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Equity financing incentive and corporate disclosure: new causal evidence from SEO deregulation

Jun Chen[1] · Ningzhong Li[2] · Xiaolu Zhou[3]

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Abstract

We provide new causal evidence for the impact of equity financing incentive on firms' voluntary disclosure decisions by exploring the 2008 seasoned equity offering deregulation, which exogenously facilitates small firms' access to public equity financing and increases their equity issuance incentives without changing their business and information environments. We argue that the heightened equity financing incentive due to the deregulation can motivate a firm to increase disclosures even in the period without actual equity issuance, because such disclosures, by signaling a commitment to disclosure, could reduce the cost of equity in case the firm issues equity in the future. Consistent with this argument, we find that, benchmarking against control firms that are not affected by the deregulation, an average treatment firm that is affected by the deregulation but does not issue equity provides more management earnings forecasts in the post-deregulation period. The effect is mainly driven by repeated forecasters and is more pronounced for firms with greater equity financing needs and firms with higher information asymmetry in the equity market.

Keywords Equity financing · Corporate disclosure · Management earnings forecast · Seasoned equity offering

JEL Classification H26 · M41

✉ Ningzhong Li
ningzhong.li@utdallas.edu

Jun Chen
chenjun332@zju.edu.cn

Xiaolu Zhou
xiaoluzhou@cuhk.edu.hk

1 Zhejiang University, 866 Yuhangtang Road, West Lake District, Hangzhou 310058, Zhejiang, China

2 University of Texas at Dallas, 800 W Campbell Road, Richardson, TX 75080, USA

3 The Chinese University of Hong Kong, 12 Chak Cheung Street, Shatin, N.T., Hong Kong

1 Introduction

The accounting literature has proposed equity financing as an important motive for voluntary corporate disclosures (e.g., Healy and Palepu 2001; Beyer et al. 2010). The basic idea is that managers who expect equity issuance have incentives to provide voluntary disclosure to reduce the information asymmetry, thereby lowering firms' costs of equity (Myers and Majluf 1984; Diamond and Verrecchia 1991; Baiman and Verrecchia 1996). In this argument, equity issuance heightens disclosure incentives by increasing the marginal benefit of disclosure (i.e., reducing information asymmetry) even when the information asymmetry in the market is constant.

Prior studies have provided empirical evidence consistent with this argument (e.g., Frankel et al. 1995; Lang and Lundholm 2000; Shroff et al. 2013; Clinton et al. 2014). These studies typically examine the change in voluntary disclosures when a firm issues equity or compare disclosure behaviors of firms that raise equity with those that do not, finding a positive association between equity issuance incentive and voluntary disclosure. However, literature reviews by Healy and Palepu (2001) and Beyer et al. (2010) both recognize an endogeneity concern in this line of studies. They argue that the decision to raise equity capital is usually driven by new investment opportunities and changes in business environments, which in turn are likely to be associated with increased information asymmetry between insiders and outsiders. When information asymmetry is heightened, the marginal benefit of disclosure increases, motivating the firm to increase disclosure. Thus, the increased disclosure associated with actual equity issuance documented by prior studies may be due to the increased information asymmetry accompanying the change of investment opportunities and business environments, not the equity issuance per se.[1]

In this study, we provide new causal evidence on the role of the equity financing incentive in firms' voluntary disclosure decisions by examining how exogenous changes in the likelihood of equity issuance affect firms' disclosures. Specifically, we explore the 2008 seasoned equity offering (SEO) deregulation (SEO deregulation hereafter), which exogenously increases the equity issuance incentives of affected firms without changing their business and information environments (Gustafson and Iliev 2017). In 2008, for the first time, the SEC began allowing listed firms with public floats of less than $75 million to raise equity capital through shelf registration, aiming to allow these firms to conduct accelerated SEOs. Accelerated SEOs offer quicker access to equity capital than traditional SEOs and lower issuance costs.[2] The deregulation has effectively increased affected small firms' equity

[1] Shroff et al. (2013) and Clinton et al. (2014) provide some causal evidence by examining how the SEC's Securities Offering Reform changes firms' disclosure prior to SEOs. The increased disclosures they document are due to the removal of disclosure restriction by the reform. Hence their studies could not fully address the endogeneity issue we discuss here because firms' equity issuance incentives are still endogenous. It is still unclear whether firms want to disclose because of equity issuance increasing marginal benefit of disclosure or because of the underlying change of investment opportunities and business environments and the consequent increase in information asymmetry.

[2] We provide institutional details in Sect. 2.2.

issuance incentives. Gustafson and Iliev (2017) document that, following the deregulation, affected small firms double their reliance on equity financing.

The SEO deregulation allows us to address the identification issue and provide causal evidence, because the deregulation is plausibly exogenous to firms' voluntary disclosure decisions. The timing of the deregulation was prompted by findings of the SEC Advisory Committee on Smaller Public Companies, which, in its 2006 public report, recommended that small companies be made eligible to use shelf registration since they have the same reporting obligations as large ones and therefore provide sufficient public disclosures for the use of shelf registration. Thus, the deregulation increases the likelihood of affected firms issuing equity without changing their business and information environments. Another advantage of this setting is that, following the deregulation, affected firms consist of two groups: those that issue equity through SEOs and those that do not have SEOs but on average have increased SEO incentives. The second group allows for a more direct examination of firms' *commitment* to disclosure in anticipation of a possible equity offering.

Economic theory suggests that a commitment by a firm to increasing the disclosure level should lower the information asymmetry component of the firm's cost of capital (e.g., Diamond and Verrecchia 1991; Baiman and Verrecchia 1996). Leuz and Verrecchia (2000) emphasize the importance of distinguishing between a commitment and a voluntary disclosure and argue that "the relation between the cost of capital and a commitment should be stronger than the relation between the cost of capital and a voluntary disclosure because only a commitment requires that information be disclosed regardless of its content" (p. 94). Because a one-time disclosure prior to equity issuance can be reversed and may not represent a disclosure commitment in the future, we argue that consistently providing disclosures in the period of no equity issuance is more likely to be perceived by the market as a disclosure commitment (Wasley and Wu 2006; Monahan 2006). Therefore, when equity issuance incentives are stronger, firms would increase disclosures even in the period when no equity is issued, to signal to the market their disclosure commitment to reduce the cost of equity in case they issue equity in the future.[3]

We focus on management earnings forecasts as our disclosure measure because they are particularly important for reducing information asymmetry in the equity market. Beyer et al. (2010) show that, for an average firm, management earnings forecasts account for around 16% of the quarterly return variance. In fact, the forward-looking nature of management earnings forecasts is so pertinent during equity issuance that, before 2005, the SEC forbade forward-looking disclosures prior to the filing of SEOs for fear that companies might use these disclosures to condition the market (Shroff et al. 2013; Clinton et al. 2014). Not surprisingly, prior studies on how external financing needs impact voluntary disclosures typically focus on management earnings forecasts as a disclosure measure (e.g., Frankel et al. 1995; Shroff et al. 2013).

[3] In this argument, firms do not need to have a specific future equity issuance plan when deciding to signal the disclosure commitment. We argue that, when equity issuance incentives are stronger, firms increase disclosures in the period of no equity issuance *unconditionally*, because the higher equity issuance likelihood increases the marginal benefit of disclosure due to the commitment effect.

Following Gustafson and Iliev (2017), we focus on firms near the \$75 million public float threshold for our analyses. We employ a difference-in-differences (DID) strategy, using firms with public floats between \$10 million and \$70 million that first gained access to shelf registration in 2008 as treatment firms and firms with public floats between \$80 million and \$150 million, which had access to shelf registration throughout the 2003–2014 sample period, as control firms.[4] We further decompose the treatment firm-years into two groups: those that do not have SEOs in the [-1, +2] window around the current year, labeled "non-issuers," and the others (those with SEOs in the [-1, +2] window), labeled "issuers."

We focus on the period 2003–2014, excluding the financial crisis period 2008–2009, to have five years in both the pre- (2003–2007) and the post-deregulation (2010–2014) periods.[5] As predicted, we find that, benchmarking against control firms, an average non-issuer treatment firm issues more management earnings forecasts in the post-deregulation period than in the pre-period.[6] The relative increase in the frequency, 22%, is economically meaningful. These results suggest that, when firms' equity issuance incentives are higher, even though they do not issue equity at all, they provide more disclosures.[7] Since the validity of the DID estimate critically depends on the parallel trends assumption, we further verify that the assumption is not violated—the time trends in the frequency of forecasts for the non-issuer treatment firms and control firms are similar in the pre-deregulation period.

Firms tend to forecast repeatedly if the purpose is to signal their disclosure commitment, because consistent disclosures provide a better commitment signal (Leuz and Verrecchia 2001). To provide evidence on this argument, we classify forecasting firm-years in the sample as repeated forecasters and non-repeated forecasters. A firm-year is identified as a repeated forecaster if it issues forecasts in the current year t as well as in $t-1$ or $t+1$ and as a non-repeated forecaster otherwise. We find the increase in forecasts for non-issuer treatment firms is due to the forecasts of repeated forecasters, not non-repeated forecasters. This evidence provides further support that affected non-issuer firms increase forecasts to signal their disclosure commitment.

To further support that our findings are due to firms' greater incentives to reduce information asymmetry when their equity issuance incentives are higher, we examine how the documented effect varies with firms' equity financing needs and information asymmetry in the equity market. Measuring equity financing needs with several proxies of growth opportunities, we find that the effect of the deregulation on the earnings forecast frequency of non-issuer treatment firms, relative to the effect for control

[4] We follow Gustafson and Iliev (2017) and exclude firms with public floats between \$70 million and \$80 million because these firms are likely to change the treatment status during the year (see Sect. 3.2 for details). Excluding these firms also helps address the concern that firms with public floats close to the threshold may manipulate their public floats to circumvent the regulation (Gao et al. 2009).

[5] Our main results are robust to including the financial crisis period.

[6] Our research design explores both within- and between-firm variations in equity issuance incentives, as do Gustafson and Iliev (2017). Sect. 3.1 describes the design in detail.

[7] In contrast, we do not find a significant change of forecast frequency for issuer treatment firms, because their equity issuance incentives, as reflected in the actual equity issuance, are similar before and after the deregulation.

firms, is stronger for firms with greater equity financing needs. We also find the effect to be stronger for firms with greater information asymmetry in the equity market, measured with bid-ask spread and the probability of informed trading (PIN).

We subject our main findings to several robustness tests. First, to address the concern that our finding may be sensitive to the measurement window for issuers and non-issuers, we show that our main results are robust to using alternative shorter or longer windows (e.g., [-1, + 1] and [-2, + 5]). Second, to mitigate the concern that our finding may be contaminated by firms that switch the treatment status during the sample period, we show that our main results are robust to using a shorter sample period (e.g., 2004–2013), which reduces the number of firms switching the treatment status. We also exclude from the analysis firms that change the treatment status during the sample period and find consistent results. Third, to focus on within-firm variations, we restrict the sample to firms that have the same treatment status during the sample period and have at least one observation in both the pre- and post-deregulation periods, finding consistent results. For this subsample, we further replace industry fixed effects with firm fixed effects and find similar results.

Fourth, to mitigate the concern that our treatment and control firms, by design, have different public floats, we restrict the sample to a narrower bandwidth around the \$75 million threshold (\$40 ~ 100 million) and find similar results. Finally, to further strengthen the identification, we conduct a falsification test based on firms with public floats between \$80 million and \$220 million using \$150 million as the pseudo-regulation threshold. We find no significant treatment effect, which provides further support that our finding is not due to a random time trend difference between small and relatively large firms.

In additional analyses, we examine whether the deregulation has different impacts on different types of management earnings forecasts, including good news versus bad news forecasts and optimistic versus pessimistic forecasts. If non-issuer treatment firms are incentivized to signal to the market their disclosure commitment, they would not increase disclosures opportunistically by asymmetrically increasing good news forecasts or optimistic forecasts (Leuz and Verrecchia 2000). We find that the deregulation significantly increases the frequencies of good news forecasts, bad news forecasts, and pessimistic forecasts of non-issuer treatment firms, relative to control firms. The effect on optimistic forecasts is also positive but insignificant, and it is not significantly different from the effect on pessimistic forecasts. These findings suggest that non-issuer treatment firms increase overall disclosures, instead of increasing disclosures opportunistically.

Finally, we address the influence of a confounding event. On December 19, 2007, the SEC passed Smaller Reporting Company Regulatory Relief and Simplification (the SRC rule, hereafter) to allow "smaller reporting companies" with a public float below \$75 million to choose reduced disclosures on 10 nonfinancial items in the periodic SEC filings beginning on February 4, 2008. To address the concern that our finding may be due to the impact of the SRC rule (Cheng et al. 2013), we separately examine treatment firms that are not affected by the SRC rule change and those that are. We find the treatment effect is significant for both groups and the difference is insignificant. Thus, our finding is unlikely to be due to the confounding rule change for smaller reporting companies.

Our study contributes to the disclosure literature by providing new causal evidence regarding the effect of equity financing incentive on firms' voluntary disclosure. While theory has established that greater equity issuance incentives could lead to more disclosures (e.g., Myers and Majluf 1984; Diamond and Verrecchia 1991; Baiman and Verrecchia 1996), the literature has provided limited causal evidence on this link. By exploiting the 2008 SEO deregulation as an exogenous increase in equity financing incentives with constant business and information environments, our study provides additional causal evidence for this link, adding to recent studies that attempt to provide causal evidence using exogenous events (e.g., Shroff et al. 2013; Clinton et al. 2014). In addition, by documenting that greater equity financing incentives lead to more disclosures even for firms that do not issue equity, we provide new evidence on how equity issuance incentive affects firms' disclosure commitment, which adds to the understanding of the impact of equity financing incentive on disclosure as well as the commitment role of voluntary disclosure (e.g., Hutton et al. 2003; Wasley and Wu 2006).

We also add to the recent research that studies the economic consequences of reducing equity financing barriers (e.g., Gustafson and Iliev 2017; Chu and Zhao 2018). Using the same SEO deregulation setting, Gustafson and Iliev (2017) show that reducing equity issuance barriers leads to more equity financing and lower equity issuance costs. Chu and Zhao (2018) find that, after the deregulation, affected banks increase mortgage lending, relative to control banks. We add to these studies by showing that reducing equity issuance barriers leads to more corporate disclosures for firms that do not issue equity.

The rest of the paper is organized as follows. Section 2 reviews prior research, discusses the institutional background, and develops the hypotheses. Section 3 presents empirical analyses. Section 4 concludes.

2 Prior research, institutional background, and hypothesis development

2.1 Prior research

Empirical studies on the impact of external financing incentive on corporate disclosure mostly examine the change in voluntary disclosures when a firm issues equity or compare disclosure behaviors of firms that raise capital with those that do not (e.g., Frankel et al. 1995; Lang and Lundholm 1993, 1996, 2000; Shroff et al. 2013; Clinton et al. 2014). This line of research finds a positive association between equity issuance incentive (or general external financing incentive) and voluntary disclosure. For instance, Frankel et al. (1995) show that the probability of a management forecast over the sample period 1980–1983 is greater for firms that finance externally during the period than for firms that do not. However, conditional on an offering, they find that firms are not more likely to forecast in the period immediately prior to the offering than at other times. Lang and Lundholm (2000) find that, beginning six months before an SEO, firms increase their disclosures, particularly disclosures over which they have the most discretion. However, they find no change in the frequency of forward-looking statements prior to the equity offering, which they attribute to the SEC expressly discouraging such disclosures before equity offerings.

Recent research reexamining voluntary disclosures surrounding SEOs consistently finds that firms increase disclosures prior to the offerings and that the richer pre-SEO information environment rewards firms with a lower cost of capital (Li and Zhuang 2012; Shroff et al. 2013; Clinton et al. 2014). In particular, Shroff et al. (2013) and Clinton et al. (2014) exploit the SEC's Securities Offering Reform (SOR) in 2005, which relaxes disclosure restrictions prior to SEOs, and document an increase in pre-offering disclosures following the reform, confirming that the no increase in management earnings forecasts prior to the offerings documented by prior research is partly due to the SEC's discouragement of pre-offering disclosures. Shroff et al. (2013) further show that the quasi-exogenous increase in pre-SEO disclosures is associated with a lower cost of equity.

As we discuss in the introduction, the literature has not fully addressed the endogeneity issue due to the concurrent changes in business and information environments accompanying equity issuances (Healy and Palepu 2001; Beyer et al. 2010). Because the decision to raise equity capital is usually driven by new investment opportunities and changes in business environments, which in turn may increase information asymmetry and the marginal benefit of disclosure, motivating the firm to disclose more. Thus, the increased disclosure associated with actual equity issuance documented by prior studies could be due to the increased information asymmetry accompanying the change of business environments, not the equity issuance per se. While Shroff et al. (2013) and Clinton et al. (2014) document some exogenous changes in disclosure prior to SEOs, because their setting represents a shock to disclosure restrictions, as opposed to a shock to equity issuance incentives, their studies do not fully address the endogeneity issue.

In addition, by focusing on disclosure behaviors of firms that actually issue equity, prior studies have missed one important aspect of the impact of the equity issuance incentives—a potential to issue equity may motivate a firm to increase disclosures to signal its disclosure commitment, even though it does not actually issue any equity. Consider two otherwise identical firms, A and B. A is not allowed to issue equity; B is. However, B does not actually issue equity due to the lack of equity financing need. In this case, B has a potential equity issuance incentive but A does not. This potential equity issuance incentive may also be an important driver of corporate disclosure. However, whether such a potential equity issuance incentive affects disclosure is not yet well understood. One nice feature of the 2008 SEO deregulation is that it allows us to measure an exogenous increase in such potential equity issuance incentive and examine its impact on firms' voluntary disclosure decisions.

2.2 Institutional background

The traditional public SEOs usually involve a lengthy SEC review as well as underwriter marketing. In contrast, the accelerated SEOs provide quicker and easier access to the equity market by expediting the equity issuance process through shelf registration. Shelf registration allows firms to pre-file expected securities offerings with the SEC. When filing shelf registration statements, firms do not need to specify security type or issuance time. When subsequently a firm wants to issue securities, it takes the securities "off the shelf" by issuing all or part of the registered securities. Since a shelf registration statement can "forward incorporate by reference" the reports that are filed *after* the shelf registration statement's effective date, the registration is automatically updated without delay or interruption when the firm is waiting for the SEC to review the offering terms.[8] In contrast to traditional SEOs in which firms have to file new or amended registration statements with the SEC for review, an effective shelf registration only requires the issuing firm to provide the SEC with a prospectus supplement that describes the offering terms. Therefore, compared with traditional SEOs, shelf registration is more cost effective and time efficient, enabling firms to access the equity market more quickly.

Small and large public firms faced very different legislative barriers to equity financing before 2008. Until 2008, public firms with public floats less than $75 million, which represent 25% of public firms, were prohibited by SEC from using shelf registration to raise equity capital. A firm was allowed to use shelf registration only if its public float was above $75 million within 60 days prior to the date of security sale. This rule was to protect less informed investors, because small firms may suffer more severe information asymmetry than large ones.

In the several years prior to 2008, there were substantial advances in electronic dissemination and accessibility of corporate disclosure transmitted over the Internet. The technology improvement greatly reduced information asymmetry among investors, especially for small firms. Thus, the SEC expanded eligibility of shelf registration usage to small firms in 2008, eliminating the $75 million public float requirement for shelf registration. The SEC argues that shelf registration confers significant advantages in terms of cost and time saving (SEC 2007). It anticipates that the deregulation would "allow more companies to benefit from the greater flexibility and efficiency in accessing the public securities markets" and that "by having more control of the timing of their offerings, these companies can take advantage of a desirable market" (SEC 2007). Recent research shows that the deregulation leads to a 49% increase in the annual probability of raising equity for affected small firms and that these firms switch from alternative financing methods to public equity (Gustafson and Iliev 2017).

2.3 Hypothesis development

The literature reviews by Healy and Palepu (2001) and Beyer et al. (2011) both list capital market transactions as the first motive for voluntary disclosures. The basic idea is that managers who expect external financing have incentives to provide voluntary disclosure to reduce the information asymmetry problem, thereby reducing their firms' cost of external financing (Myers and Majluf 1984; Healy and Palepu 2001). The theoretical literature has long recognized that information asymmetry introduces adverse selection into the share market (e.g., Copeland and Galai 1983; Glosten and Milgrom 1985; Kyle 1985; Easley and O'Hara 1987; Admati

[8] Some SEC registration statements (such as Form S-3 or Form 8-A) and other types of filings (such as Form 10-K) filed under the Securities Act or the Exchange Act enable the incorporation of certain information required by simply referring to the required information as it was disclosed or included in other forms, documents, or registration statements filed with the SEC. In some cases, filings or information may be automatically incorporated by reference from filings the issuer will make in the future. This is referred to as "forward incorporation by reference."

and Pfleiderer 1988). Less informed investors are concerned about trading with better informed investors so that they either price protect or exit the market to avoid losses from such trading, which reduces both liquidity and stock price. Such adverse impact of information asymmetry also exists when firms issue new shares. Since rational investors expect that they will trade with better informed investors in the future, they will pay less for new shares to compensate for their information disadvantages, thus increasing the cost of capital (Baiman and Verrecchia 1996).

Firms can mitigate the adverse selection problem by issuing equity when information asymmetry is low, such as after earnings releases (Korajczyk et al. 1991), or by providing more disclosures (Healy and Palepu 2001). Mandatorily or voluntarily disclosed information can bridge the information gap between less-informed and better-informed investors and lower information asymmetry, leveling the playing field among investors and reducing the cost of equity (Verrecchia 2001).

Economic theory also suggests that a *commitment* by a firm to increasing the disclosure level should lower the information asymmetry component of the firm's cost of capital (e.g., Diamond and Verrecchia 1991; Baiman and Verrecchia 1996). Consistent with the economic benefit of increasing disclosure commitment, Leuz and Verrecchia (2000) show that the information asymmetry component of the cost of capital decreases for German firms that have switched from the German to an international reporting regime, thereby committing themselves to increased disclosure. They emphasize the importance of distinguishing between a commitment and a voluntary disclosure and argue that "the relation between the cost of capital and a commitment should be stronger than the relation between the cost of capital and a voluntary disclosure because only a commitment requires that information be disclosed regardless of its content" (p. 94).

We argue that consistently providing disclosures in the period of no equity issuance is more likely to be perceived by the market as a commitment to disclosure, because a one-time disclosure prior to equity issuance can be reversed and may not represent a commitment to disclosure in the future. Therefore, when equity issuance incentives are stronger, firms would increase disclosures even in the period when no equity is issued, to signal to the market their disclosure commitment to reduce the cost of equity in case they actually issue equity in the future. Because the 2008 SEO deregulation increases affected small firms' public equity financing incentives (Gustafson and Iliev 2017) and management earnings forecasts are particularly important for reducing information asymmetry in the equity market (Beyer et al. 2010), firms affected by the deregulation (treated firms) are likely to issue more management earnings forecasts after the deregulation even when they do not actually have SEOs. In other words, in the post-deregulation period, even if a treatment firm does not plan to issue equity, if it provides forecasts in the current year, that would help reduce the cost of equity when it issues equity in the future, because consistently issuing forecasts in non-issuance years is more likely to be perceived by the market as a commitment to disclosure. Thus, we propose the following hypothesis.

H1: Non-SEO firms (firms that do not have SEOs) that are affected by the SEO deregulation provide more management earnings forecasts in the post-deregulation period.

Note that, in the hypothesis above, firms do not need to have a specific future equity issuance plan when deciding to signal the disclosure commitment. We predict that, when equity issuance incentives are stronger, firms increase disclosures in the period of no equity issuance unconditionally, because the higher equity issuance likelihood increases the marginal benefit of disclosure due to the commitment effect.

The prediction is not obvious because Gustafson and Iliev (2017) show that, while after the deregulation the treated firms increase public equity issuance, equity issuance through private investments in public equity and firm leverages decrease. To the extent that firms also have incentives to issue earnings forecasts to reduce information asymmetry when raising capital through these alternative channels, we may not be able to find a significant increase in the forecast frequency after the deregulation for non-SEO treatment firms.

We further consider the cross-sectional variation in the effect of the SEO deregulation. If the increase in the frequency of management earnings forecasts after the deregulation is driven by firms' equity financing incentives, we expect the effect to be more pronounced for firms with greater equity financing needs, such as those with greater investment opportunities. In addition, because the purpose of providing forecasts is to reduce information asymmetry when the firm issues equity, we expect the effect to be stronger for firms with greater information asymmetry in the equity market. This is especially true for SEOs through shelf registration, because prior research suggests that shelf registration can be expensive, relative to other forms of issuance, if information asymmetry is high (Smith 1986; Denis 1991; Blackwell et al. 1990).[9] To exploit shelf registration after the SEO deregulation, firms facing greater information asymmetry are likely to increase management earnings forecasts more to improve stock liquidity and lower the cost of equity. Thus, we propose the following cross-sectional hypotheses.

H2a: The effect predicted in H1 is stronger for firms with greater equity financing needs.
H2b: The effect predicted in H1 is stronger for firms with greater information asymmetry in the equity market.

3 Empirical analyses

3.1 Research design

Following Gustafson and Iliev (2017), we estimate the following difference-in-differences model to test H1.

[9] Shelf registration allows little time for investment banks to conduct sufficient due diligence because of the short period between taking issues from the shelf and selling them to the market. The resulting lack of certification by investment banks may lead to issuance price drop. This issue becomes more pronounced for firms with higher information asymmetry, especially for smaller firms (Denis 1991), as they have greater need of due diligence by investment banks.

$$\ln(1 + MEF_{it}) = \beta_0 + \beta_1 Treat - Post - NoSEO_{it} + \beta_2 Treat - Post - SEO_{it} + \beta_3 Treat - NoSEO_{it} + \beta_4 Treat - SEO_{it} + Controls_{it} + \gamma_j + \mu_t + \varepsilon_{it}, \quad (1)$$

where the subscripts *i*, *t*, and *j* denote firm, year, and industry, respectively. MEF_{it} is the total number of management earnings forecasts issued by a firm during a year. We follow Gustafson and Iliev (2017) and measure MEF_{it} during the 12 months after the second quarter end, when the public float is disclosed in the 10-K, to ensure that the disclosed public float (and thus the treatment status) is not affected by the forecasts.[10] The treatment firms are firms with public floats between $10 million and $70 million; the control firms are those with public floats between $80 million and $150 million (Gustafson and Iliev 2017). We follow Gustafson and Iliev (2017) and exclude firm-years with public floats between $70 million and $80 million, because these firms are likely to change the treatment status during the year.

We classify each firm-year as not issuing equity through SEO (non-issuer) if it does not have a SEO in the four-year window from *t-1* to *t + 2*.[11] A firm-year that is not identified as a non-issuer is classified as an issuer. *Treat-SEO*$_{it}$ (*Treat-NoSEO*$_{it}$) is an indicator variable for treatment firm-years that are issuers (non-issuers). *Treat-Post-SEO*$_{it}$ (*Treat-Post-NoSEO*$_{it}$) is an indicator variable for the post-deregulation treatment firm-years that are issuers (non-issuers). We classify fiscal years ending on or after June 30, 2008, as the post-deregulation period. The coefficient β_1 is the DID estimate of interest, which captures the average effect of the SEO deregulation on the frequency of management earnings forecasts for non-issuers in the treatment group, relative to the effect for the control group. We predict β_1 to be significantly positive. β_2 measures the average effect of the SEO deregulation on the frequency of management earnings forecasts for issuers in the treatment group, relative to the effect for the control group. While issuers in the treatment group are not our focus, they provide a good benchmark for evaluating the effect on non-issuers in the treatment group. We expect β_2 to be indistinguishable from zero because the equity issuance incentives are similar for issuers before and after the deregulation—their actual equity issuances reflect their strong equity issuance incentives.

We include four-digit SIC industry (γ_j) and year (μ_t) fixed effects to control for industry-specific and economy-wide factors associated with voluntary disclosures. We do not include an indicator for the post-regulation period because it is perfectly absorbed by the year fixed effects. We do not include firm fixed effects in Eq. (1) because we intend to exploit both within- and between-firm variations in equity issuance incentives to examine the effect of the deregulation. Using between-firm variation is important in this setting because firms could change the treatment status

[10] The results are qualitatively similar if the frequency of management earnings forecasts is measured for the fiscal year.

[11] Our results are robust to several alternative measurement windows (see Sect. 3.5). We use the full time series of a firm to identify issuers and non-issuers, not only the firm-years included in the sample.

or drop out of the sample over the sample period.[12] Moreover, as Gustafson and Iliev (2017, p. 586) note: "Even if firms stay in our sample for just one year, we still have a valid repeated cross-sectional design that tests whether firms right above the $75 million threshold behave differently relative to those under the threshold before and after the regulation change." Nevertheless, we conduct a robustness analysis to focus on within-firm variation by using firm fixed effects and a subsample of firms that do not change the treatment status and have at least one observation in both the pre- and post-deregulation periods and find similar results (see Sect. 3.5).

Controls refers to control variables. We control for the following firm characteristics that prior studies have shown to be associated with firms' disclosure incentives (e.g., Baginski and Hassell 1997; Miller 2002; Ajinkya et al. 2005; Lennox and Park 2006): returns on assets (*ROA*), an indicator for loss firms (*Loss*), the natural logarithm of market capitalization (*Size*), the leverage ratio (*Lev*), abnormal return (*Abret*), the market-to-book ratio (*Market-to-book*), earnings volatility (*Earn Vol*), the number of geographic and business segments (*Seg*), the use of large auditors (*Big4*), the existence of positive annual EPS changes (*ChEPS*), mergers and acquisitions (*M&A*), the number of analysts following a firm (*Numaf*), stock return beta (*Beta*), and institutional ownership (*IO*). Appendix Table 10 provides detailed definitions of these variables.

ROA, *Loss*, and *Abret* control for the impact of firm profitability (Degeorge et al. 1999; Ajinkya et al. 2005). *Size* controls for lower costs of disclosure by larger firms (e.g., Ajinkya et al. 2005; Kasznik and Lev 1995). *Market-to-book* is a measure of growth opportunities and controls for financing needs and proprietary costs of corporate disclosure (e.g., Bamber and Cheon 1998; Ajinkya et al. 2005). *ChEPS* controls for litigation concerns when earnings change is negative (Ajinkya et al. 2005; Crawford et al. 2020). *Big4* controls for the effect of auditor reputation (Ajinkya et al. 2005; Lang and Lundholm 1993). *Lev* controls for the effect of financial leverage (Huang et al. 2017). *Numaf* and *IO* control for analysts' and institutional investors' demands for disclosure, respectively (Lang and Lundholm 1993, 1996; Boone and White 2015). *Earn Vol* controls for forecasting difficulties and *Beta* for market risk (Li 2010; Ajinkya et al. 2005). *Seg* captures firm complexity, and *M&A* controls for the effect of organizational change (e.g., Feng et al. 2009). Standard errors are clustered by firm to account for possible within-firm dependence in the error terms.

To test the cross-sectional prediction in H2a, we partition the sample into subsamples with high versus low equity financing needs based on the sample median and estimate Eq. (1) for each subsample. We expect the treatment effect β_1 to be more positive in the high equity financing needs subsample. We proxy for firms' equity financing needs with growth opportunities, measured with Tobin's Q (*Tobin's Q*), sales growth (*Sales Growth*), and capital expenditure in the subsequent three years scaled by assets (*Future Capex*). Similarly, to test H2b, we partition the sample into subsamples with high versus low information asymmetry based on the

[12] An average firm remains in our sample for only 3.3 years. This number is comparable to that of Gustafson and Iliev (2017). In Sect. 3.5, we show that our main results are robust to removing firms that change the treatment status from the sample.

sample median and estimate Eq. (1) for each subsample. Information asymmetry is measured with the average bid-ask spread (*Bid-Ask Spread*) and the probability of informed trading (*PIN*), as estimated by Brown and Hillegeist (2007). Detailed definitions of these variables are provided in Appendix Table 10.

3.2 Data and descriptive statistics

We obtain management earnings forecasts data from I/B/E/S Guidance database, SEO data from Thomson Reuters SDC Platinum, financial data from Compustat, stock return information from CRSP, and analyst forecasts from I/B/E/S. To identify firms affected by the deregulation and a sample of unaffected firms as the control group, we use a python script to extract the public float data from 10-K filings from 2003 to 2014.[13] Since 2002, public firms have been required to report their public floats as of the end of the second fiscal quarter in their 10-K filings. Following Gustafson and Iliev (2017), we restrict the sample to firm-years with public floats between \$10 million and \$150 million and exclude firm-years with public floats between \$70 million and \$80 million, because these firms are likely to change the treatment status during the year. Removing firm-years with public floats slightly above or below the threshold also mitigates the concern that firms may manipulate public floats to circumvent the regulation (Gao et al. 2009). As a result, the treatment firms have public floats between \$10 million and \$70 million, and the control firms between \$80 million and \$150 million.

Our sample period is from 2003 to 2014. To mitigate the concern that our results may be influenced by the financial crisis, we exclude years 2008 and 2009.[14] We also exclude financial (SIC 6000–6999) and utilities (SIC 4900–4949) firms. Our final sample consists of 8,408 firm-year observations for 2,552 U.S. firms (on average, a firm has 3.3 years in the sample). There are 3,471 firm-year observations of treatment firms in the pre-deregulation period and 2,218 in the post-period. The control group consists of 1,647 firm-year observations in the pre-deregulation period and 1,072 in the post-period.

Table 1 reports descriptive statistics for the variables used in the main analysis. All continuous variables are winsorized at the 1% and 99% percentiles. Sixty-eight percent of observations are for treatment firms—58% for non-issuer treatment firms and 10% for issuers treatment firms. The means of *Treat-Post-SEO* and *Treat-Post-NoSEO* are 7% and 20%, respectively, indicating that 27% (=7% + 20%) of observations relate to treatment firms in the post-deregulation period, among which 7% relate to issuer treatment firms and 20% to non-issuer treatment firms. An average firm has a public float of \$60 million and market capitalization of \$116.9 million, issues 0.72 management earnings forecasts, and is followed by 1.8 analysts. The

[13] We verify the public float data by hand collecting the data for a random sample of observations. We also compare our data with that used by Gustafson and Iliev (2017), shared by Matthew Gustafson, to improve accuracy.

[14] Our main results are similar when the financial crisis period is included.

average market-to-book ratio is 2.59, and the average leverage ratio is 18%. Fifty-two percent of firms report a loss and the average return on asset is -14%.

3.3 The SEO deregulation and the frequency of management earnings forecasts

Figure 1 plots the average frequencies of management earnings forecasts during the 2003–2014 sample period separately for three groups: 1) non-issuer treatment firms, 2) issuer treatment firms, and 3) control firms. The forecast frequency decreases over time for each group. However, relative to the control group, the forecast frequency of non-issuer treatment firms decreases more slowly, and the difference in the average forecast frequencies between the two groups becomes smaller in the post-deregulation period, which is consistent with H1. In contrast, we do not observe a similar pattern for issuer treatment firms.

Table 2 reports the results of estimating Eq. (1) to test H1. In column 1, we exclude all control variables. This parsimonious regression helps address the concern that inclusion of covariates that may be affected by the treatment could bias the estimated treatment effect (Gormley and Matsa 2014; Imbens and Rubin 2015). The coefficient on *Treat-Post-NoSEO* is positive and significant (0.124, *t*-statistic = 3.29), indicating that, after the deregulation, an average non-issuer treatment firm issues more earnings forecasts in the post-deregulation period, relative to the difference for control firms. Column 2 reports the results after including the control variables. The coefficient on *Treat-Post-NoSEO* continues to be positive and significant (0.073, *t*-statistic = 2.04). The coefficient estimate in column 2, 0.073, suggests that, for the average non-issuer treatment firm in the pre-deregulation period, the forecast frequency increases by 22%, relative to the change for control firms after the deregulation.[15] This effect is economically significant.

In column 3, we separately examine the frequency of forecasts that are not bundled with earnings announcement (*Unbundled MEF*). Unbundled forecasts are issued separately and thus are presumably more likely to be viewed as a separate disclosure event to signal a disclosure commitment. We follow Rogers and Van Buskirk (2013) and classify forecasts not issued in the five-day window surrounding an earnings announcement as unbundled forecasts. In our sample, 25% of forecasts are unbundled forecasts. The results are consistent with those in column 2 based on all forecasts—the coefficient of *Treat-Post-NoSEO* is 0.056 and significant (*t*-statistic = 3.52).

When firms issue forecasts to signal their disclosure commitment, they tend to forecast repeatedly, because consistent disclosures provide a better commitment signal (Leuz and Verrecchia 2001). To provide evidence of this, we classify forecasting firm-years in the sample as repeated forecasters and non-repeated forecasters.

[15] The average forecast frequency for the non-issuer treatment firms in the pre-deregulation period is 0.54. The estimated coefficient of 0.073 suggests that, all else equal, the average forecast frequency after the deregulation, after benchmarking against the control group, is $e^{[\ln(1 + 0.54) + 0.073]} - 1 = 0.66$. The percentage increase in the frequency is $(0.66 - 0.54)/0.54 = 22\%$.

Table 1 Descriptive Statistics

Variable	*N*	Mean	*S.D*	P25	Median	P75
Treat-Post-NoSEO	8,408	0.20	0.40	0.00	0.00	0.00
Treat-Post-SEO	8,408	0.07	0.25	0.00	0.00	0.00
Treat-NoSEO	8,408	0.58	0.49	0.00	1.00	1.00
Treat-SEO	8,408	0.10	0.30	0.00	0.00	0.00
MEF	8,408	0.72	1.91	0.00	0.00	0.00
Ln(1 + *MEF*)	8,408	0.27	0.60	0.00	0.00	0.00
Public Float ($ million)	8,408	60.04	40.78	25.23	47.54	93.15
Market Cap ($ million)	8,408	116.94	154.24	36.75	72.88	144.20
Size	8,408	4.29	0.96	3.60	4.29	4.97
Market-to-book	8,408	2.59	5.08	1.01	1.71	3.18
Lev	8,408	0.18	0.38	0.00	0.08	0.26
ROA	8,408	-0.14	0.38	-0.19	-0.01	0.05
Loss	8,408	0.52	0.50	0.00	1.00	1.00
Abret	8,408	0.10	0.93	-0.40	-0.11	0.29
ChEPS	8,408	0.58	0.49	0.00	1.00	1.00
Earn Vol	8,408	1.27	3.75	0.10	0.24	0.71
Seg	8,408	4.06	2.71	2.00	4.00	5.00
Numaf	8,408	1.79	2.36	0.00	1.00	3.00
M&A	8,408	0.09	0.28	0.00	0.00	0.00
Beta	8,408	1.42	1.19	0.63	1.23	2.02
Big4	8,408	0.50	0.50	0.00	1.00	1.00
IO	8,408	0.28	0.23	0.09	0.23	0.43

This table reports the summary statistics for the sample. The sample period is 2003–2014, excluding the financial crisis period (2008–2009). The sample consists of 2,552 unique firms. All variables are defined in Appendix Table 10

A firm-year is identified as a repeated forecaster if it issues forecasts in the current year *t* as well as in *t-1* or *t+1* and as a non-repeated forecaster otherwise.[16] We separately use the frequency of forecasts issued by repeated forecasters (*Rep MEF*) and the frequency of forecasts issued by non-repeated forecasters (*NonRep MEF*) as the dependent variable (both in log transformation) and report the results in columns 4 and 5 of Table 2. We find that the frequency of repeated forecasts increases significantly after the deregulation for non-issuer treatment firms, relative to control firms (the coefficient of *Treat-Post-NoSEO* is 0.078 with *t*-statistic 2.15), whereas there is no significant change for the frequency of non-repeated forecasts (the coefficient of *Treat-Post-NoSEO* is -0.002 with *t*-statistic -0.29).[17] This evidence is consistent with non-issuer treatment firms increasing repeated forecasts to signal disclosure

[16] We use the full time series of a firm to identify repeated and non-repeated forecasters, not only the firm-years included in the sample.

[17] The difference in the two coefficients of *Treat-Post-NoSEO* is significant (*p*-value = 0.03).

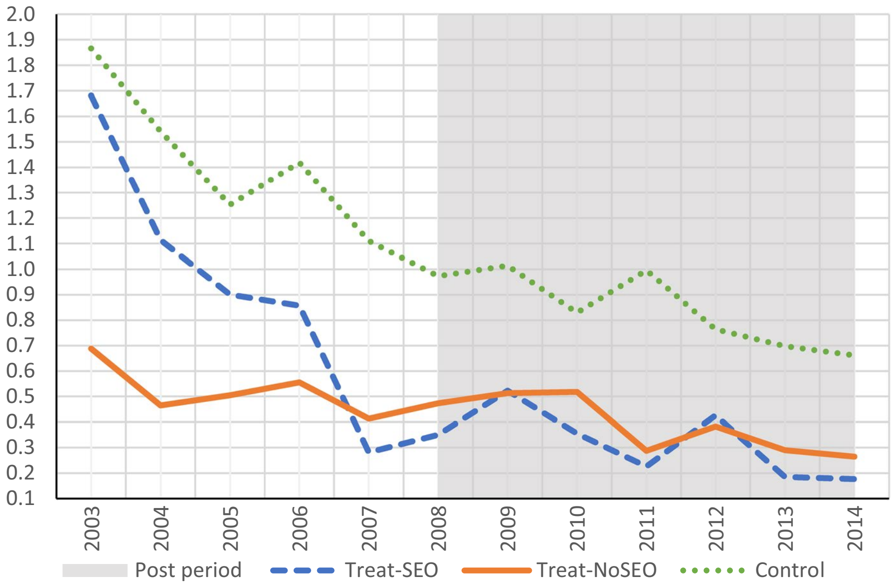

Fig. 1 Annual Frequency of Management Earnings Forecasts over the Sample Period. This figure plots the average frequencies of management earnings forecasts during the 2003–2014 sample period separately for three groups: 1) treatment firm-years with public floats between \$10 million and \$70 million that do not have SEOs in the four-year window from *t-1* to *t+2*, labeled as "Treat-NoSEO" above; 2) treatment firm-years with public floats between \$10 million and \$70 million that have SEOs in the four-year window from *t-1* to *t+2*, labeled as "Treat-SEO" above; and 3) control firm-years with public floats between \$80 million and \$150 million, labeled as "Control" above. The frequency of management earnings forecasts is calculated as the total number of earnings forecasts issued during the 12 months after the second quarter-end

commitment.[18] Taken together, the results in Table 2 indicate that, after the deregulation, an average non-issuer treatment firm issues more earnings forecasts, especially repeated forecasts, in the post-deregulation period, relative to the difference for control firms, which is consistent with our H1.[19]

The coefficient of *Treat-Post-SEO* is insignificant in all columns. These results are consistent with our expectation that the disclosure incentives of issuer treatment firms are unlikely to change significantly after the deregulation. With respect

[18] In an untabulated robustness test, we redefine repeated forecasters as firm-years that issue forecasts in *t* as well as both *t-1* and *t+1* and non-repeated forecasters as the other forecasting firm-years. We find qualitative similar but statistically weaker results. The estimated coefficient of *Treat-Post-NoSEO* is 0.060 and significant (*t*-statistic = 1.78) for repeated forecasts and is 0.014 and insignificant (*t*-statistic = 0.82) for non-repeated forecasts.

[19] We also explore the effect of the deregulation on non-issuer treatment firms' likelihoods of issuing repeated forecasts and non-repeated forecasts. We find that the likelihood of issuing repeated forecasts increases significantly by 3.7 percentage points for non-issuer treatment firms, relative to control firms, while the likelihood of issuing non-repeated forecasts does not change significantly (untabulated).

Table 2 SEO Deregulation and Management Earnings Forecasts

	Ln(1 + *MEF*)		Ln(1 + *Unbundled MEF*)	Ln(1 + *Rep MEF*)	Ln(1 + *NonRep MEF*)
	(1)	(2)	(3)	(4)	(5)
Treat-Post-NoSEO	0.124***	0.073**	0.056***	0.078**	-0.002
	(3.29)	(2.04)	(3.52)	(2.15)	(-0.29)
Treat-Post-SEO	-0.057	0.031	0.029	0.032	0.001
	(-0.91)	(0.53)	(1.02)	(0.54)	(0.08)
Treat-NoSEO	-0.316***	-0.035	-0.011	-0.035	-0.001
	(-11.57)	(-1.23)	(-0.69)	(-1.23)	(-0.14)
Treat-SEO	-0.107**	0.077	0.040	0.079	-0.000
	(-2.02)	(1.52)	(1.45)	(1.55)	(-0.03)
Size		0.051***	0.023***	0.046***	0.005*
		(4.43)	(3.84)	(4.03)	(1.82)
Market-to-book		0.000	-0.000	0.001	-0.001**
		(0.24)	(-0.19)	(0.73)	(-2.12)
Lev		-0.007	-0.000	-0.004	-0.003
		(-0.44)	(-0.04)	(-0.24)	(-1.05)
ROA		0.076***	0.013	0.075***	0.004
		(3.92)	(1.46)	(3.98)	(0.77)
Loss		-0.132***	-0.046***	-0.136***	0.005
		(-6.74)	(-5.19)	(-6.99)	(1.21)
Abret		0.005	0.003	-0.001	0.005*
		(0.67)	(0.60)	(-0.16)	(1.72)
Cheps		-0.035***	-0.023***	-0.034***	-0.002
		(-2.93)	(-3.40)	(-2.91)	(-0.59)
Earn Vol		0.002	0.000	0.002	0.000
		(0.90)	(0.45)	(0.88)	(0.17)
Seg		0.003	0.001	0.002	0.001*
		(0.77)	(0.87)	(0.40)	(1.77)
Numaf		0.076***	0.031***	0.075***	0.001
		(14.09)	(10.90)	(13.71)	(0.64)
M&A		0.022	0.006	0.024	0.001
		(0.77)	(0.45)	(0.80)	(0.10)
Beta		-0.004	-0.002	-0.003	-0.001
		(-0.67)	(-0.86)	(-0.47)	(-0.45)
Big4		0.012	-0.015	0.014	0.001
		(0.58)	(-1.59)	(0.65)	(0.19)
IO		0.214***	0.055**	0.222***	-0.007
		(3.85)	(2.29)	(4.00)	(-0.76)
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.157	0.266	0.171	0.261	0.011
N	8,408	8,408	8,408	8,408	8,408

Table 2 (continued)

This table presents difference-in-differences estimation results for the effect of the SEO deregulation on the frequency of management earnings forecasts of firms that do not have SEOs. The sample period is 2003–2014. The sample includes all firms with public floats between $10 million and $70 million (treatment firms) or between $80 million and $150 million (control firms). All variables are defined in Appendix Table 10. All regressions include year and industry (four-digit SIC) fixed effects. Standard errors are clustered at the firm level. ***, **, and * denote significance at the levels of 1%, 5%, and 10%, respectively

to the effects of control variables, we find that the frequency of earnings forecasts is positively associated with firm size (*Size*), performance (*ROA*), analyst following (*Numaf*), and institutional ownership (*IO*) and negatively associated with the existence of positive annual EPS changes (*Cheps*). These results are consistent with prior studies (e.g., Ajinkya et al. 2005; Huang et al. 2017).

Since the validity of the DID estimate critically depends on the parallel trends assumption, we further provide evidence that the assumption is not violated. We modify Eq. (1) as follows. We add *Treat-NoSEO-2006* and *Treat-NoSEO-2007* to flag non-issuers treatment firms in 2006 and 2007 and *Treat-SEO-2006* and *Treat-SEO-2007* to indicate issuer treatment firms in 2006 and 2007.[20] Detailed definitions of these variables are provided in Appendix Table 10. Table 3 reports the results. The results for control variables are omitted for brevity. Columns 1 and 2 report the models without and with control variables, respectively. For both model specifications, the coefficients of *Treat-NoSEO-2006* and *Treat-NoSEO-2007* are insignificant, suggesting that the trends in the frequency of forecasts for the non-issuer treatment firms and control firms are similar in the pre-deregulation period, supporting the parallel trends assumption. The coefficients of *Treat-Post-NoSEO* are positive and significant, consistent with the treatment effect we document in Table 2.

3.4 Cross-sectional tests

We report in Table 4 the results of testing H2a, which predicts that the effect of the deregulation on the disclosure incentives of non-issuer treatment firms is stronger among firms with greater external financing needs. Columns 1 and 2 report the sample partition based on Tobin's Q measured at year t.[21] The estimated coefficient on *Treat-Post-NoSEO* is positive and significant (0.116, t-statistic = 2.57) for the subsample of firms with high (above the sample median) Tobin's Q in column 1, whereas it is insignificant (0.005, t-statistic = 0.10) for the subsample with low Tobin's Q in column 2. Moreover, the difference in the two coefficients is significant (p-value = 0.092).

[20] This approach of testing the parallel trends assumption is commonly used in the literature (e.g., Huang et al. 2020; Costello 2020).

[21] Our partitioning variables are measured contemporaneously with the dependent variable, not in the period prior to the deregulation. Thus, we examine how the treatment effect varies with contemporaneous firm characteristics, not with the pre-deregulation firm characteristics (e.g., Gustafson and Iliev 2017; Klasa et al. 2018; Li et al. 2018; Ali et al. 2019).

Columns 3 and 4 report the sample partition based on sales growth at year *t*. We find that the coefficient on *Treat-Post-NoSEO* is positive and significant (0.135, *t*-statistic = 2.81) for the subsample of firms with high (above the sample median) sales growth (column 3), the coefficient becomes insignificant (-0.044, *t*-statistic = -0.86) for the subsample with low sales growth (column 4), and the difference in the two coefficients is significant (*p*-value = 0.003). The results reported in columns 5 and 6 for the partitioned samples based on future capital expenditure are qualitatively similar. The treatment effect is positive and significant in the high future capital expenditure subsample but is insignificant in the low future capital expenditure subsample, and the difference is significant. Collectively, the results in Table 4 are consistent with the prediction of H2a that the treatment effect is stronger for firms with greater equity financing needs.

Table 5 reports the results of testing H2b, which predicts that the treatment effect is stronger for firms with higher information asymmetry in the equity market. Columns 1 and 2 report the sample partition based on bid-ask spread; Columns 3 and 4 report the sample partition based on PIN.[22] We measure both partitioning variables at year *t-1* to ensure that they are not affected by the forecast decision in year *t*. The coefficient on *Treat-Post-NoSEO* is positive and significant for the subsample of firms with high information asymmetry, as measured by high bid-ask spread (0.157, *t*-statistic = 2.85, column 1) or high PIN (0.202, *t*-statistic = 3.22, column 3), while it becomes insignificant for the subsample of firms with low bid-ask spread (0.033, *t*-statistic = 0.68, column 2) or low PIN (0.034, *t*-statistic = 0.56, column 4). Furthermore, the differences in the coefficients between the high and low information asymmetry subsamples are significant. These results are consistent with the prediction that the positive impact of the SEO deregulation on non-issuer treatment firms' earnings forecast frequency relative to the impact for control firms is stronger for firms with higher information asymmetry in the equity market.

3.5 Robustness and falsification tests

We next provide additional evidence for our main finding in Table 2 from several robustness tests. First, we use two alternative windows to identify non-issuers and issuers: [-1, + 1] and [-2, + 5]. The results reported in columns 1 and 2 of Table 6 indicate that the estimated treatment effects are very close to that based on [-1, + 2] in our main analysis.[23] Second, we use a shorter sample period, 2004–2013, to reduce the likelihood of the analysis being contaminated by confounding events as well as the likelihood of firms changing the treatment status. We continue to find a very similar treatment effect, as reported in column 3 of Table 6.

[22] The sample period for the analyses based on PIN ends in 2011 due to the availability of the PIN measure.

[23] We also find similar results using the window [-1, + 3] (untabulated).

Table 3 Parallel Trends Test

	Ln(1 + *MEF*)	
	(1)	(2)
Treat-NoSEO-2006	0.063	0.049
	(1.27)	(1.06)
Treat-NoSEO-2007	0.079	0.069
	(1.58)	(1.48)
Treat-Post-NoSEO	0.149***	0.094**
	(3.66)	(2.43)
Treat-SEO-2006	-0.021	-0.041
	(-0.17)	(-0.34)
Treat-SEO-2007	-0.088	-0.045
	(-0.94)	(-0.50)
Treat-Post-SEO	-0.070	0.022
	(-0.98)	(0.32)
Treat-NoSEO	-0.340***	-0.056*
	(-11.37)	(-1.82)
Treat-SEO	-0.095	0.086
	(-1.50)	(1.43)
Controls	No	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes
Adj. R^2	0.157	0.266
N	8,408	8,408

This table reports the results of the parallel trends test. The sample period is 2003–2014. The sample includes all firms with public floats between $10 million and $70 million (treatment firms) or between $80 million and $150 million (control firms). All variables are defined in Appendix Table 10. Both regressions include year and industry (four-digit SIC) fixed effects, and column 2 includes all firm-level controls used in Table 2. Standard errors are clustered at the firm level. ***, **, and * denote significance at the levels of 1%, 5%, and 10%, respectively

Third, we exclude firms that change the treatment status during the sample period and re-estimate Eq. (1). While the sample size decreases from 8,408 to 4,866 (column 4 of Table 6), we continue to find a positive and significant coefficient on *Treat-Post-NoSEO* (0.162, *t*-statistic = 2.65). In fact, the estimated treatment effect is larger than that reported in column 2 of Table 2, 0.162 versus 0.073. Next, we further reduce the sample to firms that have a least one observation in both the pre- and post-deregulation periods. The sample size becomes much smaller, 1,486 observations (column 5 of Table 6). Despite the smaller sample size, the coefficient of *Treat-Post-NoSEO* remains positive and significant, and the magnitude becomes even larger (0.265). Thus, if there is any bias in our estimate of treatment effect in Table 2 due to the sample selection, it seems to understate the effect. For this

Table 4 Cross-Sectional Tests Based on Equity Financing Needs

Dependent Variable	Ln(1+*MEF*)					
Partitioning Variable	*Tobin's Q*		*Sales Growth*		*Future Capex*	
	(1)	(2)	(3)	(4)	(5)	(6)
	High	Low	High	Low	High	Low
Treat-Post-NoSEO	0.116**	0.005	0.135***	-0.044	0.140***	0.010
	(2.57)	(0.10)	(2.81)	(-0.86)	(2.88)	(0.17)
Treat-Post-SEO	0.162**	-0.111	0.033	0.019	0.144	-0.012
	(2.34)	(-1.21)	(0.39)	(0.24)	(1.64)	(-0.15)
Treated-NoSEO	-0.042	-0.037	-0.037	0.001	-0.033	-0.048
	(-1.21)	(-0.80)	(-0.98)	(0.03)	(-0.84)	(-1.08)
Treated-SEO	0.031	0.132	0.151**	-0.010	0.089	0.017
	(0.53)	(1.64)	(1.99)	(-0.17)	(1.28)	(0.23)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.301	0.261	0.284	0.245	0.319	0.243
N	4,204	4,204	4,100	4,100	3,859	3,860
p-value of diff. in coef. of *Treat-Post-NoSEO*	0.092		0.003		0.059	

This table reports the results for the impact of the SEO deregulation on management earnings forecasts of firms that do not have SEOs for the subsamples of firms with high versus low equity financing needs, measured with *Tobin's Q*, *Sales Growth*, and *Future Capex*. *Tobin's Q* is book value of liabilities plus market value of equity scaled by total assets. *Sales Growth* is the change of sales scaled by lagged sales. *Future Capex* is the sum of net capital expenditure over year $t+1$ to year $t+3$ scaled by total assets. The regressions include all firm-level controls used in Table 2 and year and industry (four-digit SIC) fixed effects. All variables are defined in Appendix Table 10. Standard errors are clustered at the firm level. ***, **, and * denote significance at the levels of 1%, 5%, and 10%, respectively.

subsample, we further replace industry fixed effects with firm fixed effects to focus on within-firm variations and find consistent results (column 6 of Table 6).[24] [25]

Finally, we restrict our sample to a narrower bandwidth around the $75 million cutoff to increase the comparability of treatment and control firms. Specifically, we focus on firms with public floats between $40 million and $100 million (still excluding firms with public floats between $70 million and $80 million). Column 7 of Table 6 reports the results of this analysis. The sample size decreases to 3,480 from 8,408. Despite the much smaller sample size, we continue to find a positive and significant treatment effect (0.116, *t*-statistic = 2.34) whose magnitude is still slightly larger than that in column 2 of Table 2 (0.116 versus 0.073).

[24] The results of columns 5 and 6 are similar when we use a shorter sample period, 2006–2012, which is not affected by the 2005 Securities Offering Reform (Shroff et al. 2013; Clinton et al. 2014).

[25] In column 6, *Treat-SEO* is dropped because the sum of *Treat-SEO* and *Treat-NoSEO* is a dummy for treatment firms, which is a linear combination of firm fixed effects.

Table 5 Cross-Sectional Tests Based on Information Asymmetry

Dependent Variable	Ln(1 + *MEF*)			
Partitioning Variable	*Bid-Ask Spread*		*PIN*	
	(1)	(2)	(3)	(4)
	High	Low	High	Low
Treat-Post-NoSEO	0.157***	0.033	0.202***	0.034
	(2.85)	(0.68)	(3.22)	(0.56)
Treat-Post-SEO	0.074	0.073	0.074	0.097
	(0.91)	(0.87)	(0.74)	(1.15)
Treat-NoSEO	-0.006	-0.027	-0.026	-0.024
	(-0.17)	(-0.69)	(-0.60)	(-0.64)
Treat-SEO	0.130*	0.009	0.158*	-0.001
	(1.91)	(0.12)	(1.95)	(-0.01)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Adj. R^2	0.216	0.286	0.267	0.298
N	4,197	4,198	3,295	3,295
p-value of diff. in coef. of				
Treat-Post-NoSEO	0.067		0.033	

This table reports the results for the impact of the SEO deregulation on management earnings forecasts of firms that do not have SEOs for the subsamples of firms with high versus low information asymmetry, measured with *Bid-Ask Spread* and *PIN*. *Bid-Ask Spread* is measured as the average daily bid-ask spread during year $t-1$. *PIN* is the estimated likelihood of informed trading for year $t-1$, obtained from the website of Stephen Brown. The regressions include all firm-level controls used in Table 2 and year and industry (four-digit SIC) fixed effects. All variables are defined in Appendix Table 10. Standard errors are clustered at the firm level. ***, **, and * denote significance at the levels of 1%, 5%, and 10%, respectively.

One alternative explanation for our finding in Table 2 is that it may be due to a random time trend difference between small and large firms. While the comparison between non-issuers and issuers among treatment firms in Table 2 and the analysis of the parallel trends assumption in Table 3 help mitigate this concern, we further conduct a falsification test based on firms with public floats between \$80 million and \$220 million, using \$150 million as the pseudo-regulation threshold. Specifically, we define treatment firms as those with public floats between \$80 million and \$145 million and control firms as those whose public floats fall between \$155 million and \$220 million and re-estimate Eq. (1). The results reported in Table 7 indicate that the estimated coefficient on *Treat-Post-NoSEO* is insignificant, whether or not we include the control variables. This analysis provides further support that our finding in Table 2 is due to the SEO deregulation, not due to a random time trend difference between small and large firms.

Table 6 Robustness Tests

	Ln(1 + *MEF*)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat-Post-NoSEO	0.073**	0.080**	0.075*	0.162***	0.265**	0.206*	0.116**
	(2.03)	(2.17)	(1.91)	(2.65)	(2.17)	(1.75)	(2.34)
Treat-Post-SEO	0.044	0.031	0.033	0.020	0.043	0.134	0.029
	(0.71)	(0.62)	(0.47)	(0.20)	(0.17)	(0.73)	(0.35)
Treat-NoSEO	-0.030	-0.040	-0.031	-0.078	-0.502***	-0.065	-0.073**
	(-1.06)	(-1.42)	(-0.97)	(-1.44)	(-3.08)	(-0.48)	(-2.10)
Treat-SEO	0.067	0.057	0.080	0.109	-0.229		0.077
	(1.21)	(1.30)	(1.29)	(1.20)	(-0.81)		(1.08)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	No	Yes
Firm FE	No	No	No	No	No	Yes	No
Adj. R^2	0.265	0.266	0.257	0.293	0.355	0.592	0.267
N	8,408	8,408	6,582	4,866	1,486	1,486	3,480
Sample	Main sample	Main sample	Sample period 2004–2013	Exclude firms changing the treatment status	Exclude firms changing the treatment status and require each firm to have obs. in both the pre- and post-periods		Public floats in [40,110]
Measurement window for non-issuers	[-1, + 1]	[-2, + 5]	[-1, + 2]	[-1, + 2]	[-1, + 2]	[-1, + 2]	[-1, + 2]

This table reports robustness tests for the impact of the SEO deregulation on management earnings forecasts of firms that do not have SEOs. Columns 1 and 2 use alternative windows to identify non-issuers versus issuers. Column 3 is based on a shorter sample period, 2004–2013. The analysis in column 4 is based on firms whose public floats always stay above or below the $75 million threshold throughout the sample period. In column 5, we further require that each firm has at least one observation in the pre-deregulation period and at least one observation in the post-period. Column 6 is based on the same sample as in column 5, with industry fixed effects replaced by firm fixed effects. In this regression, *Treat-SEO* is dropped because the sum of *Treat-SEO* and *Treat-NoSEO* is a dummy for treatment firms, which is a linear combination of firm fixed effects. Column 7 is based on the subsample of firms with public floats between $40 and $110 million, with firms with public floats between $70 million and $80 million excluded. Standard errors are clustered at the firm level. All variables are defined in Appendix Table 10. ***, **, and * denote significance at the levels of 1%, 5%, and 10%, respectively

Table 7 Falsification Tests

	Ln(1 + *MEF*)	
	(1)	(2)
Treat-Post-NoSEO	0.032	0.013
	(0.59)	(0.25)
Treat-Post-SEO	-0.100	-0.043
	(-1.30)	(-0.58)
Treat-NoSEO	-0.148***	-0.036
	(-3.91)	(-0.97)
Treat-SEO	0.006	0.078
	(0.10)	(1.38)
Controls	No	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes
Adj. R^2	0.207	0.290
N	4,247	4,247
Sample	Firms with public floats [80,220]	

This table reports falsification tests for the impact of the SEO deregulation on management earnings forecasts of firms that do not have SEOs. The analysis is based on firms with public floats between $80 million and $220 million using $150 million as a pseudo-cutoff for the regulation. Firms with public floats between $145 million and $155 million are excluded. Both regressions include year and industry (four-digit SIC) fixed effects, and column 2 includes all firm-level controls used in Table 2. Standard errors are clustered at the firm level. All variables are defined in Appendix Table 10. ***, **, and * denote significance at the levels of 1%, 5%, and 10%, respectively

3.6 The SEO deregulation and different types of management earnings forecasts

We next examine whether the easier access to the equity market for small firms due to the 2008 SEO deregulation has different impacts on different types of management earnings forecasts, including good news versus bad news forecasts and optimistic versus pessimistic ones. This analysis will shed light on whether managers provide more forecasts in general regardless of forecast types or do so opportunistically to condition the market. If non-issuer treatment firms are incentivized to signal to the market their commitment to disclosures, we expect the positive effect of the deregulation to hold for both good news and bad news forecasts and for both optimistic and pessimistic forecasts (Leuz and Verrecchia 2000). While there is evidence that firms could opportunistically bias the disclosure prior to actual equity issuance to "hype the stock" (e.g., Lang and Lundholm 2000), such opportunistic disclosure is unlikely if a firm intends to signal its disclosure commitment in the period of no equity issuance, because the disclosure bias will be detected soon, which can actually hurt the firm's reputation for transparency.

We follow Anilowski et al.'s (2007) approach to classify good news versus bad news for each quantitative forecast, using the consensus analyst forecast as the benchmark. Optimistic versus pessimistic forecasts are defined using the realized earnings as the benchmark. We use the natural logarithm of one plus the frequency of each type of forecasts as the dependent variable and estimate Eq. (1). To ensure that the difference in the estimated effects is not due to the scale of the dependent variable, we normalize all dependent variables (after log transformation) to having a mean of zero and standard deviation of one. The results reported in Table 8 indicate that the coefficients on *Treat-Post-NoSEO* are all positive, and they are significant, except for column 3, in which the dependent variable is the frequency of optimistic forecast.[26] In addition, the treatment effects are not statistically different either for good news versus bad news forecasts (columns 1 and 2) or for optimistic versus pessimistic forecasts (columns 3 and 4). These results suggest that non-issuer treatment firms increase the overall frequency of management earnings forecasts after the SEO deregulation, relative to the change for control firms, not doing so opportunistically.

3.7 Confounding regulation change

On December 19, 2007, the SEC passed Smaller Reporting Company Regulatory Relief and Simplification (the SRC rule, hereafter) to allow "smaller reporting companies" with a public float below \$75 million to choose reduced disclosures on 10 nonfinancial items in the periodic SEC filings beginning on February 4, 2008 (SEC Release No. 33-8876). Cheng et al. (2013) document that more than half of the affected firms reduce disclosure in 10-Ks after the SRC rule change. While we believe that this regulation change is unlikely to drive our finding because it provided affected firms with an option to reduce disclosure of several previously mandatory items in SEC filings, we address this alternative explanation empirically by separately examining our treatment firms that are not affected by the change—namely, firms with public floats and annual revenues both below \$25 million (Cheng et al. 2013, labeled as "small treatment firms") [27] and those that are affected by the rule change (other treatment firms, labeled as "large treatment firms").

Table 9 reports the results of this analysis. In column 1, we compare small treatment firms with control firms. The sample size is 3,513, including 794 small treatment firm-years and 2,719 control firm-years. The estimated coefficient on *Treat-Post-NoSEO* is positive and significant (0.083, *t*-statistic = 1.70). Thus, for treatment firms that are not affected by the SRC rule change, we find a similar effect of the SEO deregulation as in the full sample, mitigating the concern that our finding for H1 in the full sample may be due to the SRC rule change. Column 2 reports that the treatment effect is also positive and significant for large non-issuer treatment firms (0.070,

[26] The insignificant effect for optimistic forecasts could be due to managers' conservative disclosure decisions in order to build up a reputation for transparency.

[27] These firms, classified as small business issuers, were allowed to use scaled (reduced) disclosure since July 1992 and thus were not affected by the SRC rule change (Cheng et al. 2013).

Table 8 SEO Deregulation and Different Types of Management Earnings Forecasts

	Ln(1 + *MEF*)			
	Good News	Bad News	Optimistic News	Pessimistic News
	(1)	(2)	(3)	(4)
Treat-Post-NoSEO	0.241***	0.219***	0.068	0.156**
	(4.21)	(3.79)	(1.17)	(2.48)
Treat-Post-SEO	0.055	0.202**	0.066	0.081
	(0.50)	(2.20)	(0.72)	(0.75)
Treat-NoSEO	-0.106**	-0.092*	-0.096*	-0.019
	(-2.07)	(-1.89)	(-1.87)	(-0.41)
Treat-SEO	0.155	0.010	0.036	0.157*
	(1.49)	(0.12)	(0.43)	(1.65)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
p-value of diff. in coef. of *Treat-Post-NoSEO*	0.666		0.174	
Adj. R^2	0.195	0.253	0.156	0.235
N	8,408	8,408	8,408	8,408

This table presents difference-in-differences estimation results for the effect of the SEO deregulation on the frequencies of management earnings forecasts with different properties issued by firms that do not have SEOs. The sample period is 2003–2014, with the financial crisis period (2008–2009) excluded. The sample includes all firms with public floats between \$10 million and \$70 million (treatment firms) or between \$80 million and \$150 million (control firms). All dependent variables are normalized to having a mean of 0 and a standard deviation of 1. Columns 1 and 2 report the results for the frequencies of good news and bad news management earnings forecasts, respectively, where good (bad) news is defined by comparing the forecast with the median analyst forecast. Columns 3 and 4 report the results for the frequencies of optimistic and pessimistic management earnings forecasts, respectively, wherein an optimistic (pessimistic) forecast is defined by comparing the forecast with the actual earnings. All regressions include all firm-level controls used in Table 2 and year and industry (four-digit SIC) fixed effects. Standard errors are clustered at the firm level. All variables are defined in Appendix Table 10. ***, **, and * denote significance at the levels of 1%, 5%, and 10%, respectively

t-statistic = 1.89). The effect for large treatment firms is smaller in magnitude than that for small treatment firms. However, the difference is insignificant (*p*-value = 0.753). Thus, it seems that the SRC rule change does not have a significant effect on the frequency of management earnings forecasts of affected non-issuer firms.[28] [29]

[28] We conclude this because the effect for small non-issuer treatment firms is due to the SEO deregulation and the effect for large non-issuer treatment firms is not significantly different from that for small non-issuer treatment firms.

[29] Another possible confounding regulation change is the implementation of Sect. 404 of the Sarbanes–Oxley Act (SOX)—which requires that each annual report include a management report on internal control over financial reporting that is attested by an independent auditor—for non-accelerated filers (firms with public floats below \$75 million). For non-accelerated filers, Sect. 404 was effective for years ending after December 15, 2007, whereas the requirement for auditor attestation has been postponed several times and was eventually revoked (SEC Release No. 33–9142). We expect the requirement of manage-

Table 9 Small versus Large Treatment Firms

Dependent Variable:	Ln(1 + *MEF*)	
Treated Firms	Small	Large
	(1)	(2)
Treat-Post-NoSEO	0.083*	0.070*
	(1.70)	(1.89)
Treat-Post-SEO	0.139*	0.018
	(1.72)	(0.28)
Treat-NoSEO	-0.129**	-0.035
	(-2.29)	(-1.23)
Treat-SEO	-0.114	0.098*
	(-1.51)	(1.79)
Control variables	Yes	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes
p-value of diff. in coef. of *Treat-Post-NoSEO*	0.753	
Adj. R^2	0.303	0.268
N	3,513	7,614

This table presents difference-in-differences estimation results for the effect of the SEO deregulation on the frequency of management earnings forecasts of firms that have no SEOs separately for small versus large treatment firms. The sample period is 2003–2014, with the financial crisis period (2008–2009) excluded. The sample includes all firms with public floats between \$10 million and \$70 million (treatment firms) or between \$80 million and \$150 million (control firms). The sample of column 1 includes all control firms and small treatment firms, defined as firms with public floats and annual revenues both below \$25 million. The sample of column 2 includes all control firms and large treatment firms, defined as treatment firms that are not identified as small treatment firms. Both regressions include all firm-level controls used in Table 2 and both year and industry (four-digit SIC) fixed effects. All variables are defined in Appendix Table 10. Standard errors are clustered at the firm level. ***, **, and * denote significance at the levels of 1%, 5%, and 10%, respectively

4 Conclusion

We provide causal evidence regarding the impact of equity financing incentive on firms' disclosure decisions by exploring the 2008 seasoned equity offering deregulation, which exogenously facilitates small firms' access to public equity financing. Prior studies on the impact of equity financing incentive on corporate disclosure typically focus on the change of disclosures around actual equity issuance or the difference in disclosures of firms that issue equity versus those that do not. These studies are subject to an endogeneity concern due to the concurrent changes in business and information environments accompanying equity issuances. The SEO deregulation setting provides an ideal opportunity to provide causal evidence on the impact of equity issuance incentives on firms' disclosure decisions because it increases the likelihood of affected firms issuing equity without changing their business and information environments. It also allows for a more direct examination of firms' commitment to disclosure in anticipation of a possible equity offering.

We find that, benchmarking against a set of control firms that are not affected by the deregulation, affected treatment firms that do not issue equity increase the frequency of management earnings forecasts, especially forecasts of repeated forecasters, after the deregulation. In cross-sectional analyses, we show that the above effect is more pronounced for firms with greater equity financing needs and firms with higher information asymmetry in the equity market. Collectively, our evidence suggests that heightened equity issuance incentives lead to increased disclosures even for firms that do not issue equity for the purpose of signaling their disclosure commitment.

Our study contributes to the disclosure literature by providing causal evidence regarding the effect of equity financing incentives on firms' voluntary disclosure. We find that greater equity financing incentives lead to more disclosures even for firms that do not issue equity. This finding suggests that an increase in equity issuance incentives per se, without an increase in actual issuance, could lead to more disclosure for the purpose of signaling disclosure commitment, adding to our understanding of the impact of equity financing on disclosure. One limitation of our study is that, by design, the evidence is based on a set of relatively small firms. It is unclear whether the finding can be generalized to larger firms. Thus one should be cautious in interpreting or generalizing our results. We call for future studies to provide further evidence that is more generalizable.

Footnote 29 (continued)

ment reports on internal control to have little impact on corporate disclosure for two reasons. First, prior to that requirement, non-accelerated filers had to evaluate their internal controls and disclose any material weaknesses under Sect. 302 of SOX (Doyle et al. 2007). Second, prior research shows that the economic impacts of Sect. 404 disclosure are generally insignificant (Dechow et al. 2010).

Appendix

Table 10 Variable Definitions

Variable	Definition
Abret	Abnormal return in year *t*, measured as total buy-and-hold return excessive of market return.
Beta	Market model beta calculated using the past 36 months of data, requiring at least 12 months of nonmissing data.
Bid-Ask Spread	Average daily bid-and-ask spread in year *t-1*.
Big4	An indicator variable equal to 1 if a firm is audited by a Big Four auditor in year *t* and 0 otherwise.
ChEPS	An indicator variable equal to 1 if the change in earnings per share is positive in year *t* and 0 otherwise.
Earn Vol	Standard deviation of return on equity over the past 10 years.
Future Capex	Sum of capital expenditure from year *t+1* to year *t+3* divided by total assets at the end of year *t*.
IO	Average percentage of institutional ownership during year *t*.
Lev	Leverage ratio measured as the sum of short-term and long-term debt scaled by total assets at the end of year *t*.
Loss	An indicator variable equal to 1 if a firm's income before extraordinary items is negative in year *t* and 0 otherwise.
M&A	An indicator variable equal to 1 if a firm engages in merger and acquisition activities in year *t* and 0 otherwise.
Market-to-book	Market capitalization scaled by book value of equity at the end of year *t*.
MEF	The number of management earnings forecasts issued during the 12 months following the second quarter end of year *t*.
NonRep MEF	The number of management earnings forecasts issued during the 12 months following the second quarter-end of year *t* by non-repeated forecasters and 0 otherwise, where non-repeated forecaster is defined as a firm-year that issues forecasts in year *t* but not in year *t-1* or *t+1*.
Numaf	Number of analysts following a firm during year *t*.
PIN	Probability of informed trading in year *t-1*.
Public Float	A firm's public float at the end of the second quarter-end of year *t*, as disclosed in its 10-K.
Rep MEF	The number of management earnings forecasts issued during the 12 months following the second quarter-end of year *t* by repeated forecasters and 0 otherwise, where repeated forecaster is defined as a firm-year that issues forecasts in year *t* as well as in year *t-1* or *t+1*.
ROA	Income before extraordinary items divided by total assets at the end of year *t*.
Sales Growth	Growth rate of sales, measured as the change of sales in year *t* scaled by sales in year *t-1*.
Seg	The number of geographical and business segments in year *t*.
Size	Natural logarithm of market capitalization at the end of year *t*.
Tobin's Q	The sum of book value of liability and market value of equity scaled by total assets at the end of year *t*.
Treat-NoSEO	An indicator variable that equals 1 for firm-years with public floats below $75 million that do not issue equity through SEOs from year *t-1* to year *t+2* and 0 otherwise.

Table 10 (continued)

Variable	Definition
Treat-Post-NoSEO	An indicator variable that equals 1 for post-deregulation firm-years with public floats below $75 million that do not issue equity through SEOs from year *t-1* to year *t* + *2* and 0 otherwise.
Treat-Post-SEO	An indicator variable that equals 1 for post-deregulation firm-years with public floats below $75 million that issue equity through SEOs at least once from year *t-1* to year *t* + *2* and 0 otherwise.
Treat-SEO	An indicator variable that equals 1 for firm-years with public floats below $75 million that issue equity through SEOs at least once from year *t-1* to year *t* + *2* and 0 otherwise.
Treat-SEO-2006	An indicator variable that equals 1 for firms in 2006 with public floats below $75 million that issue equity through SEOs at least once during 2005–2008 and 0 otherwise.
Treat-SEO-2007	An indicator variable that equals 1 for firms in 2007 with public floats below $75 million that issue equity through SEOs at least once during 2006–2009 and 0 otherwise.
Treat-NoSEO-2006	An indicator variable that equals 1 for firms in 2006 with public floats below $75 million that do not issue equity through SEOs during 2005–2008 and 0 otherwise.
Treat-NoSEO-2007	An indicator variable that equals 1 for firms in 2007 with public floats below $75 million that do not issue equity through SEOs during 2006–2009 and 0 otherwise.
Unbundled MEF	The number of unbundled management earnings forecasts issued during the 12 months following the second quarter-end of year *t*, where unbundled forecasts are identified as those issued outside of the five-day window surrounding a firm's earnings announcement.

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