**STUDIES ON THE CAPITAL MARKET CONSEQUENCES OF FINANCIAL**

**REPORTING WEAKNESSES**

by

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# Abstract

This thesis is a collection of two studies on the capital market consequences of financial reporting weaknesses. Chapter 2 examines the change in voluntary disclosure of internal control deficiencies (ICD) by IPO firms after the JOBS Act. The JOBS Act postponed the compliance deadline of internal control audits after IPO and increased the number of small IPO firms with potential ICD. I find that IPO firms are more likely to disclose ICD after the JOBS Act. Further, post-JOBS IPO firms who are willing to disclose ICD experience lower underpricing. These results are consistent with a dynamic view that as investors rationally update the belief of increasing "lemons" in the IPO population after the JOBS Act, IPO firms become more forthcoming with ICD disclosure. Chapter 3, co-authored with Professors Weili Ge, Dawn Matsumoto, and Jenny Li Zhang, examines the stock market consequences of disclosing accounting irregularities for U.S. listed foreign firms. We find that foreign firms experience significantly more negative two-day stock market reactions following restatement announcements than U.S. firms. Moreover, for a sample of foreign firms that are listed on both a U.S. and home country stock exchange, we find that restating firms' U.S. investors react more negatively to the same restatement than their home-country investors. This differential market reaction appears related to firm-specific information frictions that are greater for foreign firms than U.S. firms. We also find a geographic contagion effect as non-restating firms from the same country experience significant stock price declines following restatements. Within a country-year, this contagion effect is concentrated among firms with lower accrual quality, suggesting that foreign firms' restatements cause investors to alter their assessment of the earnings quality of non-restating firms from the same country. Collectively, our results suggest that accounting irregularities cause U.S. investors to reassess the information risk associated with foreign firms.

## Lay Summary

This thesis studies the stock market consequences of financial reporting weaknesses. The first study shows that against the simple intuition that firms will take advantage of a recent regulatory relief under the JOBS Act, post-JOBS IPO firms are more willing to voluntarily disclose internal control deficiencies. This seemingly counterintuitive observation is consistent with a dynamic view on the interaction between investors and firms: As the average internal control quality of post-JOBS IPO firms worsens, investors discount non-disclosing firms more aggressively. In response, IPO firms become more forthcoming. The second study shows that U.S. investors react more negatively to foreign firms than U.S. firms for comparable accounting irregularities and that U.S. investors react more negatively than home country investors to the same accounting irregularity. It suggests that the stock market imposes greater punishment on foreign firms listed in the U.S. for breaching the bond with the U.S. regulatory system.

# Preface

Chapter 2 is original and independent work by the author, Jing Wang. Chapter 3 is co-authored with Professor Weili Ge and Professor Dawn Matsumoto from the University of Washington and Professor Jenny Li Zhang from the University of British Columbia. I initiated this project as my first-year summer paper, most of which evolved into the analysis related the second hypothesis of Chapter 3. Since coauthors joined this project, I have been responsible mainly for data analysis and I have contributed to research design decisions and editing. My co-authors are mainly responsible for formal writing of the manuscript. All authors contribute equally to conceptual foundation, idea development, and empirical analysis.

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My Ph.D. life would not have been as smooth without the help from our divisional assistant Debra Harris and Ph.D. administrator Elaine Cho. I owe my survival in the program to the friendship and support from my fellow Sauder PhD friends, in and out of the Accounting Division. I also thank my parents for tolerating the fact that I remain a student for many years. Finally, I

# Chapter 1: Introduction

This thesis is a collection of two studies on the capital market consequences of financial reporting weaknesses. The first study examines the impact of the Jumpstart Our Business Startups Act (the JOBS Act) on the voluntary disclosure of internal control deficiencies by IPO firms. Passed in April 2012, the JOBS Act was designed to make IPO more attractive for smaller firms by postponing internal control audits, one of the most controversial provisions in the Sarbanes-Oxley Act of 2002. The JOBS Act was championed by politicians, celebrated by entrepreneurs, criticized by the SEC, and cast in doubt by the financial press. The intuition expressed by the financial press was that as auditor oversight is delayed, firms will simply take advantage of the lax monitoring and reduce IPO disclosure. I find the opposite; following the JOBS Act, IPO firms become more willing to voluntarily disclose internal control deficiencies. I show that this seemingly counterintuitive empirical observation is consistent with a dynamic view where both investors and firms rationally update the expectations of the change in the IPO population brought by the JOBS Act. The second study explores how investors reassess information risk associated with foreign firms listed in the U.S. following accounting irregularities. We find that foreign firms experience significantly more negative two-day stock market reactions following announcements of accounting irregularities than U.S. firms. Moreover, for foreign firms listed on both a U.S. and home country stock exchange, we find evidence that restating firms' U.S. investors react more negatively to the same restatement than their home-country investors. This differential market reaction appears related to firm-specific information frictions that are greater for foreign firms than U.S. firms. We also find that non-restating firms from the same country experience significant negative market reactions following restatements.

Although the topics are diverse, both studies touch on the stock market consequences of financial reporting weaknesses, either in the form of internal control deficiencies or accounting irregularities (material accounting restatements). In the first study, the expectation of investors' reaction to voluntary disclosure of internal control deficiencies shapes IPO firms' disclosure decisions. The second study documents new empirical patterns of the stock market reactions to accounting irregularities made by foreign firms listed in the U.S. and aims to shed light on the role of firm-specific and country-specific factors in the market reactions. Because each study investigates a different topic, chapters were designed to be self-contained. I thus leave a more thorough discussion of the research question and contribution to the introduction specific to each chapter.

# Chapter 2: More Lemons, More Disclosure? The JOBS Act and IPO Disclosure of Internal Control Deficiencies

## 2.1 Introduction

This paper examines the impact of the Jumpstart Our Business Startups Act (the JOBS Act) on the disclosure of internal control deficiencies (ICD) during initial public offerings (IPO). Although IPO firms are generally perceived as going through higher regulatory scrutiny than publicly-traded firms, ICD disclosure is one of the few areas in which IPO firms are subject to a weaker compliance requirement. Specifically, IPO firms are not required to certify, evaluate, or disclose the effectiveness of internal controls. ICD disclosure is voluntary at the IPO stage. Under the Sarbanes Oxley Act of 2002 (SOX), the earliest compliance deadline of management assessment and auditor attestation of internal controls is the second annual report after IPO. Passed in April 2012, Title I of the JOBS Act was designed to make IPO more attractive for smaller firms by reducing regulatory burdens. In particular, it delays the requirement of auditor attestation from the second annual report until five years after the IPO date.[1]

The JOBS Act is not without controversy. Supporters of the Act argue that it smooths the IPO path for small businesses and ultimately spurs job growth. Yet this regulatory rollback has been under attack by the Securities and Exchange Commission (SEC). In a letter to the Senate dated in March 2012, the SEC Chair Mary Schapiro wrote, "I continue to believe that the internal controls audit requirement put in place after the Enron and other accounting scandals of the early 2000's has significantly improved the quality and reliability of financial reporting and provides

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[1] For instance, if a firm with a December 31 fiscal year end went public on May 30, 2013, the regulatory relief under the JOBS Act would remain effective until December 31, 2018.

important investor protections, and therefore believe this change is unwarranted." Upon the passage of the JOBS Act, some financial commentators also expressed the concern that the further delay of the internal controls audit requirement would exacerbate the dearth of ICD disclosure in the IPO process.[2]

Despite the postponed requirement of mandatory internal control audits, it is unclear ex ante whether IPO firms are more likely to voluntarily disclose ICD in registration statements after the JOBS Act. On the one hand, fewer IPO firms might reveal ICD because the regulatory rollback of auditor attestation makes it easier for them to suppress the bad news about control weaknesses. This is the situation the financial press fears. On the other hand, the JOBS Act may increase the probability of ICD among IPO firms and ultimately increase voluntary ICD disclosure. This conjecture is guided by the disclosure theory built on the assumption that investors are uncertain about the manager's information endowment (i.e., Dye 1985; Jung and Kwon 1988). In these models, the manager receives private information with an exogenous probability. In the absence of disclosure, investors are uncertain whether nondisclosure is due to absence of private information or due to its unfavorable content. In equilibrium, the manager discloses private information only if it exceeds a threshold of firm value. Non-disclosing firms are discounted by investors who infer that the pool of non-disclosing firms contains firms withholding bad news; that is, the "lemons." Mapping the disclosure theory to ICD disclosure after the JOBS Act, in the

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[2] Groupon went public in November 2011. In April 2012, its shares fell by 17 percent upon the announcement that the company identified a material weakness in its internal controls over financial reporting. Amid the news of the JOBS Act, Bloomberg View commented, "The act is a lurch in the opposite direction of what is needed…The existing protections for IPO investors were feeble before the new law. That Groupon could stay mum for so long about any control weaknesses it had, legally, is merely the latest evidence. There is only one solution for investors who aren't insiders: Don't ever buy stock in a company that just went public" (see https://www.bloomberg.com/view/articles/201 2-04-04/groupon-ipo-scandal-is-the-sleaze-that-s-legal).

absence of ICD disclosure, investors cannot distinguish whether there is no ICD or the manager is concealing private information of poor internal control quality. Because the JOBS Act increased the number of small firms going public, and because small firms are more likely to have weak internal controls due to inadequate resources, the JOBS Act could lead to poorer internal control quality on average among IPO firms.

In terms of firm value, investors would rationally assign a lower firm value to non-disclosing firms to reflect the downwards revision in their belief of the internal control quality among the population of IPOs. From the perspective of firms endowed with private information, it suggests that the benefit of deliberately withholding ICD to be pooled with other non-disclosing firms diminishes after the JOBS Act. As a consequence, IPO firms on the pre-JOBS disclosure threshold now find it in their interests to disclose internal control quality, resulting in a lower disclosure threshold in the new post-JOBS equilibrium.[3]

I test these predictions by examining the impact of the JOBS Act on voluntary ICD disclosure. The JOBS Act grants regulatory relief to IPO firms with revenue below $1 billion in the most recent fiscal year. This revenue threshold is sufficiently high so that most firms going public after the JOBS Act qualify for reduced disclosure requirements. Given a lack of contemporaneous control firms after the JOBS Act, I compare the ICD disclosure between pre- and post-JOBS IPO firms. A potential concern in this empirical strategy is that any observed

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[3] Anecdotal evidence suggests that IPO firms eligible for the JOBS Act, known as Emerging Growth Companies (EGCs), are more willing to disclose material weaknesses in internal controls due to the updated expectation. A PwC publication documents the growing trend of firms disclosing material weaknesses in registration statements, and explains the rationale for this disclosure of material weaknesses as follows, "the introduction of the JOBS Act has allowed a greater number of smaller companies to register as EGCs. Investors generally recognize that these companies are not as sophisticated as larger organizations in terms of resources, processes, and controls." (See http://www.pwc.com/us/en/deals/publications/ipo-material-weakness.html)

difference in ICD disclosure may be attributed to differences in firm characteristics between pre- and post-JOBS IPO firms. By design, the JOBS Act allows some small firms to go public that would otherwise have been turned away from the stock market. To address the potential selection bias in the post-JOBS IPO firms, I include in the regressions a set of control variables that prior literature finds to be correlated with ICD disclosure. I also use propensity score matching as an alternative approach to addressing this selection bias, and to mitigate the concern of potential functional form misspecification in linear regressions. I find that in the full (matched) sample, after the JOBS Act, IPO firms are 9.1 percent (8.3 percent) more likely to disclose the status of internal controls.

I go on to differentiate the nature of ICD disclosure. The status of internal controls entertains two possibilities: ICD have been fully remediated; ICD remain unremediated. While it is not clear whether the former is good news, the latter in itself is generally considered as bad news. However, I also find that in the full (matched) sample, IPO firms are 11.6 percent (9.2 percent) more likely to disclose that they have ICD still unremediated by the time of IPO after the JOBS Act.

To lend weight to the argument that revised expectations of the ICD probability lead to the increased disclosure, I also exploit the institutional variation that not all IPO firms are equally affected by the JOBS Act. Even before the JOBS Act, firms with a public float of less than $75 million, known as non-accelerated filers, are exempt from auditor attestation of internal controls. The delay in auditor attestation granted by the JOBS Act changes the probability of ICD in the population of IPO firms but does not affect the regulatory burden of firms expecting to qualify for the exemption. I find that the would-be exempt firms are significantly more inclined to disclose ICD after the JOBS Act. This finding is consistent with the argument that the increased voluntary

disclosure after the JOBS Act is attributed to the expectation of increased likelihood of ICD brought about by the addition of post-JOBS IPO firms.

Next, I examine the benefits of ICD disclosure in terms of reduced IPO underpricing. IPO underpricing is thought to arise in part from the information asymmetry between IPO firms and investors (Ljungqvist 2005; Ritter and Welch 2002). As the JOBS Act allows more firms with potential ICD to go public, information asymmetry is likely to be greater on average for post-JOBS IPO firms. In an attempt to reduce underpricing, IPO firms have an incentive to use voluntary disclosure to mitigate information asymmetry. For the standard disclosure game predictions to hold, the firm's voluntary disclosures must be credible. While the JOBS Act reduces the compliance requirement of ICD disclosure, it is likely to add credibility to voluntary ICD disclosure such that IPO firms can use ICD disclosure more effectively to reduce information asymmetry. Consistent with this conjecture, I show that the negative relation between ICD disclosure and underpricing is stronger after the JOBS Act. Moreover, I find that investors underprice non-disclosing firms more aggressively after the JOBS Act. This finding is consistent with the economic mechanism that more IPO firms are willing to reveal ICD because otherwise they would receive a lower price after the JOBS Act for remaining silent.

I conduct additional tests to rule out several alternative explanations to the increase in ICD disclosure. First, IPO firms may provide supplemental disclosure of the remediation measures implemented to address ICD, such as disclosure of CFO turnover, the hiring of personnel with public reporting experience, and improvement of policies and procedures. The accompanying remediation disclosure may confound the interpretation of ICD disclosure. I find that variations in these remediation disclosures do not fully explain away the stronger relation between ICD disclosure and IPO underpricing after the JOBS Act. Second, it is possible that the JOBS Act

encourages more firms with poor accounting quality to go public, and therefore increased ICD disclosure merely reflects the deteriorating accounting quality of IPO firms after the JOBS Act. I show that firms disclosing ICD after the JOBS Act are not associated with greater discretionary accruals. Finally, the SEC may invest more efforts in monitoring IPO disclosure after the JOBS Act. Using the SEC comments on internal control issues to measure the SEC regulatory oversight, I find no evidence of more intensive monitoring in the SEC review process of the post-JOBS firms with ICD disclosure.

This paper makes two contributions. First, it documents a novel mechanism leading to unintended consequences of the regulation change. As the JOBS Act reduced the disclosure requirement for IPO firms, we might expect fewer disclosures; yet I find that IPO firms are more willing to voluntarily disclose ICD after the JOBS Act. This unintended consequence is not an immediate outcome of the delayed auditor oversight granted by the JOBS Act. Rather the increased voluntary disclosure arises because the JOBS Act changes the pool of IPO firms, which in turn leads both investors and IPO firms to rationally update the probability of ICD among IPO firms. This is consistent with other studies that have found that major changes in securities disclosure regulations often lead to a different selection of public firms. For example, after SOX, small firms with highly concentrated ownership were more likely to go private (Engel et al. 2007), and small foreign firms with low trading volume were significantly more likely to delist from the US stock market (Marosi and Massoud 2008). As a regulatory rollback from SOX, the JOBS Act selects a different mix of firms by reducing compliance requirements for small firms. Indeed post-JOBS IPO firms tend to have lower revenue and are more R&D intensive (Barth et al. 2017; Chaplinsky et al. 2017; Dambra et al. 2015). As the population of firms changes, investors update. In response, firms revise disclosure choices. Overall, this paper presents a dynamic view on how firms make

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voluntary disclosure decisions by taking into account the selection of firms after a regulatory change.

Second, this paper contributes to the emerging literature that evaluates the impact of the JOBS Act. As intended, IPO volume indeed increased after the JOBS Act (Dambra et al. 2015). In response to reduced mandatory disclosure after the JOBS Act, IPO firms reduce executives' pay (Gipper 2016) and disclose a different mix of verifiable and non-verifiable information in registration statements (Gupta and Israelsen 2016). Moreover, a variety of external stakeholders also react to the JOBS Act: investors underprice IPOs more heavily (Barth et al. 2017; Chaplinsky et al. 2017); auditors charge higher fees to compensate for audit risk (Westfall and Omer 2015); affiliated analysts exhibit greater optimistic bias in earnings forecasts (Dambra et al. 2018); and the SEC uses a more negative and uncertain tone in comment letters (Gupta and Israelsen 2016). Perhaps closest to this paper, Barth et al. (2017) find that post-JOBS IPO firms provide more earnings forecasts, initiate more press releases, and file more 8-K filings *after* IPO, consistent with the conjecture that post-JOBS IPO firms provide more voluntary disclosure to mitigate information asymmetry. This paper adds to this strand of literature by studying a new aspect of firm behavior in the IPO process, the voluntary disclosure of ICD. ICD disclosure during IPO is important because ICD disclosure conveys information about credibility of financial reports during IPO when information asymmetry is arguably the greatest during a firm's public life. It is also closely linked to one of the most controversial provisions in the JOBS Act: the delay of auditor oversight on internal controls.

In the next section, I provide the institutional details on ICD disclosure. In Section 2.3, I present a simple model and develop the empirical predictions. Section 2.4 describes the sample and data, and Section 2.5 presents the main results on the likelihood of disclosure and IPO

underpricing. Section 2.6 presents additional tests on ICD remediation, discretional accruals, and the SEC comment letters. I conclude in Section 2.7.

## 2.2   Institutional Background

As yet, no securities regulation explicitly mandates ICD disclosure in the IPO process. Section 11 of the Securities Act of 1933 gives IPO investors the rights to sue IPO firms and underwriters for stock market losses due to misstatements or material omissions of information in the registration statements. Section 10 (b) of the Securities Act of 1934 allows IPO investors as well as aftermarket purchasers to sue IPO firms if the misstatement or the material omission of information is intentional. Nevertheless, IPO firms are not required to certify, evaluate or disclose the effectiveness of internal controls over financial reporting in registration statements.

After IPO, newly public firms phase in compliance with disclosure requirements mandated by SOX. Figure 2.1 provides the compliance timeline of ICD disclosure. Under Section 302 (effective in August 2002), the earliest requirement for disclosure of internal controls becomes effective at the first quarterly or annual financial report in which executives have to certify that they have presented their conclusions about the effectiveness of disclosure controls and procedures. Effective in November 2004, Section 404 sets out the requirements of management assessment and auditor attestation of internal controls. Section 404a requires that management provide a report containing its assessment of the effectiveness of internal controls. To produce such a report, management can follow a control framework at its discretion and "use its best judgement" to adopt the necessary methods and procedures. Section 404b requires that auditors provide an independent opinion on the effectiveness of internal controls. Since the audit of processes (internal controls) is different from the audit of outputs (financial statements), internal

control audits require a separate evaluation of the design, implementation and operating effectiveness of the control environment in addition to financial audits.

Further, the compliance of internal control disclosure is conditional on firm size. Large accelerated filers (firms with a public float of at least $700 million) and accelerated filers (firms with a public float of between $75 million and $700 million) are required to provide management assessment and auditor attestation of internal control effectiveness beginning with the second annual report after the IPO. For firms with a public float of less than $75 million, known as non-accelerated filers, the implementation of management assessment was deferred to 2007 and the auditor attestation requirement was permanently exempted by the Dodd-Frank Act of 2010. That is, under SOX, only large accelerated filers and accelerated filers are required to provide auditor attestation of internal controls, and they have a two-year on-ramp period before such an audit is required.

Title I of the JOBS Act was intended to make it easier to go public by relieving the disclosure requirements for a new category of issuers, Emerging Growth Companies (EGCs). An EGC is an issuer with a total gross revenue of less than $1 billion during its most recent fiscal year. An issuer is disqualified as an EGC if it is a large accelerated filer. For eligible EGCs, the JOBS Act allows a delay in auditor attestation of internal control until the fifth fiscal year after IPO.[4] In sum, the JOBS Act expands the exemption of auditor attestation that originally applied only to

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[4] The JOBS Act also allows EGCs to file registration statements with the SEC for confidential review before they decide to go public. EGCs can also engage in private communication with institutional investors and accredited investors prior to the public filings of registration statements. Before the JOBS Act, IPO firms were required to present three years of audited financial statements and five years of selected financial data in the registration statements. After the JOBS Act, EGCs are allowed to present only two years of audited financial statements and two years of selected financial data. For eligible EGCs in transition to public firms, the JOBS Act provides the following regulatory relief on financial reporting, auditing and corporate governance: reduced compensation disclosure, delayed auditor attestation of internal controls, opt-out of future auditing standards changes, delayed transition to public accounting standards, and opt-out of non-binding shareholder votes on say-on-pay.

non-accelerated filers to accelerated filers for up to five years after IPO. This paper examines ICD disclosure during the IPO process, which is voluntary for all issuers both before and after the JOBS Act. Yet the JOBS Act postpones auditor oversight of internal controls after IPO for accelerated filers.

## 2.3    Conceptual Framework and Empirical Predictions

This section presents the conceptual framework in a simple disclosure model that generates empirical predictions in the following sections. The disclosure of ICD is widely perceived as important bad news. Prior literature documents that the stock market reacts negatively to the disclosure of internal control weaknesses (Beneish et al. 2008; Hammersley et al. 2008). Firms that disclose ICD also experience subsequent increases in the cost of debt and the cost of equity (Kim et al. 2011; Ashbaugh-Skaife et al. 2009). Brown et al. (2015, 2016) provide survey evidence that both sell-side and buy-side analysts consider ICD as a red-flag of financial reporting irregularities. In addition, ICD disclosure sends a negative signal about the management. For example, internal control weaknesses are associated with less professionally qualified CFOs, less accurate management guidance, and more insider trading profits (Li et al. 2010; Feng et al. 2009; Skaife et al. 2013).

Given the existence of ICD, a number of governance and monitoring mechanisms can affect the detection and disclosure of ICD, such as the institutional ownership and accounting expertise of the audit committee (Ashbaugh-Skaife et al. 2007; Hoitash et al. 2009). Most importantly, auditor involvement plays an essential role in the detection of ICD. Bedard and Graham (2011) find that auditors detect three-quarters of unremediated ICD by control testing. Bedard et al. (2009) show that material weaknesses in internal controls are more likely to be

disclosed in the fourth quarter, a period when auditors are more involved than in the first three quarters. More recently, Ge et al. (2017) estimate that auditor attestation curbs the misreporting of ICD by 38.1 percent. Given the importance of auditor oversight in detecting and disclosing ICD, the delay in internal control audits granted by the JOBS Act arguably facilitates firms withholding the bad news of ICD from investors.

Nevertheless, in assessing the likelihood of ICD disclosure, it is important to bear in mind that by design, the JOBS Act introduces a new group of smaller issuers that may not have gone public without the regulatory rollback. While big firms tend to have more complicated accounting issues, small firms often have inadequate resources, infrastructure, and personnel for public financial reporting. Doyle et al. (2007) and Ashbaugh-Skaife et al. (2007) show that on average, firm size is negatively correlated with ICD disclosure. For the years 2007–2014, Ge et al. (2017) report that 10.9 percent of firms with market value of equity less than $75 million, 7.6 percent of firms with market value of equity between $75 million and $300 million, and 2.8 percent of firms with market value of equity greater than $300 million disclose ineffective internal controls. As the JOBS Act attracts more small firms to go public in the post-JOBS period, the probability of ICD in the pool of IPO firms increases. As investors update the expectations of the probability of ICD, the expected firm value of non-disclosing firms decreases. That is, the benefit of withholding ICD to be pooled with other non-disclosing firms diminishes after the JOBS Act. Consequently, IPO firms may be more likely to disclose ICD.

**Model Setup**

The latter mechanism can be presented in a simple model derived as a special case of Jung and Kwon (1988). Consider a single-period model in which the manager and risk-neutral investors have the common prior belief of the firm value $\tilde{x}$. This prior belief follows the uniform distribution

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$f(x)$ with its support $[\underline{x}, \overline{x}]$. I assume that firm value is monotonically increasing in internal control strength. Thus, a weaker internal control suggests a lower firm value. Figure 2.2 depicts the timeline of the disclosure game between the manager and investors. At time $t = 1$, the manager privately observes, with probability $(1 - p)$, a realization of firm value $x$ in association with internal control strength. Both $f(x)$ and $(1 - p)$ are common knowledge. At time $t = 2$, the manager decides whether to disclose private information if she has it. Nevertheless, the manager cannot credibly claim that she received no private information. At time $t = 3$, investors price the firm based on the disclosure or non-disclosure decision made by the manager.

Investors perceive that one of three mutually exclusive events, denoted by A, B, and C, will be realized. *Event A* occurs when the manager receives no private information. Since it is a null-information event, the expected firm value conditional on *Event A* is identical to unconditional mean $E[\tilde{x}|A] = \frac{(\underline{x} + \overline{x})}{2}$. If the manager receives private information of the realized firm value, she will withhold the information if it is below a threshold value of $y \in [\underline{x}, \overline{x}]$, and will disclose it if the private information exceeds the threshold $y$. *Event B* occurs when the manager receives the private information but does not disclose it due to its unfavorable content. The conditional expectation of the firm value given *Event B* is $E[\tilde{x}|B] = \int_{\underline{x}}^{y} \frac{\tilde{x}}{F(y)} dF(\tilde{x})$. *Event C* occurs when the manager receives and discloses private information. The probabilities of these three events are

**A**: no information, no disclosure $\quad Prob(A) = p$
**B**: receive and withhold information $\quad Prob(B) = (1 - p) \cdot F(y)$
**C**: receive and disclose information $\quad Prob(C) = (1 - p) \cdot [1 - F(y)]$

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In the presence of disclosure, investors infer that *Event C* occurs and price the firm as disclosed. In the absence of disclosure, investors cannot distinguish between *Event A* and *Event B*. In this case, they rule out the occurrence of *Event C* and update the probabilities as follows:

**A**: no information, no disclosure $\quad Prob(A) = \dfrac{p}{[p + (1-p) \cdot F(y)]}$

**B**: receive and withhold information $\quad Prob(B) = \dfrac{(1-p) \cdot F(y)}{[p + (1-p) \cdot F(y)]}$

**C**: receive and disclose information $\quad Prob(C) = 0$

$$where \; F(y) = Prob(\tilde{x} \leq y) = \frac{y - \underline{x}}{\overline{x} - \underline{x}}.$$

With the updated probabilities, investors price the non-disclosing firms as the weighted average of expected firm value conditional on *Event A* and expected firm value conditional on *Event B*.

$$E(\tilde{x}|ND) = \frac{p \cdot E[\tilde{x}|A]}{p + (1-p) \cdot F(y)} + \frac{(1-p) \cdot F(y)}{p + (1-p) \cdot F(y)} E[\tilde{x}|B] \qquad (2.1)$$

The manager would withhold the private information as long as the stock price in the absence of disclosure is greater than the price in the presence of disclosure. Therefore, the set of $x$ realizations that the manager would withhold satisfies $D = \{x | E(\tilde{x}|ND) \geq x\}$. Further, the rational expectation equilibrium indicates that the investors' conjecture of the disclosure threshold is consistent with the manager's optimal disclosure strategy.

$$E(\tilde{x}|ND) = y$$

$$(2.2)$$

It can be shown that there exists a unique equilibrium disclosure threshold y such that $\underline{x} < y < \frac{(\underline{x} + \overline{x})}{2}$. Equating (2.1) and (2.2) and solving for $y$ when $\tilde{x}$ follows the uniform distribution over the internal $[\underline{x}, \overline{x}]$ give

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$$y = \frac{\sqrt{p}\bar{x} + \underline{x}}{1 + \sqrt{p}}$$

(2.3)

It is straightforward to show $\frac{\partial y}{\partial \underline{x}} = \frac{1}{1+\sqrt{p}} > 0$; the disclosure threshold decreases as the lower bound of the firm value decreases. Assuming that the JOBS Act lowers the lower bound of the distribution of firm value by introducing small firms with a higher probability of ICD to the stock market, the disclosure threshold should decrease. Figure 2.3 illustrates the change in the disclosure threshold after the JOBS Act. In this case, as the lower bound shifts to the left, if the manager remains silent on internal controls, investors would assign a lower value to the non-disclosing firm. In this new equilibrium after the JOBS Act, firms on the margin of the pre-JOBS disclosure threshold find it in their interests to separate themselves from the non-disclosing firms.

In summary, while the JOBS Act makes it easier to withhold ICD by delaying auditor oversight, it also diminishes the benefits of withholding ICD by increasing the probability of ICD in the population of IPO firms. The first hypothesis is therefore stated in the null form as below:


*H1: All else equal, the JOBS Act has no impact on IPO firms' likelihood to disclose internal control deficiencies.*


Next, I turn to the relation between voluntary ICD disclosure and underpricing after the JOBS Act. Underpricing is a significant cost of raising capital that partly arises from the information asymmetry between IPO firms and investors. Voluntary disclosure can be used to communicate IPO firms' private information and ameliorate information asymmetry. Prior literature provides empirical evidence on the negative relation between voluntary disclosure and

IPO underpricing. For instance, Clarkson et al. (1992) find that voluntary disclosure of earnings forecasts is negatively related to underpricing. Schrand and Verrecchia (2002) find that more frequent pre-IPO disclosure is associated with lower underpricing. Leone et al. (2007) find that more specific disclosure of use-of-proceeds is also associated with lower underpricing.[5]

Because the JOBS Act reduces disclosure requirements and because it allows more firms with potential ICD to go public by reducing disclosure requirements, information asymmetry between IPO investors and firms is likely to increase. Empirically, Barth et al. (2017) and Chaplinsky et al. (2017) find that IPO firms experience more severe underpricing after the JOBS Act, consistent with the conjecture of greater information uncertainty among post-JOBS IPO firms. Given greater information uncertainty, IPO firms should have a greater incentive to use voluntary ICD disclosure to reduce underpricing. The JOBS Act significantly reduces the regulatory burden for IPO firms. If the JOBS Act is perceived to make it more difficult to detect internal control weaknesses, ICD disclosure becomes a stronger signal that there are no other material omissions of information. To the extent that the JOBS Act adds credibility to the ICD disclosure, IPO firms can rely more on ICD disclosure to reduce underpricing following the JOBS Act. The second hypothesis follows.

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[5] Prior literature suggests some IPO disclosures are made to reduce litigation risk, and therefore ICD disclosure may be also driven by litigation concerns. Specifically in the IPO context, Hanley and Hoberg (2012) find that strategic disclosure and IPO underpricing are substitutes for each other to hedge against litigation risk. Nevertheless, in the ICD disclosure literature, Rice et al. (2015) find that in a sample of restating firms, ICD disclosure leads to a greater likelihood of shareholder class action lawsuits because ICD disclosure makes it harder for management to claim unawareness of conditions leading to misstatements. Therefore, it is ex ante unclear how ICD disclosure affects litigation risk in the IPO context.

*H2: The negative relation between ICD disclosure and IPO underpricing is stronger after the JOBS Act.*

## 2.4 Sample and Summary Statistics

### 2.4.1 Sample and Data

The sample starts with all U.S. IPOs in the Thomson Reuters Securities Data Company (SDC) Platinum New Issues database between November 15, 2004 and March 31, 2015. The beginning of the sample period is dictated by the effective date that SOX 404 mandates on the disclosure of internal controls. I use SDC filters to exclude units offerings, rights offerings, best efforts offerings, blank check companies, closed-end funds, real estate investment trusts, limited partnerships, foreign offerings, and non-original IPOs. I retrieve all IPO characteristics from SDC and then check for any errors in the SDC data documented on Jay Ritter's website.[6] I require IPO firms to be covered by Compustat and exclude financial firms (SIC codes 6000–6999).[7] These data requirements yield an initial sample of 1,021 IPO firms.[8]

I collect ICD disclosure in the IPO prospectus (i.e., Form 424B) from the SEC Electronic Data Gathering, Analysis, and Retrieval (EDGAR) database. ICD are disclosed in the sections of *Risk Factors*, *Management Discussion & Analysis*, or occasionally in *Experts* in case of a change of the auditor. The disclosure often reveals the scope and the causes of ICD, and past or planned remediation efforts. After checking the prospectuses, I drop another 37 observations that are not

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[6] See http://bear.warrington.ufl.edu/ritter/SDC_corrections.pdf.

[7] The disclosure regulation of internal controls for banks has been historically different from industrial firms. Before the passage of SOX, the Federal Deposit Insurance Corporation Act of 1991 required banks to report on the effectiveness of internal controls, and required auditors to attest the report.

[8] I also search the ICD disclosure for all non-financial firms that are jointly covered by SDC, Compustat, and CRSP, and file for IPO between January 1st, 2003 and November 15, 2004. In anticipation of SOX implementation, one firm voluntarily discloses two significant deficiencies in 2003 and four firms disclose material weaknesses or significant deficiencies in internal controls from January to October 2004.

captured by my filters applied to SDC retrieval (in order to exclude units offering, foreign private issuers, black check companies, and limited partnerships).

To be included in the pre-JOBS sample, the firm must have a filing date and issue date before April 5, 2012. I also require the firm to have less than $1 billion annual revenue in the most recent fiscal year before the IPO such that it would have qualified as an Emerging Growth Company (EGC). To be included in the post-JOBS sample, the firm must identify itself as an EGC in the prospectus and publicly file for an IPO after April 5, 2012. I use the terms EGCs and post-JOBS issuers interchangeably hereafter. These requirements reduce the sample to 810 firms, including 543 pre-JOBS and 267 post-JOBS issuers. The additional data requirement of non-missing control variables reduces the sample to 469 pre-JOBS and 243 post-JOBS issuers. In addition to ICD disclosure, my supplemental tests require data on a variety of outcomes including IPO underpricing, discretionary accruals, the SEC Comment letters, and the control variables unique to these outcomes. I retrieve data on the SEC comment letters, restatements, and auditor changes from Audit Analytics, stock returns from CRSP, and accounting data from Compustat.

As an alternative approach to mitigating endogeneity concerns, I use propensity score matching to control for the observed difference in firm characteristics between pre- and post-JOBS issuers. In the first stage prediction, I run a logistic regression to estimate the propensity score of going public in the post-JOBS period. Then, I match without replacement each post-JOBS issuer with one pre-JOBS issuer on the closest propensity score. Following the suggestion by Shipman et al. (2016), I use the same set of control variables in the first stage prediction as those in the linear regressions using the full sample. The propensity score matching yields a matched sample of 243 pre-JOBS issuers and 243 post-JOBS issuers.

### 2.4.2    Descriptive Statistics

In increasing order of severity, the current auditing standards (Auditing Standard No. 5) classify ICD into three categories: deficiencies, significant deficiencies, and material weaknesses.[9] I collect all three levels of ICD. Considering that IPO filings potentially provide the first report on the history and current status of internal controls, the status of internal controls at the time of IPO can be characterized by two states: fully remediated ICD and unremediated ICD. In terms of disclosure of fully remediated ICD, IPO firms can reveal that internal control weaknesses were discovered when they were private companies but have fully remediated these weaknesses before the IPO. It is in essence disclosure of no ICD. Appendix A.1.1 presents such an example. Alternatively, IPO firms can disclose that the detected ICD remain unresolved by the time of IPO.[10] Appendix A.1.2 presents an example of such ICD disclosure. If a firm identified multiple deficiencies and remediated some of them, I consider the firm as having unremediated ICD as long

---

[9] A *deficiency* exists "when the design or operation of a control does not allow management or employees, in the normal course of performing their assigned functions, to prevent or detect misstatements on a timely basis." A *significant deficiency* is defined as "a deficiency, or a combination of deficiencies, in internal control over financial reporting that is less severe than a material weakness, yet important enough to merit attention by those responsible for oversight of the company's financial reporting." A *material weakness* is "a deficiency, or a combination of deficiencies, in internal control over financial reporting, such that there is a reasonable possibility that a material misstatement of the company's annual or interim financial statements will not be prevented or detected on a timely basis." More details can be found https://pcaobus.org/Standards/Auditing/Pages/Auditing_Standard_5_Appendix_A.aspx

[10] Conceptually, there are four possible scenarios of ICD disclosure decisions: (1) firms disclose that they have never ever detected ICD before IPO; (2) firms disclose that they found ICD before IPO but have fully remediated them by the time of IPO; (3) firms disclose that they found ICD before IPO and ICD remain unremediated by the time of IPO; (4) firms do not disclose any information on the past or current status of internal controls. I consider firms in the first three scenarios as disclosing firms and firms in the last scenario as non-disclosing firms. Nevertheless, I do not find any case of the first scenario in my sample.

as at least one deficiency remains prior to its IPO.[11] While fully remediated ICD may obscure the nature of disclosure, the disclosure of unremediated ICD is unambiguously bad news.

In untabulated analysis for the full sample of 712 firms, regardless of remediation progress, two firms report the discovery of deficiencies before IPO, 31 firms report the discovery of significant deficiencies before IPO, and 195 firms report the discovery of material weaknesses before IPO.[12] Panel A of Table 2.1 compares IPO disclosure of ICD for the full sample before and after the JOBS Act. 30.92 percent pre-JOBS issuers and 34.16 percent post-JOBS issuers disclose the status of internal controls. Panel A also shows that 20.68 percent pre-JOBS issuers versus 27.57 percent post-JOBS issuers disclose at least one unremediated ICD. Furthermore, issuers with IPO proceeds of at least $75 million experience a small decrease in ICD disclosure: 34.12 percent pre-JOBS issuers versus 32.89 percent post-JOBS issuers report the status of ICD. In contrast, firms with IPO proceeds of less than $75 million significantly increase ICD disclosure after the JOBS Act, climbing from 25.43 percent to 36.26 percent. The similar pattern is observed in the disclosure of unremediated ICD.

Recall that under SOX 302, the earliest mandatory disclosure starts at the first quarterly or annual financial report in which signing executives have to certify that they have evaluated and presented their conclusions about the effectiveness of the disclosure controls and procedures. Panel B of Table 2.1 tabulates the first ICD disclosure after IPO as mandated by SOX 302. Two

---

[11] I define the internal control deficiency as unremediated as long as it is not fully remediated, despite the fact that some actions may have been taken to partially remediate ICD. This definition of "unremediated" is consistent with the Annual Financial Reporting Model Regulation (the Model Audit Rule). Example G in the implementation guide of the Model Audit Rule describes the actions the example company has taken and concludes that the company has an unremediated material weakness. (see http://www.naic.org/documents/committees_e_naic_aicpa_wg_model_audit_rule_imp_guide.pdf)

[12] Only the most severe deficiency is counted for each firm. For example, if a firm discloses the detection of a material weakness and a significant deficiency in internal controls, I consider that the firm discloses the discovery of a material weakness.

observations can be made about the subsequent ICD disclosure: (1) it is rare that firms are silent on internal control weaknesses during IPO but then announce the discovery of ICD soon after IPO; only six pre-JOBS and five post-JOBS issuers do so; (2) out of 164 firms disclosing unremediated ICD during IPO, 69 firms would have no ICD per SOX 302 disclosure, which is filed within a year after IPO. Had these firms chosen to withhold control weaknesses during IPO, they would have fully remediated these weaknesses when the mandatory disclosure requirement becomes effective. To put the remediation progress in perspective, Johnstone et al. (2011) document that for a sample of 733 firm-year observations disclosing material weaknesses in internal controls, approximately 60 percent of firms remediate the material weaknesses within a year, and 30.3 percent of firms still have the same material weaknesses three years after the initial disclosure. Consistent with the disclosure theory, a significant number of IPO firms that voluntarily disclose ICD are not the worst in terms of severity of ICD.

Panels A and B of Table 2.2 present the summary statistics of variables in the ICD disclosure tests using the full and matched samples respectively. All variables are defined in Appendix B. In Panel A, pre- and post-JOBS issuers differ on a number of firm characteristics. Post-JOBS issuers are younger, smaller, and more likely to be backed by venture capital. Consistent with them being smaller firms, post-JOBS issuers have fewer segments, make fewer mergers and acquisitions, and do less restructuring. Notably, prior to IPO, 14 percent of pre-JOBS issuers restate a previously issued financial report from the three most recent fiscal years while only 7 percent of post-JOBS issuers restate such a financial report. The significant differences between pre- and post-JOBS issuers demonstrate that the IPO pool has changed after the JOBS Act. In Panel B, there is no significant difference in firm characteristics between pre- and post-JOBS issuers in the matched sample except that post-JOBS issuers have more cash. This lends

support that these firm characteristics are more balanced between pre- and post-JOBS issuers after propensity score matching.

Panels C and D of Table 2.2 present the summary statistics of variables in the underpricing tests using the full and matched samples respectively. In both samples, post-JOBS issuers disclose significantly more risk factors in registration statements and are more R&D intensive. Recall that one key assumption in the conceptual framework is that the lower bound of firm value of post-JOBS issuers shifts to the left. Looking across the summary statistics in Panels A and C, there are more losses among JOBS issuers, and they experience slower sales growth and lower returns on assets; these statistics are consistent with the leftward shift of the lower bound of firm value.

## 2.5    Main Results

As with the hypothesis development, I proceed in two parts. First I document the average effect of the JOBS Act on the likelihood of ICD disclosure, and then I explore the relation between ICD disclosure and IPO underpricing after the JOBS Act.

### 2.5.1    ICD Disclosure

The implementation of the JOBS Act presents three empirical challenges. First, the JOBS Act defines EGCs as those with annual revenue of less than the $1 billion in the most recent fiscal year. This revenue threshold is sufficiently high that very few firms going public after the JOBS Act fail to qualify the EGC status. Due to a lack of contemporaneous control firms, I compare EGCs with pre-JOBS firms that have revenue less than the $1 billion threshold. Thus these pre-JOBS issuers would have satisfied the requirement of EGCs had they gone public after the enactment of the JOBS Act. Second, as the JOBS Act substantially lowers the regulatory hurdles for IPO firms, there is an endogeneity concern that the IPO firms choosing to go public after the

JOBS Act are systematically different from those before the JOBS Act. Dambra et al. (2015) find

that the post-JOBS issuers have smaller revenue and are more R&D intensive than pre-JOBS

issuers. The descriptive statistics presented in Panel A of Table 2.2 also illustrate the significant

differences between pre- and post-JOBS issuers. A third inherent difficulty in estimating the

likelihood of ICD disclosure is that the existence of ICD is not observable. Conditional on the

existence of ICD, only disclosed ICD are observed. This exacerbates the concern that the observed

change in ICD disclosure is due to systematic differences in firm characteristics that contribute to

the existence of ICD. To control for these differences between pre- and post-JOBS issuers, I

include a set of determinants of existence and disclosure of ICD from the prior literature

(Ashbaugh-Skaife et al. 2009; Doyle et al. 2007; Ge et al. 2017). I model the probability of ICD

disclosure using the logistic regression as follows:

$$
\begin{aligned}
ICD_i = f(\beta_0 &+ \beta_1 Post_i + \beta_2 Proceeds_i + \beta_3 Cash_i + \beta_4 Loss_i + \beta_5 Age_i \\
&+ \beta_6 Segments_i + \beta_7 Foreign_i + \beta_8 M\&A_i + \beta_9 Restructure_i \\
&+ \beta_{10} ChgSales_i + \beta_{11} ChgInvt_i \\
&+ \beta_{12} Restate_i + \beta_{13} AuditorChg_i + \beta_{14} Big4_i + \beta_{15} Reputation_i \\
&+ \beta_{16} VC\_Backed_i)
\end{aligned}
\tag{2.4}
$$

where *ICD* is an indicator variable equal to one if the firm discloses the status of internal

controls and zero otherwise. *Post* is an indicator variable equal to one if the firm went public after

the JOBS Act and identified itself as an EGC, and zero if it went public before the JOBS Act and

had revenue less than the $1 billion threshold. Firm size proxied by IPO proceeds (*Proceeds*), age

*(Age)*, cash holdings (*Cash*), and accounting losses (*Loss*) are associated with the firm's ability to

establish proper internal controls. Foreign operations (*Foreign*), multiple segments (*Segments*),

restructuring charges (*Restructure*), and merger and acquisition activities (*M&A*) expose firms to

more complex transactions and accounting issues, which contribute to the likelihood of ICD.

Changes in sales (*ChgSales*) and changes in inventory (*ChgInvt*) capture firm growth, as a rapidly expanding firm may outgrow the internal controls already in place.[13] The restatement announcement (*Restate*) and auditor turnover (*AuditorChg*) prompt the reporting of ICD, while the big 4 auditors (*Big4*), underwriter reputation (*Reputation*), and venture capital backing (*VC_Backed*) are proxies for the external governance and monitoring environment in the IPO setting. All the accounting variables are measured at the most recent fiscal year end before IPO.

Recall that the JOBS Act does not affect all IPOs equally. Before the JOBS Act, firms with a public float of less than $75 million were granted exemption of auditor attestation. One may argue that firms expecting to receive exemption are unaffected by the JOBS Act in terms of auditor oversight. Meanwhile, the JOBS Act encourages more small firms with potential ICD to go public; and these would-be exempt firms are also more likely to experience revised expectations of potential ICD by investors. To assess whether the JOBS Act has a differential impact on the would-be exempt firms, I use IPO proceeds as a proxy for the future public float to distinguish firms that are likely to qualify for the permanent exemption.

Table 2.3 shows the results for the effects of the JOBS Act on the probability of ICD disclosure. To facilitate interpretation of the economic magnitude, I present estimates of the average marginal effects in the logistic regressions. The estimates capture the change in the predicted probability for a given unit change in the independent variables. The standard errors are clustered by industry-quarter to control for potential correlations in the error terms. The industry is defined by two-digit SIC codes.

---

[13] Changes in sales and inventory are scaled by total assets so that firms with zero sales or inventory at the fiscal year t-2 before IPO do not drop from the sample.

Panels A and B show the results of the full and matched samples respectively. In both

panels, columns 1–3 use all non-disclosing and disclosing firms. As stated in Section 2.3, if a firm

announces that it discovered ICD and has fully remediated them, whether this is good news or bad

news can be ambiguous. To ensure that ICD disclosure is unambiguously bad news, columns 4–6

exclude disclosing firms that have fully remediated ICD.

In Panel A, column 1 shows that the marginal effect on the *Post* indicator is 0.091 and

significant, indicating that after the JOBS Act, IPO firms are 9.1 percent more likely to disclose

the status of ICD, remediated or unremediated. After eliminating disclosing firms that have

remediated ICD in column 4, post-JOBS issuers are 11.6 percent more likely to disclose

unremediated ICD during IPO. When I partition the sample to exempt firms and non-exempt firms

as in columns 2 and 3, and columns 5 and 6, exempt firms are significantly more likely to disclose

ICD while there is no significant change in ICD disclosure after the JOBS Act for non-exempt

firms. This result is consistent with the conjecture that increased ICD disclosure is attributed to a

revised expectation on the probability of ICD, since small exempt firms are more likely to have

ICD, and are therefore more likely to be affected by the revised expectation. In comparison,

consistent with the hypothesis H1, the JOBS Act appears to have no effect on the probability of

disclosure among non-exempt firms. [14]

The estimated effects of the JOBS Act in Panel B are similar to those in Panel A only that

the marginal effects are slightly smaller in the matched sample. On average, post-JOBS issuers are

8.3 percent more likely to disclose the status of ICD in the prospectuses, and are 9.2 percent more

---

[14] Arguably, IPO firms expected to have a public float of at least $700 million are not affected by the delayed internal control audit as well because they would be disqualified as an EGC soon after going public. After dropping 13 observations of IPO firms with proceeds greater than $700 million, the results are qualitatively and quantitatively similar.

likely to disclose ICD that are still unremediated by the time of IPO. Among exempt firms, post-JOBS issuers are 13.6 percent more likely to disclose ICD and are 14.6 percent more likely to disclose the unremediated ICD. Among non-exempt firms, post-JOBS issuers also increase ICD disclosure, although the increase is statistically insignificant.

In both samples, the effects for most control variables are similar to prior studies (Ashbaugh-Skaife et al. 2009; Doyle et al. 2007; Ge et al. 2017). On average, firms with lower cash holding and more complex transactions are more likely to disclose ICD. A prior restatement before IPO and the change in auditors significantly increase the likelihood of ICD disclosure. The only exception is the presence of a big 4 auditor. While Ashbaugh-Skaife et al. (2007) find that auditor size is positively associated with the voluntary disclosure of ICD, I find the opposite. It is possible that IPO firms choosing small auditors are more likely to have weak internal controls such that the auditor choice captures the likelihood of existence of ICD rather than the reporting incentives of ICD.

The regression analysis in Table 2.3 shows that the likelihood of ICD disclosure increases after the JOBS Act, consistent with the conjecture that the disclosure threshold is lower in the post-JOBS period. To better assess the impact of the JOBS Act on the firms around the disclosure threshold, I first run the same regression (2.4) using all the pre-JOBS firms and retain the coefficient estimates. Then I use these coefficients to calculate the predicted probability of ICD disclosure for post-JOBS firms. This predicted probability yields the counterfactual probability of ICD disclosure had post-JOBS firms gone public before the JOBS Act. The difference between actual and predicted probability of ICD disclosure captures the impact of the JOBS Act. The mean of actual and predicted probabilities of ICD disclosure using the pre-JOBS coefficients are 33.89% and 26.69% respectively, suggesting an increase of 7.16% in the probability of ICD disclosure

over the firm value interval. Although it is difficult to pinpoint the disclosure threshold, it is reasonable to expect the disclosure threshold lies in the midrange rather than the upper or lower end of the internal of firm value. To zero-in on the firms around the disclosure threshold, I calculate the difference between actual and predicted probability of ICD disclosure after dropping the firms on both extremes of predicted probability. After dropping the firms on top and bottom 5% (10%) of predicted probability of ICD, I find that there is a greater increase of 8.42 % (9.35%) in the probability of ICD disclosure after the JOBS Act. This evidence is consistent with conjecture that firms on the margin of the disclosure threshold contributes to the increase in the probability of ICD disclosure.

### 2.5.2 ICD Disclosure and IPO Underpricing

To examine the consequence of ICD disclosure after the JOBS Act, I estimate the impact of the JOBS Act on the relation between ICD disclosure and IPO underpricing as follows.

$$
\begin{aligned}
Underpricing_i \\
&= \beta_0 + \beta_1 Post_i + \beta_2 ICD_i + \beta_3 Post_i * ICD_i \\
&\quad + \beta_4 Risk\ Factors_i + \beta_5 Overhang_i + \beta_6 PrcRev_i^+ \\
&\quad + \beta_7 PrcRev_i^- + \beta_8 Market\ Return_i + \beta_9 Age_i + \beta_{10} R\&D_i \\
&\quad + \beta_{11} ROA_i + \beta_{12} Size_i + \beta_{13} Reputation_i \\
&\quad + \beta_{14} VC\_Backed_i + \sum \delta_j Exchange_j + \sum \gamma_h Industry_h + \varepsilon_i
\end{aligned}
\tag{2.5}
$$

where *Underpricing* is the stock return on the first trading day, calculated as the closing price on the first trading day divided by the offer price minus one. The greater the return on the first trading day in the stock market, the more severely is the firm underpriced in the IPO process, since more money is "left on the table." *Post* again is a binary variable to indicate the status of post-JOBS issuers and *ICD* is a binary variable to indicate ICD disclosure. If ICD disclosure is merely

perceived as poor internal controls that lead to greater information uncertainty, $\beta_2$ is expected to be positive as IPO investors discount the firm value. If voluntary ICD disclosure is a credible signal that differentiates disclosing firms from firms with lower values, $\beta_2$ is expected to be negative as IPO investors offer a more favorable price after observing ICD disclosure. *Post\*ICD* is the interaction term of *Post* and *ICD*. It captures the incremental impact of the JOBS Act on the strength between ICD disclosure and IPO underpricing. The second hypothesis predicts $\beta_3$ to be negative.

I also include several control variables from the IPO underpricing literature. All the variables are defined in Appendix A.2. The number of risk factors (*Risk Factors*) in the prospectus measures the ex ante firm risk (Clarkson and Simunic 1994). Overhang (*Overhang*), or retained ownership, captures issuers' incentive to limit underpricing (Bradley and Jordan 2002). Price revisions in the IPO process (*PrcRev+, PrcRev-*) reflect IPO firms' new information learned after the filing of the initial prospectus (Hanley 1993; Hanley and Hoberg 2010). Prior market return (*Market Return*) captures the market-wide conditions before IPO. Firm age (*Age*), R&D investment (*R&D*), return on assets (*ROA*), and size (*Size*) are common firm characteristics that may affect underpricing. Finally, whether the firm has prestigious underwriters (*Reputation*) and is backed by venture capital (*VC_Backed)* reflect external institutional incentives in the IPO process. The stock exchange fixed effects and the industry fixed effects are also included where the industry is identified by two-digit SIC codes. Standard errors are clustered at the industry-quarter level for all regressions.

Panels A and B of Table 2.4 present the results on IPO underpricing using the full sample and the matched sample respectively. In Panel A, columns 1–3 use all disclosing and non-disclosing firms, while columns 4–6 exclude disclosing firms that have fully remediated ICD

before IPO. Column 1 uses all IPOs and we see that the coefficient on *Post* is positively significant. Consistent with Barth et al. (2017) and Chaplinsky et al. (2017), IPO underpricing is more severe for post-JOBS issuers, suggesting that information asymmetry is greater in post-JOBS issuers due to reduced mandatory disclosure. Yet ICD disclosure by itself is insignificant. Finally, the coefficient estimate on the interaction term is negative as predicted, albeit marginally significant. Turning to the sample that includes only unremediated ICD and non-disclosing firms in column 4, I find that the JOBS Act strengthens the negative relation between ICD disclosure and IPO underpricing at the 1 percent significance level. The coefficient estimate on the interaction term is -9.5%. In the same regression, post-JOBS issuers without ICD disclosure suffer from IPO underpricing of 11.1%, suggesting that ICD disclosure reverses approximately 77 percent of IPO underpricing (1-(0.111+0.009-0.095)/0.111=77%). The benefit of reduced underpricing is sizable in terms of economic magnitude.

Columns 2 and 3 of Panel A present the results of two subsamples separately: exempt and non-exempt firms. Consistent with Chaplinsky et al. (2017), *Post* is positively significant only in non-exempt firms which were recently granted reduced disclosure requirements by the JOBS Act. Echoing the results that increased ICD disclosure concentrates in exempt firms in Table 2.3, the interaction term is negative in the subsample of exempt firms although marginally significant. Similar results are observed after excluding firms disclosing fully remediated ICD as presented in columns 5 and 6.

In Panel B, the results in the matched sample are generally similar to those in the full sample. The post-JOBS issuers suffer from greater underpricing and more heavily so in non-exempt firms. Using all the IPOs in the matched sample, the interaction term is significantly negative for unremediated ICD disclosure. After partitioning the matching sample into exempt and

non-exempt firms, the interaction terms are negative in all columns as expected, but they are insignificant or marginally significant. The weak statistical significance in the partitioned subsamples may be attributed to the lack of power, given the small sample size. Although not uniformly significant, the results on underpricing lend some support to the conjecture that the negative relation between ICD disclosure and underpricing is more pronounced following the JOBS Act.

**Decomposition of the Interaction Terms**

As discussed in Section 2.3, the driving force for increased voluntary disclosure of ICD in the post-JOBS period is that investors assign a lower value to non-disclosing firms after the JOBS Act so that post-JOBS issuers on the margin of pre-JOBS disclosure threshold are now better off disclosing ICD. To provide direct evidence of this mechanism and shed light on the regressions with interaction terms in Table 2.4, Table 2.5 presents the results of changes in IPO underpricing after the JOBS Act conditional on ICD disclosure behavior. Columns 1-4 and columns 5-8 report the results for the full and matched samples respectively.

To facilitate comparison, I first provide the results without the interactions terms in column 1, which shows again that the average underpricing is more severe in the post-JOBS period. To distinguish whether the negative interaction terms in Table 2.4 reflect the variations in non-disclosing firms or disclosing firms in the post-JOBS period, columns 2-4 partition the sample by non-disclosure, ICD disclosure, and disclosure of unremediated ICD made by IPO firms. Looking across the columns, the results show that investors indeed discount non-disclosing firms more aggressively after the JOBS Act as the coefficient *Post* is significantly positive in column 2. In contrast, there is no significant change in IPO underpricing after the JOBS Act in disclosing firms regardless the nature of ICD disclosure, given the lack of significance in columns 3 and 4. Similar

results are observed in the matched sample shown in columns 5-8. Therefore, the negative interaction terms *Post\*ICD* documented in Table 2.4 are primarily driven by the greater discount faced by non-disclosing firms, but not by the variations in disclosing firms. Overall, this evidence is consistent with the hypothesized mechanism.

## 2.6    Additional Tests

This section conducts additional analysis to rule out several alternative explanations. I first examine whether the remediation of ICD accompanying the ICD disclosure explains the variation in IPO underpricing. Another possible explanation for the increased ICD disclosure is that it merely reflects poorer accounting quality of post-JOBS issuers. Using discretionary accruals as a measure of accounting quality, I next examine whether discretionary accruals are positively associated with post-JOBS issuers with ICD disclosure. After the JOBS Act, the SEC may increase the monitoring activity in the review process to compensate for the regulatory rollback. To rule out this explanation, I investigate the changes in the SEC comment letters on the internal control disclosure issues after the JOBS Act.

### 2.6.1    Remediation

Along with ICD disclosure, IPO firms often discuss their past and planned remediation of ICD. The remediation narrative may change the interpretation of ICD disclosure. It is possible that while existence of ICD is negative news, voluntary disclosure of ICD indicates that firms have improved or are committed to improve internal controls in the future.  For example, Cheng et al. (2013) show that investment efficiency improves after initial disclosure of internal control weaknesses, consistent with the conjecture that disclosure of internal control weakness signals that firms are expected to improve financial reporting quality in the future. To exploit variation in

remediation efforts, I classify the remediation measures into three categories: (1) hiring a new Chief Financial Officer (*NewCFO*); (2) improving the personnel for public reporting, such as providing training to the existing accounting staff, hiring new staff with professional qualifications, ensuring segregation of duties, and outsourcing financial reporting tasks to an independent accounting firm (*Employee*); and (3) improving the design and implementation of policies, procedures, financial reporting processes, and infrastructure (*Process*). Each category captures a different aspect of the remediation efforts. I create binary variables to indicate the implementation of each category. I then count the number of categories (*NumRem*) a firm has implemented or plans to implement remediate ICD before IPO to capture the scope and extent of remediation. Out of 228 firms with ICD disclosure in the full sample, either in action or plan, 49 firms hire a new CFO, 168 firms enhance personnel related to public reporting, and 153 firms improve the policies and procedures of public reporting.

To examine whether remediation bundled with ICD disclosure is more effective in decreasing IPO underpricing after the JOBS Act, I replace each remediation measure and *NumRem* by the indicator variable *ICD* in the underpricing test. Panels A and B present the results of remediation using all IPOs in the full and matched sample respectively. In both panels, Table 2.6 uses the same control variables as in the underpricing tests presented in Table 2.4. Looking across the columns in Panel A, the results show that hiring a new CFO effectively reduces IPO underpricing. This result corroborates the prior empirical evidence that firms replace CEOs or CFOs to restore their reputation after the revelation of material restatements (Wilson 2008) and

internal control material weaknesses (Johnstone et al. 2011). [15] Consistent with prior literature, the findings in Table 2.6 suggest that CFO turnover helps establish a reputation of disclosure quality after ICD disclosure in the IPO context as well. Nevertheless, hiring a new CFO does not incrementally reduces underpricing after the JOBS Act. In contrast, improvement of the financial reporting process, improvement of personnel for public reporting, and the extent of remediation on average do not seem to reduce underpricing. However, improvement of the financial reporting process and the extent of remediation decrease underpricing to greater extent after the JOBS Act as the respective interaction terms are significantly negative, in particular for firms disclosing unremediated ICD.

Turning to the results of the matched sample in Panel B, going public in the post-JOBS period does not incrementally strengthen the impact of remediation on underpricing, as the coefficient estimates of the interaction terms in all columns are statistically insignificant. Taken together, incremental to the mechanism that IPO firms are more willing to disclose ICD to avoid a more severe underpricing after the JOBS Act, I find some evidence that remediation disclosure also explains some variations of underpricing in the full sample. Nevertheless, the incremental benefits of remediation disclosure after the JOBS Act does not seem to hold in the matched sample. Given the lack of significance of in the matched sample, remediation disclosure does not fully explain the increase in voluntary disclosure of ICD after the JOBS Act.

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[15] Johnstone et al. (2011) examine a sample of public firms that file SOX section 404 reports and find that remediation of material weakness in internal controls is associated with turnover of CEO, CFO, and board members. In my IPO sample, no incidence of CEO turnover is disclosed as ICD remediation. In addition, eight pre-JOBS issuers and one post-JOBS issuer use changes in the board members as ICD remediation, such as forming an audit/compensation committee or appointing a financial expert with significant public accounting experience as the Chairman of the Audit Committee. The change in the board is not classified as a stand-alone category of remediation because it happens infrequently and some of the abovementioned board changes are mandated by the exchange listing rules after SOX.

### 2.6.2    Discretionary Accruals

This section investigates whether IPO firms with ICD disclosure have worse accounting quality than non-disclosing firms in the post-JOBS period. Ashbaugh-Skaife et al. (2008) find that firms with ICD are associated with larger abnormal accruals than firms without ICD. This finding raises the question whether the increased ICD disclosure is a manifestation of poorer accounting quality rather than an action to reduce information asymmetry. It is well-documented that IPO firms have abnormally high accruals (i.e., Aharony et al. 1993; Friedlan 1994; Teoh et al. 1998; Morsfield and Tan 2006), and such abnormal accruals are often interpreted as opportunistic earnings management because managers have the incentive to inflate the stock price and because information asymmetry is arguably the greatest around IPO. Following the convention in this literature, I examine the discretionary accruals measured at the IPO year in the following regression model:

$$
\begin{aligned}
DACC_i = \beta_0 &+ \beta_1 Post_i + \beta_2 ICD_i + \beta_3 Post_i * ICD_i + \beta_4 Age_i + \beta_5 Size_i \\
&+ \beta_6 ChgSales_i + \beta_7 ROA_i + \beta_8 Leverage_i + \beta_9 Proceeds_i \\
&+ \beta_{10} VC\_Backed_i + \beta_{11} Reputation_i + \beta_{12} Big4_i \\
&+ \sum \gamma_h Industry_h + \varepsilon_i
\end{aligned}
\tag{2.6}
$$

*DACC* denotes discretionary accruals. I calculate both the discretionary accruals based on the modified Jones model (Dechow et al. 1995) and the performance-matched accruals as described by Kothari et al. (2005).  The latter is the difference between the modified Jones discretionary accruals of firm *i* and the modified Jones discretionary accruals of the matched firm with the closest current ROA in the same 2-digit SIC industry for the same year. Compared to discretionary accruals based on the modified Jones model, the performance-matched accruals provide additional controls for firm profitability. The coefficient $\beta_3$ captures the differential impact of going public

after the JOBS Act on the relation between discretionary accruals and ICD disclosure. I control for several firm characteristics that may affect discretionary accruals, including age (*Age*), size (*Size*), sales growth (*ChgSales*), return on assets (*ROA*), and leverage (*Leverage*). I also include IPO proceeds (*Proceeds*) to mitigate the concern that abnormally large post-IPO accruals are attributed to investing IPO proceeds in working capital as suggested by Ball and Shivakumar (2008) and Armstrong et al. (2016). Consistent with the prior literature (Aharony et al. 1993; Morsfield and Tan 2006), I also add several institutional characteristics to control for external monitoring environment including venture-capital backing (*VC_Backed*), underwriter reputation (*Reputation*), and the hiring of a Big 4 auditor (*Big4*).

Table 2.7 presents the results for discretionary accruals. In all columns, the coefficient *Post* is positive and significant, indicating that post-JOBS issuers may manipulate earnings more aggressively than pre-JOBS issuers. Yet the coefficient *ICD* is statistically insignificant, suggesting that firms disclosing ICD are not more likely to manage earnings. Notably, the interaction term is insignificant for modified Jones' accruals (*MJ accruals*), and statistically negative for performance matched accruals (*KLW accruals*). The latter result could suggest that disclosing firms have better accounting quality in the post-JOBS period than in the pre-JOBS period. In the untabulated analysis, I calculate discretionary accruals in the pre-IPO year and find similar results. The coefficient estimates on the interaction terms are either insignificant or significantly negative. For brevity, I do not tabulate the results from the matched sample; the interaction terms are all statistically insignificant. Taken together, there is no systematic evidence that post-JOBS issuers that disclose ICD during IPO have worse accounting quality as measured by discretionary accruals.

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### 2.6.3 SEC Comment Letters

This section examines the SEC monitoring activity during IPO after the JOBS Act. The legal environment of public securities is jointly determined by the disclosure regulation and SEC enforcement. To carry out the intent of lawmakers in specific circumstances, the SEC exercises *ex post* enforcement in each individual case to ensure an efficient and transparent capital market. The disclosure regulation as dictated by the JOBS Act and public enforcement by the SEC could complement each other. On the surface, reduced mandatory disclosure granted by the JOBS Act cuts down the workload of the SEC in the review process. Nevertheless, the reduced mandatory disclosure also increases the search cost for the regulators to undertake an investigation to penalize potential violators (Glaeser et al. 2001). In response to the JOBS Act, the SEC may increase its monitoring to compensate for the regulatory rollback. In a letter to the Senate, the SEC Chair Mary Schapiro expressed her concern about the JOBS Act, "…we must balance our responsibility to facilitate capital formation with our obligation to protect investors and our markets. Too often, investors are the target of fraudulent schemes disguised as investment opportunities." Indeed, the SEC has recently increased the budget and the personnel specifically for the filings reviews of IPO firms.[16] Further, Gupta and Israelsen (2016) find that after the JOBS Act, the SEC's comment letters use a more negative tone and more forceful language in their recommendations. To address the concern that the increased ICD disclosure is primarily driven by more intensive SEC

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[16] In the Congressional Budget Justification for the fiscal year 2014, the SEC requested 25 new positions for the division responsible for filings reviews, the Division of Corporation Finance, and mentioned that "these positions primarily would be devoted to implementing Title I of the Jumpstart Our Business Startups (JOBS) Act…the new positions will be used to hire additional attorneys and accountants to conduct reviews of registration statements and other corporate filings, including the additional filings expected to result from the JOBS Act, and to answer interpretive questions that relate to those filings and other matters relating to the companies making them." The number of actual positions in the Division of Corporation Finance increased from 467 in the fiscal year 2012 to 497 in the fiscal year 2014.

monitoring, I investigate the relation between SEC comment letters on internal control issues and ICD disclosure after the JOBS Act.

While the SEC is required by SOX Section 408 to review each public firm's filings at least once every three years, the SEC reviews the registration statement of each and every IPO. The review process starts with the first filing of the registration statement. The SEC may issue the first round of comment letters to provide guidance, ask for more information, or request firms to revise filings. IPO firms subsequently reply in writing to the SEC's comment letters and revise the registration statements as necessary. The SEC may initiate more than one round of comment letter conversations. The conversation continues until all the issues addressed in the comment letters are resolved. Finally, the registrant's offering becomes effective with the SEC's approval.

I measure the SEC monitoring activity by the presence of the SEC comments on internal control issues (*IC_Dummy*), the number of internal control issues raised by the SEC throughout the IPO review process (*IC_Sum*), and the number of internal control issues in the first SEC comment letter in reference to the registration statement (*IC_1stLett*).[17] The presence of the SEC comments on internal control issues measures the overall SEC oversight on internal controls. The total internal control issues during the SEC review capture the extent of the comments. Finally, as the SEC review process is a conversation between the SEC and firms, an unsatisfactory corporate response may trigger more SEC follow-up comments. The number of internal control issues in the first SEC comment letter in the review process mitigates the influence of confounding corporate responses. The regression model for the SEC comment letters is

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[17] Recall that I collect ICD disclosure in the prospectus, which is normally filed at the end of the SEC review process, immediately before the issue date.

$$IC\_Dummy_i / IC\_Sum_i / IC\_1stLett_i$$
$$= \beta_0 + \beta_1 Post_i + \beta_2 ICD_i + \beta_3 Post_i * ICD_i + \beta_4 Size_i + \beta_5 Age_i$$
$$+ \beta_6 Loss_i + \beta_7 ChgSales_i + \beta_8 Segments_i + \beta_9 M\&A_i$$
$$+ \beta_{10} Restructure_i + \beta_{11} ExtFin + \beta_{12} Litigation_i + \beta_{13} Big4_i$$
$$+ \beta_{14} Second\ Tier_i + \beta_{15} Restate_i + \beta_{16} IC\_Year_t$$
$$+ \sum \gamma_h Industry_h + \varepsilon_i \qquad (2.7)$$

where $\beta_3$ captures the incremental SEC monitoring on post-JOBS disclosing issuers. I use

the control variables similar to those in Cassell et al. (2013). As there are no contemporary control

firms for post-JOBS issuers, I include the annual variation of internal control issues in the SEC

comments (*IC_Year*) to account for the time trend in the SEC regulatory oversight. It is calculated

as the number of all public firms that receive an SEC comment letter on internal controls divided

by the number of all public firms that receive an SEC comment letter in each calendar year.

Appendix A.2 gives precise definitions of all variables.

Table 2.8 presents the results of SEC comment letters. I use logistic regressions to predict

the probability of receiving an SEC comment letter on internal control issues and use linear

regressions for the other two dependent variables. I report the average marginal effects of the

logistic regressions to better assess the economic magnitude. Similarly, columns 1–3 use all

disclosing and non-disclosing firms in the full sample, and columns 4–6 exclude disclosing firms

that have fully remediated ICD. Looking across the columns, the coefficient on *Post* is generally

negative. It suggests that post-JOBS issuers are less likely to get a comment letter on internal

control issues and get fewer comments on internal controls. The strongest predictor of the SEC

monitoring on internal controls is ICD disclosure, as the coefficients on ICD are statistically

significant for all three measures of SEC comments on internal controls. Yet the interaction term

is insignificant for the presence of internal control comments and internal control comments in the

first SEC comment letter. It is significantly negative for the total internal control issues raised by

the SEC. Overall, the SEC does not exercise more intensive monitoring on post-JOBS issuers with ICD disclosure than pre-JOBS issuers.[18] This result mitigates the concern that increased ICD disclosure is primarily driven by the SEC oversight rather than voluntary disclosure.

## 2.7 Conclusion

To encourage small firms to raise capital in the public market, the JOBS Act substantially rolls back the disclosure requirements put in place to protect investors. This regulatory relief is not without controversy, and the SEC has expressed serious concerns on the delay of internal control audits. The simple intuition voiced by the financial press suggests that delayed auditor oversight would only exacerbate the dearth of IPO disclosure. Surprisingly, I find that after the JOBS Act, IPO firms are more likely to voluntarily disclose ICD. The increased ICD disclosure is robust after matching pre- and post-JOBS issuers on observable firm characteristics that prior studies find correlate with ICD disclosure. Building on the insights from the disclosure theory, this finding is consistent with a dynamic view that investors rationally update their belief on the probability of ICD among IPO firms, and IPO firms respond by providing more ICD disclosure.

Barth et al. (2017) and Chaplinsky et al. (2017) find that post-JOBS issuers on average are more underpriced than pre-JOBS issuers, due to reduced mandatory disclosure. As investors discount IPO firms more heavily on average, some IPO firms may use voluntary ICD disclosure to reduce information asymmetry. Consistent with this conjecture, I find some evidence that disclosing firms are rewarded with even lower underpricing after the JOBS Act, in particular for

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[18] I use SEC comment letters to measure SEC oversight on internal control issues. It is possible that the perceived threat of greater SEC oversight, not necessarily the actual enforcement in the form of SEC comment letters, is sufficient to motivate IPO firms to provide more voluntary ICD disclosure. It is difficult to completely eliminate this possible explanation.

firms disclosing unremediated ICD. I also provide evidence that the increased ICD disclosure is not fully explained by remediation disclosure, poorer accounting quality or tougher SEC oversight. Collectively, I interpret these results to suggest that in response to reduced mandatory disclosure, some IPO firms rely more on ICD voluntary disclosure to separate themselves from other IPO firms with the lowest internal control quality.

**Figure 2.1 Compliance Timeline of ICD Disclosure**

**Before the JOBS Act in 2012**

| IPO | 1st 10-Q/10-K | 2nd 10-K after IPO |
|-----|----------------|---------------------|

Public float ≥ $700m (large accelerated filers): management assessment & auditor attestation

$75m ≤ Public float < $700m (accelerated filers): management assessment & auditor attestation

Public float < $75m (non-accelerated filers): management assessment

No requirement

Executive certification

**After the JOBS Act in 2012**

| IPO | 1st 10-Q/10-K | 2nd 10-K after IPO | 10-K after the 5th IPO anniversary |
|-----|----------------|---------------------|-------------------------------------|

Public float ≥ $700m (large accelerated filers): management assessment & auditor attestation

$75m ≤ Public float < $700m (accelerated filers): management assessment

Public float < $75m (non-accelerated filers): management assessment

Auditor attestation

No requirement

Executive certification

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**Figure 2.2 Decision Tree of the Disclosure Model**

| $t = 1$: Manager endowed private info. with prob. $(1 - p)$ | $t = 2$: Manager decides whether to disclose | $t = 3$: Investors price the firm |
|---|---|---|

No private information on internal control strength

$p$

Nature

$(1 - p)$

Receive private information on internal control strength

**Event A**: no

**Event B**: no disclosure (The manager withholds information)

Investors give the same price to all non-disclosing firms.

**Event C**: the manager discloses information

Investors price the firm as disclosed.

**Figure 2.3 Illustration of the JOBS Act and the Disclosure Threshold**



Firms that would have withheld private information before the JOBS Act reveal it after the JOBS Act.
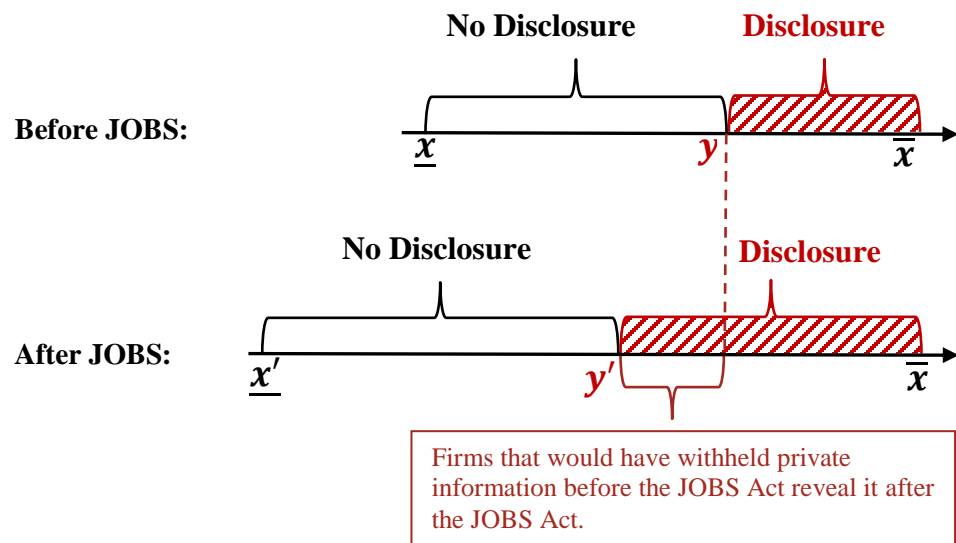
## Table 2.1 Disclosure of Internal Control Deficiencies

Panel A presents the ICD disclosure of pre-JOBS issuers that would have qualified for Emerging Growth Companies and post-JOBS Emerging Growth Companies in columns 1–3. Using the IPO proceeds as a proxy for the follow-up public float, columns 4–9 break down IPOs by the $75 million threshold to qualify for permanent exemption of auditor attestation. *Exempt* represents IPOs with proceeds below $75 million and *Non-Exempt* represents IPOs with proceeds of no less than $75 million. Panel B tabulates the matrix of ICD disclosure during IPO and the first ICD disclosure in compliance with SOX 302 after IPO. In both panels, *ICD* refers to disclosure of fully remediated and unremediated ICD. *Unrem. ICD* refers to disclosure that ICD remain unremediated.

### Panel A: IPO Disclosure

| | All IPOs | | | Exempt | | | Non-Exempt | | |
|---|---|---|---|---|---|---|---|---|---|
| | N | ICD% | Unrem. ICD% | N | ICD% | Unrem. ICD% | N | ICD% | Unrem. ICD% |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Pre-JOBS | 469 | 30.92% | 20.68% | 173 | 25.43% | 17.92% | 296 | 34.12% | 22.30% |
| Post-JOBS | 243 | 34.16% | 27.57% | 91 | 36.26% | 33.33% | 152 | 32.89% | 25.00% |

### Panel B: Post-IPO Disclosure*

| | No IPO Disclosure of ICD | | IPO Disclosure of *Unrem. ICD* | | |
|---|---|---|---|---|---|
| | Detect no ICD after IPO | Detect ICD after IPO | Detect no ICD after IPO | Detect ICD after IPO | Total |
| Pre-JOBS | 318(75.5%) | 6(1.4%) | 48(11.4%) | 49(11.6%) | 421 |
| Post-JOBS | 155(68.3%) | 5(2.2%) | 21(9.3%) | 46(20.3%) | 227 |

**\***percentages in the parentheses are calculated as dividing the number of firms by the sum of non-disclosing IPO firms and IPO firms disclosing unremediated ICD.

## Table 2.2 Descriptive Statistics

Panels A and B present the summary statistics of the full and matched samples for the ICD disclosure analysis. Panels C and D present the summary statistics of the full and matched samples for the underpricing analysis. All variables are defined in Appendix A.2. *, **, *** indicate two-tailed significance at the 0.10, 0.05, and 0.01 levels, respectively.

### Panel A: Full Sample of ICD Disclosure Analysis

| Variables | Pre-JOBS | | | | | Post-JOBS | | | | | Diff. in mean |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | N | Mean | P25 | P50 | P75 | N | Mean | P25 | P50 | P75 | |
| ICD | 469 | 0.31 | 0.00 | 0.00 | 1.00 | 243 | 0.34 | 0.00 | 0.00 | 1.00 | |
| Proceeds | 469 | 4.53 | 4.01 | 4.51 | 5.02 | 243 | 4.51 | 4.09 | 4.47 | 4.85 | |
| Cash | 469 | 0.29 | 0.04 | 0.20 | 0.46 | 243 | 0.45 | 0.10 | 0.43 | 0.82 | *** |
| Loss | 469 | 0.55 | 0.00 | 1.00 | 1.00 | 243 | 0.75 | 1.00 | 1.00 | 1.00 | *** |
| Age | 469 | 2.54 | 1.95 | 2.40 | 2.94 | 243 | 2.40 | 2.08 | 2.40 | 2.77 | ** |
| Segments | 469 | 2.18 | 1.00 | 1.00 | 3.00 | 243 | 1.70 | 1.00 | 1.00 | 2.00 | *** |
| Foreign | 469 | 0.27 | 0.00 | 0.00 | 1.00 | 243 | 0.31 | 0.00 | 0.00 | 1.00 | |
| M&A | 469 | 0.36 | 0.00 | 0.00 | 1.00 | 243 | 0.22 | 0.00 | 0.00 | 0.00 | *** |
| Restructure | 469 | 0.11 | 0.00 | 0.00 | 0.00 | 243 | 0.07 | 0.00 | 0.00 | 0.00 | * |
| ChgSales | 469 | 0.35 | 0.04 | 0.24 | 0.56 | 243 | 0.24 | 0.00 | 0.11 | 0.40 | *** |
| ChgInvt | 469 | 0.03 | 0.00 | 0.00 | 0.02 | 243 | 0.02 | 0.00 | 0.00 | 0.00 | |
| Restate | 469 | 0.14 | 0.00 | 0.00 | 0.00 | 243 | 0.07 | 0.00 | 0.00 | 0.00 | *** |
| AuditorChg | 469 | 0.11 | 0.00 | 0.00 | 0.00 | 243 | 0.05 | 0.00 | 0.00 | 0.00 | *** |
| Big4 | 469 | 0.81 | 1.00 | 1.00 | 1.00 | 243 | 0.81 | 1.00 | 1.00 | 1.00 | |
| Reputation | 469 | 8.12 | 8.00 | 9.00 | 9.00 | 243 | 7.88 | 7.50 | 8.50 | 9.00 | * |
| VC_Backed | 469 | 0.54 | 0.00 | 1.00 | 1.00 | 243 | 0.69 | 0.00 | 1.00 | 1.00 | *** |

### Panel B: Matched Sample of ICD Disclosure Analysis

| Variables | Pre-JOBS | | | | | Post-JOBS | | | | | Diff. in mean |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | N | Mean | P25 | P50 | P75 | N | Mean | P25 | P50 | P75 | |
| ICD | 243 | 0.26 | 0.00 | 0.00 | 1.00 | 243 | 0.34 | 0.00 | 0.00 | 1.00 | * |
| Proceeds | 243 | 4.54 | 3.92 | 4.47 | 5.08 | 243 | 4.51 | 4.09 | 4.47 | 4.85 | |
| Cash | 243 | 0.39 | 0.07 | 0.37 | 0.65 | 243 | 0.45 | 0.10 | 0.43 | 0.82 | ** |
| Loss | 243 | 0.71 | 0.00 | 1.00 | 1.00 | 243 | 0.75 | 1.00 | 1.00 | 1.00 | |
| Age | 243 | 2.46 | 1.95 | 2.30 | 2.83 | 243 | 2.40 | 2.08 | 2.40 | 2.77 | |
| Segments | 243 | 1.75 | 1.00 | 1.00 | 2.00 | 243 | 1.70 | 1.00 | 1.00 | 2.00 | |
| Foreign | 243 | 0.31 | 0.00 | 0.00 | 1.00 | 243 | 0.31 | 0.00 | 0.00 | 1.00 | |
| M&A | 243 | 0.25 | 0.00 | 0.00 | 0.00 | 243 | 0.22 | 0.00 | 0.00 | 0.00 | |
| Restructure | 243 | 0.08 | 0.00 | 0.00 | 0.00 | 243 | 0.07 | 0.00 | 0.00 | 0.00 | |
| ChgSales | 243 | 0.24 | 0.00 | 0.12 | 0.42 | 243 | 0.24 | 0.00 | 0.11 | 0.40 | |
| ChgInvt | 243 | 0.02 | 0.00 | 0.00 | 0.02 | 243 | 0.02 | 0.00 | 0.00 | 0.00 | |
| Restate | 243 | 0.09 | 0.00 | 0.00 | 0.00 | 243 | 0.07 | 0.00 | 0.00 | 0.00 | |
| AuditorChg | 243 | 0.05 | 0.00 | 0.00 | 0.00 | 243 | 0.05 | 0.00 | 0.00 | 0.00 | |
| Big4 | 243 | 0.81 | 1.00 | 1.00 | 1.00 | 243 | 0.81 | 1.00 | 1.00 | 1.00 | |
| Reputation | 243 | 7.97 | 8.00 | 9.00 | 9.00 | 243 | 7.88 | 7.50 | 8.50 | 9.00 | |
| VC_Backed | 243 | 0.63 | 0.00 | 1.00 | 1.00 | 243 | 0.69 | 0.00 | 1.00 | 1.00 | |

**Panel C: Full Sample of Underpricing Analysis**

| Variables | N | Mean | P25 | P50 | P75 | N | Mean | P25 | P50 | P75 | Diff. in mean |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | **Pre-JOBS** | | | | | **Post-JOBS** | | | | |
| Underpricing | 466 | 0.14 | 0.00 | 0.08 | 0.22 | 242 | 0.22 | 0.00 | 0.12 | 0.33 | *** |
| ICD | 466 | 0.31 | 0.00 | 0.00 | 1.00 | 242 | 0.34 | 0.00 | 0.00 | 1.00 | |
| Risk Factors | 466 | 3.65 | 3.50 | 3.66 | 3.83 | 242 | 3.98 | 3.87 | 3.99 | 4.09 | *** |
| Overhang | 466 | 3.04 | 1.83 | 2.61 | 3.71 | 242 | 3.33 | 1.90 | 2.83 | 4.17 | * |
| PrcRev+ | 466 | 0.07 | 0.00 | 0.00 | 0.11 | 242 | 0.07 | 0.00 | 0.00 | 0.12 | |
| PrcRev- | 466 | -0.11 | -0.20 | 0.00 | 0.00 | 242 | -0.11 | -0.21 | 0.00 | 0.00 | |
| Market Return | 466 | 1.56 | -0.26 | 1.98 | 3.57 | 242 | 1.44 | -0.94 | 1.69 | 3.96 | |
| Age | 466 | 2.55 | 2.08 | 2.40 | 2.94 | 242 | 2.41 | 2.08 | 2.40 | 2.77 | *** |
| R&D | 466 | 0.21 | 0.00 | 0.07 | 0.27 | 242 | 0.41 | 0.03 | 0.24 | 0.47 | *** |
| ROA | 466 | -0.25 | -0.39 | -0.01 | 0.07 | 242 | -0.55 | -0.72 | -0.28 | -0.00 | *** |
| Size | 466 | 4.45 | 3.51 | 4.30 | 5.47 | 242 | 4.09 | 3.08 | 4.01 | 4.90 | *** |
| Reputation | 466 | 8.14 | 8.00 | 9.00 | 9.00 | 242 | 7.91 | 7.50 | 8.75 | 9.00 | * |
| VC_Backed | 466 | 0.55 | 0.00 | 1.00 | 1.00 | 242 | 0.69 | 0.00 | 1.00 | 1.00 | *** |

**Panel D: Matched Sample of Underpricing Analysis**

| Variables | N | Mean | P25 | P50 | P75 | N | Mean | P25 | P50 | P75 | Diff. in mean |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | **Pre-JOBS** | | | | | **Post-JOBS** | | | | |
| Underpricing | 242 | 0.12 | -0.00 | 0.06 | 0.20 | 242 | 0.22 | 0.00 | 0.12 | 0.33 | *** |
| ICD | 242 | 0.26 | 0.00 | 0.00 | 1.00 | 242 | 0.34 | 0.00 | 0.00 | 1.00 | ** |
| Risk Factors | 242 | 3.67 | 3.53 | 3.69 | 3.83 | 242 | 3.98 | 3.87 | 3.99 | 4.09 | *** |
| Overhang | 242 | 3.13 | 1.88 | 2.62 | 3.81 | 242 | 3.33 | 1.90 | 2.83 | 4.17 | |
| PrcRev+ | 242 | 0.07 | 0.00 | 0.00 | 0.10 | 242 | 0.07 | 0.00 | 0.00 | 0.12 | |
| PrcRev- | 242 | -0.13 | -0.23 | 0.00 | 0.00 | 242 | -0.11 | -0.21 | 0.00 | 0.00 | |
| Market Return | 242 | 1.57 | -0.61 | 2.10 | 3.70 | 242 | 1.44 | -0.94 | 1.69 | 3.96 | |
| Age | 242 | 2.47 | 1.95 | 2.30 | 2.83 | 242 | 2.41 | 2.08 | 2.40 | 2.77 | |
| R&D | 242 | 0.32 | 0.00 | 0.14 | 0.42 | 242 | 0.41 | 0.03 | 0.24 | 0.47 | * |
| ROA | 242 | -0.44 | -0.72 | -0.16 | 0.02 | 242 | -0.55 | -0.72 | -0.28 | -0.00 | |
| Size | 242 | 4.27 | 3.26 | 4.10 | 5.38 | 242 | 4.09 | 3.08 | 4.01 | 4.90 | |
| Reputation | 242 | 7.99 | 8.00 | 9.00 | 9.00 | 242 | 7.91 | 7.50 | 8.75 | 9.00 | |
| VC_Backed | 242 | 0.63 | 0.00 | 1.00 | 1.00 | 242 | 0.69 | 0.00 | 1.00 | 1.00 | |

**Table 2.3 The Impact of the JOBS Act on IPO Disclosure of ICD**

This table presents the results of the logistic regressions to predict the probability of ICD disclosure. The dependent variable in all the regressions is the indicator variable *ICD*. It is set equal to one if the firm discloses the status of internal control deficiencies in the prospectus, and zero otherwise. *Post* is an indicator variable equal to one if the firm went public after the JOBS Act and identified itself as an EGC, and zero if it went public before the JOBS Act and had revenue of less than the $1 billion threshold. Panel A uses the full sample and Panel B uses the matched sample. In both panels, columns 1–3 include all disclosing and non-disclosing firms and columns 4–6 exclude disclosing firms that have fully remediated ICD. Columns 1 and 4 use all IPOs. Columns 2–3 and 5–6 partition the sample by the $75 million threshold of IPO proceeds. *Exempt* represents IPOs with proceeds below $75 million and *Non-Exempt* represents IPOs with proceeds no less than $75 million. The continuous variables are winsorized at 1 percent and 99 percent. In all columns, the estimates of average marginal effects are reported. Below the estimates, t-statistics are reported in parentheses and based on the standard errors clustered at the industry-quarter level. *, **, *** indicate two-tailed significance at the 0.10, 0.05, and 0.01 levels, respectively. All variables are defined in Appendix A.2.

**Panel A: Full Sample**

| | ICD | | | Unremediated ICD | | |
|---|---|---|---|---|---|---|
| | *All IPOs* | *Exempt* | *Non-Exempt* | *All IPOs* | *Exempt* | *Non-Exempt* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Post* | **0.091\*\*** | **0.148\*\*\*** | **0.049** | **0.116\*\*\*** | **0.160\*\*\*** | **0.070** |
| | **(2.370)** | **(2.636)** | **(1.021)** | **(3.025)** | **(2.851)** | **(1.436)** |
| *Proceeds* | -0.022 | -0.067 | -0.042 | -0.023 | -0.074 | -0.024 |
| | (-0.815) | (-0.918) | (-1.102) | (-0.880) | (-1.058) | (-0.638) |
| *Cash* | -0.153* | -0.152 | -0.135 | -0.159** | -0.209** | -0.104 |
| | (-1.935) | (-1.451) | (-1.168) | (-2.151) | (-2.182) | (-0.961) |
| *Loss* | 0.037 | 0.069 | 0.030 | 0.077* | 0.091 | 0.088* |
| | (0.905) | (0.997) | (0.604) | (1.929) | (1.399) | (1.807) |
| *Age* | -0.006 | -0.030 | 0.001 | -0.012 | -0.012 | -0.008 |
| | (-0.233) | (-0.543) | (0.023) | (-0.502) | (-0.243) | (-0.298) |
| *Segments* | 0.017 | 0.019 | 0.016 | 0.006 | 0.001 | 0.008 |
| | (1.246) | (0.877) | (0.994) | (0.475) | (0.062) | (0.520) |
| *Foreign* | 0.049 | 0.046 | 0.049 | 0.004 | 0.054 | -0.017 |
| | (1.148) | (0.625) | (0.973) | (0.083) | (0.772) | (-0.309) |
| *M&A* | 0.093** | 0.122* | 0.073 | 0.118*** | 0.080 | 0.118*** |
| | (2.419) | (1.695) | (1.639) | (3.375) | (1.191) | (2.832) |
| *Restructure* | -0.030 | -0.058 | -0.027 | -0.058 | 0.020 | -0.098 |
| | (-0.492) | (-0.526) | (-0.367) | (-0.981) | (0.203) | (-1.236) |
| *ChgSales* | 0.088** | 0.030 | 0.112** | 0.061 | 0.050 | 0.074 |
| | (2.146) | (0.451) | (2.080) | (1.454) | (0.758) | (1.361) |
| *ChgInvt* | 0.082 | -0.063 | 0.146 | 0.108 | -0.549 | 0.278 |
| | (0.339) | (-0.102) | (0.521) | (0.519) | (-1.052) | (1.073) |
| *Restate* | 0.184*** | 0.120 | 0.213*** | 0.198*** | 0.088 | 0.227*** |
| | (3.840) | (1.371) | (3.600) | (4.492) | (1.176) | (4.359) |
| *AuditorChg* | 0.164*** | 0.151* | 0.172** | 0.134*** | 0.111 | 0.130* |
| | (3.042) | (1.884) | (2.270) | (2.612) | (1.351) | (1.791) |
| *Big4* | -0.062 | -0.152** | 0.019 | -0.048 | -0.123* | 0.040 |
| | (-1.289) | (-2.179) | (0.285) | (-1.063) | (-1.697) | (0.610) |
| *Reputation* | -0.003 | -0.007 | 0.046 | -0.004 | -0.009 | 0.028 |
| | (-0.255) | (-0.359) | (1.520) | (-0.320) | (-0.507) | (1.244) |
| *VC_Backed* | 0.067 | 0.131 | 0.039 | 0.052 | 0.126* | 0.012 |
| | (1.264) | (1.596) | (0.610) | (1.075) | (1.648) | (0.188) |
| | | | | | | |
| *Observations* | 712 | 264 | 448 | 648 | 247 | 401 |
| *Pseudo R²* | 0.073 | 0.124 | 0.075 | 0.092 | 0.150 | 0.098 |

**Panel B: Matched Sample**

| | ICD | | | Unremediated ICD | | |
|---|---|---|---|---|---|---|
| | *All IPOs* | *Exempt* | *Non-Exempt* | *All IPOs* | *Exempt* | *Non-Exempt* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Post* | **0.083**** | **0.136**** | **0.055** | **0.092**** | **0.146**** | **0.068** |
| | **(2.155)** | **(2.363)** | **(1.052)** | **(2.315)** | **(2.356)** | **(1.250)** |
| *Proceeds* | -0.042 | -0.058 | -0.078* | -0.036 | -0.119 | -0.030 |
| | (-1.324) | (-0.702) | (-1.795) | (-1.221) | (-1.425) | (-0.732) |
| *Cash* | -0.206** | -0.144 | -0.246** | -0.233*** | -0.225* | -0.191* |
| | (-2.201) | (-1.091) | (-2.105) | (-2.928) | (-1.903) | (-1.692) |
| *Loss* | 0.024 | 0.111 | 0.042 | 0.104** | 0.172 | 0.102 |
| | (0.510) | (0.991) | (0.768) | (2.130) | (1.554) | (1.547) |
| *Age* | -0.023 | -0.033 | -0.042 | -0.061* | -0.009 | -0.021 |
| | (-0.625) | (-0.439) | (-0.951) | (-1.797) | (-0.118) | (-0.578) |
| *Segments* | 0.025 | -0.002 | 0.021 | 0.020 | 0.002 | 0.028 |
| | (1.288) | (-0.060) | (0.897) | (1.004) | (0.063) | (1.288) |
| *Foreign* | 0.045 | 0.154* | 0.002 | -0.064 | 0.101 | -0.076 |
| | (0.929) | (1.831) | (0.027) | (-1.158) | (1.269) | (-1.153) |
| *M&A* | 0.062 | 0.150 | 0.044 | 0.155*** | 0.081 | 0.098* |
| | (1.259) | (1.590) | (0.784) | (3.419) | (0.752) | (1.898) |
| *Restructure* | 0.026 | -0.059 | 0.027 | -0.048 | -0.052 | -0.174 |
| | (0.319) | (-0.420) | (0.246) | (-0.559) | (-0.378) | (-1.406) |
| *ChgSales* | 0.134** | -0.043 | 0.136** | 0.108* | -0.064 | 0.100 |
| | (2.461) | (-0.400) | (2.174) | (1.836) | (-0.610) | (1.480) |
| *ChgInvt* | -0.083 | -0.267 | 0.013 | 0.002 | -0.426 | 0.195 |
| | (-0.301) | (-0.473) | (0.040) | (0.007) | (-0.615) | (0.651) |
| *Restate* | 0.184*** | -0.061 | 0.226*** | 0.204*** | 0.040 | 0.216** |
| | (2.700) | (-0.567) | (2.853) | (3.865) | (0.429) | (2.568) |
| *AuditorChg* | 0.073 | 0.169 | 0.107 | -0.028 | -0.060 | 0.070 |
| | (0.920) | (0.930) | (0.935) | (-0.314) | (-0.268) | (0.730) |
| *Big4* | -0.114** | -0.215*** | 0.051 | -0.091* | -0.170** | 0.027 |
| | (-2.070) | (-2.580) | (0.680) | (-1.850) | (-2.101) | (0.327) |
| *Reputation* | -0.013 | -0.020 | 0.046 | -0.012 | -0.009 | 0.023 |
| | (-0.909) | (-0.971) | (1.354) | (-0.919) | (-0.457) | (0.946) |
| *VC_Backed* | 0.118* | 0.131 | 0.045 | 0.083 | 0.126 | 0.050 |
| | (1.762) | (1.361) | (0.625) | (1.507) | (1.363) | (0.661) |
| *Observations* | 486 | 182 | 304 | 454 | 174 | 280 |
| *Pseudo R²* | 0.091 | 0.203 | 0.072 | 0.123 | 0.196 | 0.086 |

## Table 2.4 ICD Disclosure and IPO Underpricing: Interaction

This table presents the results of ICD disclosure and IPO underpricing. The dependent variable in all the regressions is *Underpricing,* which is defined as the closing price on the first trading day divided by the offer price minus 1. *Post* is an indicator variable equal to one if the firm went public after the JOBS Act and identified itself as an EGC, and zero if it went public before the JOBS Act and had revenue less than the $1 billion threshold. Panel A uses the full sample and Panel B uses the matched sample. In both panels, columns 1–3 use all disclosing and non-disclosing firms and columns 4–6 exclude disclosing firms that have fully remediated ICD. Columns 1 and 4 use all IPOs. Columns 2 and 3, columns 5 and 6 partition the sample by the $75 million threshold of IPO proceeds. *Exempt* represents IPOs with proceeds below $75 million and *Non-Exempt* represents IPOs with proceeds no less than $75 million. The continuous variables are winsorized at 1 percent and 99 percent. Below the coefficient estimates, t-statistics are reported in parentheses, based on the standard errors clustered at the industry-quarter level. *, **, *** indicate two-tailed significance at the 0.10, 0.05, and 0.01 levels, respectively. All variables are defined in Appendix A.2.

### Panel A: Full Sample

|  | ICD | | | Unremediated ICD | | |
|---|---|---|---|---|---|---|
|  | *All IPOs* | *Exempt* | *Non-Exempt* | *All IPOs* | *Exempt* | *Non-Exempt* |
|  | (1) | (2) | (3) | (4) | (5) | (6) |
| *Post* | 0.115*** | 0.055 | 0.145*** | 0.111*** | 0.053 | 0.135*** |
|  | (4.597) | (1.591) | (4.295) | (4.337) | (1.519) | (3.838) |
| *ICD* | 0.003 | 0.012 | -0.002 | 0.009 | 0.007 | 0.003 |
|  | (0.211) | (0.486) | (-0.096) | (0.526) | (0.257) | (0.118) |
| *Post*ICD* | -0.065* | -0.073* | -0.065 | -0.095*** | -0.086* | -0.095* |
|  | (-1.847) | (-1.731) | (-1.267) | (-2.870) | (-1.865) | (-1.839) |
| *Risk Factors* | -0.053 | -0.064 | -0.037 | -0.049 | -0.065 | -0.021 |
|  | (-1.484) | (-1.417) | (-0.773) | (-1.273) | (-1.409) | (-0.398) |
| *Overhang* | 0.009* | 0.003 | 0.008 | 0.009** | 0.005 | 0.008 |
|  | (1.916) | (0.429) | (1.353) | (1.983) | (0.694) | (1.321) |
| *PrcRev+* | 1.236*** | 0.945*** | 1.214*** | 1.241*** | 0.946*** | 1.238*** |
|  | (9.758) | (4.369) | (9.285) | (8.669) | (4.348) | (8.258) |
| *PrcRev-* | 0.118** | 0.018 | 0.100 | 0.116** | 0.014 | 0.120 |
|  | (2.204) | (0.231) | (1.009) | (2.123) | (0.176) | (1.113) |
| *Market Return* | 0.005** | 0.001 | 0.005* | 0.005* | 0.001 | 0.005 |
|  | (2.116) | (0.413) | (1.668) | (1.816) | (0.174) | (1.414) |
| *Age* | -0.010 | 0.007 | -0.006 | -0.003 | 0.021 | 0.000 |
|  | (-0.954) | (0.291) | (-0.446) | (-0.253) | (0.842) | (0.029) |
| *R&D* | -0.018 | 0.002 | -0.114** | -0.018 | 0.005 | -0.127** |
|  | (-0.692) | (0.124) | (-2.303) | (-0.654) | (0.278) | (-2.267) |
| *ROA* | 0.025 | 0.023 | -0.011 | 0.020 | 0.020 | -0.027 |
|  | (1.332) | (1.567) | (-0.305) | (1.008) | (1.359) | (-0.681) |
| *Size* | -0.006 | -0.003 | -0.021 | -0.004 | -0.003 | -0.016 |
|  | (-0.574) | (-0.167) | (-1.541) | (-0.295) | (-0.152) | (-1.144) |
| *Reputation* | 0.001 | -0.006 | 0.013* | 0.001 | -0.006 | 0.014* |
|  | (0.101) | (-0.674) | (1.765) | (0.097) | (-0.524) | (1.871) |
| *VC_Backed* | 0.043** | 0.024 | 0.049 | 0.051** | 0.031 | 0.058 |
|  | (1.986) | (0.808) | (1.443) | (2.202) | (0.847) | (1.632) |
| *Constant* | 0.376*** | 0.477** | 0.142 | 0.315** | 0.428* | 0.188 |
|  | (2.610) | (2.199) | (0.870) | (2.034) | (1.958) | (0.986) |
| *Industry FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Exchange FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Observations* | 708 | 262 | 446 | 644 | 245 | 399 |
| *Adjusted R²* | 0.494 | 0.204 | 0.520 | 0.493 | 0.239 | 0.519 |

**Panel B: Matched Sample**

| | ICD | | | Unremediated ICD | | |
|---|---|---|---|---|---|---|
| | *All IPOs* | *Exempt* | *Non-Exempt* | *All IPOs* | *Exempt* | *Non-Exempt* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Post* | **0.107***** | **0.047** | **0.170***** | **0.101***** | **0.031** | **0.145***** |
| | **(3.591)** | **(1.095)** | **(4.011)** | **(3.410)** | **(0.779)** | **(3.457)** |
| *ICD* | **-0.010** | **-0.025** | **0.036** | **0.014** | **-0.010** | **-0.001** |
| | **(-0.418)** | **(-0.769)** | **(0.942)** | **(0.596)** | **(-0.326)** | **(-0.051)** |
| *Post*ICD* | **-0.044** | **-0.046** | **-0.109*** | **-0.091**** | **-0.080** | **-0.102*** |
| | **(-1.138)** | **(-0.908)** | **(-1.712)** | **(-2.581)** | **(-1.613)** | **(-1.880)** |
| *Risk Factors* | -0.025 | -0.027 | -0.040 | 0.009 | 0.029 | -0.016 |
| | (-0.527) | (-0.377) | (-0.561) | (0.187) | (0.473) | (-0.216) |
| *Overhang* | 0.005 | 0.007 | 0.009 | 0.010* | 0.012 | 0.013* |
| | (0.863) | (0.885) | (1.103) | (1.720) | (1.368) | (1.677) |
| *PrcRev+* | 1.440*** | 1.263*** | 1.303*** | 1.481*** | 1.199*** | 1.311*** |
| | (9.635) | (5.351) | (7.653) | (8.527) | (5.300) | (6.939) |
| *PrcRev-* | 0.131** | -0.015 | 0.100 | 0.105 | -0.013 | 0.125 |
| | (2.029) | (-0.150) | (0.718) | (1.588) | (-0.155) | (0.888) |
| *Market Return* | 0.006** | 0.001 | 0.009** | 0.007** | -0.000 | 0.007 |
| | (2.094) | (0.175) | (2.076) | (2.076) | (-0.021) | (1.417) |
| *Age* | -0.014 | -0.000 | -0.014 | 0.000 | 0.017 | -0.012 |
| | (-0.967) | (-0.012) | (-0.613) | (0.020) | (0.561) | (-0.551) |
| *R&D* | -0.031 | -0.007 | -0.133** | -0.041* | 0.013 | -0.145** |
| | (-1.450) | (-0.358) | (-2.334) | (-1.706) | (0.630) | (-2.210) |
| *ROA* | 0.004 | 0.014 | -0.028 | -0.010 | 0.012 | -0.056 |
| | (0.310) | (0.852) | (-0.662) | (-0.723) | (0.667) | (-1.196) |
| *Size* | 0.002 | 0.002 | -0.024 | 0.001 | 0.021 | -0.013 |
| | (0.120) | (0.079) | (-1.205) | (0.053) | (0.719) | (-0.594) |
| *Reputation* | -0.006 | -0.015 | 0.009 | -0.000 | -0.021 | 0.008 |
| | (-0.764) | (-1.251) | (1.072) | (-0.001) | (-1.345) | (1.056) |
| *VC_Backed* | 0.037 | 0.054 | 0.033 | 0.025 | 0.048 | 0.043 |
| | (1.332) | (1.347) | (0.722) | (0.924) | (0.884) | (0.902) |
| *Constant* | 0.289 | 0.298 | 0.141 | 0.035 | -0.070 | -0.009 |
| | (1.501) | (1.019) | (0.587) | (0.178) | (-0.286) | (-0.038) |
| *Industry FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Exchange FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Observations* | 484 | 181 | 303 | 452 | 173 | 279 |
| *Adjusted R²* | 0.539 | 0.236 | 0.507 | 0.553 | 0.314 | 0.536 |

**Table 2.5 ICD Disclosure and IPO Underpricing: Decomposition of Interaction Terms**

This table presents the results of the change in IPO underpricing after the JOBS Act conditional on ICD disclosure.. The dependent variable in all the regressions is *Underpricing,* which is defined as the closing price on the first trading day divided by the offer price minus 1. *Post* is an indicator variable equal to one if the firm went public after the JOBS Act and identified itself as an EGC, and zero if it went public before the JOBS Act and had revenue less than the $1 billion threshold. Columns 1-4 use the full sample and columns 5-8 use the matched sample. Columns 1 and 5 use all IPOs in the full and matched samples respectively. Column 2 and 6 use IPOs with no ICD disclosure. Columns 3 and 7 use IPOs that disclose the status of ICD. Columns 4 and 8 use IPOs disclosing that at least one internal control deficiency remains unremediated around IPO. The continuous variables are winsorized at 1 percent and 99 percent. Below the coefficient estimates, t-statistics are reported in parentheses, based on the standard errors clustered at the industry-quarter level. *, **, *** indicate two-tailed significance at the 0.10, 0.05, and 0.01 levels, respectively. All variables are defined in Appendix A.2.

| | Full Sample | | | | Matched Sample | | | |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| | *All IPOs* | *Non-Disclosure* | *ICD* | *Unrem. ICD* | *All IPOs* | *Non-Disclosure* | *ICD* | *Unrem. ICD* |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Post* | 0.093*** | 0.109*** | 0.073 | 0.033 | 0.094*** | 0.096*** | 0.065 | -0.002 |
| | (4.133) | (4.208) | (1.644) | (0.772) | (3.635) | (2.773) | (1.268) | (-0.041) |
| *Control* | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *Exchange FE* | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *Industry FE* | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| *Observations* | 708 | 481 | 227 | 163 | 484 | 338 | 146 | 110 |
| *Adjusted R²* | 0.491 | 0.528 | 0.405 | 0.373 | 0.537 | 0.569 | 0.386 | 0.371 |

## Table 2.6 ICD Remediation and IPO Underpricing

This table presents the results of ICD remediation and IPO underpricing in the full sample. The dependent variable in all the regressions is *Underpricing*, which is calculated as the closing price on the first trading day divided by the offer price minus 1. *Post* is an indicator variable equal to one if the firm went public after the JOBS Act and identified itself as an EGC, and zero if it went public before the JOBS Act and had revenue less than the $1 billion threshold. Panel A uses the full sample and Panel B uses the matched sample. *NewCFO* is an indicator variable equal to one if the firm discloses that a new CFO is or will be hired to remediate ICD and zero otherwise. *Employee* is an indicator variable equal to one if the firm has improved or plans to improve personnel for public reporting and zero otherwise. *Process* is an indicator variable equal to one if the firm has improved or plans to improve policies, procedures, and the infrastructure of financial reporting, and zero otherwise. *NumRem* is the sum of *NewCFO*, *Employee,* and *Process*. Panel B and Panel C partition all IPOs by the $75 million threshold of IPO proceeds. In all panels, columns 1–4 include all disclosing and non-disclosing firms, and columns 5–8 exclude disclosing firms that have fully remediated ICD. The continuous variables are winsorized at 1 percent and 99 percent. Below the coefficient estimates, t-statistics are reported in parentheses, based on the standard errors clustered at the industry-quarter level. \*, \*\*, \*\*\* indicate two-tailed significance at the 0.10, 0.05, and 0.01 levels, respectively. All variables are defined in Appendix A.2.

**Panel A: Full Sample**

| | ICD | | | | Unremediated ICD | | | |
|---|---|---|---|---|---|---|---|---|
| | *Rem as NewCFO* | *Rem as Employee* | *Rem as Process* | *NumRem* | *Rem as NewCFO* | *Rem as Employee* | *Rem as Process* | *NumRem* |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post | 0.096*** | 0.113*** | 0.101*** | 0.109*** | 0.087*** | 0.106*** | 0.095*** | 0.101*** |
| | (4.120) | (4.726) | (4.238) | (4.542) | (3.617) | (4.306) | (3.736) | (4.029) |
| Rem | -0.057** | 0.010 | -0.005 | -0.003 | -0.060* | 0.004 | 0.003 | -0.002 |
| | (-2.113) | (0.560) | (-0.270) | (-0.344) | (-1.738) | (0.189) | (0.116) | (-0.171) |
| Post*Rem | -0.037 | -0.075** | -0.033 | -0.027 | -0.028 | -0.092*** | -0.054 | -0.034** |
| | (-0.889) | (-2.003) | (-0.782) | (-1.553) | (-0.582) | (-2.636) | (-1.305) | (-2.013) |
| Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Exchange FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 708 | 708 | 708 | 708 | 644 | 644 | 644 | 644 |
| Adjusted $R^2$ | 0.495 | 0.494 | 0.490 | 0.494 | 0.489 | 0.493 | 0.487 | 0.491 |

**Panel B: Matched Sample**

| | ICD | | | | Unremediated ICD | | | |
|---|---|---|---|---|---|---|---|---|
| | *Rem as NewCFO* | *Rem as Employee* | *Rem as Process* | *NumRem* | *Rem as NewCFO* | *Rem as Employee* | *Rem as Process* | *NumRem* |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post | 0.097*** | 0.097*** | 0.095*** | 0.098*** | 0.078*** | 0.089*** | 0.085*** | 0.088*** |
| | (3.592) | (3.554) | (3.344) | (3.485) | (2.780) | (3.122) | (2.901) | (3.008) |
| Rem | -0.041 | -0.038 | -0.015 | -0.015 | -0.073 | -0.022 | 0.008 | -0.008 |
| | (-0.831) | (-1.205) | (-0.543) | (-1.045) | (-1.644) | (-0.637) | (0.261) | (-0.576) |
| Post*Rem | -0.053 | -0.013 | -0.006 | -0.009 | -0.014 | -0.049 | -0.041 | -0.021 |
| | (-0.966) | (-0.306) | (-0.132) | (-0.456) | (-0.261) | (-1.138) | (-0.927) | (-1.129) |
| Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Exchange FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 484 | 484 | 484 | 484 | 452 | 452 | 452 | 452 |
| Adjusted $R^2$ | 0.540 | 0.540 | 0.535 | 0.539 | 0.550 | 0.551 | 0.546 | 0.550 |

## Table 2.7 Discretionary Accruals in the IPO Year

This table presents the results of discretionary accruals measured at the IPO year in the full sample. The results for both the modified Jones accruals (*MJ Accruals*) and the performance-matched accruals (*KLW Accruals*) are reported. Columns 1 and 2 include all disclosing and non-disclosing firms, and columns 3 and 4 exclude disclosing firms that have remediated ICD. The continuous variables are winsorized at 1 percent and 99 percent. Below the coefficient estimates, t-statistics are reported in parentheses, based on the standard errors clustered at the industry-quarter level. *, **, *** indicate two-tailed significance at the 0.10, 0.05, and 0.01 levels, respectively. All variables are defined in Appendix A.2.

| | ICD | | Unremediated ICD | |
| --- | --- | --- | --- | --- |
| | *MJ Accruals* | *KLW Accruals* | *MJ Accruals* | *KLW Accruals* |
| | (1) | (2) | (3) | (4) |
| *Post* | 0.081*** | 0.066** | 0.083*** | 0.065** |
| | (4.486) | (2.247) | (4.545) | (2.175) |
| *ICD* | -0.020 | -0.003 | -0.017 | 0.018 |
| | (-1.136) | (-0.127) | (-0.892) | (0.625) |
| *Post\*ICD* | -0.027 | -0.080* | -0.032 | -0.122** |
| | (-0.938) | (-1.805) | (-1.043) | (-2.402) |
| *Age* | 0.028** | -0.007 | 0.025** | -0.002 |
| | (2.277) | (-0.361) | (1.996) | (-0.100) |
| *Size* | -0.033*** | -0.041** | -0.039*** | -0.054*** |
| | (-2.692) | (-2.575) | (-3.134) | (-3.296) |
| *ChgSales* | -0.008 | -0.082*** | -0.005 | -0.072** |
| | (-0.456) | (-2.770) | (-0.242) | (-2.512) |
| *ROA* | 0.049*** | 0.032*** | 0.048*** | 0.033*** |
| | (5.088) | (3.076) | (4.986) | (3.155) |
| *Leverage* | 0.054 | 0.137* | 0.076* | 0.118 |
| | (1.298) | (1.691) | (1.709) | (1.545) |
| *Proceeds* | 0.020 | 0.018 | 0.021 | 0.027 |
| | (1.230) | (0.734) | (1.328) | (1.093) |
| *VC_Backed* | -0.029 | -0.015 | -0.035* | -0.048 |
| | (-1.458) | (-0.509) | (-1.746) | (-1.590) |
| *Reputation* | -0.000 | -0.016 | 0.003 | -0.007 |
| | (-0.033) | (-1.492) | (0.378) | (-0.714) |
| *Big4* | 0.024 | 0.039 | 0.035 | 0.041 |
| | (1.102) | (1.255) | (1.549) | (1.294) |
| *Constant* | 0.037 | -0.038 | 0.028 | -0.091 |
| | (0.457) | (-0.362) | (0.335) | (-0.859) |
| | | | | |
| *Industry FE* | Yes | Yes | Yes | Yes |
| *Observations* | 695 | 695 | 631 | 631 |
| *Adjusted $R^2$* | 0.202 | 0.074 | 0.203 | 0.058 |

## Table 2.8 SEC Comment Letters

This table presents the results of SEC comment letters in the full sample. Columns 1–3 include all disclosing and non-disclosing firms, and columns 4–6 exclude disclosing firms that have fully remediated ICD. Columns 1 and 4 report the average marginal effects of the logistic regressions to predict the probability of receiving a comment letter on internal control issues (*IC_Dummy*). The other columns report the results of linear regressions. The dependent variables in columns 2 and 5 are the number of internal control issues raised during the SEC review (*IC_Sum*). The dependent variables in columns 3 and 6 are the number of internal control issues in the first SEC comment letter (*IC_1stLett*). The continuous variables are winsorized at 1 percent and 99 percent. Below the estimates, t-statistics are reported in parentheses, based on the standard errors clustered at the industry-quarter level. *, **, *** indicate two-tailed significance at the 0.10, 0.05, and 0.01 levels, respectively. All variables are defined in Appendix A.2.

| | ICD | | | Unremediated ICD | | |
|---|---|---|---|---|---|---|
| | *IC_Dummy* | *IC_Sum* | *IC_1st Lett* | *IC_Dummy* | *IC_Sum* | *IC_1st Lett* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Post* | -0.116* | -0.261** | -0.090 | -0.149*** | -0.316*** | -0.166** |
| | (-1.868) | (-2.237) | (-1.090) | (-2.689) | (-2.813) | (-2.126) |
| *ICD* | 0.233*** | 0.753*** | 0.518*** | 0.222*** | 0.840*** | 0.520*** |
| | (8.419) | (5.531) | (4.875) | (7.933) | (4.960) | (4.270) |
| *Post\*ICD* | 0.042 | -0.360** | -0.170 | 0.036 | -0.405* | -0.158 |
| | (0.648) | (-1.986) | (-1.142) | (0.579) | (-1.890) | (-0.951) |
| *Size* | 0.015 | 0.002 | 0.009 | 0.009 | -0.000 | 0.008 |
| | (1.022) | (0.053) | (0.282) | (0.621) | (-0.007) | (0.290) |
| *Age* | -0.019 | 0.011 | -0.002 | 0.013 | 0.051 | 0.054 |
| | (-0.704) | (0.211) | (-0.056) | (0.449) | (0.936) | (1.285) |
| *Loss* | -0.024 | -0.139 | -0.058 | 0.012 | -0.072 | 0.022 |
| | (-0.700) | (-1.500) | (-0.832) | (0.367) | (-0.782) | (0.327) |
| *ChgSales* | 0.063* | 0.056 | 0.106 | 0.035 | 0.063 | 0.111 |
| | (1.673) | (0.693) | (1.433) | (1.064) | (0.720) | (1.411) |
| *Segments* | -0.002 | 0.007 | 0.008 | 0.010 | 0.034 | 0.019 |
| | (-0.239) | (0.336) | (0.417) | (1.101) | (1.457) | (1.014) |
| *M&A* | 0.025 | 0.100 | 0.104 | -0.020 | -0.018 | 0.006 |
| | (0.731) | (1.118) | (1.334) | (-0.570) | (-0.202) | (0.075) |
| *Restructure* | -0.083* | -0.217** | -0.100 | -0.041 | -0.173* | -0.086 |
| | (-1.676) | (-2.129) | (-1.063) | (-0.819) | (-1.928) | (-1.140) |
| *ExtFin* | 0.006 | 0.047 | 0.060 | 0.005 | 0.043 | 0.060 |
| | (0.637) | (1.048) | (1.398) | (0.608) | (0.917) | (1.384) |
| *Litigation* | 0.068 | 0.249 | 0.102 | 0.070 | 0.217 | 0.053 |
| | (1.138) | (1.630) | (0.757) | (1.148) | (1.337) | (0.385) |
| *Big4* | 0.009 | 0.055 | 0.013 | 0.011 | 0.092 | 0.024 |
| | (0.158) | (0.510) | (0.142) | (0.199) | (0.806) | (0.249) |
| *Second Tier* | 0.017 | 0.090 | 0.112 | -0.031 | -0.004 | 0.016 |
| | (0.285) | (0.621) | (0.828) | (-0.487) | (-0.027) | (0.115) |
| *Restate* | 0.145*** | 0.526*** | 0.225** | 0.156*** | 0.583*** | 0.247** |
| | (4.248) | (3.111) | (2.055) | (5.192) | (3.476) | (2.126) |
| *IC_Year* | 0.122 | -1.814 | -0.095 | -0.318 | -2.574** | -0.955 |
| | (0.365) | (-1.488) | (-0.108) | (-0.935) | (-2.191) | (-1.164) |
| *Constant* | | 0.177 | 0.004 | | 0.006 | -0.182 |
| | | (0.721) | (0.022) | | (0.021) | (-0.894) |
| | | | | | | |
| *Industry FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Observations* | 655 | 712 | 712 | 591 | 648 | 648 |
| *Pseudo/Adjusted R²* | 0.306 | 0.221 | 0.130 | 0.348 | 0.240 | 0.130 |

# Chapter 3: What are the Costs of Disclosing Accounting Irregularities for U.S. Listed Foreign Firms?

## 3.1 Introduction

The purpose of this study is to examine the stock market consequences to disclosing accounting irregularities for U.S. listed foreign firms. Prior research has provided extensive evidence that investors tend to underweight foreign equities in their investment portfolios – a phenomenon known as "home bias" (French and Poterba 1991). One of the primary reasons suggested in the literature for this home bias is the poor quality and low credibility of financial information associated with foreign firms (Ahearne et al. 2004; Bradshaw et al. 2004; Kang and Stulz 1997). One way in which foreign firms can mitigate this risk is by listing on a U.S. stock exchange and committing themselves to higher levels of disclosure and regulatory oversight (Doidge et al. 2004), including requirements to promptly restate previously-issued financial statements if they are found to contain a material error.[19] Such restatements have been found to have significantly negative stock market consequences to U.S. firms (Palmrose et al. 2004; Karpoff et al. 2008), partly due to increases in investors' assessments of information risk (Kravet and Shevlin 2010). Given U.S. investors' pre-existing concerns about the quality and credibility of financial information of foreign equities, it is interesting to understand how investors react to a foreign firm restatement. Such evidence would shed light on the costs of failing to meet the more stringent regulatory requirements in the U.S.

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[19] Following the disclosure requirement set forth by the SEC, foreign firms listed in the U.S. have the same duty as U.S. firms to correct inaccurate, incomplete or misleading financial statement disclosures in the SEC filings upon discovery. Once identified, the nature and the effect of accounting misstatement must be disclosed.

We ask three specific research questions. First, do U.S. investors penalize foreign firms more than domestic firms when they report an accounting irregularity? Second, if so, is the greater stock market penalty for foreign firms related to institutional features of the home country or firm-specific information frictions common to foreign firms? Third, are there contagion effects of foreign firms' accounting irregularities; that is, do these announcements raise concerns about the credibility of financial statements issued by non-restating foreign firms from the same country? We focus on accounting irregularities – restatements that are more egregious than simple accounting errors – because this subset of restatements is likely to elicit greater concerns over the quality of a firm's information.

Prior research suggests that restatements cause investors to revise their beliefs about the quality and credibility of information about the firm, which we argue can stem from both firm-specific factors (such as managerial credibility, corporate governance structure, internal control strength, etc.) and country-specific factors (such as the regulatory and legal environment). Although U.S. listed foreign firms are subject to regulatory oversight by the SEC, local institutional features likely impact the business culture and the quality of financial reporting systems in each country. In addition, foreign firms likely face greater information frictions than U.S. firms because investors have less insight into and fewer sources of information about the operations of the firm. Thus, we argue that when foreign firms announce an irregularity, U.S. investors will have greater downward revisions in their assessments of the quality and credibility of firm information, leading to more negative stock price reactions.

In contrast, because the country-specific institutional features that influence the production of financial information in the U.S. are well known to U.S. investors, any revisions in beliefs about the information risk stemming from country-specific factors will be less significant for U.S. firms.

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Likewise, the country-specific institutional features of foreign firms are well known to their home country investors, so revisions in beliefs about country-specific information risk should be less significant for home country investors. In addition, U.S. investors have access to other sources of information about U.S. firms, as do the home country investors to their domestic firms. Thus, we hypothesize that the U.S. stock market reaction to accounting irregularities by foreign firms will be more negative than 1) the market reaction to accounting irregularities made by similar U.S. firms and 2) the market reaction to the same accounting irregularity in the foreign firms' home country.[20]

To the extent foreign firms' irregularities cause U.S. investors to question country-specific factors that influence the quality of financial information produced by the firm, we would also expect such restatements to impact investors' assessments of the information risk of non-restating firms from the same country. Thus, we predict a contagion effect – i.e., that non-restating foreign firms will experience a negative price reaction around restatement announcements by firms from their same country.

Using a sample of accounting irregularities announced by foreign firms listed in the U.S. from 2001-2016, we examine the two-day [0, 1] stock market reactions around the irregularity announcements. We find that foreign firms experience significantly more negative stock market reactions than their U.S. peers. On average, U.S. firms announcing accounting irregularities experience a −3% stock return surrounding the announcements while foreign firms experience a

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[20] It is possible that U.S. investors price the associated information risk related to country-specific factors into the stock price of foreign firms prior to the restatement announcements. In this case, we would not observe any systematic differences in stock market reactions to restatements between foreign firms and U.S. firms and between market reactions in the U.S. market vs. the home country market.

−5% stock return. This difference is statistically significant after controlling for the severity of the irregularity as well as a number of other firm characteristics. In addition, the results are robust to using sample-level entropy balancing to control for underlying differences between the U.S. and foreign firms.

Next, we identify, within our foreign irregularity sample, a subgroup of foreign firms that are listed in the U.S. as well as their home country stock exchanges. We compare market reactions to the same irregularity announcement in a firm's home country relative to the market reaction in the U.S. Consistent with the conjecture that investors generally react more negatively to foreign irregularities, we find a statistically significant divergence in the three-day market reaction to the same irregularity announcement between a foreign firm's U.S listed shares (–5.0%) and their home-country listed shares (–4.2%).[21]

Both analyses suggest that U.S. investors impose a more severe stock market penalty to the accounting irregularities of foreign firms, likely due to the uncertainty surrounding country-specific institutional features that influence the financial reporting process, as well as the lack of non-accounting information about foreign firms. In a cross-country analysis, we do not find statistically significant cross-sectional variation in the market penalty using various proxies for country-specific institutional differences that one might expect to explain this variation (e.g., differences in accounting principles, auditing and enforcement quality, and rule of law tradition). We do, however, find that a firm-specific measure of information frictions – the proportion of a firm's sales earned outside the U.S. – is negatively associated with the market reaction to

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[21] This difference is -0.8%, which is both statistically and economically significant. Gagnon and Karolyi (2010) show that, for a sample of 506 U.S. cross-listed stocks from 35 different countries, deviations from price parity average an economically small 4.9 basis points.

irregularity announcements. Thus, our findings suggest that one source for the differential response to foreign and U.S. irregularities is firm-specific information frictions that are more prominent for foreign firms.

Despite the fact that we do not find evidence that our three proxies for country-specific institutional features explain the differential market reaction, we do find evidence of a country-specific contagion effect. Specifically, we find that non-restating firms from the same country experience an average market-adjusted stock price reaction of −0.589% over the 6-day window [0, 5] following restatements. This suggests that there is some country-specific factor that investors associate with a foreign firm's accounting irregularity. It does not appear that this country-specific factor is related to institutional factors that we have identified because we do not find an association between the contagion effect and these factors. We do, however, find that, within a given country-year, the stock price decline is greater for non-restating firms with lower accruals quality. This evidence suggests that the country-level contagion effect is greater for firms whose firm-specific information risk is higher (as earnings quality is one aspect of information risk).

Our study makes three contributions to the literature. First, it contributes to our understanding of the negative stock market reactions to accounting misstatements. Our findings are consistent with the conjecture that, in the event of irregularities, perceived information risk increases more for foreign firms than U.S. firms and more among U.S. investors of foreign firms than their home country investors. Taken together, this increase in perceived information risk for foreign firms appears more related to the information frictions U.S. investors face with foreign firms than to the institutional features of a foreign firm's home country.

Our findings also contribute to the literature on the bonding hypothesis and the cross-listing premium. While prior studies show that foreign firms experience benefits (e.g., lower cost of

capital; see Hail and Leuz 2009) from listing in the U.S., our findings suggest that these firms also experience additional costs from breaching their bonds with the U.S. regulatory system when they report an accounting irregularity. These costs appear related to information frictions that are specific to a firm's operations (i.e., foreign sales) and suggest that simply bonding to the U.S. regulatory environment does not completely overcome these frictions.

Finally, our findings contribute to the literature on contagion. Contagion occurs when news released by one firm affects other firms in a peer group (e.g., in the same industry). Prior research documents contagion across industry associated with accounting restatements (Gleason et al. 2008). Our results suggest that contagion associated with restatements also occurs geographically.[22]

## 3.2 Prior literature and predictions

### 3.2.1 Related literature

This paper touches on two streams of prior research. The first is the literature on accounting restatements, which is the event that we examine in this paper. The second line of related research is the literature on foreign firms that are traded in the U.S.

A number of prior studies have documented significant negative market reactions to announcements of misstatements by U.S. firms (Palmrose et al. 2004; Karpoff et al. 2008). In general, these negative returns are attributed to either 1) changes in investors' expectations of

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[22] Our study is related to a recent paper by Jia and Zhao (2017) that examines the contagion effect of restatements for foreign firms listed in the U.S. Our paper differs from their paper in two ways. First, we examine whether there are any systematic differences in stock market reactions to restatements between foreign firms and U.S. firms or between foreign firms' U.S.-shares and its home country-listed shares. Second, while they examine the effect of rule of law on the contagion effect, we examine whether the contagion effect varies with a broader set of country-level factors as well as firm-specific information quality.

firms' future cash flows (a numerator effect) due to direct effects, such as expected litigation costs, or to indirect effects such as increases in future contracting costs (Graham et al. 2008; Karpoff et al. 2008), or 2) changes in investors' assessments of the riskiness of these cash flows (a denominator effect) (Hribar and Jenkins 2004). This latter effect – an increase in restating firms' cost of capital – appears at least partially due to changes in the perceived quality of information in firms' financial reports. Specifically, prior research has found that restatements are followed by an increase in information risk (Kravet and Shevlin 2010) as well as lower credibility of earnings (Wilson 2008; Chen et al. 2014). Building on this line of research, we examine whether home country institutional features and/or firm-specific information frictions associated with foreign firms affect investors' reactions to foreign firms' restatements.

Our study also relates to the literature on foreign firms cross-listed in the U.S. One important phenomenon documented in this literature is called the 'home bias puzzle,' which refers to the finding that U.S. investors overweight their portfolios in favor of U.S. stocks over foreign stocks (French and Poterba 1991; Ahearne et al. 2004). The most compelling explanation for the bias is that it is the rational response to the information disadvantage that U.S. investors face relative to local investors in the foreign firm's home market (Van Nieuwerburgh and Veldkamp 2009).

One of the ways in which firms attempt to overcome the home bias is by listing their shares in the U.S. (either directly or through ADRs), thereby committing themselves to greater regulatory oversight and higher levels of disclosure in the U.S. However, there is mixed evidence on whether the listing requirements for foreign firms are successful at improving the disclosures of these firms. Prior research finds that compared to U.S. firms, foreign firms 1) have earnings that are of lower quality (Lang et al. 2006) and, 2) issue less frequent management earnings guidance (Hope et al.

2013).  In contrast, Lundholm et al. (2014) find that foreign firms write more readable MD&A and press releases than comparable U.S. firms.  More importantly, the prior literature does not address whether adverse events, like restatements, differentially affect investors' beliefs about firms' disclosure quality for foreign firms relative to U.S. firms.

### 3.2.2 Predictions

As discussed previously, restatements alter investors' assessments of restating firms' future cash flows and the risk associated with these cash flows, particularly risk stemming from information asymmetry (i.e., information risk).[23]  We hypothesize that the nature of this information risk can stem both from firm-specific factors as well as from country-specific factors.  Firm-specific factors that might influence investors' assessment of information risk include the credibility of managers (Karpoff et al. 2008), the corporate governance structure of the firm (DeFond et al. 2005), the strength of the firms' internal control system (Ashbaugh-Skaife et al. 2009), and the firms' commitment to transparent disclosures (Baginski and Rakow 2012).  In contrast, country-specific factors relate to institutional structures in a firm's home country that influence the reporting outcomes of the firm, such as the accounting, auditing and legal institutions in a country (Leuz et al. 2003; Hail and Leuz 2006).

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[23] We use the term "information risk" broadly to refer to the risks and uncertainties investors face from having imprecise or low quality information.  We recognize that there is considerable debate over whether such informational risks are non-diversifiable and therefore priced by the market.  Because there is some evidence that the negative returns around restatement announcements are due to a priced information risk factor (Kravet and Shevlin 2010), we discuss our predictions in terms of the effects on U.S. investors' perceptions of "information risk" from restatements made by foreign firms.  Moreover, Lambert et al. (2007) provide a model that links higher quality accounting information with a lower cost of capital through its effects on assessed covariances with other firms' cash flows (and thus does not rely on a separately priced "information risk" factor).  Thus, although we use the term "information risk", we do not take a stand on the mechanism through which the quality of a firm's accounting information influences its cost of capital (i.e., through a separately priced information risk factor or through its effects on beta) – only that it does have an effect (which is generally supported by prior research, i.e., Hribar and Jenkins 2004).

Although foreign firms listed in the U.S. are subject to similar regulatory reporting requirements as U.S. firms, it is likely that their home country institutional structures still significantly influence their reporting outcomes. Prior research has shown that cross-listed firms from weaker investor protection countries are associated with more earnings management (Lang et al. 2006), and are less likely to disclose internal control deficiencies (Gong et al. 2013) or restate earnings (Srinivasan et al. 2015), suggesting country-level factors continue to influence the financial reporting practices of U.S.- listed foreign firms. Thus, foreign firms' irregularities likely increase U.S. investors' uncertainty about the quality of the institutional structures in the restating firm's home country. In contrast, because the quality of U.S. institutions is well-known to U.S. investors, restatements made by U.S. firms are less likely to cause increases in investors' uncertainty about U.S. institutional structures. Thus, the information risk stemming from country-specific factors is likely to increase more for foreign firms relative to U.S. firms and, as a result, we expect a more negative stock market reaction among U.S. investors to an irregularity announcement made by a foreign firm relative to one made by a U.S. firm.

Likewise, for foreign firms that have listed shares in both the U.S. and their home countries, irregularity announcements are less likely to cause changes in investors' perception about the quality of the institutional features of the country among their home country investors. Thus, we expect a more negative stock market reaction to a foreign firm's restatement for its U.S. listed shares relative to its home country listed shares.[24]

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[24] This prediction assumes the U.S. market returns are driven by trading among U.S. investors while the home country returns are driven by trading among home country investors, an assumption supported by the literature (Baele et al. 2007; Chan et al. 2005; Strong and Xu 2003).

In addition, because much of foreign firms' sales and operations occur outside of the U.S., U.S. investors are likely less familiar with and have fewer sources of information about foreign firms. Thus, the credibility of a foreign firm's financial statements is likely to be of greater importance to U.S. investors. Prior research suggests that restatements cause investors to revise downward their beliefs about the credibility of financial reports (e.g., Wilson 2008) and, with greater weight placed on these statements by U.S. investors, one would expect a strong downward price reaction to restatements by foreign firms. In contrast, U.S. investors likely have greater familiarity with the operations of a U.S. company and have access to other sources of information beyond a firm's financial statement for these firms – as would the home country investors of foreign firms. For these reasons, our first set of hypotheses (stated in alternative form) is as follows:

H1a:  The US stock market reaction to restatement announcements is more negative for foreign firms cross-listed in the U.S. than for U.S. firms.

H1b:  The stock market reaction to restatement announcements made by foreign firms is more negative in its U.S.-listed shares relative to its home country-listed shares.

It is also possible that U.S. investors rationally anticipate restatements caused by country-specific factors and price the associated risk into the stock price of foreign firms prior to the announcement of restatements. In other words, it is possible that restatements do not cause differential revisions in beliefs about information risk for foreign firms relative to domestic firms (or relative to the foreign firm's home country investors). In this case, we would not expect to observe any systematic differences in stock market reactions to restatements between foreign firms

and U.S. firms or between foreign firms' U.S.-shares and its home country-listed shares.[25] Ultimately, any difference in market reactions is an empirical question.

Our first set of hypotheses argue that differences in market reaction to foreign firms' restatements relative to U.S. firms' restatements arise because: 1) U.S. investors' perceived uncertainty about the quality of the institutional structures in a foreign firm's home country increases more following a restatement, and/or 2) U.S. investors are less familiar with and have fewer sources of information about the operations of foreign firms, causing them to place greater weight on financial statement information. Thus, we would expect the stock market reaction to be more negative for: 1) foreign firms from countries with greater uncertainty about the institutional structures governing the financial reporting process, and 2) foreign firms that U.S. investors are less familiar with and for which they have fewer alternative sources of information. We explore these two possibilities further by examining cross-sectional differences in the stock market penalty to foreign firm restatements based on 1) country-level institutional factors, including accounting standard differences, auditing and accounting enforcement quality, and the rule of law tradition of the country, and 2) firm-level differences in investor familiarity and access to other sources of information using a firm's percentage of non-U.S. sales.

Our second hypothesis relates to the contagion effects of restatements. Accounting restatements not only convey information about the credibility of the restating firms' financial

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[25] In addition, H1a focuses on the "denominator effect" of restatements and does not consider differential "numerator effects" for foreign and U.S. firms. It is unclear whether foreign firms would differ from U.S. firms in direct or indirect effects of restatements on expected cash flows, and therefore, we do not make predictions based on such potential differences. However, we include several control variables to capture the severity of the restatement, which will likely affect investors' beliefs about future cash outflows associated with the restatement. Moreover, because H1b predicts a differential market reaction between the U.S.-listed shares and the home country-listed shares of the same foreign firm, differential cash flows (i.e., "numerator effects") cannot explain any documented differences.

statements, it can also motivate investors to reassess the credibility of financial statements issued by other firms subject to the same institutional features and business environment. To the extent non-restating firms share similar characteristics to the restating firm and these characteristics are viewed as contributing factors to the restatement, investors will similarly revise their assessments of the credibility of the non-restating firm. As a result, the non-restating firm will experience negative market reactions to the restating firm's announcement of a restatement. Gleason et al. (2008) provide evidence of such a "contagion effect" within industry groups, documenting a $-0.5\%$ abnormal return for non-restating firms over the three-day window around the restatement announcement of a firm from the same industry. Similarly, Kang (2008) find that reputational penalties due to financial fraud spill-over between firms connected by director interlocks.

We extend this line of research by examining the geographical contagion effect at the country level. To the extent the accounting restatement of a foreign firm is perceived to be (at least partially) attributable to country-specific factors, investors are likely to revise their beliefs about the credibility of the information reported by non-restating firms from the same country.[26] Therefore, we expect the negative stock returns of restating foreign firms to spill over to other cross-listed firms from the same country. Our next hypothesis is stated as follows:

H2: The stock prices of non-restating foreign firms from the same country will decline in response to restatements of foreign firms.

To the extent the contagion effects are the result of U.S. investors' perceived uncertainty about the quality of the institutional structures in a foreign firm's home country, we would expect

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[26] Again we acknowledge that if U.S. investors rationally price the associated country-specific information risk factors of non-restating foreign firm, then we would not expect to observe any spill-over effect of restating foreign firms' negative stock returns.

the contagion effect to be greater in countries where there is greater uncertainty about the institutional structures governing the financial reporting process. We explore this possibility further in Section 3.5.

### 3.3 Sample selection and variable definitions

### 3.3.1 Sample selection

We obtain our restatement sample from Audit Analytics, which covers all financial statement restatements disclosed by SEC registrants since 2001. Our restatement sample consists of restatements filed from 2001 through 2016 by foreign and U.S. firms listed on major U.S. exchanges (i.e., NYSE, NASDAQ, and AMEX). Our sample begins with 6,139 U.S. and 686 foreign restatements. We first delete OTC traded firms because foreign OTC firms do not need to register with the SEC and are thus not subject to the same disclosure requirement (Doidge et al. 2004; Srinivasan et al. 2015). We also restrict the sample to common shares (shrcd=10, 11, 12, 30, 31). This step yields 5,753 U.S. and 677 foreign restatement announcements.

Prior research suggests that it is important to distinguish between restatements stemming from intentional misstatements, referred to as "irregularities," and restatements stemming from unintentional misstatements, referred to as "errors" (Hennes et al. 2008) because errors have become increasingly common in recent years and often result in only small market reactions. Using the data provided in Audit Analytics, we classify a restatement as an irregularity if (1) the restatement is identified by Audit Analytics as due to financial fraud, irregularities and misrepresentations; or (2) the restatement involves an SEC investigation; or (3) the company discloses Board of Directors and/or Audit Committee's knowledge or involvement in the

restatement; or (4) the restatement involves the revenue account.[27,28] We identify irregularities

using these criteria among the 5,753 U.S. and 677 foreign restatement announcements and require

non-missing returns over the event window [0,1], resulting in a final sample of 2,325 U.S and 198

foreign irregularities from 32 different countries (shown in Table 3.1 Panel A).

For our analysis of the market reaction to accounting irregularities made by foreign firms

relative to U.S. firms (H1a), we gather financial statement data from Compustat and stock return

data from CRSP. For our analysis of the market reaction to foreign firms' restatements in their

U.S.-listed shares relative to their home country-listed shares (H1b), we get unadjusted closing

prices for the underlying stock at home and for its U.S.-listed counterpart from Thomson Reuter's

Datastream. For the analysis on geographic spillover effects (H2), we use all the non-restating

foreign firms with available data.

### 3.3.2   Foreign firm definition

We classify a firm as a foreign firm if its headquarter is located outside of the U.S. Desai

(2009) discusses three different notions of "home country" for a firm, which need not be the same

country. A firm's financial home is where it trades, meets disclosure requirements, and offers its

investors legal protection. For our sample of foreign firms listed in the U.S., the U.S. is at least

one of their financial homes. A firm's legal home is where it has tax obligations and is subject to

corporate law, and would typically be the country of incorporation. Finally, a firm's home for

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[27] Using a sample of firms alleged by the SEC to have manipulated earnings, Dechow et al. (2011) show that the revenue account is the most frequently manipulated account.

[28] One alternative approach to define an irregularity is to examine whether the restatement is disclosed through item 4.02 (non-reliance on previously issued financial statements) in Form 8-K. We do not adopt this approach because the disclosure requirement for item 4.02 only became effective in 2004. More importantly, this requirement does not apply to foreign firms that are not required to file Form 8-K.

managerial talent is where it has access to executive labor and where the firm's cultural identity is established. This is best captured by the country where the headquarters are located. Prior studies argue that using the country where the executives work and where the cultural identity is established better captures perceived home bias (Lundholm et al. 2014; Srinivasan et al. 2015). Therefore, we identify a firm's home country based on its historical headquarter location (HLOC in CRSP/Compustat Merged).[29]

### 3.4    Market Reactions to Irregularities (H1)

### 3.4.1    Comparison of market reaction to U.S. firms and foreign firms' irregularities

### 3.4.1.1    Research Design

Following Palmrose et al. (2004), we use a two-day announcement stock return window. To examine H1a, we estimate the following model using the U.S. and foreign irregularity samples, in which *CAR* is the cumulative two-day [0, 1] value-weighted market-adjusted returns centered around the restatement announcement date.[30]

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[29] Under Rule 405 of Regulation C of the Securities Act, companies that qualify as foreign private issuers (FPI) have different filing requirements than domestic companies, including the ability to report financial statements using their home country accounting standards along with a reconciliation to U.S. GAAP (on form 20-F). In contrast, firms that do not qualify as an FPI, must file financial statements using U.S. GAAP (on form 10-K). A foreign company qualifies as an FPI if: 1) more than 50% of its outstanding voting securities are held by non-U.S. residents, or 2) if more than 50% of its outstanding voting securities are held by U.S. residents, but none of the following three circumstances apply: a) the majority of its executive officers or directors are U.S. citizens or residents; b) more than 50% of the issuer's assets are located in the U.S.; or c) the issuer's business is administered principally in the U.S. Further, any company that is incorporated in a state, territory, or possession of the U.S. can never qualify as a FPI. Thus, our definition of foreign firms includes both FPIs and non-FPI firms whose headquarters are located outside the U.S. For example, Sohu.com Inc. is incorporated in Delaware and therefore, does not qualify as an FPI and the company must file 10-Ks with the SEC (rather than a 20-F). However, since the company's headquarter is located in Beijing, China, it is considered a foreign firm in our analysis. Despite the fact that non-FPI foreign firms have the same filing requirements as domestic U.S. firms (i.e., filing 10-Ks), we believe that investors will still experience greater uncertainty about the role of country-specific factors in the quality of financial information that is produced by these companies. In our analysis in Table 3, we have 73 FPI's and 97 non-FPI foreign firms.

[30] We employ this event window so that we can compare the magnitude of the market reaction to that documented in the prior literature. We tested the sensitivity of our results to using a three-day or five-day event window. Results are inferentially similar.

$$CAR[0,1]_{it} = \alpha_0 + \alpha_1 FF_{it} + \alpha_2 DURATION_{it} + \alpha_3 CHG\_NI_{it} + \alpha_4 NONQUANTIFY_{it} +$$

$$\alpha_5 NUM\_ACCT_{it} + \alpha_6 LOW\_VIS_{it} + \alpha_7 PCT\_II_{it} + \alpha_8 LEVERAGE_{it} + \alpha_9 BM_{it} + \alpha_{10} EP_{it} +$$

$$\alpha_{11} LOSS_{it} + \alpha_{12} CFO_{it} + \alpha_{13} ACC_{it} + \alpha_{14} INT\_COV_{it} + \alpha_{15} SIZE_{it} + \alpha_{16} BIG4_{it} + \alpha_{17} AGE_{it} +$$

$$\alpha_{18} EA_{it} + \alpha_{19} SURP_{it} + \sum_{t=1}^{16} \beta_t YEAR_t + \sum_{n=1}^{48} \theta_n IND_n + \varepsilon_{it} \qquad (3.1)$$

*FF* is an indicator variable equal to one if the firm's headquarter is located outside the U.S. and zero otherwise. H1a predicts a negative coefficient on *FF* (i.e., foreign firms experience a more negative stock market reaction to irregularity announcements).

We include four control variables to capture the magnitude and severity of the restatements. First, we include a control variable for the duration of the misstatement (*DURATION)*, defined as the gap between the restatement ending date and restatement beginning date, scaled by 365 days (both dates from Audit Analytics). We expect the market reaction to be more negative for restatements that are longer in duration. Second, we include a variable that captures the relative impact of the restatement on net income (*CHG_NI*), defined as restated net income less originally reported income (summed over all restated periods), scaled by lagged total assets reported at the year-end immediately prior to the announcement of the restatement (Palmrose et al. 2004; Feroz et al. 1991). We expect the coefficient on *CHG_NI* to be positive. Third, because some firms do not quantify the restatement amount, we include an indicator variable, *NONQUANTIFY* equal to one if the restating firm does not quantify the restatement amount (because *CHG_NI* is equal to 0 when a firm does not quantify the restatement *CHG_NI* is essentially the restatement amount x (1-*NONQUANTIFY*)). To the extent that failing to quantify the impact of a restatement is interpreted as bad news, we would expect a negative coefficient on *NONQUANTIFY*. Fourth, we capture the pervasiveness of the restatement within the financial statements by including a count of the number of accounting issues involved in the restatement based on the categories provided in Audit

Analytics (*NUM_ACCNT*). We expect the market reaction to be more negative for restatements that involve more accounting issues.

We also include a variable to capture the level of visibility of the restatement announcement. Prior studies on restatements show that disclosure prominence is significantly negatively associated with returns after controlling for restatement magnitude and other restatement characteristics (Files et al. 2009; Myers et al. 2013; Badertscher and Burks 2011; Srinivasan et al. 2015). Therefore, Equation (3.1) includes *LOW_VIS*, equal to 1 if the restatement was announced in a periodic report or its amendments, and equal to 0 if the restatement was announced in a more visible channel such as the 8-K, 8-K/A, 6-K, 6-K/A, and press releases. We expect the coefficient on *LOW_VIS* to be positive.

In addition, we include eleven variables to control for company characteristics that might affect market reactions to restatements. First, we control for the level of institutional ownership (*PCT_II*) because the presence of sophisticated investors potentially affects the speed with which news is incorporated in prices. Next, following Badertscher et al. (2011), we control for leverage (*LEVERAGE*), book-to-market (*BM*), the earnings to price ratio for positive earnings (*EP*), loss (*LOSS*), cash flows from operations (*CFO*), accruals (*ACC*), interest coverage ratio (*INT_COV*), firm size (*SIZE*), and the use of a big four auditor (*BIG4*). We also include the log of number of years the firm has been listed in a U.S. exchange (*AGE*), as investors are likely to have more confidence in a firm with a long listing history of trading on a U.S. stock exchange than a newly listed firm (Lang 1991). All variables are measured in the year immediately before the restatement

announcement quarter.[31] We also control for earnings announcements that are made concurrently with the restatement announcement. Specifically, *EA* is an indicator variable equal to one if the restatement occurs within a three-day window of the earnings announcement, and *SURP* is the amount of unexpected earnings for these concurrent earnings announcements (and zero if not concurrent). Finally, we include year fixed effects to control for worldwide events in a year that might influence all firms the same way, as well as industry fixed effects to control for industry-specific factors that may affect the characteristics of restatements and the market reaction to them. The definitions of these variables are provided in Appendix B. We cluster standard errors by industry-year.

An alternative to directly controlling for the systematic differences between U.S. and foreign restatements is entropy balancing. Entropy balancing is a matching technique that identifies weights for each control sample observation to equalize the distributions of underlying variables across treatment and control samples (McMullin and Schonberger 2016). Relative to propensity score matching, entropy matching has the advantage of controlling for variance in variables and ensures that higher-order moments of the covariate distribution are similar across treated and control samples.

We use entropy balancing to identify weights for each of the observations in the control sample (U.S. irregularities) such that the mean and variance of the variable distribution of the weighted control sample are nearly identical to those of the treatment sample (foreign

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[31] Badertscher et al. (2011) do not control for the existence of a loss in the previous quarter. However, a loss in the year immediately before the restatement announcement causes the *EP* ratio to be negative, which is not economically meaningful. Therefore, we include a *LOSS* indicator variable and define *EP* as the earnings to price ratio for positive earnings, and zero otherwise.

irregularities). We select all the restatement and firm characteristics previously described as covariates and set the tolerance level at 0.015.

### 3.4.1.2    Descriptive statistics

The descriptive statistics are provided in Table 3.2.  The mean abnormal return for foreign restatements that involve irregularities is –5%, which is significantly higher than the mean market reaction for U.S. firms (–3%).[32] Although this result does not yet control for many confounding variables, they are nonetheless consistent with our first hypothesis, that the stock market penalizes foreign firms more than U.S. firms for irregularities.

Table 3.2 also provides the descriptive statistics for the control variables. Foreign irregularities have shorter average durations (2.11 years) than U.S. irregularities (2.45 years).  In addition, foreign firms are less likely than U.S. firms to use prominent disclosure mediums such as press releases (*LOW_VIS* is 0.40 for foreign firms, significantly higher than 0.29 for U.S. firms). Turning to firm characteristics, an average foreign restating firm has a lower percent of institutional investors, a higher book-to-market ratio, higher earnings to price ratio, higher accruals, tends to be larger, are less likely to use Big 4 auditors and are younger than U.S. firms. This evidence suggests that it is important to control for restatement severity, disclosure prominence, as well as firm characteristics.

### 3.4.1.3    Results

Table 3.2 presents the regression results for Equation (3.1). Column (1) and (2) report the results before and after entropy balancing, respectively.  The overall results in these two columns

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[32] Our average announcement period return is significantly smaller than that reported in Palmrose et al. (2004) (mean return of −9%).  This smaller return is consistent with evidence reported in Scholz (2008), which documents a decline in the market reaction to restatements in recent years (and an increase in the number of restatements).

are very similar: the coefficient on *FF* are significantly negative in both Columns: $-0.0248$ and $-0.0219$, respectively, both significant at the 5% level (two-tailed), consistent with H1a. The results suggest the average abnormal stock return associated with foreign irregularities is 2.48% (2.19%) lower than that for U.S. irregularities. The evidence provides support for H1a that the U.S. stock market penalizes foreign firms more than U.S. firms for accounting irregularities. The results for control variables are generally consistent with our prediction and the prior literature (Palmrose et al. 2004).

### 3.4.2 Comparison of foreign and domestic investor responses to foreign firms' irregularities

Our prior analysis compares the reaction of the same group of investors (i.e., U.S. investors) to restatements of U.S. firms versus those of foreign firms. One concern with this analysis is that foreign firms experiencing restatements are different from U.S. firms experiencing restatements on some dimension and that the characteristics of the restatements of foreign firms are different from those of U.S. firms. We include numerous control variables and use entropy balancing in attempt to address these concerns; however the possibility of unidentified firm or restatement characteristics remains.

To further address this concern, we next examine the market reactions to the *same restatement* by a foreign firm's home country investors relative to its U.S. investors. To the extent that investors react more negatively to foreign restatements due to unfamiliarity with the foreign country's institutional features, we expect a sharper price decline in response to the same restatements in a foreign firms' U.S. listed shares relative to its home country-listed shares (H1b). Arguably, this design is a cleaner setting because the underlying firm fundamentals as well as restatement characteristics are held constant. As such, the differential market reaction should

primarily reflect differential revisions in perceived information risk by U.S. and home-country investors. The disadvantage of this analysis is not all foreign firms have dual-listed shares (i.e., some foreign firms are only listed on a U.S. exchange) and, as result, the sample size for this analysis is small.[33]

### 3.4.2.1 Research Design

We first identify within our foreign irregularities, a subsample of 53 firms that are traded on both a U.S. exchange and their home country exchange. Next, for each of the 53 foreign firms, we get unadjusted closing prices for the underlying stock on the home country exchange as well as for its cross-listed counterpart in the U.S. from Thomson Reuter's Datastream around the restatement announcement dates. We use the home market stock price that are translated into U.S. dollars by Datastream at the end of each trading day using the appropriate exchange rates. For each restatement, we calculate returns for both its U.S. listed shares as well as its home country shares over three different windows: [0, 1], [1, 2], and [0, 2], where day 0 is the restatement announcement date in the U.S. As Canadian Stocks trade synchronously with their U.S. counterparts and Latin American stock markets differ by at most two hours from the U.S. market, we assume that closing prices on the same day in the home market are based on similar information sets as closing prices in the U.S. market for these countries. For the remaining countries, trading in the stock markets leads the U.S. market with at most two hours of overlap (London) or no overlap. Thus, we assume the closing prices on the following day in the home market match the closing prices in the

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[33] Notably, there are no firms from China in this analysis as none of the Chinese firms in our sample are listed on a Chinese exchange.

U.S.[34] We expect the U.S. market reaction to irregularity announcements to be more negative than the market reaction in the home country (H1b).

### 3.4.2.2 Results

Table 3.4 reports the results of our analysis of the market reaction to foreign firms' irregularities in its U.S. listed shares and its home country-listed shares for the subsample of 53 foreign firms that are traded on both a U.S. and home country exchange. The average foreign firm experiences a -5.25%, –3.06%, and –4.96% market return over the event windows: [0, 1], [1, 2], and [0, 2] in the U.S. market, respectively. In contrast, their average home market reactions during the same event windows are smaller (–1.93% over the window [1, 2] and –4.16% over the window [0, 2], respectively), although the differences (–1.13% and –0.80%, respectively) are economically and statistically significant.

Note that we examine two groups of investors' differential reaction to the same underlying restatement. If our assumption that the shares listed in the U.S. (home country) are mostly traded by U.S. (domestic) investors is valid, the evidence presented in this table supports our thesis that accounting irregularities increase investors' uncertainty about information risk more when investors are unfamiliar with the institutional features of the firm's home country and have less access to alternative sources of information.

### 3.4.3 Cross-sectional analysis of market reaction to irregularities

As discussed in Section 3.2.2., differences in market reaction to foreign firms' restatements

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[34] For example, if a Canadian firm announced a restatement on March 29, 2002, the [0, 1] CAR would be measured from March 29th to 30th for both the U.S. listed shares and the Canadian listed shares. The same restatement by a Japanese firm would be measured from March 29th to 30th for the U.S. listed share but from March 30th to 31st for the Japanese listed shares.

could arise because of uncertainty about the institutional structures in a given country or because of limited familiarity with and access to information about foreign firms. In this section, we explore these possibilities by examining cross-sectional variation in the market reaction to irregularities.

### 3.4.3.1 Research design

We explore three country-level factors that potentially impact investors' reactions to restatements: accounting rule differences, audit and enforcement quality, and the rule of law tradition of the country. We define these variables relative to the U.S. and thus refer to them in terms of their difference or distance from the U.S.

*GAAPdiff* captures differences between a country's local GAAP and U.S. GAAP, based on a comparison of 52 accounting rules conducted by Bae et al. (2008). Two rules are considered the same if they both comply with IFRS, or, if neither complies with IFRS. If one country conforms to IFRS and the other country does not, then the two countries are deemed to have different GAAP for that item.[35]

*AUD_ENFdiff* uses the index score from Brown et al. (2014) to captures differences in the quality of public company auditors (e.g., license requirements, ongoing professional development, requirements for audit firm/partner rotation) and accounting enforcement activity by independent

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[35] This measure was constructed based on a country's accounting standards in 2001 and since then, many countries have adopted IFRS. Thus, we modify the Bae et al. (2008) definition by changing the local accounting standards to IFRS after each country's first IFRS annual report date. We obtain the first IFRS annual report dates for the 65 countries that adopted IFRS between 2001 and 2013 from Table 4 of De George et al. (2016). For example, the first IFRS annual report date in Canada is Dec 31, 2011. Prior to 2011, we compare Canadian GAAP and US GAAP (by indirectly comparing each of them with IFRS) and find that the two standards differ for 3 out of the 52 accounting rules examined. As a result, *GAAPdiff* is 3 for Canada up until 2011. Starting from 2011, Canadian firms adopted IFRS, which differed from US GAAP on 4 of the 52 accounting standards. Accordingly, the *GAAPdiff* measure for firms headquartered in Canada is 4 for the years 2012 and onwards.

enforcement bodies (e.g., monitoring of financial statements by security regulators) in the U.S. relative to other countries. The index score is scaled from 0 to 56 with lower scores representing a lower quality auditing environment and less active regulatory oversight over financial reporting.[36]

*LAWdiff* is based on each country's rule of law index from the Worldwide Governance Indicators created by the World Bank. The rule of law index is generally regarded as a summary measure of compliance with laws and regulations, which captures country-level institutional features related to investor protection or the extent of self-dealing behavior (Srinivasan et al. 2015).[37] This index ranges from approximately -2.5 to 2.5, with higher scores indicating stronger rule of law.

To test whether market reactions to foreign firms' restatements vary cross-sectionally with country-level institutional features, we estimate the following model:

$$CAR[0,1]_{it} = \alpha_0 + \alpha_1 FF_{it} + \alpha_2 DISTANCE_{it} + \alpha_3 DURATION_{it} + \alpha_4 CHG\_NI_{it} +$$
$$\alpha_5 NONQUANTIFY_{it} + \alpha_6 NUM\_ACCNT_{it} + \alpha_7 LOW\_VIS_{it} + \alpha_8 PCT\_II_{it} + \alpha_9 LEVERAGE_{it} +$$
$$\alpha_{10} BM_{it} + \alpha_{11} EP_{it} + \alpha_{12} LOSS_{it} + \alpha_{13} CFO_{it} + \alpha_{14} ACC_{it} + \alpha_{15} INT\_COV_{it} + \alpha_{16} SIZE_{it} +$$
$$\alpha_{17} BIG4_{it} + \alpha_{18} AGE_{it} + \alpha_{19} EA_{it} + \alpha_{20} SURP_{it} + \sum_{t=1}^{16} \beta_t YEAR_t + \sum_{n=1}^{48} \theta_n IND_n + \varepsilon_{it} \quad (3.2)$$

*FF* captures the incremental reaction to foreign irregularities relative to U.S. irregularities.

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[36] One advantage of this measure is that it captures time series variations in accounting enforcement over time. An index measure for 51 countries for each of the years 2002, 2005 and 2008 were created using data provided by the International Federation of Accountants (IFAC), the World Bank and the national securities regulators. We assign the index measured as of 2002 to all restatements filed before 2005, the index measured as of 2005 to restatements filed from 2005 through 2007, and the index measured as of 2008 to restatements filed from 2008 onwards. For parsimony, we report the three-year average (2002, 2005, and 2008) for each country in Table 1 Panel B.

[37] Specifically, the rule of law index measures the extent to which "agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence" (Kaufmann et al. 2009). The World Bank web site provides annual data on the rule of law measure for more than 200 countries around the world for the years 1996, 1998, 2000, 2002 and then annual data after that, up to 2015. We assign the rule of law index measured as of 2000 to irregularities filed in 2001 and the rule of law index value as of 2015 to irregularities filed in 2016. The index dataset can be downloaded from the following website: http://info.worldbank.org/governance/wgi/index.aspx#home.

*DISTANCE* is one of the three country-level measures described above and captures within foreign firm variation in their two-day market reactions. We expect the coefficient on *DISTANCE* to be negative if the stock market reaction to the irregularity announcement is more negative when the firm is further away from the U.S. in accounting standards, auditing and enforcement, or rule of law.

In estimating equation (3.2), we include all observations in this analysis, including U.S. firms, to more efficiently estimate the effect of the control variables on the market reaction to a restatement announcement (e.g., the effect of the magnitude and duration of the restatement). By definition *DISTANCE* is 0 for the U.S. sample. While we are interested in cross-country variation among foreign firms, significant loading on *DISTANCE* might be driven by U.S. firms being different than an average foreign firm. To address this concern, we modify our country-level distance measures to isolate the main effect of being a foreign firm versus a U.S. firm. Specifically, we subtract the grand mean of the distance variables for the foreign sample so that the mean distance of a foreign firm is zero, the same value as the distance variables for the U.S. sample. We estimate Equation (3.2) using the modified distance variables.

Second, we explore whether the cross-sectional variation in market reactions to foreign firm irregularity announcements is due to investor familiarity with and/or access to alternative sources of information about a firm's operations. If a foreign firm sells products in the U.S., U.S. investors likely perceive less information asymmetry because the product market interaction can reduce the cost of acquiring information about the firm's business operations and financial conditions (Lundholm et al. 2014). Accordingly, we create a measure based on the location of the firm's sales. For each firm with the necessary geographic segment sales data, we define *SALESDIST* as the percentage of sales made outside the U.S. We measure this variable both for

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foreign firms as well as for U.S. firms. U.S. firms also vary in terms of the proportion of their sales that are generated outside the U.S. and, arguably, a U.S. firm whose product market is largely in foreign countries would face similar perceived information asymmetries when reporting an irregularity.

To test the effect of *SALESDIST* on the market reaction to irregularity announcements, we modify Equation (3.2) in two ways. First, we replace *FF* and *DISTANCE* (the country-level distance variable) with *SALESDIST*. Second, we include country fixed effects to examine whether, within a given country, market reaction to restatements varies with the extent of the restating firm's non-U.S. sales.

### 3.4.3.2    Descriptive statistics

Panel B of Table 3.1 reports the means of our institutional distance variables by country. Canada and South Africa have accounting standards that are the closest to U.S. GAAP (*GAAPdiff* =3), while Russia has the greatest distance on this dimension (*GAAPdiff* =21). Note that while we modify this measure to reflect IFRS adoptions where applicable, for brevity, we only report the pre-IFRS period *GAAPdiff*.

For *AUD_ENFdiff,* the U.S. has the highest raw score and thus, the differences between the U.S. and all other countries are positive. Higher values of *AUD_ENFdiff* represent lower quality auditing and enforcement. Overall, Argentina is the furthest away from the U.S. in accounting enforcement (*AUD_ENFdiff* =41), while Canada has similar enforcement to the U.S. (*AUD_ENFdiff* =0).

*LAWdiff* (the U.S.'s rule of law index minus each country's rule of law index) is sometimes negative, indicating that these countries have a stronger rule of law tradition than the U.S., with Norway having the strongest rule of law (*LAWdiff* = – 0.37). Therefore, for *LAWdiff*, the regression

analysis examines whether stock market reactions to restatement announcements are more negative for foreign firms from countries with weaker rule of law (i.e., we do not use the absolute value of the distance from the U.S. as our measure of rule of law distance).

Table 3.2 reports the *SALESDIST* for foreign and U.S. firms. Unsurprisingly, foreign firms have a significantly higher percent of sales that occur outside the U.S. (73 percent) relative to U.S. firms (23 percent).

### 3.4.3.3    Results

Table 3.5 presents the regression results for Equation (3.2), examining the effect of country-level variables on the stock market reactions to restatement announcements. The results without and with entropy balancing are very similar. For parsimony, we only report results using entropy balancing. Recall that *DISTANCE* is 0 by construction for the U.S. sample, and is mean-adjusted to 0 for the foreign sample. Thus, the coefficient on *FF* in these regressions captures the average effect of being in the foreign sample versus the U.S. sample, and the coefficient on *DISTANCE* captures the incremental impact of the country-level distance variables. We find significant negative coefficients on *FF* across all three columns, similar to the result in Table 3.3: the average reaction to a foreign firm's irregularity is more negative than to a U.S. firm's irregularity. However, none of the three distances appears to influence the market reaction to foreign irregularities. Thus, we find no evidence that the differential market reaction to irregularity announcements by foreign firms relative to U.S. firms is driven by investors' concern over differences in accounting principles, auditing and enforcement quality, or rule of law tradition.

However, in an untabulated analysis, we run a regression within the foreign firm sample including country fixed effects, all the control variables, as well as industry and year fixed effects. We then perform an F-test for the joint significance of the country fixed effects and find

significance at less than one percent (F-statistic = 3.02). Thus, we can reject the null hypothesis of no country-level fixed effect on stock market reactions to irregularities, suggesting that country-level factors play a role in U.S. investors' reactions; however, our three country-level distance variables are either poor proxies for the underlying constructs or a separate factor explains these country-level differences.[38]

Columns (1) and (2) of Table 3.6 reports the regression results of our cross-sectional analysis using *SALESDIST*, without and with country fixed effects, respectively. We find significant negative coefficients on *SALESDIST* in both specifications. The result in Column (2) suggests that, within a country, the fewer sales a firm makes in the U.S. the more negative their U.S investors react to their restatement announcements.

One difficulty with our *SALESDIST* measure is that the data requirement for geographic segment sales reduces the sample to 1,687 observations. As an alternative, we code any firm without geographic segment data as having all its sales in their home country (i.e., we assign firms headquartered in the U.S. *SALESDIST* = 0 and firms headquartered outside of U.S. *SALESDIST* = 1). Columns (3) and (4) report the results of this analysis. We find similar, albeit slightly weaker, results. Collectively, the evidence in this table is consistent with the notion that when U.S. investors are less familiar with and have less information about the operations of a firm, the announcement of an irregularity results in greater increases in information risk. Therefore, their

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[38] In a separate regression, we include the U.S. irregularity sample but exclude the U.S. country dummy variable so each country's fixed effect can be considered as relative to the U.S. The five countries with the highest fixed effects (i.e., the least negative market reaction relative to the U.S.) are British Virgin Islands, Japan, South Korea, Switzerland and Norway. The five countries with the lowest fixed effect (i.e., most negative market reaction) are Ireland, India, Australia, Indonesia, and Israel.

reaction to irregularity announcements is more negative when investors are less familiar with the product and/or service of the restating firm.

## 3.5    Contagion Effects: Consequences for Non-restating Firms (H2)

### 3.5.1    Research Design

H2 predicts contagion effects of foreign restatements to non-restating foreign firms from the same country. Although we find little evidence that the market reaction to irregularities for foreign firms varies with country-specific institutional features, it is possible investors perceive a country-specific commonality in irregularities (that we have not identified) and that would lead to country-level contagion. We begin by examining the average short-window stock returns of non-restating firms. We expect a significant negative reaction to non-restating firms from the same country around the restatement announcement. We then examine whether our country-level *DISTANCE* variables explain the variation in the contagion effects. We estimate the following model:

$$CAR_{it}[0,5] = \alpha_0 + \alpha_1 DISTANCE_{it} + \alpha_2 PCT\_II_{it} + \alpha_3 AGE_{it} + \alpha_4 LEVERAGE_{it} + \alpha_5 BM_{it}$$
$$+ \alpha_6 EP_{it} + \alpha_7 LOSS_{it} + \alpha_8 INT\_COV_{it} + \alpha_9 SIZE_{it} + \alpha_{10} SAME\_IND_{it}$$
$$+ \alpha_{11} CAR\_REST_{it} + \alpha_{12} HMKT\_RET_{it} + \sum_{m=1}^{48} \beta_t INDUSTRY_m$$
$$+ \sum_{t=1}^{16} \beta_t YEAR_t + \varepsilon_{it} \qquad\qquad (3.3)$$

The dependent variable is the cumulative abnormal return for a non-restating firm, measured over the event windows [0, 1] and [0, 5] around the restatement announcement of another firm from the same country.

Our variable of interest, *DISTANCE*, is one of our three distance measures: *GAAPdiff, AUD_ENFdiff, LAWdiff.* In this analysis, we include the percent of institutional investors (*PCT_II*)

because sophisticated investors may be more prone to seeing the underlying cause of a restatement as due to some pervasive issue in the country. We also control for firm listing history in the U.S. (*AGE*). Similar to Gleason et al. (2008), we include the following firm characteristics as control variables in Equation (3.3): the debt-to-assets ratio (*LEVERAGE*), book-to-market ratio (*BM*), earnings-price ratio (*EP*), loss (*LOSS*), interest coverage ratio (*INT_COV*), and firm size as measured by the log of total assets (*SIZE*). Because Gleason et al. (2008) document an industry contagion effect, we also include *SAME_IND* to ensure that geographical contagion effects do not merely capture industry spillovers that might be due to industries concentrating within specific countries. *SAME_IND* is an indicator variable, equal to one if the restating firm comes from the same Fama and French 48 industry as the non-restating firm. *CAR_REST* is the cumulative abnormal return over the event window [0, 1] or [0, 5] of the restating firms. We use *CAR_REST* as a summary measure to control for the severity and disclosure prominence of the restatements. In addition, we include home country market return (*HMKT_RET*) over the same event window as the dependent variable ([0, 1] or [0, 5]) to control for home country market-wide news during the event window. Finally, we include year as well as industry fixed effects to control for industry-level, year-specific events that may affect all firms in that industry or year the same way. To control for correlated errors, we cluster our standard errors by restatement event. All variables are defined in Appendix B.

Next, we examine whether the contagion effects of accounting restatements vary with firm-specific differences in accruals quality. This analysis provides further evidence as to whether irregularities cause investors to reassess the earnings quality of non-restating firms from the same country. We measure accruals quality (*AQ*) based on Dechow and Dichev (2002) and Francis et al. (2005). The calculation of *AQ* is described in Appendix B. This measure of accrual quality

captures the extent to which accruals map into cash flow realizations, where a poor match is reflected in greater volatility in residuals from year to year and signifies low accruals quality. Since *AQ* is an inverse measure of accruals quality, we expect the coefficient on *AQ* to be negative, consistent with the prediction that the negative stock price contagion effect will be greater for firms with lower accruals quality (i.e., higher *AQ* values). We estimate this equation including country-year fixed effects to examine whether variation in earnings quality, within a given country-year, is associated with the contagion effect. Similar to estimating Equation (3.3), we cluster standard errors by restatement events.

### 3.5.2    Results for the contagion effect

Table 3.7 reports descriptive statistics on abnormal stock returns of foreign restating firms and non-restating firms from the same country.[39]  We examine four stock return event windows from a two-day window to an eleven-day window.  We note that the announcement period return may be positive for some firms in our sample because other events or announcements might occur in conjunction with the restatement announcement (e.g., an earnings announcement).  To reduce the effect of these potentially confounding events, we follow Gleason et al. (2008) and keep only irregularities with negative two-day announcement abnormal returns (129 restatements) for our contagion analyses. Not surprisingly, Table 3.7 shows much greater negative stock price reactions to restating firms, up to -10.28% over the 11-day return window.  Turning to the contagion effect, the non-restating firms from the same country show a statistically significant cumulative abnormal

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[39] There are 3 cases where two restatements from the same country were announced on the same day. Including both restatements biases the contagion effect because of the inclusion of the other restating firm as a non-restating firm (subject to contagion) as well as overstating the number of independent observations (because non-restating firms would be included more than once). Therefore, we keep the restatement with the greater restatement magnitude (*CHG_NI*) and exclude the other restating firm from the analysis.

return of -0.445% over the two-day return window surrounding the restatement announcement. This stock price decline for non-restating firms continues until day +5 (-0.589%). These results suggest that the announcement of a discovered accounting misstatement by one firm in the country triggers information contagion among peer firms from the same country (H2). [40] Thus, it appears investors perceive a country-specific factor associated with accounting irregularities.

Next we examine whether the above-documented contagion effect varies by country-level factors. The results from estimating Equation (3.3) are shown in Table 3.8. For parsimony, we only report the results for CAR [0, 5], because Table 3.6 shows that the contagion effect is strongest over this event window. Across all three columns, we notice that the coefficients on the distance variables are negative, but statistically insignificant. Overall, there is little evidence suggesting that the contagion effect of irregularities varies with our country-level variables.

Finally, we examine whether within a country in a given year, the geographic contagion effect documented above is concentrated in firms that demonstrate lower earnings quality. A higher *AQ* indicates lower accruals quality. We estimate a regression similar to Equation (3.3) but replacing *DISTANCE* with *AQ*. The results for both event windows [0, 1] and [0, 5] are given in Table 3.9. For both windows, the first column employs the specification in Equation (3.3), while the next column adds country-year fixed effects. The results indicate an overall negative association between accruals quality and the contagion effect. In particular, the results in column (4) suggest that within a country in a particular year, non-restating firms with lower earnings quality (higher *AQ*) experience greater negative market reactions around an irregularity

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[40] In a robustness check, we separately examine stock returns for same-country peer firms from the same or different industries. We find a significant negative market reaction to peer firms from different industries, suggesting that our results are not driven by the industry-level contagion effect documented in Gleason et al. (2008).

announcement by another firm from the same country. By including country-year fixed effects, we ensure that our results on *AQ* are not driven by the fact that certain country-years tend to have higher earnings quality than other country-years.

Taken together, the above results are consistent with our prediction (H2) that negative stock returns around foreign restatement announcements spill over to non-restating firms from the same country. This result suggests investors perceive a country-specific factor associated with irregularities; however, it does not appear to be related to the institutional features we have identified. Nevertheless, within a country-year, the spill-over effect is strongest among firms with low earnings quality, suggesting the spill-over is related to information quality.

## 3.6 Sensitivity analysis

We also conduct a number of other sensitivity analyses to address possible alternative explanations for our main findings. First, it is likely that the information environment of U.S. firms is richer than that of foreign firms, resulting in more related news leading up to the restatement announcement for U.S. firms. As a result, the probability that the market reaction to the restatement might be preempted is greater for U.S. firms than foreign firms. As a robustness check, we use the preannouncement CAR as the dependent variable (day [-5,-1]), we find no significant difference between market reaction to foreign and U.S. irregularities.

Second, as discussed in Section 3.3, to the extent that a firm's headquarters is not a good representation of a company's true "home" our classification of foreign firms will contain measurement error. To address this concern, we examine the robustness of our results in Table 3.3 by eliminating the largest ten percent of all foreign restating firms in the pooled sample, under the assumption that the largest foreign firms are more likely to be multinational corporations with

an ambiguous home country. In untabulated results, the coefficients on *FF* in Table 3.3 are slightly larger (-2.52%) than that reported in Table 3.3 (-2.48%).

Third, the foreign country with the largest number of irregularities is Canada (44 irregularities per Table 3.1 Panel A). To ensure that our results are not driven by Canadian firms, we examine the robustness of our results by excluding Canadian irregularities from our analysis. The results of our main tests (Tables 3.3-3.8) are inferentially similar. In addition, the SEC issued a warning to investors against investing in any foreign firms coming through reverse mergers (Chinese firms in particular) in June 2011.[41] To the extent this announcement caused investors to revise their beliefs about the information risk associated with these firms, the reaction to restatement announcements occurring after this announcement are likely to be less severe. We find that the results of our main tests (Tables 3.3-3.8) remain inferentially similar.[42]

Finally, we also consider the possibility that the greater negative price reaction to foreign irregularities is due to investor over-reaction. Specifically, we investigate whether there is a future price reversal of the initial negative return to the restatement announcement. We re-estimate Equation (3.1), replacing our two day CAR [0,+1] with CARs measured over two different windows subsequent to the restatement announcement: CAR [t+2, t+121] and CAR[t+2, t+181]. We choose these two windows to capture either the subsequent earnings announcement or the subsequent two earnings announcements. We find that, across both future return windows, the coefficient on *FF* is statistically insignificant (t-statistics are 0.14 and -0.31 respectively, for the

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[41] See https://www.sec.gov/news/press/2011/2011-123.htm and https://www.sec.gov/investor/alerts/reversemergers.pdf.

[42] We identified Chinese reverse merger companies from this website: http://www.bloomberg.com/news/articles/2011-06-22/table-of-chinese-reverse-merger-companies-listed-on-u-s-stock-exchanges.

120 and 180-day future return window). Overall, the evidence is inconsistent with the notion that investor reactions to foreign firms' irregularity announcements are irrational.

## 3.7 Conclusion

In this paper, we examine the stock market consequences of accounting irregularities for U.S. listed foreign firms. We first show that holding the U.S. investors constant, foreign irregularities trigger greater negative market reaction than U.S. irregularities. We obtain this result after controlling for the restatement-specific and other firm characteristics. We then hold the underlying irregularity constant, and show corroborating evidence that the restating firms' U.S. investors react more negatively to the same restatement than their home-country investor counterparts. Taken together, the evidence from these two tests provides convincing evidence that investors react more unfavorably to irregularities made by foreign firms. However, we do not find that the market reactions vary with proxies for country-level institutional features that might be thought to increase information risk. Rather, we find that the market reaction varies with a firm-specific measure of information frictions, the percent of non-U.S. sales, which are higher for foreign firms.

Despite the fact that we do not find variation in market reactions with country-level institutional features, we still find that non-restating firms from the same country experience significant stock price declines following restatements, after controlling for the industry contagion effect documented in Gleason et al. (2008). This country-level contagion suggests investors perceive a common country-specific factor associated with irregularities; however, this factor does not appear to be any of the country-level institutional features we examine. Nevertheless, within a country, the contagion effect is strongest among firms with low earnings quality, suggesting that

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the contagion effect is related to the quality of information from non-restating firms in the same country.

Overall, our findings suggest that the credibility of financial reporting is particularly important for foreign firms listed in the U.S. In the event of an irregularity, it appears that the perceived information risk increases more for foreign firms relative to U.S. firms, likely due to information frictions that are greater amongst foreign firms. While we do not find evidence that the differential market reaction to foreign firms' irregularity announcements is related to country-specific institutional features, it is possible that measurement error in our proxies prevent us from finding such an effect. The fact that we find a country-specific contagion effect suggests that investors perceive a country-specific commonality to irregularities. The source of this perceived commonality remains a puzzle that we leave for future research.

## Table 3.1 Descriptive Statistics by Country

Panel A of this table reports the distribution of irregularities by country and year. Panel B of this table reports the average value of the three distance measures for each of the 33 countries in our sample.

Panel A: Distribution of Irregularities

| Country | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | Total |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Argentina | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 |
| Australia | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 2 |
| Bermuda | 1 | 1 | 0 | 1 | 2 | 3 | 2 | 2 | 0 | 1 | 1 | 0 | 2 | 0 | 0 | 1 | 17 |
| Brazil | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 4 |
| British Virgin Islands | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 3 |
| Canada | 0 | 1 | 2 | 5 | 5 | 8 | 5 | 3 | 2 | 1 | 1 | 1 | 4 | 1 | 3 | 2 | 44 |
| China | 1 | 0 | 0 | 0 | 0 | 4 | 3 | 3 | 3 | 14 | 10 | 4 | 7 | 3 | 1 | 2 | 55 |
| Colombia | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 2 |
| France | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 2 |
| Germany | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| Hong Kong | 0 | 0 | 1 | 0 | 2 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 2 | 7 |
| Iceland | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| India | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| Indonesia | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Ireland | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 3 |
| Israel | 0 | 1 | 0 | 1 | 2 | 4 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 12 |
| Italy | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Japan | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 3 |
| Kazakhstan | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 3 |
| Luxembourg | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 | 0 | 2 |
| Netherlands | 0 | 0 | 0 | 2 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 2 | 0 | 6 |
| Netherlands Antilles | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 2 |
| Norway | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Puerto Rico | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| Russia | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| South Africa | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |

| | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| South Korea | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 2 |
| Spain | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| Switzerland | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Taiwan | 0 | 0 | 0 | 0 | 2 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 4 |
| United Kingdom | 0 | 1 | 0 | 1 | 3 | 1 | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 8 |
| Venezuela | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Total | 2 | 6 | 8 | 12 | 22 | 27 | 14 | 12 | 9 | 18 | 15 | 6 | 17 | 9 | 9 | 12 | 198 |
| United States of America | 84 | 87 | 105 | 174 | 444 | 374 | 226 | 131 | 104 | 85 | 85 | 96 | 107 | 74 | 75 | 74 | 2,325 |

**Table 3.1 (continued)**

Panel B: Distance Variables

| Country | GAAPdiff | AUD_ENFdiff | LAWdiff |
|---------|----------|-------------|---------|
| Argentina | 13 | 41 | 2.26 |
| Australia | 9 | 2 | -0.21 |
| Bermuda | NA | NA | 0.59 |
| Brazil | 13 | 30 | 1.82 |
| British Virgin Islands | NA | NA | NA |
| Canada | 3 | 0 | -0.20 |
| China | 13 | 15 | 1.98 |
| Colombia | NA | NA | 2.08 |
| France | 6 | 7 | 0.15 |
| Germany | 11 | 15 | -0.11 |
| Hong Kong | 8 | 14 | 0.04 |
| Iceland | NA | NA | -0.22 |
| India | 12 | 32 | 1.54 |
| Indonesia | 9 | 36 | 2.22 |
| Ireland | 4 | 18 | -0.11 |
| Israel | 8 | 12 | 0.62 |
| Italy | 14 | 8 | 1.12 |
| Japan | 8 | 21 | 0.23 |
| Kazakhstan | NA | NA | 2.33 |
| Luxembourg | 18 | NA | -0.25 |
| Netherlands | 8 | 24 | -0.23 |
| Netherlands Antilles | NA | NA | 0.69 |
| Norway | 6 | 9 | -0.37 |
| Puerto Rico | NA | NA | 0.82 |
| Russia | 21 | 25 | 2.41 |
| South Africa | 3 | 26 | 1.47 |
| South Korea | 6 | 28 | 0.64 |
| Spain | 13 | 17 | 0.46 |
| Switzerland | 20 | 6 | -0.30 |
| Taiwan | 11 | 32 | 0.60 |
| United Kingdom | 4 | 3 | -0.13 |
| Venezuela | 13 | NA | 3.11 |
| United States of America | 0 | 0 | 0 |

**Table 3.2 Descriptive Statistics for Regression Variables**

This table reports the descriptive statistics on all the variables used in stock market reaction tests for the foreign and the U.S restating sample, respectively. The last column reports the t-test results for difference in the mean of each variable between the foreign and U.S. sample. *, **, *** indicate significance at the 10 percent, 5percent, and 1 percent levels, respectively. All the variables are winsorized at 1percent and 99 percent to mitigate the effect outliers. All variables are defined in Appendix B.

| Variable | Foreign firms | | | | | US firms | | | | Difference in mean | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | N | Mean | Median | Q1 | Q3 | N | Mean | Median | Q1 | Q3 | | |
| CAR[0,1] | 170 | -0.05 | -0.03 | -0.07 | 0.006 | 1,953 | -0.03 | -0.01 | -0.06 | 0.01 | -0.02 | *** |
| DURATION | 170 | 2.11 | 1.87 | 1.00 | 3 | 1,953 | 2.45 | 2.00 | 1.00 | 3.25 | -0.34 | ** |
| CHG_NI | 170 | -0.04 | -0.002 | -0.02 | 0 | 1,953 | -0.03 | -0.004 | -0.018 | 0 | -0.01 | |
| NONQUANTIFY | 170 | 0.15 | 0 | 0 | 0 | 1,953 | 0.12 | 0 | 0 | 0 | 0.03 | |
| NUM_ACCNT | 170 | 2.51 | 2 | 1 | 3 | 1,953 | 2.71 | 2 | 1 | 4 | -0.20 | |
| LOW_VIS | 170 | 0.40 | 0 | 0 | 1 | 1,953 | 0.29 | 0 | 0 | 1 | 0.11 | *** |
| PCT_II | 170 | 0.23 | 0.10 | 0.01 | 0.33 | 1,953 | 0.54 | 0.58 | 0.25 | 0.83 | -0.32 | *** |
| LEV | 170 | 0.22 | 0.20 | 0.03 | 0.33 | 1,953 | 0.25 | 0.20 | 0.04 | 0.39 | -0.03 | |
| BM | 170 | 0.79 | 0.62 | 0.28 | 1.08 | 1,953 | 0.56 | 0.47 | 0.24 | 0.77 | 0.23 | *** |
| EP | 170 | 0.05 | 0.02 | 0 | 0.07 | 1,953 | 0.04 | 0.02 | 0 | 0.05 | 0.02 | *** |
| LOSS | 170 | 0.44 | 0 | 0 | 1 | 1,953 | 0.41 | 0 | 0 | 1 | 0.04 | |
| CFO | 170 | 0.02 | 0.03 | -0.05 | 0.11 | 1,953 | 0.03 | 0.06 | 0.00 | 0.12 | -0.01 | |
| ACC | 170 | -0.05 | -0.05 | -0.12 | 0.02 | 1,953 | -0.09 | -0.06 | -0.12 | -0.02 | 0.04 | *** |
| INT_COV | 170 | 0.12 | 0.06 | 0 | 0.23 | 1,953 | 0.15 | 0.08 | 0.00 | 0.26 | -0.03 | |
| SIZE | 170 | 6.35 | 6.08 | 4.71 | 7.80 | 1,953 | 6.03 | 5.98 | 4.64 | 7.27 | 0.32 | ** |
| EA | 170 | 0.17 | 0 | 0 | 0 | 1,953 | 0.24 | 0 | 0 | 0 | -0.07 | ** |
| SURP | 170 | -0.000003 | 0 | 0 | 0 | 1,953 | -0.0009 | 0 | 0 | 0 | 0.00 | |
| BIG4 | 170 | 0.62 | 1 | 0 | 1 | 1,953 | 0.76 | 1 | 1 | 1 | -0.14 | *** |
| AGE | 170 | 1.75 | 1.88 | 1.08 | 2.54 | 1,953 | 2.36 | 2.48 | 1.85 | 3.08 | -0.61 | *** |
| SALESDIST | 134 | 0.73 | 0.49 | 0.86 | 1 | 1,553 | 0.23 | 0 | 0.12 | 0.41 | 0.50 | *** |

## Table 3.3 Market Reaction to Irregularity Announcements (H1a)

This table reports the results of estimating Equation (3.1) − market reaction to restating firms, using the U.S. and foreign irregularity sample. *, **, *** indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively, based on two-tailed tests. In the brackets below the coefficient estimates, t-statistics are reported. All variables are defined in Appendix B. All the variables are winsorized at 1 percent and 99 percent to mitigate the effect of outliers. The standard errors are clustered at the industry-year level.

|  | *No Entropy Balancing* | *Entropy Balancing* |
|---|---|---|
|  | (1) | (2) |
| *FF* | **-0.0248**** | **-0.0219**** |
|  | **(-2.46)** | **(-2.06)** |
| *DURATION* | -0.000270 | 0.00446 |
|  | (-0.27) | (1.15) |
| *CHG_NI* | 0.0611** | 0.0453 |
|  | (2.55) | (1.05) |
| *NONQUANTIFY* | -0.0260*** | -0.0397** |
|  | (-2.94) | (-2.08) |
| *NUM_ACCNT* | -0.00152 | -0.00706** |
|  | (-1.11) | (-2.32) |
| *LOW_VIS* | 0.0302*** | 0.0450*** |
|  | (6.21) | (4.92) |
| *PCT_II* | -0.0265*** | -0.0343 |
|  | (-2.78) | (-1.61) |
| *LEVERAGE* | -0.00811 | -0.0504 |
|  | (-0.72) | (-1.59) |
| *BM* | -0.00182 | -0.00135 |
|  | (-0.41) | (-0.23) |
| *EP* | 0.0394 | 0.0288 |
|  | (0.88) | (0.44) |
| *LOSS* | -0.00358 | -0.0159 |
|  | (-0.59) | (-1.19) |
| *CFO* | -0.00182 | 0.00330 |
|  | (-0.15) | (0.07) |
| *ACC* | -0.0398*** | -0.0943** |
|  | (-3.20) | (-2.26) |
| *INT_COV* | -0.000601 | 0.00554 |
|  | (-0.14) | (0.69) |
| *SIZE* | -0.0000676 | -0.00385 |
|  | (-0.04) | (-1.21) |
| *EA* | 0.0236*** | 0.0391*** |
|  | (4.39) | (3.08) |
| *SURP* | 1.162*** | 2.426*** |
|  | (3.38) | (3.63) |

| | | |
|---|---|---|
| BIG4 | 0.0118* | 0.0138 |
| | (1.69) | (1.10) |
| AGE | 0.00759*** | 0.00418 |
| | (3.36) | (0.92) |
| Constant | -0.0140 | -0.00687 |
| | (-0.59) | (-0.15) |
| Industry FE | Yes | Yes |
| Year FE | Yes | Yes |
| Observations | 2,123 | 2,123 |
| Adjusted R-squared | 0.0593 | 0.2364 |

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**Table 3.4 Market Reaction to Announcements of Foreign Firm Irregularities: U.S. vs. Home Country Shares (H1b)**

For a subsample of 53 foreign firms that are traded on both a U.S. exchange and their home country exchange, this table reports the market reactions to the same foreign restatement by the firm's U.S. investors and its home country investors over various event windows. T-test for the difference in market reactions between the two groups of investors over each event window is provided in the last column. *, **, *** indicate significance at the 10 percent, 5percent, and 1 percent levels, respectively, based on two-tailed tests.

|  | **U.S.** | **Home** | **diff** | **t-stat** |
|---|---|---|---|---|
| *RET[0,1]* | -5.25% | -5.07% | -0.18% | (-0.53) |
| *RET[1,2]* | -3.06% | -1.93% | -1.13%** | (-2.27) |
| *RET[0,2]* | -4.96% | -4.16% | -0.80%* | (-1.75) |
| *Observations* | 53 | 53 | 53 | |

**Table 3.5 Market Reaction to Irregularity Announcements and Country-Level Variables**

This table reports the results of estimating Equation (3.2), which examines the effect of country-level variables on the stock market reactions to restating firms, using the U.S. and foreign restating sample. *, **, *** indicate significance at the 10 percent, 5percent, and 1 percent levels, respectively, based on two-tailed tests. In the brackets below the coefficient estimates, t-statistics are reported. All variables are defined in Appendix B. All the variables are winsorized at 1 percent and 99 percent to mitigate the effect of outliers. The standard errors are clustered at the industry-year level.

| | *DISTANCE as GAAPdiff* | *DISTANCE as AUD_ENFdiff* | *DISTANCE as LAWdiff* |
|---|---|---|---|
| *Variables* | **(1)** | **(2)** | **(3)** |
| *FF* | **-0.0267\*\*** | **-0.0266\*\*** | **-0.0281\*\*\*** |
| | **(-2.55)** | **(-2.50)** | **(-2.79)** |
| *DISTANCE* | **0.000417** | **0.000312** | **0.00135** |
| | **(0.20)** | **(0.33)** | **(0.15)** |
| *DURATION* | -0.000231 | -0.000218 | -0.000248 |
| | (-0.23) | (-0.22) | (-0.25) |
| *CHG_NI* | 0.0623\*\*\* | 0.0625\*\*\* | 0.0613\*\* |
| | (2.61) | (2.61) | (2.56) |
| *NONQUANTIFY* | -0.0263\*\*\* | -0.0264\*\*\* | -0.0260\*\*\* |
| | (-2.98) | (-3.00) | (-2.92) |
| *NUM_ACCNT* | -0.00156 | -0.00156 | -0.00148 |
| | (-1.13) | (-1.13) | (-1.07) |
| *LOW_VIS* | 0.0294\*\*\* | 0.0294\*\*\* | 0.0303\*\*\* |
| | (6.02) | (6.03) | (6.23) |
| *Firm Characteristics* | Yes | Yes | Yes |
| *Industry FE* | Yes | Yes | Yes |
| *Year FE* | Yes | Yes | Yes |
| *Observations* | 2,102 | 2,099 | 2,119 |
| *Adjusted R-squared* | 0.0588 | 0.0588 | 0.0605 |

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## Table 3.6 Market Reaction to Irregularity Announcements and Foreign Sales

This table examines whether stock market reaction to restatements vary with the proportion of a firm's sales that occur outside the U.S. *, **, *** indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively, based on two-tailed tests. In the brackets below the coefficient estimates, t-statistics are reported based on clustered standard-errors, with event (restatement announcement) clusters. All variables are defined in Appendix B. All the variables are winsorized at 1% and 99% to mitigate the effects of outliers.

| | Drop Firms with No Segment Disclosure | | Keep Firms with No Segment Disclosure | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *SALESDIST* | **-0.0190**** | **-0.0213**** | **-0.0219***** | **-0.0175*** |
| | **(-2.06)** | **(-1.98)** | **(-2.64)** | **(-1.72)** |
| *DURATION* | 0.000120 | 0.000185 | -0.000233 | -0.000215 |
| | (0.10) | (0.15) | (-0.23) | (-0.21) |
| *CHG_NI* | 0.0617* | 0.0656** | 0.0622*** | 0.0648*** |
| | (1.94) | (2.08) | (2.59) | (2.67) |
| *NONQUANTIFY* | -0.0203* | -0.0170 | -0.0268*** | -0.0243*** |
| | (-1.88) | (-1.57) | (-3.03) | (-2.75) |
| *NUM_ACCNT* | 0.000363 | 0.000183 | -0.00129 | -0.00145 |
| | (0.23) | (0.12) | (-0.93) | (-1.04) |
| *LOW_VIS* | 0.0326*** | 0.0320*** | 0.0296*** | 0.0293*** |
| | (5.96) | (5.76) | (6.14) | (6.02) |
| *Constant* | -0.0209 | -0.0444 | -0.0131 | -0.0356 |
| | (-0.72) | (-0.60) | (-0.55) | (-0.49) |
| *Firm Characteristics* | Yes | Yes | Yes | Yes |
| *Industry FE* | Yes | Yes | Yes | Yes |
| *Year FE* | Yes | Yes | Yes | Yes |
| *Country FE* | No | Yes | No | Yes |
| *Observations* | 1,687 | 1,687 | 2,123 | 2,123 |
| *Adjusted R-squared* | 0.0495 | 0.0527 | 0.0592 | 0.0605 |

**Table 3.7 Geographic Contagion Effect of Foreign Irregularities (H2)**

This table reports the results of testing H2 that the negative market reaction to restating firms will spill over to the non-restating firms from the same country. The average CAR highlighted in bold are significantly different from zero at the 10% level, based on two-tailed t-tests. For non-restating firms, t-statistics are reported based on clustered standard-errors, with event (restatement announcement) clusters. The CAR are winsorized to 1% and 99% to mitigate the influence of outliers.

| Event Window | Restating Firms | | Non-restating Firms | |
|---|---|---|---|---|
| | Mean CAR | t-stat | Mean CAR | t-stat |
| [0,1] | -9.605% | **(-8.80)** | -0.445% | **(-3.11)** |
| [0,2] | -9.233% | **(-8.08)** | -0.380% | **(-2.25)** |
| [0,5] | -9.190% | **(-7.45)** | -0.589% | **(-2.45)** |
| [0,10] | -10.279% | **(-7.28)** | -0.453% | (-1.31) |
| *Observations* | 129 | | 13,879 | |

**Table 3.8 Determinants of Contagion Effects – CAR [0, 5]: Country-Level Variables**

This table reports the results of estimating Equation (3.3), which examines the effect of country-level variables on the contagion effect on the non-restating foreign firms. *, **, *** indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively, based on two-tailed tests. In the brackets below the coefficient estimates, t-statistics are reported based on clustered standard-errors, with event (restatement announcement) clusters. All the variables are defined in Appendix B. All the variables are winsorized at 1 percent and 99 percent to mitigate the effect of outliers.

| | DISTANCE as GAAPdiff | DISTANCE as AUD_ENFdiff | DISTANCE as LAWdiff |
|---|---|---|---|
| | (1) | (2) | (3) |
| *DISTANCE* | **-0.0000612** | **0.000182** | **0.000587** |
| | **(-0.10)** | **(0.67)** | **(0.19)** |
| *PCT_II* | 0.00265 | 0.00441 | 0.00346 |
| | (0.55) | (0.95) | (0.71) |
| *AGE* | 0.00111 | 0.00133 | 0.00123 |
| | (1.04) | (1.21) | (1.14) |
| *LEVERAGE* | -0.00384 | -0.00386 | -0.00362 |
| | (-0.59) | (-0.59) | (-0.55) |
| *BM* | -0.0000968 | -0.000258 | -0.000197 |
| | (-0.08) | (-0.20) | (-0.16) |
| *EP* | -0.0198 | -0.0207 | -0.0200 |
| | (-1.46) | (-1.52) | (-1.47) |
| *LOSS* | -0.00295 | -0.00266 | -0.00280 |
| | (-1.16) | (-1.06) | (-1.10) |
| *INT_COV* | -0.00306 | -0.00291 | -0.00301 |
| | (-1.45) | (-1.38) | (-1.42) |
| *SIZE* | 0.000987 | 0.000894 | 0.000952 |
| | (1.49) | (1.34) | (1.43) |
| *SAME_IND* | 0.00117 | 0.000885 | 0.00118 |
| | (0.23) | (0.17) | (0.23) |
| *CAR_REST* | 0.0162 | 0.0159 | 0.0161 |
| | (1.22) | (1.20) | (1.21) |
| *HMKT_RET* | 0.385*** | 0.386*** | 0.385*** |
| | (4.81) | (4.83) | (4.81) |
| *Constant* | -0.00867 | -0.0115 | -0.0107 |
| | (-0.58) | (-0.89) | (-0.76) |
| *Industry FE* | Yes | Yes | Yes |
| *Year FE* | Yes | Yes | Yes |
| *Observations* | 11,592 | 11,582 | 11,592 |
| *Adjusted R-squared* | 0.0292 | 0.0293 | 0.0292 |

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## Table 3.9 Determinants of Contagion Effects: Accruals Quality

This table reports the results on whether the contagion effects of foreign restatements vary with differences in accruals quality within a given country-year. *, **, *** indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively, based on two-tailed tests. In the brackets below the coefficient estimates, t-statistics are reported based on clustered standard-errors, with event (restatement announcement) clusters. All variables are defined in Appendix B. All the variables are winsorized at 1% and 99% to mitigate the effects of outliers.

| | CAR[0,1] | | CAR[0,5] | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| AQ | -0.0344*** | -0.0332*** | -0.0334* | -0.0355* |
| | (-2.82) | (-2.82) | (-1.68) | (-1.82) |
| PCT_II | 0.00325 | 0.00601** | 0.00534 | 0.0103** |
| | (1.25) | (2.09) | (1.02) | (2.09) |
| AGE | -0.000683 | -0.000882 | 0.000266 | -0.0000856 |
| | (-1.03) | (-1.42) | (0.25) | (-0.09) |
| LEVERAGE | 0.00439 | 0.00464 | 0.00127 | 0.00255 |
| | (0.95) | (0.99) | (0.17) | (0.33) |
| BM | -0.000254 | -0.000472 | -0.000288 | -0.000730 |
| | (-0.34) | (-0.65) | (-0.19) | (-0.50) |
| EP | 0.00524 | 0.00307 | -0.00903 | -0.00936 |
| | (0.44) | (0.26) | (-0.59) | (-0.66) |
| LOSS | -0.000565 | -0.000325 | -0.00303 | -0.00254 |
| | (-0.35) | (-0.20) | (-1.06) | (-0.91) |
| INT_COV | 0.00172 | 0.00186 | -0.00216 | -0.00225 |
| | (1.14) | (1.25) | (-0.90) | (-0.96) |
| SIZE | -0.000218 | -0.000270 | 0.000977 | 0.000623 |
| | (-0.51) | (-0.62) | (1.24) | (0.83) |
| SAME_IND | -0.00422 | -0.00462 | 0.00237 | 0.000869 |
| | (-1.45) | (-1.52) | (0.36) | (0.13) |
| CAR_REST | -0.00671 | -0.00924 | 0.000387 | 0.00601 |
| | (-0.57) | (-0.62) | (0.03) | (0.41) |
| HMKT_RET | 0.352*** | 0.432*** | 0.457*** | 0.434*** |
| | (4.86) | (4.52) | (4.70) | (4.48) |
| Constant | -0.00999 | -0.0129* | -0.0472*** | -0.0338*** |
| | (-1.31) | (-1.88) | (-3.21) | (-3.16) |
| Industry FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | No | Yes | No |
| Country-Year FE | No | Yes | No | Yes |
| Observations | 7,851 | 7,851 | 7,851 | 7,851 |
| Adjusted R-squared | 0.0375 | 0.0466 | 0.0383 | 0.0570 |

# Chapter 4: Conclusion

This thesis is a collection of two studies that seek to broaden our understanding of the capital market consequences of financial reporting weaknesses. The first study documents that although the JOBS Act postpones deadline of internal control audits after IPO, post-JOBS issuers are more willing to provide voluntary disclosure of internal control deficiencies. This unintended consequence arises because both investors and IPO firms rationally update the belief of deteriorating internal control quality in the IPO population after the JOBS Act. It highlights the novel mechanism that both investors and firms take into consideration the change of firm population.

Upon the passage of the JOBS Act, the SEC voiced genuine concern on weakened investor protection. The results of the first study paint a brighter picture of IPO disclosure after the JOBS Act. Although on average information uncertainty is greater for post-JOBS issuers due to reduced mandatory disclosure, some firms are more willing to voluntarily disclose private information to separate themselves from those with the lowest internal control quality. Note that this study focuses on the IPO firms that would go public both before and after the JOBS Act and are on the margin of disclosing internal control deficiencies before the JOBS Act. The inference is not focused on the firms that are attracted to go public only after the JOBS Act. Therefore, I caution against concluding from this study that disclosure quality improves for all IPO firms such that the concern of weakened investor protection due to deregulation from the JOBS Act is utterly unwarranted.

The second study documents that U.S. investors react more unfavorably to accounting irregularities made by foreign firms. While there is no evidence that the market reactions vary in intensity of several country-level institutional characteristics that prior literature finds to play an important role in the stock market, we finds that market reaction varies with the firm-specific

measure of information frictions, the percent of non-U.S. sales. In addition, we find the contagion effect that non-restating firms from the same country also experiences stock price declines. Collectively, it suggests that investors reassess information risk associated with foreign firms after accounting irregularities although the perceived commonality remains a puzzle.

For future work, I plan to extend my thesis in several dimensions. For instance, the first study investigates change in voluntary disclosure of ICD around the JOBS Act. I would like to link voluntary IPO disclosure to the mandatory disclosure of ICD when auditors get involved with internal control audits five years after IPO. There are also a lot of research avenues to explore following the second study. To hold the accounting irregularity constant, it compares the market reactions from different stock exchanges for the same irregularity announcement. This empirical setting can be applied to other major corporate information events. Finally, since foreign firms experience greater market value loss in the U.S. market upon announcement of accounting irregularities, it would be interesting to explore what actions foreign firms take to restore the credibility of financial reporting in the U.S. market after revelation of accounting irregularities.

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# Appendices

## Appendix A   Appendix for Chapter 2

### A.1     Examples of ICD Disclosure during IPO

This appendix provides one example of disclosure of no ICD in the subsection A.1.1 and another example of disclosure of ICD unremediated by the time of IPO in the subsection A.1.2.

### A.1.1 ICD Disclosure:   Aratana Therapeutics Inc.

Aratana Therapeutics Inc. is a pharmaceutical company that went public in June 2013. Below is an excerpt from the section *Risk Factors* of its registration statement.

*A material weakness in internal control was identified in connection with the preparation of our financial statements and the audit of our financial results for 2011. We determined that we had a material weakness relating to accounting for complex transactions and cut-off of expenses. During 2012, we added personnel to our accounting staff with appropriate levels of experience to remediate the aforementioned material weakness. As of December 31, 2012, we determined the material weakness had been remediated as a result of the actions taken above and the resulting improvements in our internal controls.*

## A.1.2 ICD Disclosure: Textura Corporation

Textura Corporation is a software company. It went public in June 2013 and identified itself as an *Emerging Growth Company* in the prospectus. Textura Corporation discloses one material weakness due to general inadequate personnel with financial reporting requirements, another material weakness specific to the accounting for convertible debentures, and a third material weakness related to the accounting issue of business acquisitions. To remediate the material weaknesses, Textura Corporation has hired additional personnel and made an effort to improve the design and implementation of an effective financial reporting system. As the implementation is still on-going, it can be inferred that material weaknesses remain around the IPO. Below is an excerpt from the section *Management Discussion & Analysis* of the registration statement.

### Internal Control over Financial Reporting

*In preparation for this offering and for future compliance with Section 404 of the Sarbanes-Oxley Act, we concluded that a material weakness in internal control over financial reporting related to our control environment existed as of September 30, 2012 as described below.*

*A material weakness is a deficiency, or combination of deficiencies, in internal control over financial reporting, such that there is a reasonable possibility that a material misstatement of our annual or interim consolidated financial statements will not be prevented, or detected and corrected on a timely basis.*

*•We did not maintain a sufficient complement of personnel with the appropriate level of accounting knowledge, experience, and training in the application of GAAP commensurate with our financial reporting requirements. Specifically, we did not maintain adequate qualified personnel with regard to certain significant complex transactions and technical accounting matters and we lacked adequate controls regarding training in the relevant accounting guidance, review and documentation of certain complex accounting transactions and review of related accounting disclosures such as the accounting for convertible debenture financing agreements, including any embedded features, business combinations and share-based compensation transactions in accordance with GAAP.*

*This material weakness in our control environment contributed to the following individual material weaknesses in our internal control over financial reporting:*

*•We did not maintain effective internal controls related to our accounting for convertible debentures to provide reasonable assurance that (a) the instruments were valued correctly and (b) all pertinent facts related to the convertible debentures, including the impact of conversion and*

*redemption or other embedded or derivative features, were identified and considered for appropriate accounting in accordance with GAAP. Specifically, this material weakness resulted in material misstatements and audit adjustments of non-cash interest expense, convertible debenture and derivative liabilities and additional paid-in capital to the consolidated financial statements for the fiscal years ended September 30, 2010, 2011 and 2012.*

*•We did not maintain effective internal controls related to the accounting for business acquisitions to provide reasonable assurance that (a) business combination accounting identified and considered all pertinent factors related to all classes of securities of the acquired entity, including any non-controlling interests and (b) there was appropriate review of the purchase price allocation entries recorded in the consolidated financial statements. Specifically, this material weakness resulted in material misstatements and audit adjustments to non-controlling interest and the related income (loss) attributable to our company and the non-controlling interest, additional paid-in capital, deferred revenue and revenue, goodwill, intangible assets net and the related amortization expense to the consolidated financial statements for the fiscal year ended September 30, 2012.*

*These material weaknesses could result in misstatements of the aforementioned accounts that would result in a material misstatement to the annual or interim consolidated financial statements that would not be prevented or detected on a timely basis.*

### *Plan for Remediation of the Material Weaknesses*

*We are currently in the process of finalizing our remediation plans and implementing the same. To date we have implemented and are continuing to implement a number of measures to address the material weaknesses identified. In January 2013, we hired a corporate controller with appropriate experience applying GAAP technical accounting guidance and are in the process of hiring additional accounting personnel. We also are designing additional controls around identification, documentation and application of technical accounting guidance with particular emphasis on events outside the ordinary course of business. These controls are expected to include the implementation of additional supervision and review activities by qualified personnel, the preparation of formal accounting memoranda to support our conclusions on technical accounting matters, and the development and use of checklists and research tools to assist in compliance with GAAP with regard to complex accounting issues. We intend to complete the implementation of our remediation plan during fiscal 2013.*

*In addition, we intend to engage a third-party provider to help us assess and improve our internal controls for complying with the Sarbanes-Oxley Act. The process of designing and implementing an effective financial reporting system is a continuous effort that requires us to anticipate and react to changes in our business and the economic and regulatory environments and to expend significant resources to maintain a financial reporting system that is adequate to satisfy our reporting obligations. As we continue to evaluate and take actions to improve our internal control over financial reporting, we may determine to take additional actions to address control deficiencies or determine to modify certain of the remediation measures described above.*

*We cannot assure you that the measures we have taken to date, or any measures we may take in the future, will be sufficient to remediate the material weaknesses we have identified or avoid potential future material weaknesses.*

## A.2    Variable Definitions

| Variable | Definition |
|---|---|
| **JOBS Act and ICD variables** | |
| *ICD* | An indicator variable equal to one if the firm discloses the status of internal control deficiencies in the prospectus and zero otherwise. |
| *Post* | An indicator variable equal to one if the firm went public after the JOBS Act and identified itself as an *Emerging Growth Company* in the prospectus, and equal to zero if the firm went public before the JOBS Act and had a total revenue of less than $1 billion in the most recent fiscal year before IPO. |
| *Post*ICD* | The interaction term of *Post* and *ICD*. |
| *NewCFO* | An indicator variable equal to one if the firm discloses that a new CFO has been or will be hired to remediate ICD and zero otherwise. |
| *Employee* | An indicator variable equal to one if the firm discloses that it has improved or plans to improve personnel to remediate ICD and zero otherwise. |
| *Process* | An indicator variable equal to one if the firm discloses that it has improved or plans to improve policies and procedures to remediate ICD and zero otherwise. |
| *NumRem* | The number of remediation measures the firm has implemented or planned to implement before IPO (the sum of *NewCFO*, *Employee*, and *Process*). |
| **Dependent variables** | |
| *Underpricing* | Closing price on the first trading day divided by the offer price minus 1. |
| *MJ Accruals* | Discretionary accruals based on the modified Jones model. |
| *KLW Accruals* | Performance-matched discretionary accruals following Kothari et al. (2005). I match each IPO firm with a non-IPO firm with the closest ROA in the same 2-digit SIC industry for the same year. KLW Accruals are discretionary accruals of the IPO firm minus the matched firm's discretionary accruals. |
| *IC_Dummy* | An indicator variable equal to one if the SEC issues a comment letter that involves internal control disclosure during IPO and zero otherwise. The comment letters on internal control issues are identified by Audit Analytics taxonomies. |
| *IC_Sum* | The number of internal control issues raised in all SEC comment letters throughout the IPO process. |
| *IC_1st Lett* | The number of internal control issues raised in the first SEC comment letter in reference to IPO registration statements. |
| **Control variables** | |
| *Age* | The logarithm of 1 plus the year of IPO minus the year when the firm was founded. The founding dates mainly come from https://site.warrington.ufl.edu/ritter/ipo-data/. I thank Jay Ritter for making the data available. For the remaining missing observations of founding dates, I collect the inception dates from IPO registration statements, company profiles in Thomson One, and corporate websites. |
| *AuditorChg* | An indicator variable equal to one if the auditor resigns or is dismissed in the most recent three years before IPO and zero otherwise. |
| *Big4* | An indicator variable equal to one if the auditor is the big 4 audit firm and zero otherwise. |
| *Cash* | The ratio of cash (CHE) over total assets (AT). |
| *ChgInvt* | Change in inventory (INVT) scaled by average total assets (AT). |

| | |
|---|---|
| *ChgSales* | Change in sales (SALE) scaled by average total assets (AT). |
| *ExtFin* | The sum of equity financing and debt financing deflated by total assets, following Ettredge et al. (2011). Equity financing is the sum of sales of common and preferred stock (SSTK) minus purchases of common and preferred stock (PRSTKC) and cash dividends (DV). Debt financing equals long-term debt (DLTIS) issued minus long-term debt reduction (DLTR) minus the change in current debt (DLCCH). |
| *Foreign* | An indicator variable equal to one if foreign income (PIFO) is non-zero and non-missing in the most recent two fiscal years before IPO and zero otherwise. |
| *IC_Year* | *The ratio of the number of firms that receive an SEC comment letter on internal control issues over the number of firms that receive an SEC comment letter over the calendar year.* |
| *Leverage* | The sum of debt in current liabilities (DLC) and long-term debt (DLTT) scaled by total assets (AT). |
| *Litigation* | An indicator variable equal to one if the firm is in an industry of high litigation risk (four-digit SIC industry codes 2833–2836, 3570–3577, 3600–3674, 5200–5961, or 7370–7374, following Francis et al. 1994) and zero otherwise. |
| *Loss* | An indicator variable set equal to 1 if the firm has negative aggregate earnings before extraordinary items ($IB_t + IB_{t-1} < 0$) before IPO and zero otherwise. |
| *M&A* | An indicator variable equal to one if the firm reports non-zero acquisition or mergers (AQC, AQI or AQS) in the most recent two fiscal years before IPO and zero otherwise. |
| *Market Return* | Cumulative value-weighted market return over 21 trading days prior to the issue date. |
| *Overhang* | The ratio of retained shares over total shares offered. |
| *PrcRev+* | $\max(0, \Delta p)$, where $\Delta p$ is the percentage difference between the offer price and the original midrange file price. |
| *PrcRev-* | $\min(0, \Delta p)$, where $\Delta p$ is the percentage difference between the offer price and the original midrange file price. |
| *Proceeds* | The logarithm of IPO proceeds. |
| *Risk Factors* | The logarithm of the number of risk factors disclosed in the prospectus. |
| *Restate* | An indicator variable equal to one if the firm restates the most recent three years of financial statements before IPO issue date and zero otherwise. |
| *Restructure* | An indicator variable equal to one if the firm has non-zero restructuring costs (RCP) in the most recent two years before IPO and zero otherwise. |
| *R&D* | R&D expenses (XRD) scaled by total assets (AT). |
| *ROA* | Income before extraordinary items (IB) scaled by the average total assets. |
| *Second Tier* | An indicator variable equal to one if the auditor is the second tier audit firm (i.e., BDO Seidman, Crowe Horwath, Grant Thornton, or McGladrey & Pullen), and zero otherwise. |
| *Segments* | The number of business segments and geographic segments. |
| *Size* | The logarithm of total assets (AT). |
| *Reputation* | The lead underwriter's reputation ranking based on Carter and Manaster (1990) and updated by Loughran and Ritter (2004). I thank Jay Ritter for making the data available at https://site.warrington.ufl.edu/ritter/ipo-data/. |
| *VC_Backed* | An indicator variable equal to one if the IPO firm is backed by venture capital and zero otherwise. |

## Appendix B  Appendix for Chapter 3

### B.1      Variable Definition

| Variable | Definition |
|---|---|
| **Consequence measures** | |
| CAR[0,1] | Cumulative two-day [0, 1] value-weighted market-adjusted returns of the restating firms centered around the restatement announcement date; |
| CAR[0,5] | Cumulative six-day [0, 5] value-weighted market-adjusted returns of the non-restating firms centered around the restatement announcement date; |
| **Distance measures** | |
| FF | An indicator variable equal to one if the firm's headquarter is located outside U.S. and zero otherwise; |
| *Accounting distance* | |
| GAAPdiff | Differences between the home country accounting standards and the US GAAP, modified from Bae et al. (2008). For countries that do not adopt IFRS, our measure is identical to *GAAPdiff2* in Bae et al. (2008), which is based on a survey examining the extent local accounting standards deviate from IFRS for a list of 52 accounting rules. Two rules are considered the same if they both comply with IFRS, or, if neither complies with IFRS. If one country conforms to IFRS and the other country does not, then the two countries are deemed to have different GAAP for that item. For IFRS adopting countries, the measure is calculated in the same way in the pre-adoption period and is set to 4 after the first IFRS annual report date. |
| *Audit and enforcement distance* | |
| AUD_ENFdiff | The audit and enforcement index for the U.S. less the index for a firm's home country. This index measures the quality of auditing environment and the degree of accounting enforcement in the country for the years 2002, 2005 and 2008. We assign the index measured at the year 2002 to restatements filed before 2005, the index measured at the year 2005 to restatements filed between 2005 and 2007, and the index measured at the year 2008 to restatements filed from 2008 onwards.  The index is scaled from 0 to 56 with lower scores representing lower quality of auditors' working environment and less active legal enforcement. *Source: Brown et al. (2014);* |
| *Law and enforcement distance* | |
| LAWdiff | The rule of law index for the U.S. less the index for a firm's home country, where the rule of law index reflects perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence. Ranging from approximately -2.5 (weak) to 2.5 (strong), the rule of law index is from the Worldwide Governance Indicators created by the World Bank. *Source:* http://info.worldbank.org/governance/wgi/index.aspx#doc |
| **Control variables** | |
| *Restatement Characteristics* | |
| NONQUANTIFY | An indicator variable equal to 1 if the restating firm does not quantify the change in net income as a result of the restatement and 0 otherwise; |
| CHG_NI | Change in net income as a result of restatement, defined as the difference between restated net income and net income previously reported, divided by lagged total assets, coded from Audit Analytics database. It is 0 for all restatement announcements that do not quantify the restatement impact on net income; |
| DURATION | The number of years restated, defined as the gap between the restatement ending date and restatement beginning date divided by 365, both from Audit Analytics database; |

| | |
|---|---|
| *NUM_ACCNT* | The number of accounting issues involved in the restatement, coded from Audit Analytics database; |
| *LOW_VIS* | An indicator variable equal to 1 if the restatement was announced in a periodic report, their amendments, late notice, and etc., and it is set to 0 if the restatement was announced in a more visible channel such as the 8-K, 8-K/A, 6-K, 6-K/A and press releases; |
| ***Firm Characteristics*** | |
| *PCT_II* | Percentage ownership by U.S. institutional investors in the firm from Thomson Reuters Institutional (13f ) Holdings data, defined as total market value of shares owned by U.S. institutions divided by the total market value of the firm at end of the fiscal year; |
| *LEVERAGE* | The debt-to-total assets ratio, calculated as [(long-term debt + debt in current liabilities)/total asset]: [(DLTT+DLC)/AT]; |
| *BM* | The book to market ratio, calculated as book value of equity divided by market value of equity: [CEQ/(CSHO*PRCC_F)]; |
| *EP* | Earnings-to-price ratio, calculated as (earnings per share/year-end market price): [EPSPX/PRCC_F]; it is equal to zero for negative earnings. |
| *LOSS* | An indicator variable equal to 1 if earnings per share (EPSPX) is negative and 0 otherwise; |
| *CFO* | Cash flow from operations divided by lagged total assets: [OANCF/L_AT]; |
| *ACC* | Total accruals, calculated as income before extraordinary items minus cash flow from operations divided by lagged total assets: [(IB–OANCF)/L_AT]; |
| *INT_COV* | The inverse of a firm's interest coverage ratio, calculated as interest expense divided by operating income before depreciation XINT/OIBDP; |
| *SIZE* | Firm size, calculated as log of total assets at the end of the fiscal year: [Log(AT)]; |
| *BIG4* | An indicator variable equal to one if the firm is audited by one of the Big 4 audit firms that year (or Big 5 before the exit of Arthur Andersen) and zero otherwise: coded from AU; |
| *EA* | An indicator variable equal to 1 if the restatement announcement is within a three-day window [-1,0,1] of the earnings announcement, and 0 otherwise; |
| *SURP* | The earnings surprise measured as the difference between annual EPS and the mean consensus analyst forecast deflated by stock price at the beginning of the year [(EPS – Meanest)]/L_PRCC_F]. It is 0 for all restatement announcements that do not fall within a three-day window [-1,0,1] of the earnings announcement date; |
| *AQ* | Accruals quality developed by Dechow and Dichev (2002) and modified by Francis et al. (2005). We first regress total current accruals on lagged, current and future cash flows, the change in revenue, and PPE. The regression model is estimated by year for each Fama and French 48 industry group with at least 20 observations in a given year. AQ at year *t* is then calculated as the standard deviation of the firm-level residuals from year *t-4* through *t*. |
| *SAME_IND* | Indicator variable equal to 1 if the non-restating firm is in the same Fama and French 48 industry as the restating firm; |
| *CAR_REST* | Cumulative six-day [0, 5] value-weighted market-adjusted returns of the restating firms centered around the restatement announcement date; |
| *AGE* | Log of the number of years the firms has been listed in the U.S. exchange; |
| *HMKT_RET* | Cumulative market return in the home country over the event window of [0, 1] or [0, 5]. The daily return is the percentage change in the total return index (RI) of Datastream country indices denominated in US dollars. |
| *SALESDIST* | One minus the ratio of the U.S. segment sales over sales (SALE). |