm of total book assets (LN(ASSETS)) as a control variable.

Alternative explanations for firms' issuing choice include the lender specialization hypothesis (Huang and Ramirez, 2010). It argues that for firms which exhibit large information asymmetries it is optimal to opt for private placement to reduce ownership dispersement and free-riding on supervision and governance. Information asymmetries tend to be most severe for small or young firms which are complex to value and offer little in the way of collateral. To control for factors which proxy asymmetric

information, control for age. The control variable AGE describes how many years the issuing firm's oldest CRSP records data back at the moment of issuance. As the oldest CRSP records data back to 1960, AGE does not allow differentiating across mature firms but reliably identifies very young firms, which suffices in this context. Similarly, the share of book assets which is intangible (INTANGIBLES), the share of market value that is intangible and cannot be collateralised (TOBINS Q) and the pace at which depreciation and amortization cause the asset value to decline (D&A/ASSETS) indicate that a firm is potentially complex to value and are negatively related with the amount of collateral it can offer.

Yet another explanation, the inadequate disclosure hypothesis, argues that in the absence of registration there will be less disclosure and reduced issuance time leaves investors less time for due diligence. In line with this argument, Chaplinsky and Ramchand (1998) find that investors require significant premia for issues on the Rule 144A market [12] while Fenn (2000) suggests that after controlling for credit rating, the premia on 144A debt disappeared shortly after the inception of the 144A market [13]. Following Fenn (2000) we control for credit rating by introducing a dummy variable indicating whether firms had an investment-grade rating at the time of issuance (IG RATING) [14]. There is no credit rating information available for almost half of the observations in the complete sample. As some of the missing observations may correspond to firms without rating, which are likely to be small and risky, missing observations will likely be more similar to speculative grade firms in terms of credit quality. Therefore the IG RATING dummy will likely be less biased than a speculative rating dummy in estimating the effect of rating quality on placement choice. we further control for firms' leverage. LEVERAGE is the share of book assets which are financed by eq-

[12]Chaplinsky and Ramchand (1998) use data from 1996 and find that premia on domestic 144A issues initially where a whopping 179bp and later declined to 109bp in 1996.

[13]There is indeed little evidence that disclosure was a big concern for investors. Press accounts document mixed investor sentiment on the significance of disclosure and due diligence issues (Investment Dealers' Digest, 1997). Further, if private issuance under Rule 144A exposed investors to information asymmetries, this should be reflected in the premia paid on Rule 144A debt vis-Ã -vis publicly registered debt.

[14]For securities which are only rated by either Moodys or S&P, we take the available rating. Whenever Moodys and S&P disagree, we apply the better rating.

uity. To avoid further reducing the sample size and systematically eliminating issues of distressed firms, negative values of book equity are set to zero.

4.4 Descriptive Statistics

Table 1 reports summary statistics on issues and issuing firms in the 2003 to 2008 period. After applying the selection criteria, a total of 2969 capital issues enter the analysis. The table shows that 46.23% of the observations in the sample are SEOs. In the treatment group with firms with a public float larger than \$700M, the share of equity issues is only 16.51% while it is 64.78% in the control group of issues by firms with less than \$700M public float. Further, 19.79% of all issues are by firms with investment grade ratings. Among treated (=large) firms, this ratio is 39.49% while a negligible 0.68% of the control (=small) firms have an investment rating. This corresponds to the notion that small and risky firms are less leveraged and borrow from banks or raise equity whereas larger and less risky firms can take on more leverage and enjoy facilitated access to capital markets. The mean (median) age is 21 (14) years in the complete sample. Treated firms are significantly older than control firms with an average age of 29 years as opposed to 13 years. The average (median) firm had a public float of \$8.7BN (\$0.7BN) which is slightly more than the mean (median) market capitalization of \$9.16BN (\$0.7BN).

[INSERT TABLE 1 HERE]

In choosing the length of the period to be analysed around the Reform, there are two confounding factors to be traded off. Longer periods will increase the number of observations and render results statistically more significant while shorter periods provide for a cleaner identification and fewer unrelated events will be picked up by the analysis. As a compromise we include the period from 2005-2006 in the analysis, thus the year preceding and following the Reform. Table 2 reports descriptive statistics on the two sub-samples of the overall 2005-2006 sample which we use as treatment

and control group. The treatment group is restricted to firms with public float values >\$700M at the end of the fiscal quarter preceding the issue. The control sample is restricted to firms with public float values <\$700M at the end of the fiscal quarter preceding the issue. The last two columns of table 2 report whether the differences in mean (distribution) are significant for all variables. In line with theory treated firms hold relatively smaller cash balances than control firms (Δ mean -8.47%). Further, a larger share of their assets is intangible (δ mean 3.41%), their book leverage is higher on average (Δ mean 2.06%), and they are older (Δ mean 13).

[INSERT TABLE 2 HERE]

Figure 2 is a discontinuity plot which illustrates how firms' propensity to issue straight and convertible debt on private markets decreases with size (public float) in the pre-treatment period. After the Reform took effect, a significantly smaller fraction of treated issues takes place in private markets. Due to the small number of observations with large public float(\$2000M), those points are more likely to be outliers.

[INSERT FIGURE 2 HERE]

Figure 3 depicts the frequency of straight debt and convertible debt offerings, separately for firms exceeding (treated) and firms falling short (control) of the \$700M public float cut-off stipulated by the SEC. We can see that the absolute amount of offerings of treated firms stayed roughly constant throughout the sample period. Control firms' offerings peaked in late 2003 but stayed at a relatively constant level from 2004 until 2008. Control firms' share of private offerings was highest in 2003 and the second half of 2008, thus periods characterized by the recession following the internet bubble and the subprime bubble respectively. More importantly, and in line with our predictions, control firms' share of public offerings exhibits only little variation between 2004 and

2007. Treated firms' share of private debt issues is constantly at slightly above 50% before the Reform, and declines to about 30% at the exact moment the Reform is incepted.

[INSERT FIGURE 3 HERE]

5 Results

5.1 WKSI Status and Placement Choice

In order to estimate the effect of the Reform on WKSI's inclination to issue capital on private markets, we estimate a fixed effects difference-in-difference Equation (1) for the core control- and treatment samples. Table 4 reports fixed effects difference-in-difference results based on a series of specifications that include different sets of control variables which control for the confounding effects described in section 3.2. To capture different intercepts across industries, we use industry fixed effects and identify industries on a 2 digit SIC code level. Further the reported t-statistics are robust Huber-White standard errors.

Table 4, Model 1 presents the baseline difference-in-difference mode without industry fixed effects. D_{Treat} estimates the effect of having a public float larger than \$700M on firms' likelihood to issue on private markets. A_{Post} estimates the time effect on firms' inclination to issue on private markets. $D_{Treta} \times A_{POST}$ is the average treatment effect on the treated (ATET), and as such estimates the impact of the Reform on treated firms' likelihood to issue on private markets. The rather low R^2 of 18% as well as slightly lower statistical significance of the ATET are due to omitting the effects of leverage and investment grade rating. Controlling for these and leads to a slight improvement of the already significant ATET. Further adding industry fixed effects predictably allows for more variation in the intercepts and to a much higher R^2 to 45%. Further, we find that the effect of being a convertible issue is insignificant and

with -3% not economically large. This confirms our decision to jointly analyse issues of straight debt and convertible debt.

Overall, all models' estimates of the ATET are statistically significant at the one percent confidence level and economically large, suggesting that the Reform has led to a reduction in private placements among WKSIs in the range of 25%.

Note that the coefficients of control variables have the expected sign where they are statistically significant. Most importantly, the effect of being in the treatment sample almost disappears, indicating that it is not size LN(ASSETS) (strongly correlated with forcing variable) but leverage and investment rating (riskiness) that drive the likelihood of private issuance. This is important as it bolsters the identifying assumption of parallel time trends. Being investment grade, reducing the agency cost of debt, decreases the probability of private issuance by 50%. Confirming the empirical findings of Kulak (2011) and the predictions of Hugonnier, Malamud, and Morellec (2010), we find that firms with large cash holdings are more likely to issue on public markets. Also hinging on the agency cost of missing collateral, note that the higher the rate at which a firm's intangible and tangible assets are amortized or depreciated, the lower the value of collateral and accordingly the larger are the agency benefits of private placement.

[INSERT TABLE 3 HERE]

5.2 Robustness to Time Interval Length

The choice of the length of the time interval preceding and following the Reform is educated by the trade-off between two confounding considerations. A larger interval increases the sample size and improves the significance of the estimated coefficients. Shorter intervals allow for a cleaner identification of the Reform's effect on WKSIs' likelihood to opt for private placement, as our regressions are less likely to pick up unrelated events. The underlying assumption of difference-in-difference estimations is

that the time effect between the pre-event and post-event period is the same for treated and control firms. Further, we implicitly assume that the causal effect of belonging to the treatment group, i.e. having a public float larger than $700M, is the same in the pre- and post period. If any of these two assumptions is violated, $D_{Treta} \times A_{POST}$ fails to isolate the ATET, using the simple difference-in-difference model (equation 1).

In order to verify whether the difference-in-difference estimates are robust to variations in time-interval length, we repeat the regressions model 1 and model 6 in table 3 while using interval lengths of 1 year, 2 years, 3 years, and 4 years. All periods are symmetrically centred around the inception of the Reform. The results are shown in table 4.

[INSERT TABLE 4 HERE]

Note that the ATET is clearly negative and significant at the one percent confidence level for the baseline model as well as for the complete model. All other coefficients exhibit the same signs and similar t-scores for the different time interval lengths. Most notably, treatment dummy and time dummy are insignificant and have changing signs for the complete model, regardless of the time interval lengths chosen.

5.3 Placebo Analysis

The evidence thus far is fully consistent with a causal channel from the time delay in raising outside capital to firms opting for private placement. To reinforce this view, we further conduct a series of placebo regressions which test whether the convincing results obtained with time intervals centred around the Reform (2005-06) can be replicated for four non-reform periods.

Table 5 reports the results. For all four regressions we use the baseline regression model shown before (Table 3, model 1). Further, the interval length remains two years. For each regression the beginning and end date are shifted by six month, as is the hypothetical reform cut-off in the middle of both periods. The intervals are

overlapping, starting in 2003 and ending in 2008. In the middle in column 4 the results for the actual reform period are reported for comparison [15].

[INSERT TABLE 5 HERE]

The results diffuse concerns that changes in the frequency of private placements would differ systematically across treated and control firms in the same way they do around the reform date. The treatment effect for the period from Q3 2004 until Q2 2006 is significant with a coefficient roughly half as big as in the reform period. This is not surprising as the second half of the placebo post reform period still captures the actual reform. While the Reform's effect ($D_{Treat} \times A_{Post}$) is significant at the one percent confidence level for the period around the Reform, the, the ATET estimates for the placebo periods are all insignificant and their coefficients exhibit changes in signs.

Overall, the evidence from the placebo analysis unequivocally supports the difference-in-differences identifying assumption that treatment and control samples exhibit similar dynamics in the absence of external interventions.

5.4 Robustness of Model Specification

The difference-in-difference estimates of the ATET hinge on the identifying assumption that the ATET does not differ across units. We test whether this, admittedly quite restrictive assumption, fails by applying the model specified in equation 2 and 3. In table 6, we show the results from repeating the results from all 6 models in table 3, but interact leverage with the main specifications of the difference-in-difference model (Panel A) and by interacting all controls with the main specifications of the

[15] The 2004 placebo results may be affected by the Homeland Investments Act of 2004, which granted US firms with foreign operations a temporary tax break on repatriated earnings, thus temporarily reducing their need for external financing (Blouin and Krull, 2009; **?**). As we can reasonably assume that firms with foreign holdings are large on average, and as our analysis shows that large firms are more likely to rely on public financing channels, we might suspect a slight bias against the effect of the Reform

difference-in-difference model (Panel B). Finally, we repeat the simple difference-in-difference model with all configurations of control terms in table 3, but skip the fixed effects. (Panel C)

[INSERT TABLE 6 HERE]

Interacting leverage with the diff-in-diff model's main specification only very marginally increases R^2, which is not remarkable considering the larger number of variables. Most importantly, estimates of the ATET's coefficient remain unchanged, that is significant at 1% confidence. We conclude that our regression results are robust to ATET changing with leverage.

Interacting all control variables with the diff-in-diff model's main specification, we still obtain significant, albeit partly different, estimates. This confirms that our standard difference-in-difference model from equation 1 is correctly specified.

Finally, Panel C shows that omitting industry fixed effects estimates does not affect R^2 but leads to consistently lower, although still extremely significant, t-scores. This confirms our choice of a fixed effects model.

6 Conclusion

In this paper we empirically show that the time delay firms face in raising outside capital on public markets has a direct causal impact on the probability with which firms will opt for placements on private markets. We exploit the 2005 Security Offerings Reform as a quasi-natural experiment. Through the Reform, a subset of large U.S. firms with possessing a public float in excess of $700M is afforded the possibility to raise new outside capital from public markets without facing regulatory delay. Difference-in-difference estimates suggest a direct link between regulatory delay on public market and the share of private placements. Following the Reform, affected firms have reduced their share of private market placements by 25-30% in comparison

to unaffected control-group firms. The empirical findings lead to the conclusion that the 2005 Security Placement Reform has been a success as the SEC has achieved its purpose to contain the growth of private placements.

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A Logit difference-in-difference

This appendix provides details about the logit difference-in-difference regression procedure, and demonstrates that the results obtained by logit regression are by and large consistent with those obtained through OLS. We adapt the non-linear difference-in-difference approach of Puhani (2012) and chose notation consistent with Angrist and Pischke (2008).

A.1 Methodology

Consider a standard difference-in-difference model where Y^1 and Y^0 are two potential binary outcomes with and without treatment respectively. D is the treatment dummy and A is the post period dummy. TA is an interaction dummy for treated observations in the post period. X is a vector of covariates with the coefficients Θ. In a linear model, the treatment effect on the treated $\delta = \delta(D = 1, A = 1)$ is:

$$\begin{aligned} E[Y \mid D, A, X] &= DA \times [\delta + \gamma D + \lambda A + X\Theta] + (1 - DA) \times [\gamma D + \lambda A + X\Theta] \\ &= \gamma D + \lambda A + \delta DA + X\Theta \end{aligned} \tag{4}$$

Now consider a non-linear difference-in-difference model, where $\Phi(\bullet)$ be the conditional distribution function of a standard normal distribution. Puhani (2012) shows that this result applies to all non-linear models of parametric structure as $\Phi(\bullet)$ could be any non-linear but strictly monotone transformation function. As such, the standard logit difference-in-difference model is:

$$E[Y \mid D, A, X] = \Phi(\gamma D + \lambda A + \delta DA + \Theta) \tag{5}$$

and the treatment effect τ in a logit difference-in-difference model is equal to the cross-differences so that:

$$\begin{aligned}\tau(D=1,A=1,X) &= E[Y^1 \mid D=1,A=1,X] - E[Y^0 \mid D=1,A=1,X] \\ &= \Phi(\gamma+\lambda+\delta+X\Theta) - \Phi(\gamma+\lambda+X\Theta) \qquad (6)\end{aligned}$$

Therefore the treatment effect τ is zero ***iff*** the coefficient δ of the interaction term DA is zero. As Φ is strictly monotone, the sign of δ is equal to the sign of the treatment effect τ. Ai and Norton (2003) discuss that the treatment effect is the marginal change of the coefficient of the interaction term δ, allowing locally for a linear interpretation of δ when keeping $\sigma, \lambda,$ and Θ constant. We use the Stata command "margin dydx" to find the local impact of a covariate on the dependent variable by determining the dependent variables first derivative w.r.t. the covariate. In order to facilitate interpretation of the regression outputs, all tables display such local linear estimates of the coefficients. As the impact is contingent on the values the remaining covariates take, we set all remaining covariates equal to the sample mean of the sub-sample the regression uses.

We estimate the standard logit difference-in-difference model

$$\begin{aligned}Prob[Y_i=1] &= \alpha+\gamma D_i+\lambda A_i+\delta(D_i\times A_i)+X_i\Theta+\epsilon_i \\ &= \alpha+\gamma 1_{[Treat],i}+\lambda 1_{[Post],i}+\delta\left(1_{[Post],i}\times 1_{[Treat],i}\right)+X_i\Theta+\epsilon_i \qquad (7)\end{aligned}$$

where Y_i is an issues conditional probability to be conducted as a private placement, D_i is an indicator variable that defines whether an issuing firm was affected by the Reform (discussed below in more detail) and A_i is an indicator variable defining whether a security placement took place in the period after the Reform. DA_i is an indicator variable that defines placements which are conducted by treated firms after the Reform. Finally, X_i is a vector of control variables. δ is the salient coefficient of equation (x) as it represents the Reform's average treatment effect on the treated (ATET).

A.2 Logit Results

The results depicted in table 9 show, that estimates of the average treatment effect which are obtained using a logit difference-in-difference model are in the same ballpark as the estimates obtained by ordinary least squares (compare table 8). In a joint estimate for common equity, debt and convertible debt, a logit model without controls (table 9, model 1) predicts a 15% decrease in private placements (table 8, model 4) while OLS predicts an 18% decline. A logit model with all controls (table 9, model 5) predicts a 29% decrease, while an OLS model with similar controls predicts a 21.5% decline. As mentioned before, the logit model only yields local linear estimates, so that a marginal interpretation of the intercept is not possible.

[INSERT TABLE 8 HERE]

B Comparison Across Security Classes

Table 9 illustrates average characteristics of firms in our sample, conditional on the security types they issue and the issue channel they opt for. All proxies of size, such as public float, book assets and market capitalization data, are lowest for common stock issues, a bit higher for convertible debt issues and by far the highest for straight debt issues. Within each security class, private issuers tend to be smaller than public issuers, but more strikingly, issuers of public stock are still smaller on average than issuers of private convertible debt, and issuers of public convertible debt are on average smaller than issuers of private straight debt. This corroborates our assumption that firms, based on their characteristics, primarily chose a suitable security class, and only then decide on private or public issues. A similar pattern also shows for leverage, age, cash holdings, and Tobin's Q.

[INSERT TABLE 9 HERE]

Figures 4-7 confirm that small firms are especially likely to issue debt (many more control firms than treatment firms). Further, as stated in the literature (Fenn, 2000) figure 5 and 6 confirm that small firms (control) place in the range from 80-100% of their straight debt and convertible debt issues on private markets. For large (treated) firms, the share of private debt issues sharply declined immediately following the Reform.

For common stock, we see no clear trend for small (control) firms. Given the small number of treated equity issuers, and most notably the 2 year period around the Reform with no private equity issues by treated firms, no prediction can be made.

[INSERT FIGURE 4 HERE]

[INSERT FIGURE 5 HERE]

[INSERT FIGURE 6 HERE]

[INSERT FIGURE 7 HERE]

Table 1: **Descriptive Statistics for the entire 2003-08 Sample.**
The table reports descriptive statistics on key firm level variables for issues of debt, equity and convertible securities in the period from 2003 - 2008. Only issues that pass the sample criteria and that have non-missing values for FLOAT are retained in the sample. FLOAT is the amount of common equity outstanding and held by non-affiliates at the end of the fiscal quarter preceding the capital issue, as reported in the firm's Annual 10-K report. PRIVATE is a dummy indicating whether an issue took place on a private market. MKTCAP is the market value of the common stock outstanding, including the shares held by affiliates. LEVERAGE is the share of book assets which are debt financed, so that an all debt financed firm gets a value of 1 and an all equity financed firm gets a value of 0. Negative book equity is set to zero. IG RATING is a dummy indicating whether the issuer had an S&P investment grade rating at the time of issuance. ASSETS are total book assets held by the issuing firm at the time of issuance. CASH are the issuing firm's cash and short-term investments divided by assets, held at issuance. AGE describes how many years the issuing firm's earliest CRSP records date back at the moment of issuance. TOBINS Q is proxied as the sum of book debt and market equity, divided by book assets. $D\&A/ASSETS$ divides depreciation and amortization cost from the income statement by assets at the time of issuance. INTANGIBLES gives the proportion of book assets which are intangible and is approximated as $[Assets] - [CurrentAssets] - [PP\&E]/[Assets]$.

	All Firms						Pub. Float $\geq \$700M$			(Pub. Float $\geq \$700M$)		
Variable	Mean	SD	P25	Median	P75	N	Mean	Median	N	Mean	Median	N
PRIVATE	0.54	0.50	0.00	1.00	1.00	2969	0.42	0.00	1506	0.66	1.00	1463
FLOAT ($M)	8750	29089	182	727	3486	2969	17028	3440	1508	231	180	1468
MKTCAP ($M)	9174	31326	186	701	3204	2969	17821	3136	1508	269	184	1468
LEVERAGE (%)	56.05	23.75	40.39	57.03	72.31	2969	61.14	60.59	1508	50.87	50.01	1468
IG RATING	0.19	0.39	0.00	0.00	0.00	2969	37.47	0.00	1508	0.68	0.00	1468
ASSETS ($M)	8030	33528	101	717	3882	2969	15545	3624	1508	307	102	1468
CASH (%)	22.64	27.31	2.81	9.67	34.54	2960	12.24	5.32	1507	33.31	22.60	1468
AGE (Years)	21	17	8	14	32	2969	29	24	1506	13	10	1463
TOBINS Q	2.51	2.28	1.28	1.77	2.82	2969	2.04	1.66	1505	3.01	1.99	1460
D&A / ASSETS (%)	4.80	4.30	2.37	3.80	5.83	2960	4.28	3.66	1505	5.32	4.05	1461
INTANGIBLES (%)	23.79	20.49	5.71	18.25	38.04	2858	27.76	24.89	1420	19.89	13.04	1442

Table 2: **Descriptive Statistics for the 2005-06 Core Sample.**
The table reports means and medians for the core treatment and control samples. The treatment sample comprises of debt issues, straight and convertible, made during 2005 and 2006 by firms which had a public float larger than $700M at the end of the fiscal quarter preceding the capital issue. The control sample comprises of all 2005 and 2006 issues by firms which had a public float smaller than $700M at the end of the preceding fiscal quarter. The last two columns reports differences between the variables' means and medians between control and treatment group. Values in brackets below the mean differences represent t-statistics of the H:0 of equality of means. Values below median differences represent z-statistics of a Wilcoxon rank-sum test of the equality of distributions. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively
.

	Treated (Pub. Float $\geq \$700M$)			Control (Pub. Float $\geq \$700M$)			Treated - Control	
Variable	Mean	Median	N	Mean	Median	N	Mean	Median
PRIVATE	0.51	1.00	363	0.95	1.00	122	-0.44 ***	0.00 ***
							(13.44)	(8.68)
FLOAT ($M)	15273	3704	363	294	256	122	14979 ***	3448 ***
							(-10.07)	(-16.53)
MKTCAP ($M)	16872	3435	363	435	289	122	16437 ***	3145 ***
							(-8.59)	(-15.55)
LEVERAGE (%)	59.71	58.91	363	57.65	55.02	122	2.06	3.89
							(-0.88)	(-1.40)
IG RATING	0.40	0.00	363	0.03	0.00	122	0.37 ***	0.00 ***
							(-12.14)	(-7.63)
ASSETS ($M)	14089	3892	363	500	251	122	13590 ***	3641 ***
							(-9.40)	(-14.70)
CASH (%)	11.13	5.69	362	19.60	8.69	122	-8.47 ***	-3.00 ***
							(3.54)	(2.60)
AGE (Years)	29	25	363	16	13	122	13 ***	13 ***
							(-9.18)	(-7.56)
TOBINS Q	1.94	1.61	362	2.81	1.76	122	-0.87 **	-0.15
							(2.57)	(0.71)
D&A / ASSETS (%)	3.81	3.41	363	5.03	3.94	122	-1.22 ***	-0.52 **
							(2.92)	(2.37)
INTANGIBLES (%)	29.91	28.65	330	26.50	21.37	118	3.41	7.28 *
							(-1.43)	(-1.96)

Table 3: **Descriptive Statistics by Security Type.**
The table reports means, grouped by all [issue channels × security] combinations, available to issuers, providing a *prima facie* look at possible issuer self-selection. Panel A reports means for the entire sample. Panel B reports means for the 2005-2006 Core Sample. IG RATING is only available for debt issues and as such not reported for other security types. The last column for comparison reports means of the complete sample

Panel A: Complete Sample

	Common Stock		Convertible		Debt		All
	Private	Public	Private	Public	Private	Public	
FLOAT ($M)	236	2207	2401	3271	5891	32918	8750
MKTCAP ($M)	242	2145	2368	3414	6151	34881	9174
LEVERAGE (%)	46.02	52.25	54.14	63.03	62.86	64.07	56.06
IG RATING					0.17	0.82	0.19
ASSETS ($M)	218	3340	2383	4860	6491	27327	8030
CASH (%)	44.84	30.78	25.79	16.02	6.43	6.30	22.67
AGE (Years)	11	15	15	22	24	39	21
TOBINS Q	3.86	2.78	2.43	2.12	1.65	1.95	2.51
D&A / ASSETS (%)	5.54	4.68	5.00	5.37	4.81	4.01	4.80
INTANGIBLES (%)	17.42	18.11	27.22	19.81	27.18	30.70	23.92
N treated	17	232	275	61	344	577	1506
N control	489	462	304	18	170	20	1463

Panel B: Core Sample

	Common Stock		Convertible		Debt		All
	Private	Public	Private	Public	Private	Public	
FLOAT ($M)	140	778	3796	2580	6112	24832	6825
MKTCAP ($M)	154	782	3583	2544	6476	28360	7533
LEVERAGE (%)	43.87	51.83	52.33	67.19	59.70	63.38	54.85
IG RATING					0.16	0.76	0.18
ASSETS ($M)	163	844	1840	3792	5256	24685	6369
CASH (%)	46.60	33.32	27.89	20.99	6.91	6.30	23.85
AGE (Years)	12	13	14	21	26	37	20
TOBINS Q	3.85	2.90	3.04	2.09	1.78	1.80	2.62
D&A / ASSETS (%)	5.43	4.54	4.12	5.17	4.53	3.55	4.43
INTANGIBLES (%)	16.02	16.89	27.82	20.42	27.74	32.92	23.77
N treated	1	75	69	20	116	158	439
N control	126	160	68	3	48	3	408

Table 4: **The Effect of WKSI Status on likelihood of private placement.** The table shows difference-in-difference estimates of the effect of WKSI status on firms' placement choice (ATET). The sample comprises of debt issues - straight and convertible - in 2005 and 2006. The dependent variable in all regressions is a dummy indicating that an issue was a private placement. D_{Treat} is a dummy variable indicating whether the issuing firm was assigned WKSI status based on its public float (>$700M) at the end of the fiscal quarter preceding the capital issue. A_{Post} is a dummy variable indicating whether a capital issue occurred after the 2005 Reform, thus in 2006. $D_{Treat} \times A_{Post}$ is the interaction dummy variable indicating capital issues by WKSI firms after the 2005 Reform, and estimates the causal effect of the reform. LN(ASSETS) is the logarithm of book assets. Regressions (2)-(6) include fixed effects at the 2-digit SIC level. t-statistics (in parentheses) are based on heteroskedasticity-robust standard errors, clustered by industry. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
$D_{Treat} \times A_{Post}$	-0.2252 ***	-0.2384 ***	-0.2637 ***	-0.2368 ***	-0.2367 ***	-0.2645 ***
	(-3.57)	(-4.21)	(-4.40)	(-4.17)	(-4.08)	(-4.47)
D_{Treat}	-0.3322 ***	-0.0832 *	-0.0392	-0.0708	-0.0058	0.0107
	(-7.22)	(-1.67)	(-0.81)	(-1.42)	(-0.10)	(0.17)
A_{Post}	0.0400	0.0408	0.0567	0.0424	0.0431	0.0618
	(1.11)	(0.89)	(1.16)	(0.93)	(0.92)	(1.29)
LEVERAGE		-0.3927 ***	-0.3985 ***	-0.3819 ***	-0.3736 ***	-0.4015 ***
		(-5.30)	(-5.00)	(-4.81)	(-3.95)	(-3.74)
IG RATING		-0.4987 ***	-0.5328 ***	-0.4635 ***	-0.4475 ***	-0.4820 ***
		(-8.05)	(-9.70)	(-7.25)	(-6.99)	(-8.70)
LN (ASSETS)					-0.0369 **	-0.0218
					(-2.51)	(-1.36)
CONVERTIBLE					-0.0081	-0.0152
					(-0.12)	(-0.22)
CASH					-0.0780	-0.1881
					(-0.53)	(-0.88)
AGE (Years)				-0.0023		-0.0019
				(-1.29)		(-1.09)
TOBINS Q				0.0020		0.0052
				(0.41)		(0.54)
D&A / ASSETS			0.7384			0.4274
			(1.00)			(0.48)
INTANGIBLES			-0.0221			-0.0806
			(-0.24)			(-0.58)
Intercept	0.9367 ***	1.1417 ***	1.1093 ***	1.1683 ***	1.3506 ***	1.3217 ***
	(34.05)	(19.75)	(13.96)	(17.69)	(11.96)	(8.51)
Industry FE	No	Yes	Yes	Yes	Yes	Yes
R^2	0.18	0.45	0.47	0.45	0.46	0.48
N of Industries		52	51	52	52	51
N treat	363	363	330	362	362	328
N control	122	122	118	122	122	118

Table 5: **Robustness to Model Specification.**
The table repeats the difference-in-difference regressions from Table 4 with the exact same configurations of controls [(1) No Controls; (2) LEVERAGE, IG RATING; (3) LEVERAGE, IG RATING, D&A / ASSETS, INTANGIBLES; (4) LEVERAGE, IG RATING, AGE, TOBINS Q; (5) LEVERAGE, IG RATING, CONVERTIBLE, CASH; (6) All Controls], but varying model specifications. All regressions use debt issues from the 2005-2006 period. The dependent variable in all equations is a dummy indicating that an issue was a private placement. The table reports estimates of the causal effect of WKSI status on the likelihood of private issuance ($D_{Treat} \times A_{Post}$), but not the estimates of other controls. Panel A reports the results of the regressions in table 4 without fixed effects. Panel B reports difference-in-difference estimates with fixed effects and additionally estimating a full set of interactions of LEVERAGE and the difference-in-difference terms. Panel C reports difference-in-difference estimates with fixed effects and with a full set of interactions of difference-in-difference dummies with all ALL control variables used in the corresponding regressions from table 4. Where interacted controls are indicated, the control variable is replaced by 4 interaction terms. Term 1 is the controls' deviation from mean of treated in period of treatment is calculated. Then, (Term 2) this deviation is multiplied with the treatment dummy D, (Term 3) the post-treatment period dummy A, and (Term 4) their interaction dummy DA. t-statistics (in parentheses) are based on heteroskedasticity-robust standard errors, clustered by industry. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Difference-in-Difference estimates without FE						
$D_{Treat} \times A_{Post}$	-0.2252 ***	-0.1810 ***	-0.1768 ***	-0.1796 ***	-0.2047 ***	-0.2032 ***
	(-3.57)	(-3.10)	(-3.02)	(-3.06)	(-3.44)	(-3.37
Interacted Controls	No	No	No	No	No	No
Industry FE	No	No	No	No	No	No
R^2	0.18	0.45	0.45	0.46	0.48	0.49
N treat	363	363	330	362	362	328
N control	122	122	118	122	122	118
Panel B: Difference-in-Difference estimates with interacted leverage						
$D_{Treat}A_{Post}$		-0.2393 ***	-0.2619 ***	-0.2370 ***	-0.2395 ***	-0.2656 ***
		(-4.23)	(-4.39)	(-4.19)	(-4.18)	(-4.56
Interacted Controls		Leverage	Leverage	Leverage	Leverage	Leverage
Industry FE		Yes	Yes	Yes	Yes	Yes
R^2		0.46	0.49	0.46	0.47	0.50
N of Industries		52	52	52	52	51
N treat		363	330	362	362	328
N control		122	118	122	122	118
Panel C: Difference-in-Difference estimates with fully Interacted controls						
$D_{Treat} \times A_{Post}$		-0.2862	-0.3271 ***	-0.2655 ***	-0.2990 ***	-0.3255 ***
		(-0.29)	(-5.12)	(-3.05)	(-2.66)	(-3.10
Interacted Controls		All	All	All	All	All
Industry FE		Yes	Yes	Yes	Yes	Yes
R^2		0.47	0.51	0.47	0.48	0.52
N of Industries		52	51	52	52	51
N treat		363	330	362	362	328
N control		122	118	122.00	122	118

Table 6: **Variations in Time Interval Length.**
The table illustrates the robustness of a baseline difference-in-difference model without controls as well as a complete difference-in-difference model with all control variables to variations in the length of time-intervals. The first four columns correspond to the parsimonious model. The last four columns correspond to the complete model. Both equations are repeated four times with 1-year, 2-year, 3-year and 4-year intervals around the 2005 Reform. The regressions using 2-year intervals (column 2 and 6, table 4) are included for comparison. The dependent variable in all equations is a dummy indicating that an issue was a private placement. D_{TREAT} is a dummy variable indicating whether the issuing firm was assigned WKSI status based on its public float (>$700M) at the end of the fiscal quarter preceding the capital issue. A_{POST} is a dummy variable indicating whether a capital issue occurred after the 2005 Reform, thus in 2006. $D_{Treat} \times A_{POST}$ is the interaction dummy variable indicating capital issues by WKSI firms after the 2005 Reform, and estimates the average treatment effect on the treated (ATET) of the reform. LN(ASSETS) is the logarithm of book assets. *t*-statistics (in parentheses) are based on heteroskedasticity-robust standard errors, clustered by industry. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) 1Y	(2) 2Y	(3) 3Y	(4) 4Y	(5) 1Y	(6) 2Y	(7) 3Y	(8) 4Y
$D_{Treat} \times A_{Post}$	-0.3230 ***	-0.2920 ***	-0.2012 ***	-0.1752 ***	-0.2816 ***	-0.2645 ***	-0.2047 ***	-0.1406 ***
	(-3.60)	(-4.58)	(-2.90)	(-3.55)	(-2.89)	(-4.47)	(-3.51)	(-2.91)
D_{Treat}	-0.2384 ***	-0.2252 ***	-0.2561 ***	-0.2633 ***	-0.0491	0.0107	0.0181	0.0249
	(-3.47)	(-4.20)	(-5.32)	(-6.13)	(-0.44)	(0.17)	(0.41)	(0.84)
A_{Post}	0.1075 *	0.0627 **	0.0159	-0.0332	0.0403	0.0618	0.0078	-0.0413
	(1.66)	(2.00)	(0.5)	(-0.93)	(0.43)	(1.29)	(0.22)	(-1.21)
LEVERAGE					-0.6699 ***	-0.4015 ***	-0.2129 **	-0.1789 **
					(-3.84)	(-3.74)	(-2.17)	(-2.35)
IG RATING					-0.5379 ***	-0.4820 ***	-0.5018 ***	-0.5038 ***
					(-7.83)	(-8.70)	(-9.34)	(-11.43)
LN (ASSETS)					-0.0099	-0.0218	-0.0307 **	-0.0397 ***
					(-0.37)	(-1.36)	(-2.35)	(-3.56)
CONVERTIBLE					-0.0035	-0.0152	0.0276	-0.0035
					(-0.04)	(-0.22)	(0.53)	(0.08)
CASH					-0.2211	-0.1881	-0.0858	-0.0038
					(-0.98)	(-0.88)	(-0.56)	(-0.03)
AGE (Years)					-0.0002	-0.0019	-0.0009	-0.0012
					(-0.09)	(-1.09)	(-0.71)	(-0.92)
TOBINS Q					0.0247	0.0052	-0.0060	-0.0049
					(1.51)	(0.54)	(-0.55)	(-0.61)
D&A / ASSETS					0.7621	0.4274	-0.2837	-0.2047
					(0.65)	(0.48)	(-0.64)	(-0.58)
INTANGIBLES					-0.0744	-0.0806	-0.0125	0.0340
					(-0.27)	(-0.58)	(-0.11)	(0.33)
Intercept	0.8713 ***	0.8716 ***	0.8908 ***	0.8939 ***	1.3504 ***	1.3217 ***	1.2723 ***	1.2967 ***
	(18.61)	(25.83)	(30.60)	(34.99)	(5.33)	(8.51)	(11.28)	(15.39)
R^2	0.15	0.18	0.16	0.16	0.48	0.48	0.47	0.48
N of Industries	49	52	56	56	48	51	55	55
N treat	196	363	589	815	181	328	541	754
N control	53	122	212	295	51	118	206	285

Table 7: **Placebo Analysis.**
The table shows seven placebo regressions for different overlapping two-year periods from 2004-2007. The two-year period corresponding to each equation is reported at the top of each column. Results from the actual reform period from 2005-2006 are included in column 4 for comparison. The regression reports ATET estimates from the complete difference-in-difference model specified in the last column from table 4. The dependent variable in all equations is a dummy indicating that an issue was a private placement. D_{Treat} is a dummy variable indicating whether an issuing firm would have been assigned WKSI status at time of issue. Issues by firms with public float larger than \$700M are treated and firms with a public float smaller than \$700m are in the control group. A_{Post} corresponds to the post reform dummy variable from previous equations and for each equation divides the two-year sample period into a hypothetical pre- and post-reform period. $D_{Treat} \times A_{Post}$ illustrates ATET. t-statistics (in parentheses) are based on heteroskedasticity-robust standard errors, clustered by industry. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Period	Q3 2003 - Q2 2005	Q1 2004 - Q4 2005	Q3 2004 - Q2 2006	Q1 2005 - Q4 2006	Q3 2005 - Q2 2007	Q1 2006 - Q4 2007	Q3 2006 - Q2 2008
$D_{Treat} \times A_{Post}$	0.0614	-0.0042	-0.1669 ***	-0.2676 ***	-0.0121	0.1685	0.0646
	(1.39)	(-0.06)	(-3.08)	(-4.59)	(-0.23)	(1.66)	(0.93)
D_{Treat}	-0.0526	-0.0316	-0.0215	-0.0287	-0.2145 ***	-0.3240 ***	-0.2119 **
	(-1.64)	(-0.76)	(-0.53)	(-0.54)	(-3.82)	(-4.33)	(-2.46)
A_{Post}	0.0054	0.0091	0.0200	0.0608	-0.0409	-0.1412	-0.0620
	(0.20)	(0.19)	(0.52)	(1.27)	(-1.13)	(-2.10)	(-1.00)
Intercept	1.0284 ***	1.0081 ***	1.1593 ***	1.1997 ***	1.2077 ***	1.2469 ***	1.0897 ***
	(11.62)	(10.28)	(9.83)	(8.88)	(10.03)	(9.55)	(6.91)
Controls	Complete	Complete	Complete	Complete	Complete	Complete	Complete
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.4631	0.4291	0.4551	0.4715	0.4381	0.4597	0.4903
N treat	438	397	366	363	419	419	361
N control	259	196	155	122	110	99	90

Table 8: **Security Class Comparison.**
The table compares the effect of WKSI status on the likelihood of private placement across different security classes. Columns 1-4 correspond to the parsimonious model without controls (table 4, model 1) . Columns 5-8 correspond to the complete model (table 4, model 6). Both models are repeated for Common Equity, Convertible Debt, Straight Debt, and all issues together. The dependent variable in all equations is a dummy indicating that an issue was a private placement. D_{TREAT} is a dummy variable indicating whether the issuing firm was assigned WKSI status based on its public float (>\$700M) at the end of the fiscal quarter preceding the capital issue. A_{POST} is a dummy variable indicating whether a capital issue occurred after the 2005 Reform, thus in 2006. $D_{Treat} \times A_{POST}$ is the interaction dummy variable indicating capital issues by WKSI firms after the 2005 Reform, and estimates the average treatment effect on the treated (ATET) of the reform. LN(ASSETS) is the logarithm of book assets. t-statistics (in parentheses) are based on heteroskedasticity-robust standard errors, clustered by industry. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Common Equity	Convertible Debt	Straight Debt	All	Common Equity	Convertible Debt	Straight Debt	All
$D_{Treat} \times A_{Post}$	-0.0473	-0.1820 **	-0.3187 ***	-0.1843 ***	0.0384	-0.2318 ***	-0.1862 **	-0.2132 ***
	(-0.54)	(-2.46)	(-4.72)	(-2.60)	(0.38)	(-3.35)	(-2.08)	(-3.42)
D_{Treat}	-0.3791 ***	-0.0950	-0.3228 ***	-0.0413	-0.0974	-0.0674	-0.0010	0.0797
	(-7.19)	(-1.38)	(-4.23)	(-0.81)	(-1.41)	(-0.82)	(-0.01)	(1.53)
A_{Post}	0.0193	0.0568	0.0298	-0.0136	-0.0326	0.0769 *	-0.0332	0.0050
	(0.19)	(1.57)	(0.58)	(-0.20)	(-0.39)	(1.73)	(-0.45)	(0.09)
LEVERAGE					0.2114 ***	-0.3171 **	-0.3776 ***	-0.1581 **
					(2.74)	(-2.10)	(-3.18)	(-2.22)
IG RATING							-0.4170 ***	-0.2633 ***
							(-6.19)	(-4.68)
LN (ASSETS)					-0.1817 ***	-0.0012	-0.0594 ***	-0.0359
					(-4.92)	(-0.04)	(-2.68)	(-1.55)
CONVERTIBLE								0.4022 ***
								(10.43)
CASH					0.1661	-0.0766	0.1874	-0.1612
					(1.23)	(-0.49)	(0.46)	(-1.06)
AGE (Years)					0.0028	-0.0003	-0.0010	0.0030
					(1.26)	(-0.05)	(-0.58)	(1.16)
TOBINS Q					-0.0233	0.0104	-0.0071	0.0062
					(-1.40)	(1.10)	(-0.20)	(0.72)
D&A / ASSETS					0.4393	-1.6416	1.4056	0.7556
					(0.55)	(-1.47)	(1.32)	(1.06)
INTANGIBLES					0.2584 *	0.2159	-0.2017	0.1903
					(1.74)	(1.34)	(-1.01)	(1.36)
Intercept	0.4289 ***	0.9424 ***	0.8925 ***	0.5851 ***	1.0872 ***	1.1214 ***	1.5667 ***	0.6977 ***
	(9.38)	(27.87)	(15.72)	(15.62)	(4.07)	(5.44)	(6.86)	(3.90)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.13	0.09	0.19	0.04	0.29	0.18	0.49	0.23
N of Industries	41	31	49	56	39	31	48	55
N treat	75	89	274	438	69	85	243	397
N control	284	71	51	406	281	71	47	399

Table 9: **Logit Difference-in-Difference Regression.**
The table shows logit difference-in-difference estimates of the effect of WKSI status on firms' placement choice. The sample comprises of issues in 2005 and 2006. The dependent variable in all regressions is a dummy indicating that an issue was a private placement. D_{TREAT} is a dummy variable indicating whether the issuing firm was assigned WKSI status based on its public float (>$700M) at the end of the fiscal quarter preceding the capital issue. A_{POST} is a dummy variable indicating whether a capital issue occurred after the 2005 Reform, thus in 2006. $D_{TREAT} \times A_{POST}$ is the interaction dummy variable indicating capital issues by WKSI firms after the 2005 Reform, and estimates the causal effect of the reform. LN(ASSETS) is the logarithm of book assets. t-statistics (in parentheses) are based on heteroskedasticity-robust standard errors, clustered by industry. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
$D_{TREAT} \times A_{POST}$	-0.1497 **	-0.2239 ***	-0.2588 ***	-0.2895 ***	-0.2910 ***
	(-2.13)	(-2.87)	(-3.03)	(-3.07)	(-3.08)
D_{TREAT}	-0.0912 *	-0.6297 ***	-0.5036 ***	-0.1758	-0.4399
	(-1.88)	(-6.71)	(-5.2)	(-1.57)	(-1.62)
A_{POST}	-0.0242	0.0457	0.0440	0.0145	0.0136
	(-0.48)	(0.79)	(0.76)	(0.23)	(0.22)
EQUITY ISSUE (%)		-0.8145 ***	-0.9551 ***	-1.2012 ***	-1.2009 ***
		(-9.22)	(-10.42)	(-11.09)	(-11.03)
IG RATING (%)			-0.6600 ***	-0.5476 ***	-0.5662 ***
			(-9.8)	(-6.94)	(-6.75)
D&A/ASSETS (%)				0.5781	0.6732
				(0.96)	(1.05)
INTANGIBLES (%)				0.2093	0.2339 *
				(1.63)	(1.69)
CASH (%)					0.0594
					(0.54)
LN (ASSETS)				-0.1455 ***	-0.1447 ***
				(-6.78)	(-6.42)
LEVERAGE (%)				-0.1133	-0.1035
				(-1.09)	(-0.95)
AGE (Years)					0.0014
					(0.71)
Intercept	***	***	***	***	***
	(3.04)	(8.16)	(9.21)	(10.16)	(8.59)
Pseudo R^2	0.03	0.19	0.29	0.36	0.37
N treat	439	439	439	400	399
N control	411	411	411	403	403
Log Likelihood	-570.65	-477.85	-416.83	-353.56	-352.78

Figure 1: **Regulatory Delay for Offers of Additional Common Stock, 1990-1994.** This figure is reproduced from a 1996 report by the SEC Advisory Committee on the Capital Formation and Regulatory Processes (SEC, 1996). It illustrates the regulatory delay of SEOs during the priod from 1990-1994. 1453 filings underwent the SEC's preliminary review (light bars) and 633 filings underwent the SEC's detailed review (dark bars).

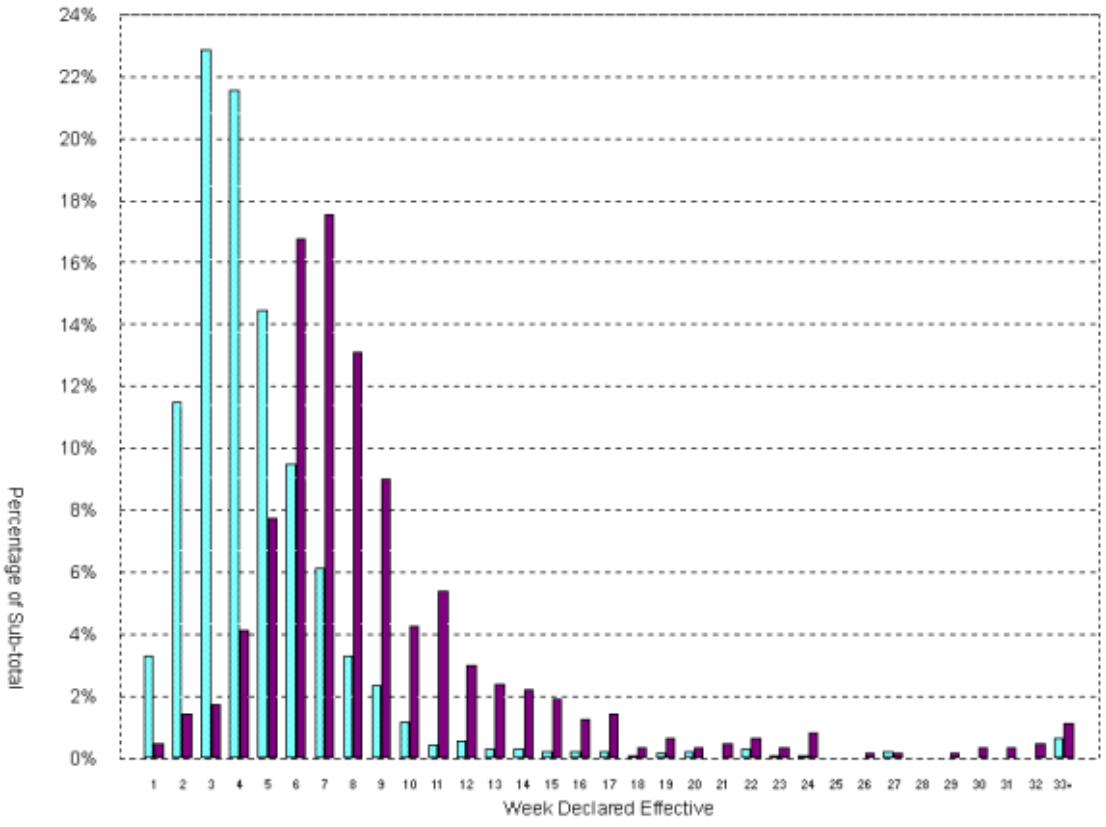
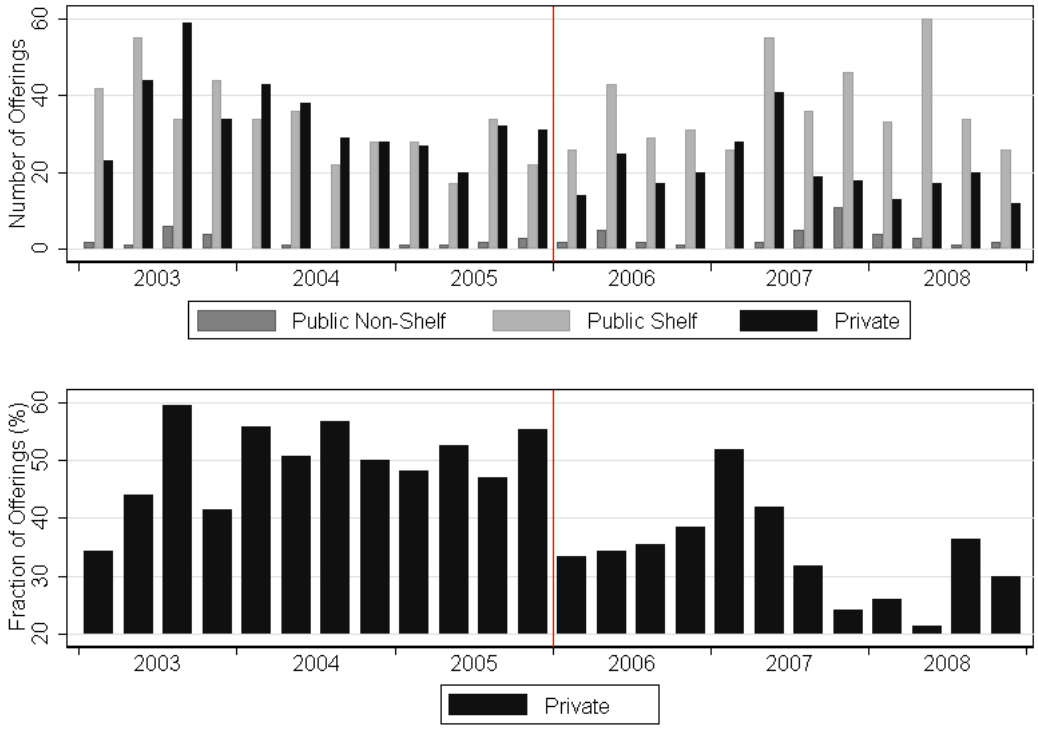


Figure 2: **Placement Choice by Treated and Control Firms** The figure displays the frequency of security offerings, separately for firms exceeding (treated) and for firms falling short (control) of the 700 million float cutoff. Each bar corresponds to the number of offerings in one quarter, where quarters are defined as three-months intervals ranging from December to February, March to May and so on (see text). Offerings are categorized into Private, Public Non-Shelf, and Public Shelf Offerings. Included are issues of common stock, debt, convertible debt, convertible and non-convertible preferred stock by public firms incorporated in the US. The frequency counts do not distinguish among security types.

Panel A: Treated Firms



Panel B: Control Firms

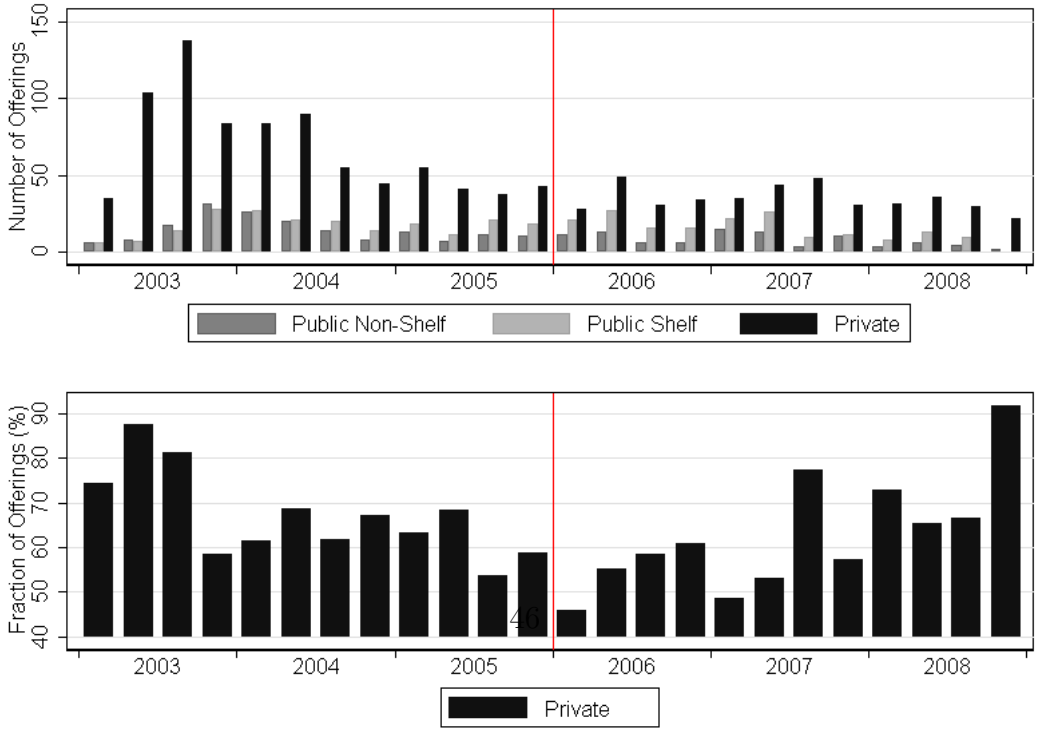
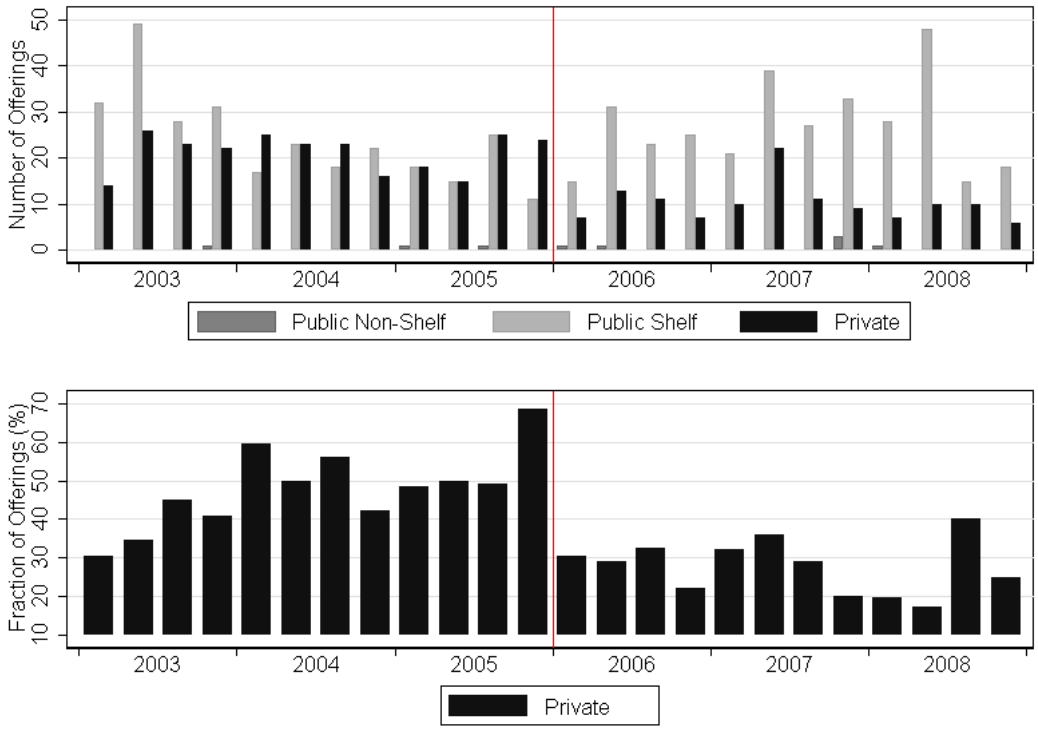


Figure 3: **Placement Choice of Debt** The figure displays the frequency of debt issues from 2003 to 2008, separately for firms exceeding (treated) and for firms falling short (control) of the 700 million float cutoff. Each bar corresponds to the number of offerings in one quarter, where quarters are defined as three-months intervals ranging from December to February, March to May and so on (see text). Offerings are categorized into Private, Public Non-Shelf, and Public Shelf Offerings.

Panel A: Treated Firms



Panel B: Control Firms

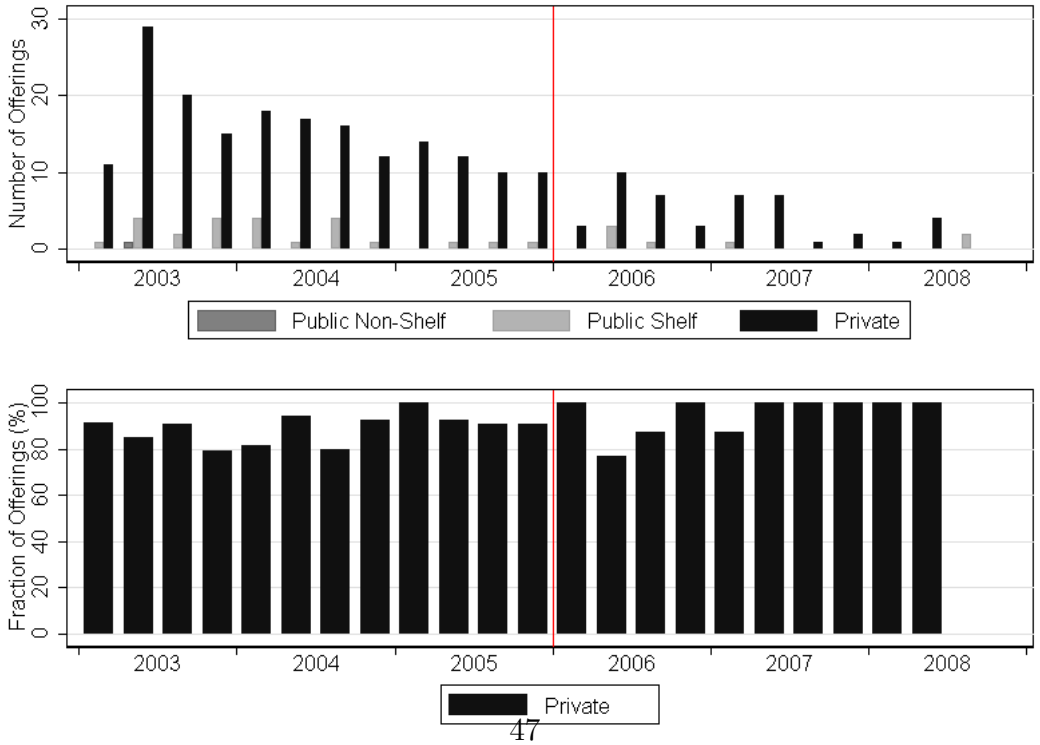
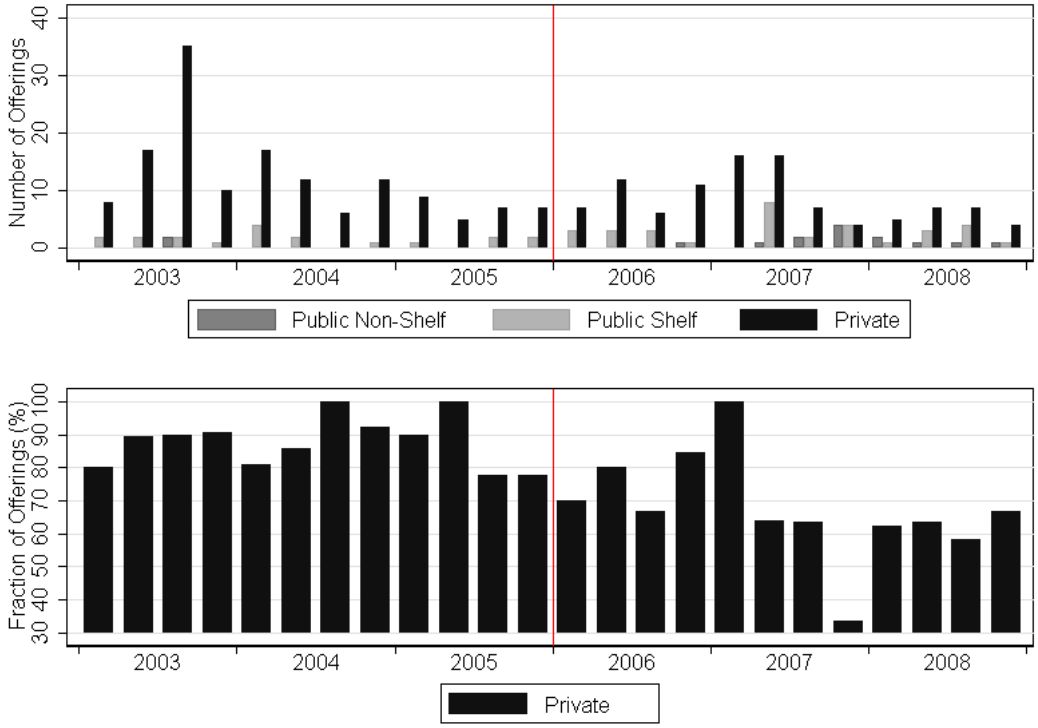


Figure 4: **Placement Choice of Convertible Securities** The figure displays the frequency of convertible debt and convertible preferred stock offerings from 2003 and 2008, separately for firms exceeding (treated) and for firms falling short (control) of the 700 million float cutoff. Each bar corresponds to the number of offerings in one quarter, where quarters are defined as three-months intervals ranging from December to February, March to May and so on (see text). Offerings are categorized into Private, Public Non-Shelf, and Public Shelf Offerings.

Panel A: Treated Firms



Panel B: Control Firms

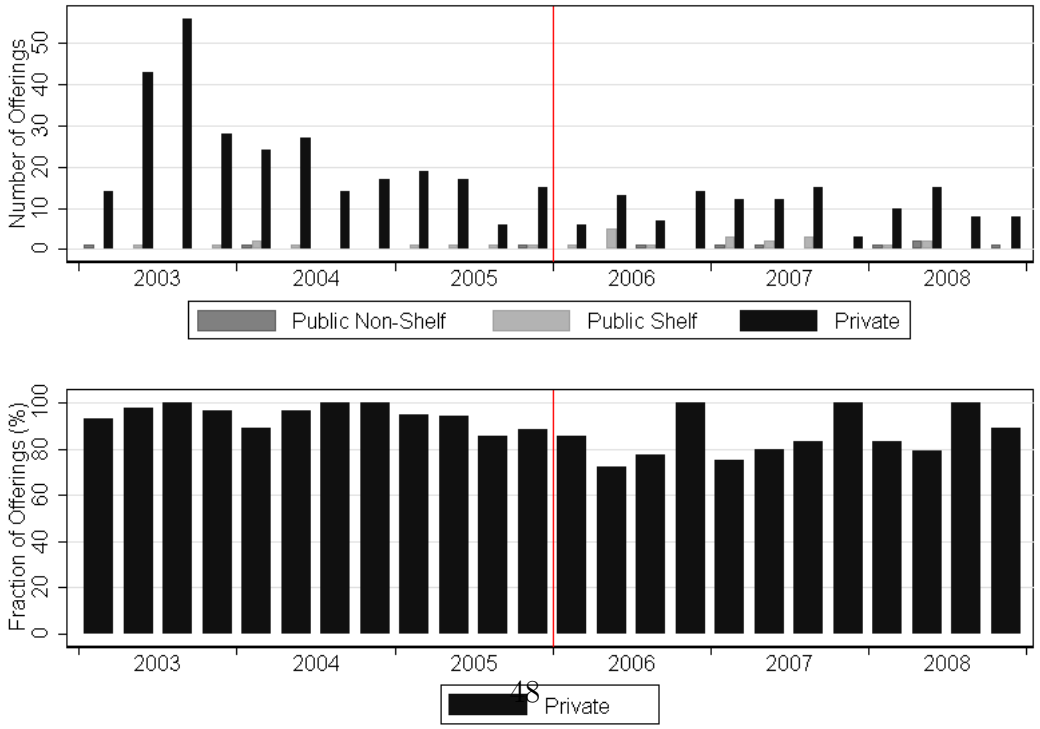
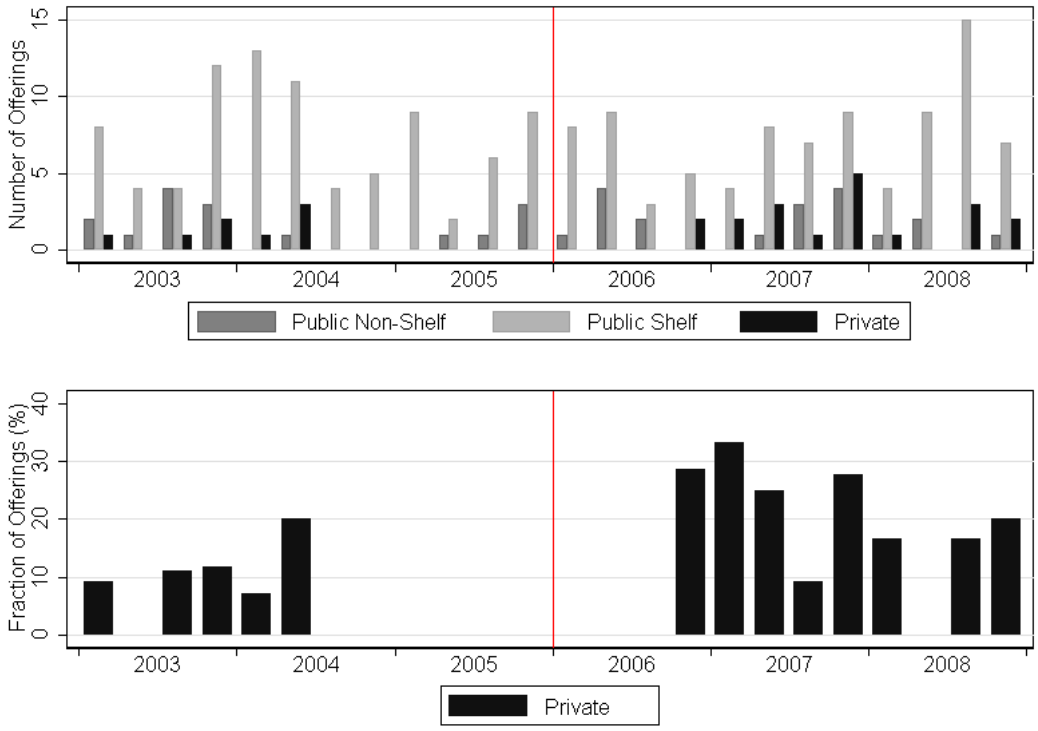


Figure 5: **Placement Choice of Common Stock** The figure displays the frequency of common stock offerings from 2003 to 2008, separately for firms exceeding (treated) and for firms falling short (control) of the 700 million float cutoff. Each bar corresponds to the number of offerings in one quarter, where quarters are defined as three-months intervals ranging from December to February, March to May and so on (see text). Offerings are categorized into Private, Public Non-Shelf, and Public Shelf Offerings.

Panel A: Treated Firms



Panel B: Control Firms

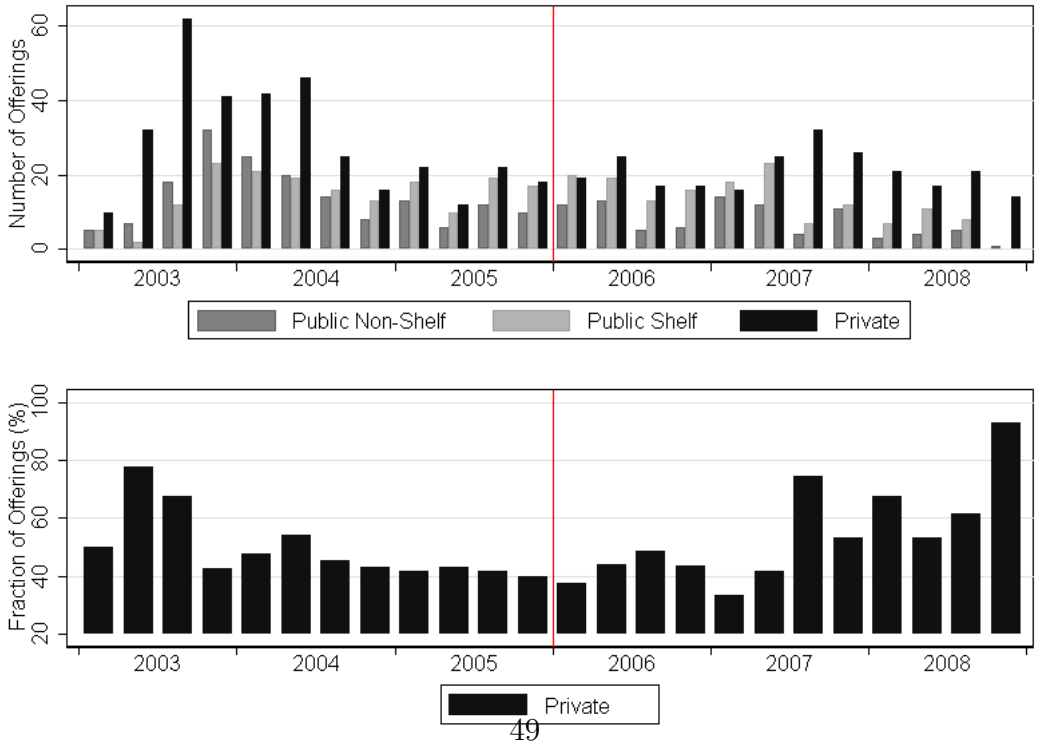


Figure 6: **Discontinuity Plot: Straight and Convertible Debt** The figure displays the fraction of private debt debt offerings (straight and convertible) in public float bins around the $700 million threshold. Each dot corresponds to a bin of $200 million in width.