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Essays On The Economic Consequences Of Regulatory Monitoring

Yicheng Zhu
University of Texas at El Paso

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YICHENG ZHU

Doctoral Program in Business Administration

APPROVED:

Zuobao (Eddie) Wei, Ph.D., Chair

David Folsom, Ph.D.

Feixue (Faith) Xie, Ph.D.

Stephen L. Crites, Jr., Ph.D.
Dean of the Graduate School

ESSAYS ON THE ECONOMIC CONSEQUENCES OF REGULATORY MONITORING

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YICHENG ZHU

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Abstract

This dissertation studies the economic consequences of regulatory monitoring in two chapters. In the first chapter, using hand-collected data on Financial Industry Regulatory Authority (FINRA) violations, I examine how enforcement actions on financial institutions affect their investment bankers' role as mergers and acquisitions (M&A) advisors. I document that FINRA violations in the current year lead to a loss of M&A market share in the following year. I further find that client performance improves in future M&A deals advised by the sanctioned banks. Specifically, I document that future bidder (client) cumulative abnormal returns (CARs) are positively related to both the number of the violations and size of the monetary fines. Moreover, in future deals advised by the sanctioned investment banks, I find a decrease in premiums paid by bidders to targets and a lower likelihood of an upward offer price revision, which are value-increasing propositions for the bidders. I conjecture that after being sanctioned, investment bankers are more mindful of their behavior and increase their due diligence in advising future M&A deals, leading to better client performance. My study adds novel evidence to the existing literature that regulatory monitoring through enforcement actions on M&A advisors is effective.

In the second chapter, I examine the association between voluntary disclosure and real earnings management in seasoned equity offering (SEO) firms. The Securities Offering Reform (SOR) in 2005 eased restrictions on firm disclosure prior to equity offerings. Prior literature find a better information environment with increased disclosure and reduced information asymmetry in seasoned equity offering (SEO) firms after the regulation. Building on these findings, my paper documents that SEO firms reduce real earnings management activities after the SOR became effective. My results indicate a substitution effect between voluntary disclosure and real earnings management in SEO firms. Specifically, when restrictions on disclosure prior to equity offerings

are removed, SEO firms opt for increased disclosure and reduced level of real activities manipulation. My paper adds to the literature on earnings management activities around SEOs. It contributes to the studies on the impact of voluntary disclosure on earnings management by employing a difference-in-differences design to address the endogeneity issue in this line of research. It also has important implications for regulations on securities offerings.

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1.1. INTRODUCTION

Mergers and acquisitions (M&A) are among the most important corporate investment decisions. Corporations hire investment bankers as advisors in M&A deals for their expertise in identifying potential targets, valuing the deals, and negotiating with the targets. However, existing empirical literature is inconclusive on the impact of investment bank advisors on M&A outcomes. Relevant prior studies largely focus on whether top-tier banks are associated with better deal outcomes. While the majority of these studies find insignificant or negative links between investment bank reputation and M&A outcomes,[1] some find a positive association between top-tier advisors and higher bidder returns in public acquisitions (Golubov, Petmezas and Travlos, 2012).

The literature is largely silent on how enforcement actions on financial institutions affect their investment bankers' role as M&A advisors. This paper helps fill this gap.

In this study, I focus on the routine monitoring from Financial Industry Regulatory Authority (FINRA) on brokerage firms through regulatory sanctions. Specifically, I examine these two related questions: 1) Do FINRA regulatory sanctions imposed on a financial institution result in a loss of its M&A advisory market share? 2) How do these enforcement actions affect the outcomes of subsequent M&A deals advised by the sanctioned investment banks? Answers to these questions are helpful in my understanding of whether regulatory sanctions are effective in the M&A advisory market.

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[1] See Bowers and Miller (1990), Michel, Shaked, and Lee (1991), Servaes and Zenner (1996), Hunter and Jagtiani (2003), Rau (2000), and Ismail (2010), among others.

FINRA is an industry self-regulatory organization authorized by Congress and supervised by the Security and Exchange Commission (SEC) to regulate America's broker-dealer industry. It oversees more than 634,000 brokers across the US. FINRA conducts risk-based cycle examinations annually. With the aim of informing investors about the brokerage firms with whom they do business, FINRA publishes BrokerCheck reports that contain information about the registered brokers or brokerage firms, including the violations identified through FINRA's cycle examinations.

Regulatory monitoring through enforcement actions is effective in altering misbehavior only if imposing sanctions can negatively impact the future business of the sanctioned firms. As BrokerCheck reports are made public on FINRA's website, an investment bank's reputation will be damaged by the disclosure of violations. Large fines on financial institutions are reported in FINRA's media center as well as by the mainstream media. This could lead to diminished client confidence in the sanctioned institutions. Investment banks rely on their reputation to attract potential clients, as reputation is useful in reducing information asymmetry between investment banks and their clients. A group of "bulge bracket" firms dominate the investment banking industry based on their reputation as experts in executing M&A deals. The literature widely documents that reputational loss from regulatory enforcement is more serious than the monetary punishment itself (Karpoff, Lee, and Martin, 2008; Karpoff, 2012; Armour, Mayer, and Polo, 2017).

In this study, I focus on the loss of market share as an economic penalty for the reputational loss. The reputational damage caused by the sanctions increases information asymmetry between the investment bank and its potential clients. The cost of increased information asymmetry will manifest itself in the loss of the investment banker's M&A market share as potential clients become more cautious. Therefore, I first examine whether investment bankers' market shares are negatively affected by regulatory sanctions, as a negative impact on their future business will surely force them to correct their misbehavior.

Our results show that more severe FINRA's enforcement actions during the previous year, as measured by the number of violations and size of the monetary fines, lead to a lower M&A business market share in the following year at the sanctioned financial institutions. Specifically, an increase in the severity of enforcement actions results in a bigger loss of M&A market share. I expect that the sanctioned banks attempt to rebuild their market shares, as market share is an important determinant of their future mandates (Bao and Edmans, 2011). Both academics and industry participants use an investment bank's ranking in the League Table, which is based on market share, as a proxy for the banker's quality (Bao and Edmans, 2011; Derrien and Dessaint, 2018).

To rebuild its reputation and hence market share after being sanctioned, an investment bank must be able to produce favorable outcomes for its M&A bidder-clients.[2] I examine three measures of M&A outcomes: bidder cumulative abnormal returns (CARs), bidder premiums paid to targets, and offer price revisions. Sibilkov and McConnell (2014) find that, when choosing financial advisors, acquiring firms consider prior acquisition performance, measured as announcement period CARs. I also examine bidder premiums paid to targets and final offer price revisions and expect that increased efforts by the sanctioned banks should lead to lower premiums being paid to targets and a lower likelihood of upward offer price revisions. Lower premiums and the lower likelihood of upward offer revisions represent value added to the sanctioned bank's M&A clients.

[2] In this study, I only focus on investment banks' bidder clients.

I employ a comprehensive sample of US public, private, and subsidiary acquisitions over a nine-year period from 2010 through 2018.[3] I hand-collect the information of FINRA violations on each brokerage firm's BrokerCheck report from FINRA's website. I use the number of violations and the dollar amount of fines to proxy for the severity of misconduct by financial institutions and the intensity of regulatory monitoring and enforcement. I examine whether a greater severity of violations and the subsequent enforcement actions are associated with better M&A outcomes in subsequent deals advised by the sanctioned investment bankers.

Our results show that client performance improves, an indication that the sanctioned banks exert more effort on due diligence when advising their future M&A clients. Specifically, I find that bidder CARs are positively related to both the number of FINRA violations and size of the fines levied during the previous year. I identify two sources of value creation for the bidders. I find that premiums paid by the bidders to the targets decrease. Furthermore, the likelihood of an upward price revision also decreases. These results indicate that regulatory monitoring through enforcement actions leads to improved due diligence from financial advisors. My results are robust to different measures of the severity of enforcement actions. The results continue to hold after implementing the Heckman (1979) two-step procedure to control for potential selection biases. My results are not driven by deals advised by top-tier investment banks.

Our study makes two useful contributions to the literature. First, this paper contributes to the literature on the effectiveness of regulatory monitoring of financial institutions. While prior studies have linked regulatory violations to corporate culture (Pacelli, 2019), others have examined the effect of regulatory monitoring on sell-side analysts (Call, Sharp, and Wong, 2019). However,

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[3] FINRA was formed in 2007 and 2008-2009 happened to coincide with the global financial crisis. To minimize the potential confounding effects of the financial crisis, I decided to start the sample period from 2010.

the existing literature informs us very little about the effectiveness of regulatory monitoring of investment bankers in their role as M&A advisors. My study helps fill this literature gap by providing novel empirical evidence in this regard. My paper also joins the latest discussions on the enforcement actions and public disclosure of financial advisors' violations as market mechanisms to punish and discipline misconduct.[4] Second, this paper adds to the literature on the role of financial advisors in M&A activities. The existing M&A literature has largely focused on cross-sectional differences in deal outcomes between top-tier and non-top-tier investment banks and the findings are inconclusive. I take the view that corporate ethics matters for investment bankers when advising M&A deals and my findings support that view. Specifically, my results show that poor ethics in financial institutions as revealed in the regulatory sanctions is linked to a loss of business in the M&A market. Once disciplined by regulatory authorities, I conjecture that M&A advisors increase their effort in due diligence, leading to improved client performance.

1.2. Background, Literature Review and Hypothesis Development

1.2.1. Regulatory monitoring of financial institutions

FINRA was created by the US Congress to protect investors and ensure the integrity of the broker-dealer industry. It was formed in 2007 through the consolidation of the National Association of Securities Dealers (NASD) and the membership regulation, enforcement, and arbitration arm of the New York Stock Exchange (NYSE). As an independent and non-governmental agency under the supervision of the SEC, FINRA creates and enforces rules that regulate brokerage firms and exchange markets. To conduct business with the public in the United States, all financial institutions and individual broker-dealers must be registered with FINRA. To

[4] See Egan, Matvos, and Seru, 2019; Roychowdhury and Srinivasan, 2019; Law and Mills, 2019; Dimmock, Gerken, and Graham, 2018.

ensure broker-dealers' compliance with federal securities laws, rules, and regulations, FINRA conducts annual risk-based cycle examinations.[5] At the end of an examination, FINRA provides the firm with an examination report summarizing the risks associated with business activities in the firm and takes disciplinary actions against the firm for any identified violations.

To inform investors about the brokerage firms with which they do business, FINRA publishes BrokerCheck reports that contain information about the registered brokers or brokerage firms, including any violations identified through FINRA's cycle examinations.[6] The BrokerCheck report provides information about firm profiles, firm history, and firm operations. The details of the regulatory sanctions are disclosed in the Disclosure Events section of the BrokerCheck report. Securities regulators report the disclosed sanctions against financial institutions through the Central Registration Depository (CRD). The Disclosure Event section discloses the current status of the sanctions (pending, on appeal, or final), the allegations against the brokerage firm, the date of initiation, the docket/case number, the resolution date, and the sanctions order, along with other details of the event.

Recently, the data in BrokerCheck reports have been used in two empirical papers that study security research in the sell-side industry. Using the number of annual security code violations in BrokerCheck reports as a proxy for corporate culture, Pacelli (2019) finds that financial institutions with more FINRA violations are associated with greater agency problems. Analysts in the sanctioned institutions cater to the needs of institutional investors at the expense of individual investors. They spend more resources on broker-hosted conferences, while producing poor analyst forecasts that are more likely to be used by individual investors. Call et al. (2019)

[5] https://www.finra.org/sites/default/files/14_0453%201_What%20to%20Expect_Cycle%20Exam.pdf
[6] https://www.finra.org/investors/about-brokercheck-reports

focus on the changes in analyst forecasts following regulatory sanctions. They find that analysts in both sanctioned firms and non-sanctioned peer firms are more likely to lower their stock recommendations against a positive bias. They attribute the latter finding to industry spillover effects. These empirical studies show that FINRA monitoring is effective in identifying and disciplining problematic sell-side analysts.

Our paper differs from the aforementioned studies in several aspects. First, I examine the effect of regulatory monitoring on the investment banking side of business in a financial institution, whereas the prior papers focus on the equity research side. Second, I examine a much wider range of regulatory violations in a larger sample of financial institutions. Pacelli (2019) studies violations in 29 large conglomerates, while Call et al. (2019) study 81 violations related to equity research. I examine a full range of sanctions initiated by major US regulatory agencies that are related to lead financial advisors in all M&A deals over a nine-year period of 2010-2018. My final sample includes 762 violations committed by 44 financial advisors over the sample period.[7] Third, methodologically, my paper differs from Pacelli (2019). Pacelli (2019) studies the relation between violations and analyst forecast quality in the same year, whereas I examine the association between lagged violations and current year investment banking quality. I argue that the lagged independent variable design is a useful strategy to strengthen a causal relationship between regulatory monitoring and investment banker behavior.

1.2.2. The role of investment bankers in M&A deals

Empirical papers have found conflicting evidence regarding whether investment bankers matter for M&A deal outcomes. Using various proxies for bank quality, those papers examine whether high-quality investment banks are associated with better M&A outcomes. Using the

[7] Details of my sample selection are discussed in Section 1.3.1.

prestige of investment bankers' names as a proxy for bank quality, Bowers and Miller (1990) and Michel et al. (1991) fail to find that prestigious banks are related to better acquirer returns. Bowers and Miller (1990) show that top-tier banks are unable to convert deal synergies into a bargaining advantage. Michel et al. (1991) find that deals advised by Drexel Burnham Lambert, a less prestigious bank, outperform deals advised by First Boston, a more prestigious bulge bracket investment bank. Servaes and Zenner (1996) find that whether or not external financial advisors are used in acquisitions is not related to announcement returns after controlling for transaction characteristics. Both Hunter and Jagtiani (2003) and Ismail (2010) find that top-tier banks are associated with negative announcement returns. Using market share as a proxy for bank quality, Rau (2000) finds that top-tier banks are associated with lower acquirer announcement returns in merger deals but higher acquirer announcement returns in tender offer acquisitions. Top-tier banks complete a higher proportion of tender offers, but the deal completion rates in mergers are similar across different tiers of banks.

Contradicting the conclusion in the aforementioned studies that investment bank quality is either negatively or insignificantly related to M&A announcement returns, several studies suggest that the choice of investment bankers is relevant in M&A deals. Bao and Edmans (2011) find a significant investment bank fixed effect in announcement returns and conclude that the choice of M&A advisors has an impact on deal returns. Golubov et al. (2012) find that top-tier advisors are associated with higher bidder returns, but only in public acquisitions. Their conclusion is consistent with findings in Rau (2000) that top-tier banks are associated with higher acquirer announcement returns in tender offers, as all tender offers are public acquisitions. It is also consistent with the positive association between bank reputation and announcement returns in tender offers found in Kale, Kini, and Ryan (2003). Golubov et al. (2012) further find that top-tier advisors have no impact on deal completion rates in public or private deals, whereas top-tier banks are linked to higher deal completion rates for subsidiary acquisitions. Huang, Jiang, Lie, and Yang (2014) study the role of investment bank directors in M&A deals and find that director investment banking experience is related to higher announcement returns and lower takeover premiums. Liu (2018) finds that increased transparency in the fairness opinion valuation provided by investment banks has a positive wealth effect on shareholders.

Several researchers also examine whether prior client performance matters for the choice of investment bankers as M&A advisors. Rau (2000) finds that bank market share is unrelated to the prior acquirer performance advised by the bank but is positively related to the deal completion rate of the bank. Sibilkov and McConnell (2014) argue that when choosing advisors, acquirers consider investment banks' prior performance in acquisition deals. They show that prior client performance is positively related to the likelihood of being chosen and is also positively related to changes in the advisor's future market share.

1.2.3. Hypothesis Development

Regulatory sanctions are effective in monitoring and disciplining financial institutions only if their future business is negatively affected. If corporate ethics matters for sell-side analysts (Pacelli, 2019; Call et al., 2019), it should also matter for investment bankers at the same firm working on M&A deals. Call et al. (2019) find that regulatory monitoring has a significant inter-firm spillover effect. I believe the intra-firm spillover effect should be stronger given that corporate ethics is a firm-wide phenomenon that permeates all divisions. Therefore, I argue that once regulatory authorities sanction a financial institution, its reputation is damaged corporate-wide, which causes a negative impact on business in all divisions. For example, even though the FINRA enforcement actions may not be specifically levied against the investment banking division of a financial institution, its investment banking reputation is still negatively affected. While BrokerCheck reports are made public on FINRA's website, large fines on brokerage firms are reported in FINRA's media center as well as in other mainstream media in the US. Therefore, the reputational damage at the corporate level has an intra-firm negative spillover effect. In this paper, I focus on the effect on a financial institution's M&A advisory business, one of its most profitable business activities.

The existing literature has documented multiple forms of economic penalties for the value decrease in a firm's reputation capital. Karpoff, Lee, and Martin (2008) measure reputational penalties as lowered sales and higher contracting and financing costs. Armour et al. (2017) measure reputational damage to firms in the UK after enforcement actions by regulatory authorities for financial misconduct as a decrease in firm value. They show that the effect of regulatory sanctions on firm value is greater than the mandated fine itself. Beatty, Bunsis, and Hand (1998) attribute the decline in an underwriter's IPO market share to a deterioration in the value of its reputation capital following SEC investigations. Wang and Whyte (2010) measure an investment bank's reputation in the M&A market using its market share. Following the abovementioned literature, I focus on the decline in the sanctioned investment bank's market share as the economic penalty for the reputational loss resulted from regulatory violations (Beatty et al., 1998; Megginson and Weiss, 1991; Wang and Whyte, 2010). Investment bankers' reputation is important for their future business. Investment bankers in M&A deals serve as intermediaries between bidders and potential target companies to reduce the information asymmetry between the two parties (Chemmanur and Fulghieri, 1994). To address their own information asymmetry issue with clients, investment banks rely on their reputation. A group of "bulge bracket" firms dominate the investment banking industry based on their reputation as experts in executing M&A deals.

Imposing regulatory sanctions on an investment bank would increase information asymmetry between the bank and its potential clients, as the potential clients become less certain about the investment bank's quality and ethics. The cost of the increased information asymmetry will manifest itself in the loss of the sanctioned advisor's market share. Therefore, I expect investment bankers to experience a loss in M&A market share after being sanctioned by FINRA.

H1: *FINRA sanctions have a negative impact on M&A advisors' future market shares.*

Empirical studies have shown that investment bankers care deeply about their market shares, since a bank's market share is an important determinant for its future business opportunities (Bao and Edmans, 2011). An investment banker's market share also determines its ranking in the League Table, which is widely publicized in the financial media and routinely used by industry participants as a proxy for the bank's quality (Bao and Edmans, 2011; Derrien and Dessaint, 2018). To rebuild their reputation and thus market shares, investment bankers at the sanctioned firms are expected to exert more effort on due diligence when advising future M&A deals. Only after producing good outcomes for their clients are the sanctioned banks able to gradually rebuild their market shares and reputations.

I use three proxies of M&A deal outcomes, namely, bidder CARs, acquisition premiums, and offer price revisions. Bidder CAR on the announcement date is the most commonly used proxy for deal performance, as it captures market expectations of the value created to the bidder by the M&A deal. To rebuild reputation after being sanctioned, M&A advisors are expected to put more effort into advising deals so they can identify more suitable targets and negotiate better terms for their bidder clients, all other things being equal. Therefore, I expect bidder CARs to increase in M&A deals advised by sanctioned investment bankers.

H2: *Bidder CAR in future M&A deals advised by sanctioned investment bankers is positively related to the severity of regulatory sanctions*.

The increased effort in due diligence by the sanctioned investment bankers is likely to produce more suitable targets and better deal terms for their bidder-clients. As such, I hypothesize that the bidders in these deals will pay lower premiums to the targets in the year following FINRA sanctions.

H3: *The acquisition premium paid to targets in future M&A deals advised by sanctioned investment bankers is negatively related to the severity of regulatory sanctions*.

Sanctioned banks' increased effort in due diligence will also manifest itself in the decreased likelihood of an upward offer price revision in future deals given that as financial advisors, they are supposed to lower the offer price for their clients. Therefore, I hypothesize that the likelihood of an upward price revision from the announcement date initial offer price to the resolution date final offer price decreases in the year following sanctions against the deal advisors.

H4: *The likelihood of an upward price revision in future M&A deals advised by the sanctioned investment banks is negatively related to the severity of regulatory sanctions*.

1.3. DATA AND METHODOLOGY

1.3.1. Sample selection

I collect a sample of acquisitions over the period of January 1, 2010 to December 31, 2018, from the Thomson Reuter SDC Platinum Mergers and Acquisitions Database. Panel A of Table 1.1 summarizes my sample selection process. I start with a sample of US domestic deals, including both successful and unsuccessful deals classified as "Acquisition," "Acquisition of Assets," "Acquisition of Majority Interest," and "Merger" by SDC Platinum. I impose the following filters in my sample construction process. To ensure a transfer of control in the deals, I follow prior literature to exclude deals in which the bidder owned more than 50% of the target shares six months prior to the announcement or was seeking to own less than 50% of target shares after the transaction. I further exclude transactions with a deal value less than ten million dollars and require targets to be US public, private, or subsidiary firms. I require each deal to have at least one financial advisor on the bidder side as reported by SDC Platinum. The same financial advisor may have different names reported by the SDC, and I manually adjust for that. When one investment bank acquires another, I assign the financial advisor under the name of the target bank to the acquiring bank. When there are multiple bidder financial advisors reported for a deal, I pick the first listed advisor on the SDC Platinum as the lead financial advisor.[8] I require the acquirer to be covered in the Center for Research in Security Prices (CRSP) and Compustat.[9] Finally, I require a financial advisor to be the lead financial advisor in at least eight deals within the nine-year period in my sample.[10] My final sample contains 2,068 deals and 44 lead financial advisors. Panel B of Table 1.1 provides descriptive statistics on the number and nature of M&A deals each year over my sample period. Table 1.2 lists the financial advisors in my sample.

[8] The names of advisors are not listed in alphabetical order. Literature using the SDC New Issuance database use the same method to identify lead managers.

[9] In my advisor market share analysis, to preserve as many deals as possible, I only require acquirers to have CRSP data to calculate CARs. I do not require the acquirer to be covered by Compustat since bidder firm characteristics are not used in analyses.

[10] Bao and Edmans (2011) require an M&A advisor to have at least ten deals over a period from 1980-2007. In my study, if I filter in financial advisors with at least seven deals, I would have 48 advisors in my final sample, 10 of which with no violations (21%). Using eight deals leaves us with 44 advisors, 10 of which with no violations (22%). Using nine deals as a filter leaves us with 36 advisors, still 10 of which with no violations (28%). My overall results are largely consistent if I use either seven or nine-deal filter.

1.3.2. Measure of enforcement actions intensity

To measure the severity of enforcement actions, I rely on the regulatory sanctions reported as disclosure events in BrokerCheck reports.[11] The BrokerCheck report discloses sanctions against the financial institution as reported by securities regulators through Central Registration Depository (CRD). I download the BrokerCheck report from the FINRA website for each of the 44 financial advisors in my sample.[12] I manually read the allegations and hand-collect information on the types of violations in the disclosure event section of each report. Following Pacelli (2019), I focus on sanctions initiated by the main regulatory agencies, including FINRA, the NASD, the NYSE, and the SEC. I count only completed disclosure events with a disclosed docket/case number and an indicated fine. In my final sample, I have 762 disclosure events (violations) for the 44 financial advisors over the nine-year period from 2010 to 2018. Panel A of Table 1.2 presents the total number of violations and the total dollar amount of fines levied against each financial institution during the sample period. Panel B of Table 1.2 reports the number of violations and dollar amount of fines during each sample year.

I categorize disclosure events based on the security code violations mentioned in the allegations on the BrokerCheck report (Pacelli, 2019). I identify 1,684 unique security code violations among the 762 disclosure events. Panel A of Table 1.3 reports the distribution of the number of security code violations identified in a disclosure event. As shown, most disclosure events indicate at least one security code violation. Almost half of the disclosure events indicate more than one security code violations. Panel B of Table 1.3 lists the most frequently violated security codes. Likely due to the difficulty of regulating equity research and investment banking

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[11] For an example of a BrokerCheck report, see https://files.brokercheck.finra.org/firm/firm_7059.pdf.

[12] https://brokercheck.finra.org/

businesses in brokerage houses, most of the rules regulate practices in the trading division. Other rules, like NASD 3010, are broader in scope and are applicable to employees in all divisions of the financial institution. As shown in Panel B, the most frequently violated rules identified in my study are consistent with findings in Pacelli (2019) and by FINRA. The top three types of violations are identified in around 20% of the disclosure events in my sample and are related to "Standards of Commercial Honor and Principles of Trade" (FINRA 2010 and NASD 2110) and "Supervision" (NASD3010). Appendix A provides examples of the BrokerCheck reports that include violations of the top security code violations.

I construct two variables to measure the severity of enforcement actions. The first measure, Ln#Violations, is defined as the logarithm of one plus the number of annual violations reported in BrokerCheck reports. The second, Ln$Fines, is defined as the logarithm of one plus the dollar amount of annual fines reported in BrokerCheck reports. These two measures are the key explanatory variables in my multivariate regression analyses.

1.3.3. Multivariate analysis

1.3.3.1. The effect of enforcement actions on M&A advisors' market share

As I argue earlier, regulatory monitoring is effective only if it can inflict significant costs on financial institutions for misconduct. An investment bank's reputation is its most important asset. The reputation cost imposed by regulatory sanctions may manifest itself in lost future market share in the M&A market. In this study, I examine how regulatory sanctions affect banks' future market share in the M&A market. Following prior literature, I measure market share by total value of deals advised by the investment bank (Wang and Whyte, 2010; Bao and Edmans, 2011). Rau (2000) and Bao and Edmans (2011) find that investment bank's market share is strongly related to its prior market share. Therefore, I include lagged market share and bank fixed effects in my regression model. Specifically, I test H1 by estimating the following multivariate regression model:

$$Market\ share_{j,t} = \beta_0 + \beta_1 Violations_{j,t-1} + \gamma \Sigma Controls_{j,t-1} + Bank_j + Year_t + \epsilon \quad (1)$$

where subscript j denotes investment bank advisor j. Violations is measured as either Ln#Violations or Ln$Fines, as defined earlier. H1 predicts a negative and significant value for the coefficient of Violations (β1). The control variables are the same as those used in Rau (2000) and Sibilkov and McConnell (2014). I also include year fixed effects and cluster my standard errors at both the bank and year level to account for two dimensions of within-cluster correlation (Peterson, 2009).

In an alternative specification, I follow Sibilkov and McConnell (2014) and focus on changes in a bank's future market share, instead of the level of its future market share. Specifically, I employ a change-in-change model to examine how changes in the severity of regulatory sanctions affect changes in the bank's future market share.

1.3.3.2. The effect of enforcement actions on M&A outcomes

Bidder CARs

To test H2, I examine whether more severe enforcement actions lead to higher bidder CARs in OLS regression analysis. The dependent variable, bidder CAR, is measured as the cumulative abnormal return of the bidder stock in the five-day event window (-2, +2) around the deal announcement date. The benchmark return is estimated from the market model over the period beginning 240 days and ending 41 days prior to the announcement date, using CRSP value-weighted return as the market return. I estimate the following model:

$$CAR_k = \beta_0 + \beta_1 Violations_{j,t-1} + \gamma_1 \Sigma Firm\ Characteristics_{i,t-1} + \gamma_2 \Sigma Deal\ Characteristics_k + Bank_j + Industry_m + Year_t + \epsilon, \tag{2}$$

where subscripts k, i, and j denote M&A deal k, bidder i, and investment bank advisor j, respectively.

Our main explanatory variable is lagged violations (Violations_(j,t-1)), equal to Ln#Violations or Ln$Fines, as defined earlier. According to Bao and Edmans (2011), bidder CAR analysis faces a performance attribution challenge as CARs in acquisitions may be attributed to the acquirer or the financial advisor. Some prior studies attribute the entire CAR to financial advisors without controlling for either acquirer characteristics or deal characteristics in their bidder CAR analysis (Bowers and Miller, 1990; Michel et al., 1991; Rau, 2000; Hunter and Jagtiani, 2003). This may cause an over-attribution in deals where the target is determined by the acquirer and financial advisors only serve as a deal executor (Bao and Edmans, 2011). Other studies only control for deal characteristics. This may cause an under-attribution in cases where some deal characteristics are the choices of financial advisors (Servaes and Zenner, 1996; Kale et al., 2003). I follow relevant empirical studies to control for both firm characteristics, including LnSize, ROA, Herfindahl, BTM, Run-up, Sigma, Leverage, and Liquidity; and deal characteristics, including Non-Tender, Public Target, Relative Size, Payment Incl Stock, All-Cash Deal, Focus Deal, Complete Deal, Complex Deal, and Hostile.[13] I also control for bank, industry, and year fixed

[13] Moeller, Schlingemann, Stulz, and Press (2004), Masulis, Wang, and Xie (2007), Dong, Hirshleifer, Richardson, and Teoh (2006), Rosen (2006), Moeller, Schlingemann, and Stulz (2007), Maloney et al. (1993), Lang, Stulz, and Walkling (1991), Jensen and Ruback (1983), Schwert (1996), Loughran and Vijh (1997), Fuller, Netter, and Stegemoller (2002), Capron and Shen (2007), Morck, Shleifer, and Vishny (1990); Servaes (1991).

effects when estimating Eq. (2). I cluster standard errors at both the bidder and year level. I expect the coefficient of Violations ($\beta 1$) to be significant and positive.

Endogeneity and selection bias

Our interpretation of the empirical results may be biased by the endogeneity issues arising from two aspects. First, the arrival of regulatory sanctions may not be exogenous. Recent literature documents that some brokerage firms consistently have a higher level of misconduct as they are located in regions with more unsophisticated customers or hire less-reputable auditors (Egan et al., 2019; Cook, Kowaleski, Minnis, Sutherland, and Zehms, 2020). Second, since large financial institutions are more likely to be subject to closer regulatory scrutiny, the number of violations and total dollar amount of fines are naturally greater in larger financial institutions than in smaller ones (Pacelli, 2019). Since there is a significant overlap between top-tier investment banks and large financial institutions, my measure of the severity of violations may also capture the effect of investment bank reputation. I employ two empirical strategies to address these endogeneity concerns. First, I control for bank fixed effects in all my multivariate regressions regarding M&A outcomes. Bank fixed effects absorb time-invariant characteristics of a financial institution such as its location and target customers, as well as its industry experience and historical business relations. Second, in my robustness check, I exclude deals advised by top financial institutions and re-estimate my baseline regressions. I classify top institutions using the list of financial conglomerates identified by Pacelli (2019).

In addition, firms in the setting of my study make two non-random decisions: the decision to acquire and the choice of M&A advisors. As such, my results may be influenced by sample selection biases (Li and Prabhala, 2007). To address these selection bias, I employ the Heckman (1979) two-stage procedure.

Acquirer premiums paid to target

To test H3, I examine whether more-severe regulatory violations lead to lower premiums paid by bidders to targets. According to Eaton, Liu, and Officer (2019), a premium measured using a fixed window beginning 105 trading days prior to the announcement date is insignificantly different from one measured using a private deal initiation date. Based on this, my dependent variable Premium is measured as the difference between an acquirer's offer price and the target's stock price 105 trading days prior to the deal announcement date divided by the latter. I control for both firm and deal characteristics. I estimate the following model with bank and industry fixed effects and standard errors clustered at both the bidder and year level. I expect the coefficient of Violations (β1) to be significant and negative.

$$Premium_k = \beta_0 + \beta_1 Violations_{j,t-1} + \gamma_1 \Sigma Firm\ Characteristics_{i,t-1} + \gamma_2 \Sigma Deal\ Characteristics_k + Bank_j + Industry_m + \epsilon. \tag{3}$$

Offer price revision

To test H4, I examine whether more-severe regulatory violations lead to a lower likelihood of upward offer price revisions. The dependent variable Price Revision is equal to one if the final offer price is greater than the initial offer price and zero otherwise. I control for deal characteristics that previous papers found to cause offer price revisions, including a Hostile dummy as a proxy for target resistance and Multiple Bidder in deals involving multiple bidders (Bates and Becher, 2017). I estimate the following model with bank and industry fixed effects and standard errors clustered at both the bidder and year level. I expect the coefficient of Violations (β1) to be significant and negative.

$$\Pr(Price\ Revision_k = 1) = f\big(\beta_0 + \beta_1 Violations_{j,t-1} + \gamma_1 \Sigma Firm\ Characteristics_{i,t-1} + \gamma_2 \Sigma Deal\ Characteristics_k + Bank_j + Industry_m + \epsilon\big). \quad (4)$$

1.3.4. Summary statistics

Table 1.4 provides summary and descriptive statistics at both the firm (bidder, Panel A) and deal (Panel B) levels. All variables are defined in Appendix B. As shown in Panel A, the mean (median) bidder Size in my full sample is $13,593.7 ($2,600.3) million, which is greater than the mean (median) bidder Size in most previous M&A studies. This is due to the nature of my study, which examines the effect of financial advisors' FINRA violations on M&A outcomes. I include only deals that are advised by financial advisors who have advised at least eight deals during my sample period. Large corporations are more likely than small ones to seek large investment banks to advise their M&A deals. Panel A also shows that the mean (median) bidder Size is greater in the subsample for top financial institutions than that for non-top financial institutions. The mean (median) bidder Size in my subsample for deals advised by non-top financial institutions is $9,474.5 ($1,724.3) million, which is similar to the mean (median) bidder Size in most previous M&A studies (Table 1.4, Panel A, column (3)). To rule out the possibility that my results are biased and driven by a group of large bidders, I control for bidder size in all my regression analyses. In addition, I re-estimate my main results in the subsample of deals advised by non-top financial institutions in my robustness check, as large bidders are more likely to hire one of the top investment banks to advise their M&A deals.

The mean (median) ROA is 0.024 (0.021) for the full sample, as shown in Panel A. Bidders in the top financial institution subsample are more profitable than those in the non-top institution subsample. The mean (median) BTM ratio is 0.647 (0.534), while the mean (median) Leverage ratio is 0.301 (0.281). These numbers are similar across the two subsamples.

As shown in Panel B, the mean (median) deal size is $2,090.2 ($441.1) million, which is relatively large, again due to the nature of my sample selection criteria. Deals advised by top institutions have a mean (median) of $2,632.2 ($734.4) million, significantly larger than the deals advised by non-top institutions, which have a mean (median) of $1,702.0 ($278.0) million. The mean (median) bidder five-day window CAR in my full sample is 0.016 (0.004), which is consistent with recent studies that find slightly positive and increased CARs in the 2010s (Netter, Stegemoller, and Wintoki, 2011; Alexandridis, Antypas, and Travlos, 2017). The mean (median) bidder five-day window CAR in my subsample of public acquisitions (untabulated) is -0.002 (-0.003), which is consistent with negative bidder announcement returns found in prior studies that focus on US public acquisitions.[14] The mean (median) acquisition premium is 0.410 (0.354), close to what previous studies find. Table 1.5 shows the pairwise correlations between the main variables used in my main analyses.

1.4. EMPIRICAL RESULTS

1.4.1. The effect of enforcement actions on M&A advisors' market shares

I first examine how the severity of regulatory sanctions affects an investment bank's future market share. I estimate Eq. (1) and report the regression results in Table 1.6. The coefficients (β1) of my main independent variables, Ln#Violations and Ln$Fines, are significant and negative. Consistent with H1, I find that FINRA sanctions have a negative impact on M&A advisor's future market shares.

[14] See Jensen and Ruback (1983), Jarrell, Brickley, and Netter (1988), Andrade, Mitchell, and Stafford (2001).

I also use an alternative specification and focus on changes in a bank's future market share. I start with a univariate test of market share change from year t-1 to t between M&A advisors who experience positive versus negative changes in violation severity from year t-2 to t-1. The results reported in Table 1.7 Panel A show that financial advisors who experience an increase in the number of violations (higher dollar amount of fines) during the previous year experience a loss of market share in the following year. On the other hand, those who experience a decline in the number of violations (lower dollar amount of fines) during the previous year exhibit a gain in market share in the following year. The differences in market share changes between these two groups of advisors are statistically significant.

I then conduct multivariate regression by regressing changes in market share on changes in the magnitude of violations; the results are reported in Panel B of Table 1.7. As shown, the regression results are largely consistent with those in my univariate analysis. The coefficient of my main independent variable, ΔLn#Violations (ΔLn$Fines) is significant and negative, consistent with the prediction of H1. These results indicate that FINRA sanctions against a financial institution lead to a negative change in the market share of its investment banking business in the M&A market.

As documented in Sibilkov and McConnell (2014), potential clients pay attention to an investment banker's prior client performance when choosing an M&A advisor. To rebuild the lost market share caused by FINRA sanctions, investment bankers are expected to exert greater effort in advising future M&A deals to produce favorable outcomes for their clients. In the next subsections, I examine whether FINRA sanctions lead to better client performance in the year following the sanctions.

1.4.2. Bidder CARs

I first conduct a multivariate regression analysis of bidder CAR by estimating Eq. (2). Table 1.8 presents the results. The coefficients of Ln#Violations and Ln$Fines are significant and positive in all model specifications. Interpreted in economic terms, a one standard deviation increase in Ln#Violations (Ln$Fines) leads to a 0.99% (0.68%) increase in bidder CAR for the average bidder in the sample.[15] The results are consistent with H2—after being sanctioned by FINRA, investment bankers increase their advising efforts in future deals, resulting in increased bidder CAR for their bidder clients.

Most of the control variables have the predicted signs. Acquirers with higher leverage and lower run-up obtain higher announcement returns (Maloney et al., 1993; Rosen, 2006). Deals involving private targets usually have higher announcement returns than public targets, while stock acquisitions are usually associated with lower announcement returns (Fuller et al., 2002; Capron and Shen, 2007).

1.4.3. Acquisition premiums

Next, I examine whether future acquisition premiums paid by bidders to targets are reduced in deals advised by sanctioned investment bankers. I estimate Eq. (3) and report the OLS regression results in Table 1.9. The coefficients (β1) of my main independent variables, Ln#Violations and Ln$Fines, are significant and negative. A one standard deviation increase in Ln#Violations (Ln$Fines) leads to a 6.02% (6.81%) decrease in the acquisition premium, both of which are strongly economically significant.[16] Consistent with H3, I find that the premium paid by bidders is negatively associated with the intensity of FINRA monitoring and enforcement. This is

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[15] 0.99%=0.8992 ($\sigma(Ln\#Violations)$)*0.011; 0.68%=6.8102($\sigma(Ln\$Fines)$)*0.001.

[16] $6.02\% = 0.8992\big(\sigma(Ln\#Violations)\big) * (0.067);\ 6.81\% = 6.8102\big(\sigma(Ln\$Fines)\big) * (0.010)$.

consistent with my expectation that sanctioned investment bankers improve their due diligence so that their bidder-clients are less likely to overpay.

1.4.4. Offer price revision

Finally, I examine whether the likelihood of bidder-clients paying a final offer price that is higher than the initial offer price is reduced in deals advised by sanctioned investment bankers. I estimate Eq. (4) and report the logic regression results in Table 1.10. The results show that the coefficients (β1) of my main independent variables, Ln#Violations and Ln$Fines, are significant and negative. This is consistent with my hypothesis (H4) that after being sanctioned, M&A advisors increase their due diligence efforts in future deals, leading to a lower likelihood that bidder-clients will pay a higher final offer price than the initial offer price.

Most control variables have the predicted signs. The likelihood of an upward offer price revision is higher in hostile takeovers and in deals involving competing bidders (Bates and Becher, 2017).

1.4.5. Heckman two-stage procedure

As mentioned earlier, my sample firms make two non-random decisions: the decision to acquire and the choice of M&A advisors. These non-random decisions contribute to sample selection biases (Li and Prabhala, 2007). To address these biases, I employ the Heckman (1979) two-stage procedure.

To address the first type of selection bias, I model the probability of making an M&A decision in the first stage. The explanatory variables include LnAssets, MTB, Leverage, Liquidity, and ROA. I also control for industry and firm fixed effects. I estimate the Inverse Mills Ratio from the first-stage regression as the fitted value of the logistic model. I include the Inverse Mills Ratio in my second-stage regressions where I re-estimate Eq. (2). The results presented in Table 1.11

columns (1) – (3) are largely consistent with those from my baseline analyses. I conclude that my results are not driven by the selection bias of firms that conduct M&A deals.

To address the second type of selection bias, I estimate the choice of financial advisors between top financial institutions and non-top financial institutions in the first stage. In this model, I need to have a variable that affects the choice of advisor but does not affect the outcome variables in my main regressions. Following Fang (2005) and Golubov et al. (2012), I construct a variable, Score, which measures the extent to which a firm used top financial institutions as equity or debt issuance underwriters or M&A advisors in previous years. My data on the issuance of common stocks and non-convertible bonds are from the SDC platinum Global New Issues Database. For each M&A deal in my sample, I define Score as follows. Over the past five years prior to the announcement year, Score equals one if the acquirer uses top financial institutions for one of the three types of businesses: equity issuance, debt issuance, and M&A; Score equals two if the acquirer uses top financial institutions for two of the aforementioned three types of businesses; Score equals three if the acquirer uses top financial institutions for all three types of businesses; Score equals zero if the acquirer never uses a top financial institution for any of the three types of businesses. I control for LnSize, BTM, Leverage, Liquidity, Run-up, Sigma, All-Cash Deal, Focus Deal, and Hostile in the first-stage logistic model. I estimate the Inverse Mills Ratio from the first-stage regression and include it in my second-stage regressions. In the second stage, I re-estimate Eq. (2) on a subsample excluding M&A deals advised by top financial institutions to account for the possibility that my results are driven by a group of top financial institutions. The results reported in Table 1.11 columns (4) – (6) are largely consistent with those found in my main analyses. I show that my results are not driven by a sample of M&A deals advised by top financial advisors.

1.5. Conclusions

Using hand-collected data on FINRA violations, I examine how regulatory monitoring of financial institutions affects their investment bankers' role as M&A advisors. Employing a comprehensive sample of US public, private, and subsidiary acquisitions over a nine-year period from 2010-2018, I find that more severe enforcement actions during the previous year, measured as the number of violations and the dollar amount of fines, leads to a lower M&A business market share in the following year at the sanctioned institutions. I argue that the publicized FINRA sanctions damage the reputation of the financial institutions, leading to a loss of its business in the M&A market.

To rebuild their reputations and hence M&A market share, the sanctioned banks increase their due diligence efforts when advising future M&A deals. I expect that this increased effort leads to improved deal outcomes for their clients (bidders). Indeed, my results show that client performance improves in future M&A deals advised by the sanctioned financial institutions. Specifically, I find that bidder CARs are positively related to both the number of FINRA violations and the size of fines. Moreover, I find that both acquisition premiums paid by bidders and the likelihood of an upward offer price revision are negatively related to the severity of violations.

In conclusion, my study provides strong evidence that regulatory monitoring is effective in that it inflicts reputational cost on the sanctioned financial institutions in the form of lost future M&A market shares. Furthermore, regulatory monitoring is effective in that it alters the sanctioned investment bankers' behavior to the benefit of their bidder clients.

Table 1.1: Sample Construction

Panel A: Sample Selection

Filters	# of Deals
Domestic deals including both successful and unsuccessful deals classified as "Acquisition," "Acquisition of Assets," "Acquisition of Majority Interest," and "Merger": January 1, 2010 to December 31, 2018	83,523
Bidders: US public firms	19,679
Percentage of target shares bidder owned six months prior to the announcement <= 50% and percentage of target shares bidder seeking to own after the transactions > 50%	18,961
Deal value: >= $ 10 million	7,556
Targets: US public, private, or subsidiary firms	7,506
Each deal has at least one financial advisor on the bidder side as reported by the SDC Platinum	2,844
Bidder covered in Center for Research in Security Prices (CRSP) and Compustat	2,344
Financial advisor must be the lead financial advisor in at least eight deals within the nine-year period	2,068

Panel B: Sample Distributions

This panel provides statistics on the number of deals by year and by deal type for a sample of US public, private, and subsidiary acquisitions announced over 2010-2018.

	Full Sample	Complete	Withdrawal	Public Target	Private Target	Subsidiary Target	Tender Offers	Non-Tender Offers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2010	192	181	10	83	41	68	15	177
2011	168	152	14	57	47	64	8	160
2012	223	220	3	70	66	87	14	209
2013	225	215	8	81	55	89	8	217
2014	269	255	13	99	70	100	11	258
2015	277	252	15	112	73	92	16	261
2016	252	241	3	102	58	92	15	237
2017	232	208	10	83	67	82	13	219
2018	230	212	13	100	75	55	5	225
Total	2,068	1,936	89	787	552	729	105	1,963

Table 1.2: Sample of Financial Institution Violations

Panel A: This table provides descriptive statistics of total violations of financial advisors in my sample from 2010 to 2018. Column (1) shows the total number of violations and column (2) shows the total dollar amount of violations for each financial advisor during the sample period. Column (3) reports if the investment bank is classified as belonging to a top financial institution.

Financial Institution	CRD Number	# Violations (1)	$ Fines (2)	Top Institution (3)
Allen & Company LLC	1042	1	16,000	
Merrill Lynch, Pierce, Fenner & Smith Incorporated	7691	104	619,604,092	
Barclays Capital Inc.	19714	50	106,401,500	
BMO Capital Markets Corp.	16686	8	250,925	
Boenning & Scattergood, Inc.	100	5	412,500	
Canaccord Genuity LLC	1020	21	907,500	
Centerview Partners LLC	133796	0	0	
Citigroup Global Markets Inc.	7059	64	145,542,028	YES
Cowen and Company	7616	5	195,000	
Credit Suisse Securities (USA) LLC	816	51	112,707,500	YES
D.A. Davidson & Co.	199	10	1,315,000	
Deutsche Bank Securities Inc.	2525	50	80,232,586	YES
Duff & Phelps Securities LLC	36927	0	0	
Evercore Group L.L.C.	42405	0	0	
B. Riley FBR, Inc.	25027	1	10,000	
Fig Partners, LLC	41554	2	37,500	
Greenhill & Co., LLC	40290	0	0	
Griffin Financial Group, LLC	119004	0	0	
Goldman Sachs & Co. LLC	361	45	118,362,755	YES
Guggenheim Securities, LLC	40638	8	911,000	
Houlihan Lokey Capital, Inc.	17708	0	0	
Jefferies LLC	2347	19	8,004,652	
J.P. Morgan Securities LLC	79	53	149,774,700	YES
Keefe, Bruyette & Woods, Inc.	481	3	87,500	
Keybanc Capital Markets Inc.	566	11	1,488,500	
Lazard Freres & Co. LLC	2528	0	0	

Macquarie Capital (USA) Inc.	36368	3	2,979,000	
Moelis & Company LLC	145115	0	0	
Morgan Stanley & Co. LLC	8209	42	126,323,118	YES
Needham & Company, LLC	16360	1	2,500,000	
Perella Weinberg Partners LP	138618	1	60,000	
PJ Solomon Securities, LLC	28041	0	0	
Piper Sandler & Co.	665	9	1,602,500	
Raymond James & Associates, Inc.	705	24	11,470,000	
RBC Capital Markets, LLC	31194	35	33,533,500	
Robert W. Baird & Co. Incorporated	8158	13	2,024,000	
Sandler, O'neill & Partners, L.P.	23328	2	82,500	
Stephens	3496	8	1,661,500	
Sterne, Agee & Leach, Inc.	791	14	1,102,500	
Stifel, Nicolaus & Company, Incorporated	793	28	4,035,000	YES
Tudor, Pickering, Holt & Co. Securities, LLC	129772	0	0	
UBS Securities LLC	7654	45	45,337,000	YES
Wells Fargo Securities, LLC	126292	16	22,661,000	YES
William Blair	1252	10	5,063,000	
Total	44	762	1,606,695,856	

Panel B. This table reports the number of disclosure events and dollar amount of fines by year

Year	# Disclosure events	$ Fines
2010	59	18,487,500
2011	72	80,039,750
2012	96	136,409,905
2013	93	84,125,000
2014	104	214,190,302
2015	96	244,595,496
2016	113	637,829,334
2017	72	106,185,424
2018	57	84,833,145
Total	762	1,606,695,856

Table 1.3: Characteristics of Security Code Violations

Panel A: Frequency of Security Code Violations

This table reports the distribution of the number of security code violations identified in a disclosure event.

# Security Codes	# Disclosure Events	% Disclosure Events
0	223	29%
1	178	23%
2	112	15%
3	82	11%
4	57	7%
5	38	5%
>5	71	10%
Total	762	100%

Panel B: Examples of Security Code Violations

This table shows the ten most frequent security code violations in all disclosure events

No.	Security Code	Security Code Description	# Security Code Violations	% Disclosure Events
1	FINRA 2010	Standards of Commercial Honor and Principles of Trade	142	19%
2	NASD 2110	Standards of Commercial Honor and Principles of Trade	139	18%
3	NASD 3010	Supervision	129	17%
4	THE EXCHANGE ACT 17A-3	Records to be made by certain exchange members, brokers, and dealers	51	7%
5	THE SECURITIES ACT 17(A)	Fraudulent interstate transactions	50	6%
6	FINRA 6730	Transaction Reporting	49	6%
7	FINRA 7450	Order Data Transmission Requirements	40	5%
8	NASD 3110	Books and Records	40	5%
9	THE EXCHANGE 10B-10	Confirmation of transactions	38	5%
10	THE EXCHANGE ACT 15B	Manner of registration of brokers and dealers	37	5%

Table 1.4: Summary Statistics

This table presents summary statistics on bidder and deal characteristics. Panel A provides the mean, median, and standard deviation for bidder characteristics. Panel B provides the mean, median, and standard deviation for deal characteristics. Appendix B provides the definitions of variables. Column (1) presents statistics for deals in my full sample. Column (2) presents statistics for a subsample of deals advised by top financial institutions. Column (3) presents statistics for a subsample of deals advised by non-top financial institutions.

Panel A: Bidder Characteristics												
	Full Sample (1)				Deals Advised by Top Institutions (2)				Deals Advised by Non-Top Institutions (3)			
	N	Mean	Median	Std. dev.	N	Mean	Median	Std. dev.	N	Mean	Median	Std. dev.
Size ($mil)	2068	13593.7	2600.3	35860.1	863	19345.2	4647.5	43963.7	1205	9474.5	1724.3	27985.8
ROA	2068	0.024	0.021	0.099	863	0.032	0.031	0.096	1205	0.019	0.013	0.101
BTM	2068	0.647	0.534	1.201	863	0.590	0.442	0.892	1205	0.687	0.615	1.380
Leverage	2068	0.301	0.281	0.196	863	0.295	0.260	0.203	1205	0.304	0.290	0.191
Liquidity	2068	0.100	0.053	0.125	863	0.111	0.068	0.129	1205	0.093	0.046	0.123
Herfindahl	2068	0.206	0.148	0.200	863	0.236	0.179	0.211	1205	0.185	0.115	0.189
Run-up	2068	0.060	0.024	0.307	863	0.063	0.026	0.336	1205	0.058	0.024	0.284
Sigma	2068	0.017	0.015	0.009	863	0.016	0.014	0.008	1205	0.017	0.015	0.009
Panel B: Deal Characteristics												
Deal Value	2068	2090.2	441.1	7483.5	863	2632.2	734.4	7080.6	1205	1702.0	278.0	7738.8
Relative Size	2068	0.510	0.198	2.246	863	0.470	0.204	1.957	1205	0.539	0.196	2.432
CAR (-2,+2)	2068	0.016	0.004	0.158	863	0.006	0.002	0.087	1205	0.023	0.005	0.193
Premium	581	0.410	0.354	0.362	269	0.410	0.351	0.365	312	0.410	0.356	0.360
Price Revision	747	0.078	0	0.268	326	0.092	0	0.290	421	0.067	0	0.249

Table 1.5: Correlation Matrix

This table presents correlations of variables used in main analyses. Appendix B provides the definitions of variables.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1)	Ln#Violations	1.00								
(2)	Ln$Fines	0.92	1.00							
(3)	CAR	0.01	-0.02	1.00						
(4)	Premium	-0.06	-0.04	-0.09	1.00					
(5)	LnSize	0.34	0.31	-0.10	-0.03	1.00				
(6)	ROA	0.13	0.12	-0.03	-0.02	0.29	1.00			
(7)	BTM	-0.03	-0.01	-0.30	-0.00	-0.05	0.01	1.00		
(8)	Leverage	-0.11	-0.08	0.02	0.04	-0.26	-0.21	-0.03	1.00	
(9)	Liquidity	0.07	0.04	0.07	0.02	0.10	0.01	-0.09	-0.50	1.00
(10)	Hirfindahl	0.21	0.19	0.02	0.00	0.24	0.14	-0.08	-0.17	0.10
(11)	Run-up	0.09	0.10	-0.06	0.05	0.03	-0.06	-0.01	0.14	-0.04
(12)	Sigma	-0.03	-0.06	0.12	0.10	-0.41	-0.30	-0.23	0.12	0.22
(13)	Relative Size	0.07	0.04	0.15	-0.06	-0.17	-0.13	-0.20	0.16	0.03
(14)	Public Target	0.01	0.01	0.03	-0.02	0.02	0.02	-0.04	-0.07	0.04
(15)	Non-Tender	-0.09	-0.06	-0.03	-0.06	-0.21	-0.13	0.05	0.24	-0.25
(16)	Focus Deal	0.10	0.10	0.03	0.01	-0.03	-0.05	-0.05	0.08	0.00
(17)	All Cash Deal	0.02	0.02	0.04	0.09	0.01	0.01	-0.00	-0.06	0.05
(18)	Hostile	0.04	0.01	0.01	0.03	0.05	-0.07	-0.05	0.04	0.03
		(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
(10)	Hirfindahl	1.00								
(11)	Run-up	-0.01	1.00							
(12)	Sigma	0.04	0.12	1.00						
(13)	Relative Size	0.04	-0.01	0.26	1.00					
(14)	Public Target	0.05	-0.01	0.01	0.02	1.00				
(15)	Non-Tender	-0.15	0.04	-0.01	0.11	-0.03	1.00			
(16)	Focus Deal	-0.19	0.08	0.07	0.08	0.05	-0.01	1.00		
(17)	All Cash Deal	0.01	-0.06	-0.03	-0.10	0.03	-0.17	-0.05	1.00	
(18)	Hostile	0.08	0.04	0.08	0.23	0.02	-0.13	0.07	-0.02	1.00

Table 1.6: Regression Analysis of Financial Advisor Market Share

This table reports the results of advisor-level OLS regression analysis of market share on FINRA violations for a sample of financial advisors in U.S. public, private, and subsidiary acquisitions announced over 2010-2018. The dependent variable in all columns is the financial advisor market share. The main independent variable in columns (1) and (2) is the logarithm of one plus the number of FINRA violations of the financial advisor in the prior year. The main independent variable in columns (3) and (4) is change in the logarithm of one plus the dollar amount of FINRA violation fines of the financial advisor in the prior year. Standard errors are clustered at the bank-year level. Variables are defined in Appendix B.

VARIABLES	(1)	(2)	(3)	(4)
Ln#Violations	-0.011**	-0.011**		
	(-2.409)	(-2.423)		
Ln$Fines			-0.001**	-0.001**
			(-2.579)	(-2.381)
Lagged Mkt Shr	-0.117	-0.116	-0.130*	-0.131*
	(-1.538)	(-1.583)	(-1.771)	(-1.830)
EWCAR	-0.019*		-0.025***	
	(-1.965)		(-2.926)	
VWCAR		0.024		0.012
		(0.681)		(0.344)
%Completed	0.002	0.001	0.003	0.002
	(0.146)	(0.043)	(0.222)	(0.141)
%All-cash	0.003	0.003	0.003	0.003
	(0.604)	(0.607)	(0.676)	(0.669)
%Hostile	0.035	0.036	0.034	0.036
	(1.302)	(1.367)	(1.224)	(1.303)
Constant	0.002	0.004	0.001	0.003
	(0.149)	(0.263)	(0.090)	(0.198)
Observations	301	301	301	301
Bank FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Adjusted R-squared	0.665	0.665	0.665	0.664

t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Change Analysis of Financial Advisor Market Share

Panel A: Univariate Test of Change in Financial Advisor Market Share

This table reports statistics from univariate analysis of the change in financial advisor's market share from year t-1 to year t, sorted by positive and negative changes in the number of violations and total fine from year t-2 to year t-1.

	Change in Lagged Ln#Violations			Change in Lagged Ln$Fines		
	(1) Positive	(2) Negative	(3) Difference	(4) Positive	(5) Negative	(6) Difference
Change in Advisor Market Share (%)	-0.84	0.58	-1.42*** (-2.95)	-0.78	0.81	-1.59*** (-3.58)
Observations	110	106		131	113	

t-statistics in parentheses

*** P<0.01, ** P<0.05, *P<0.1

Panel B: Multivariate Regression Analysis of Change in Financial Advisor Market Share

This table reports the results of advisor-level OLS regression analysis of the change in market share on the lagged change in FINRA violations for a sample of financial advisors in U.S. public, private, and subsidiary acquisitions announced over 2010-2018. The dependent variable in all columns is the change in the financial advisor's market share from year t-1 to year t. The main independent variable in columns (1) and (2) is change in the logarithm of one plus the number of FINRA violations from year t-2 to year t-1. The main independent variable in columns (3) and (4) is change in the logarithm of one plus the dollar amount of FINRA violation fines from year t-2 to year t-1. Standard errors are clustered at the bank-year level. Variables are defined in Appendix B.

VARIABLES	(1)	(2)	(3)	(4)
Lagged ΔLn#violations	-0.008***	-0.008***		
	(-3.033)	(-3.081)		
Lagged ΔLn$Fines			-0.001***	-0.001***
			(-3.116)	(-2.961)
Lagged ΔMkt Shr	-0.364***	-0.359***	-0.380***	-0.376***
	(-4.952)	(-4.787)	(-5.029)	(-4.951)
Lagged Mkt Shr	-0.253***	-0.251***	-0.252***	-0.250***
	(-5.431)	(-5.848)	(-5.215)	(-5.564)
Lagged ΔEWCAR	-0.014		-0.016	
	(-1.089)		(-1.331)	
Lagged ΔVWCAR		0.038		0.032
		(1.416)		(1.241)
Lagged Δ%Completed	-0.008	-0.008	-0.008	-0.008
	(-1.086)	(-1.032)	(-1.084)	(-1.068)
Lagged Δ%All-cash	0.002	0.002	0.003	0.003
	(0.924)	(1.098)	(1.018)	(1.118)
Lagged Δ%Hostile	0.006	0.007	0.006	0.007
	(0.788)	(0.817)	(0.733)	(0.780)
Constant	0.003***	0.003	0.003***	0.004***
	(4.162)	(.)	(4.073)	(7.363)
Observations	287	287	287	287
YEAR FE	YES	YES	YES	YES
Adjusted R-squared	0.327	0.330	0.331	0.332

t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.8: Regression Analysis of Bidder CARs on the Full Sample

This table reports the results of deal-level OLS regression analysis of bidder CARs on financial advisor FINRA violations for a sample of U.S. public, private, and subsidiary acquisitions announced over 2010-2018. The dependent variable in all columns is CAR(-2,+2). The main independent variable from column (1) is the logarithm of one plus the number of FINRA violations of the lead financial advisor in an M&A deal in the year prior to the M&A deal. The main independent variable from column (2) is the logarithm of one plus the dollar amount of FINRA violation fines of the lead financial advisor in an M&A deal in the year prior to the M&A deal. Standard errors are clustered at the bidder-year level. Variables are defined in Appendix B.

VARIABLES	(1)	(2)
Ln#Violations	0.011*	
	(1.687)	
Ln$Fines		0.001*
		(1.705)
LnSize	-0.007	-0.007
	(-1.337)	(-1.330)
ROA	-0.058	-0.057
	(-0.915)	(-0.905)
BTM	-0.074**	-0.074**
	(-2.438)	(-2.440)
Leverage	0.047*	0.045*
	(1.940)	(1.887)
Liquidity	0.004	0.003
	(0.148)	(0.121)
Herfindahl	0.008	0.008
	(0.575)	(0.588)
Run-up	-0.027**	-0.027**
	(-2.511)	(-2.526)
Sigma	0.167	0.199
	(0.234)	(0.275)
Public Target	-0.023***	-0.023***
	(-2.680)	(-2.659)
Non-Tender	-0.003	-0.003
	(-0.300)	(-0.340)
Relative Size	0.033***	0.033***
	(2.812)	(2.813)
Payment Incl Stock	-0.024***	-0.024***
	(-3.209)	(-3.154)
All Cash Deal	-0.010	-0.010
	(-1.144)	(-1.142)
Complete Deal	0.009	0.009

	(0.812)	(0.794)
Focus Deal	-0.000	-0.000
	(-0.065)	(-0.083)
Complex Deal	0.002	0.002
	(0.261)	(0.302)
Hostile	-0.045	-0.045
	(-1.480)	(-1.478)
Constant	0.098	0.098
	(1.266)	(1.280)
Observations	2,068	2,068
Bank FE	YES	YES
Industry FE	YES	YES
Year FE	YES	YES
Adjusted R-squared	0.416	0.415

t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.9: Regression Analysis of Acquisition Premiums

This table presents the results of deal-level OLS regression analysis of acquisition premiums for a sample of U.S. public acquisitions announced over 2010-2018. The dependent variable is the premiums paid by the bidders, calculated as the offer price relative to the target's stock price 105 trading days before the acquisition announcement date divided by the latter. The main independent variable in column (1) is the logarithm of one plus the number of FINRA violations of the lead financial advisor in an M&A deal in the year prior to the M&A deal. The main independent variable in column (2) is the logarithm of one plus the dollar amount of FINRA violation fines of the lead financial advisor in an M&A deal in the year prior to the M&A deal. Standard errors are clustered at the bidder-year level. Variables are defined in Appendix B.

VARIABLES	(1)	(2)
Ln#Violations	-0.067***	
	(-3.148)	
Ln$Fines		-0.010***
		(-3.986)
Public Target	-0.275*	-0.282*
	(-1.786)	(-1.738)
Target LnSize	-0.160***	-0.161***
	(-4.382)	(-4.383)
Non-Tender	-0.016	-0.014
	(-0.292)	(-0.265)
Relative Size	0.094**	0.088**
	(2.503)	(2.344)
Hostile	0.098	0.095
	(1.586)	(1.515)
Payment Incl Stock	0.030	0.031
	(0.517)	(0.541)
All Cash Deal	-0.066	-0.068
	(-0.731)	(-0.760)
Focus Deal	-0.007	-0.006
	(-0.224)	(-0.176)
Multiple Bidder	0.026	0.026
	(0.411)	(0.408)
Firm Characteristics Control	YES	YES
Observations	581	581
Bank FE	YES	YES
Industry FE	YES	YES
Adjusted R-squared	0.195	0.196

t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.10: Regression Analysis of Offer Price Revision

This table reports the results of deal-level logistic regression analysis of offer price revision for a sample of US public, private, and subsidiary acquisitions announced over 2010-2018. The dependent variable is a binary variable taking the value for one if the final offer price is greater than the initial offer price, and zero otherwise. The main independent variable in column (1) is the logarithm of one plus the number of FINRA violations of the lead financial advisor in an M&A deal in the year prior to the M&A deal. The main independent variable in column (2) is the logarithm of one plus the dollar amount of FINRA violation fines of the lead financial advisor in an M&A deal in the year prior to the M&A deal. Standard errors are clustered at the bidder-year level. Variables are defined in Appendix B.

VARIABLES	(1)	(2)
Ln#Violations	-0.613*	
	(-1.649)	
Ln$Fines		-0.136**
		(-2.177)
Public Target	-3.713**	-3.987**
	(-2.170)	(-2.180)
Non-Tender	-0.583	-0.534
	(-0.743)	(-0.678)
Hostile	3.525***	3.483***
	(5.523)	(5.428)
Relative Size	0.596***	0.564***
	(5.611)	(4.693)
Focus Deal	-0.380	-0.400
	(-0.818)	(-0.779)
Payment Incl Stock	0.283	0.259
	(0.673)	(0.616)
Premium	1.303***	1.220***
	(3.243)	(2.791)
Multiple Bidder	2.011***	2.102***
	(3.262)	(3.727)
Firm Characteristics Control	YES	YES
Observations	571	571
Bank FE	YES	YES
Industry FE	YES	YES
Pseudo-r2	0.4679	0.4747

z-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.11: Robustness Checks

This table presents results from Heckman two-stage procedure analysis for a sample of U.S. public, private, and subsidiary acquisitions over 2010-2018. Columns (1) - (3) present results from the full sample. Columns (4) - (6) present results from the non-top institution subsample. Columns (1) and (4) presents the results from the first-stage regressions. The dependent variable in column (1) is a dummy equal to one for an M&A firm-year, zero otherwise. The dependent variable in column (4) is a dummy equal to one if the deal is advised by a top institution. Columns (2) - (3) and (5) - (6) present the results from the second-stage regressions, where the dependent variable is CAR (-2, +2). The main independent variable from columns (2) and (5) is the logarithm of one plus the number of FINRA violations of the lead financial advisor in a M&A deal in the year prior to the M&A deal. The main independent variable from columns (3) and (6) is the logarithm of one plus the dollar amount of FINRA violation fines of the lead financial advisor in a M&A deal in the year prior to the M&A deal. Robust z-statistics are in parentheses in columns (1) and (4). t-statistics are in parentheses in columns (2) - (3) and (5) - (6), where standard errors are clustered at the bidder-year level. Variables are defined in Appendix B.

	Selection	Full Sample		Selection	Non-top Institutions
VARIABLES	(1)	(2)	(3)	(4)	(5)
Ln#Violations		0.011*			0.024***
		(1.648)			(2.691)
Ln$Fines			0.001*		
			(1.886)		
LnSize		-0.028	-0.028	0.333***	0.021
		(-1.244)	(-1.245)	(8.162)	(1.194)
ROA	0.063	-0.059	-0.058		-0.052
	(0.808)	(-0.963)	(-0.953)		(-0.616)
BTM		-0.077**	-0.077**	0.155**	-0.058*
		(-2.270)	(-2.272)	(2.018)	(-1.815)
Leverage	-1.476***	0.102	0.100	0.667**	0.116*
	(-12.495)	(1.561)	(1.551)	(1.964)	(1.922)
Liquidity	-1.485***	0.138	0.137	0.175	0.010
	(-8.264)	(1.185)	(1.187)	(0.414)	(0.329)
Herfindahl		0.007	0.007		0.014
		(0.537)	(0.547)		(0.533)
Run-up		-0.017**	-0.017**	-0.031	-0.031**

		(-2.270)	(-2.268)	(-0.188)	(-2.031)
Sigma		0.134	0.165	5.173	1.476
		(0.199)	(0.240)	(0.639)	(1.186)
Public Target		-0.024***	-0.024***		-0.026***
		(-3.103)	(-3.074)		(-2.744)
Non-Tender		-0.003	-0.003		-0.029***
		(-0.332)	(-0.369)		(-3.237)
Relative Size		0.032***	0.032***		0.037*
		(2.820)	(2.819)		(1.937)
Payment Incl Stock		-0.023***	-0.023***		-0.015
		(-2.998)	(-2.960)		(-1.375)
All Cash Deal		-0.010	-0.010	0.103	0.011
		(-1.248)	(-1.247)	(0.947)	(0.837)
Complete Deal		0.009	0.009		0.005
		(0.848)	(0.832)		(0.380)
Focus Deal		0.000	0.000	0.162	0.004
		(0.099)	(0.077)	(1.496)	(0.359)
Complex Deal		0.001	0.001		-0.001
		(0.155)	(0.198)		(-0.129)
Hostile		-0.044	-0.044	0.224	-0.036
		(-1.500)	(-1.496)	(0.664)	(-0.969)
Inverse Mills Ratio		-0.085	-0.085		0.112
		(-1.114)	(-1.118)		(1.216)
Total Assets	0.295***				
	(27.840)				
MTB	-0.008				
	(-0.569)				
Score				0.066	
				(0.889)	
Constant	-4.866***	0.527	0.528	-2.297**	-0.088
	(-8.269)	(1.213)	(1.218)	(-2.333)	(-0.452)

Observations	40,202	2,068	2,068	2,051	1,198
Bank FE		YES	YES		YES
Industry FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Regr	Logit	OLS	OLS	Logit	OLS
Pseudo-r2	0.087			0.144	
Adjusted R-squared		0.421	0.420		0.496

*** p<0.01, ** p<0.05, * p<0.1

Chapter 2: Voluntary Disclosure and Real Earnings Management in SEO Firms: Evidence from the 2005 Securities Offering Reform

2.1. INTRODUCTION

In this paper, I study how the removal of pre-offering disclosure restrictions following the enactment of the 2005 Security Offering Reform (SOR) affects real earnings management activities in seasoned equity offering (SEO) firms. SOR's enactment could affect both accruals and real earnings management. However, for at least three reasons, I choose to focus on real earnings management. First, the announcement of an SEO naturally attracts more attention from regulators, investors, and analysts. Second, when under increased scrutiny during special periods, such as those surrounding SEOs, firms are more likely to manage real earnings, as opposed to managing accruals (Cohen and Zarowin, 2010; Graham et al., 2005), as real earnings management is less likely to be scrutinized by regulators and analysts (Ball and Shivakumar 2008; Kothari et al. 2016). Third, accruals management is more likely to be illegal and/or unethical than real earnings management.

The SOR eases prior restrictions imposed by the "gun-jumping" provisions on firm disclosure prior to equity offerings. For a brief period, it provides a safe harbor for disclosure 30 days prior to the filing with the SEC for all issuers. It also removes restrictions on regular disclosure of factual business information and forward-looking information at any time for all issuers. Relevant prior studies find that SEO firms increase their pre-offering disclosure after the regulation is put into effect (Shroff et al., 2013; Clinton et al., 2014). Disclosure increases in the forms of both factual information (e.g. 10Ks, 8Ks) and management earnings forecasts, leading to a more transparent pre-SEO information environment characterized by reduced information asymmetry and decreased cost of equity capital. Building on these findings, I explore how the post-SOR increased pre-offering disclosure affects real earnings management activities in SEO firms. A large literature has examined SEO firms' earnings management activities.[17] However, most of the relevant studies focus only on accrual-based earnings management. There is a paucity of evidence on earnings management through real activities in SEO firms, with two notable exceptions of Cohen and Zarowin (2010) and Kothari et al. (2016). Real activities manipulation is less likely to be scrutinized by auditors and regulators than accrual manipulation but is more likely to bring severe consequences to the firm in the long term. Both Cohen and Zarowin (2010) and Kothari et al. (2016) show that SEO firms' engagement in real activities manipulation is large and can cause severe post-SEO under-performance. During the pre-SEO periods when firms' disclosure activities draw great scrutiny from investors and regulators, firms are more likely to engage in earnings manipulation activities that are less likely to be detected, such as manipulating real activities. Hence, my paper focuses on changes in real earnings management activities following the enactment of SOR.

I hypothesize that SEO firms reduce real earnings management activities in the post-SOR periods for two reasons. First, in the post-SOR periods, the information environment is more transparent due to greater voluntary disclosure flexibility, making real earnings management activities easier to be detected by investors and regulators. Second, in the post-SOR periods, the reduced cost of equity capital (i.e. higher firm value) attributable to reduced information asymmetry between the issuance firms and investors provides managers fewer incentives to inflate offer prices through earnings management activities.

To measure real earnings management, I follow Cohen and Zarowin (2010) and employ the model developed by Roychowdhury (2006), which is based on Dechow et al. (1998). I

[17] See Rangan, 1998; Teoh *et al.,* 1998; Shivakumar, 2000; DuCharme *et al.*, 2004; Jo and Kim, 2007.

construct five proxies of real earnings management: abnormal cash flow from operations (ACFO), abnormal production costs (APRO), abnormal discretionary expenses (AEXP), an aggregate measure of production costs and discretionary expenses (RM1), and an aggregate measure of cash flow from operations and discretionary expenses (RM2).

I implement the difference-in-differences (DiD) design to address the potential endogeneity issue between voluntary disclosure and real earnings management. In the DiD setting, I am able to identify the causal changes in real earnings management resulting from the increased voluntary disclosure post-SOR. The DiD design requires the construction of a treated group and a control group. I compare the changes in real earnings management pre- and post-SOR in the treated group to a control group that is unaffected by the regulation. The treated group consists of all SEO firm-years. The control group consists of all non-SEO firm-years in my main analysis. In further analysis, I identify a matched control group consisting of non-SEO firm-years that have a similar likelihood of issuing equities as the SEO firm-years. The matched control group has similar firm characteristics as the treated group. Thus, any effect detected by the DiD model is less likely to be caused by the systematic differences between the two groups.

To further investigate the cross-sectional differences in the effect of the SOR on real earnings management in SEO firms, I partition the full sample into two subsamples based on whether the firm misses the consensus analyst forecasts and re-estimate the DiD model on the two subsamples separately. Cohen et al. (2008) cites meeting or beating consensus analyst forecasts as one of the reasons for real earnings management, based on survey results from Graham et al. (2005). As such, firms that meet or beat the consensus analyst forecasts are more likely to have engaged in real earnings management activities than those that miss the consensus analyst forecasts. The firms that miss the consensus analyst forecasts would not have had the incentives to engage in real activities manipulation, because doing so would incur the unnecessary costs of being detected without the intended benefits from manipulating earnings. Thus, the effect of the SOR is expected to be significant only in the subsample where firms meet or beat consensus analyst forecasts. Finally, to address the concern that the reduction in real earnings management after the SOR in SEO firms is driven by the predetermined trend of real earnings management over time, I perform a placebo test. I manually shift the SOR year to 2007, which is two years after the actual regulation year. I expect to find insignificant results when I re-estimate the model.

My empirical results are consistent with my aforementioned conjectures. Specifically, SEO firms exhibit reduced real activities manipulation in the post-SOR periods. The results are robust using different proxies of real earnings management. I further document that the effect is significant only among firms that meet or beat consensus analyst forecasts. The results also hold in the matched sample constructed using propensity-score matching based on a vector of variables that are predicted to determine the likelihood of issuing equity. The results pass the placebo test.

I continue to find that the impact of the SOR on real earnings management only exists in firms with high voluntary disclosure. These findings suggest the existence of a substitution effect between voluntary disclosure and real earnings management in SEO firms, i.e. greater flexibility of voluntary disclosure leads to fewer incentives to manipulate real activities. Post SOR, SEO firms made a tradeoff between the benefits from increased voluntary disclosure as a selling effort and the potential costs associated with earnings management activities being more easily detected in more transparent information environment.

My paper makes several useful contributions to the literature. First, this paper adds to the debate on the effectiveness of the 2005 Securities Offerings Reform. The SOR has received limited attention in academia. This is most likely because it is released during a period when many important regulations came out (e.g. Reg FD, SOX, NASD rule 2711, and NYSE rule 472 in 2002; and Global Settlement and Settlement and Regulation Analysts Certification in 2003). Supporters of the SOR argue that the improved information environment through communications between firms and investors in the form of voluntary disclosure helps investors form more accurate expectations about the issuing firm. Opponents believe that the eased disclosure restrictions would encourage firms to “hype” their offerings. My results show that the SOR regulation, which allows firms greater voluntary disclosure flexibility before equity offerings, is effective in reducing SOE firms’ incentives to manipulate earnings during the pre-offering periods. Second, this paper contributes to the literature on real earnings management in SEO firms. Earnings management is commonplace around SEOs, but the empirical evidence on real earnings management is limited, as mentioned in Cohen and Zarowin (2010) and Kothari et al. (2016). This paper helps fill the gap.

2.2. Background, Literature Review, and Hypothesis Development

2.2.1. Disclosure regime changes during the offering process

2.2.1.1. Before the 2005 Securities Offering Reform

With the intent of preventing issuing firms from conditioning the market, Section 5(c) of the Securities Act, known as the “gun-jumping” provisions, restricts communications between anyone involved in an upcoming offering of securities and potential investors. Specifically, before a registration statement is filed with the SEC, any “offers” are prohibited. The restriction period is called the “quiet period”. After the filing, written offers are limited to the statutory prospectus filed with the SEC and any other disclosure is impermissible by Section 5(b)(1) of the Securities Act. However, the rules do not provide a clear definition of what “offers” entail and no clear definition with regards to how long the "quiet period" is.[18] As a result, firms tend to reduce the frequency of their regular disclosure, since the consequences of violation of the "gun-jumping" provisions can be serious (Shroffet et al., 2013).[19]

2.2.1.2. After the 2005 Securities Offering Reform

Cognizant of the development of modern communications technology after the "gun-jumping" provisions were developed, through which more information is provided to the market on a more non-discriminatory, current, and ongoing basis, the SEC believes the provisions restrict beneficial communications between firms and the market. Consequently, the SEC enacted the Securities Offering Reform (SOR) in December 2005, which eased restrictions during the equity offerings for the purpose of providing a greater information flow to investors before offerings. The SOR provides safe harbors for disclosure before equity offerings. Rule 163A of the SOR clarifies the "quiet period" as 30 days prior to the filing with SEC. No communication will be considered a violation of the "gun-jumping" provisions made up to 30 days prior to the filing for all issuers. Rule 168 removes restrictions on the regular disclosure of factual business information and forward-looking information at any time for all reporting issuers. The rules permit the use of other written offers of securities, generally defined as a "free writing prospectus" (FWP), in addition to the filed or statutory prospectus after the filing of the registration statement for all issuers.

[18] Securities Act Section 2(a)(3) [15 U.S.C. 77b(a)(3)] defines "offer" as any attempt or offer to dispose of, or solicitation of an offer to buy, a security or interest in a security, for value. The term "offer" has been interpreted broadly and goes beyond the common law concept of an offer. See Diskin v. Lomasney & Co., 452 F.2d 871 (2d. Cir. 1971); SEC v. Cavanaugh, 1 F. Supp. 2d 337 (S.D.N.Y. 1998). The Commission has explained that "the publication of information and publicity efforts, made in advance of a proposed financing which have the effect of conditioning the public mind or arousing public interest in the issuer or in its securities constitutes an offer * * *." Guidelines for the Release of Information by Issuers Whose Securities are in Registration, Release No. 33–5180 (Aug. 16, 1971) [36 FR 16506].
[19] Violation of gun-jumping restrictions can cause the SEC to impose a significant delay in the offering or the purchasers of the securities in the offering may acquire a one-year rescission right.

2.2.2. Literature review and hypothesis development

Prior relevant studies have documented that pre-offering information environment becomes richer through public information disclosure following SOR's enactment. Clinton et al. (2014) shows that SEO firms release more frequent management earnings forecasts, more 8-K filing, and other information post SOR. Shroffet et al. (2013) and Hemmings et al. (2018) document decreased information asymmetry between informed and uninformed traders around SEOs, using proxies of the bid-ask spread, market depths, and analyst forecast accuracy. They also find less negative SEO announcement abnormal returns and a reduced cost of equity capital.

There are two potential arguments with respect to how the removal of disclosure restrictions by the SOR may affect earnings management activities in SEO firms. SEO firms face a trade-off between voluntary disclosure as a selling effort and earnings manipulation as a mechanism to inflate offer price.[20] Specifically, while voluntary disclosure reduces information asymmetry and the level of underpricing,[21] the improved information environment would make firms' earnings manipulation activities more easily detected by the market.[22] In the pre-SOR years, firm disclosure during SEOs is largely constrained, which creates an opaque information environment where information asymmetry between firms and investors is high (Smith, 1977). Facing the restrictions on disclosure, firms may turn to earnings manipulation to inflate their offer price. Post SOR, pre-offering disclosure restrictions are eased. As such, firms may choose to engage in less earnings management and to voluntarily disclose more helpful information to the market leading up to SEOs. Clinton *et al*. (2014) show that firms indeed release more frequent management earnings forecasts, more 8-K filing, and other information post SOR. Shroff *et al*. (2013) and Hemmings *et al*. (2018) further document a decreased information asymmetry post SOR, as

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[20] See Dechow et al. (1996); DuCharme et al. (2004); Kim and Park (2005); Rangan (1998); Shivakumar (2000)
[21] See Brown and Hillegeist (2007); Healy and Palepu (2001); Diamond and Verrecchia (1991)
22 See Jo and Kim (2007); Lobo and Zhou (2001); Riahi and Arab (2011)

evidenced by narrower bid-ask spread, greater market depths, and more accurate analyst forecasts, along with less negative abnormal returns around SEOs. In the post-SOR improved information environment, investors should be able to better discern earnings manipulation activities, *ceteris paribus*. Moreover, the reduced SEO underpricing attributable to the decreased cost of equity capital in the post SOR gives managers fewer incentives to inflate offer prices through earnings management activities. Based on this line of reasoning, real earnings manipulation activities in SEO firms should decrease post SOR.

It is also likely that increased voluntary disclosure lead to more earnings management. Kasznic (1999) suggests that biased voluntary disclosure would lead to more earnings management for two reasons: fearing legal actions by investors and loss of reputation for accuracy. Specifically, Kasznic (1999) finds that firms use more discretionary accruals when their earnings would otherwise fall below management earnings forecasts. The upward biased disclosure can be especially commonplace during special periods like SEOs when firms have great incentives to hype the stock (Jo and Kim, 2007; Lang and Lundholm, 2000). This is exactly the reason why disclosure prior to SEOs is severely restricted by the Securities Act of 1933. Firms face litigation risk and reputation loss if their actual earnings cannot meet their expectations.[23] Therefore, firms have incentives to mitigate forecast errors while releasing optimistic forecasts to maximize issuance proceeds. Based on this line of reasoning, earnings manipulation activities in SEO firms should increase after the SOR.

Although both types of earnings management, i.e. real earnings management and accrual earnings management, could be affected by the SOR, I argue that the effect, if any, should be more pronounced for real activities manipulation. Earnings management around SEOs is extensively studied in prior literature (Rangan, 1998; Teoh *et al*., 1998; Shivakumar, 2000; DuCharme *et al*., 2004). With two notable exceptions (Cohen and Zarowin, 2010; Kothari *et al*., 2016), real earnings management in SEO firms has received limited attention in the literature. Compared to accrual

[23] Rule 10b-5 that deals with forecast issuance fraud is still in effect.

manipulation, real activities manipulation is less likely to be scrutinized by auditors and regulators. However, earnings manipulation through altering real activities can cause firms to deviate from normal business operations. Thus, they could have severe negative long-term consequences at the operational level. Cohen and Zarowin (2010) show that real earnings management activities in SEO firms are more severe than accrual earnings management. Combined with the effect from accrual earnings management activities, the real earnings management activities lead to post-SEO underperformance. Kothari *et al*. (2016) provides further evidence that real earnings management provides more consistent predictions of post-SEO underperformance than accrual earnings management. During special periods such as SEOs, firms are more incentivized to manipulate earnings in a manner that is more difficult to be detected, such as manipulating real activities. This is because during special periods such SEOs, firms face greater scrutiny from regulators and investors. Firms that face greater scrutiny prefer to use real earnings management than accrual-based earnings management (Cohen and Zarowin, 2010; Graham et al., 2005). As such, SEO firms are more likely to engage in real earning management than accrual-based earnings management. This is consistent with the findings in Kothari et al. (2016).

Based on the aforementioned two competing arguments, I do not predict the sign of the change in real earnings managements after the SOR. I thus present this study's hypothesis in null form below:

H0: *After the enactment of the 2005 SOR, there is no change in SOE firms' engagement in real earnings management.*

2.3. Sample selection, variable measurement, and research design

2.3.1. Sample selection

My sample period is from January 1, 2003 to December 31, 2008. The SOR was effective on December 1, 2005. I classify years after 2005 as post-event years and classify years of 2005 and before as pre-event years. I manually adjust SEOs during December 2015 as pre-event. Data on the SEOs is from the Securities Data Corporation (SDC) New Issuance database. Other data used for control group construction and control variables in the main regressions include: stock prices from Center for Research in Security Prices (CRSP) databases, firm characteristics from Compustat, and analyst data from Institutional Brokers' Estimate System (IBES). The SEOs in my sample have to be issued by U.S. issuers that are listed on NYSE Amex or NASDAQ. Following Cohen and Zarowin (2010), I exclude the following offers: (1) spin-offs, (2) LBOs, (3) closed-end funds/trusts, (4) REITs, (5) limited partnerships, (6) rights issues, (7) simultaneously offerings, (8) unit issues, (9) offer prices less than $5, (10) in financial or utility industries, (11) lacking annual financial information from Compustat to compute earnings management proxies. Appendix D presents the sample selection process.

Separate samples for different proxies of real earnings management are created to keep as many SEO firms as possible due to the greater loss of observations measuring some types of real earnings management. There are 181 SEO firms in the sample using abnormal discretionary expenses as a proxy for real earnings management, 626 SEO firms in the abnormal production costs proxy sample, and 649 SEO firms in the abnormal cash flows from the operations proxy sample. Table 2.1 Panel A reports the summary statistics on firm characteristics and offer prices of the SEO firms in my sample. The statistics are consistent with SEO samples in other studies (Teoh et al., 1998; Rangan, 1998; Cohen and Zarowin, 2010). The median firm has a market value from 737 million to 747 million, total assets from 443 million to 460 million, a return-on-assets ratio from 0.033 to 0.047, a leverage from 0.134 to 0.191, a market-to-book ratio from 2.011 to 2.111, and seven analysts following in different SEO samples. The median offer size is about 20% of the firm market value. Panel B of Table 2.1 presents the number of SEOs each sample year. Panel C of Table 2.1 shows the distribution of SEOs by industry. Consistent with what is found in Cohen and Zarowin (2010), SEOs cluster in certain high-tech industries like chemical products, computer equipment and services, and electronic equipment.

2.3.2. Real earnings management proxies

I follow prior literature to construct my real earnings management proxies. Roychowdhury (2006) and Zang (2012) develop proxies for three types of real earnings management activities following the survey from Graham, Harvey, and Rajgopal (2005) that documents the widespread use of real activities manipulation. In the first type of real earnings management, firms would accelerate the timing of sales by using price discounts and other strategies like credit sales in order to temporarily boost their earnings. These activities result in negative abnormal cash flows from operations for the current period. Second, firms would overproduce inventory to decrease the cost of goods sold in order to produce higher operating margins for the current period. These activities would result in abnormal production costs. Third, firms would reduce discretionary expenses including advertising, R&D, and SG&A costs to boost the current period earnings.

The real earnings management model is developed by Roychowdhury (2006) based on Dechow *et al*. (1998) and is applied in papers that study real activities manipulation during SEOs (Cohen and Zarowin, 2010). The normal levels of cash flows from operations, discretionary expenses, and production costs are the predicted values from estimating the regressions below by year and 2-digit SIC industry code:

$$\frac{CFO_{i,t}}{Assets_{i,t-1}} = k_1 \frac{1}{Assets_{i,t-1}} + k_2 \frac{Sales_{i,t}}{Assets_{i,t-1}} + k_3 \frac{\Delta Sales_{i,t}}{Assets_{i,t-1}} + \epsilon \qquad (1)$$

$$\frac{PROD_{i,t}}{Assets_{i,t-1}} = k_1 \frac{1}{Assets_{i,t-1}} + k_2 \frac{Sales_{i,t}}{Assets_{i,t-1}} + k_3 \frac{\Delta Sales_{i,t}}{Assets_{i,t-1}} + k_4 \frac{\Delta Sales_{i,t-1}}{Assets_{i,t-1}} + \epsilon \ (2)$$

$$\frac{DISX_{i,t}}{Assets_{i,t-1}} = k_1 \frac{1}{Assets_{i,t-1}} + k_2 \frac{Sales_{i,t-1}}{Assets_{i,t-1}} + \epsilon \qquad (3)$$

where CFO is the cash flow from operations, PROD is the production costs, and DISX is the discretionary expenses. The production costs (PROD) is computed as the sum of the cost of goods sold and changes in inventory. The abnormal cash flow from operations is actual CF O minus the normal level of CFO calculated using the estimated coefficients from Eq. (1). The abnormal production costs are actual PROD minus the normal level of PROD calculated using the estimated coefficients from Eq. (2). The abnormal discretionary expenses are actual DISX minus the normal level of DISX calculated using the estimated coefficients from Eq. (3).

Managing earnings upward through real activities manipulation will result in negative abnormal cash flow from operations through the accelerated timing of sales using price discounts and other strategies, negative abnormal discretionary expenses including advertising, R&D, and SG&A costs, and positive abnormal production costs. Therefore, lower abnormal cash flow from operations, lower abnormal discretionary expenses, and higher abnormal production costs are linked to higher likelihood that firms engage real earnings management activities. In this study, I focus on the magnitudes of real activities manipulation, instead of direction of the manipulation, as I are interested in the impact of the 2005 SOR on firms' trade-off between voluntary disclosure and earning management leading up to the SEOs. Hence, I take the absolute values of each of the real earnings management proxies: an absolute value of abnormal cash flow from operations (ACFO), an absolute value of abnormal production costs (APRO), and an absolute value of discretionary expenses (AEXP).

Panel A of Table 2.2 shows the median real earnings management proxies -3 years to +3 years relative to SEO years (year 0). The table shows that the median level of real earnings management during SEOs years is greater than the surrounding years, consistent with what is found in prior literature (Cohen and Zarowin, 2010; Kothari et al., 2016). This indicates that SEO firms engage in real earnings management activities. Panel B of Table 2.2 shows the median real earnings management proxies each year during the sample period. The statistics show that before and during

2005, the level of real earnings management in SEO firms is greater. After the SOR, the level of real earnings management reduces.

To capture the aggregate effect of real earnings management, I also apply two aggregate proxies of real earnings management (RM1 and RM2) used in the real earnings management literature (Cohen and Zarowin, 2010; Irani and Oesch, 2016). The two proxies are computed as follows:

$$RM_1=APRO+AEXP$$

$$RM_2=ACFO+AEXP,$$

where RM1 captures the aggregate effect of producing abnormal production costs and cutting discretionary expenses and RM2 captures the total effect of cutting cash flow from operations and abnormal discretionary expenses. The higher these proxies are, the more likely firms are doing real activities manipulation. I report results using all five of the real earnings management proxies.

2.3.3. Research design

2.3.3.1. The effect of the SOR on real earnings management in SEO firms

To investigate the association between voluntary disclosure and real earnings management, I employ a difference-in-difference (DiD) design using the SOR as a plausible exogenous shock to firms' pre-offering disclosure in SEO firms. The DiD design gets around the problem of omitted trends in the time-series comparison and the problem of unobserved differences between treatment and control groups in the cross-sectional comparison (Roberts and Whited, 2013). I compare the changes in real earnings management proxies in SEO firms after the SOR to changes in a control group that is unaffected by the SOR. Applying the DiD design, I am able to identify the causal changes in real earnings management during SEOs resulting from the increased voluntary disclosure. I estimate the following DiD regression model on each of the real earnings management proxy samples:

$$EM_{i,t} = \beta_0 + \beta_1 SEO_{i,t} + \beta_2 SEO * SOR_{i,t} + \Sigma\gamma COntrols_{i,t-1} + Industry_i + Year_t + \epsilon, \tag{4}$$

where EM represents alternative proxies of real earnings management discussed above, which includes absolute abnormal cash flow from operations (ACFO), absolute abnormal production costs (AP RO), absolute abnormal discretionary expenses (AEXP), an aggregate proxy of abnormal production costs and abnormal discretionary expenses (RM1), and an aggregate proxy of abnormal cash flow from operations and abnormal discretionary expenses (RM2). SOR is an indicator variable equal to one for years after 2005 and zero otherwise; SEO is an indicator variable equal to one for SEO firm-years and zero if the firm-year is in the control group. In the main analysis, I use all the non-SEO firm years as the control group. In further analysis, I construct a matched control group, consisting of non-SEO firm-years that have a similar likelihood of issuing equity as the SEO firm-years. The detailed construction of the matched sample is discussed in Section 1.3.3.3.

To overcome the omitted variable bias, I control for firm characteristics that affect both the likelihood of issuing equity and engaging in real activities manipulation. I control for basic firm characteristics: book leverage (LEV), market-to-book ratio (MTB), cumulative abnormal returns (CAR), all measured in t − 1. I control for sales growth (SALESG), return on assets (ROA), firm age (LnAGE), and the logarithm of market value of equity (LnMVE), all measured in t−1. These variables control for firms' investment opportunities, financial conditions, and profitability. I also control for analyst-related variables, including the logarithm of the number of analysts following (LnNUM) and the absolute value of analyst forecast errors scaled by market value of equity

(ANAF RE), all measured in year t. The detailed variable definitions are provided in the Appendix C. The main effect SOR is omitted from the regression as year indicators are included to avoid the dummy trap. The main coefficient of interest is β_2, which measures the changes in real earnings management in SEO firms after the SOR compared to the changes in real earnings management of control firms. I expect β_2 to be negative if real earnings management in SEO firms is reduced after the SOR compared to the control firms.

2.3.3.2. Cross-sectional heterogeneity in the effect on real earnings management

To investigate how the effect of the SOR on the real earnings management in SEO firms varies across firms, I partition the full sample into two subsamples based on whether the earnings of the firm-year miss consensus analyst forecasts. I re-estimate Eq. (4) on the two subsamples. Firms that meet or beat consensus analyst forecasts are more likely to have engaged in real earnings management activities. If firms would miss the consensus analyst forecasts anyway, they would not have manipulated earnings and bear the risk of being detected. Based on this, I expected to find β_2 significant in the subsample where firms meet or beat consensus analyst forecasts while insignificant in the subsample where firms miss consensus analyst forecasts.

2.3.3.3. Matched sample analysis

To address the possibility that the systematic differences between equity issuance firms and other firms may cause different outcomes of interest, I construct a matched control group selected from non-SEO firms. The treated group used in this analysis is the SEO sample using the two aggregate real earnings management proxies (RM1 and RM2). I use propensity-score matching (PSM) to identify the control group. The controlled group consists of non-SEO firm-years with a similar likelihood of issuing equity as SEO firm-years. To begin with the matching, I estimate a logistic regression of SEO dummy on a set of matching variables that predict the likelihood of issuing equity. Following Shroff *et al*. (2013), the matching variables I use are: logarithm of market value (LnMVE), Tobin's Q (Q), return on assets (ROA), sales growth (SALESG), cash holdings (CASH), firm age (LnAGE), the logarithm of the number of analysts following (LnNUM) and cumulative abnormal returns (CAR), all measured in year t-1. Market value (LnMVE), Tobin's Q (Q), return on assets (ROA), and sales growth (SALESG) control for firm investment opportunities. Firms with greater investment opportunities are more likely to issue equity. Cash holdings (CASH), firm age (LnAGE), and the number of analysts following control for firm financial conditions and information environment. Firms with financial constraints and greater information uncertainty are less likely to obtain cost-effective capital from the equity market. The detailed variable constructions are provided in the Appendix C. I estimate the following logistic regression:

$$
\begin{aligned}
&Pr\left(SEO_{i,t} = 1\right) \\
&\qquad = f\big(\beta_0 + \beta_1 LnMVE_{i,t-1} + \beta_2 Q_{i,t} + \beta_3 ROA_{i,t-1} + \beta_4 SALESG_{i,t-1} \\
&\qquad + \beta_5 CASH_{i,t-1} + \beta_6 CAR_{i,t-1} + \beta_7 LnAGE_{i,t-1} + Year_t + Firm_i + \epsilon\big),
\end{aligned} \tag{5}
$$

where *f* is the cumulative distribution function of the standard normal distribution. SEO equals one for SEO firm-years, zero otherwise. The predicted value from the logistic regressions is the propensity score of issuing equity for a firm-year. Based on the propensity scores, I perform a nearest neighbor match. I match each SEO firm-year with one non- SEO firm-year that has the closest propensity score. The matched non-SEO firm-years constitute the control group. I then re-estimate Eq.(4) on the matched sample to examine the effect of the SOR on real earnings management in SEO firms.

2.4. EMPIRICAL RESULTS

2.4.1. Full sample analysis results

To study the effect of the SOR on real earnings management in SEO firms, I estimate Eq. (4). Table 2.3 shows the Pearson correlation between the main variables used in this paper. Table 2.4 reports the results from estimating the difference-in-differences model using SEO firm-years as the treated group (SEO = 1) and all the non-SEO firm-years as the control group (SEO = 0). The control variables have signs consistent with prior literature. Firms with greater investment opportunities, proxied by sales growth and market-to-book ratio are more likely to engage in real earnings management to avoid underinvestment. The coefficient on SEO is significant and positive, which is consistent with the descriptive statistics in Table 2.2 that firms engage in more real earnings management during SEO years than non-SEO years. The coefficient of the main variable of interest SEO*SOR is significant and negative in most of the real earnings management proxy samples: when the dependent variables are abnormal cash flows from operations (ACFO) in Column (1), abnormal production costs samples (AP RO) in Column (2), an aggregate proxy of production costs and discretionary expenses (RM1) in Column (4), and an aggregate proxy of cash flow from operations and discretionary expenses (RM2) in Column (5). The results suggest that real earnings management in SEO firms is reduced more compared to the real earnings management changes in non-SEO firms after the SOR. The results are consistent with my hypotheses that the increased voluntary disclosure after the SOR reduces the level of real earnings management activities in SEO firms.

2.4.2. Subsample analysis results

To provide further evidence on the cross-sectional differences in the impact of the SOR on real earnings management in SEO firms, I re-estimate Eq. (4) on two subsamples partitioned based on whether the firm misses the consensus analyst forecasts. I expect the negative effect of the SOR on real earnings management to be significant only in firms that meet or beat analyst forecasts, as meeting or beating consensus analyst forecasts is the main purpose of managing earnings. The results are reported in Table 2.5. Panel A of Table 2.5 shows the results from estimating Eq. (4) on the subsample that beats or meets consensus analyst forecasts. The results are consistent with my hypothesis. The coefficients of SEO have the predicted positive sign in all columns, meaning real earnings management in SEO firms is greater than in other firms. The coefficients of the main variable of interest SEO*SOR are significant and negative in all columns with different proxies of real earnings management as the dependent variable. The table provides consistent results that for firms that meet or beat analyst forecasts, the level of real earnings management in SEO firm-years is reduced compared to non-SEO firm-years after the SOR became effective. Panel B of Table 2.5 shows the results from the subsample that miss consensus analyst forecasts. The coefficients of SEO*SOR are not significant in any of the columns. The subsample analysis shows cross-sectional differences in the effect of the SOR on real earnings management in SEO firms, with significant results only in firms that meet or beat analyst forecasts. The differences in the coefficients of SEO*SOR across the two subsamples are also statistically different (Chi2=6.77 and p=0.01 for RM2, Chi2=2.95 and p=0.09 for RM2).

2.4.3. Matched sample analysis results

To address the possibility that the systematic differences between equity issuance firms (the treated group) and other firms may cause different outcomes of interest, I construct a matched control group from non-SEO firms by estimating Eq. (5). Table 2.6 shows the estimation results from the logistic regression model, which predicts the likelihood of issuing equity. The variable coefficients are generally consistent with what has been found in prior literature. Firms with more investment opportunities as indicated by Tobin's Q (Q) and sales growth (SALESG) are more likely to issue equity. Firms with less financial constraints proxied by firm size (LnMVE) and cash holdings (CASH) are less likely to issue equity.

To test the successfulness of the propensity-score matching, I do a covariate balance test. The covariate balance test compares the firm characteristics that determine the likelihood of issuing equity in the treated group and control group, both before and after the matching. If the propensity-score matching is successful, I should see that after matching, the firm characteristics that determine the likelihood of issuing equity are not significantly different between the treated group and the control group. Table 2.7 reports the matched sample covariate balance tests. Panel A reports the t-test differences in firm characteristics between the treated group and the unmatched control group. Panel B reports the t-test differences in firm characteristics between the treated group and the propensity-score-matched control group. Panel A shows that most of the firm characteristics that determine the likelihood of SEOs are significantly different between the treated group and the unmatched control group. Panel B shows that after matching, the firm characteristics of the treated group and the control group are not significantly different. Overall, the covariate balance tests show that the matched sample construction is successful.

The matched sample is constructed using the nearest neighbor matching. I then re-estimate Eq.(4) on the matched sample. Table 2.8 shows the results from estimating Eq. (4) in the matched sample constructed using propensity-score matching. Panel A shows the results from the estimation in the full sample. Panel B shows the results in the subsample where firms meet or beat analyst forecasts. The results are consistent with what is found in the main analysis and are consistent with my hypothesis. In both samples, the coefficient of SEO remains positive. The coefficient of the main variable of interest SEO*SOR is significant and negative. The results confirm that after the SOR, the difference in real earnings management between SEO firm-years and non-SEO firm-years is reduced. After matching, the firm characteristics of the firms in the treated group and control group are not significantly different. Thus, the different levels of changes in the real earnings management between the two groups after the SOR are less likely to be caused by the systematic differences between the two groups of firms. Panel C shows the results in the subsample where firms miss analyst forecasts. The coefficient of SEO*SOR is not significant.

2.4.4. Placebo test

The reduction in real earnings management after the enactment of the SOR in SEO firms may be driven by the predetermined trend of real earnings management over time. To test the parallel assumption of the DiD model, I employ a placebo test. I manually shift the SOR to 2007, which is two years later than the actual SOR year of 2005. I re-estimate Eq. (4) on both the full sample and the subsample where firms meet or beat consensus analyst forecasts. If the results are driven by the pre-determined trend, I should still see a significant and negative DiD coefficient even if I have shifted the SOR year. The results of the placebo test are reported in Table 2.9. Panel A shows the results from the full sample, Panel B shows the results from the subsample that meet or beat consensus analyst forecasts, while Panel C shows the results from the subsample that miss consensus analyst forecasts. I find that the coefficients of SEO*SOR in all samples are insignificant. Therefore, I rule out the possibility that the reduction in real earnings management in SEO firms is driven by the predetermined reduction trend of real earnings management.

2.4.5. Voluntary disclosure

I hypothesize that increased voluntary disclosure after the SOR reduces real earnings management. Thus, I expect the effect to be more pronounced in firms with high voluntary disclosure. I divided the full sample into two subsamples where firms have above-median and below-median frequency of management forecasts. The results are presented in Table 2.10. I find that the coefficient of SEO*SOR is significant only in the subsample where firms have high voluntary disclosure.

2.5. CONCLUSION

The Securities Offering Reform (SOR) in 2005 eases the restrictions imposed by "gum jumping" rules on disclosure before equity offerings. It is an attempt by the SEC to reduce information asymmetry between issuing firms and investors around equity offerings. Previous studies on the SOR show that SEO firms release more 8k filings and management earnings forecasts in the post-SOR years. The increased disclosure leads to a better information environment around SEOs as evidenced by reduced underpricing, reduced cost of equity capital, and less information asymmetry. Building on these findings, I examine how SEO firms' real earnings management activities change following the passage of the SOR. My results show that firms reduce real activities manipulation during SEOs. In a more transparent information environment, firms reduce the level of their real earnings management activities during SEOs for two potential reasons. First, during special periods such as SEOs, firms face closer scrutiny from investors and regulators. Furthermore, the improved information environment post SOR makes earnings management activities more easily detected. Second, the reduced information asymmetry between issuing firms and investors post SOR reduces the magnitude of underpricing and thus provides fewer incentives for firms to manipulate earnings. I also find that the effect of the SOR on real earnings management in SEO firms is significant only in firms with high voluntary disclosure.

My paper has important policy implications in that I add to the debate concerning the effectiveness of regulations on security offerings in general and the SOR in particular. I provide supporting empirical evidence that the 2005 SOR is effective in that easing restrictions on voluntary disclosure during SEOs incentivizes firms to engage in less real earning management.

Table 2.1: Descriptive Statistics for SEO firms

Panel A: SEO characteristics

This panel provides summary statistics of the real earnings management proxies and variables used in the analyses. Column (1) reports statistics for the sample using absolute abnormal discretionary expense as real earnings management proxy. Column (2) reports statistics for the sample using absolute abnormal production costs as real earnings management proxy. Column (3) reports statistics for the sample using absolute abnormal cash flow from operations as real earnings management proxy. All samples span from 2003 to 2008. Sample sizes dependent on the data availability of calculating the real earnings management proxies. See Appendix for the definitions of the variables.

	(1) ACFO		(2) APRO		(3) AEXP	
	Median	Std. dev.	Median	Std. dev.	Median	Std. dev.
MVE(mil.)	747.628	2249.4	737.069	2315.66	743.12	2453.14
AT(mil.)	459.564	3408.17	452.606	3502.97	443.324	2051.54
OFFERSIZE	0.183	0.23	0.183	0.199	0.18	0.285
Q	1.972	1.208	1.976	1.242	2.098	1.314
ROA	0.034	0.157	0.033	0.166	0.047	0.086
MTB	2.011	1.195	2.015	1.228	2.111	1.307
LEV	0.191	0.187	0.19	0.19	0.134	0.204
SALESG	0.23	0.331	0.218	0.329	0.237	0.306
CASH	0.161	0.29	0.166	0.293	0.194	0.233
AGE	11	11.456	11	11.991	9	9.8
NUM	7	4.883	7	4.778	7	4.511
Observations	649		626		181	

Panel B: Number of SEOs by Year

This panel shows the number of SEOs in each of the sample years from 2003 to 2008. Column (1) reports the number of SEOs each year for the sample using absolute abnormal discretionary expense as real earnings management proxy. Column (2) reports the number of SEOs each year for the sample using absolute abnormal production costs as real earnings management proxy. Column (3) reports the number of SEOs each year for the sample using absolute abnormal cash flow from operations as real earnings management proxy.

	ACFO		APRO		AEXP	
	Number	Frequency	Number	Frequency	Number	Frequency
2003	124	19.11	118	18.85	39	21.55
2004	162	24.96	157	25.08	38	20.99
2005	103	15.87	99	15.81	26	14.36
2006	114	17.57	111	17.73	37	20.44
2007	104	16.02	101	16.13	31	17.13
2008	42	6.47	40	6.39	10	5.52
Total	649	100	626	100	181	100

Panel C: Number of SEOs by Industry

This panel shows the number of SEOs in each of the industry covered in the samples. Column (1) reports the number of SEOs in each industry for the sample using absolute abnormal discretionary expense as real earnings management proxy. Column (2) reports the number of SEOs in each industry for the sample using absolute abnormal production costs as real earnings management proxy. Column (3) reports the number of SEOs in each industry for the sample using absolute abnormal cash flow from operations as real earnings management proxy.

		(1)	(2)	(3)
	2-digit SIC Codes	ACFO	APRO	AEXP
All Others		75	75	25
Chemical Products	28	136	137	18
Computer equipment and services	35, 73	108	105	49
Durable goods	50	15	15	6
Eating and drinking establishments	58	4	4	4
Electronic equipment	36	62	60	14
Entertainment services	70, 78, 79	18	16	9
Food products	20	6	6	5
Health	80	17	14	3
Manufacturing	30-34	31	31	8
Oil and gas	13, 29	83	70	0
Paper and paper products	24-27	9	9	0
Retail	53, 54, 56, 57, 59	18	17	17
Scientific instruments	38	44	44	18
Transportations	37, 39, 40-42, 44, 45	23	23	5
N		649	626	181

Table 2.2: Descriptive Statistics for Real Earnings Management Proxies

Panel A: Real earnings management proxies around SEOs

This table reports real earnings management proxies from year -3 to year +3 relative to SEO years (year 0).

Year	-3	-2	-1	0	1	2	3
ACFO	0.066	0.066	0.077	0.093	0.089	0.078	0.078
APRO	0.167	0.166	0.172	0.211	0.187	0.187	0.196
AEXP	0.124	0.117	0.135	0.147	0.158	0.175	0.149

Panel B: Real earnings management proxies by years.

This table reports real earnings management proxies each year in the sample period.

Year	2003	2004	2005	2006	2007	2008
ACFO	0.096	0.08	0.135	0.075	0.097	0.085
APRO	0.155	0.125	0.172	0.144	0.119	0.104
AEXP	0.167	0.107	0.155	0.129	0.195	0.108

Table 2.3: Pearson Correlations

This table provides Pearson correlations between variables used in the main analysis. See the Appendix for the definitions of these variables. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Q	1										
Sales Growth	0.30***	1									
ROA	0.11***	0.01	1								
Mkt Val	0.31***	0.01	0.36***	1							
MTB	0.99***	0.29***	0.12***	0.32***	1						
Leverage	-0.23***	-0.07***	-0.03***	0.10***	-0.22***	1					
Forecast Error	-0.01	-0.01	-0.00	-0.02**	-0.01	-0.01	1				
CAR	0.31***	0.25***	0.15***	0.09***	0.31***	-0.05***	0	1			
Cash	0.38***	0.16**	-0.19***	-0.04***	0.37***	-0.46***	-0.01	0.08***	1		
Age	-0.14***	-0.22***	0.22**	0.41***	-0.13***	0.15***	-0.01	-0.02***	-0.28***	1	
LnNum of Analyst	0.29***	0.10***	0.28***	0.72***	0.30***	0.04***	-0.01**	0.06***	0.03***	0.19***	1

Table 2.4: Difference-in-differences Regression

This table shows the results from the difference- in-differences regressions estimated in the full sample. The dependent variables are absolute abnormal cash flows from operations (Column 1), absolute abnormal production costs (Column 2), absolute abnormal discretionary expenses (Column 3), a combined proxy based on abnormal production costs and discretionary expenses (Column 4), and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 5). Industry (2-digit SIC) and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1)	(2)	(3)	(4)	(5)
	ACFO	APRO	AEXP	RM1	RM2
SEO	0.0226***	0.0328***	0.0428***	0.0760***	0.0530**
	(4.03)	(3.23)	(2.68)	(3.32)	(2.56)
SEO*SOR	-0.0133*	-0.0198	-0.0367*	-0.0851***	-0.0529**
	(-1.70)	(-1.57)	(-1.69)	(-2.77)	(-1.99)
SALESG	0.0155***	0.0255***	0.1125***	0.1713***	0.1459***
	(3.54)	(4.02)	(9.28)	(10.93)	(9.40)
ROA	-0.1491***	-0.0324**	-0.1332***	-0.0890***	-0.2446***
	(-13.80)	(-2.34)	(-5.23)	(-2.94)	(-7.08)
LnMVE	-0.0091***	-0.0252***	-0.0268***	-0.0511***	-0.0369***
	(-8.28)	(-9.56)	(-8.02)	(-9.91)	(-8.93)
MTB	0.0273***	0.0394***	0.0333***	0.0635***	0.0562***
	(18.00)	(12.59)	(8.90)	(12.43)	(12.42)
LEV	-0.0373***	-0.0576***	-0.1019***	-0.1652***	-0.1546***
	(-4.38)	(-3.73)	(-3.87)	(-4.08)	(-4.58)
ANAFRE	0.0000	-0.0000***	0.0000**	-0.0000	0.0000
	(0.07)	(-5.89)	(2.11)	(-0.22)	(0.00)
CAR	0.0018	-0.0065**	0.0013	-0.0077	-0.0004
	(0.89)	(-2.20)	(0.31)	(-1.23)	(-0.07)
CASH	0.0363***	-0.0307**	0.0486**	0.0271	0.0788***
	(4.90)	(-2.50)	(2.30)	(0.93)	(2.91)
LnAGE	-0.0053***	0.0056	0.0078	0.0348***	0.0112
	(-2.85)	(1.39)	(1.14)	(3.24)	(1.37)
LnNUM	-0.0005	0.0032	0.0027	0.0003	0.0027
	(-0.28)	(1.20)	(0.59)	(0.04)	(0.46)
Constant	0.0995***	0.2137***	0.1934***	0.3848***	0.3275***
	(5.12)	(7.33)	(8.04)	(9.90)	(11.79)
Industry indicators	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes
Number of observations	14459	14278	5823	5733	5733
R-squared	0.071	0.077	0.131	0.170	0.152

Table 2.5: Subsample Analysis

Panel A Meeting or Beating Consensus Analyst Forecasts

This table shows the results from the difference-in-differences regressions estimated in the subsample where firms meet or beat consensus analyst forecasts. The dependent variables are absolute abnormal cash flow from operations (Column 1), absolute abnormal production costs (Column 2), absolute abnormal discretionary expenses (Column 3), a combined proxy based on abnormal production costs and discretionary expenses (Column 4), and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 5). Industry (2-digit SIC) and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) ACFO	(2) APRO	(3) AEXP	(4) RM1	(5) RM2
SEO	0.0187***	0.0296***	0.0365**	0.0696***	0.0415*
	(2.96)	(3.57)	(2.16)	(2.80)	(1.94)
SEO*SOR	-0.0171*	-0.0339***	-0.0508**	-0.1161***	-0.0581**
	(-1.79)	(-2.60)	(-1.99)	(-3.32)	(-1.98)
SALESG	0.0168***	0.0320***	0.1383***	0.1988***	0.1697***
	(2.90)	(3.57)	(8.56)	(9.36)	(8.66)
ROA	-0.1331***	-0.0290*	-0.1397***	-0.0845**	-0.2134***
	(-9.77)	(-1.74)	(-3.82)	(-2.10)	(-4.87)
LnMVE	-0.0087***	-0.0249***	-0.0268***	-0.0516***	-0.0371***
	(-7.06)	(-9.51)	(-6.45)	(-8.70)	(-7.91)
MTB	0.0274***	0.0407***	0.0305***	0.0666***	0.0553***
	(16.44)	(11.50)	(7.16)	(11.24)	(10.91)
LEV	-0.0258***	-0.0660***	-0.0970***	-0.1706***	-0.1346***
	(-2.73)	(-3.79)	(-2.75)	(-3.04)	(-3.23)
ANAFRE	0.0006***	-0.0019***	0.0094	0.0275	-0.0127
	(4.05)	(-21.94)	(0.55)	(1.04)	(-0.43)
CAR	-0.0013	-0.0085**	-0.0001	-0.0143*	-0.0072
	(-0.49)	(-2.33)	(-0.03)	(-1.87)	(-1.11)
CASH	0.0321***	-0.0273*	0.0546**	0.0532	0.0693**
	(3.78)	(-1.87)	(2.21)	(1.48)	(2.51)
LnAGE	-0.0058***	0.0085**	0.0073	0.0360***	0.0080
	(-2.97)	(2.07)	(1.00)	(3.11)	(0.98)
LnNUM	0.0006	0.0011	0.0001	-0.0036	0.0018
	(0.28)	(0.35)	(0.01)	(-0.46)	(0.27)
Constant	0.1052***	0.2149***	0.1887***	0.3598***	0.3179***
	(5.34)	(7.96)	(6.55)	(8.14)	(10.00)
Industry indicators	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes
Number of observations	9099	8963	3803	3746	3746
R-squared	0.058	0.112	0.150	0.195	0.176

Panel B Missing Consensus Analyst Forecasts

This table shows the results from the difference-in-differences regressions estimated in the subsample where firms miss consensus analyst forecasts. The dependent variables are absolute abnormal cash flow from operations (Column 1), absolute abnormal production costs (Column 2), absolute abnormal discretionary expenses (Column 3), a combined proxy based on abnormal production costs and discretionary expenses (Column 4), and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 5). Industry (2-digit SIC) and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) ACFO	(2) APRO	(3) AEXP	(4) RM1	(5) RM2
SEO	0.0334***	0.0343	0.0370	0.0672	0.0750
	(3.03)	(1.59)	(1.02)	(1.60)	(1.31)
SEO*SOR	-0.0116	-0.0141	0.0093	0.0130	-0.0307
	(-0.75)	(-0.53)	(0.18)	(0.22)	(-0.43)
SALESG	0.0190***	0.0246**	0.0848***	0.1381***	0.1163***
	(2.64)	(2.57)	(4.01)	(5.37)	(3.86)
ROA	-0.2125***	-0.0717***	-0.1937***	-0.1297***	-0.3701***
	(-12.82)	(-3.35)	(-5.55)	(-2.88)	(-6.62)
LnMVE	-0.0065***	-0.0163***	-0.0220***	-0.0359***	-0.0306***
	(-4.60)	(-5.95)	(-5.38)	(-5.76)	(-5.59)
MTB	0.0308***	0.0392***	0.0408***	0.0636***	0.0659***
	(12.21)	(9.22)	(6.92)	(8.21)	(8.32)
LEV	-0.0315**	-0.0532**	-0.0940***	-0.1492***	-0.1337***
	(-2.42)	(-2.37)	(-2.87)	(-3.25)	(-3.10)
ANAFRE	0.0000	-0.0000***	0.0000*	0.0000	0.0000
	(1.23)	(-3.74)	(1.83)	(0.41)	(0.69)
CAR	0.0035	-0.0133***	-0.0036	-0.0056	-0.0010
	(0.99)	(-2.64)	(-0.49)	(-0.54)	(-0.08)
CASH	0.0522***	-0.0165	0.0524	0.0005	0.1085**
	(4.46)	(-1.06)	(1.51)	(0.01)	(2.19)
LnAGE	-0.0019	0.0048	0.0131	0.0343**	0.0207*
	(-0.73)	(0.93)	(1.41)	(2.51)	(1.79)
LnNUM	-0.0034	0.0051	0.0061	0.0079	0.0031
	(-1.35)	(1.20)	(0.93)	(0.89)	(0.34)
Constant	0.0709**	0.1711***	0.1290***	0.2770***	0.2483***
	(2.46)	(4.21)	(4.39)	(5.98)	(6.66)
Industry indicators	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes
Number of observations	5360	5315	2020	1987	1987
R-squared	0.092	0.062	0.123	0.144	0.126

Table 2.6: Logistic Regression of Seasoned Equity Offerings

This table presents the results from logistic regressions of the likelihood of seasoned equity offerings, for the sample using aggregate real earnings management proxies (RM1 and RM2). The dependent variable is a binary variable that equals one for SEO firm-years, zero for non-SEO firm-years. See Appendix for the definitions of the independent variables. Firm and year fixed effects are included. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) Pr(SEO)=1
LnMVE	-0.7408**
	(-2.27)
Q	0.4108**
	(2.16)
ROA	-2.5712**
	(-2.04)
SALESG	0.7300*
	(1.83)
CASH	-3.0810**
	(-2.51)
CAR	0.5064**
	(2.14)
LnAGE	1.0007
	(0.81)
LnNUM	1.9959***
	(4.82)
Firm indicators	Yes
Year indicators	Yes
Number of observations	647
Chi2	100.54

Table 2.7: Matched Sample Covariate Balance

Panel A: Unmatched Sample

This panel reports the covariate balance in the unmatched sample. T test of the mean difference is performed for each of the covariates. See Appendix for the definitions of the variables. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1)	(2)	(3)
	Treated	Control	Difference
LnMVE	6.3426	6.5225	-0.1799
			(-1.49)
Q	2.4973	2.2362	0.2611
			(2.32)**
ROA	0.002	0.0099	-0.0079
			(-0.68)
SALESG	0.2985	0.2168	0.0817
			(2.96)***
CASH	0.3561	0.2611	-0.0293
			(-1.43)
CAR	0.3561	0.1114	2.2447
			(5.03)***
LnAGE	2.3019	2.4011	-0.0992
			(-1.71)*
LnNUM	2.0695	1.9888	0.0807
			(-1.38)

Panel B: Matched Sample

This panel reports the covariate balance in the matched sample constructed using propensity-score matching. T test of the mean difference is performed for each of the covariates. See Appendix for the definitions of the variables. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) Treated	(2) Control	(3) Difference
LnMVE	6.3426	6.5726	-0.23
			(-1.66)*
Q	2.4973	2.5931	-0.0958
			(-.66)
ROA	0.002	-0.0046	0.0066
			-0.43
SALESG	0.2985	0.3352	-0.0367
			(-0.92)
CASH	0.2318	0.2643	-0.0325
			(-1.33)
CAR	0.3561	0.3894	-0.0333
			(-0.53)
AGE	2.3019	2.2495	0.0524
			-0.69
LnNUM	2.0695	2.227	-0.1575
			(-2.56)**

Table 2.8: Difference-in-differences Regression in the Matched Sample

Panel A Full Sample

This table shows the results from the difference-in-differences regressions estimated in the full matched sample. The dependent variables are a combined proxy based on abnormal production costs and discretionary expenses (Column 1) and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 2). Industry (2-digit SIC) and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) RM1	(2) RM2
SEO	0.0921***	0.0725***
	(3.09)	(2.77)
SEO*SOR	-0.1149**	-0.0923**
	(-2.51)	(-2.22)
SALESG	0.1499**	0.1536***
	(2.42)	(2.60)
ROA	-0.1177	-0.3645**
	(-0.70)	(-2.14)
LnMVE	-0.0904***	-0.0516**
	(-3.18)	(-2.56)
MTB	0.1073***	0.0828***
	(5.81)	(5.47)
LEV	-0.2934*	-0.2131*
	(-1.88)	(-1.83)
ANAFRE	-0.3785	0.3851
	(-1.30)	(1.51)
CAR	-0.0494	-0.0298
	(-1.40)	(-1.11)
CASH	0.2960	0.1972
	(1.56)	(1.59)
LnAGE	0.0132	-0.0139
	(0.37)	(-0.46)
LnNUM	0.0510	0.0503
	(1.01)	(1.38)
Constant	0.6992***	0.4607***
	(4.36)	(3.51)
Industry indicators	Yes	Yes
Year indicators	Yes	Yes
Number of observations	328	328
R-squared	0.379	0.323

Panel B Meeting or Beating Consensus Analyst Forecasts
This table shows the results from the difference-in-differences regressions estimated in the matched subsample where firms meet or beat consensus analyst forecasts. The dependent variables are a combined proxy based on abnormal production costs and discretionary expenses (Column 1) and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 2). Industry (2-digit SIC) and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) RM1	(2) RM2
SEO	0.0816**	0.0484
	(2.23)	(1.52)
SEO*SOR	-0.1303**	-0.0870*
	(-2.18)	(-1.67)
SALESG	0.1480*	0.1562**
	(1.91)	(2.00)
ROA	-0.0460	-0.2622
	(-0.24)	(-1.43)
LnMVE	-0.1037***	-0.0588***
	(-3.33)	(-2.76)
MTB	0.0999***	0.0648***
	(4.93)	(3.99)
LEV	-0.2651	-0.1264
	(-1.44)	(-0.92)
ANAFRE	-0.2340	0.5637**
	(-0.76)	(2.02)
CAR	-0.0559	-0.0334
	(-1.28)	(-1.05)
CASH	0.3478	0.2324
	(1.53)	(1.57)
LnAGE	-0.0145	-0.0285
	(-0.39)	(-1.02)
LnNUM	0.0652	0.0698
	(0.96)	(1.54)
Constant	0.8813***	0.5193***
	(5.18)	(4.08)
Industry indicators	Yes	Yes
Year indicators	Yes	Yes
Number of observations	246	246
R-squared	0.410	0.328

Panel C Missing Consensus Analyst Forecasts

This table shows the results from the difference-in-differences regressions estimated in the matched subsample where firms miss consensus analyst forecasts. The dependent variables are a combined proxy based on abnormal production costs and discretionary expenses (Column 1) and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 2). Industry (2-digit SIC) and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) RM1	(2) RM2
SEO	0.0176	0.0582
	(0.28)	(0.88)
SEO*SOR	0.0815	-0.0088
	(0.77)	(-0.09)
SALESG	0.0048	0.0245
	(0.05)	(0.23)
ROA	-0.7630**	-1.0549***
	(-2.51)	(-3.51)
LnMVE	-0.0126	-0.0316
	(-0.38)	(-0.99)
MTB	0.1004***	0.1218***
	(3.06)	(4.39)
LEV	-0.1399	-0.2195
	(-0.63)	(-1.61)
ANAFRE	-19.5737*	-6.3781
	(-1.89)	(-0.68)
CAR	0.0540	0.0568
	(0.99)	(1.15)
CASH	-0.1499	-0.0463
	(-0.58)	(-0.25)
LnAGE	0.1467*	0.0862
	(1.66)	(1.21)
LnNUM	-0.1113	-0.0191
	(-1.24)	(-0.29)
Constant	0.0230	0.1529
	(0.06)	(0.46)
Industry indicators	Yes	Yes
Year indicators	Yes	Yes
Number of observations	82	82
R-squared	0.764	0.693

Table 2.9: Placebo Test

Panel A: Full Sample

This table shows the results from the difference - in-differences regressions estimated in the full sample. The SOR year is manually shifted to 2007, which is two years later than the actual SOR year. The dependent variables are absolute abnormal cash flow from operations (Column 1), absolute abnormal production costs (Column 2), absolute abnormal discretionary expenses (Column 3), a combined proxy based on abnormal production costs and discretionary expenses (Column 4), and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 5). Industry (2-digit SIC) and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) ACFO	(2) APRO	(3) AEXP	(4) RM1	(5) RM2
SEO	0.0150**	0.0221*	0.0147	0.0090	0.0117
	(2.50)	(1.79)	(1.33)	(0.50)	(0.82)
SEO*SOR	0.0090	0.0025	0.0143	0.0429	0.0373
	(0.98)	(0.17)	(0.74)	(1.52)	(1.35)
SALESG	0.0162***	0.0253***	0.0940***	0.1427***	0.1263***
	(3.64)	(3.71)	(7.79)	(10.51)	(9.33)
ROA	-0.1189***	-0.0481***	-0.1094***	-0.0795***	-0.1449***
	(-11.78)	(-3.41)	(-5.01)	(-3.06)	(-5.39)
LnMVE	-0.0085***	-0.0229***	-0.0229***	-0.0424***	-0.0316***
	(-8.30)	(-8.45)	(-7.25)	(-9.14)	(-8.54)
MTB	0.0264***	0.0362***	0.0258***	0.0478***	0.0452***
	(16.40)	(10.01)	(7.13)	(11.02)	(10.17)
LEV	-0.0311***	-0.0471***	-0.1030***	-0.1750***	-0.1453***
	(-3.58)	(-3.18)	(-4.48)	(-5.18)	(-4.66)
ANAFRE	0.0000	-0.0000***	0.0000***	0.0000**	0.0000*
	(1.20)	(-7.68)	(3.71)	(1.99)	(1.72)
CAR	0.0032	-0.0030	0.0052	0.0049	0.0069*
	(1.59)	(-1.13)	(1.56)	(1.18)	(1.75)
CASH	0.0410***	-0.0286*	0.0568***	0.0169	0.0587**
	(5.45)	(-1.68)	(2.65)	(0.61)	(2.35)
LnAGE	-0.0052***	0.0085**	0.0058	0.0219**	0.0034
	(-2.72)	(2.08)	(0.92)	(2.28)	(0.46)
LnNUM	0.0011	0.0063**	0.0075*	0.0109*	0.0075
	(0.66)	(2.07)	(1.93)	(1.86)	(1.60)
Constant	0.1000***	0.1903***	0.1562***	0.3382***	0.2811***
	(3.90)	(4.67)	(5.64)	(8.28)	(8.59)
Industry indicators	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes
Number of observations	14125	13898	5858	5753	5753
R-squared	0.067	0.065	0.123	0.161	0.153

Panel B: Meeting or Beating Consensus Analyst Forecasts

This table shows the results from the difference-in-differences regressions estimated in the subsample where firms meet or beat consensus analyst forecasts. The SOR year is manually shifted to 2007, which is two years later than the actual SOR year. The dependent variables are absolute abnormal cash flow from operations (Column 1), absolute abnormal production costs (Column 2), absolute abnormal discretionary expenses (Column 3), a combined proxy based on abnormal production costs and discretionary expenses (Column 4), and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 5). Industry (2-digit SIC) and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) ACFO	(2) APRO	(3) AEXP	(4) RM1	(5) RM2
SEO	0.0112*	0.0097	0.0089	-0.0017	0.0039
	(1.70)	(0.99)	(0.71)	(-0.08)	(0.24)
SEO*SOR	0.0194*	0.0144	0.0218	0.0449	0.0466
	(1.66)	(0.99)	(0.94)	(1.33)	(1.31)
SALESG	0.0163***	0.0179*	0.1254***	0.1663***	0.1541***
	(2.77)	(1.76)	(8.58)	(8.98)	(9.14)
ROA	-0.1134***	-0.0544***	-0.1084***	-0.0961***	-0.1482***
	(-8.84)	(-3.15)	(-3.76)	(-2.88)	(-4.18)
LnMVE	-0.0069***	-0.0215***	-0.0241***	-0.0418***	-0.0296***
	(-6.13)	(-8.60)	(-6.11)	(-7.42)	(-6.63)
MTB	0.0253***	0.0359***	0.0258***	0.0505***	0.0452***
	(14.14)	(8.66)	(6.66)	(9.76)	(9.22)
LEV	-0.0294***	-0.0369**	-0.1083***	-0.1559***	-0.1357***
	(-2.85)	(-2.02)	(-3.96)	(-3.81)	(-3.93)
ANAFRE	0.0006***	-0.0014***	0.0135***	0.0223***	0.0189***
	(3.96)	(-14.85)	(8.54)	(9.28)	(9.25)
CAR	0.0009	0.0006	0.0012	0.0011	-0.0021
	(0.35)	(0.17)	(0.32)	(0.19)	(-0.44)
CASH	0.0444***	-0.0124	0.0717***	0.0731**	0.0973***
	(5.29)	(-0.80)	(2.94)	(2.27)	(3.58)
LnAGE	-0.0053***	0.0098**	0.0148**	0.0353***	0.0139*
	(-2.68)	(2.36)	(2.17)	(3.46)	(1.82)
LnNUM	0.0001	0.0049	0.0036	0.0054	0.0027
	(0.07)	(1.58)	(0.72)	(0.71)	(0.45)
Constant	0.1205***	0.1728**	0.1351***	0.2562***	0.2097***
	(3.40)	(2.37)	(3.88)	(5.39)	(5.28)
Industry indicators	Yes	Yes	Yes	Yes	Yes
Year indicators	Yes	Yes	Yes	Yes	Yes
Number of observations	8890	8719	3798	3731	3731
R-squared	0.053	0.080	0.168	0.183	0.181

Panel C Missing Consensus Analyst Forecasts

This table shows the results from the difference-in-differences regressions estimated in the subsample where firms miss consensus analyst forecasts. The SOR year is manually shifted to 2007, which is two years later than the actual SOR year. The dependent variables are absolute abnormal cash flow from operations (Column 1), absolute abnormal production costs (Column 2), absolute abnormal discretionary expenses (Column 3), a combined proxy based on abnormal production costs and discretionary expenses (Column 4), and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 5). Industry and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1) ACFO	(2) APRO	(3) AEXP	(4) RM1	(5) RM2
SEO	0.0239**	0.0413*	0.0302	0.0341	0.0334
	(2.04)	(1.81)	(0.98)	(0.72)	(0.76)
SEO*SOR	-0.0111	-0.0171	-0.0311	-0.0018	-0.0020
	(-0.65)	(-0.64)	(-0.68)	(-0.03)	(-0.03)
SALESG	0.0152**	0.0376***	0.0516**	0.0989***	0.0842***
	(2.19)	(3.69)	(2.43)	(4.46)	(3.39)
ROA	-0.1789***	-0.0839***	-0.1851***	-0.0908**	-0.2289***
	(-11.31)	(-3.53)	(-4.92)	(-1.99)	(-5.43)
LnMVE	-0.0076***	-0.0156***	-0.0206***	-0.0346***	-0.0320***
	(-5.30)	(-5.08)	(-5.28)	(-6.21)	(-6.76)
MTB	0.0288***	0.0358***	0.0290***	0.0517***	0.0550***
	(11.24)	(7.41)	(4.68)	(7.50)	(8.02)
LEV	-0.0291**	-0.0643***	-0.0574**	-0.1514***	-0.1010***
	(-2.31)	(-3.12)	(-1.98)	(-3.51)	(-2.61)
ANAFRE	0.0000**	-0.0000***	0.0000**	0.0000*	0.0000**
	(2.03)	(-4.45)	(2.56)	(1.70)	(1.96)
CAR	0.0070**	-0.0119***	0.0156*	0.0120	0.0183*
	(2.19)	(-2.92)	(1.88)	(1.43)	(1.92)
CASH	0.0485***	-0.0153	0.0664*	-0.0236	0.0605
	(4.21)	(-0.66)	(1.81)	(-0.55)	(1.47)
LnAGE	-0.0032	0.0072	0.0024	0.0095	0.0005
	(-1.17)	(1.41)	(0.31)	(0.81)	(0.05)
LnNUM	0.0025	0.0097*	0.0177***	0.0254***	0.0196***
	(1.03)	(1.95)	(3.02)	(3.12)	(2.73)
Constant	0.0524**	0.1379***	0.1114***	0.2933***	0.2558***
	(2.31)	(5.27)	(3.34)	(6.06)	(6.66)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	5235	5179	2060	2022	2022
R-squared	0.066	0.053	0.090	0.133	0.115

Table 2.10: Voluntary Disclosure

This table shows the results from the difference-in-differences regressions estimated in the subsamples where firms have high and low voluntary disclosure. High voluntary disclosure is an indicator variable equal to one for firms with above-median frequency of management forecast of earnings per share in the fiscal year, and zero otherwise. The dependent variables are a combined proxy based on abnormal production costs and discretionary expenses (Column 1 and 3) and a combined proxy based on abnormal cash flow from operations and discretionary expenses (Column 2 and 4). Industry (2-digit SIC) and year fixed effects are included. See Appendix for the definitions of the independent variables. T-statistics are based on robust standard errors that are clustered by firm. *, **, and *** denote 10%, 5%, and 1% levels of significance, respectively.

	(1)	(2)	(3)	(4)
	High Voluntary Disclosure		Low Voluntary Disclosure	
	RM1	RM2	RM1	RM2
SEO	0.1002*	0.0633*	0.0511*	0.0207
	(1.91)	(1.93)	(1.73)	(0.88)
SEO*SOR	-0.1720**	-0.1133*	-0.0497	-0.0500
	(-2.17)	(-1.95)	(-1.34)	(-1.63)
SALESG	0.1292***	0.1095***	0.1502***	0.1290***
	(3.22)	(3.12)	(5.12)	(4.37)
ROA	0.0102	-0.0676	-0.0972*	-0.2253***
	(0.11)	(-0.75)	(-1.73)	(-3.50)
LnMVE	-0.0628***	-0.0388***	-0.0592***	-0.0449***
	(-5.73)	(-4.76)	(-6.83)	(-7.16)
MTB	0.0857***	0.0654***	0.0634***	0.0488***
	(8.04)	(7.57)	(6.62)	(6.67)
LEV	-0.2486***	-0.2124***	-0.1430***	-0.1239***
	(-2.90)	(-3.71)	(-2.63)	(-2.65)
ANAFRE	-3.0712	5.2960	-0.1739	-0.2242**
	(-0.36)	(0.86)	(-1.40)	(-2.23)
CAR	-0.0309*	-0.0137	-0.0066	0.0022
	(-1.76)	(-1.02)	(-0.69)	(0.29)
CASH	0.1768**	0.1308***	-0.0070	0.0251
	(2.00)	(2.64)	(-0.16)	(0.66)
LnAGE	0.0445**	0.0189	0.0399**	0.0180*
	(2.19)	(1.47)	(2.57)	(1.71)
LnNUM	-0.0270*	-0.0204*	0.0031	0.0080
	(-1.67)	(-1.71)	(0.30)	(0.97)
Constant	0.5311***	0.3936***	0.3854***	0.3556***
	(5.43)	(6.19)	(6.66)	(8.70)
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Number of observations	1386	1386	1737	1737
R-squared	0.256	0.232	0.146	0.146

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Appendix

Appendix A: Examples of the BrokerCheck Report

1. DEUTSCHE BANK SECURITIES INC.

CRD# 2525

Disclosure 89 of 267

Reporting Source:	Regular
Current Status:	Final
Allegations:	FINRA RULES 2010, 2360(B)(3), 2360(B)(3)(A)(VII)(B)(4)(B), 2360(B)(5), NASD RULES 2110, 2860(B)(5), 3010 - DEUTSCHE BANK SECURITIES, INC FAILED TO REPORT OPTIONS POSITION IN CONVENTIONAL OPTIONS TO THE LARGE OPTION POSITION REPORTS (LOPR) FOR OVER TWO YEARS. ON SIX CONSECUTIVE DAYS, ONE OF THE FIRM'S CUSTOMERS EXCEEDED THE POSITION LIMIT ON THE BEARISH SIDE OF THE MARKET IN A SECURITY. THE FIRM FAILED TO SUBMIT TO THE OPTIONS CLEARING CORPORATION ITS OPTIONS CONTRACT EQUIVALENT TO NET DELTA (OCEND) FOR APPROXIMATELY TWO MONTHS. THE FIRM FAILED TO ACCURATELY REPORT OPTIONS POSITIONS IN CONVENTIONAL OPTIONS TO THE LOPR. THE FIRM FAILED TO IMPLEMENT AND MAINTAIN AN ADEQUATE SYSTEM OF FOLLOW-UP AND REVIEW DESIGNED TO REASONABLY ENSURE THE SUBMISSION OF COMPLETE AND ACCURATE LOPRS.
Initiated By:	FINRA
Date Initiated:	11/21/2012
Docket/Case Number:	2008016167401
Principal Product Type:	Options
Resolution:	Acceptance, Waiver & Consent (AWC)
Resolution Date:	11/21/2012
Does the order constitute a final order based on violations of any laws or regulations that prohibit fraudulent, manipulative, or deceptive conduct?	No

Sanctions Ordered:	Censure Monetary/Fine $35,000.00
Sanction Details:	WITHOUT ADMITTING OR DENYING THE FINDINGS, THE FIRM CONSENTED TO THE DESCRIBED SANCTIONS AND TO THE ENTRY OF FINDINGS; THEREFORE, THE FIRM IS CENSURED AND FINED $35,000. FINE PAID IN FULL ON 12/12/12.

2. MERRILL LYNCH, PIERCE, FENNER & SMITH INCORPORATED
CRD# 7691
Disclosure 135 of 562

Reporting Source:	Regular
Current Status:	Final
Allegations:	FINRA RULE 2010, NASD RULES 2110, 3010, 3012(A)(2)(B)(I) - MERRILL LYNCH'S SUPERVISORY CONTROL SYSTEM FAILED TO INCLUDE A POLICY OR PROCEDURE REQUIRING A REVIEW TO DETECT OR PREVENT MULTIPLE TRANSMITTALS OF FUNDS FROM MULTIPLE CUSTOMERS GOING TO THE SAME THIRD PARTY ACCOUNTS. THE FIRM'S SYSTEM FAILED TO INCLUDE EXCEPTION REPORTS THAT WOULD HAVE IDENTIFIED MULTIPLE CUSTOMER WIRES GOING TO THE SAME THIRD PARTY ACCOUNT. CONSEQUENTLY, THE FIRM FAILED TO DETECT THAT A REGISTERED REPRESENTATIVE HAD INITIATED FUND TRANSFERS TOTALING APPROXIMATELY $887,931 OUT OF CUSTOMER ACCOUNTS TO BANK ACCOUNTS HE APPARENTLY CONTROLLED. THE REGISTERED REPRESENTATIVE HAS BEEN BARRED FROM ASSOCIATION WITH ANY FINRA MEMBER IN ANY CAPACITY.
Initiated By:	FINRA
Date Initiated:	08/03/2012
Docket/Case Number:	2010022652202
Other Product Type(s):	WIRE TRANSFERS
Resolution:	Acceptance, Waiver & Consent (AWC)
Resolution Date:	41124
Does the order constitute a final order based on violations of any laws or regulations that prohibit	No

fraudulent, manipulative, or deceptive conduct?	
Sanctions Ordered:	Censure Monetary/Fine $450,000.00
Sanction Details:	WITHOUT ADMITTING OR DENYING THE FINDINGS, THE FIRM CONSENTED TO

Appendix B: Variable Definitions for Chapter 1

This table provides definitions of variables used in the multivariate analysis.

Variable	Definition
	Dependent variable and FINRA violation-related variables
CAR (-2, +2)	Cumulative abnormal return of the bidder stock in the five-day event window (-2, +2) where 0 is the SDC deal announcement date. The returns are calculated using the market model with the parameters estimated over the period beginning 240 days and ending 41 days prior to the announcement date using CRSP value-weighted return as the market return.
Premium	The offer price relative to the target's stock price 105 trading days before the acquisition announcement divided by the latter. Values greater than two are truncated.
Price Revision	Dummy equal to one if final offer price is greater than the initial offer price and zero otherwise.
Violations:	
Ln#Violations	Logarithm of one plus the number of annual violations reported in BrokerCheck reports.
Ln$Fines	Logarithm of one plus the dollar amount of annual fines reported in BrokerCheck reports.
	Firm characteristics
LnSize	Logarithm of bidder market value of equity (number of shares outstanding times the stock price from CRSP) four weeks prior to the SDC deal announcement date
ROA	Income before extraordinary items divided by total assets at the fiscal year-end prior to the announcement from Compustat.
BTM	Book value of equity at the fiscal year-end prior to the announcement divided by the market value of equity four weeks prior to the SDC deal announcement date.
Leverage	Total debt (long-term debt plus debt in current liabilities) divided by the sum of total debt and market value of equity at the fiscal year-end prior to the announcement from Compustat.
Liquidity	Cash and short-term investments divided by total assets at the fiscal year-end prior to the announcement from Compustat.
Herfindahl	Sales divided by the sum of annual industry sales.
Run-up	Market-adjusted buy-and-hold return of the bidder stock over the period beginning 205 days and ending six days prior to the SDC deal announcement date using CRSP value-weighted return as the market return.
Sigma	Standard deviation of the market-adjusted daily returns of the bidder stock over the period beginning 205 days and ending six days prior to the SDC deal announcement date using CRSP value-weighted return as the market return.
	Deal characteristics

Non-Tender	Dummy equal to one if the deal is reported as non-tender offer in SDC, zero otherwise.
Public Target	Dummy equal to one if the target is reported as a public firm in SDC, zero otherwise.
Relative Size	Value of transaction divided by bidder market value of equity four weeks prior to the SDC deal announcement date.
Payment Incl Stock	Dummy equal to one if the considerations reported in SDC include stock, zero otherwise.
All Cash Deal	Dummy equal one if the sole consideration reported in SDC is cash, zero otherwise.
Focus Deal	Dummy equal to one if bidder and target are in the same industry in SDC, zero otherwise.
Complex Deal	Dummy equal to one if there are multiple financial advisors in a deal reported in SDC, zero otherwise.
Hostile	Dummy equal to one if the deal is reported as hostile in SDC, zero otherwise.
Multiple Bidder	Dummy equal to one if there are multiple bidders in a deal as reported in SDC, zero otherwise.
Financial advisor characteristics	
Mkt Shr	Financial advisor's market share by dollar value of transactions over a calendar year.
EWCAR	Equal-weighted average of the CARs of financial advisor's clients over one-year period.
VWCAR	Value-weighted average of the CARs of financial advisor's clients over one-year period, calculated by multiplying the bidder CARs of financial advisor's clients by bidder market value of equity four weeks prior to the SDC deal announcement date, normalized by the total equity market value of these clients.
%Completed	Number of completed deals divided by total number of deals over one-year period.
%All-cash	Number of all-cash deals divided by total number of deals over one-year period.
%Hostile	Number of hostile deals divided by total number of deals over one-year period.

Appendix C: Variable Definitions for Chapter 2

This table provides definitions of variables.

Variable Name	Measurement
Real Earnings management measures (EM)	
APRO	Absolute abnormal production costs
AEXP	Absolute abnormal discretionary expenses
ACFO	Absolute abnormal cash flow from operations
RM1	APRO+AEXP
RM2	ACFO+AEXP
Firm characteristics	
SEO	Dummy equals one for SEO firm-years; zero for non-SEO firm-years
SOR	Dummy equals one after 2005; zero in or before 2005
OFFERSIZE	Offer Price
AT	Total assets
LEV	(Long term debt + Debt in current liabilities) / (Long term debt + Debt in current liabilities + Market value)
MVE	Price * Common shares outstanding
LnMVE	Logarithm of MV E
MTB	(Total assets Stockholders equity + Market value) / Total assets
ANAFRE	Abs (Actual earnings consensus analyst forecasts)/lag(Market Value)
NUM	Number of analysts following
LnNUM	Logarithm of the number of analysts following
CASH	Cash and short-term investments / Total assets
SALESG	ΔSales/lag(Sales)
CAR	Cumulative value weighted abnormal return in the prior year calculated using monthly returns
ROA	Income before extraordinary item / Total assets
AGE	Number of years since first appearance in Compustat
LnAGE	Logarithm of AGE
Q	(Market value + Total assets Book value of equity Deferred Taxes) / Total assets
High Voluntary Disclosure	An indicator variable equal to one for firms with above-median frequency of management forecast of earnings per share in the fiscal year, and zero otherwise.

Appendix D: Sample Selections for Chapter 2

Filters	# Offerings
All common stock offerings from SDC New Issuance database from 01/01/2003 to 12/31/2008	36,979
Issuer nation: US	4,221
Listing: NYSE amex or NASDAQ	1,649
Exclude spinoffs	1,631
Exclude LBO firms	1,619
Exclude closed-end fund/trusts	1,614
Exclude unit investment trusts	1,600
Exclude limited partnerships	1,560
Exclude rights issues	1,546
Exclude simultaneous offerings	1,512
Exclude unit issues	1,512
Exclude offer price less than $5	1,429
Exclude offerings lacking financial information from Compustat, stock price information from CRSP, and analyst forecast information from I/B/E/S to construct real earnings management proxies and control variables:	
Abnormal cash flows (ACFO) sample	649
Abnormal production costs (APRO) sample	626
Abnormal discretionary expenses (AEXP) sample	181

Vita

Yicheng Zhu obtained her Bachelor of Business Administration in Accounting from University of Wisconsin – Eau Claire in 2015. She also obtained a Master of Business Taxation from University of Minnesota in 2017. In the same year, she joined the doctoral program in Business Administration at the University of Texas at El Paso with a Finance concentration.