Determinants of Undetected, Unintentional Errors in Audited Financial Statements

by

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AUTHOR'S DECLARATION

I hereby declare that I am the sole author of this thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners.

I understand that my thesis may be made electronically available to the public.

Abstract

This dissertation investigates the associations between financial restatements and characteristics of the parties responsible for preventing and detecting unintentional errors, i.e., boards (through their audit committees), management (through chief financial officers (CFOs)), and auditors. To conduct this investigation, I developed a theoretical model of restatement determinants that is more complete than models used in previous archival research as it includes characteristics of all three parties and the moderating effects of chief financial officers' financial expertise and influence on the disruptive effects of organizational change. To identify restatements that correct unintentional error, I conducted automated text searches of over 10,000 restatement disclosures for language asserting or implying lack of intent. This language-based proxy is automated, direct, transparent, easily replicable, scalable, and classifies as error-correcting a smaller proportion of restatements as error-correcting than other proxies. I validated this proxy by contrasting the characteristics of the unintentional error restatements against other restatements based on theory-derived expectations. I find that annual financial statements restated to correct unintentional error(s) for years with *Sarbanes Oxley Act of 2002* (SOX) Section 404 auditor's opinions exhibit less net income smoothing, less earnings persistence, and less positive accruals than such firm-years of other restatements. Finally, I tested the theoretical model using logistic regressions and data from financial statements, proxy statements, and auditor's SOX 404 opinions of 346 companies (i.e., 121 companies that restate to correct unintentional error; 121 companies without restatement matched by year, industry, and company size; and 104 companies with other restatements that proxy for restatements of intentional misstatement).

Results show that of the three parties responsible for financial reporting quality, the CFO plays the major role with respect to unintentional error: The likelihood of restatement to correct unintentional error is decreasing in CFO financial expertise and influence, but only when companies are undergoing organizational change. Results also show that CFOs' (audit committees') financial expertise is more strongly associated with restatements that correct unintentional error (intentional misstatement) than intentional misstatement (unintentional error). However, I find no evidence of significant associations between auditor quality and either restatements that correct unintentional error or intentional misstatement.

This research contributes to the emerging literature that examines variation in associations between type or severity of restatements and the influence of parties responsible for financial reporting quality. The new language-based proxy for restatements that correct unintentional error developed in this thesis will facilitate future research that uses type of restatement to proxy for constructs of interest.

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Finally, this dissertation would never, ever have been completed had it not been for the constant love and support of family and friends. Words of acknowledgment do not begin to express my never-ending gratitude.

Dedication

To the memory of my father, Douglas Anderson.

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Chapter 1 - Introduction

In this dissertation I develop and test a theoretical model of the determinants of unintentional error in audited financial statements of public companies. Inadvertent errors are not caused by management motivated to meet or beat benchmarks for personal gain or investor wellbeing. Hence, management is unlikely to resist correcting unintentional errors detected by auditors. Accordingly, a study in an unintentional error setting reflects on the competence and effort, rather than reporting incentives, of those responsible for financial statement preparation and audit. Caskey et al.'s (2010) theoretical paper warns that research designs common in empirical research bias estimates when management and audit effects are investigated separately. The model that I develop and test is more complete than the models used in most audit research. It includes characteristics of management, audit committees, and auditors. The purpose of this research is to shed more light on the roles played by those responsible for preventing and detecting unintentional error in audited financial statements.

Measures of unintentional error and intentional misstatement in audited financial statements are required to test the theoretical model developed in this study. Restatements (other than technical restatements such as those following mergers) provide direct evidence of low audit quality and the breakdown of internal control (e.g., DeFond and Jiambalvo 1991, Francis et al. 2013, Kinney 2000, Kinney and McDaniel 1989, Palmrose et al. 2004, PCAOB Office of Research and Analysis 2013a, Public Oversight Board 2000). The number of restatements rose rapidly after enactment of the Sarbanes-Oxley Act of 2002 (SOX) (Hennes et al. 2008, Whalen et al. 2013). So too did the number of restatement studies. Dechow et al. (2010, 374) in their

review of determinants and consequences of earnings quality proxies call for studies that partition restatements based on management intent because the "[g]enerally weak and mixed evidence across the determinants of restatements suggests that they are not a reliable indicator of intentional misstatements". The proportion of restatements that do not correct fraud or irregularities increased dramatically post-SOX (Burks 2010, Hennes et al. 2008) making empirical study of restatements that correct unintentional error feasible. Prior research uses refined proxies for restatements that correct intentional misstatement and partitions restatements based on various measures of restatement severity[1]. However, restatement literature lacks a directly determined proxy for restatements that correct unintentional error.

Chapter 2 further discusses the need for a directly determined proxy for restatements that correct unintentional error and describes the construction and validation of such a proxy. To date, restatements that correct error have been identified indirectly. For example, researchers treat a restatement as correcting error if the restatement is *not* associated with negative market reaction or SEC, Department of Justice, or other investigations. I take a more direct, language-based approach to the identification of restatements that correct unintentional error. In short, I conduct automated text searches of restatement announcements for language asserting or implying lack of management intent to identify restatements that correct unintentional error. As management intent is unobservable, such a language-based proxy is subject to measurement error. Even when management does not initially seek to deceive, overconfident management may optimistically bias earnings estimates and start down a "slippery slope" to intentional misreporting upon discovery that future earnings are not sufficient to cover income correction (Schrand and Zechman 2012, 312). What begins as "doing nothing more than legitimately managing earnings,

[1] Files et al. 2009, Hennes et al. 2013, and Huang and Scholz 2012 discuss severity measures including market reaction to restatement announcement and the direction of net income correction.

merely exploiting ambiguities in the accounting rules" may lead to management concealment schemes (Public Oversight Board 2000 Section 3.24). Given the measurement error associated with this language-based proxy, I validate the proxy using several tests suggested by restatement and earnings quality research (Dechow et al. 2010, Plumlee and Yohn 2010). I find for firm-years with auditor's SOX Section 404 opinions that restatements identified as correcting unintentional error are associated with less net income smoothing, less earnings persistence, and less positive accruals than other restatements. This new language-based proxy classifies less than one third of restatements as correcting unintentional error, in contrast to current proxies (e.g., Hennes et al. 2008, Plumlee and Yohn 2010) that classify over half of restatements as error correcting.

Chapter 3 reviews literature and develops and tests hypotheses to investigate the associations between restatements and auditor quality, audit committee expertise and chief financial officer (CFO) expertise and influence. In this chapter I argue for selecting for hypotheses development only these few characteristics of the players responsible for preventing and detecting unintentional error. To my knowledge the joint effects of auditor, audit committee, and management characteristics on restatements that correct unintentional error have not yet been studied. I find that associations between restatements and CFO and audit committee characteristics differ. CFO financial expertise and influence are associated with restatements that correct unintentional error, whereas audit committee financial expertise is associated with restatements that correct intentional misstatement. Furthermore, the associations between CFO financial expertise and influence depend on whether or not companies are undergoing organizational change. In periods of major organizational change restatements are decreasing in CFO financial expertise and influence while, consistent with CFOs turning their attention to

more strategic responsibilities in periods when financial reporting systems are not disrupted, restatements that correct unintentional error are increasing in CFO financial expertise and influence. Moreover, in a model that includes these interactions between restatements and CFO and audit committee characteristics, there is no support for predictions that either restatements that correct unintentional error or restatements that correct intentional misstatement are decreasing in auditor quality.

I draw more heavily on recent research to develop my hypotheses. The U.S. audit market has changed radically following the accounting scandals at the turn of the century. For example, PCAOB oversight has replaced auditor self-regulation, client acceptance processes have changed, and the market share of non-Big 4 auditors has increased (see Cassell et al. 2012, DeFond and Francis 2005, and DeFond and Zhang 2014 for a discussion of audit market changes). Further, audit committee composition and roles also changed dramatically following the accounting scandals. SOX Section 301 requires all members of audit committees to be independent and makes audit committees responsible for auditor appointment and compensation (Caskey et al. 2010, SEC 2003b, SOX Section 301 2002). Also, compensation penalties for CFOs following restatement and the reporting of internal control weaknesses became more severe (Collins et al. 2009, Li et al. 2010, Wang 2010) and the proportion of management compensation comprised of stock option grants decreased (Cohen et al. 2008). In addition, firms have increasingly adopted real earnings management techniques instead of accrual-based earnings management (Cohen et al. 2008). Moreover, the passage of the *Private Securities Litigation Reform Act of 1995* changed auditor liability rules from joint-and-several to proportionate liability and capped plaintiff damage claims: these changes have reduced the

frequency of frivolous lawsuits against non-culpable auditors with "deep pockets" (Lee and Mande 2003).

Chapter 4 summarizes findings of Chapters 2 and 3 and discusses this study's limitations, contributions, and opportunities for future research. A limitation of this research is that the language-based proxy used to identify restatements that correct unintentional error may be no better than existing proxies. In spite of this limitation this study makes several important contributions. First, it develops and tests a theoretical model of the determinants of unintentional error in audited financial statements which is more complete than models used in prior research. Second, in spite of proxy limitations, tests using this model extend prior research by showing that the association between unintentional error and CFO financial expertise and influence depends on whether or not companies are undergoing major organizational change. Third, this study also extends prior research by examining which characteristics of those responsible for financial reporting quality are more strongly associated with restatements that correct intentional misstatement versus restatement that correct unintentional error in a more complete model of financial reporting quality. Finally, this study contributes a new, direct method of proxy construction of restatements that correct unintentional error that is automated and hence, replicable, scalable, and transparent.

One useful extension of this study would be to determine whether or not this language-based proxy classifies restatements more accurately than prior proxies. Another extension would be to investigate whether or not the differences in strengths of associations between the characteristics of those responsible for financial reporting and restatements that correct intentional misstatement versus restatements that correct unintentional error observed in this study are found when the sample is expanded to include non-accelerated filers. Even though

prior research finds market reaction to error restatement is smaller than to fraud and irregularity misstatement (Hennes et al. 2008, Palmrose et al. 2004), understanding the determinants of unintentional error in audited financial statements is important. Unintentional errors in audited financial statements may reflect the presence of errors in internal management reports used by managers for day-to-day operational decisions (Feng et al. 2009) and deficiencies in internal controls that affect both financial reporting and operations (Feng et al. forthcoming).

Chapter 2 - Identifying Unintentional Errors in Restatement Disclosures

2.1 Introduction

The Public Company Accounting Office Oversight Board (PCAOB) made identifying audit quality indicators a priority project for 2013 (Hanson 2013, PCAOB 2012b). In a background paper prepared for the project, the PCAOB's Office of Research and Analysis observes that "[r]estatements that correct misstatements are a relatively solid indicator of audit quality" (PCAOB Office of Research and Analysis 2013, 23) [2, 3]. The background paper does not address whether restatements are a better indicator of an auditor's error detection ability or an auditor's ability to detect earnings management attempts. It is rare to find an archival research design (an exception being Keune and Johnstone 2012) that supports conclusions about whether an auditor quality measure is associated more strongly with detection ability or with the ability to resolve detected misstatements[4]. Since management has little reason to resist correction of auditor detected unintentional error, research designs that test the relationship between auditor quality indicators (e.g., auditor size, auditor industry experience, and PCAOB inspection outcomes) and restatements that correct only *unintentional error* would better support conclusions about auditor detection abilities. Similarly, testing associations between restatements

[2] The PCAOB states in its 2012-2013 strategic plan that "[i]nitiating a project to identify audit quality measures, with a longer-term goal of tracking such measures with respect to domestic global network firms and reporting collective measures over time" is a "priority project" for 2013 (PCAOB 2012b, 5).

[3] The background paper includes an Appendix summarizing work done related to audit quality indicators by the United States Department of the Treasury's Advisory Committee on the Auditing Profession, the International Auditing and Assurance Standards Board, The United Kingdom's Financial Reporting Council, and the PCAOB's Investor Advisory and Standing Advisory Groups.

[4] Knechel et al. (2013) overview literature on associations between restatements and audit quality.

that correct unintentional error and management and audit committee expertise and incentives may lead to a more nuanced understanding of internal control and governance mechanisms. Unfortunately, while restatement research has developed refined proxies for restatements that correct *intentional misstatement* (Dechow et al. 2011, Hennes et al. 2008, Plumlee and Yohn 2010), a direct method to construct a proxy for restatements that correct *unintentional error* is not available.

Some restatement announcements directly assert, or otherwise imply, that the errors corrected by the restatements are unintentional. Although text search and content analysis methodologies are increasingly being used in accounting research (Fisher et al. 2010), such analysis of the language used in restatement announcements has not been applied to classify restatements. Instead, restatement studies either presume that all restatements correct intentional misstatements (Dechow et al. 2010 p. 374, Hennes et al. 2008 p. 1488) or identify error correcting restatements as the restatements that remain after restatements that correct intentional misstatement are subtracted from all restatements. For example, Hennes et al. (2008) subtract restatements that are attributed to fraud or irregularities from all 8-Ks announcing restatements to correct misapplications of GAAP (filed from 2002 through 2005) and label the remaining three quarters of restatements as restatements that correct error.

The purpose of this chapter is to develop and validate a language-based proxy for restatements that correct unintentional error in annual financial statements. To begin, I developed a list of words and phrases that are indicators of unintentional error in restatement announcements. I prepared this list by reading several hundred restatements announced after 2002 (the year of *Sarbanes-Oxley Act (SOX)* enactment*)* and coded by *AuditAnalytics* as "errors: accounting and clerical applications". Then, using this list of words and phrases, I conducted

automated text searches[5] of the 10,623 restatement announcements in the *AuditAnalytics* database that were disclosed post-2002. Based on the results of these text searches, I coded approximately one fifth of the restatement announcements as corrections of unintentional error. Next, I coded restatements as intentional misstatements based on my review of all U.S. Security and Exchange Commission (SEC) Accounting and Auditing Enforcement Releases (AAERs) released after December 31, 2002. After coding the restatements as correcting unintentional error, intentional misstatements, both, or neither (i.e., the unclassified restatements), I downloaded financial data for all available years from the COMPUSTAT Unrestated Quarterly ("As First Reported") – US database, and matched the coded restatement data with financial data, matching on the earliest fiscal year when the restated period extends beyond a single fiscal year. Finally, I dropped restatements lacking financial data (4,388), quarterly restatements (1,649), multiple instances of firm-year restatements (93), and foreign business and holding company restatement firm-years (786).

Three samples drawn from the remaining 3,707 restatements (751 unintentional error corrections, 127 intentional misstatement corrections, and 2,829 unclassified restatements that are not identified by the restatement announcements and AAERs searches), are used in proxy validation tests. The 751 unintentional error corrections (UE) and the 127 intentional misstatement corrections (IM) comprise one sample (the UEvsIM sample). Two other samples, comprised of restatements with auditor's SOX 404 reports issued in the earliest year in the restatement period, are also used in validation tests. These two samples are validated because internal control quality is a variable of interest in my dissertation research and SOX 404 opinions are a frequently used proxy for internal control quality. One of these samples (the UEvsUN

[5] I conducted the search in November, 2012.

sample) is comprised of 243 unintentional error correcting (UE) and 588 unclassified (UN) restatements with auditor SOX 404 opinions. The final sample (the UEvsOther sample) consists of this UEvsUN sample plus the 17 restatements that correct intentional misstatement firm-years that have auditor's SOX 404 reports. There are currently an insufficient number of intentional misstatement restatements with auditor's SOX 404 reports identified by AAER searches (17) to construct the internal control quality proxies needed for Chapter 3 tests. Hence, unclassified restatements in the UEvsOther sample (or a subset of such restatements) are also used in Chapter 3 tests to proxy for restatements that correct intentional misstatements.

I developed tests based on prior restatement and earnings quality research to validate the new proxy. I predicted, relative to restatements that correct intentional misstatement, that unintentional error restatements will correct proportionately fewer revenue recognition issues (as coded by *AuditAnalytics*) and change net income less frequently. I also predicted that, relative to firm-years restated to correct intentional misstatement, unintentional error firm-years will also be characterized by less income persistence, less income smoothing, less positive accruals, and a lower likelihood of meeting or beating analysts' income forecasts by a small amount.

I find in validation tests using the UEvsIM sample (i.e., the sample of firm-years without auditor's SOX 404 reports) that, in relation to restatements that correct intentional misstatements, the error-correcting restatements are associated with proportionately fewer revenue recognition issues, less positive accruals, and a lower likelihood of meeting or beating analysts' income forecasts. Further, I find in validation tests using the UEvsUN sample (i.e., the sample of firm-years with auditor's SOX 404 reports) that, in relation to unclassified restatements, restatements that correct unintentional error are associated with less net income smoothing, less income persistence, and less positive accruals. Moreover, these UEvsUN sample results are the same for

the UEvsOther sample (i.e., the UEvsUN sample plus the 17 restatements that correct intentional misstatement firm-years with SOX 404 reports). Thus, I conclude that my method of directly identifying error correcting restatements based on the language in restatement announcements constructs a valid proxy for restatements that correct unintentional error when auditor's SOX 404 reports are available for at least the first year of the restated period.

This language-based proxy construction method contributes to the restatement literature. Constructing the new proxy uses publicly available information, the language in restatement announcements that asserts or implies lack of management intent. This information has been largely ignored in classifying restatements to date. Since automated text searches are used, proxy construction may be easily replicated[6] and scaled to search a greater number of restatements at little additional cost as more restatements become available. This proxy classifies less than one third of restatements with auditor's SOX 404 reports filed for the first restated year as correcting unintentional error. In contrast, Hennes et al. (2008) classify three quarters of their 8K-based sample as error restatements. Plumlee and Yohn (2010) classify over half of restatements -- restatements not attributed to manipulation, transaction complexity, lack of clarity in accounting standard, or the use of judgment in applying a standard -- as correcting "internal errors". A limitation of automated text search is the sensitivity of search results to the choice of the words and phrases used to conduct the search (Kuechler 2007). In this study, validation test results are robust to using both a longer and shorter list of search words and phrases. A further potential limitation is reliance on management assertions in restatement announcements. If management misrepresent their intent, or are "optimistically biased" and let what begins as unintentional error become intentional misstatement (Schrand and Zechman 2012), language in restatement

[6] Appendix A lists the words/phrases used in the automated searches.

announcements may mischaracterize the nature of the restatement correction[7]. These potential limitations aside, the proxy for restatements that correct unintentional error developed in this paper may enhance the power of tests used to study associations between unintentional error and the expertise and incentives of CFOs, audit committees, and auditors. While I conclude that my automated search of restatement announcements for words and phrases indicating lack of intent results in a valid proxy for restatements that correct unintentional error, I leave to future research the study of research settings for which this language-based proxy is best suited and the effects of proxy choice on research findings. In particular, an extension of this research might be to investigate differences in the descriptive statistics and consequences of restatements classified by searching for words and phrases asserting or implying lack of intent rather than focusing on market reaction, indicators of fraud, irregularities, complexity and standards' issues as done in prior research (e.g. Files et al. 2009, Hennes et al. 2008, Plumlee and Yohn 2010).

2.2 Background

Given that "[t]he basic recordkeeping function embodied in the modern journal entry lies at the core of all accounting systems" (Basu and Waymire 2006, 202), and recordkeeping requirements are embodied in statute and professional guidance (e.g., PCAOB Auditing Standard No. 5, Securities and Exchange Act of 1934 Rule 13b2 [SEC 2005], and Canadian Business Corporations Act of 1985), it is surprising that there is virtually no archival research on unintentional errors. Some of these errors are never identified but some are discovered and, if material, can lead to restatements. In comparison with intentional misstatements, unintentional errors (1) are associated with market reaction of a smaller magnitude (Hennes et al. 2008;

[7] In addition to the search of AAERs undertaken in this study, future research could also search court rulings and the business press for indicators of intentional misstatements.

Palmrose et al. 2004), (2) are more likely to be associated with weak internal controls (Ashbaugh-Skaife et al. 2008), (3) occur more frequently (Plumlee and Yohn 2010), (4) affect a broader range of accounts (Plumlee and Yohn 2010), and (5) are as likely to lead to auditor turnover if restatement occurs (Hennes et al. 2014).

External users and audit committee members may be able to rationally infer the presence of intentional misstatements made when management is motivated to manage earnings or commit fraud to meet certain objectives (Dechow et al. 2010). However, unintentional errors are less transparent and hence, may be more likely to result in decision errors. Furthermore, unintentional errors may reflect the competence rather than the integrity of management and auditors. Whereas intentional misstatements may be partially attributed to management incentives[8], unintentional errors indicate either management is less able (Demerjian et al. 2013) or less incented to implement and maintain effective controls over financial reporting (Balsam et al. 2014, Hoitash et al. 2012). Management is likely to react differently to auditor detection of intentional vs. unintentional misapplications of GAAP. Management motivated by concerns such as shareholder reaction to missing analysts' forecasts, the effects of unexpected financial results on merger negotiations, and the labour market penalties for poor financial results is more likely to resist correcting intentional misstatement. Since unintentional error is unlikely to be associated with such incentives, management should be less resistant to correcting material, auditor detected, unintentional errors. Accordingly, restatements that correct unintentional error indicate not only that management has failed to implement and maintain effective controls over financial reporting but also that the auditor lacks the requisite skills to plan and conduct an effective audit.

[8] Armstrong et al. (2010) overview the literature investigating associations between executives' equity incentives and accounting irregularities.

A broad range of research over the past thirty years shows that unintentional errors in published financial statements may occur more frequently than intentional misstatements. As Scott A. Taub, Acting Chief Accountant of the SEC, observed, over half of all restatements by accelerated filers during the early years of SOX reporting were caused by "ordinary books and records deficiencies or by simple misapplications of the accounting standards".[9] Eilifsen and Messier (2000) summarize the results of six pre-SOX studies of auditor detected differences before the issuance of financial statements and find that judgment error led to less than one-third of the errors, whereas over half related to personnel issues, lack of accounting knowledge, and mechanical error/routine clerical errors. Bell et al. (1998), using auditor-supplied data, find that audit differences relate more frequently to incorrect data capture and manual computation than to management judgment and that computerized processes are associated with more unintentional errors than non-computerized processes.[10]

Some restatement announcements include managements' explanations for the misreporting. For example, The AES Corporation filed a Form 8-K on July 27, 2005 that includes this restatement explanation:

> *On July 27, 2005, the Company announced via press release that it is reviewing certain accounting practices and previously reported financial statements as a result of possible errors discovered by management of the Company.*
>
> *As a result of the continuing evaluation of the Company's deferred income tax accounting and reconciliation controls process disclosed in the Company's 2004 Form 10-K, the Company announced that it would restate its 2002, 2003, 2004 and first quarter 2005 financial statements.*

[9] Scott A. Taub, Acting Chief Accountant of the SEC, in his speech at the Financial Executive International Meeting on November 17, 2006 (http://www.sec.gov/news/speech/2006/spch111706sat.htm) which was cited by Hennes et al. (2008).

[10] Bell et al. (1998) attributed this finding to automation's magnification of previously existing personnel and other control weaknesses. Many organizations depend on fragmented financial reporting systems that combine new technologies with legacy systems (Wagner and Dittmar 2006). Consequently, both automated and non-automated processes co-exist (e.g., in many period end and close processes) in even the most sophisticated systems today.

The adjustments requiring restatement primarily relate to the accounting treatment for deferred taxes associated with certain acquisitions completed prior to 2001…. These adjustments primarily impact deferred tax balances, fixed assets and the other comprehensive income portion of stockholder's equity as well as income tax expense, depreciation expense and foreign currency gains and losses on the remeasurement of deferred taxes.

In addition, accounting calculation errors were identified related to our subsidiary in Cameroon resulting in adjustments that primarily will impact the balance sheet fixed asset and currency translation accounts.

Based on management's review, it believes that all errors were inadvertent and unintentional. The Company has not completed its analysis and has not yet determined the final amount and nature of the adjustments.

The previously issued financial statements and report of the Company's independent registered public accounting firm, Deloitte & Touche L.L.P., should no longer be relied upon. The Company will file an amended 2004 Form 10-K and an amended first quarter 2005 Form 10-Q reflecting the restated amounts as soon as practicable. The decision to restate prior financial statements was made on July 26, 2005 by the Audit Committee of AES's Board of Directors, upon the recommendation of management and has been discussed with Deloitte & Touche L.L.P.

The above example shows that restatement disclosure language (e.g., "unintentional" and "inadvertent") may indicate a lack of intent. However, when Plumlee and Yohn (2010) read restatement announcements to classify restatements they looked for indications of manipulation, complexity, clarity of standard, and use of judgment in applying a standard rather than looking for language disclosing lack of intent. They labeled restatements without such indications, i.e., restatements that were "left over" from their classification process, as "internal error" restatements. Based on their hand-coding of 3,744 restatements filed from 2003 through 2006, Plumlee and Yohn (2010) attribute 3% of restatements to manipulation, 3% to transaction complexity, 37% to lack of clarity in accounting standard or to the use of judgment in applying a standard, and 57% to basic internal company errors. Their internal company error category includes not only "books or records deficiency and simple misapplication of generally accepted accounting standards", but also disclosures that include "no discussion that suggests these errors were intentional" (Plumlee and Yohn 2010, 46).

I argue that it may be possible both to automate the restatement classification process with respect to unintentional errors and to refine the classification by conducting automated searches for words and phrases such as "unintentional" and "inadvertent" in restatement announcements. However, it is not sufficient to show there are words and phrases indicating lack of intent in restatement disclosures. It is also necessary to show that such language has discriminatory power to differentiate restatements that correct unintentional error from both restatements that correct intentional misstatements and unclassified restatements. It would also be desirable to show that the resulting classification is superior to alternative approaches, but this is beyond the scope of this study.

2.3 Proxy Validation Tests Development

In this section I develop six validation tests to examine differences between restatements that correct unintentional error and restatements that correct intentional misstatement. The first two tests are based on Plumlee and Yohn's (2010) detailed analysis of restatement reasons and income effects. An additional four validation tests are based on Dechow et al.'s (2010) discussion of four earnings quality proxies: earnings persistence, accrual quality, income smoothness, and benchmarks (observations at or slightly above targets).

Plumlee and Yohn (2010) identify revenue recognition issues as a cause of 53% of the intentional manipulation restatements (3% of their restatement sample). Dechow et al. (2011) report a similar proportion (59.5%) of intentional misstatements in AAERs. In contrast, Plumlee and Yohn (2010) find revenue recognition issues are disclosed in only 8% of the internal error restatements that comprise 57% of their restatement sample. These findings suggest the first validation test:

> ***V1:*** *Intentional misstatement restatements have a greater proportion of revenue recognition issues than unintentional error restatements.*

My second prediction is based on the intuition that companies that opportunistically misstate are more likely to bias income reporting than companies making unintentional errors. Tucker and Zarowin (2006) and Leuz et al. (2003) review literature that concludes management frequently engages in income smoothing that leads to both income overstatements and understatements. Plumlee and Yohn (2010) find that the proportion of restatements that corrects intentional manipulation and restates income is greater than the proportion of intentional manipulation restatements that has no income effect. While unintentional error may also distort income upwards and downwards, Plumlee and Yohn (2010) find that the proportion of restatements that corrects "internal error" without changing income is greater than the proportion of "internal error" restatements that changes income. The intuition that intentional misstatement is more likely to bias income reporting than unintentional error, combined with these prior research findings, leads to my second validation test:

> ***V2:*** *Proportionately more intentional misstatement restatements than unintentional error restatements correct net income as originally reported.*

"Persistence may be achieved in the short run by engaging in earnings management" (Dechow et al. 2010, 351). I expect the motivations of management to manage earnings will lead to more persistent earnings for firms with restatements that correct intentional misstatement than for firms with restatements that correct unintentional error. I also expect staffing or system limitations that lead to unintentional error will hamper any earnings management attempts. Management relies on accurate, reliable records of *unmanaged* earnings for managing earnings towards desired amounts. Inaccurate records contribute to overshooting and undershooting targets. Accordingly, as record-keeping errors come to light in subsequent periods, I expect record-keeping corrections and estimation adjustments to be reflected in less persistent earnings trends, leading to my third validation test:

> ***V3:*** *Intentional misstatement restatements are associated with greater income persistence than unintentional error restatements.*

If, as intuition suggests, intentional misstatement biases income upwards more than downwards, unintentional error restatements may be associated with less positive accruals than intentional misstatement restatements. Consistent with this intuition, Plumlee and Yohn (2010) find that restatements that correct manipulations are more likely to correct overstated income. Further, many prior research studies conclude that greater discretionary accruals are also associated with earnings management. For example, Dechow et al. (2011) find that 1979-2002 firm-years with AAER-alleged GAAP violations which overstate income, have more positive accruals than firm-years without such violations. Dechow et al. (2011, 18) conclude that, "manipulating firms have more ability to change and adjust assumptions to influence short-term earnings". Prior research also finds greater accrual magnitude (absolute value) is associated with intentional manipulation. However, lower accrual quality is also associated with internal control

weaknesses such as understaffing and/or poorly designed and maintained systems (e.g., Ashbaugh-Skaife et al. 2008, Doyle et al. 2007a). I do not predict *absolute* accrual magnitude will differ between intentional misstatement and unintentional error restatements but rather that "internal control weaknesses are more likely to lead to unintentional errors that add noise to accruals than intentional misstatements that bias earnings upwards" (Ashbaugh-Skaife et al. 2008, 217). Thus, my fourth validation test is:

> ***V4:*** *Unintentional error restatements are associated with less positive accruals than intentional misstatement restatements.*

The fifth validation test draws from income smoothing research. While many earnings management studies begin with the premise that earnings are managed to mislead, Tucker and Zarowin (2006) conclude income is smoothed to improve informativeness. Using analogous arguments to those presented in developing *V3*, the persistence validation test, I argue errors caused by books and records deficiencies and inadequately trained or resourced accounting personnel make it more difficult to predict future earnings and hamper management's income smoothing attempts. This reasoning leads to my fifth validation test:

> ***V5:*** *Intentional misstatement restatements are associated with greater income smoothing than unintentional error restatements.*

Arguments for my final prediction parallel those made for my income persistence and income smoothing predictions. Management motivated to manage earnings to benchmarks, such as a previous year's earnings per share or analysts' forecasts, may be less successful in doing so if record-keeping weaknesses lead to unintentional error. Feng et al. (2009) find that firms reporting ineffective internal controls provide less accurate management guidance. This reasoning leads to my last validation test:

> ***V6:*** *Meeting or beating a benchmark by a small amount is associated proportionately more frequently with intentional misstatement restatements than unintentional error restatements.*

2.4 Method and Sample Selection

2.4.1 Unintentional error identification

I used automated text searches to identify restatements likely to correct unintentional error in a sample comprised of the 10,623 restatements in the *AuditAnalytics* database[11] disclosed after 2002 (the year of SOX enactment). To identify error-correcting restatements I used *AuditAnalytics'* text search tool and a list of error indicators (i.e., words and phrases) that I believe are likely to be associated with the disclosure of restatements that correct unintentional error. For example, The AES Corporation restatement announcement extract presented in the background section of this chapter would be coded as an unintentional error as it contains the words "inadvertent" and "unintentional". Appendix A describes in detail the method that I used to create the list and includes the complete list of error indicator words and phrases used in the text searches. In summary, to create the list I began by reading over 200 restatement

[11] Scholtz (2008) in a report on restatements between 1997 and 2006 prepared for the US Treasury, finds *Audit Analytics* includes "nearly all restatements captured in the GAO lists and Lexis-Nexis searches, and some that are not identified through these methods".

announcements that *AuditAnalytics* manually coded as "Errors: accounting and clerical applications" looking for error indicators – language indicating lack of intent - and concluded by grouping into nine groups for exposition purposes all the error indicators in these 200 restatement announcements. The nine groups, together with the number of post-2002 restatement announcements identified by text search for one or more of the group's error indicators and examples of indicator words and phrases (see Appendix A for a complete list of error indicators), are as follows: calculation (579 – e.g., error in the calculation, incorrectly calculated, and computational error); unintentional error - explicitly stated (357 – e.g., inadvertent, mistake, and unintentional); information system (203 – e.g., accounting system, spreadsheet, and bookkeeping); immaterial (202 – e.g., immaterial error, immaterial correction of an error, and miscellaneous corrections); clerical (196 – e.g., clerical error, typographical error, and filing error); reconciliation (51 – e.g., account reconciliation and reconciliation errors); and administrative (18 – e.g., administrative error and administrative oversight); recording (790 - e.g., not recorded, error in recording, posting, and recorded twice); and classification (1,582 – e.g., classification, reclassified).

In total, 2,048 restatements of the 10,623 restatements (approximately one fifth of restatements) disclosed after 2002 are coded as correcting unintentional errors. These 2,048 error-correcting restatements are identified by searching restatement announcements for all error indicators shown in Appendix A except those in the classification group. Validation tests results for the UEvsIM, UEvsUN, and UEvsOther samples are robust when text searches for error-correcting restatements include (exclude) classification group error indicators (the reporting group and classification group error indicators).

Restatements that correct both intentional misstatement and unintentional error present a classification challenge. By reviewing post-2002 AAERs (a review that is described in more detail below), I find 56 of the 2,048 error-correction restatements also correct intentional misstatement. I chose to code these 56 restatements as correcting unintentional error, thus ensuring that each restatement is included in only one classification and that anyone using this language-based proxy for restatement that correct unintentional error in the future could do so without considering SEC AAER allegations. An alternative approach to constructing a language-based proxy for restatements that correct unintentional errors would be to deduct from error-correcting restatements any restatements that also correct intentional misstatement. My classification of these 56 restatements as error-correcting biases against finding predicted validation test results for the UEvsIM sample. Similarly, any restatements classified as error-correcting that also correct intentional misstatements that do not come to light by a review of SEC AAERs will bias against finding predicted validation test results for the UEvsUN sample. Results of validation tests for the UEvsIM, UEvsUN, UEvsOther samples are robust to either coding these 56 restatements that correct both error and misstatement as intentional misstatements, or to excluding these 56 restatements from analyses.

Type I and II identification errors of all automated text search strategies are affected not only by subjectivity in the choice of words/phrases in the search dictionary (i.e., the list in Appendix A) but also by the choice of the search tool. I decided to use a simple text search strategy and *AuditAnalytics'* text search tool instead of more complex search logic and specialized content analysis software. This decision, while a limitation, permits others to replicate the identification process without importing restatement announcements into

specialized content analysis software. Appendix A presents the list of error indicators formatted for use with *AuditAnalytics'* text search tool.

2.4.2 Intentional misstatement identification

I visually scanned all AAERs issued after December 31, 2002 included in the Securities and Exchange Commission website (www.sec.gov/divisions/enforce/friactions.shtml) to determine if the AAER referenced a company for a time period matching any of the restatements. I found 357 AAERs that include the name of the restating company where the time period for the AAER allegations of auditing or accounting misconduct overlaps the restatement time period. Upon reading these 357 AAERs for allegations of senior management fraud, violation of anti-fraud provisions or language implying senior management intent (e.g., words such as "scheme", "manipulated and concealed", and "knew or should have known"), I coded 276 of the matched restatements as correcting intentional misstatements. [12] Fifty six of these restatements are also identified as correcting unintentional error. As discussed above, results of validation tests using the UEvsIM, UEvsUN, and UEvsOther samples are robust to both excluding these 56 restatements and classifying them as either intentional misstatements or unintentional error restatements.

2.4.3 Financial data source and sample creation

Financial data required for variables was obtained from the COMPUSTAT Unrestated Quarterly ("As First Reported") – US database, a database reflecting "what investors see and

[12] I do not code as intentional misstatements 81 restatements matched to AAERs which allege foreign corrupt practices by subsidiaries, employee fraud, and/or stock option backdating, or misleading representations without other allegations of senior management fraud or intent. Results are robust to including these restatement-matched AAERs as restatements that correct intentional misstatements in the main statistical analyses.

react to on the SEC filing date, untainted by restatements" (Callen et al. 2006, 1025). Financial data, available from 1987 onwards, was merged with restatements, matching financial data for the earliest fiscal year in the restated period when restatements spanned more than one fiscal year. As shown in Table 1, fourth quarter Total Asset data and four quarters of both unrestated and restated quarterly Net Income data[13] is unavailable from the Unrestated Quarterly database for 4,388[14] of the 10,623 restatements.

Appendix B illustrates sample selection decisions when a company's restatement announcements include quarterly restatements or multiple restatements that correct the same firm-year. Only restatements that correct annual statements are retained in the UEvsIM, UEvsUN, and UEvsOther samples. Restatements that correct annual filings were identified by checking that the restated period spans the month COMPUSTAT codes as the month of the fiscal year end. Lobo and Zhao (2013) find that including restatements of unaudited quarterly reports confounds study of the relation between audit effort and subsequent restatements. The UEvsIM, UEvsUN, and UEvsOther samples include only one restatement per firm-year. When more than one restatement corrects a firm-year, the restatement retained is selected by considering the type of misreporting corrected, the "begin date" of the restated period, and the restatements disclosure date as explained in Appendix B.

Table 1 summarizes the method used to arrive at a sample of 3,707 restatements for firm-years from 1987 through 2012 (i.e., 751 unintentional error restatements, 127 intentional misstatements restatements, and 2,829 unclassified restatements) and the subsample of 848

[13] Net income data is required for four of the six validation tests and asset data is required as a control in multivariate analyses used in three validation tests. Only the revenue recognition validation test could be conducted without net income or asset data.

[14] The *AuditAnalytics* database includes restatements for over the counter and non-US issuers.

restatements for firm-years with auditor's SOX 404 reports (i.e., 243 unintentional error restatements, 17 intentional misstatements restatements, and 588 unclassified restatements). The UEvsIM sample is comprised of the 751 unintentional error restatements and the 127 intentional misstatement restatements as described in Table 1 (footnote h). The UEvsUN sample is comprised of the 243 unintentional error restatements and the 588 unclassified restatements as described in Table 1 (footnote i). The UEvsUN sample includes only restatements with auditor's SOX 404 reports for at least the first year of the restated period. The UEvsOther sample is comprised of the UEvsUN sample plus the 17 restatements that correct intentional misstatement in firm-years with auditor's SOX 404 reports.

The UEvsUN sample is required for this study because auditor's SOX 404 reports are needed to construct internal control quality proxies to test the hypotheses developed in Chapter 3. As discussed in Chapter 1, following SOX enactment, the relative frequency of error-related restatements increased dramatically as the number of "innocuous" restatements increased (Burks 2010, Hennes et al. 2008), "labour market penalties for former CFOs of restating firms" became more severe (Collins et al. 2009, 1), and auditing dramatically changed from being a self-regulating profession to being overseen by the PCAOB (e.g., Defond and Francis 2005). In particular, SOX 404b requires that an auditor opine on the effectiveness of internal controls over financial reporting for accelerated filers (companies with more than $75 million in market capitalization). Differences in the audits of accelerated and non-accelerated filers stemming from SOX 404b, one of the reasons for the 2007 change from Auditing Standard 2 to Auditing Standard No. 5[15], may be reflected in restatement differences. However, there are too few (17)

[15] The SEC announcement of the PCAOB approval of Auditing Standard No. 5 discusses the reduction in the number of mandatory requirements from Auditing Standard No. 2 http://www.sec.gov/news/press/2007/2007-144.htm.

restatements that correct intentional misstatements with corresponding SOX 404 auditor's reports to conclude on the statistical significance of validation test results for a sample comprised of restatements with SOX 404 auditor's reports that correct intentional misstatements (17) and unintentional errors (243). Since unclassified restatements correct a mix of intentional misstatement and unintentional error, significant differences in validation tests between unclassified restatements and restatements that correct unintentional error also validates using a language-based proxy for restatements that correct unintentional error.

2.5 Variable Definitions

Table 2 describes the variables used in tests and their construction. The choices of variables used in tests are discussed in the results section.

2.6 Descriptive Statistics

Table 3 reports descriptive statistics for the three samples used in validation tests. The UEvsIM sample is comprised of the 751 restatements that correct unintentional error and the 127 restatements that correct intentional misstatements in the 1987 – 2012 time period. The UEvsUN sample is comprised of restatements for firm-years with SOX 404 auditor's reports that correct unintentional error (243) and unclassified restatements (588). The UEvsOther sample is the UEvsUN sample plus the 17 restatements for firm-years with auditor's SOX 404 reports that correct intentional misstatements.

2.6.1 Financial statement comparisons

Table 3 presents descriptive statistics derived from amounts reported in financial statements and compares the means (*t*-test), medians (Wilcoxon ranksum test) and distributions (Kolmogorov-Smirnov test) of unintentional error firm-years to intentional misstatement firm-years (1987–2012) using the UEvsIM sample and, for firm-years with SOX 404 auditor's reports, to unclassified restatements using the UEvsUN sample and to unclassified restatements and intentional misstatements using the UEvsOther sample. Panel A reports comparisons for the unintentional error restatements and the intentional misstatement restatements in the UEvsIM sample. Panel A's Wilcoxon ranksum tests show that, relative to intentional misstatement firms, unintentional error firms are smaller, slower growing, and less profitable, i.e., unintentional error firm-years are characterized by smaller total assets, receivables, inventories, market capitalization, sales, sales growth, and income ($p<0.05$) in the 1987 through 2012 time period. In contrast, Panel B of Table 3 shows for the firm-years in the UEvsUN sample, that the unintentional error and unclassified firm-years with auditor's SOX 404 reports are the same ($p>0.05$) on all of the 15 reported median measures with one exception: the median return on assets is lower for restatements that correct unintentional error than for unclassified restatements. *T*-tests and Wilcoxon ranksum tests results of comparisons using the UEvsOther sample (Panel C) are consistent with the results shown in Table 3 for the UEvsUN sample (Panel B).

2.6.2 Market capitalization, restatement year, and industry differences

Table 4 examines the differences in market capitalization, restatement year and industry between unintentional error and intentional misstatement firm-years. The table is modeled after Dechow et al.'s (2011) Table 2 which, in turn, attributes the industry classification (detailed in

the footnote to Table 4) to Frankel et al. (2002). Panel A shows that over half (54.2%) of the intentional misstatement firm-year sample are in the top three market capitalization deciles, deciles where pressure to meet market expectations may be the greatest. In contrast, approximately one third of unintentional error firm-years and unclassified restatement firm-years are in these top three deciles. Panel B shows that for the UEvsIM comparison that the majority (74.0%) of intentional misstatement firm-years are in the pre-SOX 404 years (1987 – 2003) whereas the majority of unintentional error firm-years (62.6%) are years in which SOX 404 reporting requirements are in effect (2004 - 2012). The UEvsUN (UEvsOther) post-SOX comparisons reveal no consistent pattern of differences between restatements that correct unintentional error and unclassified restatements (unclassified plus intentional misstatement restatements). Panel C shows 29.9% of intentional misstatement firm-years vs. less than 15% of unintentional error and unclassified firm-years are in the computer industry, the industry for which Dechow et al. (2011) report the most misstatements. This difference is the reason that restatements in five of the twelve industries (i.e., the durable manufacturers, computer, banks and insurance, services, and retail industries) account for approximately 80% of the intentional misstatements vs. approximately 64% of the unintentional error restatements in the 1987 – 2012 time period. In the post-SOX time period, relative to other restatements, restatements that correct unintentional error are proportionately more (less) frequent in the mining and construction and durable manufacturers industries (banking and insurance) industries ($p<0.11$ not in tables). Overall, Table 4 shows the need to control for company size, industry, and restatement year in multivariate validation tests conducted using the UEvsIM, UEvsUN, and UEvsOther samples.

2.7 Results – Proxy Validation Tests

2.7.1 Accounts corrected by restatements

unintentional error vs. intentional misstatement

Table 5 compares the proportion of accounts (as coded by *Audit Analytics* with some codes combined for table conciseness) corrected by unintentional error vs. intentional misstatement restatements (the UEvsIM sample), for unintentional error vs. unclassified restatements with SOX 404 auditor's reports (the UEvsUN sample), and for unintentional error vs. other restatements (both intentional misstatements and unclassified restatements) with SOX 404 auditor's reports (the UEvsOther sample). Overall, the reasons for unintentional error restatements are wide-spread. Supporting *V1*, revenue recognition issues (the issue Plumlee and Yohn (2010) identify as a restatement cause of 53% of manipulation restatements and Dechow et al. (2011) identify as an issue in 59.5% of misstatements) affect 44.1% of intentional misstatement firm-years vs. 18.4% of unintentional error firm-years ($p<0.001$) in the UEvsIM sample comparisons.

unintentional error vs. unclassified restatements

On the other hand, when the UEvsUN (UEvsOther) sample is used in comparisons of the accounts corrected by restatement, there is no difference ($p>0.05$) in the proportion of revenue recognition issues for error-correcting restatement firm-years vs. unclassified restatement (unclassified plus intentional misstatement restatement) firm-years with auditor's SOX 404 reports.

In summary, when using the UEvsIM sample (UEvsUN sample) the revenue recognition test results support (do not support) *V1*. Even when unclassified restatements with auditor's SOX 404 reports are combined with the 17 firm-years that restate intentional misstatements with auditor's SOX 404 reports in the UEvsOther sample, the proportion of revenue recognition issues does not differ (p>0.8 not in tables) between restatements with auditor's SOX 404 reports that correct unintentional errors and the other restatements.

2.7.2 Restatement income correction

unintentional error vs. intentional misstatement

Table 6 compares the correction of net income, income before extraordinary items and operating income for unintentional error vs. intentional misstatement restatements (the UEvsIM sample), for unintentional error vs. unclassified restatements with SOX 404 auditor's reports (the UEvsUN sample), and for unintentional error vs. other restatements (both intentional misstatements and unclassified restatements) with SOX 404 auditor's reports (the UEvsOther sample). Panel A of Table 6 reports the results for the UEvsIM comparisons and shows weak support for *V2*. The proportion of unintentional error restatements in the UEvsIM sample that does not correct net income is greater (67.7%) than the proportion of intentional misstatement restatements that does not correct net income (57.5%) (p<0.05). While similar comparisons for the income before extraordinary items and operating income measures show directional differences consistent with that shown for net income, the differences are not significant at conventional levels (p>0.05). Income overstatement and understatement corrections do not differ proportionately between unintentional error and intentional misstatement groups on any of the three income measures. While Table 6 shows that unintentional error restatements change net

income less frequently than intentional misstatement restatements, Panel B shows that unintentional error restatements and intentional misstatement restatements correct similar amounts of income (relative to company size) (p>0.05) in the 1987 – 2012 time period.

unintentional error vs. unclassified restatements

Panels A of Table 6 shows corrections of net income, income before extraordinary items and operating income are the same (p>0.05) in the UEvsUN (UEvsOther) sample for unclassified restatements with SOX 404 auditor's reports (unclassified restatements plus intentional misstatement restatements with SOX 404 auditor's reports) and unintentional error restatements with SOX 404 auditor's reports. Furthermore, the insignificant results in Panel A for the UEvsUN (UEvsOther) sample are not in the direction predicted for the validation test, i.e., unintentional error restatements correct income proportionately more, but not significantly more (p>0.05), than unclassified restatements with SOX 404 auditor's reports (unclassified restatements plus intentional misstatement restatements with SOX 404 auditor's reports). Panel B shows in tests using the UEvsUN (UEvsOther) sample that unintentional error restatements for firm-years with SOX 404 auditor's reports correct lesser amounts of absolute net income and absolute income before extraordinary items (relative to company size) than unclassified restatements (unclassified plus intentional misstatement restatements) (p>0.05). There is no difference in the average amount of operating income before depreciation (relative to company size) corrected by unintentional error restatements for firm-years with SOX 404 auditor's reports in comparison with unintentional error restatements (UEvsUN) and unintentional error restatements plus intentional misstatement restatements (UEvsOther).

Overall, Table 6 shows mixed support for *V2,* i.e., that proportionately more intentional misstatement restatements than unintentional error restatements correct net income. Results of comparisons of unintentional error restatements to intentional misstatement restatements (unclassified restatements) using the UEvsIM (UEvsUN) sample support (do not support) V2.

2.7.3 Earnings persistence

unintentional error vs. intentional misstatement

Table 7 reports the results of tests of *V3*, the persistence validation test. The OLS models in Table 7 regress return on assets (measured by earnings before extraordinary items scaled by average assets) on lagged return on assets, an indicator variable for unintentional error, the interaction of lagged return on assets and the indicator variable for unintentional error, and control variables shown by prior research to be associated with earnings persistence[16]. A significant negative coefficient on the interaction between lagged return on assets and the indicator of unintentional error restatements and a significant *F*-test showing the negative total of the main and interactive effect of the lagged return on assets differs from zero supports *V3.* Model 1 of Table 7 shows the interaction term, while in the predicted direction, is not significant (t = -0.92) when the UEvsIM sample is used in the regression. However, when the subsample of

[16] Lev (1983) finds the type of product, industry barriers-to-entry, capital intensity and firm size are associated with earnings persistence. I use industry dummies (for industries shown in Table 4 panel C) to control for product type and barriers-to-entry. I control for capital intensity using the ratio of depreciation, amortization and interest to sales, following Lev (1983) and for company size using logged total assets. Dechow et al. (2010) overview literature that shows growth is also associated with persistence. I use the percentage change in total sales to proxy for growth. These control variables are defined in Table 2. Capital intensity and growth are not significantly correlated (p>0.05) with lagged return on assets, the unintentional error indicator variable, the interaction term, and each other (|r|<0.1). Company size is significantly associated (p<0.001) with lagged return on assets, the unintentional error indicator, and the interaction of return on assets with the unintentional error indicator and the capital intensity and growth control variables (|r|<0.3). The post-regression test of variance inflation factors indicates correlations are unlikely to affect interpretation of results (VIFs< 9 for main variables).

the UEvsIM sample with auditor's SOX 404 reports is used in Model 2, the significant negative coefficient on the interaction term (p<0.01 one-sided), combined with the significant F-test (F=12.41) showing the total negative effect of the interaction and main effect of the unintentional error interaction term differs from zero, supports *V3*.

unintentional error vs. unclassified restatements

Model 3 reports results of the earnings persistence validation test for the UEvsUN sample. Supporting *V3*, Model 3 results show a significant negative coefficient for the interaction of lagged return on assets with the indicator variable for unintentional error restatements and a negative sum of the coefficients of the main and interactive effects of the unintentional indicator variable that differs significantly from zero (p<0.01 one-sided).

Together, the results for samples restricted to restated firm-years with auditor's SOX 404 reports i.e., the subsample of the UEvsIM sample with auditor's SOX 404 reports (Model 2) and the UEvsUN sample (Model 3) support *V3*. Consistent with these findings, the significant (p<0.01 one-sided) interaction and F-test reported for Model 4, the earnings persistence test with the UEvsOther sample, supports *V3* and shows earnings of firm-years with auditor's SOX 404 reports restated to correct unintentional error are less persistent than earnings of other restated firm-years with auditor's SOX 404 reports.

2.7.4 Accrual measures

unintentional error vs. intentional misstatement

Table 8 reports *V4* test results, logistic regressions of an indicator variable for restatement type on various accrual measures and control variables using the UEvsIM, UEvsUN, and

UEvsOther samples. Four accrual metrics are tested: total accruals (*Accruals*); working capital accruals (*WC_acc*); Kothari et al.'s (2005) performance matched residual from the modified Jones discretionary accrual model, estimated cross-sectionally (*pmatch*); and the studentized residuals from the Dechow and Dichev (2002) model, estimated cross-sectionally (*sresid*)[17]. Appendix C describes in more detail than Table 2 how I computed the four accrual measures. While the performance matched discretionary accrual measure (*pmatch*) is used frequently in financial quality research, Jones et al. (2008, 500) find that "only the accrual estimation errors estimated from cross-sectional models of working capital changes on past, present, and future cash flows… have explanatory power for fraud beyond total accruals". I chose control variables (total assets, return on assets, leverage and an indicator variable 1 if a Big 4/5/6/8 auditor and 0 otherwise) following Jones et al.'s (2008) logistic regression study of the association between a fraud indicator variable and eleven measures of discretionary accruals.

Supporting *V4*, Table 8 Panel A shows that positive accrual measures are less likely to be associated with restatements that correct unintentional errors than with restatements that correct intentional misstatements in the UEvsIM sample for three of the four accrual measures, *WC_acc* (Model 2), *pmatch* (Model 3), and *sresid* (Model 4) (p<0.01 one-tailed; pseudo R2>=0.14; Hosmer-Lemeshow goodness-of-fit test p>0.05, not in tables). These findings are consistent with univariate analyses (not in tables) that show the means of all four accrual measures (i.e., total accruals (*Accruals*), working capital accruals (*WC_acc*), performance matched discretionary accruals (*pmatch*), and studentized residuals (*sresid*)), winsorized at 1% and 99% to mitigate

[17] To check my computation of accruals measures, I compared my computation of median measures to medians reported in prior literature. Dechow et al. (2011) report three of the four measures using COMPUSTAT firm-years in the 1979-2002 time period. Using all unrestated quarterly data from 1987 (the start of the unrestated quarterly data) to 2002 and winsorized at 1% and 99% following Dechow et al. (2011), the comparison of median values (Dechow et al. 2011 in brackets) is as follows: *WC_acc* 0.005 (0.006); *pmatch* 0.000 (0.001); and *sresid* 0.008 (0.015). I compute median total *Accruals* of -0.045 for 1987 – 1999. Dechow and Dichev (2002) report median total accruals of -0.044 for the same time period.

against outliers, are less positive for unintentional error firm-years than intentional misstatement firm-years (p<0.05 one-tailed for t-statistics computed using robust standard errors clustered on company). The untabulated median measures of all four accrual measures (i.e., total accruals (*Accruals*), working capital accruals (*WC_acc*), performance matched discretionary accruals (*pmatch*), and studentized residuals (*sresid*)) are also less positive for unintentional error firm-years than intentional misstatement firm-years (p<0.05 one-tailed Wilcoxon ranksum z-statistics). Taken together, the multivariate results and the additional untabulated univariate analyses support *V4*.

unintentional error vs. unclassified restatements

Models 5 through 7 of Table 8 Panel B report the results for the UEvsUN sample. Models 5 and 7 of Table 8 Panel B show that when the sample is restricted to companies filing auditor's SOX 404 reports in the restated year, unclassified restatements are associated with more positive total accruals (*Accruals*) and studentized Dechow Dichev discretionary accruals (*sresid*) relative to restatements that correct unintentional error restatements, (p<0.05 one-tailed; pseudo R2>=0.05). These results are consistent with univariate results for these two accrual measures (not in tables). However, the results for the working capital accrual model (*WC_acc*) for companies with auditor's SOX 404 reports in the restatement year (Model 6) were not significant (and in the direction opposite to the predicted direction) which is consistent with univariate t-test and Wilcoxon ranksum test results (not in tables). The unintentional error vs. unclassified misstatements *pmatch* accrual univariate t-test (Wilcoxon ranksum test) results are in the direction opposite to the predicted direction and significant (not significant) (not in tables). The *pmatch* accrual model results for the UEvsUN sample are not reported in Table 8 Panel B as the overall model fit is not significant (p>0.2).

In addition to tests with the UEvsIM and UEvsUN samples, I conducted tests of the accruals validity test using the UEvsOther sample (Models 8 -10) and find significant results for the UEvsUN sample are robust to including the 17 restatements with SOX 404 reports that correct intentional misstatement with the unclassified restatements with SOX 404 reports (univariate results not shown in tables $p<0.05$ one-tailed; multivariate results Models 8 and 10 $p<0.01$ one-tailed).

Table 8 shows overall that seven of ten multivariate tests of *V4* (the accrual validation tests) with model fit required for statistical inference support *V4*. Of the three multivariate accrual validation tests shown in Table 8 that do not support *V4*, two show insignificant results directionally as predicted (Models 1 and 9). Only the test of the association with working capital accruals using the UEvsUN sample (Model 6) shows results ($p>0.7$ not in tables) directionally opposite to the prediction. Univariate results are consistent with multivariate results reported in Table 8. Together, the results support *V4*.

2.7.5 Income smoothness

unintentional error vs. intentional misstatement

Table 3 Panel A compares mean and median measures of net income smoothness (*smooth_NI*) and operating income smoothness (*smooth_opinc*) for the UEvsIM sample. I compute net income and operating income smoothness (Table 2) following Leuz et al. (2003)[18] as the median ratio of the standard deviation of income [respectively, total of four quarters of net income (*niqr*) and total of four quarters of operating income before depreciation (*oibdpqr*)]

[18] There is insufficient data to compute Tucker and Zarowin's (2006) measure that ranks firms' income smoothness within industry-year.

(scaled by lagged total assets) to the standard deviation of cash flow from operations (*oancfqr*) (scaled by lagged total assets) where there is complete data for the current and previous four years. A smaller smoothness measure indicates "smoother" income. There is sufficient data to calculate the income smoothness measures for less than half of the sample. I find income smoothness is the same (p>0.05) for unintentional error and intentional misstatement restatements when the two smoothness measures are winsorized at 1% and 99% to mitigate against outliers (Table 3 Panel A). In median measure comparisons, results are directionally consistent (inconsistent), although not significant, with the prediction for net income (operating income) smoothness. Overall, *V5* is not supported in tests using the UEvsIM sample.

unintentional error vs. unclassified restatements

Contrary to the above comparisons of income smoothness between unintentional error and intentional misstatement groups, a univariate Wilcoxon ranksum test for the UEvsUN sample reported in Panel B of Table 3 shows unclassified restatement firm years with SOX 404 auditor's opinions are characterized by greater net income smoothness (z=1.76, p<0.05 one-sided, p-value not shown in table) than unintentional errors firm-years. This is consistent with the multivariate results discussed above for the UEvsUN sample that show greater earnings persistence characterizes firm-years with unclassified restatement than firm-years with restatements that correct unintentional error for firm-years with SOX auditor's reports. While the differences are not significant (p>0.4 not in tables) operating income smoothness is also less for unintentional error restatements than unclassified restatements in the UEvsUN sample.

When firm-years for the 17 restatements that correct intentional misstatement with auditor's SOX 404 reports are combined with the UEvsUN sample in the UEvsOther sample

(Table 3 Panel C), results of a Wilcoxon rank-sum tests show firm-years for restatements that correct unintentional error with auditor's SOX 404 reports, while having the same operating income smoothness, have less smooth net income reporting (z=1.17, p<0.05 one-tailed, p-value not shown in table).

Overall, comparisons of net income smoothness using the UEvsUN and UEvsOther samples (the UEvsIM sample) validate (do not validate) *V5*.

2.7.6 Managing to a benchmark

unintentional error vs. intentional misstatement

To test *V6* I first compared the likelihood that the groups in the UEvsIM sample would "meet or beat" the latest analysts' earnings forecasts by one penny per share or less. [19] Reichelt and Wang (2010, 672) overview literature that supports (1) using the I/B/E/S unadjusted detail file to gather both analysts' forecasts and announcements of companies' actual EPSs to ensure consistency and avoid the issues related to stock splits; (2) determining the latest forecast to be the "most recent EPS forecast prior to the date of the earnings announcement" (or the median of forecasts when more than one analysts' forecasts are announced on the same last day); and (3) computing the standard deviation of forecasts, and the logarithm of the number of forecasts, using forecasts "no older than two months before the earnings announcement date" to avoid stale-dated forecasts. Forecasts within two months of the earnings announcement date are available for approximately one third of both the unintentional error and intentional misstatement restatements in the UEvsIM sample.

[19] An anonymous reviewer suggested comparing proportions of restatement classifications that meet or beat analysts' forecasts by a small amount.

The proportion (28.6%) of the 42 restatements that correct intentional misstatement that "meet or beat" the latest forecast by a penny per share or less is greater (p<0.05) relative to the proportion of the 223 restatements that correct unintentional errors (16.6%) (not in tables). Table 9 reports the results of multivariate "meet or beat" validation tests that control for company size (*size*), being in a litigious industry (*litigation*), and other variables shown by Reichelt and Wang (2010) and prior research to be associated with a meet or beat test. When the binary meeting or beating by a penny variable used in the multivariate logistic regression test is computed following Reichelt and Wang (2010), regression results for the UEvsIM sample show only 95 observations are used and overall model fit is not significant (Wald Chi-square p>0.1). Model 1 of Table 9 relaxes the restriction that forecasts for the UEvsIM sample must be within two months of the earnings announcement date and includes observations with forecasts within six months of the earnings announcement date. Consistent with the univariate results (not in tables), Table 9 Model 1 results support *V6*.

unintentional error vs. unclassified restatements

Model 2 of Table 9 investigates *V6* using the UEvsUN sample and examines differences in the likelihood of firm-years restating unintentional error vs. unclassified restatement firm-years with SOX 404 auditor's reports meeting or beating analysts' forecasts by one penny. Forecasts within two months of the earnings announcement date are available for approximately one half of both the unintentional error and unclassified restatements in the UEvsUN sample. The results of Model 2, while not significant, are in the predicted direction.

Model 3 of Table 9 reports results of the "meet or beat" validity test using the UEvsOther sample. Consistent with the results for the UEvsUN sample, Model 3 results, while not

significant, are in the predicted direction. In summary, when using the UEvsIM sample (the UEvsUN sample and the UEvsOther sample) the results of the "meet or beat analysts' forecast by one penny" support (do not support) V6.

2.7.7 Summary of proxy validation results

Table 10 summarizes proxy validation test results for the UEvsIM (N=878), UEvsUN (N=831), and UEvsOther (N=848) samples as well as the results for a subsample of the UEvsOther sample (N=225) used for Chapter 3 tests (Table 11). Overall, the three left columns of Table 10 show that the majority of test results were directionally as predicted (22 of 28 tests with sufficient data to conduct the tests). Results of nine of ten tests comparing restatements that correct unintentional error to restatements that correct intentional misstatements – tests using the UEvsIM sample - were directionally as predicted, six significantly so (p<0.05 one-sided). Results of six (seven) of the nine tests comparing restatements that correct unintentional error to unclassified (unclassified plus intentional misstatement) restatements where data was sufficient and SOX 404 auditor's reports were filed for the restated year – tests using the UEvsUN (UEvsOther) sample – were directionally as predicted, four significantly so (p<0.05 one-sided).

On balance, these validation test results support using the language-based proxy for restatements that correct unintentional error when auditor's SOX 404 reports are filed for the restated years.

2.8 Restatement Proxies for Chapter 3 Tests

Table 11 shows the criteria used to select a subsample of the UEvsOther sample for Chapter 3 tests. Chapter 3 tests include comparisons of associations between characteristics of

those responsible for financial reporting quality and restatements that correct unintentional error vs. restatements that correct intentional misstatement. As there are only 17 AAER search identified restatements with auditor's SOX 404 reports, I also chose a subsample of unclassified restatements from the UEvsOther sample to proxy for restatements that correct intentional misstatement[20]. First, to reduce the time required to hand-collect data from companies' proxy statements, I retained only one restatement per company. Retaining only the earliest restatement per company reduced the sample by approximately one half to a subsample comprised of 484 restatements including 339 unclassified restatements. Next, to further reduce collection time, I selected the 125 of these unclassified restatements with a scaled score greater than or equal to 0.5 based on a scoring system (described in the next paragraph) constructed to assign higher scores to restatements that have more characteristics that I predicted would be associated with intentional misstatement than unintentional error.

I scored the restatements (maximum 9) by scoring one if the restatement corrected a revenue recognition issue, one if the restatement corrects net income, one if the actual EPS meets or beats the latest analyst forecast by one penny or less, and one for each of the four accrual measures and two smoothness measures that exceed the median of the unclassified restatements with auditor's SOX 404 reports. Total scores ranged from zero to seven with a median score of

[20] Hennes, Leone, and Miller make publically available (https://sbaleone.bus.miami.edu/) a dataset of the 2,705 restatements in the GAO database (1997-2006) which they code as either errors (1,990) or irregularities (715) following the method used in Hennes et al. (2008). Analysis of this database supports proxying restatements that correct intentional misstatement with unclassified restatements: The number of restatements that correct irregularities would be insufficient for Chapter 3 tests even if the intentional misstatement classification based on AAER presence (Table 1) were expanded to include restatements with disclosures referring to "fraud" and "irregularities" or when SEC, Department of Justice, and other independent investigations are undertaken (Hennes et al. 2008). Hennes, Leone, and Miller identify as correcting irregularities 291 restatements in the GAO database that have restated periods that begin after the effective date of SOX 404 Of these, 25 restatements for companies other than foreign and holding companies have a restatement period longer than 364 days. Of these 25 restatements - restatements likely to be annual restatements - 21 do not meet the data requirements for Chapter 3 tests, i.e., 11 are without an auditor's SOX 404 report filing for the first year of the restated period and 10 are not the first restatement of the company. Two of the remaining four restatements are included in the sample used for Chapter 3 tests as determined in Section 2.8.

three (M=2.77 SD=1.77). Since data was not available for all nine scoring criteria for all restatements, I scaled the total score of each restatement by the number of its non-missing scores. Scaled scores ranged from zero to one with a median scaled score of 0.38 (M=0.38 SD=0.23). Of the 588 unclassified restatements with auditor's SOX 404 reports, 339 are companies' first or only restatement and 125[21] of these have scaled scores of 0.5 or greater. Table 11 shows proxy statements filed with the SEC contain the management and board data required for Chapter 3 tests for 98 of these 125 unclassified restatements and for six of the seven (121 of the 138) restatements that correct intentional misstatement (unintentional error) which are a company's first or only restatement.

Table 10 (column 4) shows the results of validation tests using the subsample of the UEvsOther sample selected for Chapter 3 tests, a subsample comprised of 121 error-correcting restatements vs. 104 restatements that proxy for misstatement correcting restatements (i.e., 6 AAER-identified restatements combined with 98 unclassified restatements). Not surprisingly, given the method used to select the unclassified restatements for the subsample, all of the seven tests where overall model fit permits interpretation of results, are directionally as predicted and six of the seven are significant (p<0.05 one-sided). Further, for the three multivariate tests with insignificant overall model fit, results of univariate Wilcoxon ranksum tests are as predicted and significant (p<0.1 one-sided not in tables).

Prior research concludes that intentional misstatements are associated with more negative market reaction than other restatements (Hennes et al. 2008, Palmrose et al. 2004). I compared the cumulative abnormal return (CAR) to restatement announcements for the two restatement

[21] Restatements in the top quartile; 69 of the 588 unclassified restatements with auditor's SOX 404 reports had scaled scores of 0.5.

groups in the sample to be used in Chapter 3 tests[22]. It is noteworthy that I find no evidence that CARs differ between the two types of restatements. One explanation for this sameness of market reaction is that 59% (41%) of unintentional error (intentional misstatement) restatements were announced not in 8-Ks or press releases (where the restatement announcements would be separated from other news), but instead in other filings such 10-Ks, the so called "stealth" disclosures that are more common in recent years (Files et al. 2009) where restatement would not be the only trigger for market reaction.

2.9 Robustness Tests

To test the robustness of the above findings using the UEvsIM, UEvsUN, and UEvsOther samples to different text searches, I repeated the validation tests when error-correcting restatements were identified using two different word lists. Under the more restrictive (narrow) definition of unintentional error I do not search for words/phrases in the "recording errors" group listed in Appendix A, reasoning that management may use phrases such as "recording error" to merely indicate the need to restate rather than to signify lack of intent. Consequently, under the more restrictive definition, only 546 of the 751 unintentional error firm-years are identified as unintentional error restatements. Under the less restrictive (wide) definition of unintentional error I also include restatements identified by text searches for classification error indicators listed in Appendix A, which increases sample size from 751 to 1298 unintentional error firm-years.

[22] I computed the CAR for each restatement announcement by summing the difference between the CRSP daily stock return (without dividends) and the predicted normal return over a three day window centered on the announcement date. To estimate predicted normal returns, I regressed actual returns on the CRSP equally weighted index (without dividends) for the restatements with auditor's SOX 404 reports that have CRSP returns data for at least 30 trading days within a 360 trading day estimation window. A three day reaction window centered on the event date is frequently used in main tests or sensitivity analyses in restatement research (e.g., Palmrose et al. 2004, Files et al. 2009).

I find that the results showing predicted differences between unintentional error firm-years and intentional misstatement firm-years for the UEvsIM sample (i.e. tests of *V1,* the proportion of restatements correcting revenue recognition issues; *V3,* income persistence; *V4,* less positive accruals; and *V6,* meet or beat analysts' forecasts by a small amount) are robust to both a more restrictive definition of unintentional error and less restrictive definition of unintentional error[23].The results showing predicted differences between unintentional error firm-years and unclassified restatement firm-years with SOX 404 auditor's reports for the UEvsUN and UEvsOther samples (i.e., tests of *V3*, income persistence, *V4* less positive accruals, and *V5* net income smoothness) are robust to both the more and restrictive definitions of unintentional error.

2.10 Summary

2.10.1 Discussion of results

Although accounting errors have been studied for decades (e.g., Kinney and McDaniel 1989), archival studies of the reporting quality of financial statements filed with securities regulators acknowledge, but few investigate, unintentional errors. This research gap may exist because researchers believe: (1) unintentional errors either balance each other out by year end or have an immaterial effect on financial reporting[24]; (2) management will prevent, or detect and correct, unintentional errors that distort internal management reports upon which day-to-day

[23] Under the more (less) restrictive definition 14.7% (35.0%) of the 3,707 restatements used in this study (Table 1) include words indicating lack of intent. To achieve a significant omnibus *F*, it was necessary to include an indicator variable for loss (equal to 1 if there is a loss in the restated or prior year and 0 otherwise) in the multivariate test of *V3* when the sample with the less restrictive choice of error indicators was used.

[24] As Brief (1965) observed, "a bookkeeping error and a decision-maker's error are not the same. In order to attach economic significance to the idea of an accounting error one would have to show that some decision makers acted as if the erroneous information has been correct."

decisions depend (Feng et al. 2009); (3) with an appropriate combination of controls, public companies of all sizes and complexity can prepare reliable financial statements that conform to generally accepted accounting principles (US GAO 2006 and PCAOB Auditing Standard No. 5); (4) all public companies are required to maintain books and records that accurately and fairly reflect transactions and have an independent audit that will either prevent, deter or detect material unintentional errors; and (5) auditors, motivated by litigation and reputation concerns, increase their efforts to detect errors when control risk and/or inherent risk is high[25]. Alternatively, data collection considerations may be the reason researchers rarely consider differences in unintentional errors and intentional misstatements in research designs. This study addresses the last consideration by developing a language-based proxy for identifying restatements that correct unintentional error using text search.

Using automated text searches, I identify a subset of restatements with language indicating that the restatement corrects unintentional error. Validity tests show, relative to AAER identified intentional misstatement firm-years, unintentional error firm-years have proportionately fewer revenue recognition issues, have proportionately fewer net income corrections, are characterized by less positive accruals, and are less likely to meet or beat analysts' earnings forecasts by a small amount. As observed by an anonymous reviewer, these results are perhaps not too surprising given that the intentional misstatement sample contained restatements that correct fraud and earnings management that is so egregious that it attracted SEC attention and sanctions. The unclassified restatements include intentional misstatements that have not attracted SEC sanctions. In the subsample where an auditor's SOX 404 report is filed

[25] Hogan and Wilkins (2008) overview audit risk model literature and find audit fees, a proxy for audit effort, increase when control risk and inherent and/or information risk is high but do not rule out that higher audit fees reflect the litigation and reputation concerns.

for at least the first year of the restated period, validation tests show unintentional error firm-years are characterized by significantly less positive accruals, less income persistence, and less net income smoothing than unclassified restatements that correct a mix of unintentional error and intentional misstatement. Overall, the results of the validation tests support my use in this study of a proxy for restatements that correct unintentional error that is constructed directly by searching restatement announcements for language asserting or implying lack of intent.

2.10.2 Contributions, limitations, and future research opportunities

The language-based method used in this chapter to construct a proxy for restatements that correct unintentional error differs from other proxies not only in the directness of proxy construction but also in its cost-effectiveness and in the proportion of restatements classified as error correcting. Other researchers wishing to identify restatements that correct unintentional errors may do so using the list of error indicators reported in Appendix A together with *AuditAnalytics* text search tool, i.e., proxy construction is automated and hence, replicable, scalable, and transparent. Automated text search is more cost effective than approaches such as Plumlee and Yohn's (2010) that rely on researchers reading restatements in order to classify them, especially when the number of restatement announcements is large. This method also classifies less than one third of post-2002 annual restatements as restatements that correct unintentional error, whereas Hennes et al. (2008) classify three quarters of restatements in their sample as errors and Plumlee and Yohn (2010) classify over half the restatements in their sample as internal error. If this smaller proportion reflects a better identification of restatements of unintentional error, the proxy may improve the power of tests in studies of the determinants and consequences of unintentional error. A useful extension of this research would be to determine

whether or not this language-based proxy classifies restatements more accurately than prior proxies.

As with all studies, this study is subject to limitations. One limitation of this study is the measurement error introduced by subjectivity in the choice of word(s) used to identify unintentional error restatements. I conducted robustness tests to test the sensitivity of validation test results to different choices of error indicators in text searches but it is not practical to test the robustness of results for all combinations of error indicators. Another limitation is the risk that variables omitted from multivariate analyses are biasing results. Although I tried to incorporate control variables shown to be significant in similar tests by prior research in multivariate analysis, differences in firm characteristics may explain my findings (Lawrence et al. 2011).

To date, lack of a direct approach to construct a proxy for unintentional error has hindered research into the comparative determinants and consequences of unintentional error. Researchers using financial restatements in their studies have used an "error" proxy that is the leftover from a fraud classification process. Subdividing this group into restatements that correct unintentional error and restatements that are more likely to correct income smoothing and more benign earnings management (unclassified restatements) may enhance the power of tests of associations between unintentional error and the determinants of financial reporting quality in future restatement research. Prior archival audit research shows links between improved public company financial reporting quality and certain auditor characteristics, e.g., auditor size (e.g., DeAngelo 1981), industry specialization (e.g., Lim and Tan 2010, Reichelt and Wang 2010), supply chain specialization (Johnstone et al. 2014), and local engagement office size (e.g., Choi et al. 2010, Francis and Yu 2009, Francis et al. 2014). However, only tentative conclusions can be drawn by these studies as to whether the improvements result from enhancing audit detection

skills, improving auditors' engagement acceptance decisions, or by bolstering auditors' ability to resist management's strategically motivated misreporting. Associations between financial reporting quality and management's expertise, incentives, corporate governance, firm characteristics, and audit quality determinants have been extensively studied. The differential effects of such determinants on intentional misstatements and unintentional errors have not. The contribution of this proxy is not limited to future academic research. This new proxy may also be useful to the PCAOB and other regulatory bodies engaged in audit quality indicator projects.

Chapter 3 - Determinants of Unintentional Error in Restatement Disclosures

3.1 Introduction

This chapter reviews literature and develops and tests hypotheses to investigate the associations between restatements and the parties responsible for preventing, deterring and detecting unintentional error - boards (through their audit committees), management (through their chief financial officers (CFOs)) and auditors. Understanding these effects is important because restatements that correct unintentional errors reflect the presence of internal control weaknesses that may signal unintentional error in both external financial reports and internal management reports. Unintentional errors in internal management reports affect not only external party decisions but also day-to-day operational decisions (Feng et al. 2009, Feng et al. forthcoming). A better understanding of the determinants of unintentional error may not only help management reduce errors arising from books and records deficiencies but also may assist audit committees, auditors and standard setters to address control risk.

Section 3.2 develops a theoretical model of determinants of unintentional error in audited financial statements of public companies. Effective internal control over financial reporting underpins financial statement reliability (PCAOB Auditing Standard No. 5, US GAO 2006, SEC 2003a). Organizational change has the potential to disrupt effective internal control. Management and the audit committee are responsible for overseeing the design and maintenance of internal controls to ensure control remains effective and that financial statements are free of both unintentional error and intentional misstatement. Auditors assess internal control and plan their audits to reduce "audit risk [a function of the risk of material misstatement and detection

risk] to an appropriately low level" (PCAOB Auditing Standard No. 8). Prior research finds management and auditor characteristics included in the theoretical model are associated with internal control quality and earnings quality. However, to my knowledge, the joint effects of auditor, audit committee, and management characteristics on restatements that correct unintentional error, and the comparative effects on restatements that correct unintentional error versus restatements that correct intentional misstatement, have not yet been studied. Such study may shed light on the characteristics of those responsible for financial statement preparation and audit that are associated with competence and effort, rather than reporting incentives. For example, auditors can explain their failure to detect intentional misstatements as the result of managements' strategic actions to conceal such misstatements. However, such explanations would not apply to their failure to detect unintentional errors. Instead, the explanation would focus on failure to understand the entity and its environment so as to assess risks correctly or failure to design and execute effective detective procedures.

Section 3.3 reviews literature and develops 13 hypotheses to study the joint effects of management, audit committee, and auditor characteristics on restatements. Four hypotheses test, with a more complete model than used in prior research, whether or not previous research findings on the associations between internal control quality and organizational change, CFO financial expertise, CFO influence, and audit committee financial expertise hold in the post-SOX period. Two hypotheses test whether the effects of organizational change on internal control quality depend on the financial expertise and influence of the CFO, dependencies not yet studied. Four hypotheses test associations between restatements that correct unintentional error and CFO financial expertise, CFO influence, audit committee financial expertise, and auditor quality. Three hypotheses compare associations between CFO influence, audit committee financial

expertise, and auditor quality and reductions in the likelihood of restatements that correct unintentional error versus restatements that correct intentional misstatement.

Section 3.4 describes the samples used in tests and presents the variable definitions and research design. Logistic models test the association between the expertise and incentives of those responsible for financial reporting quality and the likelihood of restatement and reporting internal control weaknesses. The research design includes tests of the mediating effect of internal control quality.

Section 3.5 presents test results. I do not find support for my predictions that error-correcting restatements are decreasing in auditor quality and that, relative to restatements that correct error, restatements that correct intentional misstatement are more strongly associated with auditor quality (proxied by auditor size measured at both the firm and engagement office level). On the other hand, I do find that the likelihood of unintentional error restatements is related to CFO financial expertise and influence. However, I also find that while the likelihood of error-correcting restatements are decreasing in CFO expertise and influence, this association depends upon whether or not companies are undergoing organizational change. In comparisons between associations of CFO financial expertise and influence, audit committee financial expertise and auditor quality with restatements that correct unintentional error versus restatements that correct intentional misstatement, I find that CFO (audit committee) financial expertise is more strongly associated with error-correcting (intentional misstatement-correcting) restatements. In contrast to these findings of associations between restatements and characteristics of those responsible for financial reporting quality, I find no associations between these characteristics and internal control quality (proxied by auditor's SOX 404 opinions).

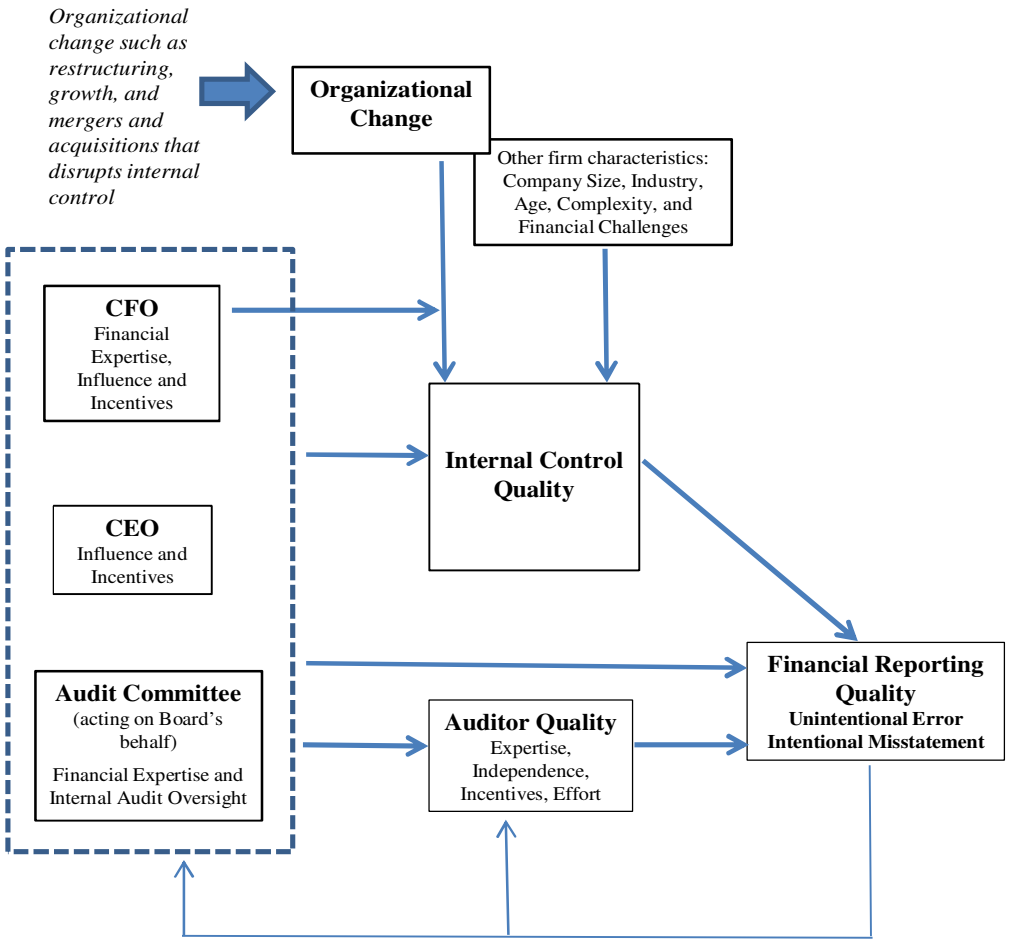
Section 3.6 summarizes by presenting both the study limitations and contributions and suggesting ideas for future research.

3.2 Theoretical Model

Figure 1 presents a theoretical model of determinants of unintentional error in audited financial statements of public companies. This section explains my choice of the firm, management, audit committee, and auditor characteristics included in the model. This section also highlights relationships between these characteristics and internal control quality and restatement that have been investigated in prior research, and the associations that to my knowledge have yet to be studied.

Figure 1

Determinants of Undetected, Unintentional Errors in Audited Financial Statements



3.2.1 Organizational change

Of the firm characteristics that Doyle et al. (2007b) find to be associated with internal control quality (i.e., organizational change, firm size, age, financial challenges, and complexity), organizational change is the one that signals change in the other firm characteristics and alters

the control risks faced by the company. Restructuring, growth, foreign expansion, acquisition activity and other organizational changes alter business processes and may introduce, or make more contentious, accounting issues. Organizational change may lead to internal control weaknesses if management and the audit committee do not anticipate the impact of change and modify controls and processes appropriately. Conversely, internal control may improve following organizational change if management and the audit committee respond to change with more vigilant internal control oversight.

Prior research shows mixed results for the associations between organizational change and internal control strength and restatements (Ashbaugh-Skaife et al. 2007, Doyle et al. 2007a, Goh 2009, Hoitash et al. 2009, Johnstone et al. 2011, Li et al. 2010, Lisic et al. 2014, Zhang et al. 2007). Studying the association between organizational change and internal control in a model that includes management, governance, and audit effects - as suggested by Caskey et al.'s (2010) theoretical paper - may shed light on these mixed results.

3.2.2 CFO and CEO financial expertise, influence, and incentives

Of all the senior managers, the CFO plays the most significant role in overseeing financial reporting and ensuring internal control over financial reporting is established and maintained in compliance with SOX and other business and regulatory requirements (Aier et al. 2005, Ge et al. 2011, Geiger and North 2006, Li et al. 2010, Mian 2001, Wang 2010). Both CFOs and CEOs file certifications of financial information and assessments of the effectiveness of internal controls over financial reporting (SEC 2003a) and influence auditor selection (Cohen et al. 2010). As expected, prior research concludes that CFOs shoulder greater responsibility for financial reporting and internal control than CEOs. Relative to CEOs, CFOs receive larger bonus

reductions following reporting of internal control weaknesses (Hoitash et al. 2012), turn over at a greater rate following restatements due to irregularities[26] (Hennes et al. 2008), and suffer greater turnover and within-firm demotions following non-fraudulent restatements (Burks 2010). While the CFO plays the most significant internal control oversight role, their role may be limited if the CEO is incented to misstate and is not constrained by a strong, independent board.

CFO financial expertise[27] is associated with both the revelation and remediation of internal control weaknesses (Johnstone et al. 2011, Krishnan 2005, and Li et al. 2010). However, CFOs' financial expertise may not be sufficient to ensure effective internal controls if CFOs do not have sufficient influence within the organization to muster or reconfigure accounting resources to respond to organizational change. Jiang et al. (2010, 515) argue that CFOs receiving greater equity incentives relative to CEOs "have more control over financial reporting". Two recent studies (Balsam et al. 2014, Hoitash et al. 2012) find that internal control quality is increasing in CFOs' equity incentives and support the view that the association between CFO influence and internal control quality should be included in the theoretical model. A third study (Bedard et al. 2014) finds companies with CFOs who are members of their companies' boards, not only have more effective internal control over financial reporting but also a lower likelihood of restatement and higher accruals quality. Neither the relationships between CFOs' financial expertise and influence and restatements that correct unintentional error, nor the relationships

[26] Identified by Hennes et al. (2008, 1489) as restatements containing the word "fraud" or "irregularity" or variants thereof, restatements related to SEC or Department of Justic investigations, and the presence or absence of "other investigations into the accounting matters (e.g., the Audit Committee hires a forensic accounting firm").

[27] Definitions of financial expertise vary in prior research. Frequently researchers restrict financial expertise to accounting expertise evidenced by accounting qualifications (i.e., Certified Public Accountants and Chartered Accountants), prior audit experience (work or partner experience at a Big 4/5/6/8 or national audit firm), or experience supervising accounting functions (i.e. prior experience as a chief financial officer, chief accounting officer, vice president of finance, controller, or treasurer). The definition of financial expertise is sometimes expanded (e.g. Keune and Johnstone 2011) to include those with finance expertise (i.e., bankers, investment bankers, Certified Financial Analysts, venture capitalists, and business school faculty).

between internal control quality and the interactions of organizational change and CFOs' financial expertise and influence have been investigated thus far. A better understanding of such associations may assist CFOs to allocate their time between financial reporting and strategic management responsibilities and improve auditors' control risk assessments.

3.2.3 Audit committee financial expertise

Regulatory and listing changes over the past 15 years have bolstered audit committee independence, financial knowledge, and control over audit and financial reporting processes (Abbott and Parker 2000, DeFond and Francis 2005, DeZoort et al. 2008, SEC 2003b, SOX Section 301 2002). Overall, recent archival research (Bruynseels and Cardinaels 2014, Carcello et al. 2011b, Lisic et al. 2014) and semi-structured interviews of external auditors (Cohen et al. 2010) and of public company audit committee members (Beasley et al. 2009) reveal that audit committees are "more active, diligent, knowledgeable, and powerful" post-SOX (Cohen et al. 2010, 752). The audit committee is now directly responsible for the appointment, compensation and oversight of the work of the external auditor.

Yet the audit committee's role remains "inherently limited given the nature of the function. Audit committees only meet periodically, usually deal with complex but limited second-hand information, and include members with less knowledge of the company's operations, controls, and reporting than management" (DeZoort et al. 2002, 41). Management and both external and internal auditors inform audit committees. SOX Section 302 requires management to disclose any internal control deficiencies and weaknesses to both auditors and the audit committee. External auditors are required to communicate to audit committees in writing regarding any significant deficiencies and internal control weaknesses (PCAOB Auditing

standards No. 2 and No. 5). The Chief Audit Executive in charge of the internal audit function (IAF) commonly reports to the audit committee, although audit committee oversight of the IAF is not mandated, and the nature and effectiveness of the IAF can vary greatly (Abbott et al. 2012)[28].

Changes in regulation and best practices have reduced post-SOX variation in audit committee composition and activity (Carcello et al. 2011a, Cohen et al. 2008). Now, virtually all audit committees are composed of all independent directors. Most have three members and aim to meet at least quarterly. Public companies are required to designate an audit committee financial expert or explain why they have not done so (SOX Section 301). The financial expertise of audit committees varies not only with the number of financial experts on the committee but also with whether or not the financial experts have financial expertise (e.g., accounting qualifications or prior audit or CFO experience) rather than supervisory expertise gained overseeing the financial reporting process (e.g., experience as a Chief Executive Officer, Board Chair, and Chief Operating Officer). Supervisory expertise is sufficient to qualify an audit committee member as a financial expert.

Many studies show audit committee financial expertise is related to internal control or financial reporting quality (e.g., Abbott et al. 2004, Agrawal and Chadha 2005, Cohen et al. 2014, Dhaliwal et al. 2010, Goh 2009, Hoitash et al. 2009, Johnstone et al. 2011, Keune and Johnstone 2012, Krishnan 2005, Lisic et al. 2014, and Zhang et al. 2007). Studies show that more

[28] Due to the absence of IAF data, empirical research on the IAF contribution to financial reporting quality is scarce (Abbott et al. 2012, DeFond and Zhang 2013, Prawitt et al. 2009). Two of the few archival studies, Lin et al. (2011) and Prawitt et al. (2009), obtain data from the Institute of Internal Auditors (IIA) Global Audit Information Network (GAIN) database, a compilation of survey response of Chief Audit Executives of IIA members. Prawitt et al. (2009) find that the IAF is (is not) associated incrementally over and above the external auditors with smaller positive (negative) abnormal accruals and meeting and beating analyst forecasts. Lin et al. (2011) find the nature and scope of IAF activities and the IAF education attribute are associated with SOX 404 material weakness disclosure.

independent audit committees appoint higher quality auditors (DeZoort et al. 2002) and that audit committees with CPA members are more supportive of auditor-proposed adjustments post-SOX (DeZoort et al. 2008). The association between audit committee financial expertise and financial reporting quality is underlined by Srinivasan's (2005) finding that audit committee members with financial expertise lose more positions on other boards relative to other audit committee members following accounting restatements. Boards with a clear understanding of their firm circumstances and these associations will be better informed to choose audit committee members who bring the best mix of financial expertise and other expertise (e.g., supervisory, industry, and legal) to the audit committee.

3.2.4 Auditor quality

Whether or not internal control weaknesses result in financial reporting error depends on auditor quality. Skilled auditors plan and conduct audits recognizing internal control weaknesses. While archival researchers "hypothesize that auditors are a determinant of earning quality because of their role in mitigating intentional and unintentional misstatements" (Dechow et al. 2010, 383), the aspects of auditor quality driving this mitigation (e.g., auditor expertise, independence, incentives, and effort) continue to be investigated.

In their synthesis of audit research, Knechel et al. (2013, 385) conclude that there is "little consensus about how to define, let alone measure, audit quality". This is not surprising given archival researchers lack the publicly available data necessary to triangulate many findings of experimental, survey, and qualitative audit research (Francis 2011). The publicly observable data used by archival researchers to construct auditor quality proxies include both output measures (e.g. restatement frequency, results of PCAOB inspections, and clients' accruals quality) and

input measures (e.g., auditor size, industry specialization, client fees, and auditor tenure). The output measures are not suitable audit quality proxies for this study because: (1) Restatements are used in this study to construct the proxy for unintentional error and accordingly cannot also be used as a proxy for auditor quality; (2) The publicly available portions of PCAOB inspection reports cannot be matched to restatement data because inspection reports do not refer to the client or engagement office and are less likely to be available for firm-years when smaller auditors conducted the audit;[29] and (3) Accruals quality is inflated by both intentional misstatement and unintentional error and hence is an unsuitable proxy for auditor quality in tests that compare the associations of determinants between restatements that correct unintentional error vs. intentional misstatement. Many auditor quality input proxies (e.g., industry specialization, client influence, and auditor effort) are constructed from publicly available audit fee data and thus are highly correlated with both client and auditor size.

Theory suggests higher quality auditors are less dependent on any one client, invest more in audit methodologies and technologies, and exert more effort to protect their reputations. Consistent with theory, many studies find earnings quality increasing in auditor quality (e.g., Becker et al. 1998, Choi et al. 2010, Francis and Krishnan 1999, Francis et al. 1999, Francis and Yu 2009, Lennox 1999, Reynolds and Francis 2001, Teoh and Wong 1993, and Weber and Willenborg 2003). These studies proxy auditor quality with engagement office and/or firm level measures of auditor size. However, recent studies find mixed evidence of association between restatement and auditor quality proxied by auditor size (Cohen et al. 2014, Ettredge et al. 2014, Francis et al. 2013, Francis and Michas 2013, and Newton et al. 2013). Hennes et al. (2014) find

[29] The PCAOB conducts inspections annually for firms that regularly provide audit reports for more than 100 issuers, and at least triennially for firms that regularly provide audit reports for 100 or fewer issuers (www.pcaobus.org/inspections).

auditors are dismissed after restatements (albeit dismissal likelihood is less for less severe restatements). To my knowledge, a study of the association between auditor quality and restatements that correct unintentional error, and a comparison of the associations between auditor quality and restatements that correct unintentional error vs. restatements that correct intentional restatements, has not yet been undertaken to investigate these mixed findings. Investigating these associations may lead not only to audit committees appointing auditors that better fit firm circumstances but also may help regulators refine their use of restatements as an audit quality metric.

3.3 Hypotheses Development and Related Literature

This section reviews literature cited in developing the theoretical model and other research that I rely on to develop 13 hypotheses.

3.3.1 Internal control and organizational change

Some types of change disrupt control systems more than others. For example, control systems that accommodate modest sales and asset growth may be disrupted by downsizing, merger, or other restructuring activities that introduce new accounting and business disposition/integration issues which require greater change in financial reporting and control processes. Internal control weaknesses and restatements evidence a failure of management to either design or maintain effective control at some point during the evolution of the business. Plumlee and Yohn (2010) find internal control weaknesses are reported by 44% of companies restating financial statements filed in the 2003-2006 time period. Regression results of prior research show mixed results for the associations between organizational change and both internal

control quality (Ashbaugh-Skaife et al. 2007, Doyle et al. 2007a, Goh 2009, Hoitash et al. 2009, Johnstone et al. 2011, Li et al. 2010, Lisic et al. 2014, and Zhang et al. 2007) and restatement likelihood (Carcello et al. 2011b, Cohen et al. 2014, Ettredge et al. 2014, Francis et al. 2013, and Lisic et al. 2014). Results of the two recent restatement studies that include not only organizational change but also both auditor quality and audit committee measures in regressions (Carcello et al. 2011b, Cohen et al. 2014), show a significant positive association between organizational change and restatements that evidence internal control breakdown, thus leading to my first prediction, worded in the alternative:

H1: *Internal control quality is negatively associated with organizational change.*

3.3.2 CFO financial expertise and influence

CFO financial expertise is associated with both the revelation and remediation of internal control weaknesses (Johnstone et al. 2011, Krishnan 2005, and Li et al. 2010). These findings, together with Ashbaugh-Skaife et al.'s (2008) conclusion that internal control deficiencies are associated with unintentional error[30] suggest that internal control weaknesses will be decreasing in CFO financial expertise. Since firms may invest in superior auditor quality to compensate for weak internal controls (Hogan and Wilkins 2008, Hoitash et al. 2009, Lu et al. 2011, and Simunic 1980) and CFOs influence auditor selection (Cohen et al. 2010), the association between unintentional error in audited financial statements and CFO financial expertise is an empirical question. I rely on Aier's et al. (2005) pre-SOX-sample study that finds restatements

[30] Ashbaugh-Skaife et al. (2008, 233) find that relative to a control sample, firms with internal control deficiencies "report, on average, larger positive and larger negative abnormal accruals … consistent with internal control weaknesses adding noise to accruals through unintentional errors, inadvertent oversights, and accidental misstatements that affect earnings in both directions."

are decreasing in CFO financial expertise to form the following hypotheses written in the alternative:

H2: *Internal control quality is positively associated with CFO financial expertise.*

H3: *The likelihood of restatement to correct unintentional error is negatively associated with CFO financial expertise.*

Expertise alone may not be sufficient to prevent control breakdown and unintentional error if the CFO does not have the stature within the organization to ensure sufficient resourcing of financial reporting and control processes and the appointment of a high quality auditor. In all but the smallest of businesses, financial reporting systems are integrated with business processes and many people exercising important controls are not under the direct supervision of the CFO. Integrated systems frequently span departments, divisions, and subsidiaries. For example, Hoitash et al. (2012, 792) observe that company-wide "problems pertaining to information technology (e.g., the use of a legacy system that lacks integration or a system that is not properly configured and therefore does not enforce segregation of duties) can potentially impact multiple business process areas such as the order-to-cash-collection process and the purchase-to-pay process." Effective control involves training and monitoring personnel both inside and outside the accounting area.

CFOs with the greatest influence are not necessarily those with the most financial expertise. Hoitash et al. (2012) find account specific, but not company-wide, control weaknesses are associated with changes in CFO compensation. A decade ago, a CFO "saw the business more through an accounting lens than through strategy and value-creation lenses" (Groysberg et al.

2011), whereas now, "[n]early one-third of new CFOs … have spent a sizable portion of their career in investment banking, consulting, or private equity" (Agrawal et al. 2013). In many organizations a CFO plays a more strategic role, particularly in larger organizations with a separate controller and treasurer reporting to the CFO. Nonetheless, in a post-SOX survey, Indjejikian and Matejka (2009) find that financial performance continues to be an important determinant of CFO bonuses and that greater bonus weight is placed on financial performance measures for CFOs with longer tenure and for CFOs of larger firms.

Jiang et al. (2010, 515) argue that CFOs receiving greater equity incentives relative to CEOs "have more control over financial reporting". Supporting this view, post-SOX studies find that internal control quality is increasing in CFOs' equity incentives (Balsam et al. 2014, Hoitash et al. 2012). My next two predictions, expressed in the alternative, are based on Balsam et al.'s (2014) and Hoitash et al.'s (2012) findings, together with Ashbaugh-Skaife et al.'s (2008) conclusion that internal control deficiencies are associated with unintentional error:

H4: *Internal control quality is positively associated with CFO influence.*

H5: *The likelihood of restatement to correct unintentional error is negatively associated with CFO influence.*

Internal control will deteriorate if not modified appropriately in response to business change. For example, access rights need to be updated for personnel changes and period-end processes need to be modified as a firm expands globally. When change is anticipated and viewed as an opportunity for control improvement, management will tune controls to changing business processes and avoid deterioration of financial reporting quality. For instance, firms may improve Enterprise Resource Planning (ERP) systems or invest in staff training leading up to

expanding operations or merging with another firm. Such improvements may explain Ashbaugh-Skaife et al.'s (2008, p. 237) "contradictory" finding that firms that engage in restructuring report higher quality accruals. Similar reasoning has been used to explain why the Year 2000 (Y2K) fears of wide-spread failures of older systems at the start of the new millennium were not realized.

I argue that CFOs with greater financial expertise are more likely to recognize any new accounting or control issues that arise as a result of organizational change. I further argue that CFOs with greater influence are more likely to be positioned within the management structure (1) to learn of organizational change sooner (which provides time to plan control changes to keep controls and business processes better matched); (2) to participate in organizational change planning; and (3) to have the power to influence the personnel, system and business process changes necessary to improve internal control. Accordingly, my next two predictions (in alternative form) are:

> ***H6:*** *The association between internal control quality and organizational change is moderated by CFO financial expertise.*

> ***H7:*** *The association between internal control quality and organizational change is moderated by CFO influence.*

My last hypothesis concerning the CFO associations with unintentional error posits that the negative association between CFO influence and misreporting is greater for restatements that correct unintentional error than those that correct intentional misstatement. When bonus and equity incentives constitute a large portion of CFO compensation, CFOs may engage in opportunistic behavior. Feng et al. (2011) find that CFOs succumb to CEOs in material accounting manipulations, suggesting the association of CFO incentives and intentional

misstatements may mimic that of CEOs (see Armstrong et al. 2010 for a review of recent CEO incentives literature). However, such succumbing need not lead to intentional misstatement if the CEO is not motivated to misreport. Jiang et al. (2010) find the association between CEO equity incentives and discretionary accruals is not significant post-SOX and that greater CFO incentive is associated with lower discretionary accruals post-SOX. Similarly, Armstrong et al. (2010) fail to find post-SOX evidence of an association between greater CEO equity incentives and manipulation related restatements, AAERs, and lawsuits. CFOs' equity incentives, which are less post-SOX (Indjejikian and Matejka 2009), are more important than CEOs' equity incentives in explaining associations with accruals and meeting earnings benchmarks (Balsam et al. 2014, Jiang et al. 2010).

The Armstrong et al. (2010), Feng et al. (2011) and Jiang et al. (2010) results, taken together with the arguments used to develop hypothesis *H7*, suggest the negative relationship between CFO influence and unintentional error would be greater than the relationship with intentional misstatement. While the association between CFO influence and intentional misstatement is not posited or tested in this study, a negative association would bias against finding results for this prediction. I predict (in alternative form):

H8: *Restatements that correct unintentional error are more likely to be decreasing in CFO influence than restatements that correct intentional misstatement.*

3.3.3 Audit committee financial expertise

Restatement signifies that the audit committee failed in its oversight of the internal control and financial reporting processes. Agency theory (Jensen and Meckling 1976) predicts an independent audit committee with appropriate expertise performs a monitoring role that reduces

agency costs. Conversely, the institutional and managerial hegemony perspectives suggest that audit committee expertise does not guarantee monitoring quality. Institutional theory predicts audit committee members may not be selected for their monitoring ability but instead for credentials and experience that better conform to regulatory guidance and best practices (see Cohen et al. 2008 and Beasley et al. 2009 for literature overviews). Emphasis on financial expertise in selecting audit committee members may compromise audit committee performance if less able committee members are selected or audit committee members become less inclined to ask challenging questions. Management hegemony theory, from the strategy literature, predicts that board members selected by management will be more passive and challenge management less (see Cohen et al. 2008 for literature overview). Consistent with this prediction, recent studies find CEO involvement in selecting board members (Carcello et al. 2011b) or the presence of a more powerful CEO (Lisic et al. 2014) moderates the negative association between audit committee expertise and restatement. Moreover, although financial expertise is "by far the most common reason for being asked to serve on the audit committee" (Beasley et al. 2009), CEOs frequently have social connections with audit committee members. Audit committee monitoring is weaker (i.e., greater earnings management, lower audit fees, fewer going concern opinions, and disclosure of fewer internal control weaknesses) when the CEO has friendship ties, but not professional and educational ties, with the audit committee (Bruynseels and Cardinaels 2014). The recent studies that find association between internal control quality and audit committee financial expertise while controlling for CEO characteristics (Bruynseels and Cardinaels 2014, Lisic et al. 2014) do not control for CFO characteristics. Thus, testing, in a more complete model, the association between audit committee financial expertise and both internal control

quality and restatements that evidence breakdown of internal control leads to my next two predictions:

H9: *Internal control quality is increasing in audit committee financial expertise.*

H10: *The likelihood of restatement to correct unintentional error is decreasing in audit committee financial expertise.*

In contrast to prior research that finds few associations between accounting restatements and a wide range of governance measures (Carcello et al. 2011a, Larker et al. 2007)[31], the negative association between restatements and audit committee financial expertise shown in both pre-SOX and post-SOX studies (Abbott et al. 2004, Agrawal and Chadha 2005, Badolato et al. forthcoming, Carcello et al. 2011b, Cohen et al. 2014, Lisic et al. 2014) is consistent with agency theory. Nonetheless, it is not clear that restatement samples used in prior research showing a relationship between restatements and audit committee financial expertise support a conclusion that audit committee financial expertise is associated with restatements that correct unintentional error.

It is possible that associations with restatements that correct intentional misstatement and not restatements that correct unintentional error are driving the audit committee financial expertise restatement association research results. Audit committees aware of management's incentives to misstate may ask probing questions that lead to their fuller appreciation for

[31] Carcello et al. (2011a) summarize selected results from 12 corporate governance literature reviews or meta-analysis papers and cite only audit committee independence and expertise associations with restatements. Larker et al. (2007, 10) during the June 2002 to March 2003 sample period find "little relation to accounting restatements" between 14 governance variables constructed using principal component analysis of 39 governance characteristics that include audit committee independence, size, meeting frequency, but not audit committee expertise, measures. Baber et al. (2012) find observing associations between restatements and composite measures of Board and external governance factors, depends on the time period and whether or not governance interactions are included in regressions.

potential misstatement. While audit committees may also ask probing questions about internal control quality in general, audit committees may ask less probing questions about *specific* unintentional errors as they are unlikely to be asked to intervene in negotiations between management and auditors in correction of unintentional error. Carcello et al. (2011b, 22) find audit committee financial expertise is associated only with more severe restatements that involve "fraud, SEC AAERs, litigation, a change in net income/revenue of more than -10 percent, or a market reaction to the announcement of more than -10 percent". Cohen et al. (2014) find audit committee expertise is associated with restatement in the expected direction overall and in the partition with negative market reaction to restatement announcement but do not report results for the partition with the few observations with positive market reaction. Badolato et al.'s (forthcoming) study, the only study I am aware of that partitions restatements using *AuditAnalytics'* accounting and clerical error classification code, find no evidence of an association between audit committee expertise and restatements that correct unintentional error. Badolato et al. (forthcoming) find that audit committees with both financial expertise and high status relative to management is associated with fewer irregularities (i.e., firm-years with SEC and Department of Justice enforcement actions that establish intent and settled class-action lawsuits that allege GAAP violations) but that irregularities are not decreasing in audit committee financial expertise alone.

Given the results of prior research on the association between unintentional error and audit committee financial expertise is limited and mixed, I base my prediction that audit committee financial expertise is more strongly associated with correction of intentional misstatement than unintentional error on consideration of the audit committee's role. The audit committee is more likely to engage in discussions of estimates subject to intentional

misstatement than correction of unintentional error: when discovered, unintentional errors require management correction not negotiations with management and auditors. An association between audit committee financial expertise and unintentional error is more likely to be an indirect effect mediated by the audit committee's effect on resourcing of controls and the accounting function. I reason that the association between the audit committee's financial expertise and negotiations with management and auditors over contentious financial statement amounts is likely to be stronger than the association with internal control quality, given the CFO has primary responsibility for internal control. Thus, I make the following prediction concerning the association between restatements and audit committee financial expertise, written in the alternative:

> ***H11:*** *Restatements that correct intentional misstatement are more likely to be decreasing in audit committee financial expertise than restatements that correct unintentional error.*

3.3.4 Auditor quality

Economic dependence theory predicts higher quality auditors will be less dependent on any one client and are therefore less incented to succumb to management pressure (DeAngelo 1981). Auditor size is a frequently used proxy of auditor quality. Given client loss is likely to be felt more acutely at the engagement office level, researchers measure auditor size at the engagement office level as well as at the firm level (e.g., Craswell et al. 2002, Reynolds and Francis 2001). While economic dependence theory relates to auditor independence, knowledge theory relates more directly to an auditor's ability to detect accounting errors and misstatements. The PCAOB Office of Research and Analysis (2013a, 22) asserts that "spending earmarked toward areas such as technology and systems, training and guidance, audit methodology and risk

management tools, and technical consulting resources … should theoretically help to improve audit quality". PCAOB inspectors consider both audit performance and infrastructure when assessing audit quality control policies and procedures of larger audit firms (Church and Shefchik 2012). The Big 4 (Deloitte, EY, KPMG, and PwC) [32] invest more than smaller firms in proprietary audit methodologies, audit technologies, and codification of best practices and GAAP interpretation (Bedard et al. 2008, Dowling and Leech 2007, Dowling and Leech 2014, and Francis et al. 2014). Infrastructure spending that facilitates knowledge sharing also builds reputational capital (DeAngelo 1981). The theory of reputation protection (see Reynolds and Francis 2001 for an overview) predicts that larger auditors, with more reputation capital at risk, are perceived as more independent by their clients and will exert more audit effort.

Consistent with theory, many studies find earnings quality (proxied by accruals quality, restatements, going concern opinions, ERCs, meeting and beating benchmarks, reporting comparability, etc.) increasing in auditor quality (proxied by auditor size measured at the firm level and office level) (Choi et al. 2010, Francis 2004, Francis and Michas 2013, Francis and Yu 2009, Francis et al. 2014, Reynolds and Francis 2001). In contrast, evidence of association between restatement and auditor quality is mixed in recent restatement studies (Cohen et al. 2014, Ettredge et al. 2014, Francis and Michas 2013, Francis et al. 2013, Newton et al. 2013) [33]. Furthermore, of these recent restatement studies only Cohen et al. (2014) includes audit committee expertise in tests. Cohen et al. (2014) predict, but did not find, a negative association

[32] I chose to label as "Big 4" the Big 8/6/5/4 classifications used in the various studies based on the time period of the study. The Big 8 shrank to the Big 4 with the collapse of Arthur Andersen and audit firm mergers since 1989. The Big 4 audit approximately four fifths of accelerated SEC registrants and half of non-accelerated SEC registrants (based on data extracted Dec. 13, 2013 from the *AuditAnalytics* database).

[33] Contrary to Lobo and Zhao's (2013) recommendations based on findings in their audit fee restatement study, these recent studies (Ettredge et al. 2014 being the possible exception) include both quarterly and annual restatements in restatement samples and do not adjust audit fee measures of audit effort for pre-audit risk.

between auditor quality (included as a control variable and proxied by auditor size) and restatements.

The evidence that auditor quality is associated with litigation risk is mixed (Casterella et al. 2010). Based on their extensive review of both older and more recent theoretical and empirical auditor litigation research, DeFond and Zhang (2014, 70) note that it is "somewhat surprising that the link to audit quality is not more conclusive". Lys and Watts (1994) study lawsuits against auditors from 1955-1994 and conclude that the likelihood of a lawsuit may have more to do with a client's size and stock performance than audit failure. Notwithstanding the fact that higher quality auditors have larger clients on average (Lawrence et al. 2011) and "deeper pockets", it does not necessarily follow that auditors will increase audit effort in response to increased litigation risk. Prior research shows auditors respond to litigation risk by charging higher fees, increasing going concern opinions, and shedding riskier clients, but most studies are not able to disentangle whether or not auditors respond with greater audit effort (DeFond and Zhang 2014). In fact, researchers suggest auditors may respond to legal rules and damage award precedents with "excessive auditor conservatism" rather than increased audit effort (DeFond and Zhang 2014, 24). Therefore, while "[u]niversally, the accounting profession has long argued that their culpability and, accordingly, liability for failures to detect and report management fraud is less than that for failures to detect and report financial statement errors" (Ferguson and Majid 2003, 365), it may be that different quality auditors do not differ in their audit effort response to litigation risk. In international settings, where the litigation risk is lower than in the United States, studies find reputation protection incentives extend beyond the auditor litigation incentives (e.g., Lennox and Li 2012, Skinner and Srinivasan 2012).

Even though the recent restatement studies do not focus on unintentional error, three of the recent restatement studies report results of additional analyses partitioned by restatement severity which may shed light on whether it is unintentional error or intentional misstatement that is driving the auditor quality associations. The results of these additional analyses are mixed. Francis et al. (2013) find auditor quality (proxied by engagement office size) is negatively associated with income-decreasing, income-increasing, and no-net-income-effect restatements. Newton et al.'s (2013) tabulated results show a significant association only between auditor quality (proxied by engagement office size) and income-decreasing (more severe) restatements. Inconsistently, Newton et al.'s (2013) tabulated results in Big 4 and non-Big 4 partitions show no significant association between restatements and engagement office size in the Big 4 partition and significant association only with income-increasing restatements (less severe restatements) in the non-Big 4 partition. Ettredge et al. (2014, 259) find their results, showing that clients of lower quality auditors (proxied by non-Big 4 firms and smaller engagement offices) are associated with more restatements than clients of higher quality auditors during the 2008 recession, are driven by restatements with more negative market reaction and not "merely with smaller errors in financial statements".

The Ettredge et al. (2014), Francis et al. (2013) and Newton et al. (2013) results cited above are somewhat surprising. Given that higher quality auditors have greater reputational capital at risk would they not increase effort to detect less severe unintentional errors as well as intentional misstatements that management might try to conceal? It is possible that auditors do not perceive reputational consequences for less severe restatements. Supporting this possibility, Hogan and Wilkins (2008, 236) find fees, a proxy for audit effort, are not significantly higher if the weaknesses are "less severe and are relatively isolated (e.g., account-specific or subsidiary-

specific)". However, Hennes et al. (2014) find auditors are dismissed after both more and less severe restatements, albeit dismissal likelihood is less for less severe restatements and the likelihood is further reduced for Big 4 auditors with operationally complex clients where the choice of replacement auditors is limited.

It is also possible that smaller auditors (a frequent proxy for lower auditor quality) have more experience estimating the risk of unintentional error than larger auditors. As Francis (2011, 133) observes "large and small accounting firms … can have quite different clienteles at the extremes". Auditor selection bias leads to smaller, less profitable firms being audited by smaller auditors (Lawrence et al. 2011). Hence, smaller auditors may have more experience auditing the smaller, financially weaker companies that are more likely to have entity-wide control problems (Doyle et al. 2007b). Moreover, the audit staff at smaller engagement offices assessing the evidence used to make error risk decisions may work more directly with experienced partners than audit staff at larger engagement offices. The knowledge sharing benefits realized from infrastructure investments by the Big 4 may be inherently limited (Vera-Munoz et al. 2006). Types of critical auditor knowledge that may only be shared by working closely together rather than relying on centralized resources and transmission across a network of decentralized offices may be better shared in smaller engagement offices.

Assessing unintentional error risk involves understanding clients' internal controls and conducting control tests of appropriate controls. Bedard et al. (2008) overview the literature on issues in using the formalized decision support systems adopted for risk management practices and procedures by larger firms. Such decision support systems enable centralizing of control over certain parts of the audit, force compliance with firms' methodology and standards, facilitate monitoring and review, and yet may have their potential limited if implemented so as to

encourage a "ticking the box" over judgment mentality (Bedard et al. 2008, 199). The PCAOB finds even large, annually inspected accounting firms have deficiencies in identification and testing of controls (Church and Shefchik 2013, PCAOB 2012a). In 2013, the PCAOB issued a 36 page staff audit practice alert, to notify engagement partners, senior members of the engagement team, engagement quality reviewers, and audit committees about on-going deficiencies in audits of internal control and suggested remedies including improving risk assessment (PCAOB 2013).

Given the mixed evidence of association between auditor quality and restatement, the lack of research on the association of auditor quality and unintentional error, the possible implication of auditor selection bias, and the PCAOB's concerns about risk assessment, it is an empirical question as to whether or not auditor quality is related to unintentional error in audited financial statements. Accordingly, I predict in the alternative:

> ***H12:*** *The likelihood of restatement to correct unintentional error is decreasing in auditor quality.*

While I predict there is a negative relationship between auditor quality and restatements that correct unintentional error, I expect that higher quality auditors will be associated with fewer restatements that correct intentional misstatements. This prediction is consistent with Francis et al. (2013) and Ettredge et al.'s (2014) findings that higher quality auditors (proxied by auditor size) are associated with fewer severe restatements (respectively, income-decreasing restatements and restatements with more negative market reaction to restatement announcement) but not less severe restatements. These findings of association between auditor quality and more severe restatement are consistent with reputation protection theory and Lennox and Pittman's (2010) finding that an association with AAERs is four times more likely for non-Big 4 auditors (a common proxy for lower auditor quality) than Big 4 auditors. Whereas I argued smaller

auditors may better estimate the risk of unintentional error than larger auditors, I do not expect this to be the case for the risk of intentional misstatement. Intentional misstatements being indicated by observable benchmarks such as management earnings guidance, analysts' forecasts, and bonus targets are more transparent than unintentional errors. Since auditors of greater and lesser quality are likely to recognize transparent audit risks, reputation theory predicts higher quality auditors will respond with more audit effort than auditors of lesser quality.

In other words, while the relationship between auditor quality and restatements that correct unintentional error may be a competence story, I expect the relationship between auditor quality and restatements that correct intentional misstatement is more of an incentives story. I do not develop a hypothesis or test directly the prediction that auditor quality is negatively related to restatements that correct intentional misstatements. Rather, I predict a comparatively greater negative association between auditor quality and restatements that correct intentional misstatement than between auditor quality and restatements that correct unintentional error. A positive association or lack of association between auditor quality and restatements that correct intentional misstatement will bias against finding predicted results. My final prediction, developed based on reputation theory, the reasoning supporting *H12* and empirical evidence, expressed in the alternative is:

> ***H13:*** *Restatements that correct intentional misstatement are more likely to be decreasing in auditor quality than restatements that correct unintentional error.*

3.4 Sample Selection and Research Design

3.4.1 Samples

The sample used to test hypotheses includes 121 restatements that correct unintentional error, 104 restatements that proxy for restatements that correct intentional misstatement, and a control group of 121 firm-years without restatement. Chapter 2 details my selection of the restatements that correct unintentional error and restatements that proxy for restatements that correct intentional misstatement (Table 11). To create the control group, I chose companies that are nearest in asset size to the companies that restated to correct unintentional error that also (1) have all necessary data for tests of hypotheses, (2) are within the same two digit SIC code and year, and (3) are without restatements in the *AuditAnalytics* database at the time of data collection[34]. These criteria resulted in matching 70% of companies without restatement within 10% of the asset size of their matched unintentional error restatement observation. Pairs of these three types of companies are combined in different ways to form three samples, i.e., the ERROR sample is comprised of the 121 restatements that correct unintentional error and the 121 control companies; the INTENT sample is comprised of the 104 restatements that proxy for restatements that correct intentional misstatement and the 121 restatements that correct unintentional error; and the INTENTvsNORESTATE sample is comprised on the 104 restatements that proxy for restatements that correct intentional misstatement and the 121 control companies without restatement.

[34] April, 2013. There is always the possibility that one or more of the control companies will restate in the future.

3.4.2 Research design

I use three logistic models to test the 13 hypotheses. Model 1, estimated using the ERROR sample, tests the six hypotheses that investigate associations between internal control quality and CFO and audit committee characteristics. Model 2, also estimated using the ERROR sample, tests the associations between restatements that correct unintentional error and CFO, audit committee, and auditor characteristics. Model 3, estimated using the INTENT sample, compares restatement likelihoods associated with CFO, audit committee, and auditor characteristics between restatements that correct unintentional error and restatements that correct intentional misstatement. I also re-estimate Model 2 using the INTENTvsNORESTATE sample to aid in the interpretation of Model 3 results. While Models 2 and 3 investigate the total effects of CFO, audit committee, and auditor characteristics and suffice to investigate the hypotheses, mediated logistic models will be used to test the indirect effect on restatements that correct unintentional error (intentional misstatement) through internal control quality when total effects are significant.

Table 12 presents all variables used in the models. Table 12 also itemizes alternate measures of several test variables and the five components of one composite measure. While models will be estimated and reported using all measures shown in Table 12, hypotheses tests will be conducted on models that omit control variables that do not improve model fit and are not significant in any of the models.

3.4.2.1 Model 1: Internal control quality association tests

To test the hypotheses that investigate associations between internal control quality and organizational change (*H1, H6,* and *H7*), CFO financial expertise (*H2*), CFO influence (*H4*) and

audit committee financial expertise (*H9*), I estimate the following logistic regression (Model 1) using the ERROR sample comprised of 121 restatements that correct unintentional error and the matched control group of 121 firm-years without restatement:

$$
\begin{aligned}
ICWEAK_{it} = {} & \beta_0 + \beta_1 ORGCHANGE_{it} + \beta_2 CFOEXP_{it} + \beta_3 CFOPOWER_{it} \\
& + \beta_4 ORGCHANGE_{it} \; X \; CFOEXP_{it} + \beta_5 ORGCHANGE_{it} \; X \; CFOPOWER_{it} \\
& + \beta_6 ACEXP_{it} + \beta_7 AUDITORQUAL_{it} + \beta_8 AUDITFEE_{it} + \beta_9 AUDITINF_{it} \\
& + \beta_{10} AUDITTENURE_{it} + \beta_{11} AUDITORCHANGE_{it} + \beta_{12} CFOCHANGE_{it} \\
& + \beta_{13} CFO\_EQUITY\_PAY_{it} + \beta_{14} GOVERNANCE_{it} + \beta_{15} IAF_{it} \\
& + \beta_{16} FIRMSIZE_{it} + \beta_{17} AGE_{it} + \beta_{18} LOSS_{it} + \beta_{19} COMPLEXITY_{it} \\
& + \beta_{20} FOREIGN_{it} + \beta_{21} LEVERAGE_{it} + \beta_{22} MTB_{it} + \beta_{23} ISSUE_{it} \\
& + \beta_{24} VOLATILITY_{it} + \beta_{25} MISSTATEINCENT_{it} \\
& + \sum_t YEAR_t + \sum_I INDUSTRY_t + \varepsilon_{it} \qquad \text{[1a, 1b]}
\end{aligned}
$$

The binary variable (*ICWEAK*) in Model 1 captures auditor opinions on the effectiveness of internal control over financial reporting. *AuditAnalytic*'s database codes auditor's SOX 404 opinions as originally filed and as amended. Accordingly, I plan to use two complementary measures of internal control quality. Model 1a's dependent variable is auditor's SOX 404 opinions as originally filed (*ICWeakORIGINAL*). Model 1b's dependent variable is auditor's SOX 404 opinions after amendments, if any (*ICWeakAMENDED*). Internal control quality assessments may be revisited and SOX 404 reports amended upon discovery of the need to restate financial statements. Thus, although *ICWeakAMENDED* may more accurately reflect internal control quality than *ICWeakORIGINAL*, results of tests using the amended auditor opinions of internal control may be confounded by differences in auditors' control evaluations given that evaluations may be revisited more frequently in the restatement group than in the control group.

My measure of organizational change (*ORGCHANGE)* captures changes resulting from the company engaging in restructuring, acquisitions or a first-time global expansion during the

restated year. The theoretical model predicts that the association between organizational change (*ORGCHANGE*) and internal control quality depends on the CFO's financial expertise (*CFOEXP*) and influence (*CFOPOWER*). A significant β_4 (β_5) supports *H6* (*H7*) that CFO financial expertise (influence) moderates the association between organizational change and internal control quality.

H1 tests the association between organizational change and internal control quality. Since I hypothesize that the association between organizational change and internal control quality depends on CFO characteristics, I conclude *H1* is supported if the confidence interval of the product of the odds ratios of *ORGCHANGE*, *CFOEXP X ORGCHANGE,* and *CFOPOWER X ORGCHANGE* is greater than one[35]. However, if I do not find support for *H6* (*H7*), I will instead examine the significance of *ORGCHANGE* in Model 1 with the interaction variables omitted to test *H1*.

Similarly, given the hypothesized interaction of organizational change and CFO characteristics, investigating the association between internal control quality and CFO financial expertise (*CFOEXP*) (*H2)* and influence (*CFOPOWER*) (*H4)* involves considering both main and interaction effects. Accordingly, I will investigate whether or not the product of the odds ratios for *CFOEXP* (*CFOPOWER*) and *CFOEXP X ORGCHANGE* (*CFOPOWER X*

[35] While I present results of logistic regressions both as logits (log odds) and as odds ratios (exponentiated logits), interpretation of results is more straight-forward when results are expressed as odds ratios because "a remarkable property of logistic regression [is] that the odds ratio of an effect is constant regardless of the values of the covariates" (STATA technical bulletin, January 2000, p. 21). This constancy does not hold for logistic results expressed as logits where interpretation of results depends on values of the other variables (Ai and Norton 2003). When results are reported as odds ratios, an association is significantly positive if the confidence interval for that variable is greater than 1. Similarly, when results are reported as odds ratios, an association is significantly negative if the confidence interval is less than 1. Results for hypotheses tests are insignificant if the confidence interval includes 1 when results are reported as odds ratios. Interactions expressed as odds ratios operate multiplicatively. Accordingly, the test of the overall significance of a variable with a significant interaction when results are expressed as odds ratios is whether or not the confidence interval of the product of the main and interaction effects contains 1.

ORGCHANGE) is less than one to test *H2 (H4).* If the β_4 and β_5 odds ratios are not significant, then finding that the confidence interval of the odds ratio of β_2 (β_3) is less than one in a Model 1 without interaction terms supports *H2 (H4).*

Internal control quality's association with the audit committee's financial expertise (*ACEXP*) is tested by *H9*. If the confidence interval of the odds ratio β_6 is less than one, *H9* is supported. I do not predict a significant association between auditor quality and internal control quality based on the theoretical model that I developed.

Model 1 test variable measures

I considered using six alternative *CFOEXP* measures to capture differing aspects of CFO financial expertise, i.e., a CFO's audit experience at a Big 4/5/6/8 audit firm (depending upon the time period), (*CFOBIG4),* audit experience as a Partner at a Big 4/5/6/8 or national firm *(CFO_BIG4PARTNER),* professional designations (*CFOCPA*), graduate business education (*CFOMBA*), previous experience as a CFO at another company (*CFOPREVIOUS*), and tenure at the restating company (*CFOTENURE*). The first four of these measures, or combinations thereof, are frequently used in the literature to proxy for financial expertise and capture experience gained outside of the company. Aier et al. (2005) test years of work experience as CFO, work experience as a CFO at another company, whether or not the CFO has an MBA and whether or not the CFO has CPA accreditation. They find all but work experience as CFO at another company are associated with restatement likelihood. The *CFOBIG4* and *CFO_BIG4PARTNER* measures capture whether or not the CFO has experience with different accounting systems, while *CFOTENURE* captures experience with the company's systems.

While I report descriptive statistics for all six measures of CFO financial expertise in Table 15, for parsimony, I use only *CFOBIG4* in tests of hypotheses. I selected *CFOBIG4* as the measure of financial expertise for hypotheses testing because (1) there are too few companies with CFOs with experience as national audit Partners (*CFO_BIG4PARTNER)* to conduct tests; (2) three other measures of financial expertise (*CFOCPA*, *CFOMBA*, and *CFOPREVIOUS*) do not vary between subsamples used in tests (Table 16 $p > 0.25$); and (3) the length of time the CFO has been with a company (*CFOTENURE*) may capture not only financial expertise but also CFO influence (*CFOPOWER*) which may confound results of tests.

The logarithm of the total compensation of the CFO scaled by the total compensation of the CFO is my *CFOPOWER* measure. I base this measure on Feng et al.'s (2011) computation of CEO payslice (i.e., CEO total compensation divided by compensation of top five executives' compensation) and compute CFO and CEO total compensation following Hoitash et al. (2012). I use total compensation rather than equity compensation in the measure since post-SOX CFOs' incentive compensation has been reduced (Indjejikian and Matejka 2009). Balsam et al. (2014) and Armstrong et al. (2010) discuss selection bias resulting from restricting a sample to firms covered in Execucomp. In this study over one third of the sample lacks Execucomp data. Furthermore, Execucomp changed the computation of equity incentives during the sample period (Kini and Williams 2012). Accordingly, I gather compensation data for all companies from proxy statements. When proxy statements do not report stock options and stock awards at grant date values, I value these equity grants using a Black-Scholes model and information from proxy statements and 10-Ks.

Defond et al. (2005) discuss the controversy surrounding the qualification and experience requirements of audit committee financial experts. Defond et al. (2005) find that the market

reacts positively (does not react) to the appointment of financial experts defined narrowly as those with prior accounting or audit related experience (defined broadly to include those with experience supervising the preparation of financial statements). Three measures of *ACEXP* consistent with a narrow definition of financial expertise are: (1) whether or not at least one member of the audit committee has audit experience at a Big 4 firm (*ACBIG4*); (2) whether or not at least one member of the audit committee has a CPA or CA qualification (*ACCPA*); and (3) the proportion of audit committee members with prior accounting or audit related experience measured following Hoitash et al. (2009) and Cohen et al. (2014) (*ACAFE*). A fourth measure, the proportion of audit committee members with MBAs (*ACMBA)* is more likely to capture the power or status of the audit committee, an idea that is investigated in recent studies (e.g. Badolato et al. forthcoming). Just as I report descriptive statistics for multiple measures of CFO financial expertise (Table 15) but select only one measure for hypotheses testing for parsimony, I report descriptive statistics for all four measures of audit committee financial expertise (Table 15) but use only the *ACBIG4* measure, the measure that is most likely to capture experience with financial statements of public companies, in hypotheses tests.

3.4.2.2 Model 2: Unintentional error restatement association tests

To test the hypotheses that investigate associations between restatements that correct unintentional error and CFO financial expertise (*H3*), CFO influence (*H5*), audit committee financial expertise (*H10*) and auditor quality (*H12*), I estimate the following logistic regression (Model 2) using the ERROR matched sample comprised of the 121 restatements that correct unintentional error and the matched control group of 121 firm-years without restatement:

$$
\begin{aligned}
ERROR_{it} \quad = {} & \beta_0 + \beta_1\, ORGCHANGE_{it} + \beta_2 CFOEXP_{it} + \beta_3 CFOPOWER_{it} \\
& + \beta_4 ACEXP_{it} + \beta_5 AUDITORQUAL_{it} + \beta_6 AUDITFEE_{it} + \beta_7 AUDITINF_{it} \\
& + \beta_8 AUDITTENURE_{it} + \beta_9 AUDITORCHANGE_{it} + \beta_{10} CFOCHANGE_{it} \\
& + \beta_{11} CFO\_EQUITY\_PAY_{it} + \beta_{12} GOVERNANCE_{it} + \beta_{13} IAF_{it} \\
& + \beta_{14} FIRMSIZE_{it} + \beta_{15} AGE_{it} + \beta_{16} LOSS_{it} + \beta_{17} COMPLEXITY_{it} \\
& + \beta_{18} FOREIGN_{it} + \beta_{19} LEVERAGE_{it} + \beta_{20} MTB_{it} + \beta_{21} ISSUE_{it} \\
& + \beta_{22} VOLATILITY_{it} + \beta_{23} MISSTATEINCENT_{it} \\
& + \textstyle\sum_t YEAR_t + \sum_j INDUSTRY_t + \varepsilon_{it} \qquad [2]
\end{aligned}
$$

Model 2 is Model 1 with a different dependent variable, *ERROR,* and the interactions between organizational change and the CFO's financial expertise and influence omitted. *ERROR* is an indicator variable that is equal to 1 if the company restates to correct unintentional error and 0 if the company is without restatement.

I exercise caution before concluding that a confidence interval of the odds ratio of β_2 (β_3) for *CFOEXP (CFOPOWER)* that is less than one supports *H3 (H5)*, i.e., that increased CFO financial expertise (influence) is associated with a lower likelihood of restatement to correct unintentional error. If model fit is significantly improved by including the interaction of organization change and CFO financial expertise (influence) in Model 2[36], I will modify Model 2 to include interactions between organizational change and CFO financial expertise (influence). Accordingly, if these interactions are included, I will investigate whether or not the confidence interval of the product of the odds ratios for *CFOEXP* (*CFOPOWER*) and *CFOEXP X ORGCHANGE* (*CFOPOWER X ORGCHANGE*) is less than one before concluding on *H3 (H5).*

A confidence interval of the odds ratio β_4 of *ACEXP* that is less than one supports *H10* and shows that increased audit committee financial expertise is associated with a lower

[36] To decide whether or not model fit is improved, I will check both that the area under the ROC curve is greater when the interactions are added to Model 2, relative to Model 2 without interactions, and that a likelihood ratio test comparing the expanded Model 2 with interactions to Model 2 without interactions is significant.

likelihood of restatement to correct unintentional error. A confidence interval of the odds ratio β_5 of *AUDITQUAL* that is less than one supports *H12* and shows that increased auditor quality is associated with a lower likelihood of restatement to correct unintentional error.

Model 2 test variable measures

The same *CFOEXP*, *CFOPOWER*, and *ACEXP* measures used in tests of respectively, *H2*, *H4*, and *H9,* are also used to test, respectively, *H3*, *H5*, and *H10*. I use an office size (*OFFICESIZE*) measure of auditor size to proxy for auditor quality (*AUDITORQUAL*) in main hypotheses tests and a firm size measure (*BIG4*) only in additional analyses. I measure *OFFICESIZE* as the logarithm of total office-specific audit fees following Francis and Yu 2009 and Francis and Michas 2013. *BIG4* (used in additional analysis) captures whether or not the company engaged a Big 4/5/6/8 auditor, depending upon the time of the engagement (i.e., Deloitte, EY, KPMG, PwC today).

3.4.2.3 Model 3: Comparison of restatement likelihood between restatement types

Model 3 tests hypotheses *H8 (*whether or not increasing CFO influence reduces the likelihood of restating to correct unintentional error more than the likelihood of restating to correct intentional misstatement), *H11 (*whether or not increasing audit committee financial expertise reduces the likelihood of restating to correct intentional misstatement more than the likelihood of restating to correct unintentional error), and *H13 (*whether or not increasing auditor quality reduces the likelihood of restating to correct intentional misstatement more than the likelihood of restating to correct unintentional error). Model 3 is Model 2 with a different dependent variable, *INTENT*. *INTENT* is an indicator variable that is equal to 1 if the company restates to correct intentional misstatement and 0 if the company restates to correct unintentional

error. The same *CFOPOWER*, *ACEXP,* and *AUDITQUAL* measures used in tests of, respectively, *H5*, *H10*, and *H12,* are also used to test, respectively, *H8*, *H11*, and *H13*. I estimate Model 3 using the INTENT sample comprised of the 104 intentional misstatement restatements and the 121 restatements that correct unintentional error.

Implicit in the design of Model 3 tests is the assumption that increasing CFO influence, audit committee financial expertise, and auditor quality are associated with changes in the likelihood of restating to correct unintentional error and intentional misstatement in a directionally consistent way. For example, I assume that increasing audit committee financial expertise will not be associated with a significant decrease in the likelihood of unintentional error while also being associated with a significant increase in the likelihood of intentional misstatement. When significant associations between a test variable and the two types of restatements are directionally opposite, it is not readily apparent which of the two associations is driving any significant difference in Model 3. Therefore, before concluding on Model 3 tests I will regress *INTENTvsNORESTATE,* a variable equal to 1 if the company restates to correct intentional misstatement and 0 if the company is without restatement, on the right hand side variables in Model 2 and compare the results of this regression to Model 2 results to ensure that this assumption is not violated.

A confidence interval for the odds ratio β_4 of *ACEXP* that is less than one supports *H11* (i.e., restatements that correct intentional misstatement are more likely to be decreasing in audit committee financial expertise than restatements that correct unintentional error) provided that there is no reason to suspect that the directional consistency assumption is violated and that restatement likelihood is decreasing in audit committee financial expertise, for any significant restatement/audit committee financial expertise association(s).

Similarly, a confidence interval for the odds ratio β_5 of *OFFICESIZE* that is less than one supports *H13* (i.e., restatements that correct intentional misstatement are more likely to be decreasing in auditor quality than restatements that correct unintentional error) provided there is no reason to suspect that the directional consistency assumption is violated and restatement likelihood is decreasing in auditor quality, for any significant restatement/auditor quality association.

Before I can conclude *H8* is supported (i.e., restatements that correct unintentional error are more likely to be decreasing in CFO influence than restatements that correct intentional misstatements), results must show that the confidence interval of *CFOPOWER* (or of the product of *CFOPOWER* and *CFOPOWER X ORGCHANGE* if model fit is significantly better with interactions added) is greater than one provided that there is no reason to suspect that the directional consistency assumption is violated and that restatement likelihood is decreasing in CFO influence for any significant restatement/CFO influence association.

3.4.2.4 Control variables

Because my hypotheses investigate the association between internal control quality and restatements and organizational change, auditor quality, audit committee expertise, and CFO expertise and influence, I include variables to control for other auditor, management, governance, and firm characteristics that prior research shows are associated with internal control quality and restatements. I also include variables used in prior restatement research to control for restatement risk since Models 1 and 2 compare firm-years restated to correct unintentional error to a control group without restatement.

Specifically, since my measure of auditor quality is auditor size, I include variables to control for other auditor characteristics that, if omitted, might bias results. I expect auditor effort (*AUDITFEE*) to increase as control weaknesses and the likelihood of restatement increase. I measure *AUDITFEE* as the natural logarithm of audit fees (Cohen et al. 2014, Keune and Johnstone 2012, Lobo and Zhao 2013).[37] *AUDITINF* captures the impact the loss of the company as an audit client would have on the audit office (Francis and Michas 2013, Francis and Yu 2009). I do not predict the sign of the association with *AUDITINF* as auditors may either waive adjustments more willingly to retain an influential client or resist client demands to protect their reputation and ensure long-term viability. Krishnan (2005) finds control weaknesses are decreasing in auditor tenure. In contrast, Keune and Johnstone (2012) cite several studies showing that audit quality diminishes with the length of the client-auditor relationship. As there is potential for both loss of independence and greater knowledge of the client's business with longer tenure (*AUDITORTENURE*), I control for auditor tenure but do not make a directional prediction (see Davis et al. 2009 and Knechel et al. 2013 for overviews of audit tenure literature). I also control for, but do not make a directional prediction on, *AUDITORCHANGE.* While there is a risk that in the first year of an audit engagement (*AUDITORCHANGE*), auditors may not have an adequate understanding of the client`s business, it is also possible that auditors expend extra effort in the initial months of the engagement to ensure any contentious issues are addressed promptly.

[37] I chose this simpler measure rather than using the residual of a fee model because the fee models are not ideal for this study setting. The Keune and Johnstone (2011) include internal control weaknesses, a dependent variable in this study, as one of the predictor variables in their fee model. The Lobo and Zhao (2013) fee model includes the predicted probability of misstatement, based on the misstatement detection model of Dechow et al. (2011) who created their model to predict AAERs. AAERs relate to intentional misstatement but not necessarily to unintentional error restatement risk.

I also control for CFO incentives to misreport (*CFO_EQUITY_PAY*) and a change in CFO (*CFOCHANGE).* I choose to use CFO equity compensation as a control variable rather than CEO equity compensation as Balsam et al. (2014) overview prior literature that finds that "equity incentives of CFO play a more significant role in determining internal control quality than those of the CEO". Jiang et al. (2010, 515) find in the 1993-2006 period that "CFO equity incentives range from 28% (lower quartile) to 81% (the upper quartile) of CEO equity incentives". I measure *CFO_EQUITY_PAY* as the logarithm of the proportion of equity pay in a CFO's total compensation (CFO_EQUITY_PAY). The untransformed measure is commonly used by compensation consultants (Armstrong et al. 2010). I compute equity pay as the grant date value of restricted stock award grants and stock options (using a Black-Scholes model). I do not predict a direction for associations with *CFO_EQUITY_PAY*. Consistent with a pre-SOX literature finding strong association between misreporting and stock option compensation (e.g., Burns and Kedia 2006), Jiang et al. (2010, 514) find that "CFO incentives are still increasing in likelihood of beating analyst forecasts", albeit "neither CEO nor CFO equity incentives are positively associated with the magnitude of accruals during the 2002-2006 post-SOX period". However, in untabulated results, Balsam et al. (2014) find a negative association between reporting of material weaknesses and CFO equity-based compensation and Indjejikian and Matejka (2009) find that CFO incentive compensation is reduced post-SOX. *CFOCHANGE* is an indicator variable equal to 1 if the CFO is appointed in the restated year and 0 otherwise. *CFOCHANGE* controls for the CFO having had insufficient time to modify internal control after appointment and for any change in resourcing of accounting and control functions that accompanies CFO change. Since resources that accompany such a change may either increase or decrease, I make no directional prediction for *CFOCHANGE*.

To control for differences in corporate governance I use *CEOINDEP,* an indicator variable for whether or not the CEO is also the board chair, a dual role that is associated with a greater likelihood of misstatement (Efendi et al. 2007). I also report descriptive statistics for *GOV*, a composite measure, of governance in Table 15. Prior research frequently uses such composite measures for governance (e.g., Carcello et al. 2006, DeFond et al. 2005) as an alternative to the simpler measure of CEO duality. Earlier composite measures included audit committee size and audit committee independence measures for which there is little variation post-SOX. Accordingly, I construct my *GOV* measure, drawing on Hoitash et al.'s (2012) and Lisic et al.'s (2014) composite measures, by summing five indicator variables that assign higher values to companies where (1) the CEO is not the board chair, (2) the CEO is not a founder of the company, (3) the number of directors is less than the sample median, (4) the percentage of directors who are independent is greater than the sample median, and (5) the percentage of independent board members with tenure longer than the CEO's tenure is greater than the sample median. In choosing a single governance measure for parsimonious hypotheses testing, I selected *CEOINDEP* rather than *GOV* as Table 15 shows the later composite measure did not vary between subsamples ($p>0.7$). I expect that improving governance will improve internal control quality and decrease the likelihood of restatement.

Other measures are included in tests to control for other differences in internal control quality and restatement likelihood. *IAF* indicates whether or not the company had an internal audit function in the year used in the sample. Whereas external auditors may take a "top-down" approach to assessing controls if a substantive audit approach is adopted, the mandate of most internal audit functions would require a more complete assessment of internal controls. Accordingly, I would expect companies that refer to an internal audit function in the audit

committee charter or proxy statements to have fewer control weaknesses and restatements to correct unintentional errors. I also include variables to control for firm size, age, financial challenges, and complexity as Doyle et al. (2007b) find these firm characteristics, in addition to organizational change, are related to internal control quality. Smaller (*FIRMSIZE*), younger (*AGE*) and less profitable (*LOSS*) companies may not adequately resource their accounting and control functions. I expect the likelihood of poor internal control quality and unintentional error will be greater for these companies and for more diverse companies with a greater number of geographic and operating segments (*COMPLEXITY*) that need to integrate accounting systems and records.

I also include other controls documented in restatement research to control for differences in misstatement incentives; i.e., firm leverage (*LEVERAGE*), the market to book ratio (*MTB*), volatility of sales (*VOLATILITY*), an indicator variable of analyst following the company (*ANALYSTFOLLOW*), and whether or not the company is in a litigious industry (*LITIGIOUS*) (Bedard et al. 2014, Carcello et al. 2011b, Cohen et al. 2014, Ettredge et al. 2014, Francis et al. 2013). I expect that the associations between these five control variables and restatements proxying for restatements that correct intentional misstatements will be greater than associations with restatements correcting unintentional error in Model 3 but do not make a directional prediction for associations in Models 1 and 2. Indicator variables for year and industry are also included in tests.

3.4.2.5 Additional analyses

internal control mediation analyses

The theoretical model depicts both direct and indirect associations between CFO and audit committee characteristics and restatements that correct unintentional error; i.e., internal control quality mediates associations between CFO and audit committee characteristics and restatement likelihood. Models 2 and 3 test whether or not the total effect – the direct plus any indirect effects – are significant. In additional analyses I use mediated logistic models with internal control as a mediating variable to determine the portion of the total significant CFO financial expertise, CFO influence, audit committee financial expertise and auditor quality main effects on restatement likelihood that is mediated by internal control quality. The mediated logistic analysis estimates three regressions to compute an indirect effect: (a) Model 1 without interaction terms, (b) Model 2 (3) which estimates the total effect on restatements that correct unintentional error (compares associations between restatements that correct intentional misstatement and restatements that correct unintentional error), and (c) Model 2 (3)[38] with *ICWEAK,* the mediating variable, included. Mediated logistic analyses will only be conducted when the main effect of a test variable(s) (i.e., *CFOEXP*, *CFOPOWER*, *ACEXP*, and *AUDITQUAL*) is shown to be significantly associated with both internal control quality (Model 1) and either the *ERROR* (Model 2) or *INTENT* (Model 3) restatement variable.

[38] The statistical software I am using for the mediated analysis will compute indirect effects, but does not provide standard errors or statistical tests for coefficients. As needed, I will bootstrap to compute the standard errors and confidence intervals.

endogeneity

Since only the earliest restated year of restating companies is included in the sample, I do not expect the CFO's choice to accept an appointment, the audit committee members' willingness to serve on the audit committee, or the auditor's choice to accept the engagement to be related to restatement decisions. However, to address concerns that those responsible for financial reporting quality with greater expertise or influence select companies with lower financial reporting risk, I will conduct a Hausman test where the null hypothesis is the model as described and the alternative is the model that includes lagged values of *CFOEXP*, *CFOPOWER*, *ACEXP* and *AUDITQUAL* (computed using the previous year's data) as instrumental variables.

3.4.3 Results

3.4.3.1 Descriptive statistics

Table 13 compares market capitalization, frequency of restated years, and frequency of industry for the 121 companies that restate to correct unintentional error in the ERROR and INTENT samples, the matching control group of 121 companies without restatement in the ERROR sample, and the 104 companies that restate to correct intentional misstatement in the ITNENT sample. Table 13 shows that these three groups of companies have similar distributions of market capitalization, fiscal years, and industries.

Table 14 Panel A presents descriptive statistics for test and control variables, together with Wilcoxon rank-sum and Pearson *Chi*-square comparisons between groups with and without internal control weaknesses. Continuous variables are winsorized at 1% and 99% to mitigate against outliers. Only the *ICWeakAMENDED* measure of internal control quality is used in Table

14 as only the *ICWeakAMENDED* measure of internal control quality can be used in Model 1 tests. Model 1 cannot be estimated with *ICWeakORIGINAL* because auditors initially filed so few reports of ineffective control (i.e., 11 out of the 242 auditor's SOX 404 reports for the ERROR sample) that many industries and years predict effective control perfectly. When these industries and years are dropped from the estimation of Model 1, there are too few observations to run the regression required for Model 1 tests.

Consistent with prior internal control quality research, Panel A of Table 14 shows that for the ERROR sample (i.e., the sample used in tests of Models 1 and 2 that is comprised of companies that restate to correct unintentional error and companies without restatement), smaller (*FIRMSIZE*), less profitable (*LOSS*) companies and companies undergoing organizational change (*ORGCHANGE*) have less effective internal control (or disclose internal control weaknesses more frequently) on average. Of the CFO, audit committee, and auditor measures reported, only the *CFO_EQUITY_PAY* measure differs between companies with and without control weaknesses in the ERROR sample: on average, reporting of internal control weaknesses is associated with CFO compensation packages that include a lower proportion of stock award and stock option grants relative to total compensation.

Panel B of Table 14 compares control effectiveness between companies in the INTENTvsNORESTATE sample. This sample is comprised of companies that proxy for companies that restate to correct intentional misstatement and the group of companies without restatement included in the ERROR sample. Like Panel A, and consistent with the similar group distribution of market capitalization, firm-years, and industries reported in Table 13, Panel B shows reporting of ineffective control is associated with smaller, less profitable companies and with CFO compensation packages with proportionately less stock awards and stock option

grants. However, Panel B also shows a pattern of differences between companies with effective vs. ineffective control that differs from the pattern shown in Panel A: Panel B (Panel A) shows (1) that organizational change is the same (differs) for companies with and without control weaknesses; (2) greater analyst following is associated with less (the same) reporting of control weaknesses on average; and (3) audit committees with member(s) with Big 4 audit experience are associated with less (the same) reporting of control weaknesses on average.

Panel C of Table 14 compares test and control variables between groups in the ERROR sample. Companies in the group with restatements that correct unintentional error report control weaknesses more frequently on average ($p<0.01$) than companies in the control group without restatement. Consistent with Panel A's comparisons that show internal control is less effective on average for smaller, less profitable companies experiencing greater organizational change, Panel C shows companies in the group that restate to correct unintentional error are less profitable ($p<0.001$) and are experiencing greater organizational change ($p<0.05$) relative to companies without restatement. Panel C shows all test variables except organizational change (i.e., CFO financial expertise, CFO influence, audit committee financial expertise, and auditor quality) are the same ($p> 0.1$) in the group that restates to correct unintentional error and in the control group without restatement.

Panel D of Table 14 compares groups in the INTENTvsNORESTATE sample, the sample comprised of intentional misstatement correcting companies and the group without restatement. In addition to showing that companies that restate have less effective internal control ($p<0.001$), Panel D shows a pattern of differences between comparisons for the INTENTvsRESTATE sample (Panel D) and for the ERROR sample (Panel C) that is consistent with the differences summarized in discussion of Panel B comparisons. Specifically, Panel D

(Panel C) shows for the INTENTvsNORESTATE (ERROR) sample (1) that organizational change is the same (differs) for companies with and without restatement; (2) greater analyst following is associated with less (the same) likelihood of restatement on average; and (3) audit committees with member(s) with Big 4 audit experience are associated with less (the same) likelihood of restatement on average. Panel D shows test variables except audit committee financial expertise and CFO influence (i.e., organizational change, CFO financial expertise, and auditor quality) are the same (p> 0.2) in the group that restates to correct intentional misstatement and in the control group without restatement.

Panel E of Table 14 compares the two groups of restating companies in the INTENT sample. While both groups of restating companies experienced a greater incidence of loss on average than the group without restatement (Panels C and D), Panel E shows that companies that restate to correct unintentional error are less profitable than companies that correct intentional misstatement. While this might suggest that companies restating to correct unintentional error are more resource constrained, ineffective internal control is reported more frequently by companies that restate to correct intentional misstatement than companies that restate to correct unintentional error. This difference may reflect reporting differences rather than actual differences in internal control quality as analyst following is greater (p=0.056) for companies that restate to correct unintentional errors and ineffective control is reported for only 32.7% (16.5%) of companies that restate to correct intentional misstatement (unintentional error). Panel E shows test variables except audit committee financial expertise and CFO financial expertise (i.e., organizational change, CFO influence, and auditor quality) are the same (p> 0.2) in the group that restates to correct intentional misstatement and in the control group without restatement. The average CFO and audit committee financial expertise is greater in companies

that restate to correct unintentional error than in companies that restate to correct intentional misstatement (p<0.1).

Finding few significant differences for test variables in Panels A (i.e., the associations with Internal Control), Panel C (i.e., associations with restatement of unintentional error), and Panel E (differences in association strength between restatements that correct intentional misstatement and restatements that correct unintentional error) is less concerning than it might be in other studies. Table 14 does not model associations when there are significant interaction effects. Accordingly, Table 14 univariate comparisons may not be good indicators of results of five Model 1, two Model 2, and one Model 3 tests given that I predict CFO financial expertise and influence moderate the associations between organizational control and internal control quality and error-correcting restatements. Five hypotheses tests are not affected by these CFO characteristic interactions (i.e., the association between audit committee financial expertise and internal control quality (*H9*) and error-correcting restatements (*H10*); the association between auditor quality and error-correcting restatements (*H12*), and the stronger associations of audit committee financial expertise and auditor quality between restatements that correct intentional misstatements than error-correcting restatements (respectively, *H11* and *H13*)). While univariate results do not support four of these five predictions, Panel E demonstrates that companies that restate to correct intentional misstatement have on average less audit committee financial expertise than companies that restate to correct unintentional error as predicted by *H11*.

Table 15 also presents descriptive statistics and group comparisons. However, continuous variables in Table 15 are not winsorized. Table 15 includes other measures of CFO and audit committee financial expertise and a composite governance measure that are not used in hypotheses tests or reported in Table 14. Auditor quality, is the one test variable that is

continuous and for which interactions with organizational change are not predicted. Univariate results reported in Table 15 for auditor quality are insignificant as are univariate results reported in Table 14 for the association between auditor quality and error-correcting restatements (*H12*) and the stronger associations of auditor quality between restatements that correct intentional misstatements than error-correcting restatements (*H13*).

Table 16 Panel A reports the Pearson correlations between test and control variables for the ERROR sample, the sample used to test Models 1 and 2. Correlations between test variables (i.e., *ORGCHANGE*, *CFOBIG4*, *CFOPOWER*, *ACBIG4*, and *OFFICESIZE*) are low ($|r| < 0.2$). With two exceptions, significant correlations ($p<0.05$) between test and control variables included in Models 1 and 2 are also low ($|r|<0.3$). The two exceptions are the negative correlation between auditor quality (*OFFICESIZE*) and audit client influence (*AUDITINF*: $r= -0.708$ $p<0.001$) and the positive correlation between auditor quality and audit fees (*AUDITFEE*: $r=0.453$ $p<0.001$). The absolute values of significant correlations between pairs of control variables are less than 0.6. The average variance inflation factors (VIFs) are less than two, and all individual VIFs (including VIFs of the interactions with organizational change) are less than five for the ERROR sample (not in tables) indicating that multi-collinearity is not an issue.

Table 16 Panel B reports the Pearson correlations between test and control variables for the INTENT sample, the sample used to test Models 3. With one exception, the absolute value of the correlation is less than 0.61 for all significantly correlated variables ($p<0.05$) that are used together in any of the models. The exception is the correlation between auditor quality (*OFFICESIZE*) and the influence of the audit client (*AUDITINF*) ($r=-0.720$ $p<0.001$). Audit committee financial expertise (*ACBIG4*) is not correlated with any test or control variables and CFO financial expertise (*CFOBIG4*) is correlated with only one control variable (*AUDITFEE*)

(r= 0.144, p<0.05). In contrast, CFO influence within the organization (*CFOPOWER*) is correlated with eight control variables (|r|<0.4), and two test variables, i.e., organizational change (*ORGCHANGE*) (r=0.173; p<0.05) and auditor quality (*OFFICESIZE*) (r=-0.166; p<0.05)[39]. Organizational change (*ORGCHANGE*) is correlated not only with CFO influence (*CFOPOWER*), but also with several control variables (|r|<0.3). While audit quality (*OFFICESIZE*) is correlated *AUDITINF* (r=-0.720 p<0.001), *AUDITFEE* (r=0.453 p<0.001) and eight other control variables (|r|<0.4), it is correlated (r=-0.173 p<0.05) with only one other test variable, CFO influence (*CFOPOWER*). The average variance inflation factors (VIFs) are less than two, and all individual VIFs (including VIFs of the interactions with organizational change) are less than five, for the INTENT sample used in Model 3 tests (not in tables).

In summary, with the exception of the correlation between auditor quality (*OFFICESIZE)* and the control variable *AUDITINF* (r=-0.720 p<0.001), correlations are unlikely to confound results. Given the significant correlation with the test variable *OFFICESIZE*, *AUDITINF* will not be included in main tests.

3.4.3.2 Results presentation: logit vs. odds ratio metrics

Panel A of Table 17 presents logit metric test results for all three models. Panel A reports estimation results when control variables that are not significant and do not improve overall model fit are not included in model tests[40]. Panel F presents logit metric test results for all three models with all control variables included (i.e., the regressions in which the Panel A models are "nested"). I conducted likelihood ratio tests that show including all control variables listed in

[39] While *CFOPOWER* is correlated (r=-0.176 p<0.05) with engaging and auditor from a Big 4 audit firm (*BIG4)*, *BIG4* is used in additional analysis rather than in the main hypotheses tests.

[40] I retained all determinants that Doyle et al. (2007b) find associated with internal control quality, i.e., organizational change (*ORGCHANGE*), firm size (*FIRMSIZE*), profitability (*LOSS*), age (*AGE*), and complexity (*COMPLEXITY*).

Table 12 in Panel F regressions does not significantly improve the model fit over the model fit of the "nested" models reported in Panel A (p>0.2) (not in tables). Since, in contrast to effects measured with logits, odds ratio effects are constant regardless of the values of covariates, interpretation of results is more straight-forward when results are expressed as odds ratios. Accordingly, Panels B through E of Table 17 present odds ratio metric results, together with 90% confidence intervals (which are equivalent to one-sided 95% confidence intervals), and directional predictions that are not reported in Panels A and F. Panel B presents the odds ratio metric results for Model 1 using the ERROR sample. Panel C presents the odds ratio metric results for Model 2 using the ERROR sample. Panel D presents the odds ratio metric that tests Model 2 using the INTENTvsNORESTATE sample to aid in interpretation of any significant results of Model 3. Panel E presents the odds ratio metric results for Model 3 using the INTENT sample.

3.4.3.3 Model 1 test results: ERROR sample

Panel B of Table 17 presents results of tests of the six hypotheses that predict associations between internal control quality and CFO financial expertise, CFO influence, and audit committee financial expertise. Results are reported for Model 1b, the model that uses *ICWeakAMENDED* as the binary dependent variable. There are insufficient observations in years and industries that do not perfectly predict effective control to estimate Model 1a – the version of Model 1 that proxies internal control quality with auditor's SOX 404 opinions as originally filed (*ICWeakORIGINAL*). Reported results for the estimation of Model 1b exclude observations in industries that perfectly predict effective internal control (i.e., the Food and Tobacco; the Lumber, Furniture, and Parking; Chemicals; and Services industries). It is noteworthy that even after amendments auditors report ineffective control in SOX 404 reports for only one company

of the 121 without restatement and 20 companies of the 121 (16.5%) that restate to correct unintentional error in the ERROR sample used to test Models 1 and 2.

The results of the Hosmer-Lemeshow *Chi*-square test (p=0.94) and area under the ROC curve (0.8929) suggest good overall model fit. A likelihood ratio test comparing Model 1 estimated with all control variables listed in Table 12 (Panel F) to the nested model reported in Panels A and B is not significant (*Chi2* = 2.45, p=0.9309 not in tables).

Panel B shows the interactions between organizational change and CFO financial expertise and influence are not significant: the p-value of the interaction *CFOBIG4*ORGCHANGE* (*CFOPOWER*ORGCHANGE*) is equal to 0.318 (0.644) and the 90% confidence intervals cross one (respectively, CI: 0.05 – 2.12 and CI: 0.14 – 2.99). Therefore, *H6* and *H7* are not supported. Accordingly, I test *H1*, *H2* and *H4* by estimating Model 1 without the interactions of CFO financial expertise and influence and organizational change (not in the tables). I find using a model without the interaction that organizational change (*ORGCHANGE*) is significant and that the odds of internal control weaknesses are at least 11% greater when companies are undergoing organizational change (CI: 1.11 – 12.15, p<0.05 one-sided not in tables). Thus, *H1* is supported. I also find using a model without interactions that increasing CFO influence (*CFOPOWER*) is associated with an <u>increase</u> in the odds of internal control weakness (CI: 1.15 – 5.64, p<0.05 one-sided, not in tables) which is opposite to the *H4* prediction. Hence, *H4* is not supported. Further, when using a model without interactions I find that the odds of internal control weakness are the same for CFOs with greater and lesser financial expertise (*CFOBIG4*) (CI: 0.42 – 3.177, p>0.8, not in tables). Consequently, *H2* is not supported. These results for *H1*, *H2* and *H4* using the model without interactions are consistent with results (p< 0.1 one-sided) reported in Panel B for respectively for *ORGCHANGE* (p<0.1 one-sided),

CFOBIG4 ($p>0.5$), and *CFOPOWER* ($p<0.05$ one-sided, CI: 1.01 – 8.98) when the insignificant interactions are included in Model 1 tests.

The final Model 1 test investigates the association between internal control quality and audit committee expertise (*H9*). The result using a model without interactions is the same as the result reported in Panel B: while the odds ratio for *ACBIG4* shows the odds of internal control weaknesses for companies with audit committees with greater financial expertise is half that of companies with audit committee with lesser financial expertise, this difference in odds is not significant (CI: 0.21-1.47). Accordingly, this result, while directionally consistent with *H9,* does not support H9. Significant associations between control variables and internal control quality (i.e., associations with *AUDITFEE* and *FIRMSIZE*) are directionally as predicted.

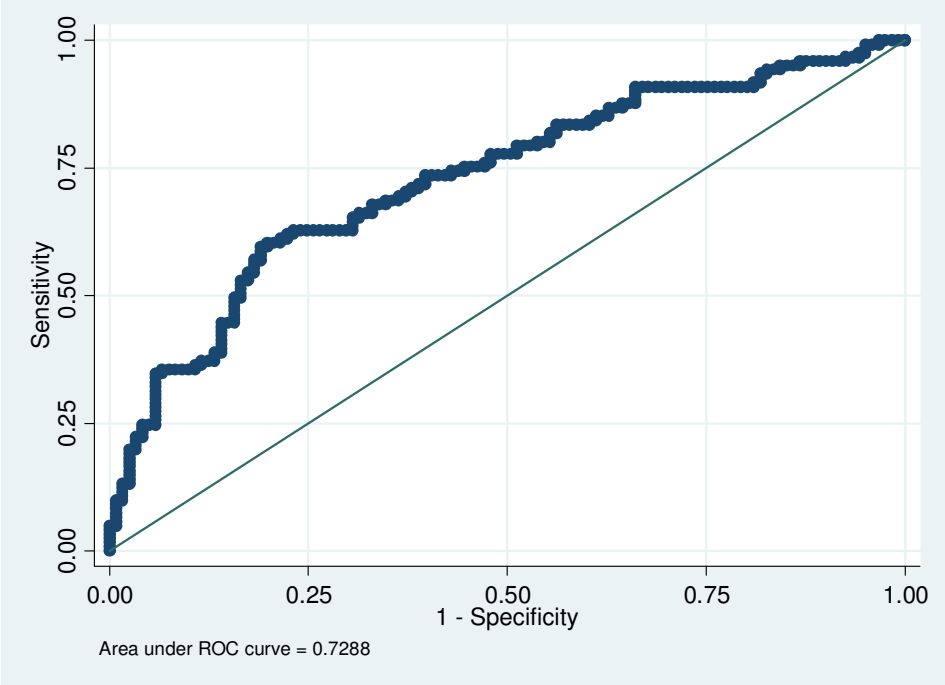
In summary, of the six hypotheses of associations with internal control quality, only *H1*, the negative association between internal control quality and organizational change is supported. Results are directionally consistent but not significant for three of the other five hypotheses – the moderating effect of CFO financial expertise and influence on the association between organizational change and internal control quality (*H6* and *H7)* and the positive association between audit committee financial expertise and internal control quality (*H9)*. The predicted positive relationship between internal control quality and increasing CFO financial expertise (*H2*) and increasing CFO influence (*H4)* is not supported. In short, Model 1 tests show firm factors - organizational change and firm size (*ORGCHANGE* and *FIRMSIZE*) - rather than the influence of CFOs or the financial expertise of CFOs and audit committees are associated with internal control quality proxied by auditor's SOX 404 opinions of control effectiveness.

3.4.3.4 Model 2 test results: ERROR sample

Panel C of Table 17 and the second column of Panel A show results of tests for the four hypotheses that predict associations between restatements that correct unintentional error and CFO financial expertise (*H3*), CFO influence(*H5*), audit committee financial expertise (*H10*), and auditor quality (*H12*). Model 2 tests use the ERROR sample. The interactions of organizational change and both CFO financial expertise (*CFOBIG4*ORGCHANGE*) and CFO influence (*CFOPOWER*ORCHANGE*) are included in the regressions shown in Panels A and C. These interactions are included because organizational change (*ORGCHANGE)* is significant ($p<0.1$ one-sided, not in tables) in a model estimated without these two interactions. Furthermore, a likelihood ratio test shows that the including these two interactions significantly improves ($p<0.05$) model fit in comparison with the model without these interactions. The results of the Hosmer-Lemeshow *Chi*-square test ($p=0.31$) reported in Panel C and the area under the ROC curve shown in Figure 2 (0.7288) suggest the overall model fit is adequate. A likelihood ratio test that compares Model 2 estimated with all control variables listed in Table 2 and the two interactions with organizational change (Panel F) to the nested model reported in Panels A and C is not significant (*Chi2* = 3.01 $p=0.8840$ not in tables).

FIGURE 2

Area Under the ROC[a] Curve for the Logistic Regression Reported in Table 17 Panel C[b]



a. The area under the receiver operator characteristic (ROC) curve above is a measure of the overall ability of the logistic regression reported in Table 17 Panel C to discriminate between firm-years that restate to correct unintentional error and "matched" firm-years (in the same year and two digit SIC code nearest in asset value) without restatement. The curve is created by plotting the false positives / true positives (sensitivity) against 100% - false negatives / true negatives (1 - specificity) at various classification cut-off thresholds. The curve depicts the trade-offs between false positive and false negative.

b. The logistic regression reported in Table 17 Panel C tests associations between restatements that correct unintentional error and CFO financial expertise, CFO influence, audit committee financial expertise and auditor quality.

Panel C reports the odds ratio that compares the odds of restating to correct unintentional error for companies with greater CFO financial expertise to the odds of error-correcting restatement for companies with lesser CFO financial expertise. The confidence interval results reported at the bottom of Panel C show that overall, the odds of restatement to correct unintentional error for companies with CFOs with greater financial expertise are less than half

the odds of restatement for companies with lesser financial expertise (CI: -2.49 – 0.47). Hence, *H3* is supported. However, the confidence interval is greater than one for *CFOBIG4* (CI: 1.01 – 3.39) and less than one for *CFOBIG4*ORGCHANGE* (CI: 0.06 – 0.60), the variables that were combined to compute the overall CFO financial expertise effect reported at the bottom of Panel C. These divergent results shows that the effect of CFO financial expertise depends on organizational change and that companies undergoing organizational change are driving the results for *H3*.

Panel C also reports the odds ratio that compares the odds of restating to correct unintentional error for companies with more influential CFOs to the odds for companies with lesser influential CFOs. The results reported at the bottom of Panel C show that overall, the odds of restatement to correct unintentional error for companies with influential CFOs are less than one third the odds of restatement for companies with less influential CFOs (CI: -1.34 – 0.34). This result shows that *H5* also is supported. However, the confidence interval is greater than one for *CFOPOWER* (CI: 1.10 – 3.00) and very near less than one for *CFOPOWER*ORGCHANGE* (CI: 0.18 – 1.08), the variables that were combined to compute the overall CFO influence effect reported at the bottom of Panel C. These divergent results shows that the effect of CFO influence depends on organizational change and that companies undergoing organizational change are driving the results for *H5*. My conclusion that the change in the odds of restatement to correct unintentional error in response to change in CFO influence (financial expertise) depends on organizational change is the reason the relationship between CFO influence (financial expertise) and the likelihood of restatement to correct unintentional error is not observed in univariate analyses and when the multivariate model does not include the interaction of CFO influence (financial expertise) and organizational change ($p > 0.1$ not in tables).

Panel C shows the odds of restatement to correct unintentional error are the same for audit committees with greater and lesser financial expertise (CI: 0.60 - 1.68) and for greater and lesser auditor quality (CI: 0.88 - 1.24). Accordingly, *H10* and *H12* are not supported. Panel C also shows that profitability (*LOSS*) is the only control variable significantly associated with the likelihood of restatement to correct unintentional error ($p<0.1$). Less profitable (*LOSS*) companies are approximately one to five times more likely (CI: 1.72 – 5.76) to restate to correct unintentional errors than more profitable companies.

In summary, increasing CFO financial expertise and influence reduces the likelihood of restatement to correct unintentional error (albeit these relationships depends upon whether or not companies are undergoing organizational change) while increasing audit committee financial expertise and auditor quality do not. Support for *H3* (*H5*), the association between the likelihood of restatements to correct unintentional error and CFO financial expertise (influence), is only observable when the dependence of this relationship on organizational change is modeled: in companies undergoing organizational change, the odds of restatement are smaller for CFOs with greater financial expertise (influence) relative to other CFOs but the reverse is true in companies not undergoing organizational change. The odds of restatement to correct unintentional error are the same for audit committees with more or less financial expertise (*H10*) and for greater and lesser auditor quality (*H12*).

Model 2 results: INTENTvsNORESTATE sample

Column 3 of Panel A (Panel D) reports the logit metric (odds ratio metric) results for Model 2 estimated using the INTENTvsNORESTATE sample. The INTENTvsNORESTATE sample is comprised of the 104 companies that restate to correct intentional misstatement and the

121 companies without restatement. While no hypotheses are tested by the Model 2 regression estimated using the INTENTvsNORESTATE sample, results are presented to show that the consistency assumption described in the research design is not violated. This assumption, (i.e., that any significant associations between CFO financial expertise, CFO influence, audit committee financial expertise, and auditor quality and restatement are in the same direction for restatements that correct unintentional error and restatements that correct intentional misstatement) underlies interpretation of Model 3 results.

Confidence intervals for products of *CFOBIG4* and *CFOBIG4*ORGCHANGE* (*CFOPOWER* and *CFOPWER*ORGCHANGE)* are not reported as the interaction *CFOBIG4*ORGCHANGE* (*CFOPOWER* and *CFOPWER*ORGCHANGE)* is not significant ($p>0.3$) and a likelihood ratio test shows that including these interactions in the model does not improve model fit ($p>0.6$ not in tables). The interactions are retained so that the presentation of models is consistent across Panels A – F. None of the significant variables in Panel D are also significant in Panel C and vice versa. Therefore, the consistency assumption described in the research design section is not violated.

It is noteworthy that the odds of restating to correct intentional misstatements is up to two times (the reciprocal of 0.31 – 1) more likely for audit committees with less financial expertise relative to audit committees with greater financial expertise (CI: 0.31 – 0.99). This is consistent with the univariate comparison in Table 14 Panel D that shows audit committees of companies that restate to correct intentional misstatements have less financial expertise than audit committees of companies without restatements ($p<0.05$).

In contrast to the significant association between audit committee financial expertise and restatements to correct unintentional error, the odds of restatement to correct intentional misstatements are the same for companies with differing levels of CFO financial expertise, CFO influence, and auditor quality. The odds of restatement change with changes in three of the control variables. The odds of restatement for auditors with the greatest tenure may as much as double the odds of restatements for recently engaged auditors (CI: 1.01 – 2.04). Conversely, restatements to correct intentional misstatement are less likely when stock awards and options granted in the year comprise a greater proportion of CFO total compensation (CI: 0.04 – 0.47) and for companies with greater analyst following (CI: 0.23 -0.92).

3.4.3.5 Model 3 test results: INTENT sample

Panel E of Table 17 and the fourth column of Panel A report results of tests for the three hypotheses that compare the relative strength of associations between restatements that correct intentional misstatement and restatements that correct unintentional error. These comparisons of associations test the relative effects of CFO influence (*H8*), audit committee financial expertise (*H11*), and auditor quality (*H13*) on the two types of restatements. Confidence intervals for products of *CFOBIG4* and *CFOBIG4*ORGCHANGE* (*CFOPOWER* and *CFOPWER*ORGCHANGE)* are not reported as the interaction *CFOBIG4*ORGCHANGE* (*CFOPOWER* and *CFOPWER*ORGCHANGE)* is not significant (p>0.3) and a likelihood ratio test shows that including these interactions in the model does not improve model fit (p>0.5). The interactions are retained so that the presentation of models is consistent across Panels A – F. The results of the Hosmer-Lemeshow *Chi*-square test (p=0.92) and area under the ROC curve (0.7719) suggest the overall model fit is adequate. A likelihood ratio test that compares Model 2 estimated with all control variables listed in Table 12 and the two interactions with

organizational change (Panel F) to the nested model reported in Panels A and E is not significant (*Chi2* = 8.27 *p*=0.4078 not in tables).

Panel E shows that the confidence interval for CFO influence (*CFOPOWER)* includes 1.0 (CI: 0.49 – 1.39) which shows that the change in the odds of restatement in response to increasing CFO influence (*CFOPOWER*) does not differ for the two types of restatements. *H8* is not supported. However, while not hypothesized, Panel E also shows that the confidence interval for CFO financial expertise (*CFOBIG4*) does not include 1.0 (CI: 0.20 – 0.80, $p<0.05$) which shows that the change in the odds of restatement in response to increasing CFO financial expertise (*CFOBIG4*) does differ for the two types of restatements. Results reported in Panel C (D) show that the odds of restatement to correct unintentional error (intentional misstatement) are less (not significant) for CFOs with greater financial expertise. However, Model 2 results also show that the effect of CFO financial expertise on restatement to correct unintentional error depends on organizational change and this dependence is not modeled in tests using Model 3. Accordingly, I conclude that the change in odds ratio in response to increasing CFO financial expertise differs between the two types of restatements and that this difference is driven by the relationship of CFO financial expertise to restatements that correct unintentional error but do not attribute any significance to the direction of this difference.

The odds ratio of audit committee financial expertise (*ACBIG4*) reported in Panel E is less than 1.0 (CI: 0.24 – 0.79, $p<0.05$). This finding, coupled with both the significant negative association between audit committee financial expertise and restatement to correct intentional misstatement (Panel D) and the insignificant association between audit committee financial expertise and restatement to correct unintentional error (Panel C), supports *H11* - restatements

that correct intentional misstatement are more likely than restatements that correct unintentional error to be decreasing in audit committee financial expertise.

Finally, Model 3 tests whether or not increasing auditor quality is associated more with one type of restatement than the other. Consistent with increasing auditor quality (*OFFICESIZE*) not changing the likelihood of restatement to correct unintentional error (Panel C) or restatement to correct intentional misstatement (Panel D), an increase in auditor quality does not change the likelihood of one type of restatement more than the other (CI: 0.80 – 1.13, $p>0.6$). Model 3 tests find no evidence to support *H13*. Results of Model 3 tests also show differences in strength of association between likelihood of restatements to correct intentional misstatement vs. unintentional error for several of the control variables. First, (taking into account Model 2 tests that show the consistency assumption is not violated) Model 3 test results show that the positive (negative) association between auditor tenure (CFO equity compensation) and the likelihood of restatement is stronger for restatements correcting intentional misstatement than unintentional error-correcting misstatements. Model 3 tests (taking into account Model 2 tests that show the consistency assumption is not violated) also show that the positive association between losses and the likelihood of restatement is stronger for restatements correcting unintentional misstatement than restatements correcting intentional error which is consistent with less profitable companies investing less in internal control.

In summary, Model 3 results (1) provide no support that any change in restatement likelihood in response to a change in auditor quality differs between the two types of restatements; (2) show that an increase in audit committee financial expertise is more likely to reduce restatements that correct intentional misstatement than unintentional error; and (3) show

that an increase in CFO financial expertise (influence) is (is not) differentially associated with changes in the likelihood of the two different types of restatements.

3.4.3.6 Additional analyses

Interpreting the moderating effect of CFO financial expertise on organizational change

Table 17 Panel C shows the multiplicative effects of CFO financial expertise and organizational change is significant. However, the results reported in Table 17 Panel C do not show whether this significant interaction with organizational change is explained by a decrease in the odds of restatement to correct unintentional error for companies with CFOs with financial expertise (i.e., a shift of attention from strategic to financial reporting during times of change explanation), or rather, an increase in the odds of restatement to correct unintentional error for companies without CFOs with financial expertise (i.e., a competence explanation). To test which of the explanations is more plausible, I first computed the odds of restatement to correct unintentional error for the four combinations of CFO financial expertise and organizational change and then tested whether or not the marginal effect of organizational change (change in odds) is significant for companies with and without CFOs with financial expertise[41]. The odds (not in tables) of error-correcting restatement for companies with CFOs with financial expertise is 1.56 (1.68) for companies not experiencing (experiencing) organizational change and the marginal effect of organizational change is not significant (p>0.8 not in tables). The odds (not in tables) of error-correcting restatement for companies with CFOs without financial expertise is

[41] First, I reran Model 2 with a constant baseline variable equal to 1 added to the regression and the intercept suppressed to show the baseline odds and odds ratios following Newson (2003). Second, following Buis (2010), I used Stata's margin command to display the odds of restatement to correct unintentional error for the four combinations of CFO financial expertise and organizational change. Finally, again followed Buis (2010), I used Stata's lincom command to compute the marginal effect of organizational change - the change from the baseline odds – for CFOs with and without financial expertise.

1.01 (3.41) for companies not experiencing (experiencing) organizational change and the marginal effect of organizational change is marginally significant (p=0.13 not in tables). Based on this additional analysis, I conclude that CFOs with greater financial expertise are better able to modify internal controls in response to business changes and that their greater competence, relative to CFOs with lesser financial expertise, is reflected in a lower likelihood of restatement to correct unintentional errors in times of organizational change.

Mediated logistic analysis

Several factors encountered during the main hypotheses tests necessitated changes to the planned mediated logistic analyses to determine the portion of the total association between restatements and CFO financial expertise, CFO influence, audit committee financial expertise, and auditor quality that is mediated by internal control. First, mediated logistic analyses cannot be conducted to study any indirect effects in Model 2. Only one of the companies without restatement has auditor SOX 404 reported control weaknesses, the proxy for internal control quality. As a result, the reporting of internal control quality for the ERROR sample is too unbalanced and mediated logistic analyses related to Model 2 tests cannot be undertaken. Second, auditor quality is not significantly associated with internal control quality, restatements that correct unintentional error or restatements that correct intentional misstatement: any reported measure of indirect and direct effects of auditor quality would be without meaning. Third, CFO financial expertise and influence associations with restatements depend on organizational change and the program I used to compute the mediated effects cannot deal with such dependencies. Accordingly, mediated logistic analysis is limited to the study of the mediation of audit committee financial expertise in Model 3.

Table 18 Panel A presents Model 2 and 3 results when internal control quality is included in the regression. I*CWeakAMENDED* is significant ($p<0.01$) in all models presented in Panel A. However, CFO financial expertise and influence are still significant in Model 2 and audit committee financial expertise is still significant in Model 3. Hence, the proxy for restatements that correct unintentional error captures variance not captured by the commonly used proxy for internal control quality.

Table 18 Panel B presents the mediated logistic model that computes the proportion of the audit committee financial expertise mediated by internal control quality. As shown at the bottom of Panel B, 14.8% of the effect of audit committee financial expertise in Model 3 is mediated by internal control. So the majority of the effect of increasing audit committee financial expertise on reducing the likelihood of restatement to correct intentional misstatement would not be through the audit committee's oversight of internal control but rather through their direct interactions with management and auditors at audit committee meetings.

Sensitivity of results to CFO changes

Tests were re-estimated excluding the 38 companies with CFO changes. The main findings are the same when these 38 companies are excluded.

Endogeneity

The Hausman tests for endogeneity in Models 1, 2 and 3 using the lagged values of CFO financial expertise, CFO influence, audit committee financial expertise and auditor quality as instrumental variables could not be performed with the sample data. Weesie (1999) suggests using the STATA *suest* command to conduct generalized Hausman tests with seemingly

unrelated estimations. Seemingly unrelated estimation could (could not) be performed using the lagged values of CFO financial expertise, CFO influence, audit committee financial expertise and auditor quality instrumental variables for Model 2 (Models 1 and 3). Post estimation tests for Model 2 show coefficients of CFO financial expertise, CFO influence, audit committee financial expertise, and auditor quality did not differ between the regressions in the seemingly unrelated estimations (p>0.1). Correlations between the test variables and their lagged values used as instrumental variables are greater than 0.4 (p<0.0001) (i.e., for CFO financial expertise 0.87; audit committee financial expertise 0.87; and auditor quality 0.44).

Badolato et al. (2014) argue that whether or not a firm's headquarters is located within one of the largest ten metropolitan statistical areas (MSAs) may be used to instrument for financial expertise. Following their logic, I also argue that CFOs and audit committee members with greater financial expertise and influence, and auditors of higher quality, have more choice of companies with which to associate in MSAs with larger populations. I conduct generalized Hausman tests using the population of MSAs of the audit engagement office to instrument for test variables instead of lagged values in seemingly unrelated estimations[42]. In post-estimation results of seemingly unrelated estimations of all three models using MSA population as an instrumental variable, I find the coefficients of proxies for auditor quality, CFO financial expertise, CFO influence, audit committee financial expertise are the same (p<0.11) in the simultaneously estimated regressions. Results of these seemingly unrelated estimations do not evidence endogeneity.

[42] Additional analyses, not in the tables, shows that MSA population is more likely to be a better instrument for auditor quality than financial expertise in Chapter 3 tests. Audit engagement office size (*OFFICESIZE*), the proxy for auditor quality in Chapter 3 tests, is positively related to MSA population for the ERROR sample (r=0.11 p=0.1), the INTENT sample (r=0.27 p=0.0001), and the INTENTvsNORESTATE sample (r=0.13 p=0.04). In contrast, the correlations between CFO and audit committee financial expertise and MSA population are much weaker (r< 0.06 and p>0.3). MSA population is not significantly correlated (p<0.6) with any of the dependent variables used in Chapter 3 tests (i.e., *ICWeakAMENDED*, *ERROR*, *INTENT*, and *INTENTvsNORESTATE*).

Alternative measure of auditor quality

Given the lack of associations between auditor quality and internal control quality and restatements, I reran all model tests substituting the firm measure (*BIG4*) for the engagement office size measure of auditor quality. Results for *BIG4* were insignificant ($p>0.3$ not in tables) in all models. I also reran Models 2 and 3 for only those companies with Big 4 auditors. Once again, the auditor quality measure (*OFFICESIZE*) is not significant ($p>0.7$ not in tables). I was unable to estimate Model 1 for only companies with Big 4 auditors as there were too few industries and years that did not perfectly predict effective internal control to complete the estimation. I conclude the lack of significance of auditor quality in tests is not due to the choice of O*FFICESIZE* rather than *BIG4* as a measure of auditor quality.

3.4.4 Summary

3.4.4.1 Findings

The pattern of association between those responsible for financial reporting quality and restatements differs for restatements that correct unintentional error and restatements that correct intentional misstatement. CFO expertise and influence, rather than audit committee expertise or auditor quality, are related to restatements that correct unintentional error. The odds of such restatements are more than one half (one third) less for companies with CFOs with greater financial expertise (influence). However, this difference in the odds of restatement is only observable when the model used to test associations with restatements that correct unintentional errors includes the interactions of major organizational change (e.g., restructuring or merger and acquisition activity) with CFO financial expertise and CFO influence. In the absence of major organizational change the likelihood of restatements that correct unintentional error is increasing

in CFO financial expertise and influence. This relationship is consistent with CFOs with greater expertise and influence having greater strategic management responsibilities and concentrating their efforts on those responsibilities rather than financial reporting. However, in the presence of organizational change, the odds of restatement to correct unintentional error increases (does not increase) for companies without CFOs with financial expertise. This finding suggests that CFOs with greater financial expertise and influence are better able than CFOs with lesser financial expertise and influence to change financial reporting systems as required in response to change.

While both CFO financial expertise and influence are associated with restatement to correct unintentional error, it is CFO financial expertise and not CFO influence that is associated with restatements that correct unintentional error more strongly than with restatements that correct intentional misstatement. This is consistent with CFOs with greater power not only being better able to influence the personnel, system and business process changes so as to prevent unintentional financial reporting error, but also being better able to stand up to CEOs and resist intentional misstatement.

In contrast, I find that restatements that correct intentional misstatement are decreasing in audit committee financial expertise. This finding is consistent with Keune and Johnstone's (2012, 1641) finding that "that audit committees with greater financial expertise are less likely to allow managers to waive material misstatements compared to audit committees with less expertise". While Keune and Johnstone (2012) investigate negotiations to resolve already detected misstatements, my study is not so restricted. I find no support for my prediction that greater audit committee financial expertise is associated with a reduction in restatements to

correct unintentional error[43]. I conclude from my findings that while audit committees with greater financial expertise may ask more probing questions and play a bigger role in negotiations between management and external auditors, that greater financial expertise is not necessarily related to securing greater resourcing for financial reporting systems and personnel.

The relationships between restatements that correct unintentional error and CFO financial expertise, CFO influence, and organizational change observed with Model 2 tests are not mimicked by associations between internal control quality and CFO characteristics and organizational change in Model 1 tests. This would be surprising had Rice and Weber (2012, 811) not found that "only a minority of … firms acknowledge their existing control weaknesses during their misstatement periods, and that this proportion has declined over time". Hence, I do not conclude that CFO characteristics are not associated with internal control quality but rather that Model 1 results are affected by internal control quality measurement error. In this study, auditors report ineffective control in SOX 404 reports as originally filed or amended for only 20 companies of the 121 (16.5%) that restate to correct unintentional error.

Finally, in tests of a theoretical model that includes CFO financial expertise and influence and audit committee financial expertise, I find no evidence that auditor quality, measured by both firm and engagement office size, is associated with either restatement to correct unintentional error or restatement to correct intentional misstatement. This finding adds to the mixed results of the small number of recent restatement studies that include auditor quality as either a test or control variable (Cohen et al. 2014, Ettredge et al. 2014, Francis and Michas 2013, Francis et al.

[43] In their study of the interaction of audit committee financial expertise and status, Badolato et al. (2014, 12) test the association of audit committee financial expertise and restatements coded by Audit Analytics as errors as a "Falsification test" to "rule out the potential for false positives". While they do not find an association between restatement to correct error and audit committee financial expertise and, it is not clear if their sample includes quarterly restatements, Badolato et al. (2014) also find no association between audit committee financial expertise and irregularities unless the audit committee also has high status relative to management.

2013, Newton et al. 2013). Most of these studies find restatements decreasing in auditor quality, at least for the more severe restatements. However, consistent with the results of this study, Cohen et al. (2014) (the only one of these studies that includes audit committee expertise in tests) predict, but do not find, a negative association between auditor quality (included as a control variable and proxied by auditor size) and restatement.

3.4.4.2 Limitations and opportunities for future research

This study of determinants of unintentional error in audited financial statements of public companies has several limitations. First, interpreting the moderating effects of CFO financial expertise and CFO influence on the association between organizational change and restatements that correct unintentional error is challenging when using logistic and mediated logistic models. Structural equation modeling (SEM) may lead to additional insights in future research. Secondly, either the measure of auditor quality or the correlation between the various audit control variables may explain the failure to find evidence to support the hypotheses of association between auditor quality and restatements. In future studies, results of PCAOB inspections and audit firms' internal engagement quality reviews (Bell et al. 2013, Epps and Messier 2007), and self-regulated peer reviews (Casterella et al. 2009), where data is available, may better proxy for auditor quality. Using an audit fee model to predict abnormal audit fees may permit a more nuanced exploration of auditor measure associations. Finally, this is a small sample study. The decision to include in the sample only companies with auditor's SOX 404 reports for the first year of their restated period and no prior restatements, limited the sample size. Less the one fifth of the companies that restate to correct unintentional error report ineffective control in auditor's SOX 404 reports (as amended). Given I did not find the predicted associations between the characteristics of those responsible for financial reporting quality and the proxy for internal

control quality (i.e., auditors' SOX 404 reports), there is an opportunity to expand the sample in the future by including non-accelerated filers not required to obtain an auditor's opinion on the effectiveness of internal control over financial reporting. The sample could also be expanded by including quarterly restatements, albeit auditors' responsibilities with respect to annual and quarterly financial statements differ. Similarly, there is an opportunity to expand the sample of restatements that correct intentional misstatements in future years as the number of AAERs increase and by including restatements related to SEC, Department of Justice, or other investigations. Expanded samples would increase the power of tests that compare the associations between restatements that correct intentional misstatements and restatements that correct unintentional error.

3.4.4.3 Contributions

In spite of these limitations, this study's development and testing of a theoretical model of the determinants of unintentional error in audited financial statements of public companies makes several contributions. The model that is developed in this chapter is more complete than models used to date. In particular, it includes the interaction of CFO characteristics and organizational change. When these interactions are not included, the effects of CFO characteristics may not be observed in archival research in tests. This study's investigation of these interactions has shown that the association between unintentional error and CFO financial expertise and influence depends on whether or not companies are undergoing major organizational change. This study has also shown that restatements that correct unintentional error and internal control measures based on auditor's SOX 404 reports are not substitute proxies. Finally, this study has shown that the determinants of unintentional error differ from determinants of intentional misstatement. In particular, whereas an increase in CFO financial

expertise is more likely to be associated with restatements that correct unintentional error, an increase in audit committee financial expertise is more likely to reduce restatements that correct intentional misstatement.

Chapter 4 - Summary and Conclusion

In this dissertation I develop a theoretical model of the determinants of unintentional error in audited financial statements of public companies. The theoretical model developed includes characteristics of organizations and their management, audit committees, and auditors with the goal of shedding more light on the roles played by those responsible for preventing and detecting unintentional error in audited financial statements. This model is more complete in several respects than the models used in most audit research. The first difference is that, the model includes characteristics of all three parties responsible for financial reporting quality, whereas, with few exceptions, prior research includes characteristics of at most two of these parties. The model developed also includes an interaction to model the potential for CFOs and audit committees to mitigate disruptive organizational change effects. While organizational change will disrupt control systems, change will lead to misreporting only if management and the audit committee do not anticipate the impact of change and modify controls and processes appropriately. Finally, I model not only the direct associations between restatement and determinants tested in other restatement research, but also the indirect, mediating effects of CFOs and audit committees on restatements through internal control quality.

To test the theoretical model, I developed and validated a language-based proxy for identifying restatements that correct unintentional error using text search. Chapter 2 discusses several advantages this new method of proxy construction has over other proxies currently used in restatement research. First, this new method of proxy construction is direct. Some other methods indirectly classify as error-correcting, restatements that do not correct intentional misstatement (e.g., Hennes et al. 2008) or that do not exhibit attributes frequently associated with intentional misstatement (e.g., more severe market reaction or revenue recognition correction). In

contrast, this new method classifies restatements as error-correcting directly if the restatement announcement contains any of the words or phrases that are listed in Appendix A which assert or imply a lack of intent. Second, while other proxy construction methods rely on manual coding of restatement announcements (e.g., Plumlee and Yohn 2010), this new proxy is automated and hence, replicable, scalable, and transparent. Using the list of error indicator words and phrases listed in Appendix A, together with *AuditAnalytics* text search tool, automated test searches of restatement announcements may be conducted swiftly, even when the number of restatement announcements is large. Automating identification of restatements that correct unintentional error reduces data collection costs that may have discouraged researchers in the past from undertaking studies of determinants and consequences of unintentional errors. Third, the language-based approach developed in Chapter 2 is not confounded as market reaction proxies are when restatement announcements are made in 10-Ks and other regular SEC filings instead of in 8-Ks and press releases. Finally, with automated text searches I classify less than one third of post-2002 annual restatements as restatements that correct unintentional error, whereas Hennes et al. (2008) classify three quarters of restatements in their sample as errors and Plumlee and Yohn (2010) classify over half the restatements in their sample as internal error. If this smaller proportion reflects a better identification of restatements of unintentional error, the proxy may improve the power of tests in studies of the determinants and consequences of unintentional error.

Chapter 2 also itemizes some limitations of this newly developed language-based proxy for restatements that correct unintentional error. For example, the success of the language-based identification of restatements that correct unintentional error depends on the choice of word(s) listed in Appendix A that are used to identify unintentional error restatements. No doubt further

study of restatements announced after the list was created could identify additional error-identifiers that should be included. Furthermore, more sophisticated search criteria using more sophisticated text analytics software may improve error restatement identification. However, requiring more complex search criteria and tools may also discourage some researchers from adopting this language-based proxy. I have conducted validation tests and find that, for years with auditor's SOX 404 opinions, that relative to intentional misstatement firm-years, language-identified-error-correcting restatements have less persistent income, less positive accruals, and less income smoothness. However, validation tests do not prove that this language-based proxy is a better proxy for restatements that correct unintentional error than other proxies. A useful extension of this research would be to determine whether or not this language-based proxy classifies restatements more accurately than prior proxies.

In Chapter 3 I develop and test the theoretical model. Results of tests show that CFO financial expertise and influence moderate the disruptive effects of organizational change and decrease the likelihood of restatements to correct unintentional error. Restructuring activity accounted for coding organizational change as present for virtually all of the companies identified as undergoing organizational change. An extension of this research would be to investigate whether or not CFOs with greater financial expertise and influence are also better able to mitigate the disruptive effects of change relative to other CFOs for a sample of companies experiencing greater variation in change triggers that include not only restructuring but also merger and acquisition activity and rapid growth. While several restatement studies look at the management turnover consequences of restatements (e.g. Hennes et al. 2008), few studies examine management characteristics as determinants of restatements. This dissertation

contributes to the stream of literature that examines associations between managerial characteristics and restatements directly (e.g. Aier et al. 2005).

I find no evidence that audit committee financial expertise is associated with unintentional error. This implies either that audit committees are unlikely to hear about unintentional errors not discovered by auditors or that greater audit committee financial expertise does not ensure greater resourcing of internal control systems. Badolato et al. (2014) find that both audit committee financial expertise and status (relative to management) are necessary for reduction of irregularities and that reduction of errors is not associated with audit committee financial expertise and status. While confirming Badolato et al.'s (2014) finding with respect to unintentional error, using a different sample I find that audit committee financial expertise is associated with restatements that correct intentional misstatement even when audit committee status is ignored.

Surprisingly, I find no support for my predictions that increasing auditor quality, proxied by office size, reduces the likelihood of unintentional error restatement. I also find no evidence of association between auditor quality and reduction of intentional error misstatement, suggesting similar levels of effectiveness or ineffectiveness in reducing restatements by large and small audit offices. These findings add to the mixed results of a number of recent restatement studies that include auditor quality as either a test or control variable (Cohen et al. 2014, Ettredge et al. 2014, Francis and Michas 2013, Francis et al. 2013, Newton et al. 2013). While recognizing the limitations of the auditor size proxy for auditor quality, this finding suggests further research is necessary to determine whether or not the arguments put forward in Section 3.3.4 (i.e., that smaller auditors may have more experience auditing the smaller, financially weaker companies and that smaller auditors may provide knowledge sharing benefits to

inexperienced staff accountants that are not as readily afforded by larger firms) also explain the lack of auditor quality results.

In addition to differences in associations between test variables and the two types of restatements, I also observe differences in associations with control variables and the two types of restatements. For example, I find that losses (a proxy for resource constraints) are more strongly associated with the likelihood of restatements that correct unintentional error than restatements that correct intentional misstatement. Similarly, with this research design I find that auditor tenure (a proxy for auditor independence) is more strongly associated with the likelihood of restatements that correct intentional misstatement than with restatements that correct unintentional error. Overall, I conclude that the pattern of association between determinants and restatements to correct unintentional error differs from the pattern of association between determinants and intentional misstatement.

In addition to the contributions described in the preceding paragraphs, this dissertation makes three other contributions to the literature. First, the new, language-based proxy construction method provides not only a way of identifying restatements that correct unintentional error but also an automated means of partitioning restatements other than partitioning on market reaction. In contrast to prior research (Palmrose et al. 2004, Hennes et al. 2008), I did not find a market reaction difference between restatements correcting unintentional error and restatements that correct intentional misstatement. While partitioning based on market reaction may achieve separation based on restatement severity, such partitioning may not be as useful for identifying restatements that correct unintentional error.

Secondly, the results of my theoretical model tests suggest that restatements that correct unintentional error may be useful as a proxy for internal control quality. Diminishing data

availability is making it increasingly difficult to study internal control determinants and consequences using SOX 404 opinions: most misstating firms do not report control weaknesses and the proportion reporting weaknesses is declining over time (Rice and Weber 2012). This dissertation provides some evidence, using a sample matched by industry and firm size, that factors such as organizational change, firm losses, and financial expertise that prior research finds associated with internal control weaknesses are also associated with restatements that correct unintentional error.

Finally, in this dissertation I introduce a novel research design that used an unusual coding of the dependent variable in logistic regressions to study the relative associations of determinants between restatements of different types. By coding the dependent variable as 1(0) to indicate the presence of restatements that correct intentional misstatement (unintentional error), I was able to compare the difference in pattern of associations between CFO and audit committee financial expertise between restatement types. Most restatement research reaches conclusions based on comparisons of restatements to control groups without restatement. This is the first study of which I am aware that uses this research design to compare the relative strengths of associations between two types of restatements.

TABLE 1 Chapter 2 Sample Selection [h, i, j]

		Restatement to correct:			
	All Restatements	Unintentional Error (UE) [b]	Intentional Misstatement (IM) [c]	Both Unintentional Error and Intentional Misstatement	Unclassified (UN) [d]
Number of restatements in the *Audit Analytics* database[a] disclosed after 2002, the year of Sarbane-Oxley (SOX) enactment	10,623	1,992	220	56	8,355
less: Restatements without minimum data in COMPUSTAT[e]	(4,388)	(766)	(37)	(6)	(3,579)
less: Quarerly restatements	(1,649)	(347)	(22)	(4)	(1,276)
Annual restatements[f]	4,586	879	161	46	3,500
less: multiple firm-years [g]	(93)	(25)	(14)	(2)	(52)
less: holding companies	(229)	(38)	(7)	(2)	(182)
less: companies with foreign business address	(557)	(104)	(13)	(3)	(437)
Reclassification of restatements that correct both unintentional erorr and intentional misstatement		39		(39)	
Sample restatements, all years 1987 - 2012 [h]	3,707	751	127	0	2,829
less: Restatements pre-2002 (pre-SOX)	(789)	(151)	(56)	0	(582)
less: Restatements without Auditor's SOX 404 reports	(2,070)	(357)	(54)	0	(1,659)
Sample restatements with Auditor's SOX 404 Report [i, j]	848	243	17	0	588

a. Excludes restatements resulting from a change in accounting principle or adoption of a new standard.

b. I identified restatements that correct uninentional error using automated search of restatement annoucements for word(s) suggesting lack of intent (e.g. "inadvertent", "calculation error") in the *AuditAnalytics* database. See Appendix A.

c. I identified intentional misstatement by reading all SEC Accounting and Auditing Releases (AAERs) releases after December 31, 2002 searching for AAERs that reference a company with a restatement (overlapping the restatement time period) that alleges financial statement fraud or intentional misstatement.

d. Restatements that do not correct intentional misstatements or unintentional error as defined in b and c above.

e. Sample inclusion requires both unrestated and restated fourth quarter Total Asset data and four quarters of both unrestated and restated quarterly Net Income data in the COMPUSTAT Unrestated Quarterly ("As First Reported") -US database.

f. Annual restatements have restated periods containing one or more fiscal year-ends (based on the year-end month in AuditAnalytics). Restated periods of quarterly restatements do not include a fiscal year-end. When the restated period extends beyond a single fiscal year, the first fiscal year in the restated period determines the restatement year.

g. When firm-years are restated by more than one restatement, the order of preference in retaining is first to retain restatements that correct intentional misstatements, next to retain restatements that correct unintentional erorr, and lastly to retain unclassified restatements.

h. The "UEvsIM" sample is comprised of 751 restatements that correct unintentional error and 127 restatements that correct intentional misstatement.

i. The "UEvsUN" sample is comprised of restatements with auditor's SOX 404 reports corresponding to the first year of the restated period: i.e., the 243 restatements that correct unintentional error and the 588 unclassified restatements.

j. The "UEvsOther" sample is comprised of restatements with auditor's SOX 404 reports, i.e., the 243 restatements that correct unintentional error, the 588 unclassified restatements, and the 17 restatements that correct intentional misstatements.

TABLE 2 Chapter 2 Variable Definitions [a]

Variable Definition	Abbreviation	Calculation	As defined by:
Restatement Types:			
Restatement to correct intentional misstatement	*IM*	a restatement is coded as correcting an intentional misstatement if the restatement period overlaps the period of an AAER for the company that alleges fraud or senior management intent	
Restatement to correct unintentional error	*UE*	a restatement is coded as correcting unintentional error if the restatement annoucment in AuditAnalytics contains word(s) suggesting lack of intent (e.g. "inadvertent", "calculation error" - see additional examples in Appendix A). Restatements that meet the definition of both correcting unintentional error and intentional misstatement are coded as unintentional error.	
Unclassified Restatement	*UN*	an indicator variable equal to 1 if the Restatement is not coded as a restatements that corrects an intentional misstatement or a restatement that corrects an unintentional error; 0 otherwise.	
Dependent Variables in Mulitvariate Validation Tests *source: Compustat Unrestated Quarterly - US database unless stated otherwise*			
UEindicator	*UEindicator*	an indicator variable used in tests that compare restatements that correct unintentional errors to *either* restatements that correct intentional misstatements *or* unclassified restatements, *or* restatements that correct intentional misstatements plus unclassified restatements, depending upon the test. The variable is equal to 1 if the restatement corrects unintentional error and 0 otherwise. The restatement groups being compared as described in the Model headings (i.e., UE vs. IM or UE vs. UN or UE vs. Other)	
Return on assets	*roa* *roalag*	Total of four quarters of income before extraordinary items (ibqr), scaled by average total assets ((atqr + lagged atqr)/2); *roalag* is *roa* of the previous year	Dechow et al. 2011
Meet or beat analysts forecasts by a small amount	*MeetBeat*	an indicator variable equal to 1 if earnings per share meet or beat analysts average forecast by less than one penny and 0 otherwise. Both the actual EPS and the analyst's forecast nearest (and no more than 2 months before) the EPS announcement date (or median forecast if more than one forecast on the nearest day) are from the I/B/E/S unadjusted daily history.	Reichelt and Wang 2011

TABLE 2 continued

Firm Characteristics listed in Table 3
source: Compustat Unrestated Quarterly - US database unless stated otherwise

Variable Definition	Abbreviation	Calculation	As defined by:
Total Assets	*Total Assets*	atqr - fourth quarter	
Receivables	*Receivables*	rectqr - fourth quarter	
Inventories	*Inventories*	invtqr - fourth quarter	
Capital Assets	*Capital Assets*	ppentqr - fourth quarter	
Market capitalization	*Market Capitaliz*	Common shares o/s (cshoq) X annual fiscal price close (prcc_f): *source Compustat*	Dechow et al. 2011
Sales	*Sales*	saleqr - total of four quarters	
Sales growth	salesgrowth	[Total of four quarters of sales (saleqr) - total of four quarters of sales in the previous year] / total of four quarters of sales in the previous year	
Capital intensity	*capitalintensity*	[Total of four quarters of depreciation and amortization (dpqr) + total of four quarters of interest and related expeses (xintqr)] / total of four quarters of sales (saleqr)	adapted from Lev 1983
Operating Income	*Operating Income*	oibdpqr - total of four quarters	
Operating Cash Flow	*Op. Cash Flow*	oancfqr - 4th quarter	
Income Before Extraordinary Item	*Income Before Extra*	ibqr - total of four quarters	
Net Income	*Net Income*	niqr - total of four quarters	
Return on assets	*roa* *roalag*	Total of four quarters of income before extraordinary items (ibqr), scaled by average total assets ((atqr + lagged atqr)/2); *roalag* is *roa* of the previous year	Dechow et al. 2011
Smoothing Net Income using accruals	*smooth_NI*	median ratio of standard deviation of Net Income (total of four quarters of niqr, scaled by lagged total assets) divided by the standard deviation of cash flow from operations (fourth quarter oancfqr, scaled by lagged total assets). Require complete data in current and immediately preceding four years.	variation on Leuz et al. 2003
Smoothing operating earnings using accruals	*smooth_opinc*	median ratio of standard deviation of operating earnings (total of four quarters of oibdpqr, scaled by lagged total assets) divided by the standard deviation of cash flow from operations (fourth quarter oancfqr, scaled by lagged total assets). Require complete data in current and immediately preceding four years.	Leuz et al. 2003 p. 514

TABLE 2 continued

Accrual Measures Table 8 & 9
source: Compustat Unrestated Quarterly - US database unless stated otherwise

Variable Definition	Abbreviation	Calculation	As defined by:
Accruals	*Accruals*	[Total of four quarters of Income before extraordinary items (ibqr) - operating cash flow (oancfqr) / average assets]	Dechow and Dichev 2002
Working capital accruals	*WC_acc*	[[Δ Current Assets (actqr) – ΔCash and Short-term Investments (cheqr)]– [ΔCurrent Liabilities (lctqr) – ΔDebt in Current Liabilities (dlcqr) – ΔTaxes Payable (txpqr)]/Average total assets	Dechow et al. 2011
Performance- matched discretionary accruals	*pmatch*	The difference between the modified Jones discretionary accruals for firm i in year t and the modified Jones discretionary accruals for the matched firm in year t, following Kothari et al. 2005; each firm-year observation is matched with another firm from the same two- digit SIC code and year with the closest return on assets. Where the modified Jones is cross-sectionally estimated each year using all firm-year observations in the same two-digit SIC code (requiring a minimum of 10 observations): Accruals = α + β(1/Beginning assets) +γ(ΔSales-ΔRec)/Beginning assets + ρPPE/Beginning assets + ε. The residuals are used as the modified Jones model discretionary accruals.	Kothari et al. 2005 modification of Jones as reported by Dechow et al. 2011
Studentized DD residuals	*sresid*	The following regression is estimated for each two-digit SIC industry: ΔWC = b0+ b1*CFOt-1+ b2*CFOt + b3*CFOt+1+ ε, where ΔWC and CFOs are deflated by average total assets. Scales each residual in the regression "by its standard error from the industry-level regression. This measure leaves the sign of the residual intact and provides information on how many standard deviations the residual is above or below the regression line" (p.39). Note: Stata can obtain scaled residuals using the predict with rstudent option after regression.	modification of Dechow and Dichev 2002 by Dechow et al. 2011

TABLE 2 continued

Control Variables in Multivariate Regressions
source: Compustat Unrestated Quarterly - US database unless stated otherwise

Variable Definition	Abbreviation	Calculation	As defined by:
company size	*size*	company size proxied by the natural logarithm of total assets - fourth quarter atqr	
company size indicator	*sizeind*	company size proxied by an indicator variable equalt to 1 for firm-years with total assets greater than the sample median and 0 otherwise	
leverage	*leverage*	fourth quarter long term debt [dlttqr] scaled by total assets [atqr]	
BigN	*BigN*	an indicator variable equal to 1 if the company is audited by a Big 4/5/6/8 audit firm (depending on the year) and 0 otherwise.	
market to book	*mb*	market value of the equity (csho*prcc_f) divided by the book value of equity (ceq): source Compustat	the inverse of Dechow et al. 2011's measure
loss	*loss*	1 if net income (total of four quarters niqr) is <0 in either the current or previous year, and 0 otherwise.	
litigation	*litigation*	an indicator variable equal to 1 if the company operates in a high litigation industry biotechnology (SIC 2833–2836 and 8731 - 8734); computers (SIC 3570–3577 and 7370–7370), electronics (SIC 3600–3674), and retail (SIC 5200–5961), and 0 otherwise	following Johnstone et al. (2012) and Reichelt and Wang (2010)
Number of analysts	*Numestimates*	the logarithm of the number of analysts following the company (numest) for the restatement year: source I/B/E/S unadjusted daily history.	
standard deviation of analysts forecasts	σ(*forecasts)*	the standard deviation of analysts earnings forecasts for the restatement year (stdev): source I/B/E/S unadjusted daily history.	

a The format of this table and many of the abbreviations and variable definitions are derived from Dechow et al. 2011. All data is from the Compustat Unrestated Quarterly - US database except "prcc_f", the end of year closing stock price, from COMPUSTAT. Variables in brackets are labels in the Unrestated database. Four complete fiscal quarters of data are combined for income statement variables. The fourth quarter data is used for balance sheet and cash flow statement variables.

TABLE 3 Chapter 2 Descriptive Statistics

Panel A: Descriptive Statistics for the UEvsIM sample: firm-years with Unintentional Error Restatements vs. Intentional Misstatement Restatements 1987-2012

	Unintentional Error (UE) Restatement firm-years					Intentional Misstatement (IM) Restatement firm-years					*t*-test		Wilcoxon ranksum test		Kolmogorov-Smirnov test	
	N	mean	median	sd	skew-ness	N	mean	median	sd	skew-ness	mean diff	*t*	med. diff	*z*	D	
Total Assets	751	2664.49	300.27	13857.93	16.81	127	16685.49	525.17	84213.45	7.89	(14,021.00)	-3.38 **	(224.89)	-3.39 ***	0.19	***
Receivables	749	744.28	32.90	5254.31	13.72	126	8243.73	74.42	66107.91	9.88	(7,499.44)	-2.42 *	(41.51)	-3.60 ***	0.17	**
Inventories	742	136.80	6.24	618.21	12.50	127	300.87	12.36	1064.38	7.16	(164.07)	-1.94	(6.12)	-2.07 *	0.13	*
Capital Assets	737	567.47	32.61	2063.59	6.74	127	2053.84	57.24	6718.21	4.46	(1,486.37)	-3.96 ***	(24.62)	-1.86	0.11	
Market Capitaliz.	664	1875.36	202.38	11909.14	14.27	118	8570.19	683.52	39690.86	8.42	(6,694.83)	-2.82 **	(481.15)	-5.63 ***	0.24	***
Sales	751	1264.76	207.20	4015.25	7.49	127	4568.76	490.13	13911.19	6.07	(3,303.99)	-4.38 ***	(282.93)	-3.95 ***	0.19	***
Sales Growth	649	4.52	0.09	82.28	24.49	114	0.69	0.19	2.38	7.25	3.83	0.40	(0.10)	-2.75 **	0.17	**
Capital Intensity	497	1.87	0.08	28.16	21.11	76	0.18	0.08	0.36	5.39	1.70	0.04	(0.00)	-0.71	0.10	
Operating Income	696	226.20	19.60	870.03	8.24	117	1236.40	87.16	4853.94	6.09	(1,010.20)	-4.17 ***	(67.57)	-4.24 ***	0.24	***
Op. Cash Flow	688	164.42	8.49	698.80	9.52	119	721.45	22.60	3384.35	7.60	(557.04)	-3.14 **	(14.12)	-1.48	0.15	*
Income Before Extra	751	33.00	1.22	397.58	5.46	127	211.50	6.82	1527.04	6.39	(178.49)	-2.37 *	(5.60)	-2.60 **	0.18	**
Net Income	751	39.83	1.12	455.27	5.26	127	184.80	5.96	1508.42	6.04	(144.97)	-1.90	(4.84)	-2.23 *	0.17	**
Return on Assets	677	-0.38	0.01	3.16	-15.41	115	-0.15	0.02	0.63	-4.93	(0.22)	-0.74	(0.01)	-0.80	0.09	
smooth_NI	445	1.85	1.18	2.08	2.98	66	1.42	1.17	1.24	2.13	0.43	1.39	0.00	1.13	0.16	
smooth_opinc	370	1.25	0.98	1.14	3.51	54	1.46	1.15	1.26	2.08	(0.21)	-1.04	(0.17)	-1.14	0.14	

***, **, * significant at the two-tailed p <0.001, p <0.01 and p <0.05 levels respectively

t-statistics computed with robust standard errors clustered by company

Compustat Unrestated Quarterly - US database variables used in this study are as follows:

- Variables that use the fourth quarter unrestated data: Total Assets (atqr), Receivables (rectqr), Inventories (invtqr), Capital Assets (ppentqr), and Operating Cash Flow (oancfqr)
- Variables that use the total of four quarters of unrestated data: Sales (saleqr), Operating Income (oibdpqr), Income Before Extraordinary Item (ibqr) and Net income (niqr)
- Other variables are computed using more than one Compustat Unrestated Quarterly - US database variable as defined in Table 2
- The smoothness measures of net income (smooth_NI) and smoothness of operating income (smooth_opinc) are winsorized at 1% and 99% to mitigate against outliers.
- when continuous measures with significant mean differences (p <0.05) reported in this table are winsorized at 1% and 99% to mitigate outliers, mean differences remain significant (p <0.1) with the exception of Panel A's Income Before Extraordinary Items (p<0.199).

TABLE 3 continued

Panel B: Descriptive Statistics for UEvsUN sample: firm-years with SOX 404 Auditor Reports for Unintentional Error vs. Unclassified Restatements 2004-2012

	Unintentional Error (UE) Restatement firm-years with Auditor's SOX 404 reports					Unclassified (UN) Restatement firm-years with Auditor's SOX 404 reports					*t*-test		Wilcoxon ranksum test		Kolmogorov-Smirnov test
	N	mean	median	sd	skew-ness	N	mean	median	sd	skew-ness	mean diff	*t*	med. diff	*z*	D
Total Assets	243	3342.29	777.44	12150.77	11.47	588	5145.98	767.33	31851.75	17.52	(1,803.69)	-0.76	10.11	0.25	0.04
Receivables	243	1061.01	80.67	6969.23	12.98	584	1590.19	76.49	14090.06	16.61	(529.18)	-0.50	4.18	0.57	0.06
Inventories	238	206.39	26.09	840.74	11.43	584	415.32	19.12	4390.92	22.87	(208.93)	-0.64	6.96	1.64	0.10 *
Capital Assets	236	739.64	101.58	2145.47	5.35	575	966.17	78.83	4254.12	14.06	(226.53)	-0.68	22.74	1.67	0.10 *
Market Capitaliz.	234	1454.59	471.17	3397.28	6.20	576	2086.92	481.01	6848.16	8.49	(632.34)	-1.18	(9.84)	0.52	0.08
Sales	243	1439.59	497.63	2811.95	4.09	588	2451.44	420.93	11395.66	15.28	(1,011.85)	-1.19	76.70	1.11	0.09
Sales Growth	239	0.14	0.07	0.40	2.91	575	0.23	0.07	1.53	17.49	(0.09)	-0.79	(0.00)	-0.07	0.05
Capital Intensity	170	0.20	0.08	0.80	12.27	406	0.36	0.07	3.12	17.89	(0.16)	-0.57	0.00	1.04	0.07
Operating Income	228	259.00	67.39	838.91	7.61	558	348.94	61.44	1788.34	13.56	(89.94)	-0.64	5.95	0.36	0.05
Op. Cash Flow	220	202.87	45.75	865.09	10.99	526	154.32	42.00	1484.40	-5.57	48.55	0.40	3.75	0.76	0.08
Income Before Extra	243	11.76	10.10	251.66	-2.07	588	83.96	13.72	666.96	12.31	(72.20)	-1.44	(3.63)	-1.47	0.07
Net Income	243	10.42	10.10	246.96	-2.39	588	84.80	13.75	671.79	12.09	(74.38)	-1.47	(3.65)	-1.50	0.08
Return on Assets	240	-0.02	0.01	0.18	-0.84	584	-0.04	0.02	0.26	-4.30	0.01	0.62	(0.01)	-1.98 *	0.11 *
smooth_NI	189	1.80	1.25	1.81	2.79	450	1.70	1.09	2.03	3.12	0.09	0.50	0.16	1.76	0.14 **
smooth_opinc	163	1.24	1.00	1.09	3.46	387	1.18	0.98	0.97	3.11	0.05	0.53	0.02	0.34	0.07

***, **, * significant at the two-tailed $p < 0.001$, $p < 0.01$ and $p < 0.05$ levels respectively

t-statistics computed with robust standard errors clustered by company

Compustat Unrestated Quarterly - US database variables used in this study are as follows:

- Variables that use the fourth quarter unrestated data: Total Assets (atqr), Receivables (rectqr), Inventories (invtqr), Capital Assets (ppentqr), and Operating Cash Flow (oancfqr)
- Variables that use the total of four quarters of unrestated data: Sales (saleqr), Operating Income (oibdpqr), Income Before Extraordinary Item (ibqr) and Net income (niqr)
- Other variables are computed using more than one Compustat Unrestated Quarterly - US database variable as defined in Table 2
- The smoothness measures of net income (smooth_NI) and smoothness of operating income (smooth_opinc) are winsorized at 1% and 99% to mitigate against outliers.
- when continuous measures with significant mean differences ($p < 0.05$) reported in this table are winsorized at 1% and 99% to mitigate outliers, mean differences remain significant ($p < 0.1$) with the exception of Panel A's Income Before Extraordinary Items ($p < 0.199$).

TABLE 3 continued

Panel C: Descriptive Statistics for UEvsOther sample: firm-years with SOX 404 Auditor Reports for Unintentional Error vs. Unclassified and Intentional Misstatements 2004-2012

	Unintentional Error (UE) Restatement firm-years with auditor's SOX 404 reports					Unclassified (UN) and Intentional Misstatement (IM) Restatement firm-years with Auditor's SOX 404 reports					*t*-test		Wilcoxon ranksum test		Kolmogorov-Smirnov test	
	N	mean	median	sd	skew-ness	N	mean	median	sd	skew-ness	mean diff	*t*	med. diff	*z*	D	
Total Assets	243	3342.29	777.44	12150.77	11.47	605	5381.87	754.03	31824.28	17.14	(2,039.57)	-0.86	23.42	0.26	0.05	
Receivables	243	1061.01	80.67	6969.23	12.98	601	1579.61	76.69	13895.86	16.83	(518.60)	-0.50	3.99	0.52	0.06	
Inventories	238	206.39	26.09	840.74	11.43	601	411.01	19.99	4329.47	23.19	(204.62)	-0.63	6.10	1.58	0.10	
Capital Assets	236	739.64	101.58	2145.47	5.35	592	980.03	78.05	4254.68	13.74	(240.39)	-0.72	23.53	1.81	0.11	*
Market Capitaliz.	234	1454.59	471.17	3397.28	6.20	593	2218.56	482.07	7649.26	8.37	(763.97)	-1.28	(10.90)	0.39	0.08	
Sales	243	1439.59	497.63	2811.95	4.09	605	2514.30	425.57	11403.20	14.89	(1,074.72)	-1.26	72.06	1.06	0.08	
Sales Growth	239	0.14	0.07	0.40	2.91	591	0.23	0.07	1.52	17.59	(0.09)	-0.81	(0.00)	-0.11	0.05	
Capital Intensity	170	0.20	0.08	0.80	12.27	418	0.36	0.07	3.07	18.11	(0.16)	-0.57	0.01	1.18	0.07	
Operating Income	228	259.00	67.39	838.91	7.61	575	374.23	62.72	1881.24	12.28	(115.22)	-0.78	4.67	0.29	0.05	
Op. Cash Flow	220	202.87	45.75	865.09	10.99	542	167.14	41.66	1490.13	-5.21	35.73	0.29	4.09	0.77	0.08	
Income Before Extra	243	11.76	10.10	251.66	-2.07	605	78.27	13.74	702.10	9.86	(66.51)	-1.25	(3.64)	-1.48	0.07	
Net Income	243	10.42	10.10	246.96	-2.39	605	79.34	13.80	707.73	9.68	(68.92)	-1.29	(3.70)	-1.50	0.08	
Return on Assets	240	-0.02	0.01	0.18	-0.84	601	-0.04	0.02	0.26	-4.30	0.01	0.60	(0.01)	-2.06	0.11	*
smooth_NI	189	1.80	1.25	1.81	2.79	463	1.70	1.10	2.01	3.14	0.10	0.53	0.15	1.71	0.14	*
smooth_opinc	163	1.24	1.00	1.09	3.46	400	1.19	0.98	0.97	3.05	0.05	0.46	0.02	0.21	0.07	

***, **, * significant at the two-tailed p <0.001, p <0.01 and p <0.05 levels respectively

t -statistics computed with robust standard errors clustered by company

Compustat Unrestated Quarterly - US database variables used in this study are as follows:

- Variables that use the fourth quarter unrestated data: Total Assets (atqr), Receivables (rectqr), Inventories (invtqr), Capital Assets (ppentqr), and Operating Cash Flow (oancfqr)
- Variables that use the total of four quarters of unrestated data: Sales (saleqr), Operating Income (oibdpqr), Income Before Extraordinary Item (ibqr) and Net income (niqr)
- Other variables are computed using more than one Compustat Unrestated Quarterly - US database variable as defined in Table 2
- The smoothness measures of net income (smooth_NI) and smoothness of operating income (smooth_opinc) are winsorized at 1% and 99% to mitigate against outliers.
- when continuous measures with significant mean differences (p <0.05) reported in this table are winsorized at 1% and 99% to mitigate outliers, mean differences remain significant (p <0.1) with the exception of Panel A's Income Before Extraordinary Items (p<0.199).

TABLE 4 Restatement Frequency by Firm Size, Year, and Industry

Panel A: Restatement Frequency by firm size (market capitalization deciles following Dechow et al. 2011)

	1987 - 2012						2004 - 2012 with SOX 404 auditor's reports						2004 - 2012 with SOX 404 auditor's reports			
	Unintentional Error (UE) [a]		Intentional Misstatement (IM) [a]		binomial proportion test UE vs IM		Unintentional Error (UE) [a]		Unclassified (UN) [a]		proportion test UE vs UN		Other (UN + IM) [a]		binomial proportion test UE vs Other	
Decile rank [a]	n	Percent	n	Percent	z	sig	n	Percent	n	Percent	z	sig	n	Percent	z	sig
10	38	5.7%	28	23.7%	-6.48	***	10	4.3%	43	7.5%	-1.67		46	7.8%	-1.80	
9	57	8.6%	17	14.4%	-1.99	*	26	11.1%	61	10.6%	0.22		62	10.5%	0.28	
8	91	13.7%	19	16.1%	-0.69		47	20.1%	90	15.6%	1.53		93	15.7%	1.52	
7	86	13.0%	14	11.9%	0.33		45	19.2%	103	17.9%	0.45		106	17.9%	0.45	
6	78	11.7%	10	8.5%	1.04		46	19.7%	107	18.6%	0.36		111	18.7%	0.31	
5	71	10.7%	10	8.5%	0.73		30	12.8%	90	15.6%	-1.02		93	15.7%	-1.04	
4	76	11.4%	10	8.5%	0.95		22	9.4%	52	9.0%	0.17		52	8.8%	0.29	
3	63	9.5%	6	5.1%	1.55		8	3.4%	18	3.1%	*d*		18	3.0%	*d*	
2	52	7.8%	2	1.7%	*d*		0	0.0%	10	1.8%	*d*		10	1.7%	*d*	
1	52	7.8%	2	1.7%	*d*		0	0.0%	2	0.3%	*d*		2	0.3%	*d*	
Total	664	100.0%	118	100.0%			234	100.0%	576	100.0%			593	100.0%		

***, **, * indicates a statistical difference at respectively $p<0.001$, $p<0.01$ and $p<0.05$

a. Firm size is based on the decile rank of market value. The closing price and number of common shares outstanding used to compute market value are available from COMPUSTAT for 664 of the 751 Unintentional Error restatements and for 118 of the 127 Intentional Misstatement restatements in the period 1989-2012, and for 17 of the 17 Intentional Misstatement restatements, 234 of the 243 Unintetional Error restatements, and for 576 of the 588 Unclassified restatements with SOX 404 auditor's opinions.

d. Insufficent counts for meaningful statistical test

TABLE 4 continued

Panel B: Restatement Frequency by Restatement Year

	1987 - 2012						2004 - 2012 with SOX 404 auditor's reports						2004 - 2012 with SOX 404 auditor's reports			
	Unintentional Error (UE)		Intentional Misstatement (IM)		binomial proportion test		Unintentional Error (UE)		Unclassified (UN)		binomial proportion test		Other (IM + UN)		binomial proportion test UE vs Other	
Year [b]	n	Percent	n	Percent	z	sig	n	Percent	n	Percent	z	sig	n	Percent	z	sig
1987	1	0.1%	0	0.0%	*d*											
1992	0	0.0%	1	0.8%	*d*											
1993	1	0.1%	0	0.0%	*d*											
1994	1	0.1%	0	0.0%	*d*											
1995	4	0.5%	0	0.0%	*d*											
1996	2	0.3%	1	0.8%	*d*											
1997	3	0.4%	3	2.4%	*d*											
1998	11	1.5%	5	3.9%	-1.93											
1999	14	1.9%	14	11.0%	-5.43	***										
2000	46	6.1%	15	11.8%	-2.33	*										
2001	68	9.1%	17	13.4%	-1.53											
2002	59	7.9%	23	18.1%	-3.67	***										
2003	71	9.5%	15	11.8%	-0.96											
2004	100	13.3%	13	10.2%	1.57		31	12.8%	79	13.4%	-0.26		85	14.0%	-0.50	
2005	51	6.8%	4	3.1%	*d*		25	10.3%	101	17.2%	-2.52	*	103	17.0%	-2.48	*
2006	66	8.8%	9	7.1%	1.86		39	16.0%	87	14.8%	0.46		91	15.0%	0.37	
2007	74	9.9%	6	4.7%	3.30	**	37	15.2%	60	10.2%	2.05	*	64	10.6%	1.89	
2008	60	8.0%	0	0.0%	*d*		40	16.5%	81	13.8%	1.00		81	13.4%	1.16	
2009	55	7.3%	1	0.8%	*d*		31	12.8%	70	11.9%	0.34		71	11.7%	0.41	
2010	48	6.4%	0	0.0%	*d*		28	11.5%	67	11.4%	0.05		67	11.1%	0.19	
2011	16	2.1%	0	0.0%	*d*		12	4.9%	38	6.5%	-0.84		38	6.3%	-0.75	
2012	0	0.0%	0	0.0%	*d*		0	0.0%	5	0.8%	*d*		5	0.8%	*d*	
Total	751	100.0%	127	100.0%			243	100%	588	100.0%			605	100.0%		

***, **, * indicates a statistical difference at respectively $p<0.001$, $p<0.01$ and $p<0.05$

b. The year is the earliest fiscal year in the restated period when the restated period spans more than one year.

d. Insufficent counts for meaningful statistical test

TABLE 4 continued

Panel C: Restatement Frequency by Industry

	1987 - 2012						2004 - 2012 with SOX 404 auditor's reports						2004 - 2012 with SOX 404 auditor's reports			
	Unintentional Error (UE)		Intentional Misstatement (IM)		binomial proportion test		Unintentional Error (UE)		Unclassified (UN)		binomial proportion test		Other (IM + UN)		binomial proportion test UE vs Other	
Industry [c]	n	Percent	n	Percent	z	sig	n	Percent	n	Percent	z	sig	n	Percent	z	sig
Agriculture	4	0.5%	0	0.0%	*d*		2	0.8%	4	0.7%	*d*		4	0.7%	*d*	
Mining & Construction	33	4.4%	3	2.4%	*d*		15	6.2%	12	2.0%	3.06	**	13	2.1%	2.97	**
Food & Tobacco	17	2.3%	1	0.8%	*d*		3	1.2%	10	1.7%	*d*		10	1.7%	*d*	
Textile & Apparel	9	1.2%	0	0.0%	*d*		5	2.1%	5	0.9%	*d*		5	0.8%	*d*	
Lumber, Furniture, & Printing	17	2.3%	0	0.0%	*d*		5	2.1%	20	3.4%	*d*		20	3.3%	*d*	
Chemicals	21	2.8%	1	0.8%	*d*		8	3.3%	8	1.4%	*d*		8	1.3%	*d*	
Refining & Extractive	31	4.1%	1	0.8%	*d*		7	2.9%	33	5.6%	*d*		33	5.5%	*d*	
Durable Manufacturers	125	16.6%	15	11.8%	1.38		43	17.7%	76	12.9%	1.79		81	13.4%	1.61	
Computers	107	14.2%	38	29.9%	-4.40	***	33	13.6%	77	13.1%	0.19		78	12.9%	0.27	
Transportation	49	6.5%	5	3.9%	1.12		18	7.4%	44	7.5%	-0.04		45	7.4%	-0.02	
Utilities	22	2.9%	6	4.7%	-1.06		7	2.9%	27	4.6%	*d*		27	4.5%	*d*	
Retail	68	9.1%	18	14.2%	-1.79		19	7.8%	51	8.7%	-0.40		53	8.8%	-0.44	
Services	82	10.9%	19	15.0%	-1.32		24	9.9%	53	9.0%	0.39		55	9.1%	0.36	
Banks & Insurance	104	13.8%	12	9.4%	1.35		36	14.8%	116	19.7%	-1.67		119	19.7%	-1.65	
Pharmaceuticals	62	8.3%	8	6.3%	0.75		18	0.073	52	8.8%	-0.68		54	8.9%	-0.72	
Total	751	100.0%	127	100.0%			243	100.0%	588	100.0%			605	100.0%		

***, **, * indicates a statistical difference at respectively $p<0.001$, $p<0.01$ and $p<0.05$

c. Industries are based on SIC codes Frankel, Johnson, and Nelson (2002) as described by Dechow et al. (2011):
Agriculture: 0100–0999; Mining & Construction: 1000–1299, 1400–1999; Food & Tobacco: 2000–2141; Textiles and Apparel: 2200–2399;
Lumber, Furniture, & Printing: 2400–2796; Chemicals: 2800–2824, 2840–2899; Refining & Extractive: 1300–1399, 2900–2999;
Durable Manufacturers: 3000–3569, 3580–3669, 3680–3828, 3852-3999; Computers: 3570–3579, 3670–3679, 7370–7379; Transportation: 4000–4899;
Utilities: 4900–4999;Retail: 5000–5999; Services: 7000–7369, 7380–9999; Banks & Insurance: 6000–6999; Pharmaceuticals: 2830–2836, 3829–3851.

d. Insufficent counts for meaningful statistical test

TABLE 5 Accounts Corrected by Restatement

	1987 - 2012			2004 - 2012 with SOX 404 auditor's reports			2004 - 2012 with SOX 404 auditor's reports	
Accounts corrected [a]	Unintentional Error (UE)	Intentional Misstatement (IM)	binomial proportion test UE vs IM	Unintentional Error (UE)	Unclassified (UN)	binomial proportion test UE vs UN	Other (UN + IM)	binomial proportion test UE vs Other
	n=751	*n=127*	sig.	*n=243*	*n=588*	sig.	*n=605*	sig.
Intentional Misstatements proportionately greatest								
Revenue recognition	18.4%	44.1%	***	12.7%	11.4%		12.4%	
Derivatives and hedging	2.4%	7.9%	**	1.6%	3.7%		3.8%	
Acquisitons, mergers, disposals and reorganization	9.6%	16.5%	*	3.3%	9.7%	**	9.6%	**
Capital Assets	19.0%	26.8%	*	10.7%	10.5%		10.2%	
Consolidation	6.3%	10.2%		4.9%	6.8%		6.8%	
Inventory, vendor and cost of sales	10.5%	14.2%		8.2%	5.3%		5.1%	
Foreign, related party, subsidiary and intercompany	12.5%	15.7%		10.3%	7.7%		7.8%	
Accounts/loans receivable, investments and cash	9.1%	11.8%		6.6%	10.4%		10.2%	
Liabilities and contingency	22.6%	23.6%		12.3%	11.7%		11.9%	
Payroll, selling, general, administrative and other expenses	13.8%	14.2%		9.9%	8.7%		8.6%	
Other	6.5%	11.0%						
Unintentional Error proportionately greatest								
Tax	20.2%	11.0%	*	18.5%	18.9%		18.3%	
Misclassification	14.8%	7.1%	*	19.3%	25.9%	*	25.5%	
Debt and equity	13.7%	7.9%		5.3%	9.2%		9.1%	
Stock-based and executive compensation	13.3%	11.0%		7.8%	6.6%		6.6%	
Pension	2.7%	0.0%	^	3.7%	1.5%	^	1.5%	^

***, **, * , ^ indicates statistical difference at respectively $p<0.001$, $p<0.01$, $p<0.05$, and counts insufficent for statistical comparison.

a. Based on grouping *AuditAnalytics'* coding of Accounting Rule (GAAP/FASB) Application Failures and Financial Fraud, Irregularities and Misrepresentation Issues.

TABLE 6 Income Correction and Restatement

Panel A: Percentage of Restatements that Correct Income Overstatement and Understatement

	1987 - 2012				2004 - 2012 with SOX 404 auditor's reports				2004 - 2012 with SOX 404 auditor's reports		
	Unintentional Error (UE)	Intentional Misstatement (IM)	binomial proportion test UE vs IM		Unintentional Error (UE)	Unclassified (UN)	binomial proportion test UE vs UN		Other (UN + IM)	binomial proportion test UE vs Other	
Net Income correction :	n=751	n=127	*z*	sig	n=243	n=588	*z*	sig	n=605	*z*	sig
Corrects income overstatement	22.1%	27.5%	-1.353		26.3%	22.3%	1.256		22.5%	1.197	
Income unchanged on restatement	67.7%	57.5%	2.266	*	63.4%	67.7%	-1.198		67.1%	-1.038	
Corrects income understatement	10.2%	15.0%	-1.574		10.3%	10.0%	0.111		10.4%	-0.054	
	100.0%	100.0%			100.0%	100.0%			100.0%		
Income before extraordinary items correction	n=751	n=127			n=243	n=588			n=605		
Corrects income overstatement	24.3%	29.1%	-1.146		29.2%	26.9%	0.689		26.9%	0.670	
Income unchanged on restatement	60.0%	52.0%	1.625		52.7%	59.0%	-1.680		58.7%	-1.596	
Corrects income understatement	15.7%	18.9%	-0.902		18.1%	14.1%	1.455		14.4%	1.358	
	100.0%	100.0%			100.0%	100.0%			100.0%		
Operating Income before Depreciation correction :	n=675	n=114			n=220	n=544			n=558		
Corrects income overstatement	27.8%	29.8%	-0.452		33.6%	31.8%	0.491		32.3%	0.369	
Income unchanged on restatement	50.8%	45.6%	1.027		40.0%	44.7%	-1.179		44.6%	-1.172	
Corrects income understatement	21.4%	24.6%	-0.751		26.4%	23.5%	0.827		23.1%	0.954	
	100.0%	100.0%			100.0%	100.0%			100.0%		

* indicates statistical two-tailed significance at $p<0.05$

TABLE 6 Continued

Panel B: Absolute Value of Income Correction Scaled by Restated Total Assets [a]

	1987 - 2012				2004 - 2012 with SOX 404 auditor's reports				2004 - 2012 with SOX 404 auditor's reports		
	Unintentional Error (UE)	Intentional Misstatement (IM)	binomial proportion test UE vs IM		Unintentional Error (UE)	Unclassified (UN)	binomial proportion test UE vs UN		Other (UN + IM)	binomial proportion test UE vs Other	
Absolute Value of Net Income correction:	n=243	n=54	*t*	sig	n=89	n=190	*t*	sig	n=199	*t*	sig
Mean	0.133	0.077	0.28		0.008	0.020	-2.90	**	0.020	-2.92	**
Standard Deviation	0.435	0.323			0.015	0.062			0.060		
Absolute Value of Income before extraordinary items correction:	n=301	n=61			n=115	n=241			n=250		
Mean	0.115	0.070	0.98		0.010	0.019	-2.19	*	0.019	-2.17	*
Standard Deviation	0.414	0.309			0.024	0.055			0.054		
Absolute Value of Operating Income before Depreciation correction:	n=331	n=62			n=132	n=299			n=309		
Mean	0.050	0.073	-0.78		0.013	0.015	-0.54		0.015	-0.59	
Standard Deviation	0.177	0.217			0.030	0.033			0.030		

**, * significant at the two-tailed $p<0.01$ and $p<0.05$ levels respectively

t-statistics computed with robust standard errors adjusted by clustering on company

a. The absolute values of the net income, income before extraordinary items and operating income (respectively, b, c, and d above) restatement amounts are scaled by restated total assets (*atq*) and winsorized at 1% and 99% of observations to mitigate outliers.

TABLE 7 Earnings Persistence and Restatements

$$roa = \beta_0 + \beta_1\, roalag + \beta_2\, UEindicator + \beta_3\, roalag*UEindicator + \beta_4\, size + \beta_5\, capitalintensity + \beta_6\, salesgrowth + \varepsilon$$

UEindicator: UE - Unintentional Error (=1) IM - Intentional Misstatements (=0) UN - Unclassified Misstatement (=0) ***Restatement Sample***		Model 1 UE vs. IM 1987-2012			Model 2 UE vs. IM with auditor's SOX opinions			Model 3 UE vs. UN with auditor's SOX opinions			Model 4 UE vs. Other with auditor's SOX opinions		
	Predicted Sign	Coeff	*t*-statistic/ *F*		Coeff	*t*-statistic/ *F*		Coeff	*t*-statistic/ *F*		Coeff	*t*-statistic/ *F*	
roalag	+	0.830	30.87	***	0.882	3.98	***	0.874	6.18	***	0.873	6.22	***
UEindicator	?	0.014	0.50		0.014	0.48		0.005	0.58		0.005	0.65	
*roalag*UEindicator*	-	-0.164	-0.92		-0.721	-3.21	***	-0.712	-5.36	***	-0.710	-5.35	***
size	+	0.037	2.66	***	0.161	1.71	**	0.010	2.96	***	0.010	2.79	***
capitalintensity	-	-0.021	-4.17	***	-0.020	-0.50		0.000	-0.15		0.000	-0.17	
salesgrowth	-	0.010	2.42	***	0.099	3.89	***	0.028	2.08	**	0.033	3.08	***
Intercept		-0.278	-1.92	**	-0.071	-2.39	**	-0.066	-1.20		-0.065	-1.15	
Industry indicators		yes			yes			yes			yes		
Year indicators		yes			yes			yes			yes		
*F-test of UEindicator + roalag * UEindicator*	-	-0.151	0.67		-0.706	12.41	***	-0.707	28.75	***	-0.705	28.54	***
N		460			177			561			573		
No. of clusters (company)		410			165			484			493		
No. of clusters (year)		17			8			9			9		
F		7.63			15.56			10.46			10.81		
Prob>F		<0.0001			<0.0001			<0.0001			<0.0001		
Adjusted R-squared		0.575			0.225			0.573			0.570		

***, **, * indicate one-tailed signifance at p <0.01, p <0.05, and p <0.1 levels respectively.

t-statistics are based on robust standard errors clustered two-ways by company and year.

Variance inflation factors are less than 9 for all main variables.

OLS regression of return on asset - earnings before extraordinary items scaled by average assets (*roa*) - as a function of: lagged return on assets (*roalag*); an indicator of unintentional error (*UEindicator*) equal to 1 if unintentional error and 0 if intentional misstatement; the interaction of lagged return on assets and the unintentional error indicator (*roalag*UEindicator*); company size (*size*) proxied by logged total assets; capital intensity (*capitalintensity*) proxied following Lev (1983) as the ratio of depreciation, amortization and interest to sales (cost of sales not used as missing for many observations); sales growth (*salesgrowth*) is measured by percentage change in sales; and industry and year dummies. Return on assets (*roa*) and lagged return on assets (*roalag*) are winsorzied at 1% and 99% of observations (with and without restatement) to mitigate against outliers.

TABLE 8 Accrual Measures and Restatements

Panel A: Associations between Accrual Measures[a] and an Unintentional Error vs. Intentional Misstatement Indicator

$$UEindicator = \beta_0 + \beta_1\ AccrualMeasure + \beta_2\ size + \beta_3\ roa + \beta_4\ leverage + \beta_5\ BigN + \varepsilon$$

UEindicator variable comparisons:		Model 1		Model 2		Model 3		Model 4	
UE - Unintentional Errors (=1)		UE vs. IM		UE vs. IM		UE vs. IM		UE vs. IM	
IM - Intentional Misstatements (=0)		Indicator		Indicator		Indicator		Indicator	
UN - Unclassified Misstatements (=0)									
Restatement Sample		1987 -2012		1987 -2012		1987-2012		1987-2012	
	Predicted Sign	Coeff	*z sig*	Coeff	*z sig*	Coeff	*z sig*	Coeff	*z sig*
Accrual Measures:									
Accruals	-	-0.287							
WC_acc	-			-2.215	**				
pmatch	-					-0.985	***		
sresid	-							-1.286	***
size	-	-0.212	**	-0.118		-0.131		-0.247	**
roa	-	0.040		0.044	**	0.119		0.041	
leverage	+			0.708		0.731		0.759	
BigN	+			0.432		0.348		0.388	
Intercept		15.486	***	14.923	***	15.806	***	16.073	***
Industry indicators		yes		yes		yes		yes	
Year indicators		yes		yes		yes		yes	
N		568		514		453		441	
No. of clusters (company)		489		443		392		382	
No. of clusters (year)		13		13		13		12	
Wald chi2		276.96		291.00		338.00		319.20	
Prob> chi2		<0.0001		<0.0001		<0.0001		<0.0001	
Pseudo R2		0.134		0.146		0.149		0.159	

TABLE 8 Continued

Panel B: Associations between Accrual Measures[a] and Unintentional Error for Firm-years With Auditor's SOX 404 Reports

UEindicator variable comparisons: *UE - Unintentional Errors (=1)* *IM - Intentional Misstatements (=0)* *UN - Unclassified Misstatements (=0)* ***Restatement Sample***		Model 5 UE vs. UN Indicator with SOX opinions		Model 6 UE vs. UN Indicator with SOX opinions		Model 7 UE vs. UN Indicator with SOX opinions		Model 8 UE vs. Other Indicator with SOX opinions		Model 9 UE vs. Other Indicator with SOX opinions		Model 10 UE vs. Other Indicator with SOX opinions	
	Predicted Sign	Coeff	*z sig*	Coeff	*z sig*	Coeff	*z sig*	Coeff	*z sig*	Coeff	*z sig*	Coeff	*z sig*
Accrual Measures[a] :													
Accruals	-	-1.855	**					-1.938	***				
WC_acc	-			0.208						-0.151			
pmatch [b]	-												
sresid	-					-0.455	***					-0.506	***
size	-	-0.083		-0.048		-0.077		-0.088		-0.045		-0.080	
roa	-	1.238	**	0.429		0.150		1.285	***	0.442		0.158	
leverage	+	0.797	***	0.511	*	0.432		0.848	***	0.573	**	0.487	
BigN	+	0.501	*	0.546	*	0.805	***	0.495	**	0.523	**	0.788	***
Intercept		-0.941		-1.382		-1.492	***	-0.900		-1.391		-1.440	***
Industry indicators		yes		yes		yes		yes		yes		yes	
Year indicators		yes		yes		yes		yes		yes		yes	
N		731		633		575		747		646		589	
No. of clusters (company)		625		547		502		635		555		510	
No. of clusters (year)		8		8		8		8		8		8	
Wald chi2		50.70		38.46		33.95		50.66		38.26		34.47	
Prob> chi2		0.003		0.055		0.109		0.003		0.057		0.098	
Pseudo R2		0.059		0.052		0.058		0.058		0.052		0.057	

***, **, * indicate one-tailed signifance at p <0.01, p <0.05, and p <0.1 levels respectively.

z-statistics are based on robust standard errors clustered two-ways by company and year.

Logistic regression of an indicator of unintentional error (UEindicator) equal to 1 if unintentional error and 0 otherwise as a function of total accruals (Accruals - Models 1, 5 and 8), working capital accruals (*WC_acc* - Models 2, 6, and 9), performance matched discretionary accruals (*pmatch* - Model 3), and studentized residuals (sresid), company size (size) proxied by logged total assets, return on asset (*roa*) proxied by earnings before extraordinary items scaled by average assets, leverage (leverage) equal to long term debt scaled by total assets, and Big N (Big N), an indicator vairable equal to 1 if the company is audite by a Big 4/5/6/8 audit firm (depending on the year).

Accrual measures (*Accruals, WC_acc, pmatch,* and *sresid*) winsorized at 1% and 99% to mitigate outliers. Variables are defined in Table 2.

a. Accrual measures are defined in Table 2 and further explained in Appendix C.

b. Results are not reported for the pmatch accrual model as the overall model fit was not significant (p >0.2) for both the UEvsUN and the UEvsOther samples.

TABLE 9 Meeting or Beating Analysts' Forecasts by One Penny [a] and Restatements

$$MeetBeat = \beta_0 + \beta_1\ UEindicator + \beta_2\ size + \beta 3\ leverage + \beta 4\ mb + \\ + \beta 5\ litigation + \beta 6\ roa + \beta 7\ Accruals + \beta 8 BigN + \beta 9 Numestimates \\ + \beta 10 \sigma(forecasts) + \text{Industry and Year fixed effects} + \varepsilon$$

U***Eindicator:*** *UE - Unintentional Errors* *IM - Intentional Misstatements* *UN - Unclassified Misstatements* ***Restatement Sample***		Model 1 UE vs. IM forecasts within 6 mths of earnings announcement for overall model fit			Model 2 UE vs. UN for firm-years with SOX 404 auditor's reports forecasts within 2 mths of earnings announcement			Model 3 UE vs. UN for firm-years with SOX 404 auditor's reports forecasts within 2 mths of earnings announcement		
	Predicted Sign	Coeff	*z*-statistic		Coeff	*z*-statistic		Coeff	*z*-statistic	
UEindicator	-	-0.381	-1.78	**	-0.546	-0.63		-0.522	-0.60	
size	+	-1.468	-2.24	**	-0.893	-1.04		-0.932	-1.02	
leverage	-	1.292	1.32		2.625	3.69	***	2.686	3.91	***
mb	+	-0.468	-2.28	**	-0.292	-0.79		-0.277	-0.65	
litigation	+	1.338	1.60	*	3.334	2.80	***	3.628	3.35	***
roa	+	3.351	1.56	*	4.479	3.92	***	4.124	3.82	***
Accruals	-	1.568	0.68		-1.436	-0.73		-0.604	-0.48	
BigN	+	0.934	1.60	*	0.145	0.12		0.351	0.28	
Numestimates	+	0.579	1.87	**	0.917	4.54	***	0.944	4.93	***
σ*(forecasts)*	-	-6.313	-2.28	**	-11.248	-2.65	***	-11.696	-2.72	***
Intercept		-15.759	-20.14	***	-19.615	-10.85	***	-18.840	-10.32	***
Industry indicators		yes			yes			yes		
Year indicators		yes			yes			yes		
N		270			215			219		
No. of clusters (company)		237			200			204		
No. of clusters (year)		12			8			8		
Wald chi2		367.71			193.57			201.27		
Prob>chi2		<.0001			<.0001			<.0001		
Pseudo R2		0.183			0.224			0.234		

***, **, * indicate one-tailed signifance at p <0.01, p <0.05, and p <0.1 levels respectively.

z-statistics are based on robust standard errors clustered two-ways by company and year.

a The "meet or beat" binary variable (equal to 1 if earnings per share meet or beat analysts' forecast by less than one penny and 0 otherwise) is the dependent variable in the logistic regressions. Both the actual EPS and the analyst's forecast nearest the EPS announcement date (or median forecast if more than one on the nearest day) are from the I/B/E/S unadjusted daily history following Reichelt and Wang (2010). The "meet or beat" binary variable is regressed on an indicator of unintentional error (*UEindicator*) equal to 1 if unintentional error and 0 for the other group in the sample (i.e. in Model 1 intentional misstatement; Model 2 unclassified; and Model 3 intentional misstatement and unclassified restatements); company size (*size*) proxied by logged total assets; the number of analysts' forecasts (*Numestimates*); standard deviation of forecasts (*σ(forecasts)*); and other variables as defined in Table 2. Return on assets (*roa*) and total accruals scaled by average assets (*Accruals*) are winsorized at 1% and 99% to mitigate against outliers.

TABLE 10 Proxy Validation Test Summary

UEindicator: *UE - Unintentional Errors* *IM - Intentional Misstatements* *UN - Unclassified Misstatements* ***Restatement Sample***		UE vs. IM		UE vs. UN		UE vs. Other i.e., IM & UN		Chapter 3 Sample UE vs. Other subsample [b, c]	
Sample size		1987 -2012 *N*: UE=751; IM=127		with auditor's SOX 404 reports N: UE=243; UN=588		with auditor's SOX 404 reports N: UE=243; Other=605		with auditor's SOX 404 reports N: UE=121; Other=104	
Validation test		Directionally consistent	*sig*	Directionally consistent	*sig*	Directionally consistent	*sig*	Directionally consistent	*sig*
V1	Proportion of revenue recognition issues (see Table 5)	√	***	-		-		√	***
V2	Proportion of net income corrections (see Table 6)	√	**	-		-		√	
V3	Association with earnings persistence (see Table 7)	√	*d*	√	***	√	***	√	***
V4	Less positive accruals: (see Table 8)								
	- total accruals	√		√	***	√	***	√	***
	-working capital accruals	√	**	-		√		√	**
	- performance matched Jones discretionary accruals	√	***	*a*		*a*		*a*	
	- studentized residuals from the Dechow and Dichev (2002)	√	***	√	***	√	***	*a*	
V5	less income smoothness: (see Table 3)								
	- net income smoothness	√		√	**	√	**	√	***
	- operating income smoothness	-		√		√		√	***
V6	meet or beat analysts' forecasts by a small amount (see Table 9)	√	**	√		√		*a*	

****, **, * indicate one-tailed signifance at p<0.01, p<0.05, and p<0.1 levels respectively.*

√ directionally as predicted.

- not as predicted directionally.

a results not reported as poor overall model fit for the statistical test

b The subsample of restatements to be used for Chapter 3 tests (subject to data availability).
This subsample of the UEvsOther sample includes only one restated firm-year per Company. The first year of the earliest restatement is retained for companies with more than one restatement. This restriction, together with a further restriction for unclassified restatements (see c below), creates a subsample of the UEvsOther sample comprised of 121 UE restatements, 6 IM restatements and 98 unclassified restatements with sufficient data for Chapter 3 tests.

c Only unclassified restatements with a scaled score greater than or equal to 0.5 are retained in the subsample. Scores are constructed such that restatements with higher scores have more characterisitcs predicted to be associated with intentional misstatements than other unclassified restatements. Each restatement in the group of unclassified restatements with an auditor's SOX 404 opinions is scored one point (maximum 9) if it corrects revenue recognition (1 point); corrects net income (1 point); meets or beats the latest analyst forecasts by one penny or less (1 point); or exceeds the group accruals or smoothness medians (maximum 6 points) and the total score is scaled by the number of non missing values.

d $p<0.001$ when comparison is limited to restatements with Auditor's SOX 404 reports

TABLE 11 Chapter 3 Sample Selection

	All Restatements with SOX 404 auditor's reports	Restatements that correct Unintentional Error UE	Restatements that correct Intentional Misstatement IM	Unclassified Restatements UN
Restatements with SOX 404 auditor's reports [a]	848	243	17	588
less: multiple firm-years per Company *- the earliest firm-year is retained for companies with more than one firm-year with SOX 404 auditor's reports*	(364)	(105)	(10)	(249)
	484	138	7	339
less: restatements for which CFO, CEO, and Director biographical and compensation data is not collected from proxy statements[b].	(214)			(214)
	270	138	7	125
less: firm-years without Chapter 3 data availability	(45)	(17)	(1)	(27)
Chapter 3 Sample	225	121	6	98

a Table 1 details the sample selection criteria for restatements with SOX 404 auditor's reports.

b Unclassified restatements with a scaled score less than 0.5 are dropped from the sample to reduce data collection costs. Scores are constructed such that restatements with higher scores have more characterisitcs predicted to be associated with intentional misstatements than other unclassified restatements. Each restatement in the group of unclassified restatements with an auditor's SOX 404 opinions is scored one point (maximum 9) if it corrects revenue recognition (1 point); corrects net income (1 point); meets or beats the latest analyst forecasts by one penny or less (1 point); or exceeds the group accruals or smoothness medians (maximum 6 points) and the total score is scaled by the number of non missing values.

TABLE 12 Chapter 3 Variable Definitions

Dependent Variable	*Definition*	*Reference*
ICWEAK	Two measures of internal control quality are used:	
ICWeakORIGINAL	An indicator variable equal to 1 if the auditor's SOX 404 report as originally filed reported controls over financial reporting were ineffective in year *t*; 0 otherwise [AuditAnalytics]; and	
ICWeakAMENDED	An indicator variable equal to 1 if the auditor's amended SOX 404 report reported controls over financial reporting were ineffective in year *t*; 0 otherwise [AuditAnalytics]	
ERROR	An indicator variable equal to 1 if non-technical annual restatement announcement indicates correction of unintentional error, and 0 if no restatement (see Chapter 2).	
INTENT	An indicator variable equal to 1 if non-technical annual restatement proxies for restatement that corrects intentional misstatement, and 0 if non-technical annual restatement announcement indicates correction of unintentional error (see Chapter 2).	
INTENTvsNORESTATE	An indicator variable equal to 1 if non-technical annual restatement proxies for restatement that corrects intentional misstatement, and 0 if no restatement.	
ORGCHANGE	An indicator variable equal to 1 if the firm engaged in restructuring in year *t* (COMPUSTAT data items RCP, RCA, RCEPS, or RCD); reports sales from mergers and acquisitions (COMPUSTAT data item ACQSC); or reports foreign pretax income in year t but not in year t-1 (COMPUSTAT data item PIFO); 0 otherwise.	Restructuring: e.g. Ashbaugh-Skaife et al. 2007, Cohen et al. 2014; Acquisition e.g. Cohen et al. 2014

TABLE 12 Continued

Test Variable	*Definition*	*Reference*
CFOEXP	Six alternative measures of CFO financial expertise are discussed:	
CFO_BIG4PARTNER	An indicator variable equal to 1 if the CFO has been a partner of a Big 4 or national firm, and 0 otherwise;	
CFOBIG4	An indicator variable equal to 1 if the CFO has audit experience at a Big 4 firm, and 0 otherwise;	
CFOMBA	An indicator variable equal to 1 if the CFO has an MBA; 0 otherwise;	Aier et al. 2005
CFOCPA	An indicator variable equal to 1 if the CFO has a CPA or CA designation; 0 otherwise;	Aier et al. 2005
CFOPREVIOUS	An indicator variable equal to 1 if the CFO has previous CFO experience at another company; and 0 otherwise; and	Aier et al. 2005
CFOTENURE	The square root of the number of years with the company. The number of years with the company is reported in the descriptive statistics.	Collins et al. 2009 (without transformation)
CFOPOWER	The natural log of the ratio of the CFO's to the CEO's total compensation. Total compensation computed is the sum of salary, bonus, restricted stock granted, total value of stock options granted (using Black-Scholes), long-term incentive payouts, and other miscellaneous compensation amounts. [Proxy statements]. Both the ratio of the CFO's to the CEO's total compensation and the logarithm of the ratio is reported in descriptive statistics.	Compensation computations following Hoitash et al. (2012); ratio to CEO pay adapted from Feng et al. (2011)

TABLE 12 Continued

Test Variable	*Definition*	*Reference*
ACEXP	Four alternative measures of audit committee financial expertise are discussed:	
ACAFE	The proportion of audit committee members where biographical data indicates financial expertise gained through accounting-related experience (i.e., CPA, certified public accountant, CFO, chief financial officer, principal financial officer, chief accounting officer, principal accounting officer, treasurer, auditor, vice president-finance, controller, vice president of finance, or experience working at a Big 4/5/6/8 audit firm). (proxy statements supplemented by manually searching CapitalIQ, Businessweek, Zoominfo, company websites, etc.)	e.g., Cohen et al. (2014); Hoitash et al. (2009) The three alternative measures defined below are components of this measure.
ACBIG4	An indicator variable equal to 1 if a member of the Audit Committee has audit experience at a Big 4 firm, and 0 otherwise;	
ACCPA	An indicator variable equal to 1 if a member of the Audit Committee has a CPA or CA qualification, and 0 otherwise;	
ACMBA	The proportion of audit committee members with an MBA degree (proxy statements supplemented by manually searching CapitalIQ, Businessweek, Zoominfo, company websites, etc.)	
AUDITORQUAL	Two alternative measures of auditor size are used to proxy for auditor quality in tests:	
OFFICESIZE	Natural logarithm of total office-specific audit fees in year *t* [*AuditAnalytics*]	e.g. Francis and Yu 2009 and Francis and Michas 2013
BIG4	An indicator variable equal to 1 for a Big 4/5/6/8 auditor depending on the year (i.e., Deloitte, EY, KPMG, PwC today) and 0 otherwise [*AuditAnalytics*]	e.g. Cohen et al. 2014, Lobo and Zhao 2013, Rice and Weber 2012

TABLE 12 Continued

Control Variable	*Definition*	*Reference*
Other auditor variables:		
AUDITFEE	Natural logarithm of the audit fees paid in year *t*. [*AuditAnalytics*]	e.g. Cohen et al. 2014, Lobo and Zhao 2013, Keune and Johnstone 2012
AUDITINF	Total client audit and nonaudit fees divided by the total office fees in year *t* [*AuditAnalytics*]	e.g. Francis and Yu 2009, Francis and Michas 2013
AUDITORTENURE	Natural logarithm of the number of years the auditor has been engaged by the client [*AuditAnalytics*]. The number of years the auditor has been engaged by the client is reported in the descriptive statistics.	e.g., Balsam et al. 2014, Carcello et al. 2011, lobo and Zhao 2013
AUDITORCHANGE	An indicator variable equal to 1 if the restated year is the first year of an auditor's engagement; 0 otherwise.	e.g., Balsam et al. 2014
Other management and governance variables:		
CFO_EQUITY_PAY	The natural log of value of stock options (using Black-Scholes) and restricted stock granted to the CFO scaled by the CFO's total compensation. Equal to 0 if neither stock options nor restricted stock awards were granted in the restated year.	Adapted from: Balsam et al.'s 2014 additional analysis and Hoitash et al. 2012
CFOCHANGE	An indicator variable equal to 1 if the restated year is the year the CFO is appointed CFO; 0 otherwise.	
GOVERNANCE	The following two alternative measures of corporate governance are used in tests:	
CEOINDEP	equals 1 if the CEO is not also the board chair; 0 otherwise (proxy statements).	e.g. Cohen et al. 2013, Keune and Johnstone 2012
GOV	The sum of five variables that assigns higher value to firms where the CEO is not the board chair (*CEOINDEP*), the CEO is not a founder of the company (*CEONOTFOUNDER*), the number of directors is less than the sample median (*SMALLER_BOARD*), the percentage of directors who are independent is greater than the sample median (*BOARD_INDEP*), and the percentage of independent board members with tenure longer than the CEO's tenure is greater than the sample median. (proxy statements)	Adapted from Carcello et al. 2006, DeFond et al. 2005, Hoitash et al. 2012 and Lisic et al. 2014. Cohen et al. 2013 includes measures of the five variables included in this composite.

TABLE 12 Continued

Control Variable	*Definition*	*Reference*
CEONOTFOUNDER	An indicator variable equal to 1 if the CEO is not also a company founder; 0 otherwise (proxy statements).	
SMALLER_BOARD	An indicator variable equal to 1 if the number of board of director members is less than the sample median; 0 otherwise (proxy statements).	
BOARD_INDEP	An indicator variable equal to 1 if the percentage of independent directors is greater than the sample median; 0 otherwise (proxy statements).	
BOARD_TENURE	An indicator variable equal to 1 if the percentage of independent board members with board tenure longer than CEO tenure is greater than the sample median (proxy statements).	
Other firm characteristics associated with internal control strength and restatement		
IAF	An indicator variable equal to 1 if either the Audit Committee charter or the DEF 14A proxy statement filed with the SEC refers to an Internal Audit function; 0 otherwise.	
FIRMSIZE	Natural logarithm of firm's market value of equity (share price x number of shares outstanding) [COMPUSTAT data item PRCC x CSHO]. The firm's market value of equity is reported in the descriptive statistics.	e.g., Doyle et al. 2007b
AGE	Natural logarithm of the number of years the firm has CRSP data. The number of years the firm has CRSP data is reported in the descriptive statistics.	e.g. Balsam et al. 2014, Cohen et al. 2014, Doyle et al. 2007b
LOSS	An indicator variable equal to 1 if earnings before extraordinary items in year *t* and *t*-1 sum to less than zero, and 0 otherwise (COMPUSTAT data item NI)	e.g. Bedard et al. 2014, Cohen et al. 2014, Doyle et al. 2007b
COMPLEXITY	Natural logarithm of the sum of the number of geographic and operating segments in year *t* [COMPUSTAT].	e.g., Balsam et al. 2014, Bedard et al. 2014, Doyle et al. 2007b, Ettredge et al. 2014

TABLE 12 Continued

Control Variable	*Definition*	*Reference*
LEVERAGE	Total liabilities divided by total assets in year *t*	e.g., Bedard et al. 2014, Cohen et al. 2014, Ettredge et al. 2014
MTB	Market to book ratio [CEQ divided by (CSHO * PRCC_F)] at the end of year *t*	e.g., Carcello et al. 2011, Cohen et al. 2014, Ettredge et al. 2014
VOLATILITY	Standard deviation of Sales calculated over a period of no less than three years (12 quarters) and no more than five years (20 quarters)	e.g., Balsam et al. 2014, Cohen et al. 2014
ANALYSTFOLLOW	An indicator variable equal to 1 if the number of analysts who issued earnings forecasts within the six months preceding the earnings announcement date is greater than or equal to the sample median [adapted from Bedard et al. 2014].	Bedard et al. 2014
LITIGIOUS	An indicator variable equal to 1 if a firm is in a litigious industry – SIC codes 2833 to 2836; 3570-3577; 3600-3674; 5200-5961 and 7370; O otherwise.	e.g. Cohen et al. 2014, Ettredge et al. 2014, Bedard et al. 2014
$\sum_t YEAR_t$	A vector of year-specific indicator variables	
$\sum_t INDUSTRY_t$	A vector of industry-specific indicator variables	

TABLE 13 Chapter 3 Samples [a,b,c] Restatement Frequency by Firm Size, Year, and Industry

Panel A: Frequency by firm size (market capitalization deciles following Dechow et al. 2011)

	ERROR[a]							INTENTvsNORESTATE[b]				INTENT[c]	
	Unintentional Error (UE)		Without restatement (NORESTATE)		binomial proportion test UE vs NORESTATE			Intentional Misstatement (IM)		binomial proportion test IM vs NORESTATE		binomial proportion test UE vs IM	
Decile rank [d]	n	Percent	n	Percent	z	sig		n	Percent	z	sig	z	sig
10	11	9.1%	12	9.9%	-0.22	0.827		11	10.6%	0.16	0.871	-0.37	0.708
9	6	5.0%	19	15.7%	-2.75	0.006	**	10	9.6%	-1.36	0.174	-1.36	0.175
8	15	12.4%	12	9.9%	0.61	0.540		7	6.7%	-0.86	0.391	1.43	0.154
7	15	12.4%	9	7.4%	1.29	0.197		11	10.6%	0.82	0.409	0.43	0.670
6	14	11.6%	13	10.7%	0.20	0.838		8	7.7%	-0.78	0.433	0.98	0.329
5	14	11.6%	10	8.3%	0.86	0.390		10	9.6%	0.36	0.723	0.47	0.636
4	12	9.9%	14	11.6%	-0.42	0.678		9	8.7%	-0.72	0.472	0.32	0.745
3	10	8.3%	14	11.6%	-0.86	0.390		10	9.6%	-0.47	0.636	-0.36	0.723
2	12	9.9%	9	7.4%	0.69	0.493		14	13.5%	1.49	0.137	-0.83	0.407
1	12	9.9%	9	7.4%	0.69	0.493		14	13.5%	1.49	0.137	-0.83	0.407
Total	121	100.0%	121	100.0%				104	100.0%				

a. The ERROR sample is comprised of restatements that correct unintentional error and a control group of firm-years without restatement. I chose firm-years in the control group that are the nearest in asset size to the firm-years of companies that restate to correct unintentional error that have all necessary data and are within the same two digit SIC code (per COMPUSTAT) and year.

b. The INTENTvsNORESTATE sample is comprised of restatements that proxy for restatements that correct intentional misstatement and the control group of firm-years without restatement described in (e) above.

c. The INTENT sample is comprised of restatements that correct unintentional error and the intentional misstatement correcting restatement proxy.

d. Firm size is based on the decile rank of market value (closing price * number of common shares outstanding).

TABLE 13 Continued
Panel B: Frequency by Year

	ERROR[a]						INTENTvsNORESTATE[b]				INTENT[c]	
	Unintentional Error (UE)		Without restatement (NORESTATE)		binomial proportion test UE vs NORESTATE		Intentional Misstatement (IM)		binomial proportion test IM vs NORESTATE		binomial proportion test UE vs IM	
Year [e]	n	Percent	n	Percent	z	sig	n	Percent	z	sig	z	sig
2004	12	9.9%	12	9.9%	n/a matched on year		18	17.3%	1.63	0.104	-1.63	0.104
2005	12	9.9%	12	9.9%	n/a matched on year		18	17.3%	1.63	0.104	-1.63	0.104
2006	21	17.4%	21	17.4%	n/a matched on year		16	15.4%	-0.40	0.691	0.40	0.691
2007	19	15.7%	19	15.7%	n/a matched on year		14	13.5%	-0.47	0.636	0.47	0.636
2008	24	19.8%	24	19.8%	n/a matched on year		13	12.5%	-1.48	0.139	1.48	0.139
2009	16	13.2%	16	13.2%	n/a matched on year		7	6.7%	-1.60	0.109	1.60	0.109
2010	13	10.7%	13	10.7%	n/a matched on year		14	13.5%	0.63	0.532	-0.63	0.532
2011	4	3.3%	4	3.3%	n/a matched on year		3	2.9%	*d*		*d*	
2012	0	0.0%	0	0.0%	n/a matched on year		1	1.0%	*d*		*d*	
Total	121	100.0%	121	100.0%			104	100.0%				

a. The ERROR sample is comprised of restatements that correct unintentional error and a control group of firm-years without restatement. I chose firm-years in the control group that are the nearest in asset size to the firm-years of companies that restate to correct unintentional error that have all necessary data and are within the same two digit SIC code (per COMPUSTAT) and year.

b. The INTENTvsNORESTATE sample is comprised of restatements that proxy for restatements that correct intentional misstatement and the control group of firm-years without restatement described in (e) above.

c. The INTENT sample is comprised of restatements that correct unintentional error and the intentional misstatement correcting restatement proxy.

d. Insufficent counts for meaningful statistical test

e. The year is the earliest fiscal year in the restated period when the restated period spans more than one year.

TABLE 13 Continued

Panel C: Frequency by Industry

	ERROR[a]						INTENTvsNORESTATE[b]				INTENT[c]	
	Unintentional Error (UE)		Without restatement (NORESTATE)		binomial proportion test UE vs NORESTATE		Intentional Misstatement (IM)		binomial proportion test IM vs NORESTATE		binomial proportion test UE vs IM	
Industry [e]	n	Percent	n	Percent	z	sig	n	Percent	z	sig	z	sig
Agriculture	0	0.0%	0	0.0%	*d*		0	0.0%	*d*		*d*	
Mining & Construction	4	3.3%	7	5.8%	*d*		4	3.8%	*d*		*d*	
Food & Tobacco	2	1.7%	1	0.8%	*d*		1	1.0%	*d*		*d*	
Textile & Apparel	3	2.5%	3	2.5%	*d*		1	1.0%	*d*		*d*	
Lumber, Furniture, & Printing	2	1.7%	1	0.8%	*d*		6	5.8%	*d*		*d*	
Chemicals	3	2.5%	2	1.7%	*d*		3	2.9%	*d*		*d*	
Refining & Extractive	5	4.1%	5	4.1%	0.00	1.000	4	3.8%	*d*		*d*	
Durable Manufacturers	24	19.8%	27	22.3%	-0.31	0.754	14	13.5%	-1.72	0.086	1.42	0.155
Computers	16	13.2%	12	9.9%	0.80	0.422	6	5.8%	-1.14	0.253	1.88	0.061
Transportation	7	5.8%	5	4.1%	0.59	0.554	3	2.9%	*d*		*d*	
Utilities	5	4.1%	4	3.3%	*d*		4	3.8%	*d*		*d*	
Retail	6	5.0%	7	5.8%	-0.29	0.776	8	7.7%	0.57	0.568	-0.85	0.397
Services	13	10.7%	15	12.4%	-0.40	0.688	17	16.3%	0.84	0.398	-1.23	0.218
Banks & Insurance	21	17.4%	20	16.5%	0.17	0.864	23	22.1%	1.06	0.288	-0.90	0.369
Pharmaceuticals	10	8.3%	12	9.9%	-0.45	0.655	10	7.3%	-0.08	0.939	-0.36	0.723
Total	121	100.0%	121	100.0%			104	100.0%				

**, * indicates a statistical difference at respectively $p<0.01$ and $p<0.05$

a., b., and c. See Panels A and B for sample descriptions.

d. Insufficent counts for meaningful statistical test

e. Industries are based on SIC codes Frankel, Johnson, and Nelson (2002) as described by Dechow et al. (2011):
Agriculture: 0100–0999; Mining & Construction: 1000–1299, 1400–1999; Food & Tobacco: 2000–2141; Textiles and Apparel: 2200–2399; Lumber, Furniture, & Printing: 2400–2796; Chemicals: 2800–2824, 2840–2899; Refining & Extractive: 1300–1399, 2900–2999; Durable Manufacturers: 3000–3569, 3580–3669, 3680–3828, 3852-3999; Computers: 3570–3579, 3670–3679, 7370–7379; Transportation: 4000–4899; Utilities: 4900–4999;Retail: 5000–5999; Services: 7000–7369, 7380–9999; Banks & Insurance: 6000–6999; Pharmaceuticals: 2830–2836, 3829–3851.

TABLE 14 Chapter 3 Descriptive Statistics

Panel A: Associations with Internal Control Weaknesses using the ERROR[a] Sample (N=242)

	ICWeakAMENDED==1					ICWeakAMENDED==0					Comparison		
	N	mean	p50	sd	skewness	N	mean	p50	sd	skewness	z or Chi2	p	sig
ORGCHANGE	21	0.476	0	0.512	0.095	221	0.267	0	0.443	1.054	4.12	0.042	**
CFOBIG4	21	0.571	1	0.507	-0.289	221	0.421	0	0.495	0.321	1.77	0.183	
CFOPOWER	21	-0.620	-0.712	0.512	0.760	221	-0.867	-0.855	0.661	0.157	1.64	0.101	
ACBIG4	21	0.381	0	0.498	0.490	221	0.430	0	0.496	0.283	-0.19	0.665	
OFFICESIZE	21	16.599	17.042	2.011	-0.377	221	17.047	17.266	1.577	-0.489	-0.85	0.396	
BIG4	21	0.762	1	0.436	-1.230	221	0.801	1	0.400	-1.507	-0.18	0.671	
AUDITFEE	21	13.910	13.817	0.829	0.705	221	13.845	13.918	0.843	0.018	-0.01	0.995	
AUDITINF	21	0.223	0.086	0.307	1.611	221	0.096	0.039	0.144	3.063	1.23	0.219	
AUDITORTENURE	21	1.784	1.792	0.971	-0.150	221	1.896	1.946	0.820	-0.402	-0.56	0.574	
AUDITORCHANGE	21	0.095	0	0.301	2.758	221	0.054	0	0.227	3.934	0.59	0.443	
CFOCHANGE	21	0.238	0	0.436	1.230	221	0.140	0	0.348	2.072	1.45	0.229	
CFO_EQUITY_PAY	21	0.100	0.000	0.216	1.704	221	0.193	0.000	0.265	0.992	-1.80	0.075	*
CEOINDEP	21	0.476	0	0.512	0.095	221	0.525	1	0.501	-0.100	-0.18	0.670	
IAF	21	0.762	1	0.436	-1.230	221	0.842	1	0.366	-1.871	-0.88	0.347	
FIRMSIZE (ln of $M)	21	5.612	5.261	1.277	1.574	221	6.433	6.426	1.310	0.201	-3.09	0.002	***
AGE (ln of years)	21	2.581	2.639	0.864	-0.015	221	2.614	2.639	0.769	-0.244	-0.17	0.864	
LOSS	21	0.571	1	0.507	-0.289	221	0.335	0	0.473	0.700	4.69	0.030	**
COMPLEXITY	21	1.086	1.099	0.800	-0.128	221	1.007	1.099	0.776	-0.069	0.54	0.587	
LEVERAGE	21	0.612	0.593	0.395	0.588	221	0.558	0.534	0.291	0.627	0.42	0.674	
MTB	21	22.477	2.235	51.109	2.542	221	6.845	2.005	21.045	5.498	1.00	0.316	
VOLATILITY	21	48.282	10.710	104.357	3.592	221	60.917	23.418	105.589	3.269	-1.27	0.204	
ANALYSTFOLLOW	21	0.476	0	0.512	0.095	221	0.615	1	0.488	-0.474	-1.55	0.213	
LITIGIOUS	21	0.143	0	0.359	2.041	221	0.199	0	0.400	1.507	-0.39	0.534	

*, **, *** $p<0.1$, $p<0.05$ & $p<0.01$ two-tailed. Continuous variables are z-statistics from Wilcoxon ranksum tests.

all continuous variables are winsorized at 1% and 99% to mitigate against outliers.

TABLE 14 Continued

Panel B: Associations with Internal Control Weaknesses using the INTENTvsNORESTATE[a] sample (N=225)

	ICWeakAMENDED==1					ICWeakAMENDED==0					Comparison		
	N	mean	p50	sd	skewness	N	mean	p50	sd	skewness	z or Chi2	p	sig
ORGCHANGE	35	0.257	0	0.443	1.111	190	0.247	0	0.433	1.171	0.02	0.902	
CFOBIG4	35	0.400	0	0.497	0.408	190	0.368	0	0.484	0.546	0.13	0.723	
CFOPOWER	35	-0.746	-0.679	0.563	-1.298	190	-0.884	-0.860	0.627	0.172	2.00	0.045	**
ACBIG4	35	0.200	0	0.406	1.500	190	0.400	0	0.491	0.408	-5.08	0.024	**
OFFICESIZE	35	16.374	16.417	2.050	-0.096	190	16.971	17.248	1.565	-0.589	-1.64	0.102	
BIG4	35	0.571	1	0.502	-0.289	190	0.800	1	0.401	-1.500	-8.58	0.003	***
AUDITFEE	35	13.354	13.358	0.921	0.134	190	13.809	13.814	0.859	0.094	-2.61	0.009	***
AUDITINF	35	0.164	0.064	0.254	2.230	190	0.104	0.042	0.161	2.921	0.45	0.651	
AUDITORTENURE	35	1.686	1.946	0.911	-0.564	190	1.947	2.079	0.854	-0.347	-1.35	0.177	
AUDITORCHANGE	35	0.143	0	0.355	2.041	190	0.053	0	0.224	4.007	3.87	0.049	**
CFOCHANGE	35	0.086	0	0.284	2.960	190	0.126	0	0.333	2.250	-0.46	0.497	
CFO_EQUITY_PAY	35	0.085	0.000	0.193	2.121	190	0.153	0.000	0.234	1.272	-1.83	0.066	*
CEOINDEP	35	0.571	1	0.502	-0.289	190	0.463	0	0.500	0.148	1.39	0.239	
IAF	35	0.800	1	0.406	-1.500	190	0.847	1	0.361	-1.932	-0.49	0.482	
FIRMSIZE (ln of $M)	35	5.578	5.424	1.378	0.407	190	6.452	6.295	1.324	0.204	-3.34	<0.001	***
AGE (ln of years)	35	2.282	2.398	0.797	-0.661	190	2.629	2.639	0.800	-0.296	-1.97	0.048	**
LOSS	35	0.514	1	0.507	-0.057	190	0.247	0	0.433	1.171	10.25	0.001	***
COMPLEXITY	35	0.737	0.693	0.742	0.348	190	0.977	1.099	0.766	-0.057	-1.62	0.106	
LEVERAGE	35	0.571	0.564	0.321	-0.155	190	0.544	0.532	0.292	1.091	0.77	0.442	
MTB	35	5.936	2.039	11.942	3.444	190	6.450	2.072	23.979	6.464	-0.13	0.894	
VOLATILITY	35	42.391	10.172	120.766	5.207	190	67.244	21.665	123.398	3.299	-2.41	0.016	**
ANALYSTFOLLOW	35	0.229	0	0.426	1.293	190	0.605	1	0.490	-0.431	-16.92	<0.001	***
LITIGIOUS	35	0.143	0	0.355	2.041	190	0.168	0	0.375	1.772	-0.14	0.708	

*, **, *** $p<0.1$, $p<0.05$ & $p<0.01$ two-tailed. Continuous variables are z-statistics from Wilcoxon ranksum tests.
all continuous variables are winsorized at 1% and 99% to mitigate against outliers.

TABLE 14 Continued

Panel C: Differences between firm-years with and without restatement using the ERROR[a] sample (N=242)

	ERROR==1					ERROR==0					ERROR sample comparison		
	N	mean	p50	sd	skewness	N	mean	p50	sd	skewness	z or Chi2	p	sig
ICWeakAMENDED	121	0.165	0	0.373	1.802	121	0.008	0	0.091	10.863	18.82	<0.001	***
ICWeakORIGINAL	121	0.083	0	0.276	3.032	121	0.008	0	0.091	10.863	7.71	0.005	***
ORGCHANGE	121	0.347	0	0.478	0.642	121	0.223	0	0.418	1.330	4.56	0.033	**
CFOBIG4	121	0.463	0	0.501	0.149	121	0.405	0	0.493	0.387	0.82	0.364	
CFOPOWER	121	-0.791	-0.784	0.684	-0.039	121	-0.899	-0.873	0.617	0.332	1.54	0.123	
ACBIG4	121	0.430	0	0.497	0.284	121	0.421	0	0.496	0.318	0.02	0.897	
OFFICESIZE	121	17.073	17.347	1.705	-0.499	121	16.943	17.248	1.534	-0.545	0.75	0.455	
BIG4	121	0.810	1	0.394	-1.580	121	0.785	1	0.412	-1.388	0.23	0.631	
AUDITFEE	121	13.904	14.014	0.850	0.002	121	13.798	13.827	0.831	0.134	1.02	0.310	
AUDITINF	121	0.119	0.037	0.194	2.873	121	0.095	0.044	0.136	2.928	-0.18	0.858	
AUDITORTENURE	121	1.898	1.946	0.797	-0.405	121	1.875	1.946	0.870	-0.353	0.18	0.855	
AUDITORCHANGE	121	0.050	0	0.218	4.150	121	0.066	0	0.250	3.492	-0.30	0.582	
CFOCHANGE	121	0.182	0	0.387	1.650	121	0.116	0	0.321	2.403	2.09	0.148	
CFO_EQUITY_PAY	121	0.182	0.000	0.267	1.065	121	0.187	0.000	0.258	1.020	-0.42	0.672	
CEOINDEP	121	0.579	1	0.496	-0.318	121	0.463	0	0.501	0.149	3.25	0.072	*
IAF	121	0.835	1	0.373	-1.802	121	0.835	1	0.373	-1.802	0.00	1.000	
FIRMSIZE (ln of $M)	121	6.253	6.155	1.330	0.344	121	6.470	6.308	1.317	0.243	-1.17	0.240	
AGE (ln of years)	121	2.572	2.639	0.804	-0.217	121	2.651	2.639	0.747	-0.201	-0.64	0.525	
LOSS	121	0.471	0	0.501	0.116	121	0.240	0	0.429	1.220	14.15	<0.001	***
COMPLEXITY	121	1.045	1.099	0.754	-0.167	121	0.984	1.099	0.800	0.017	0.65	0.518	
LEVERAGE	121	0.586	0.605	0.308	0.313	121	0.539	0.509	0.293	1.044	1.39	0.164	
MTB	121	10.707	2.005	29.634	4.106	121	5.696	2.048	19.889	6.989	-0.56	0.578	
VOLATILITY	121	59.421	20.514	101.807	3.027	121	60.221	23.177	109.159	3.493	-0.29	0.770	
ANALYSTFOLLOW	121	0.570	1	0.497	-0.284	121	0.636	1	0.483	-0.567	-1.11	0.293	
LITIGIOUS	121	0.198	0	0.400	1.513	121	0.190	0	0.394	1.580	0.03	0.871	

*, **, *** $p<0.1$, $p<0.05$ & $p<0.01$ two-tailed. Continuous variables are z-statistics from Wilcoxon ranksum tests.

all continuous variables are winsorized at 1% and 99% to mitigate against outliers.

TABLE 14 Continued

Panel D: Differences between firm-years with and without restatement using the INTENTvsNORESTATE[a] sample (N=225)

	INTENT==1					ERROR==0					INTENTvsNORESTATE sample comparison		
	N	mean	p50	sd	skewness	N	mean	p50	sd	skewness	z or Chi2	p	sig
ICWeakAMENDED	104	0.327	0	0.471	0.738	121	0.008	0	0.091	10.863	43.23	<0.001	***
ICWeakORIGINAL	104	0.106	0	0.309	2.564	121	0.008	0	0.091	10.863	10.53	<0.001	***
ORGCHANGE	104	0.279	0	0.451	0.986	121	0.223	0	0.418	1.330	0.93	0.335	
CFOBIG4	104	0.337	0	0.475	0.692	121	0.405	0	0.493	0.387	-1.12	0.290	
CFOPOWER	104	-0.819	-0.734	0.620	-0.414	121	-0.899	-0.873	0.617	0.332	1.75	0.080	*
ACBIG4	104	0.308	0	0.464	0.833	121	0.421	0	0.496	0.318	-3.11	0.078	*
OFFICESIZE	104	16.803	16.983	1.798	-0.485	121	16.943	17.248	1.534	-0.545	-0.36	0.722	
BIG4	104	0.740	1	0.441	-1.097	121	0.785	1	0.412	-1.388	-0.62	0.430	
AUDITFEE	104	13.669	13.670	0.938	0.047	121	13.798	13.827	0.831	0.134	-1.02	0.310	
AUDITINF	104	0.134	0.043	0.219	2.495	121	0.095	0.044	0.136	2.928	-0.14	0.891	
AUDITORTENURE	104	1.944	2.079	0.864	-0.443	121	1.875	1.946	0.870	-0.353	0.70	0.482	
AUDITORCHANGE	104	0.067	0	0.252	3.454	121	0.066	0	0.250	3.492	0.00	0.971	
CFOCHANGE	104	0.125	0	0.332	2.268	121	0.116	0	0.321	2.403	0.05	0.831	
CFO_EQUITY_PAY	104	0.091	0.000	0.179	1.816	121	0.187	0.000	0.258	1.020	-3.00	0.003	
CEOINDEP	104	0.500	1	0.502	0.000	121	0.463	0	0.501	0.149	0.31	0.578	
IAF	104	0.846	1	0.363	-1.919	121	0.835	1	0.373	-1.802	0.05	0.815	
FIRMSIZE (ln of $M)	104	6.137	6.040	1.408	0.178	121	6.470	6.308	1.317	0.243	-1.69	0.091	*
AGE (ln of years)	104	2.487	2.485	0.867	-0.370	121	2.651	2.639	0.747	-0.201	-1.23	0.217	
LOSS	104	0.346	0	0.478	0.647	121	0.240	0	0.429	1.220	3.09	0.079	*
COMPLEXITY	104	0.888	1.099	0.724	-0.055	121	0.984	1.099	0.800	0.017	-0.84	0.399	
LEVERAGE	104	0.560	0.554	0.301	0.657	121	0.539	0.509	0.293	1.044	0.85	0.394	
MTB	104	7.155	2.066	25.303	6.255	121	5.696	2.048	19.889	6.989	-0.40	0.689	
VOLATILITY	104	67.052	15.438	137.930	3.445	121	60.221	23.177	109.159	3.493	-1.46	0.143	
ANALYSTFOLLOW	104	0.442	0	0.499	0.232	121	0.636	1	0.483	-0.567	-8.50	0.004	***
LITIGIOUS	104	0.135	0	0.343	2.141	121	0.190	0	0.394	1.580	-1.25	0.263	

*, **, *** $p<0.1$, $p<0.05$ & $p<0.01$ two-tailed. Continuous variables are z-statistics from Wilcoxon ranksum tests.
all continuous variables are winsorized at 1% and 99% to mitigate against outliers.

TABLE 14 Continued

Panel E: Differences between firm-years with restatements using the INTENT[a] sample (N=225)

	INTENT==1					INTENT = 0 (& ERROR==1)					INTENT sample comparison		
	N	mean	p50	sd	skewness	N	mean	p50	sd	skewness	z or Chi2	p	sig
ICWeakAMENDED	104	0.327	0	0.471	0.738	121	0.165	0	0.373	1.802	8.01	0.005	***
ICWeakORIGINAL	104	0.106	0	0.309	2.564	121	0.083	0	0.276	3.032	0.35	0.552	
ORGCHANGE	104	0.279	0	0.451	0.986	121	0.347	0	0.478	0.642	-1.01	0.272	
CFOBIG4	104	0.337	0	0.475	0.692	121	0.463	0	0.501	0.149	-3.70	0.054	*
CFOPOWER	104	-0.819	-0.734	0.620	-0.414	121	-0.791	-0.784	0.684	-0.039	0.54	0.957	
ACBIG4	104	0.308	0	0.464	0.833	121	0.430	0	0.497	0.284	-3.56	0.059	*
OFFICESIZE	104	16.803	16.983	1.798	-0.485	121	17.073	17.347	1.705	-0.499	-1.11	0.265	
BIG4	104	0.740	1	0.441	-1.097	121	0.810	1	0.394	-1.580	-1.56	0.211	
AUDITFEE	104	13.669	13.670	0.938	0.047	121	13.904	14.014	0.850	0.002	-1.87	0.062	*
AUDITINF	104	0.134	0.043	0.219	2.495	121	0.119	0.037	0.194	2.873	-0.04	0.971	
AUDITORTENURE	104	1.944	2.079	0.864	-0.443	121	1.898	1.946	0.797	-0.405	0.45	0.651	
AUDITORCHANGE	104	0.067	0	0.252	3.454	121	0.050	0	0.218	4.150	0.32	0.570	
CFOCHANGE	104	0.125	0	0.332	2.268	121	0.182	0	0.387	1.650	-1.37	0.241	
CFO_EQUITY_PAY	104	0.091	0.000	0.179	1.816	121	0.182	0.000	0.267	1.065	-2.46	0.014	**
CEOINDEP	104	0.500	1	0.502	0.000	121	0.579	1	0.496	-0.318	-1.39	0.239	
IAF	104	0.846	1	0.363	-1.919	121	0.835	1	0.373	-1.802	0.05	0.815	
FIRMSIZE (ln of $M)	104	6.137	6.040	1.408	0.178	121	6.253	6.155	1.330	0.344	-0.68	0.499	
AGE (ln of years)	104	2.487	2.485	0.867	-0.370	121	2.572	2.639	0.804	-0.217	-0.62	0.537	
LOSS	104	0.346	0	0.478	0.647	121	0.471	0	0.501	0.116	-3.60	0.058	*
COMPLEXITY	104	0.888	1.099	0.724	-0.055	121	1.045	1.099	0.754	-0.167	-1.56	0.118	
LEVERAGE	104	0.560	0.554	0.301	0.657	121	0.586	0.605	0.308	0.313	-0.82	0.414	
MTB	104	7.155	2.066	25.303	6.255	121	10.707	2.005	29.634	4.106	0.17	0.865	
VOLATILITY	104	67.052	15.438	137.930	3.445	121	59.421	20.514	101.807	3.027	-1.22	0.221	
ANALYSTFOLLOW	104	0.442	0	0.499	0.232	121	0.570	1	0.497	-0.284	-3.66	0.056	*
LITIGIOUS	104	0.135	0	0.343	2.141	121	0.198	0	0.400	1.513	-1.62	0.203	

*, **, *** $p<0.1$, $p<0.05$ & $p<0.01$ two-tailed. Continuous variables are z-statistics from Wilcoxon ranksum tests.
all continuous variables are winsorized at 1% and 99% to mitigate against outliers.

TABLE 15 Chapter 3 Descriptive Statistics for Alternative Measures

	INTENT==1			INTENT=0 (& ERROR=1)			INTENT sample			ERROR=0			ERROR sample			INTENTvsNORESTATE sample		
Organizational Change:	N	mean	p50	N	mean	p50	*z or Chi2*	*p*	sig	N	mean	p50	*z or Chi2*	*p*	sig	*z or Chi2*	*p*	sig
ORGCHANGE	104	0.279	0	121	0.339	0	-1.21	0.272		121	0.23	0	1.85	0.064	*	0.93	0.335	
CFO Expertise (CFOEXP) - *one of*:																		
CFO_BIG4PARTNER	104	0.067	0	121	0.050	0	0.32	0.570		121	0.066	0	-0.30	0.582		0.00	0.971	
CFOBIG4	104	0.337	0	121	0.463	0	-3.70	0.054	*	121	0.405	0	0.91	0.364		-1.12	0.290	
CFOCPA	104	0.529	1	121	0.612	1	-1.56	0.211		121	0.562	1	0.78	0.434		-0.25	0.619	
CFOMBA	104	0.462	0	121	0.372	0	1.85	0.173		121	0.380	0	-0.13	0.894		1.23	0.268	
CFOPREVIOUS	104	0.644	1	121	0.653	1	-0.02	0.892		121	0.636	1	0.27	0.788		0.00	0.995	
CFOTENURE	104	8.519	7	121	8.736	6	0.10	0.918		121	9.760	8	-1.95	0.051	*	-1.90	0.057	*
CFO Influence:																		
CFOPOWER	104	0.582	0.480	121	0.588	0.457	0.06	0.953		121	0.514	0.418	1.54	0.123		1.75	0.080	*
Audit Committee Expertise (ACEXP) - *one of:*																		
ACAFE	104	0.298	0.333	121	0.349	0.333	-1.65	0.099	*	121	0.314	0.333	1.24	0.216		-0.31	0.756	
ACBIG4	104	0.308	0	121	0.430	0	-3.56	0.059	*	121	0.421	0	0.02	0.897		-3.11	0.078	*
ACCPA	104	0.500	1	121	0.636	1	-4.25	0.039	**	121	0.537	1	2.45	0.117		-0.31	0.578	
ACMBA	104	0.326	0.333	121	0.317	0.333	0.11	0.914		121	0.337	0.333	-0.29	0.770		-0.16	0.875	
Auditor Size (AUDITORQUAL) *one of*:																		
OFFICESIZE	104	16.793	16.983	121	17.080	17.347	-1.12	0.263		121	16.943	17.248	0.75	0.455		-0.36	0.722	
BIG4	104	0.740	1	121	0.810	1	-1.56	0.211		121	0.785	1	0.23	0.631		-0.62	0.430	

*, **, *** $p<0.1$, $p<0.05$ & $p<0.01$ two-tailed. Continuous variables comparisons are z-statistics from Wilcoxon ranksum tests.
Continuous variables are not winsorized
Variables are described in Table 12.

TABLE 15 Continued

	INTENT==1			INTENT=0 (& ERROR=1)			INTENT sample			ERROR=0			ERROR sample			INTENTvsNORESTATE sample		
	N	mean	p50	N	mean	p50	*z or Chi2*	*p*	sig	N	mean	p50	*z or Chi2*	*p*	sig	*z or Chi2*	*p*	sig
Other Auditor Characteristics:																		
AUDITFEE	104	13.657	13.67	121	13.911	14.014	-1.87	0.062	*	121	13.804	13.827	1.02	0.309		-1.02	0.309	
AUDITINF	104	0.134	0.043	121	0.124	0.037	-0.04	0.969		121	0.095	0.044	-0.18	0.858		-0.14	0.891	
AUDITORTENURE	104	9.712	8	121	8.818	7	0.45	0.651		121	9.107	7	0.23	0.822		0.71	0.480	
AUDITORCHANGE	104	0.067	0	121	0.050	0	0.33	0.570		121	0.066	0	0.18	0.855		0.00	0.971	
Other Management/Governance Characterisitcs:																		
CFO_EQUITY_PAY	104	0.091	0	121	0.182	0	-2.46	0.014	**	121	0.187	0	-0.43	0.671		-3.00	0.003	***
CFOCHANGE	104	0.125	0	121	0.182	0	-1.37	0.241		121	0.116	0	2.09	0.148		0.05	0.831	
GOVERNANCE - one of:																		
CEOINDEP	104	0.500	0.500	121	0.579	1	-1.39	0.239		121	0.463	0	3.25	0.072	*	0.31	0.578	
GOV	104	2.654	3	121	2.727	3	-0.39	0.700		121	2.678	3	0.24	0.811		-0.15	0.884	
CEONOTFOUNDER	104	0.817	1	121	0.777	1	0.56	0.453		121	0.86	1	-2.78	0.096	*	-0.74	0.389	
SMALLER_BOARD	104	0.404	0	121	0.455	0	-0.59	0.444		121	0.355	0	2.47	0.116		0.56	0.455	
BOARD_INDEP	104	0.471	0	121	0.405	0	1.00	0.318		121	0.479	0	-1.36	0.244		-0.02	0.902	
BOARD_TENURE	104	0.462	0	121	0.512	1	-0.58	0.447		121	0.521	1	-0.02	0.898		-0.78	0.376	
IAF	104	0.846	1	121	0.835	1	0.05	0.815		121	0.835	1	0.00	1.000		0.05	0.815	
Firm Characteristics																		
ICWeakORIGINAL	104	0.106	0	121	0.083	0	0.35	0.552		121	0.008	0	7.71	0.005	***	10.53	0.001	***
ICWeakAMENDED	104	0.327	0	121	0.165	0	8.01	0.005	***	121	0.008	0	18.82	<0.001	***	43.23	<0.001	***
FIRMSIZE $M	104	1190.442	420.300	121	1557.804	471.247	-0.68	0.499		121	1591.588	549.149	-1.17	0.241		-1.69	0.091	*
AGE (years)	104	16.981	12.000	121	17.752	14.000	-0.63	0.532		121	18.314	14.000	-0.64	0.525		-1.24	0.215	
LOSS	104	0.346	0	121	0.471	0	-3.60	0.058	*	121	0.240	0	14.14	<0.001	***	3.09	0.079	*
ROA	104	-0.012	0.030	121	-0.029	0.010	3.08	0.002	***	121	0.014	0.047	-4.94	<0.001	***	-1.61	0.107	
COMPLEXITY	104	0.888	1.099	121	1.045	1.099	-1.70	0.089	*	121	0.986	1.099	0.65	0.519		-0.84	0.399	
LEVERAGE	104	0.596	0.554	121	0.587	0.605	-0.82	0.414		121	0.541	0.509	1.39	0.165		0.85	0.395	
MTB	104	3.584	1.896	121	4.156	1.790	1.18	0.240		121	1.885	2.003	-1.22	0.222		-0.25	0.804	
VOLATILITY	104	67.534	15.438	121	59.421	20.514	-1.22	0.221		121	62.424	23.177	-0.29	0.770		-1.47	0.143	
ANALYSTFOLLOW	104	0.231	0	121	0.438	0	-10.67	0.001	***	121	0.364	0	1.39	0.238		-4.68	0.030	**
LITIGIOUS	104	0.135	0	121	0.198	0	-1.62	0.203		121	0.190	0	0.03	0.871		-1.25	0.263	

*, **, *** $p<0.1$, $p<0.05$ & $p<0.01$ two-tailed. Continuous variables comparisons are z-statistics from Wilcoxon ranksum tests.
Continuous variables are not winsorized
Variables are described in Table 12.

TABLE 16 Correlations of Test and Control Variables

Panel A: Pearson correlations of test and control variables using the ERROR sample (N=242)

	ORG CHANGE	CFO BIG4	CFO POWER	AC BIG4	OFFICE SIZE	AUDIT FEE	AUDITOR TENURE	AUDITOR CHANGE	CFO CHANGE	CFO EQUITY PAY
ORGCHANGE	1.000									
CFOBIG4	0.186 **	1.000								
CFOPOWER	-0.116	-0.037	1.000							
ACBIG4	0.086	0.073	0.045	1.000						
OFFICESIZE	0.165 *	-0.046	-0.119	0.034	1.000					
BIG4	0.136 *	0.026	-0.138 *	0.101	0.669 ***					
AUDITFEE	0.297 ***	0.064	-0.285 ***	-0.001	0.453 ***	1.000				
AUDITINF	-0.004	0.107	0.046	-0.114	-0.708 ***	-0.057				
AUDITORTENURE	0.111	0.027	-0.098	0.118	0.290 ***	0.270 ***	1.000			
AUDITORCHANGE	0.040	0.033	0.021	-0.070	-0.144 *	-0.093	-0.563 ***	1.000		
CFOCHANGE	-0.007	0.032	0.033	0.086	-0.078	0.043	-0.039	-0.054	1.000	
CFO_EQUITY_PAY	0.095	-0.117	-0.090	-0.068	0.109	0.265 ***	-0.034	0.002	0.052	1.000
CEOINDEP	0.020	0.006	0.073	-0.011	-0.078	-0.024	-0.115	0.202 **	0.145 *	0.016
IAF	-0.040	-0.037	-0.127 *	0.136 *	0.042	0.217 ***	0.015	-0.033	-0.127 *	0.061
FIRMSIZE	0.046	-0.101	-0.250 ***	-0.089	0.289 ***	0.587 ***	0.121	-0.077	-0.065	0.272 ***
AGE	0.098	-0.010	-0.081	0.022	0.071	0.181 **	0.258 ***	-0.057	-0.085	0.030
LOSS	0.220 ***	0.082	0.052	0.059	-0.011	-0.067	0.054	-0.036	0.126 *	0.019
ROA	-0.057	0.018	-0.049	-0.016	-0.014	0.133 *	-0.008	0.002	-0.031	-0.049
COMPLEXITY	0.260 ***	0.021	-0.107	-0.021	0.236 ***	0.542 ***	0.208 **	0.031	-0.016	0.122
LEVERAGE	-0.054	0.039	-0.073	0.008	-0.136 *	-0.027	-0.017	0.020	0.066	0.003
MTB	0.123	0.074	0.054	-0.033	-0.022	-0.066	0.173 **	-0.059	-0.002	-0.083
VOLATILITY	0.113	-0.063	-0.216 ***	-0.069	0.209 **	0.563 ***	0.103	-0.003	-0.056	0.284 ***
ANALYSTFOLLOW	0.082	-0.006	-0.060	0.049	0.159 *	0.294 ***	0.016	-0.016	-0.017	0.173 **
LITIGIOUS	0.037	-0.072	0.090	0.021	0.139 *	-0.093	0.066	0.013	-0.029	-0.038

*, **, *** $p < 0.05$, $p < 0.01$ & $p < 0.001$

all continuous variables are winsorized at 1% and 99% to mitigate against outliers.

TABLE 16 Panel A Continued

	CEO INDEP	IAF		FIRM SIZE		AGE		LOSS		COMP LEXITY		LEVER AGE		MTB		VOL ATILITY		ANALYST FOLLOW		LITIGIOUS
ORGCHANGE																				
CFOBIG4																				
CFOPOWER																				
ACBIG4																				
OFFICESIZE																				
BIG4																				
AUDITFEE																				
AUDITINF																				
AUDITORTENURE																				
AUDITORCHANGE																				
CFOCHANGE																				
CFO_EQUITY_PAY																				
CEOINDEP	1.000																			
IAF	-0.004	1.000																		
FIRMSIZE	-0.081	0.242	***	1.000																
AGE	-0.095	0.174	**	0.191	**	1.000														
LOSS	0.056	-0.088		-0.322	***	-0.081		1.000												
COMPLEXITY	-0.069	0.043		0.295	***	0.337	***	-0.015		1.000										
LEVERAGE	0.080	0.071		-0.091		-0.006		0.160		-0.237	***	1.000								
MTB	0.048	-0.202	**	-0.135	*	0.006		0.234	***	-0.062		0.516	***	1.000						
VOLATILITY	-0.048	0.169	**	0.524	***	0.057		0.047		0.265	***	0.066		-0.074		1.000				
ANALYSTFOLLOW	-0.034	0.003		0.424	***	-0.082		-0.016		0.087		-0.068		-0.086		0.277	***	1.000		
LITIGIOUS	0.053	-0.063		-0.106		-0.128	*	0.181	**	-0.045		-0.126		0.230	***	-0.054		0.014		1.000

*, **, *** $p < 0.05$, $p < 0.01$ & $p < 0.001$

all continuous variables are winsorized at 1% and 99% to mitigate against outliers.

TABLE 16 Continued

Panel B: Pearson correlations of test and control variables using the INTENT sample (N=242)

	ORG CHANGE	CFO BIG4	CFO POWER	AC BIG4	OFFICE SIZE	AUDIT FEE	AUDITOR TENURE	AUDITOR CHANGE	CFO CHANGE	CFO EQUITY PAY
ORGCHANGE	1.000									
CFOBIG4	0.123	1.000								
CFOPOWER	-0.173 **	-0.110	1.000							
ACBIG4	0.049	0.001	0.059	1.000						
OFFICESIZE	0.098	-0.024	-0.166 *	0.051	1.000					
BIG4	0.156 *	0.048	-0.176 **	0.081	0.693 ***					
AUDITFEE	0.299 ***	0.144 *	-0.317 ***	0.026	0.453 *	1.000				
AUDITINF	0.008	0.040	0.084	-0.109	-0.720 ***	-0.076				
AUDITORTENURE	0.067	-0.041	-0.189 **	0.024	0.282 ***	0.298 ***	1.000			
AUDITORCHANGE	0.037	0.029	0.023	-0.034	-0.200 **	-0.143 *	-0.576 ***	1.000		
CFOCHANGE	0.052	-0.004	-0.055	-0.002	0.035	0.089	0.042	-0.106	1.000	
CFO_EQUITY_PAY	0.014	-0.005	-0.105	-0.056	0.116	0.222 ***	-0.005	-0.039	0.047	1.000
CEOINDEP	-0.067	-0.115	0.030	-0.102	-0.027	-0.079	-0.010	0.036	0.124	-0.005
IAF	-0.095	0.014	-0.174 **	0.011	0.020	0.152 *	0.027	-0.048	-0.047	0.025
FIRMSIZE	0.071	0.045	-0.288 ***	0.023	0.353 ***	0.604 ***	0.252 ***	-0.105	0.050	0.240 ***
AGE	0.036	-0.081	-0.234 ***	0.019	0.128	0.226 ***	0.317 ***	-0.040	-0.033	0.089
LOSS	0.110	0.044	0.164 *	-0.013	-0.092	-0.165 *	-0.058	0.024	0.013	-0.011
ROA	0.035	0.090	-0.031	0.054	-0.011	0.171 *	-0.023	0.036	0.002	0.040
COMPLEXITY	0.240 ***	0.030	-0.183 **	0.042	0.297 ***	0.489 ***	0.347 ***	-0.071	0.053	0.127
LEVERAGE	-0.061	0.006	-0.130	0.018	-0.137 *	0.056	0.055	-0.089	0.088	-0.029
MTB	0.154 *	0.040	0.029	-0.049	-0.053	-0.082	0.120	-0.058	0.005	-0.087
VOLATILITY	0.005	-0.001	-0.236 ***	-0.051	0.209 **	0.524 ***	0.135 *	-0.062	-0.079	0.169 *
ANALYSTFOLLOW	0.128	0.009	0.025	0.056	0.179 **	0.309 ***	0.126	-0.063	0.052	0.071
LITIGIOUS	0.153 *	-0.106	0.127	-0.054	0.141 *	-0.077	0.066	-0.010	-0.063	-0.007

*, **, *** $p < 0.05$, $p < 0.01$ & $p < 0.001$

all continuous variables are winsorized at 1% and 99% to mitigate against outliers.

TABLE 16 Panel B Continued

	CEO INDEP		IAF		FIRM SIZE		AGE		LOSS		COMP LEXITY		LEVER AGE		MTB		VOL ATILITY		ANALYST FOLLOW		LITIGIOUS
ORGCHANGE																					
CFOBIG4																					
CFOPOWER																					
ACBIG4																					
OFFICESIZE																					
BIG4																					
AUDITFEE																					
AUDITINF																					
AUDITORTENURE																					
AUDITORCHANGE																					
CFOCHANGE																					
CFO_EQUITY_PAY																					
CEOINDEP	1.000																				
IAF	0.086		1.000																		
FIRMSIZE	-0.062		0.208	**	1.000																
AGE	-0.136	*	0.160	*	0.259	***	1.000														
LOSS	-0.008		-0.151	*	-0.390	***	-0.124		1.000												
COMPLEXITY	-0.186	**	0.016		0.265	***	0.270	***	-0.042		1.000										
LEVERAGE	0.089		0.125		-0.056		0.084		0.108		-0.144	*	1.000								
MTB	-0.010		-0.160	*	-0.130		0.017		0.257	***	-0.064		0.510	***	1.000						
VOLATILITY	-0.002		0.152	*	0.503	***	0.152	*	-0.125		0.166	*	0.051		-0.091		1.000				
ANALYSTFOLLOW	-0.006		-0.039		0.433	***	0.001		-0.136	*	0.156	*	-0.106		-0.070		0.303	***	1.000		
LITIGIOUS	0.057		-0.127		-0.016		-0.122		0.224	***	-0.100		-0.119		0.221	***	0.065		0.132	*	1.000

*, **, *** $p < 0.05$, $p < 0.01$ & $p < 0.001$

all continuous variables are winsorized at 1% and 99% to mitigate against outliers.

TABLE 17 Chapter 3 Model Tests

Panel A: Test Results of Logit Regressions for Model 1, 2 and 3 (results reported in log odds, the logit metric)

Dependent Variable	Model 1 ICWeakAMENDED[a]			Model 2 ERROR[b]			Model 3 INTENTvsNORESTATE[c]			Model 3 INTENT[d]		
	Coef.	*z*		Coef.	*z*		Coef.	*z*		Coef.	*z*	
ORGCHANGE	1.67	1.57		0.61	0.87		0.93	1.34		0.15	0.20	
CFOBIG4	0.53	0.65		0.62	1.68	*	-0.18	-0.45		-0.91	-2.16	*
CFOPOWER	1.10	1.66	*	0.60	1.95	*	0.20	0.52		-0.19	-0.61	
*CFOBIG4*ORGCHANGE*	-1.17	-1.00		-1.63	-2.38	**	-0.74	-0.92		0.69	0.98	
*CFOPOWER*ORGCHANGE*	-0.43	-0.46		-0.82	-1.50		-0.26	-0.44		0.43	0.79	
ACBIG4	-0.58	-0.99		0.00	0.01		-0.59	-1.68	*	-0.84	-2.29	*
OFFICESIZE	-0.35	-1.34		0.04	0.39		-0.05	-0.39		-0.05	-0.44	
AUDITFEE	1.14	2.19	*	0.37	1.25		0.06	0.21		-0.13	-0.48	
AUDITORTENURE	-0.12	-0.35		0.02	0.08		0.36	1.67	*	0.34	1.52	
CFO_EQUITY_PAY	-1.35	-1.04		-0.16	-0.25		-2.02	-2.62	**	-1.92	-2.59	**
CEOINDEP	-0.62	-0.89		0.48	1.58		0.14	0.43		-0.81	-2.32	*
FIRMSIZE	-0.85	-2.42	**	-0.09	-0.57		-0.04	-0.19		0.08	0.42	
AGE	0.06	0.12		-0.16	-0.72		-0.35	-1.51		-0.39	-1.81	*
LOSS	1.30	1.52		1.15	3.12	**	0.49	1.24		-0.61	-1.70	*
COMPLEXITY	-0.52	-1.04		0.21	0.74		0.40	1.40		0.01	0.02	
ANALYSTFOLLOW	0.01	0.01		-0.42	-1.25		-0.77	-1.84	*	-0.49	-1.19	
_cons	-20.53	-3.40	***	-5.99	-1.66	*	12.14	3.17	**	16.47	4.28	***
N	218			242			225			225		
Pseudo R2	0.33			0.12			0.18			0.18		
log liklihood	-46.08			-147.12			-127.99			-127.67		
Hosmer-Lemeshow chi2	2.87			9.40			4.61			3.20		
Prob > chi2	0.9422			0.3099			0.7982			0.9209		
Area under ROC curve	0.8929			0.7288			0.7702			0.7719		

***, **, * indicates a statistical difference at respectively $p < 0.001$, $p < 0.01$, and $p < 0.05$ one-tailed with robust standard errors

a. ICWeakAMENDED equal to 1 if auditor SOX 404 report of ineffective control; 0 otherwise in a logit regression using the ERROR sample (see b. Below) except for observations in industries that predict effective control perfectly.

b. ERROR = 1 for the 121 firm-years restated to correct unintentional error(s); 0 for 121 matched firm-years without restatement.

c. INTENTvsNORESTATE = 1 for 104 firm-years restated to correct intentional misstatement; 0 for 121 firm-years without restatement.

d. INTENT = 1 for 104 firm-years restated to correct intentional misstatement; 0 for 121 firm-years restated to correct unintentional error(s).

TABLE 17 Continued

Panel B: Internal Control Associations - Model 1using the ERROR[a] sample

ICWeakAMENDED	Odds Ratio [b]	Robust Std. Err.	z	P>z	90% Confidence Interval		sig	prediction
ORGCHANGE	5.33	5.68	1.57	0.116	0.93	30.73		
CFOBIG4	1.70	1.38	0.65	0.513	0.45	6.47		
CFOPOWER	3.01	2.00	1.66	0.098	1.01	8.98	*	
*CFOBIG4*ORGCHANGE*	0.31	0.36	-1.00	0.318	0.05	2.12		significant *H6*
*CFOPOWER*ORGCHANGE*	0.65	0.60	-0.46	0.644	0.14	2.99		significant *H7*
ACBIG4	0.56	0.33	-0.99	0.324	0.21	1.47		<1 *H9*
OFFICESIZE	0.70	0.18	-1.34	0.182	0.46	1.08		?
AUDITFEE	3.12	1.62	2.19	0.028	1.33	7.32	*	>1
AUDITORTENURE	0.89	0.30	-0.35	0.727	0.51	1.54		?
CFO_EQUITY_PAY	0.26	0.34	-1.04	0.299	0.03	2.20		?
CEOINDEP	0.54	0.38	-0.89	0.373	0.17	1.69		<1
FIRMSIZE	0.43	0.15	-2.42	0.016	0.24	0.76	*	<1
AGE	1.06	0.54	0.12	0.903	0.46	2.47		<1
LOSS	3.68	3.16	1.52	0.129	0.90	15.11		>1
COMPLEXITY	0.60	0.30	-1.04	0.300	0.26	1.35		>1
ANALYSTFOLLOW	1.01	0.88	0.01	0.993	0.24	4.22		?
_cons	1.22E-09	7.34E-09	-3.40	0.001	5.98E-14	2.5E-05		
year fixed effects	yes							
industry fixed effects	yes							
ORGCHANGE **X** *CFOBIG4*ORGCHANGE* **X** *CFOPOWER*ORGCHANGE*					-1.82	3.49		>1 *H1*
CFOBIG4 **X** *CFOBIG4*ORGCHANGE* [b]					-2.46	1.22		<1 *H2*
CFOPOWER **X** *CFOPOWER*ORGCHANGE* [b]					-2.08	1.13		<1 *H4*

N	218
Pseudo R2	0.33
log likelihood	-46.08
Hosmer-Lemeshow chi2	2.87
Prob > chi2	0.9422
Area under ROC curve	0.8929

* indicates one is not included in the 90% confidence interval and that results are significant for one-sided preditions at the 95% level

a. The ERROR sample is comprised of 121 firm-years restated to correct unintentional error (ERROR=1) and 121 firm-years without restatement matched by nearest asset size within the same year and two-digit SIC code (ERROR=0).

b. When results are reported as odds ratios, an association is significantly positive (negative) if the confidence interval for that variable is greater (less) than 1.0 and insignificant if the confidence interval includes 1.0. Interactions expressed as odds ratios operate multiplicatively. Accordingly, the test of the overall significance of a variable with a significant interaction is whether or not the confidence interval of the product of the main and interaction effects contains 1.0.

TABLE 17 Continued

Panel C: Unintentional Error Associations - Model 2 using the ERROR[a] sample

ERROR	Odds Ratio [b]	Robust Std. Err.	z	P>z	90% Confidence Interval		sig	prediction
ORGCHANGE	1.83	1.28	0.87	0.383	0.58	5.77		
CFOBIG4	1.85	0.68	1.68	0.093	1.01	3.39	*	
CFOPOWER	1.82	0.56	1.95	0.051	1.10	3.00	*	
*CFOBIG4*ORGCHANGE*	0.20	0.13	-2.38	0.017	0.06	0.60	*	
*CFOPOWER*ORGCHANGE*	0.44	0.24	-1.50	0.134	0.18	1.08		
ACBIG4	1.00	0.32	0.01	0.996	0.60	1.68		<1 *H10*
OFFICESIZE	1.04	0.11	0.39	0.699	0.88	1.24		<1 *H12*
AUDITFEE	1.45	0.43	1.25	0.210	0.89	2.35		>1
AUDITORTENURE	1.02	0.22	0.08	0.932	0.71	1.45		?
CFO_EQUITY_PAY	0.85	0.54	-0.25	0.800	0.30	2.42		?
CEOINDEP	1.61	0.49	1.58	0.114	0.98	2.64		<1
FIRMSIZE	0.91	0.15	-0.57	0.569	0.70	1.19		<1
AGE	0.85	0.19	-0.72	0.473	0.59	1.23		<1
LOSS	3.14	1.16	3.12	0.002	1.72	5.76	*	>1
COMPLEXITY	1.23	0.34	0.74	0.462	0.78	1.95		>1
ANALYSTFOLLOW	0.66	0.22	-1.25	0.210	0.38	1.14		?
_cons	0.00	0.01	-1.66	0.097	6.67E-06	0.94	*	
year fixed effects	yes							
industry fixed effects	yes							
CFOBIG4 **X** *CFOBIG4*ORGCHANGE* [b]					-2.49	0.47	*	<1 *H3*
CFOPOWER **X** *CFOPOWER*ORGCHANGE* [b]					-1.32	0.34	*	<1 *H5*

N	242
Pseudo R2	0.12
log likelihood	-147.12
Hosmer-Lemeshow chi2	9.40
Prob > chi2	0.3099
Area under ROC curve	0.7288

* indicates one is not included in the 90% confidence interval and that results are significant for one-sided preditions at the 95% level

a. The ERROR sample is comprised of 121 firm-years restated to correct unintentional error and 121 firm-years without restatement matched by nearest asset size within the same year and two-digit SIC code.

b. When results are reported as odds ratios, an association is significantly positive (negative) if the confidence interval for that variable is greater (less) than 1.0 and insignificant if the confidence interval includes 1.0. Interactions expressed as odds ratios operate multiplicatively. Accordingly, the test of the overall significance of a variable with a significant interaction is whether or not the confidence interval of the product of the main and interaction effects contains 1.0.

TABLE 17 Continued

Panel D: Intentional Misstatement Associations - Model 2 using the INTENTvsNORESTATE[a] sample

INENTvsNORESTATE	Odds Ratio [b]	Robust Std. Err.	z	P>z	90% Confidence Interval		sig
ORGCHANGE	2.53	1.74	1.34	0.179	0.81	7.85	
CFOBIG4	0.83	0.34	-0.45	0.655	0.43	1.63	
CFOPOWER	1.22	0.46	0.52	0.600	0.65	2.28	
*CFOBIG4*ORGCHANGE*	0.48	0.38	-0.92	0.356	0.13	1.78	
*CFOPOWER*ORGCHANGE*	0.77	0.45	-0.44	0.658	0.30	2.02	
ACBIG4	0.55	0.19	-1.68	0.093	0.31	0.99	*
OFFICESIZE	0.95	0.12	-0.39	0.700	0.78	1.17	
AUDITFEE	1.07	0.33	0.21	0.835	0.64	1.76	
AUDITORTENURE	1.43	0.31	1.67	0.094	1.01	2.04	*
CFO_EQUITY_PAY	0.13	0.10	-2.62	0.009	0.04	0.47	*
CEOINDEP	1.15	0.38	0.43	0.666	0.67	1.97	
FIRMSIZE	0.96	0.18	-0.19	0.850	0.71	1.32	
AGE	0.70	0.16	-1.51	0.130	0.48	1.03	
LOSS	1.64	0.65	1.24	0.216	0.85	3.15	
COMPLEXITY	1.49	0.42	1.40	0.162	0.93	2.37	
ANALYSTFOLLOW	0.46	0.19	-1.84	0.065	0.23	0.92	*
_cons	1.87E+05	7.18E+05	3.17	0.002	341.60	1.03E+08	
year fixed effects	yes						
industry fixed effects	yes						
N	225						
Pseudo R2	0.18						
log likelihood	-127.99						
Hosmer-Lemeshow chi2	4.61						
Prob > chi2	0.7982						
Area under ROC curve	0.7702						

* indicates one is not included in the 90% confidence interval and that results are significant for one-sided preditions at the 95% level

a. The INTENTvsNORESTATE sample is comprised of 104 firm-years restated to correct intentional misstatement (INTENTvsNORESTATE = 1) and 121 firm-years without restatement INTENTvsNORESTATE = 0)

b. When results are reported as odds ratios, an association is significantly positive (negative) if the confidence interval for that variable is greater (less) than 1.0 and insignificant if the confidence interval includes 1.0. Interactions expressed as odds ratios operate multiplicatively. Accordingly, the test of the overall significance of a variable with a significant interaction is whether or not the confidence interval of the product of the main and interaction effects contains 1.0.

TABLE 17 Continued

Panel E: Comparison of Restatement Associations - Model 3 using the INTENT[a] sample

INTENT	Odds Ratio [c]	Robust Std. Err.	z	P>z	90% Confidence Interval		sig	prediction
ORGCHANGE	1.17	0.89	0.20	0.840	0.33	4.10		
CFOBIG4	0.40	0.17	-2.16	0.030	0.20	0.80	*	
CFOPOWER	0.82	0.26	-0.61	0.540	0.49	1.39		*>H8* [b]
*CFOBIG4*ORGCHANGE*	2.00	1.42	0.98	0.329	0.62	6.44		
*CFOPOWER*ORGCHANGE*	1.53	0.83	0.79	0.430	0.63	3.74		
ACBIG4	0.43	0.16	-2.29	0.022	0.24	0.79	*	*< H11* [b]
OFFICESIZE	0.96	0.10	-0.44	0.661	0.80	1.13		*< H13* [b]
AUDITFEE	0.88	0.24	-0.48	0.634	0.56	1.37		
AUDITORTENURE	1.40	0.31	1.52	0.127	0.97	2.02		
CFO_EQUITY_PAY	0.15	0.11	-2.59	0.010	0.04	0.50	*	
CEOINDEP	0.44	0.16	-2.32	0.020	0.25	0.79	*	
FIRMSIZE	1.08	0.20	0.42	0.674	0.80	1.47		
AGE	0.68	0.15	-1.81	0.071	0.47	0.97	*	
LOSS	0.54	0.20	-1.70	0.090	0.30	0.98	*	
COMPLEXITY	1.01	0.27	0.02	0.984	0.65	1.56		
ANALYSTFOLLOW	0.61	0.25	-1.19	0.234	0.31	1.21		
_cons	1.42E+07	5.47E+07	4.28	0.000	2.54E+04	7.95E+09		
year fixed effects	yes							
industry fixed effects	yes							
N	225							
Pseudo R2	0.18							
log likelihood	-127.67							
Hosmer-Lemeshow chi2	3.20							
Prob > chi2	0.9209							
Area under ROC curve	0.7719							

***, **, * indicates a statistical difference at respectively $p<0.01$, $p<0.05$, and $p<0.1$ one-tailed

a. The INTENT sample is comprised of 104 firm-years restated to correct intentional misstatement (INTENT=1) and 121 firm-years restated to correct unintentional error (INTENT=0).

b. The direction of the prediction is predicated on the assumption that restatement likelihood is decreasing in the test variable and that any significant association between the test variable is in the same direction for both types of restatement.

c. When results are reported as odds ratios, an association is significantly positive (negative) if the confidence interval for that variable is greater (less) than 1.0 and insignificant if the confidence interval includes 1.0. Interactions expressed as odds ratios operate multiplicatively. Accordingly, the test of the overall significance of a variable with a significant interaction is whether or not the confidence interval of the product of the main and interaction effects contains 1.0.

TABLE 17 Continued

Panel F: Model 1, 2 & 3 tests with all control variables (results reported in log odds, the logit metric)

	Model 1			Model 2						Model 3		
Dependent Variable	ICWeakAMENDED[a]			ERROR[b]			INTENTvsNORESTATE[c]			INTENT[d]		
	coef.	*z*		coef.	*z*		coef.	*z*		coef.	*z*	
ORGCHANGE	1.63	0.96		0.62	0.85		0.94	1.32		0.53	0.69	
CFOBIG4	0.51	0.58		0.56	1.48		-0.30	-0.70		-0.96	-2.24	*
CFOPOWER	0.98	1.07		0.58	1.85	*	0.25	0.59		-0.23	-0.65	
*CFOBIG4 *ORGCHANGE*	-1.08	-0.92		-1.64	-2.34	**	-0.66	-0.76		0.75	1.05	
*CFOPOWER *ORGCHANGE*	-0.40	-0.28		-0.81	-1.41		-0.31	-0.47		0.72	1.18	
ACBIG4	-0.39	-0.59		-0.02	-0.06		-0.47	-1.23		-0.90	-2.45	**
OFFICESIZE	-0.28	-1.07		0.06	0.51		-0.01	-0.06		-0.02	-0.16	
AUDITFEE	0.96	1.73	*	0.45	1.42		0.03	0.08		-0.28	-0.94	
AUDITORTENURE	-0.21	-0.54		-0.12	-0.50		0.53	1.93	*	0.68	2.39	**
AUDITORCHANGE	0.05	0.03		-0.57	-0.62		0.78	0.92		1.24	1.39	
CFOCHANGE	0.66	0.86		0.16	0.33		0.33	0.63		-0.12	-0.25	
CFO_EQUITY_PAY	-1.20	-0.95		-0.14	-0.21		-2.29	-2.87	**	-1.96	-2.60	**
CEOINDEP	-0.62	-0.72		0.49	1.53		0.03	0.08		-0.80	-2.17	*
IAF	-0.41	-0.43		0.10	0.23		0.21	0.45		0.07	0.14	
FIRMSIZE	-0.87	-1.72	*	-0.05	-0.28		-0.21	-0.90		0.01	0.05	
AGE	0.06	0.12		-0.18	-0.80		-0.36	-1.42		-0.45	-1.90	*
LOSS	0.88	1.17		1.21	3.10	**	0.45	1.04		-0.48	-1.28	
COMPLEXITY	-0.40	-0.72		0.23	0.81		0.31	1.01		-0.19	-0.66	
LEVERAGE	-1.30	-0.52		-0.18	-0.22		-1.23	-1.34		-0.72	-0.79	
MTB	0.02	1.19		0.01	0.82		0.01	1.42		0.00	-0.24	
VOLATILITY	0.00	0.70		0.00	-0.88		0.00	1.70	*	0.00	2.34	*
ANALYSTFOLLOW	0.00	0.00		-0.42	-1.24		-0.77	-1.82	*	-0.58	-1.33	
LITIGIOUS	0.67	0.64		-0.39	-0.75		-0.94	-1.79	*	-0.70	-1.14	
_cons	-18.60	-2.58	**	-7.01	-1.86	*	14.51	3.62	***	16.81	3.96	***
year fixed effects	yes			yes			yes			yes		
industry fixed effects	yes			yes			yes			yes		
N	218			242			225			225		
Wald chi2	626.39			41.07			270.79			210.31		
Prob > Wald chi2	<0.0001			0.56			<0.0001			<0.0001		
Pseudo R2	0.35			0.13			0.20			0.20		
log likelihood	-44.86			-145.61			-123.72			-123.56		
Hosmer-Lemeshow chi2	3.56			16.48			3.89			14.62		
Prob > HL chi2	0.8943			0.0360			0.8672			0.0669		
Area under ROC curve	0.8939			0.7329			0.7864			0.7889		

***, **, * indicates a statitical difference at respectively p<0.001, p< 0.01, and p<0.05 one-tailed with robust std. errors

a. ICWeakAMENDED equal to 1 if auditor SOX 404 report of ineffective control; 0 otherwise in a logit regression using the ERROR sample (see b. Below) except for observations in industries that predict effective control perfectly.

b. ERROR = 1 for the 121 firm-years restated to correct unintentional error(s); 0 for 121 matched firm-years without restatement.

c. INTENTvsNORESTATE = 1 for 104 firm-years restated to correct intentional misstatement; 0 for 121 firm-years without restatement.

d. INTENT = 1 for 104 firm-years restated to correct intentional misstatement; 0 for 121 firm-years restated to correct unintentional error(s).

TABLE 18 Internal Control Mediation

Panel A: Model 2 and 3 results when the models are modified to include internal control quality[a]

Dependent Variable	Model 2 ERROR[b]			INTENTvsNORESTATE[c]			Model 3 INTENT[d]		
	Coef.	*z*		Coef.	*z*		Coef.	*z*	
ICWeakAMENDED	3.25	2.94	***	4.57	4.41	***	1.06	2.39	***
ORGCHANGE	0.52	0.74		0.58	0.79		0.03	0.04	
CFOBIG4	0.62	1.64	*	-0.38	-0.85		-0.93	-2.30	**
CFOPOWER	0.52	1.66	**	0.19	0.45		-0.22	-0.69	
CFOBIG4_ORGCHANGE	-1.72	-2.39	***	-0.94	-1.08		0.63	0.89	
CFOPOWER_ORGCHANGE	-0.78	-1.45	*	-0.34	-0.58		0.42	0.76	
ACBIG4	0.03	0.10		-0.43	-1.06		-0.81	-2.21	**
OFFICESIZE	0.09	0.77		-0.05	-0.36		-0.02	-0.18	
AUDITFEE	0.26	0.85		0.14	0.40		-0.18	-0.65	
AUDITORTENURE	0.05	0.23		0.52	1.98	**	0.32	1.38	*
CFO_EQUITY_PAY	-0.05	-0.07		-2.24	-2.47	***	-1.93	-2.64	***
CEOINDEP	0.60	1.87	**	-0.14	-0.37		-0.78	-2.28	**
FIRMSIZE	0.02	0.10		-0.05	-0.27		0.16	0.86	
AGE	-0.14	-0.60		-0.32	-1.18		-0.37	-1.67	**
LOSS	1.13	2.98	***	0.37	0.81		-0.64	-1.71	**
COMPLEXITY	0.23	0.82		0.49	1.50	*	0.04	0.14	
ANALYSTFOLLOW	-0.41	-1.17		-0.33	-0.70		-0.43	-1.03	
_cons	-6.00	-1.63	*	6.95	1.42	*	14.93	3.71	***
N	242			225			225		
Pseudo R2	0.17			0.32			0.20		
log liklihood	-138.41			-106.13			-124.23		
Hosmer-Lemeshow chi2	2.78			8.82			3.04		
Prob > chi2	0.9476			0.3573			0.9318		
Area under ROC curve	0.7609			0.8374			0.7847		

***, **, * indicates a statistical difference at respectively $p < 0.01$, $p < 0.05$, and $p < 0.1$ one-tailed with robust standard errors

a. ICWeakAMENDED equal to 1 if auditor SOX 404 report of ineffective control; 0 otherwise
b. ERROR = 1 for the 121 firm-years restated to correct unintentional error(s); 0 for 121 matched firm-years without restatement.
c. INTENTvsNORESTATE = 1 for 104 firm-years restated to correct intentional misstatement; 0 for 121 firm-years without restatement.
d. INTENT = 1 for 104 firm-years restated to correct intentional misstatement; 0 for 121 firm-years restated to correct unintentional error(s).

TABLE 18 Continued

Panel B: Proportion of audit committee financial expertise effect mediated by internal control quality

DV	ICWeakAMENDED				INTENT				INTENT			
	Coef.	z	P>|z|		Coef.	z	P>|z|		Coef.	z	P>|z|	
ICWeakAMENDED									1.07	2.60	0.009	***
ACBIG4	-0.57	-1.42	0.156		-0.79	-2.31	0.021	**	-0.77	-2.21	0.027	**
ORGCHANGE	0.61	1.27	0.205		0.08	0.18	0.856		-0.06	-0.15	0.881	
CFOBIG4	0.14	0.36	0.719		-0.69	-2.07	0.038	**	-0.74	-2.16	0.031	**
CFOPOWER	0.16	0.46	0.649		-0.08	-0.30	0.762		-0.12	-0.43	0.671	
OFFICESIZE	-0.09	-0.82	0.412		-0.05	-0.46	0.644		-0.02	-0.21	0.837	
AUDITFEE	0.19	0.59	0.555		-0.12	-0.44	0.658		-0.17	-0.61	0.543	
AUDITORTENURE	0.22	0.87	0.385		0.33	1.45	0.147		0.31	1.35	0.178	
CFO_EQUITY_PAY	-0.48	-0.51	0.612		-1.87	-2.43	0.015	**	-1.87	-2.40	0.016	**
CEOINDEP	-0.36	-0.92	0.360		-0.81	-2.36	0.018	**	-0.79	-2.27	0.023	**
FIRMSIZE	-0.46	-2.03	0.042	**	0.06	0.30	0.765		0.14	0.72	0.469	
AGE	-0.26	-1.01	0.311		-0.35	-1.58	0.115		-0.33	-1.46	0.144	
LOSS	0.09	0.22	0.827		-0.61	-1.61	0.107		-0.64	-1.66	0.097	*
COMPLEXITY	-0.26	-0.82	0.412		-0.01	-0.03	0.973		0.03	0.10	0.923	
ANALYSTFOLLOW	-0.53	-1.19	0.234		-0.48	-1.22	0.222		-0.41	-1.05	0.295	
_cons	1.65	0.41	0.680		4.75356	1.37	0.171		4.2294	1.18	0.238	
year fixed effects	yes				yes				yes			
industry fixed effects	yes				yes				yes			
N	215				224				224			
LR chi2	41.640				52.420				59.500			
Prob > chi2	0.118				0.023				0.006			
Pseudo R2	0.173				0.170				0.193			
Log likelihood	-99.251				-128.329				-124.793			

***, **, * indicates a statistical difference at respectively $p<0.01$, $p<0.05$, and $p<0.1$

total indirect effect = -.0336326
direct effect = -.1941759
total effect = -.22780851
c_path = -.21475045
proportion of total effect mediated = 0.148
ratio of indirect to direct effect = 0.173
ratio of total to direct effect = 1.173

Appendix A

Words and Phrases Used in Text Searches of Restatement Announcements

AuditAnlytics[44] manually codes less than 5% of restatement announcements in its database as "errors – accounting and clerical applications" (i.e., 515 of 10,623 restatements announced in years after 2002, the year of *Sarbanes-Oxley Act* (SOX) enactment). I used a sample comprised of 361 of these restatement announcements (those with the restatement period ending after Dec 1, 2004 at a time when SOX 404 is in effect) to develop a list of words and phrases with which to construct a language-based proxy for restatements that correct unintentional error. First I sorted these 361 restatements alphabetically. Next, I read these restatements looking for the words and phrases that indicate lack of misstatement intent. For example, I added the word "inadvertent" to the list after reading Airtran Holdings Inc.'s August, 2006 announcement that it was amending its 10-K to "correct inadvertent errors in accounting for fuel expense". Similarly, I added the word "duplicate" to the list after reading Energy Source's November 2007 announcement that it was restating its 2005 and 2006 annual financial statements "to reverse the duplicate recognition of the liabilities as both accounts payable and long term debt". As I progressed through the sample selected for list creation, I found fewer and fewer new words and phrases to add to the list. After reading the first 180 restatement announcements in the sample, I chose to limit reading of the remaining 181 restatement announcements to the 48 restatements not containing words already on the list.

After making each list addition based on words and phrases in the *AuditAnalytics*-error-coded-restatement announcements, I supplemented the list with synonyms and variants of the listed words and phrases. I conducted quick searches using the *AuditAnalytics* text search tool to confirm any synonyms and variants I suspected may be used in restatement announcements to indicate unintentional error are

[44] Scholtz (2008) in a report on restatements between 1997 and 2006 prepared for the US Treasury, finds *Audit Analytics* includes "nearly all restatements captured in the GAO lists and Lexis-Nexis searches, and some that are not identified through these methods".

contained in one or more restatement announcements in the *AuditAnalytics* database. For example, I added to the list two phrases (i.e., "recorded twice" and "double recorded") that I reasoned have similar meanings to "duplicate" in the context of financial restatements after finding these two phrases are each used in several restatement announcements.

The final list of words and phrases is presented at the end of this Appendix. To format the list for use with the *AuditAnalytics* text search tool, phrases are enclosed in quotation marks, the Boolean operator "OR" is added, and the number of words/phrases displayed per line is limited to fit in the search tool. For presentation purposes, I grouped words and phrases searches into nine categories of errors (calculation, unintentional errors (explicitly stated), information system, immaterial, clerical, administrative, reconciliation, recording, and classification). The words and phrases in all groups except the classification group are used in text searches to construct the proxy for restatements that correct unintentional error validated in the paper. I found one or more of these words and phrases in 2,048 post-2002 restatements announcements when I searched using *AuditAnalytics* text search tool. The words and phrases in the classification group are used to test the robustness of validation tests when using a broader search for restatements that correct unintentional error. The words and phrases in both the recording group and the classification group are ignored in a narrower search for error correcting restatements in other robustness tests of validation test results.

The list of words and phrases also shows the proportion of restatement announcements that contain the words/phrases on each line of the list and in each group of (i) the 10,623 post-2002 restatements; (ii) the 2,048 restatements identified as correcting unintentional error; and (iii) the 515 post-2002 restatements coded as "Errors – accounting and clerical applications" by *AuditAnalytics*. It is noteworthy that text searches identify only 76.3% (77.7%) of restatements coded by *AuditAnalytics* as "Errors – accounting and clerical applications" when classification error indicators are excluded

(included). There are several possible reasons for this discrepancy between *AuditAnalytics*' manual coding and the automated text search results. Firstly, I was unable to find appropriate words or phrases that would have identified some of the *AuditAnalytics* error-coded restatements. For example, *AuditAnalytics* codes Gran Tierra Energy Inc.'s February, 2010 announcement as an error. However, I did not determine in the section of the restatement announcement that describes the error (i.e., in the text "determining the valuation of the warrant modification was inconsistent with the inputs used in the Black-Scholes option pricing model to determine the fair value of the Company's stock based compensation awards" an appropriate word or phrase that could be used in automated text searches to identify unintentional error. Secondly, as described above, I did not read 133 (181-48) of the restatements coded by *AuditAnalytics* as errors and it is possible words and phrases that would be useful for the identification of unintentional error are not included in the list. Words such as inadvertent, unintentional, and mistake that explicitly state restatements correct unintentional error are contained in 17.4% (18.6%) of restatements identified by text searches of all 10,623 post-2002 restatements (the 515 *AuditAnalytics* coded post-2002 restatements). Words and phrases that suggest restatements correct calculation errors correct are contained in 28.3% (25.8%) of all 10,623 post-2002 restatements (the 515 *AuditAnalytics* coded post-2002 restatements). The proportions of restatements identified by text searches of all 10,623 post-2002 restatements and of the 515 *AuditAnalytics* coded post-2002 restatements are similar except for the clerical error correcting group (respectively, 9.6% and 21.6%) and the recording error correcting group (respectively, 38.6% and 14.8%).

Appendix A continued

Words and phrases used in text searches of restatement announcements

Restatements announced post-2002	in the AuditAnalytics database at time of November, 2012 search (N=10,623)			coded by *AuditAnalytics* as error-correcting (N=515)	
	n	% of 10,623 restatements announced since 2002	% of 2,048 text search identified error-correcting restatements	n	% of 515 *AuditAnalytics* coded
Restatements that correct unintentional error as identified by text searches ***(excluding restatements with only classification errors)***	**2048**	**19.3%**	**100.0%**	**393**	**76.3%**
calculation group	**579**	**5.5%**	**28.3%**	**133**	**25.8%**
"error in the calculation" OR "errors in the calculation" OR "errors in the calculations"	141	1.3%	6.9%	28	5.4%
"incorrectly calculated"	65	0.6%	3.2%	12	2.3%
"calculation error" OR "calculation errors"	60	0.6%	2.9%	17	3.3%
miscalculat*	53	0.5%	2.6%	7	1.4%
"computational error" OR "computational errors"	47	0.4%	2.3%	26	5.0%
"mathematical error" OR "mathematical errors"	46	0.4%	2.2%	22	4.3%
"recalculating" OR "recalculation"	39	0.4%	1.9%	3	0.6%
"error in calculating" OR "errors in calculating"	35	0.3%	1.7%	1	0.2%
"incorrect calculation" OR "incorrect calculations"	26	0.2%	1.3%	3	0.6%
"properly calculate" OR "properly calculated"	25	0.2%	1.2%	3	0.6%
"error in the computation" OR "errors in the computation" OR "errors in the computations"	24	0.2%	1.2%	7	1.4%
"did not properly calculate" OR "improperly calculated"	15	0.1%	0.7%	1	0.2%
numerical OR arithmetical	11	0.1%	0.5%	6	1.2%
"error in calculation" OR "errors in calculation" OR "errors in calculations"	9	0.1%	0.4%	1	0.2%
"incorrectly computed"	9	0.1%	0.4%	1	0.2%
"mechanical error" OR "mechanical errors"	8	0.1%	0.4%	2	0.4%
"error in computing" OR "errors in computing"	7	0.1%	0.3%	0	0.0%
"footing error" OR "footing errors" OR "summation error" OR "summation errors"	6	0.1%	0.3%	6	1.2%
arithmetic	6	0.1%	0.3%	3	0.6%
"error in its calculation" OR "errors in its calculations" OR "errors in its calculations"	6	0.1%	0.3%	3	0.6%
"inaccurate calculation" OR "inaccurate calculations"	5	0.0%	0.2%	1	0.2%
"error in its computation" OR "errors in its computation" OR "errors in its computations"	3	0.0%	0.1%	0	0.0%
"misstatement in computing" OR "misstatements in computing"	2	0.0%	0.1%	2	0.4%
unintentional error (explicitly stated) group	**357**	**3.4%**	**17.4%**	**96**	**18.6%**
"inadvertent" OR "inadvertently"	264	2.5%	12.9%	71	13.8%
"mistake" OR "mistakenly" OR "mistakes"	74	0.7%	3.6%	19	3.7%
"unintentional" OR "unintentionally"	47	0.4%	2.3%	9	1.7%
information system group	**203**	**1.9%**	**9.9%**	**69**	**13.4%**
"accounting system" OR "accounting systems"	40	0.4%	2.0%	9	1.7%
"spreadsheet" OR "spread-sheet" OR "spread sheet" OR "spreadsheets" OR "spread-sheets"	30	0.3%	1.5%	10	1.9%
"bookkeeping" OR "book-keeping"	30	0.3%	1.5%	12	2.3%
"information system" OR "information systems"	23	0.2%	1.1%	6	1.2%
"software system" OR "installed software" OR "accounting software"	18	0.2%	0.9%	8	1.6%
legacy	17	0.2%	0.8%	1	0.2%
"new accouting system" OR "new software" OR "new system" OR "new systems"	15	0.1%	0.7%	5	1.0%
"ERP" OR "Enterprise Resource Planning" OR "enterprise resource planning"	15	0.1%	0.7%	5	1.0%
"software error" OR "software errors" OR "logic error" OR "logic errors" OR "programmatic error"	8	0.1%	0.4%	7	1.4%
"accounting software"	8	0.1%	0.4%	2	0.4%
"conversion error" OR "conversion errors" OR "error in the conversion" OR "errors in the conversion"	7	0.1%	0.3%	3	0.6%
"software program" OR "software version"	6	0.1%	0.3%	5	1.0%
"system interface failure" OR "interface"	6	0.1%	0.3%	1	0.2%
"programming error" OR "programming errors" OR "program change" OR "program change"	3	0.0%	0.1%	1	0.2%
"automated system" OR "automated systems"	2	0.0%	0.1%	1	0.2%

Appendix A continued

Words and phrases used in text searches of restatement announcements

Restatements announced post-2002	in the AuditAnalytics database at time of November, 2012 search (N=10,623)			coded by *AuditAnalytics* as error-correcting (N=515)	
	n	% of 10,623 restatements announced since 2002	% of 2,048 text search identified error-correcting restatements	n	% of 515 *AuditAnalytics* coded
clerical group	**196**	**1.8%**	**9.6%**	**111**	**21.6%**
"clerical error" OR "clerical errors"	117	1.1%	5.7%	88	17.1%
"typographic error" OR "typographical error" OR "typographic errors" OR "typographical errors"	54	0.5%	2.6%	10	1.9%
"transposed" OR "transposition" OR "transpositions"	8	0.1%	0.4%	4	0.8%
"rounding error" OR "rounding errors"	6	0.1%	0.3%	3	0.6%
transcription*	4	0.0%	0.2%	2	0.4%
"filing error" OR "filing errors"	4	0.0%	0.2%	1	0.2%
"clerical accounting error" OR "clerical accounting errors" or "clerical in nature"	3	0.0%	0.1%	3	0.6%
reconciliation group	**51**	**0.5%**	**2.5%**	**9**	**1.7%**
"account reconciliation" OR "account reconciliations"	38	0.4%	1.9%	3	0.6%
"reconciliation error" OR "reconciliation errors"	10	0.1%	0.5%	3	0.6%
"error in its reconciliation" OR "errors in its reconciliation" OR "errors in its reconciliations"	4	0.0%	0.2%	3	0.6%
"error in the reconciliation" OR "errors in the reconciliation" OR "errors in the reconciliations"	3	0.0%	0.1%	1	0.2%
administrative group	**18**	**0.2%**	**0.9%**	**4**	**0.8%**
"administrative error" OR "administrative errors"	15	0.1%	0.7%	3	0.6%
"administrative oversight"	3	0.0%	0.1%	1	0.2%
immaterial group	**202**	**1.9%**	**9.9%**	**14**	**2.7%**
"immaterial error" OR "immaterial errors"	182	1.7%	8.9%	10	1.9%
"immaterial correction of an error" OR "immaterial correction of errors"	11	0.1%	0.5%	1	0.2%
"miscellaneous corrections"	8	0.1%	0.4%	2	0.4%
"not deemed material"	6	0.1%	0.3%	1	0.2%
recording group	**790**	**7.4%**	**38.6%**	**76**	**14.8%**
"not recorded"	202	1.9%	9.9%	7	1.4%
"incorrectly recorded"	138	1.3%	6.7%	10	1.9%
"did not record"	104	1.0%	5.1%	7	1.4%
"failed to include" OR "failed to record"	70	0.7%	3.4%	4	0.8%
"improperly recorded"	68	0.6%	3.3%	6	1.2%
"error in recording" OR "inaccurate recording" OR "errors in recording"	58	0.5%	2.8%	11	2.1%
"not previously recorded"	52	0.5%	2.5%	2	0.4%
"cut-off" OR "cutoff"	52	0.5%	2.5%	4	0.8%
"posted" OR "posting" OR "postings"	45	0.4%	2.2%	11	2.1%
"failure to record"	25	0.2%	1.2%	2	0.4%
"recorded twice" OR "double recorded" OR "duplicate"	24	0.2%	1.2%	9	1.7%
"converting data" OR "coding" OR "data entry" OR "data input" OR "data inputs"	20	0.2%	1.0%	14	2.7%
"recorded incorrectly"	19	0.2%	0.9%	2	0.4%
"but not recorded"	10	0.1%	0.5%	1	0.2%
"recording error" OR "recording errors"	8	0.1%	0.4%	1	0.2%
"mis-posted" OR "misposting" OR "mispostings" OR "mis-postings"	4	0.0%	0.2%	1	0.2%
classification group (used only in Chapter 2 robustness tests)					
classifi (in restatements where text searches detect other errors)	**266**	**2.5%**	**13.0%**	**33**	**6.4%**
classifi (in restatements where text searches do not detect other errors)	**1316**	**12.4%**	**n/a**	**7**	**1.4%**

Appendix B

Restatement Classification and Determination of Restated Years Used in Statistical Tests

Definitions:

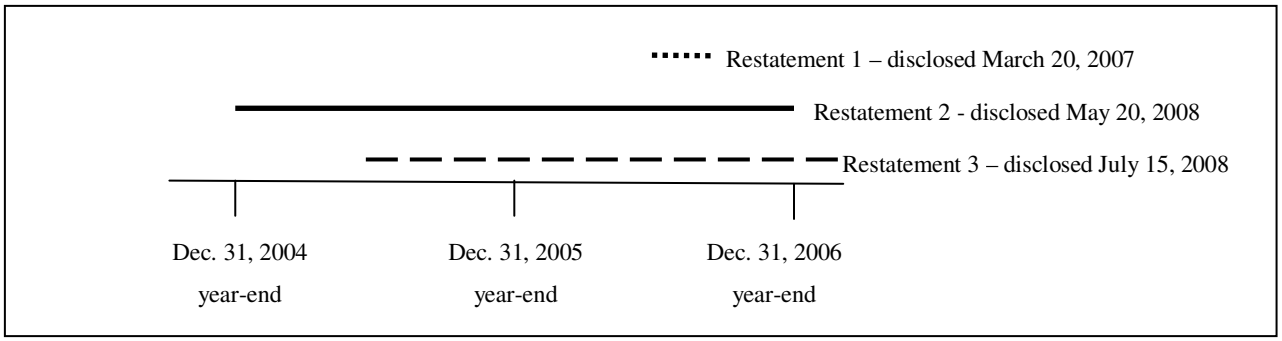
Restatement year: the earliest fiscal year in the restated period. Only restatement year data is used in statistical tests.

Intentional Misstatement: a restatement is classified as correcting an intentional misstatement if the restated period overlaps the period discussed in an AAER that alleges fraud or intentional misstatement.

Unintentional Error: a restatement is classified as correcting an unintentional error if an automated search of the restatement announcement in *AuditAnalytics* identifies a word or phrase that suggests lack of intent. Appendix A lists the words and phrases used in the automated searches and describes the method used to construct the list.

Classification when companies have multiple restatements:

The sample contains at most one restatement per firm-year per company. The following example shows how restatements are classified for a company with three restatements with overlapping restatement periods.



Restatement 1: *restated period July 1 – Sept. 30, 2006 (restatement disclosed Mar. 20, 2007)*

- Since this restatement is a quarterly restatement (does not include a fiscal year-end) it is not included in any sample. Only annual restatements are used in validation tests.

Restatement 2: *restated period Jan. 1, 2005 – Dec. 31, 2006 (restatement disclosed May 20, 2008)*

and Restatement 3: *restated period June 1, 2005 – Mar. 1, 2007 (restatement disclosed July 15, 2008)*

- When a company announces restatement of the same annual financial statement in more than one restatement disclosure (e.g. 2005 is restated by both Restatement 2 and Restatement 3), the restatement correcting any intentional misstatement is included in the sample
- The restatement that corrects intentional misstatement with the restatement period with the earliest "begin date" is chosen if there is more than one misstatement correcting restatements announced for the same firm-year.
-If all the restatements correct unintentional error in the same annual financial statements, the restatement with the earliest "begin date" is included in the sample.
- If two restatements considered for sample inclusion have the same earliest "begin date", the restatement announced first is selected.
- In the example depicted above, Restatement 3 would be used in validation tests if it corrects an intentional misstatement whereas Restatement 2 would be used if both Restatement 2 and 3 correct unintentional error since Restatement 2's restated period begins earlier.
- The 2006 firm-year would not be included in the sample used in validation tests. When the restated period extends past a single fiscal year, only the earliest year is included the sample for statistical tests.

Appendix C

Accrual Measures

The accrual measures are described briefly in Table 2, however this Appendix provides a more complete description.

Accruals: The total accrual measure (*Accruals)* is income before extraordinary items (total of four quarters of *ibqr*) less operating cash flow (*oancfqr*), scaled by average assets (*atqr*).

WC_acc: The working capital accrual metric (*WC_acc)* is the change in working capital items (as defined in Table 2) scaled by average assets.

pmatch: To compute Kothari et al.'s (2005) performance matched residual (*pmatch*), I first estimated cross-sectional coefficients of the modified Jones model for each year for the same two-digit SIC code, using all observations available in the Unrestated COMPUSTAT database (with a minimum of 10 observations):

$$Accruals_{it} = \alpha + \beta(1/AT_{it-1}) + \gamma(\Delta Sales_{it} - \Delta Rec_{it})/AT_{it-1} + \rho PPE_{it}/AT_{it-1} + \varepsilon \qquad (1)$$

Where *Accruals* are defined as above*, AT* is total assets (*atqr*)*, ΔSales* is the change in sales (total of four quarters of *saleqr*) from year *t-1* to *t, ΔRec* is the change in accounts receivable (*recchqr*) from year *t-1* to *t,* and *PPE (ppentqr)* is property plant and equipment. Discretionary accruals are the residuals from equation (1) computed using the cross-sectionally computed coefficients. A firm-year's performance matched discretionary accrual (*pmatch*) is the difference between its discretionary accrual and the discretionary accrual of the firm-year from the same two-digit SIC code and year with the closest current year return on assets (total of four quarters income before extraordinary items (*ibqr)* scaled by total assets (*atqr)* for year *t-1*).

sresid: While the performance matched discretionary accrual measure (*pmatch*) is used frequently in financial reporting quality research, Jones et al. (2008, 500) find that "only the accrual estimation errors estimated from cross-sectional models of working capital changes on past, present, and future cash flows… have explanatory power for fraud beyond total accruals". Accordingly, Table 8 includes a second discretionary accrual measure, the studentized residuals (*sresid*) from the Dechow and Dichev (2002) model, estimated cross-sectionally for the same two-digit SIC code, using all observations available in the Unrestated COMPUSTAT database (except for industries with five or fewer observations). The model regresses the change in working capital (*WC_acc* as defined in Table 2) on cash flow from operations (*oancfqr*) for year *t*, *t-1* and *t+1,* where all variables are deflated by average assets. The *sresid* metric is the residual, scaled by the standard error from the industry-level regression (Dechow et al. 2011, 39) using Stata's *predict* function with the *rstudent* option after regression. Dechow et al. (2011) find both the *pmatch* and *sresid* measures are significantly different for misstating (identified using AAERs) and COMPUSTAT firm-years in the 1979 – 2002 time period.

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