

**THREE ESSAYS ON CHARACTERISTICS AND ECONOMIC CONSEQUENCES OF
FINANCIAL STATEMENT RESTATEMENTS**

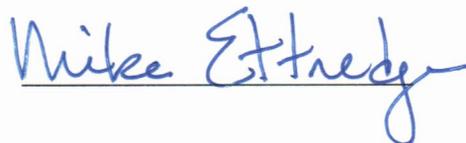
By
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Ying Huang

Submitted to the School of Business and the
Graduate Faculty of the University of Kansas
in partial fulfillment of the requirements
for the degree of Doctor of Philosophy

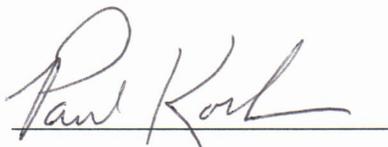
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Dissertation Co-chair:



Committee Members:







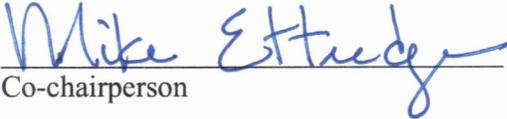
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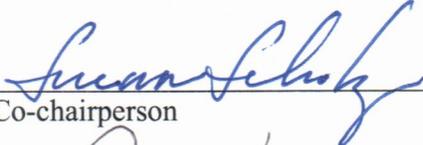
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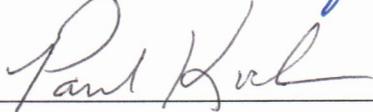
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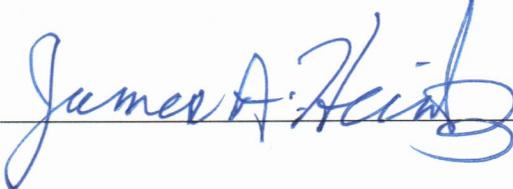
By
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Ying Huang

Committee:


Co-chairperson


Co-chairperson







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ACKNOWLEDGEMENTS

I would like to thank my dissertation committee for all of their valuable feedback, comments and suggestions. I am deeply grateful to my advisors Dr. Susan Scholz and Dr. Michael Ettredge, who guided me through this dissertation process. I am inspired by your enthusiasm, perseverance and dedication towards research. Thank you for always reading and marking up my research papers word-by-word. Thank you for numerous intellectually challenging questions you asked me and enlightening conversations in your offices. Without your encouragement and support throughout my PhD program, I could not have achieved this endeavor.

Very special thanks go to Dr. James Heintz for his wisdom, guidance, tremendous help and support in many ways during my graduate career. Your optimism and encouragement provided me the courage and energy to accomplish this dissertation and my PhD program. I will never forget the happiness shown on your face every time when I made progress. I want to thank you more than I can express.

I would like to extend my thanks to Dr. Paul Koch and Dr. Ted Juhl, who taught me how to think and how to analyze and ‘torture the data until they tell’. Thank you for teaching me two of the most useful and beneficial courses I have taken at KU.

Finally, I want to thank my family: my husband Wei, my parents Jun and Cheng, and my mother-in-law Liyu. This dissertation is also written in dedication to their immeasurable support, unwavering love and belief in me for many years.

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THREE ESSAYS ON CHARACTERISTICS AND ECONOMIC CONSEQUENCES OF FINANCIAL STATEMENT RESTATEMENTS

INTRODUCTION

Restatements are made to correct material errors in companies' financial statements previously filed with SEC. Companies try to avoid restatements because a restatement itself would raise the question of whether the management has been playing the "Numbers Game". My dissertation is motivated by the fact that there was a sharp increase in the frequency of financial restatements over the past decade. Investors, auditors and regulators are seriously concerned about restatements.

My dissertation, entitled "Three Essays on Characteristics and Economic Consequences of Financial Statement Restatements", examines restatements from three different perspectives. Study one examines accounting conservatism following restatement disclosures. This paper provides evidence that companies increased earnings conservatism following restatements. In addition, the increased conservatism is more evident among companies that make improvements in their corporate governance.

Study two investigates the changes in market reactions to restatement announcements. This study explores why the average market reaction to restatement announcements has decreased by 85 percent since year of 2001. It documents that the dampened market reaction is mainly associated with a change in the characteristics of errors companies correct, and also with who initiated the restatements and how complete the disclosure is.

Study three examines restatements from an auditing perspective. This paper examines auditor resignations from restating companies' engagements. It investigates what types of restatements are more likely to trigger resignations and how auditor-client relationship impacts resignation decisions.

STUDY ONE: RESTATEMENT DISCLOSURES AND SUBSEQUENT ACCOUNTING CONSERVATISM

ABSTRACT

This paper examines whether companies increase the conservatism of their reported income numbers following corrections of previously overstated earnings. We expect such a change in income recognition policy as managers attempt to restore earnings credibility. Similar to Basu (1997) and Watts (2003), we define accounting conservatism as recognizing bad news (economic losses) in a more timely fashion than recognizing good news (economic gains). We employ a sample of 2,132 restatements during the period 1999-2005. The results of our analyses are highly consistent across several combinations of conservatism measurement metrics, control sample strategies, and other robustness checks. In all cases we find the hypothesized increase in conservatism. We also find that increased conservatism is more evident among overstating companies that made improvements in their corporate governance.

Key Words: earnings restatement, accounting conservatism, corporate governance, income recognition

1.1 Introduction

This paper examines changes in companies' income recognition policies to increase credibility of earnings following corrections of previously overstated earnings.¹ As the numbers of income-decreasing restatements have increased dramatically in recent years,² the large numbers of restatements, together with their serious implications, have raised concerns among investors, analysts and regulators.³ A great deal of literature examines the causes and consequences of earnings errors and related restatements. However, no study has investigated whether companies' income recognition practices change predictably following restatements.⁴ We argue that this gap in the literature should be addressed if we wish to understand all the steps that companies take to restore the credibility of their financial reporting, subsequent to accounting failures.

In Figure 1 we provide a conceptual model of the effects of revealed earnings restatements on investors and managers actions, and we locate this paper in the context of the restatement literature. The figure begins with a company's initial revelation and correction of earnings errors committed in a prior period or periods.⁵ We focus on the

¹ A restatement begins with a company's announcement that previously issued financial reports have violated, or may have violated U. S. generally accepted accounting principles (GAAP). The announcement may take the form of a press release or a Form 8-K (Current Report). A restatement results in the filing of amended financial reports that correct the errors. The amended results typically are filed on Form 10-K/A or Form 10-Q/A. In some cases the first announcement of a restatement is found in an amended 10-K or 10-Q (Scholz, 2008).

² The number of earnings-decreasing restatements increased from about 160 in 2000 to about 1,200 in 2006 (Scholz, 2008). The sample size is smaller in this study due to attrition arising from data requirements.

³ Wilson (2008) provides a useful discussion in her Introduction.

⁴ Several studies document decreases in information content of earnings following restatements (Anderson and Yohn, 2002; Wu, 2002; Wilson, 2008). These studies arguably test for changes in investor perceptions of earnings quality rather than for changes in companies' income recognition practices, such as increased conservatism.

⁵ Studies by the GAO (2003) and by Scholz (2008), among others, provide evidence regarding the frequency of such events, the characteristics of the earnings errors, and the characteristics of the restating

predominant type of restatements: those that correct previous overstatements of earnings.⁶ Investors respond to revelation of a company's prior earnings overstatements by penalizing the company in various ways, such as negative stock returns, increases in costs of capital, and lawsuits.⁷ The restating company is likely to engage in actions to repair its reputation for good management and corporate governance.⁸

[Insert Figure 1 about here.]

We argue that the restating company is likely to take actions to restore earnings credibility. Although changes in corporate governance likely suggest to investors that internal controls over accounting will improve, investors may expect the company to directly address earnings credibility by altering its income recognition practices.⁹ Given that earnings previously were overstated, we argue that investors seek reassurance that the company will report earnings more conservatively in future. This study provides evidence regarding this previously untested hypothesis. In additional analyses we investigate the extent to which our sample companies engage in improvements to corporate governance, and whether improvements in governance are associated with

companies.

⁶ Although some restatements involve reclassification and disclosure issues, approximately 90% affect previously reported net income (Scholz, 2008, 5). Furthermore, 80% to 90% of earnings restatements have the effect of decreasing previously over-stated net income (Scholz, 2008, 5).

⁷ Palmrose et al. (2004) find that restatements generate substantial declines in stock prices. This is followed by decreases in expected earnings and increased cost of capital (Hribar and Jenkins, 2004). Restatements involving core earnings accounts (especially revenues), and more pervasive restatements, are associated with increased likelihood of lawsuits (Palmrose and Scholz, 2004).

⁸ Prior research documents that restating companies make improvements to their boards of directors, audit committee characteristics, and other governance characteristics (Farber, 2005). Restating companies also replace top managers (Desai et al., 2006), and reduce option-based compensation of CEOs (Cheng and Farber, 2008).

⁹ External auditors also are likely to demand that restating firms report more conservatively in future, to lower the auditors' litigation risks of association with these clients (Johnstone and Bedard, 2003; Krishnan and Krishnan, 1997).

greater increases in earnings conservatism. These associations also have not previously been studied. Finally, Figure 1 indicates that changes in a company's corporate governance and earnings recognition practices should be followed by increased investor trust in the company and its income numbers, manifested as increased stock prices and larger earnings response coefficients.¹⁰

Financial accounting is conservative if it requires greater verifiability for the accounting recognition of economic gains versus losses, thus generating an understatement of net assets relative to market-assessed values.¹¹ Although the FASB does not consider conservatism to be a desirable characteristic of accounting information,¹² conservative reporting has endured, probably because of the benefits it provides to lenders and borrowers (Beatty et al., 2008; Guay, 2008; Zhang, 2008; Gigler et al., 2009), and to shareholders (Roychowdhury and Watts, 2007; LaFond and Watts, 2008; LaFond and Roychowdhury, 2008; Chung and Wynn, 2008).

Why might managers report more conservative earnings following restatements of income-increasing errors? A number of studies have found that information risk, based on measures of earnings quality, is priced by shareholders (Aboody et al. 2005, Francis et al. 2005, Ecker et al. 2006, Chen et al. 2007). Information risk in this literature is the

¹⁰ Farber (2005) finds that restating companies that improve corporate governance subsequently experience superior stock returns. Wilson (2008) finds that information content of earnings announced by restating companies does not decrease for those companies making changes to their financial reporting governance structures. For companies experiencing decreases in information content of earnings, the decreases typically reverse within four quarters after restatement.

¹¹ LaFond and Watts (2008, 447) provide this definition, following Basu (1997), and Holthausen and Watts (2001).

¹² The FASB (2008, 28) states that “describing *prudence* or *conservatism* as a qualitative characteristic or a desirable response to uncertainty would conflict with the quality of *neutrality* because, even with the proscriptions of deliberate misstatement that appear in the existing frameworks, an admonition to be prudent is likely to lead to a bias in the reported financial position and financial performance.”

probability that firm-specific, price-relevant accounting information is of poor quality. Investors' assessments of firm-specific information risk are likely to increase following a restatement.¹³ From an agency theory perspective, increased information risk potentially corresponds to greater information asymmetry between managers and investors. Overstatements of earnings concern investors in part because they can result in unwarranted rewards to managers. Managers can reduce information risk, agency costs, and cost of capital, by convincing investors that they have taken actions to avoid future earnings overstatements. Increases in the conservatism of reported earnings directly address this concern.¹⁴

To investigate the hypothesis that companies undertake more conservative reporting after disclosing restatements of overstated earnings, we employ a sample of 2,132 such restatements during the period 1999-2005. Consistent with the literature, we define earnings conservatism as recognizing bad news (economic losses) in a more timely fashion than recognizing good news (economic gains) (see, e.g., Basu 1997, Watts 2003, Ryan 2006, LaFond and Watts 2008). We use two estimation models, based on different conservatism metrics, to investigate the association between earnings and economic gains and losses. First, we use a 'Basu-based metric' model, with market returns as proxy for economic gains and losses (Basu 1997, LaFond and Watts 2008, LaFond and Roychowdhury 2008). Second, we use an 'earnings change metric' model, with prior year earnings increases and decreases proxying for economic gains and losses. In this

¹³ Kravet and Shevlin (2006) find increases in pricing of a 'discretionary' component of information risk when companies make accounting restatements.

¹⁴ LaFond and Watts (2008, 448) state that: "conservative financial reporting is a governance mechanism that reduces the managers' ability to manipulate and *overstate* financial performance and increases the firm's cash flows and value" [emphasis added].

framework, timely recognition of economic losses results in transitory income decreases that reverse in the next year (Ball and Shivakumar 2005). Greater conservatism manifests as earnings decreases that are more transitory than earnings increases. We also employ two sample strategies. First, we treat each test company as its own control, and compare test company conservatism in the pre- versus post-restatement periods. Second, we match each test company with a portfolio of non-restating control companies in the pre- period, with matching based on conservatism levels in the pre- period. Then we compare conservatism of test versus control companies in the post-restatement period.

The results of our primary analyses are highly consistent across four methods: (1) Basu-based metric models estimated with test companies only; (2) Basu-based metric models estimated with test companies and matched portfolios of control companies; (3) earnings change metric models estimated with test companies only; (4) earnings change metric models estimated with test companies and matched portfolios of control companies. We find that test companies increase the conservatism of their reported earnings after making restatements (methods 1 and 3 above). We find that test companies report earnings more conservatively than control companies after making restatements, although test and control companies exhibit similar conservatism in the pre- period (methods 2 and 4 above). The ‘increased conservatism’ result holds under all four methods whether or not we exclude from the pre-period those years in which test companies overstated earnings.

In additional analyses we estimate the earnings model parameters with two subsets of the test company sample: companies that undertake more versus less improvement in

governance from the pre- to post-restatement periods. In general we find that increases in conservatism are more evident among test companies experiencing greater improvement in governance following restatements. Greater increases in conservatism are observed among test companies that exhibit (1) decreased insider shareholdings (entrenchment); (2) decreased percentages of insiders on the board of directors; and (3) increased board sizes.

This study makes contributions to the literature in three respects. By providing empirical evidence of greater earnings conservatism after earnings restatements, our study complements a growing body of research examining how firms respond following accounting failures.¹⁵ Accounting conservatism reduces information asymmetry between investors and managers, and improves corporate governance, by restraining managers from opportunistically inflating earnings, and by forcing them instead to report more credible financial results. Our findings suggest that managers undertake increased accounting conservatism to directly address investors' concerns in the wake of disclosed overstatements of prior years' earnings. We are unaware of any prior published study of changes in companies' income recognition practices following restatements.

In addition to the restatement literature, this study contributes to studies of earnings conservatism. This topic has generated considerable attention in recent years. Most studies of earnings conservatism have treated it as a company characteristic that is fixed over fairly long periods. We are aware of only one published study that hypothesizes

¹⁵ As such it complements studies such as Farber (2005), Desai et al. (2006), Srinivasan (2005), Cheng and Farber (2008) and Wilson (2008).

that conservatism changes in the short term in response to changes in companies' characteristics or circumstances.¹⁶ That study is by LaFond and Watts (2008), who argue that changes in information asymmetry lead changes in conservatism, and who present empirical results that support this hypothesis. Our results are consistent with their prediction. Prior research indicates that restatements generate information asymmetry between managers and investors regarding the credibility of earnings. Managers appear to reduce this asymmetry by reporting earnings that are measurably more conservative than in pre-restatement periods, and measurably more conservative than those generated by control companies that were comparable in the pre- period. Thus our study is one of the first to document how an event that affects reporting credibility triggers changes in conservatism. This enables academics, investors, and regulators to better understand the dynamics of reporting credibility.

Finally, prior evidence suggests that restating companies are able to restore credibility of earnings fairly quickly following restatements. Wilson's (2008) evidence describes a U-shaped recovery in the information content of earnings following restatements. Although the information content of earnings drops immediately following a restatement, the loss is temporary. The information content of earnings recovers in the post-restatement period over an average of four quarters. Our study provides a potential explanation of how the information content of earnings might recover: restating companies restore their credibility by increasing financial reporting conservatism. Our

¹⁶ Studies have shown that differential timeliness varies predictably across companies based on characteristics that can change over time, such as public versus private status (for example, Ball and Shivakumar, 2005). Such characteristics tend to be stable over extended periods of time, and studies typically do not investigate whether changes in characteristics are accompanied by changes in conservatism, measured as differential timeliness.

additional analyses show that increases in accounting conservatism are more evident among companies improving corporate governance following restatements. These results are new to the literature.

This paper is organized as follows. Section 2 reviews the related literature and develops the hypotheses. Section 3 presents the research design and describes the sample. Section 4 discusses the main empirical results, and additional analyses. Section 5 concludes the paper.

1.2 Hypothesis development

The number of firms issuing earnings restatements has increased dramatically over the last decade (Scholz 2008). The adverse consequences following restatement announcements are well documented in the literature (GAO 2003, Palmrose et al. 2004, Hribar and Jenkins 2004, Srinivasan 2005, Palmrose and Scholz 2004). Stock prices decrease when restatements are first announced to the public (GAO 2003, Palmrose et al. 2004). Restatements result in an increase in the firm's cost of equity capital (Hribar and Jenkins 2004). In addition, outside directors suffer high turnover and labor market penalties (Srinivasan 2005), and restatements increase the incidence of shareholder lawsuits (Palmrose and Scholz 2004).

The negative consequences of restatements provide incentives to improve corporate governance and internal controls that might prevent future earnings errors. Farber (2005) finds that companies whose managers have committed financial reporting fraud take actions to improve their governance following fraud detection. Srinivasan (2005) finds

that outside directors, especially audit committee members, are more likely to be replaced after restatements. The likelihood of director departure increases with the severity of the error being restated. Desai et al. (2006) document that managers of restating companies face increased likelihood of job loss. Both corporate boards and the external labor market impose significant penalties on managers for violating GAAP. Cheng and Farber (2008) find that the proportion of CEOs' compensation in the form of options declines significantly in the two years following a restatement, and this reduction is accompanied by a decrease in the riskiness of investments undertaken by restating companies.

We argue that, in addition to the changes in governance outlined above, restating companies are likely to undertake changes in income recognition. Earnings restatements impair the credibility of financial reporting. Low credibility is associated with high information risk, and with high agency costs, which generate increased cost of capital. The preponderance of income-decreasing restatements suggests that managers intentionally overstated earnings in order to increase their compensation and other earnings-related benefits. Conservatism is an efficient mechanism to improve the credibility of financial reporting, because conservatism constrains managers' ability to overstate financial performance (Watts 2003, LaFond and Watts 2008).

Managers' asymmetric loss function and unaligned interests with investors give them incentives to inflate financial performance in order to transfer wealth from shareholders to themselves. Watts (2003) and LaFond and Watts (2008) argue that managers have greater incentives to report less verifiable gains, but are reluctant to

report less verifiable losses. Conservatism requires managers to recognize economic losses in a more timely manner than economic gains, and thus protects investors, by limiting managers' ability to obtain excessive compensation based on overstated earnings. Conservatism thus counteracts the asymmetry in managers' income recognition incentives. Conservative recognition arguably provides investors with more credible information than would an accounting regime that applies equally strong recognition standards to both gains and losses. Thus, conservatism can help establish the credibility of earnings for outside users of financial statements. As a result, investors view the credibility of earnings to be higher, as companies report more conservative earnings (Francis et al. 2004).

In summary, disclosure of prior earnings overstatements results in various penalties for managers, directors, and the restating companies. Although such companies frequently engage in a variety of improvements to corporate governance, restating companies also need to reestablish the credibility of their reported earnings. Adopting more conservative income recognition practices (increased conservatism) provides a well-documented means of increasing credibility. Furthermore, increased conservatism is a direct response to the correction of previously overstated earnings. Therefore our hypothesis is (in alternate form):

H1: Companies that correct earnings overstatements exhibit more conservative earnings in the post-restatement periods.

1.3 Sample Selection and Research Design

1.3.1 Restatement Sample Selection

This study's sample includes firms that disclose a single restatement during January 1, 1999 through December 31, 2005. The restatements studied in our paper are limited to the correction of errors and irregularities in the application of U.S. GAAP in public company financial statements filed with the SEC.¹⁷ We identify our sample from several sources. First, we use Audit Analytics (AA) to identify firms that report restatements during the years 2000-2005. Second, we include restatements identified by using an extensive key-word search in Lexis-Nexis Library and SEC Filing Library for restatements during the years 1999-2000 (e.g., restat, revis, adjust, error).¹⁸ Third, we supplement our sample by comparing the search results to restatements listed in a Government Accountability Office (GAO) study for relevant years 1999-2002.¹⁹ Scholz (2008) compares data sources for overlapping periods, and finds that AA includes nearly all restatements identified in GAO lists and Lexis-Nexis searches during 2001-2003. Therefore, restatements in 2003-2005 are obtained only from AA. Panel A of Table 1 presents the sample distribution by year. The number of restatements increases from 77 in calendar year 1999 to 641 in 2005. In order to clearly distinguish between pre- and post-restatement periods, we restrict the test sample to companies which experienced only one restatement in the sample period. This allows comparison of conservatism levels before versus after clear restatement events. Multiple disclosures concerning one

¹⁷ Restatements that are made to correct content other than errors in financial statements are eliminated from our sample. For example, some companies use the word "restate" to present changes in accounting principles or retrospective revisions to enhance the consistency of their financial information. Those observations are not the focus of this study, and therefore eliminated from our sample. Restatements made by foreign filers to reformulate financial information under U.S. GAAP also are eliminated.

¹⁸ We are grateful to Susan Scholz for providing this database of restatements.

¹⁹ The overlapping periods are used to validate both data sources.

restatement event are treated as one restatement. We also require test companies to have at least one year of data available both before and after the restatement year. These requirements result in a maximum sample of 2,132 restatements.

[Insert Table 1 about here.]

Adoption of more conservative income reporting is likely to be a strategy of companies that correct overstatements rather than understatements of earnings. Therefore we focus on earnings overstatements: restatements that decrease previously reported earnings. In our overall restatement sample, about 90 percent of restatements are income-decreasing ($N = 3,812$), and about 10 percent are income-increasing ($N = 451$) in the original sample of restatements from 1999 to 2005. Numbers of overstatements available for the empirical tests vary based on the particular combinations of conservatism metric and sampling approaches employed. The preponderance of income-decreasing restatements in the entire restatement sample suggests that many errors are due to aggressive income recognition. Income-increasing restatements are more likely to correct unintentional errors. In robustness tests we use the overall restatement sample, including corrections of both overstatements and understatements of earnings, to estimate all regression models. Those results are quite similar to the main results.

1.3.2 Two Sampling Approaches

We employ two sampling approaches to test the hypothesis of more conservative earnings in the post- period. Figure 2 provides a graphic representation of the two

approaches. One approach, which we refer to as the within-sample approach, is to compare the restating companies' earnings conservatism in the pre- versus post- periods. This approach has the advantage that each company serves as its own perfectly matched control. It might seem tautological that companies ceasing to overstate income subsequently report more conservative earnings. However, our evidence supports the hypothesis whether or not we delete periods of misstated earnings from pre-restatement observations. This suggests that the measures of conservatism we employ are not significantly affected by earnings overstatements in the pre-restatement periods, even when such periods are not deleted.²⁰

[Insert Figure 2 about here.]

A second approach to test the hypothesis is to compare the earnings conservatism of two samples: restating companies in the post- periods, and portfolios of non-restating companies in the same periods. Each restating company is matched with a portfolio of non-restating companies having similar conservatism in the pre- period. Test and control companies therefore exhibit similar conservatism in pre- periods (by control sample construction), but we expect restating companies to display more conservatism than control portfolio companies in the post- period. We refer to this as the control-sample approach. This approach has the advantage that it controls for any economy-wide changes in conservatism from the pre- to post- periods. Differences in conservatism

²⁰ Our results in this regard are consistent with Givoly et al. (2007), who report that a sample of 166 companies that restated earnings in 2000-2001 do not exhibit lower levels of a Basu-based conservatism metric in the years when they overstated earnings. A possible explanation for our results is that the effects of overstated earnings do not map cleanly into the constructs of good economic news and bad economic news that underlie the empirical methods we use to measure conservatism. For example, in some tests we define good news as a positive annual stock return and bad news as a negative return. Overstatements of earnings likely occur in both return regimes.

between the test and control samples in post- periods therefore are more likely to be due to the event of interest, restatements.

1.3.3 Two Conservatism Metrics

The two sampling approaches discussed above do not specify how conservatism is measured. In this section we introduce the two conservatism metrics employed in this study. Each metric is employed with both sampling approaches, yielding four tests of our hypothesis. The conservatism literature distinguishes between two types of earnings conservatism: unconditional and conditional. Unconditional conservatism consists of income recognition policies that are applied unconditionally (i.e. independently of a company's circumstances or market conditions). An example would be immediate expensing of internally developed intangible assets. Conditional conservatism consists of income recognition policies that impose asymmetric timeliness for recognizing good news as gains than for recognizing bad news as losses. Both conditional and unconditional conservatism is an accounting conceptually toward reporting the lowest alternative value for assets and the highest alternative value for liability. The key difference between the two is that conditional conservatism is a reporting *conditional on companies experiencing contemporaneous economic losses*; unconditional conservatism, however, does not specify conditionally low equity or income, and thus does not address loss recognition timeliness (Ball and Shivakumar 2005).²¹

²¹ Under unconditional conservatism requirements, managers do not have flexibility to choose the extent of losses or expenses recognition, for example, R&D is expensed at 100 percent when it occurs. Such recognition is independent from news, and therefore, firms (that follow GAAP) should have the same level of unconditional conservatism. In contrast, under conditional conservatism, managers have

In this study, we will focus on the conditional conservatism, because improvement in financial credibility results primarily from the role of conditional conservatism in constraining managers' ability to overstate earnings, protecting the interests of investors and lenders (Watts 2003, LaFond and Watts 2008). Nevertheless, Ball and Shivakumar (2005) argue that unconditional conservatism is inefficient or at best neutral in improving reporting credibility, because it can be simply inverted, and potentially reduce opportunities to efficiently exert conditional conservatism²².

Accordingly, we choose Basu model-based metric and earnings change metric conservatism measure to test our hypotheses. Basu-based measure is the best proxy for conditional conservatism in a context such as ours (Ryan 2006, Ball et al. 2009). We considered but rejected the use of market-to-book equity ratios and abnormal accrual-related metrics as proxies for conservatism for several reasons. First, Basu model-based measures are more reliable than market-to-book ratios. Growth options are included in market value, but book value incorporate growth options only when acquired. This causes market-to-book ratio to include both conservatism and unverifiable growth options (see Watts 2003, Roychowdhury and Watts 2007). In contrast, the Basu coefficients incorporate neither increases nor decreases in growth options when using longer estimation intervals.²³ Second, the market-to-book equity ratio captures the effects of conservatism across all previous periods. As such it is likely

flexibility to recognize losses more timely versus gains. For example, managers exercise discretion in choosing depreciation method. Managers who choose accelerating method are more *conditionally* conservative than those choosing straight-line method.

²² Unconditional conservatism of writing off assets at acquisition, for example, eliminates the opportunity to impair them in the time of economic losses, a type of conditional conservatism.

²³ Roychowdhury and Watts (2007) provide evidence.

not sensitive to *changes* in conservatism such as we investigate (see, e.g., LaFond and Watts 2008, Roychowdhury and Watts 2007). Finally, we also considered but rejected negative abnormal accruals-related measures as conservatism metrics. Accruals shift income across periods; reduced earnings in one period can be followed by enhanced earnings in another period. In addition, abnormal accruals (and total accruals) represent only part of earnings. The conservatism metrics we employ are based on total net income.

1.3.3.1 The Basu-based Metric and Within-sample Model

Higher conservatism consists in recognizing economic losses in a more timely fashion than recognizing economic gains. Basu (1997) proposes a model in which economic gains and losses are proxied by positive and negative market returns. Conservatism is measured as the extent to which negative returns (bad news) are reflected in reported earnings more rapidly than positive returns (good news). Consider the following basic model:²⁴

$$NI_{it} = \alpha_0 + \alpha_1 NEG_{it} + \alpha_2 RET_{it} + \alpha_3 RET_{it} \times NEG_{it} + \dots \quad (1)$$

NI_{it} is net income for a period; RET_{it} is stock return for a corresponding period.²⁵ NEG_{it} is defined as zero if RET_{it} is positive, and as one if RET_{it} is negative. Coefficient α_2 is the slope coefficient for positive RET_{it} , $(\alpha_2 + \alpha_3)$ is the slope coefficient for negative RET_{it} , and α_3 is the differential slope for bad versus good economic news. Earnings are

²⁴ Control variables and use of a control sample will be introduced subsequently.

²⁵ NI_{it} is year t net income before extraordinary items (Compustat #18) of firm i , scaled by the beginning of fiscal year t market value of equity (Compustat #60). RET_{it} is annual buy-and-hold return of firm i , calculated as returns from three months after fiscal year end in year $t-1$ to three months after fiscal year end date in year t .

conservative if net income is more sensitive to bad news than to good news, i.e. if α_3 is positive.

Our hypothesized increase in conservatism implies that slope α_3 in model (1) should become more positive following a restatement. To capture this shift we extend model (1) by including the variable $POST_{it}$. $POST_{it}$ equals one for firm i if year t is after the restatement year, and zero otherwise. We include $POST_{it}$ in models to test whether overstating firms report more conservatively after versus before restatement disclosures. The extended ‘Basu-based metric’ model is:

$$\begin{aligned}
NI_{it} = & \alpha_0 + \alpha_1 NEG_{it} + \alpha_2 POST_{it} + \alpha_3 MTB_{it-1} + \alpha_4 LEV_{it-1} + \alpha_5 SIZE_{it-1} + \alpha_6 LIT_{it-1} \\
& + \alpha_7 NEG_{it} \times POST_{it} + \alpha_8 NEG_{it} \times MTB_{it-1} + \alpha_9 NEG_{it} \times LEV_{it-1} \\
& + \alpha_{10} NEG_{it} \times SIZE_{it-1} + \alpha_{11} NEG_{it} \times LIT_{it-1} + \alpha_{12} RET_{it} + \alpha_{13} RET_{it} \times POST_{it} \\
& + \alpha_{14} RET_{it} \times MTB_{it-1} + \alpha_{15} RET_{it} \times LEV_{it-1} + \alpha_{16} RET_{it} \times SIZE_{it-1} \\
& + \alpha_{17} RET_{it} \times LIT_{it-1} + \alpha_{18} RET_{it} \times NEG_{it} + \alpha_{19} NEG_{it} \times RET_{it} \times POST_{it} \\
& + \alpha_{20} NEG_{it} \times RET_{it} \times MTB_{it-1} + \alpha_{21} NEG_{it} \times RET_{it} \times LEV_{it-1} \\
& + \alpha_{22} NEG_{it} \times RET_{it} \times SIZE_{it-1} + \alpha_{23} NEG_{it} \times RET_{it} \times LIT_{it-1} + \varepsilon_{it} \quad (2)
\end{aligned}$$

In this model, coefficient α_{18} represents conservatism in the pre-period: differential timeliness of recognition for bad versus good news in the pre- period. Coefficient α_{19} represents the shift in conservatism from the pre- to the post-restatement period. In the context of our hypothesis, we expect α_{19} to be positive (an increase in conservatism). Following LaFond and Roychowdhury (2008) and LaFond and Watts (2008), we control for variation of conservatism due to firm size, leverage, market-to-book ratio and

litigation risk.²⁶ LaFond and Watts (2008, 475) argue that *SIZE* proxies for political costs, aggregation of income and returns across multiple segments and projects, and information asymmetry. Leverage, *LEV*, represents lenders' demand for conservatism (LaFond and Roychowdhury 2008, 118). The market-to-book equity ratio, *MTB*, reflects the composition of beginning value of equity (Roychowdhury and Watts 2007).²⁷ Litigation risk, *LIT*, provides managers with incentives to practice conservative recognition of economic losses (Basu 1997; Watts 2003; LaFond and Roychowdhury 2008).

1.3.3.2 The Earnings Change Metric and Within-sample Model

Ball and Shivakumar (2005) propose that timely recognition of economic gains and losses implies that they occur as transitory changes in earnings components that tend to reverse in the next period. For firms with greater accounting conservatism, the reversal is more pronounced for earnings decreases in the next period. The basic logic is that earnings decreases are more transitory than earnings increases, so timelier recognition of earning decreases in a current period will be followed by a greater extent of reversals in the next period.

²⁶ MTB_{it-1} is the scaled decile rank of the market to book ratio (Compustat #199* Compustat #25/ Compustat #60) of firm i at the beginning of the fiscal year t . LEV_{it-1} is the scaled decile rank of total debt (Compustat #9 + Compustat #34) divided by total assets (Compustat #6) of firm i at the beginning of the fiscal year t . $SIZE_{it-1}$ is the scaled decile rank of market value of equity (in millions of dollars) (Compustat #199* Compustat #25) at the beginning of the fiscal year t . LIT_{it-1} is an indicator variable that equals one if firm i is in a litigious industry (SIC codes 2833 to 2836; 3570 to 3577; 3600 to 3674; 5200 to 5961, and 7370) at the beginning of year t , and zero otherwise.

²⁷ Book value of equity reflects only recognized assets. Market value of equity reflects both recognized assets and market-assessed assets that accounting does not recognize.

Consider the following basic model:²⁸

$$NI_{it+1} = \alpha_0 + \alpha_1 DNI_{it} + \alpha_2 NI_{it} + \alpha_3 DNI_{it} \times NI_{it} + \dots \quad (3)$$

NI_{it} is the change in net income from period $t-1$ to period t .²⁹ DNI_{it} is defined as one if NI_{it} is negative, and as zero if it is positive. Coefficient α_2 is the slope coefficient for an increase in earnings for the prior period (good news). The slope coefficient for a decrease in earnings for the prior period is $(\alpha_2 + \alpha_3)$. Coefficient α_3 is the differential slope for bad versus good economic news. Earnings conservatism implies that firms recognize bad news (earnings decrease) in a faster manner than good news (earnings increase). Thus the change in net income due to bad news is more transitory than the income effect of good news, i.e. α_3 should be negative. Ball and Shivakumar (2005) assert two advantages of using change in earnings as an independent variable, rather than level of earnings: (1) changes provide the correct specification for identifying transitory income components; (2) the incremental timeliness coefficient α_3 is less likely to be affected by survival biases in a changes specification.

Our hypothesized increase in conservatism implies that slope α_3 in model (3) should become more negative following a restatement. To capture this shift we extend model (3) by including the variable $POST_{it}$, which was previously defined. The extended ‘earnings change metric’ model is:

²⁸ Equation (3) is a modification of the Ball and Shivakumar (2005) model. Ball and Shivakumar investigate the difference in conservatism between public companies and private companies. In their setting, the dummy variable representing private company status does not change over time. However, we propose that conservatism changes over time for test companies. Conceptually, if we want to measure the conservatism level in year t , we should investigate the extent to which loss recognition in year t leads to a reversal in year $t+1$. We modify the model accordingly.

²⁹ NI_{it} is year t net income before extraordinary items (Compustat #18) of firm i , scaled by the beginning of fiscal year t market value of equity (Compustat #60), minus scaled net income (similarly defined) for the prior year.

$$\begin{aligned}
\Delta NI_{it+1} = & \alpha_0 + \alpha_1 D\Delta NI_{it} + \alpha_2 \Delta NI_{it} + \alpha_3 D\Delta NI_{it} \times \Delta NI_{it} + \alpha_4 POST_{it} \\
& + \alpha_5 POST_{it} \times \Delta NI_{it} + \alpha_6 POST_{it} \times D\Delta NI_{it} + \alpha_7 POST_{it} \times D\Delta NI_{it} \times \Delta NI_{it} \\
& + \alpha_8 SIZED_{it} + \alpha_9 SIZED_{it} \times D\Delta NI_{it} + \alpha_{10} SIZED_{it} \times \Delta NI_{it} \\
& + \alpha_{11} SIZED_{it} \times D\Delta NI_{it} \times \Delta NI_{it} + \varepsilon_{it}
\end{aligned} \tag{4}$$

In this model, coefficient α_3 represents conservatism: differential persistence of income decreases (bad news) versus increases (good news) in the pre- period. Coefficient α_7 represents the shift in conservatism from the pre- period to the post-restatement period. In the context of our hypothesis, we expect α_7 to be negative (an increase in conservatism).³⁰

1.3.3.3 Selection of Non-overstating Control Sample

As discussed above (and as shown in Figure 2) we employ control portfolios of companies that do not restate their financial results. In this section we describe how we match overstating firms with non-overstating peer firms that report at similar conservatism levels prior to restatement disclosures. Control companies are companies having necessary data in COMPUSTAT (for financial statement variables) and in CRSP (for stock returns to generate RET_{it}) for the estimation of equations (5) and (6).

First we calculate each test company's and potential control company's conservatism level as of each year from 1996 to 2006. Following Francis et al. (2004), we estimate a company's conservatism level for each year t using model (1) and a window of ten prior-year observations. Recall that coefficient β_2 of model (1) measures earnings timeliness with respect to good news, and β_3 measures the asymmetric

³⁰ $SIZED_{it}$ is the decile rank of total assets at end of year t , standardized to vary between zero and one.

timeliness with respect to bad news, which is our conservatism measure. We define each company's conservatism level for a year, $CONS_t$, equal to its estimated coefficient β_3 for that year. Then we sort the sample of test and potential control companies into ten portfolios (deciles) per year, based on rank of $CONS_t$. Non-overstating companies that are in the same $CONS$ decile with an overstating company in a given year, are regarded as peer companies for the overstating company in that year. In the robustness tests discussed in section 4, we also use the conservatism measure $CONS_FR$ employed by Francis et al. (2005), calculated as $-(\beta_3 + \beta_2) / \beta_2$, to select matching deciles of peer companies. The hypothesis test results based on this measure are essentially the same as the main results.

1.3.3.4 The Basu-based Metric and Control-sample Model

To compare conservatism between overstating firms and non-overstating peer firms subsequent to restatement disclosure, we use an indicator variable $OVERSTAT_i$ that equals one if firm i is an overstating firm, and zero for control firms. Again, we use both the conservatism Basu model and the earnings change model to test the hypothesis. This section describes the model in which the Basu-based metric is used with control samples. We modify model (2) by replacing variable $POST$ with variable $OVERSTAT$. Variable $POST$ is not needed because, consistent with Figure 2, the following equation is estimated using only post- period observations:

$$\begin{aligned}
NI_{it} = & \alpha_0 + \alpha_1 NEG_{it} + \alpha_2 OVERSTAT_{it} + \alpha_3 MTB_{it-1} + \alpha_4 LEV_{it-1} + \alpha_5 SIZE_{it-1} + \\
& + \alpha_6 LIT_{it-1} + \alpha_7 NEG_{it} \times OVERSTAT_{it} + \alpha_8 NEG_{it} \times MTB_{it-1} \\
& + \alpha_9 NEG_{it} \times LEV_{it-1} + \alpha_{10} NEG_{it} \times SIZE_{it-1} + \alpha_{11} NEG_{it} \times LIT_{it-1} + \alpha_{12} RET_{it}
\end{aligned}$$

$$\begin{aligned}
& + \alpha_{13}RET_{it} \times OVERSTAT_{it} + \alpha_{14}RET_{it} \times MTB_{it-1} + \alpha_{15}RET_{it} \times LEV_{it-1} \\
& + \alpha_{16}RET_{it} \times SIZE_{it-1} + \alpha_{17}RET_{it} \times LIT_{it-1} + \alpha_{18}RET_{it} \times NEG_{it} \\
& + \alpha_{19}NEG_{it} \times RET_{it} \times OVERSTAT_{it} + \alpha_{20}NEG_{it} \times RET_{it} \times MTB_{it-1} \\
& + \alpha_{21}NEG_{it} \times RET_{it} \times LEV_{it-1} + \alpha_{22}NEG_{it} \times RET_{it} \times SIZE_{it-1} \\
& + \alpha_{23}NEG_{it} \times RET_{it} \times LIT_{it-1} + \varepsilon_{it}
\end{aligned} \tag{5}$$

In equation (5), α_{18} measures the conservatism of non-overstating control firms.

Coefficient α_{19} measures the differential conservatism of restating companies. After restatement disclosures, if overstating firms report more conservatively than do non-overstating peer firms, we expect α_{19} to be positive.

1.3.3.5 The earnings change metric and control-sample model

We alter model (4) by replacing variable *POST* with variable *OVERSTAT*. The resulting model, estimated only in post-periods, is:

$$\begin{aligned}
\Delta NI_{it+1} = & \alpha_0 + \alpha_1 D\Delta NI_{it} + \alpha_2 \Delta NI_{it} + \alpha_3 D\Delta NI_{it} \times \Delta NI_{it} + \alpha_4 OVERSTAT_{it} \\
& + \alpha_5 OVERSTAT_{it} \times \Delta DNI_{it} + \alpha_6 OVERSTAT_{it} \times \Delta NI_{it} \\
& + \alpha_7 OVERSTATE_{it} \times D\Delta NI_{it} \times \Delta NI_{it} + \alpha_8 SIZED_{it} + \alpha_9 SIZED_{it} \times D\Delta NI_{it} \\
& + \alpha_{10} SIZED_{it} \times \Delta NI_{it} + \alpha_{11} SIZED_{it} \times D\Delta NI_{it} \times \Delta NI_{it} + \varepsilon_{it}
\end{aligned} \tag{6}$$

Coefficient α_3 measures the conservatism of control companies. Coefficient α_7 measures differential conservatism for overstating companies. If overstating firms report more conservatively, subsequent to restatement disclosures, than do their non-overstating peer firms, we expect α_7 to be negative.

1.4 Empirical results

1.4.1 Descriptive statistics for within-sample models

Table 2 reports descriptive statistics on dependent and independent variables used to estimate equations (2) and (4). Panel A describes the sample for the Basu-based metric model used to estimate equation (2). The mean $POST_{it}$ value in our sample is 0.277 percent. This indicates that about 28 percent of firm-year observations are in the post-period. The mean NI_{it} value is 0.015, suggesting that overstating firms in our sample generally report positive earnings. The mean value of NEG_{it} is 0.423. This indicates that approximately 42.3 percent of the sample exhibits a negative buy-and-hold return over a fiscal year on average. The mean value of RET_{it} , about 18 percent, suggests that in general our sample companies experience positive returns. The descriptive statistics for $MVEBOOK_{it-1}$, $LEVERAGE_{it-1}$, and MVE_{it-1} indicate that our sample consists of relatively high market-to-book, highly leveraged, and large cap companies. The mean value of LIT_{it-1} , about 0.18, indicates that about 18 percent of our sample companies come from high-litigation-risk industries.

Panel B of Table 2 describes the sample for the earnings change metric model used to estimate equation (4). The mean $POST_{it-1}$ value is 0.270, indicating that about 27 percent of firm-year observations are in the post-period, which is consistent with the sample used for equation (2). The mean values of ΔNI in year $t+1$ and year t are both about 0.01. This indicates that on average our sample companies experience earnings increases. The mean value of $D\Delta NI_{it}$ is 0.412, indicating that about 41 percent of companies in the sample experience income decreases in a fiscal year, on average. The descriptive statistics for $TOTAL_ASSET_{it}$ suggest that our sample companies are

relatively large.

1.4.2 Multivariate results for within-sample models

Table 3 presents the regression estimates of equation (2) that uses the Basu-based metric model to compare conservatism for test companies between the pre- and post-restatement periods.³¹ To eliminate the effects of potential outliers and to facilitate the interpretation of the coefficients, we follow LaFond and Roychowdhury (2008), by using scaled decile ranks for all the variables except for NI_{it} , RET_{it} , NEG_{it} and LIT_{it-1} .³² To make sure our findings are not significantly affected by earnings overstatements in the pre-restatement periods, we report the regression coefficient estimates, excluding and including misstated periods, respectively. The results are essentially the same.

The first two result columns of Table 3 report the coefficients estimated using Equation (2) and the sample excluding misstated periods. We find the coefficient on $RET_{it} \times NEG_{it}$ is significantly positive as expected and, more importantly, the coefficient on $RET_{it} \times NEG_{it} \times POST_{it}$ is positive and highly significant. These results suggest that detectable conservative income recognition policies exist in the pre-restatement period. Further, in the post-restatement period, test companies report more conservatively than before, as earnings become more asymmetrically timely in recognizing bad news. When we use the sample including misstated periods, the results (shown in the right-most two columns) lead to the same conclusions.

³¹ We use the Huber-White procedure to correct the autocorrelation of the residuals (Peterson, 2009).

³² We first rank observations by year into 10 groups from zero to nine, and then divide ranking number by nine. Thus, the ranked variables range from 0 to 1 and therefore, the regression coefficients can be interpreted as the impact of a change in the corresponding variable from the bottom decile to the top decile.

Table 4 reports results obtained using the earnings change metric to capture conservatism. We find the coefficient on $D\Delta NI_{it} \times \Delta NI_{it}$ is significantly negative. This indicates detectable conservative income recognition practices exist in the pre-period, as the tendency for income decreases (bad news) to revert is stronger than for income increases. Further, the coefficient on $POST_{it} \times D\Delta NI_{it} \times \Delta NI_{it}$ is negative and highly significant. This result indicates that subsequent to restatement disclosures, the conservatism of test companies increases. As shown in the second two results columns, the results are unchanged when we include the misstated periods in the estimation sample. In summary, the empirical results in Tables 3 and 4 suggest that test companies increase the conservatism of their earnings recognition practices after they disclose restatements.

1.4.3 Descriptive statistics for control-sample models

In this section we report the results obtained by matching each test company with non-restating control companies that have similar conservatism levels in the pre-period. Table 5 reports descriptive statistics for the dependent and independent variables used to estimate equations (5) and (6). Panel A of Table 5 describes the sample for the Basu-based metric model used to estimate equation (5). The mean $OVERSTAT_{it}$ value in our sample is 0.073. This indicates that 7.3 percent of our sample consists of overstating test companies, and 92.7 percent of the sample consists of companies in matched control portfolios. The descriptive statistics for other variables are similar to those reported in Panel A of Table 2.

Panel B of Table 5 describes the sample for the earnings change metric model used to estimate equation (6). The mean $OVERSTAT_{it}$ value is 0.071, indicating that 7.1 percent of our sample consists of test companies and 92.9 percent consists of companies in matched control portfolios. The descriptive statistics for other variables are similar to those in Panel B of Table 2.

1.4.4 Multivariate results for control-sample models

Table 6 presents the regression estimates of equation (5) that uses the earnings metric model to compare conservatism between test companies and control companies in the post-period.³³ The first two results columns are obtained using matching control portfolios indentified after computing test company conservatism metrics using a test company sample that excluded misstated periods. The second two results columns are obtained using matching control portfolios indentified after computing test company conservatism metrics using a test company sample that included misstated periods. The coefficients on $RET_{it} \times NEG_{it}$ are significantly positive, and the coefficients on $RET_{it} \times NEG_{it} \times OVERSTAT_{it}$ are positive and highly significant. The former results indicate that companies in the matching control portfolios practice conservative income recognition policies in the post- periods. Further, test companies report more conservatively than peer companies in the post- periods, as test company earnings are asymmetrically timely in recognizing bad news.

Table 7 reports the estimated coefficients obtained using the earnings change metric

³³ We use the Huber-White procedure to correct the autocorrelation of the residuals (Peterson, 2008).

and Equation (6). The columns labeled as excluding and including misstated periods have the same meaning as in Table 6. The coefficients on $D\Delta NI_{it} \times \Delta NI_{it}$ are significantly negative. This indicates conservative income recognition policies among control companies in the post- periods. Further, the coefficients on $OVERSTAT_{it} \times D\Delta NI_{it} \times \Delta NI_{it}$ are negative and highly significant. This indicates that subsequent to restatement disclosures, the conservatism of test companies is greater than that of the matched control companies. To summarize the results in Table 6 and Table 7, test companies report earnings more conservatively than control companies, after making restatement announcements.

1.4.5 Additional analysis

1.4.5.1 Improvements in corporate governance

The results reported in the main analyses provide evidence that companies generally increase the conservatism of their earnings after making restatements. We expect that this increase in conservatism is associated with improvement in companies' corporate governance. In this section, we investigate the expected association using two subsets of the test companies: those that do versus do not improve their governance from the pre- to post-periods. We expect to observe that the increase in conservatism is stronger for "improved" companies than for "unimproved" ones.

We employ three proxies to capture corporate governance: percentage of insider shareholdings, percentage of insiders on board of directors, and board size (Ahmed and

Duellman, 2006).³⁴ We define the test companies as “improved” if they (1) decrease the percentage of insider holdings, (2) decrease the percentage of insiders on the board, or (3) increase the size of the board from pre- to post-restatement periods.

Table 8 reports results for the improved and unimproved test companies on the three dimensions of corporate governance.³⁵ The results are consistent with our predictions: the coefficient on $RET_{it} \times NEG_{it} \times POST_{it}$ is significantly positive for all three improved governance sub-samples, but is insignificant for the unimproved sub-samples. The differences in coefficients between improved and unimproved sub-samples are consistently significant at the 0.05 level for all three governance dimensions. These results suggest that the increase in conservatism is more evident in companies experiencing stronger improvement in governance accompanying restatements.³⁶

1.4.5.2 Conservatism of test companies preceding restatement disclosures

The Table 3 results indicate that test companies exhibit conservatism in the pre-restatement periods (differential timeliness in recognizing bad news, i.e. a significant, positive coefficient of $RET \times NEG$) even if years when misstatements occurred are not excluded. This poses a question previously raised by results reported in Givoly et al. (2007): can the Basu-based differential timeliness measure be valid if it

³⁴ The corporate governance data are obtained from the IRRC database for the period 1996-2006. The additional analyses are conducted only for the subsample of companies that have the necessary governance data in IRRC.

³⁵ We only report the results using the earnings metric model. Qualitatively similar results are obtained using the earnings change metric model.

³⁶ We also examine other proxies for corporate governance including percentage of independent directors on the board, the existence of a written governance policy, and the number of committees the CEO sits on. Improvements in these proxies are not associated with differences in levels of differential test company conservatism in the post- period.

indicates conservative reporting in periods when companies are overstating earnings? Givoly et al. found that a sample of 498 companies that overstated earnings exhibited similar Basu-based differential timeliness metrics for misstatement years as for earlier, presumably “clean” reporting years. They also found that the test companies exhibited significantly greater conservatism during misstatement years compared to 993 similar size companies and 3,237 companies in the same industries. Given the Table 3 results, we revisit the question raised by Givoly et al. (2007) using our significantly larger samples.

We estimate equation (5) with a pooled sample of test companies’ and control companies’ firm-year observations. This regression analysis differs from the results reported in Table 6 in two aspects. First, since the purpose of this additional analysis is to test whether conservatism is lower among overstating companies before they make restatements, we employ test firm annual observations from their pre-restatement periods, rather than observations from their post-periods as in Table 6. Second, we use all available control companies regardless of their conservatism levels. In contrast, the main analysis in Table 6 uses portfolios of non-restating companies matched to test companies in pre-restatement periods.

Table 9 reports the results. The coefficient on $RET_{it} \times NEG_{it}$ is positively significant, suggesting that non-restating firms report conservatively in general. More importantly, we find that the coefficient on $RET_{it} \times OVERSTAT_{it}$ is significantly positive, and the coefficient on $RET_{it} \times NEG_{it} \times OVERSTAT_{it}$ is negative and highly significant. These results indicate that, preceding restatements, test companies report less conservative

earnings. That is they recognize economic gains (good news) more quickly, and economic losses (bad news) more slowly, than do non-restating control companies. The difference between our result and the result reported by Givoly et al. (2007) could arise for several reasons. One possibility is that the difference is due to our use of a more extensive Basu-based estimation model. Another possibility is that we employ a substantially larger number of test and control company-year observations (42,975 versus a maximum of 3,725 for Givoly et al.). Although our Table 9 results provide support for the use of a Basu-based metric as proxy for cross sectional differences in earnings conservatism, they do not resolve all difficulties. It is problematic that overstating test companies exhibit conservatism in their pre-restatement periods (differential timeliness in recognizing bad news, i.e. a significant, positive coefficient of $RET \times NEG$). Given our results, and those of Givoly et al. (2007), future research might try to determine whether refinements to the Basu-based metric can resolve this discrepancy.

1.4.5.3 Use of an alternative measure to identify peer non-restating firms

Recall that to obtain matching control companies, we estimate conservatism for each control company using model (1). We define the Basu differential timeliness coefficient β_3 as $CONS_t$; then we use the rank of $CONS_t$ to generate portfolios of non-restating control companies that are matched to test companies' conservatism levels in the pre-restatement periods. In this section, we test the sensitivity of our findings to the identification of matching control companies. We estimate each test and control

company's conservatism as $-(\beta_3 + \beta_2)/\beta_2$ (which we designate as $CONS_FR_t$) following Francis et al. (2005). Then we use the rank of $CONS_FR_t$ to identify the portfolio of non-overstating peer companies that have similar conservatism to a given test company in the pre-period.

We re-estimate equations (5) and (6) using the newly matched control groups and obtain results that are similar to the findings in the main analysis. The untabulated results show that the coefficient on $NEG_{it} \times RET_{it} \times OVERSTAT_{it}$ is 0.165 (t-statistic = 3.09, p-value = 0.001) in the estimation of equation (5). The coefficient on $OVERSTAT_{it} \times DANI_{it} \times \Delta NI_{it}$ is -0.274 (t-statistic = 3.74, p-value=0.000) in the estimation of equation (6). These results indicate that our findings are robust to the measure of conservatism used to select matching control groups.

1.4.5.4 Use of the total restatement sample

In the main analysis we restrict our sample of test companies to those that correct overstated earnings. In this section, we test the sensitivity of our findings to the use of the total restatement sample, including companies that originally understated earnings. We re-estimate equations (2) and (4) using the earnings metric model, and equations (5) and (6) using the earnings change metric model. In models where it appears, variable $OVERSTAT$ is replaced by variable $RESTATE$, which equals “one” for all restatement companies, and “zero” for all control companies.

The untabulated results are similar to the findings in the main analysis: restating companies taken as a whole increase the conservatism of their earnings after making

restatement disclosures. The results are consistent using the two sampling methods shown in Figure 2 and the two conservatism metrics. The results estimated using the earnings metric model show that the coefficient on $NEG_{it} \times RET_{it} \times POST_{it}$ is 0.088 (t-statistic = 2.12, p-value = 0.009) in the estimation of equation (2). The coefficient on $NEG_{it} \times RET_{it} \times RESTATE_{it}$ is 0.097 (t-statistic = 2.23, p-value = 0.006) in the estimation of equation (5). These results indicate that restating firms in general report more conservatively in the post- than the pre-periods.³⁷ Similarly, the results estimated using the earnings change metric model show that the coefficient on $POST_{it} \times \Delta NI_{it} \times \Delta NI_{it}$ is -0.169 (t-statistic = -2.14, p-value = 0.008) in equation (4). The coefficient on $RESTATE_{it} \times \Delta NI_{it} \times \Delta NI_{it}$ is 0.097 (t-statistic = 2.23, p-value = 0.006) in equation (6). These results also indicate that restating firms in general report more conservatively in the post- than the pre-periods.

1.5 Conclusions

We examine the change in companies' income recognition policies after they disclose restatements to correct previous non-GAAP reporting. We focus on companies that overstated earnings, and argue that to restore credibility, such companies will undertake a more conservative income recognition strategy after the disclosures. We find that test companies increase conservatism of earnings subsequent to restatement announcements. The increased conservatism is evident using both a within-test-sample, before-and-after approach and a control-sample, cross-sectional approach. These

³⁷ We note however that the magnitudes of these coefficients are smaller than the corresponding coefficients reported in Tables 3 and 6.

findings are consistent using both a Basu-based (1997) earnings level model and a Basu-based earnings change model.

In additional analyses we find stronger post-period conservatism for test companies that simultaneously experience improvement in any of several corporate governance quality metrics than for unimproved test companies. This result supports our conjecture that the detectable increase in conservatism is associated with companies' improvements in corporate governance.

Our results provide additional empirical evidence (beyond that documented in prior literature) of remedial actions that restating companies undertake to restore their earnings credibility. Our results have implications for accounting research examining management behaviors responding to earnings management and revealed accounting failures. Accounting failure is the consequence of earnings management, which unavoidably impaired investors' trust. The increase in conservatism of earnings directly addresses investors' concern on earnings credibility. Second, although prior study largely debates on the power and reliability of alternative models to capture conservatism, our finding in four tests consistently indicate that companies increase conservatism after restatements. Our results have implications for future research investigating how specific events trigger the *changes* in earnings conservatism. Finally, our results extend Givoly et al. (2007) by investigating the sensitivity of Basu models in measuring conservatism in the restatement setting. Using a more extensive Basu-model and a large number of cross-sectional observations, we provide support for the use of Basu-based metric in our context.

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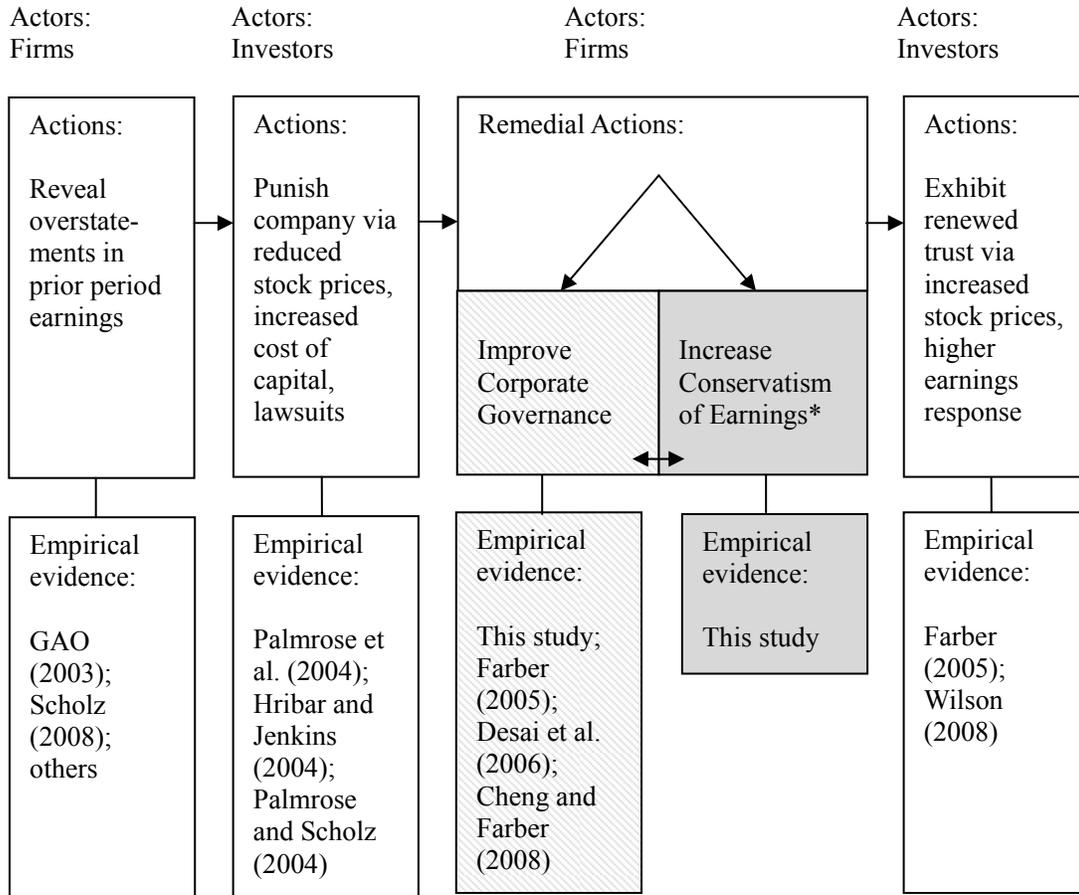
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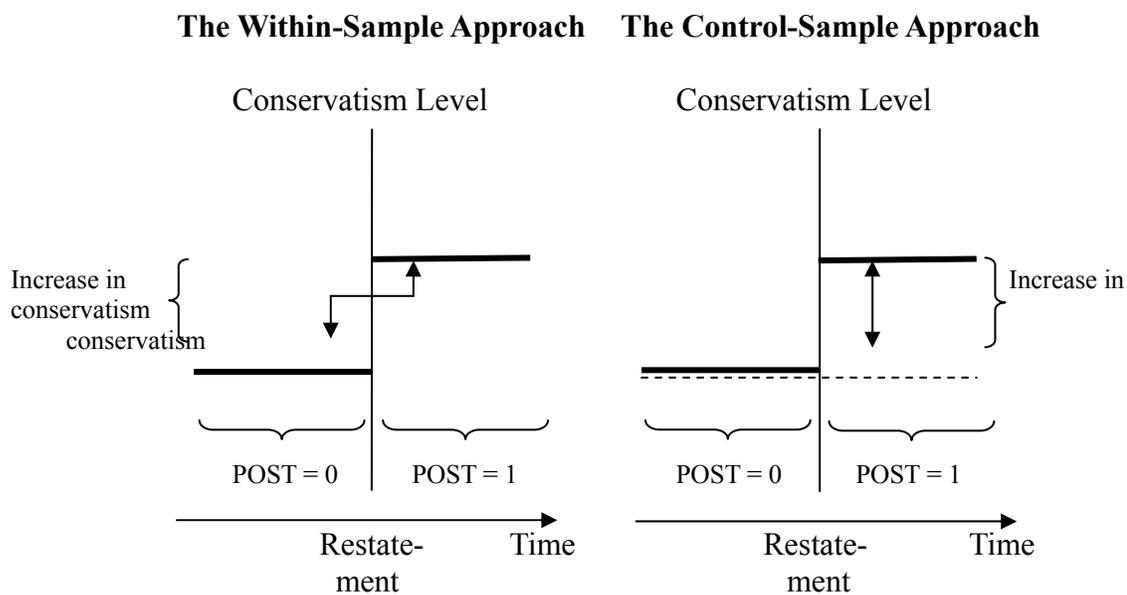
Figure 1.1 Conceptual Model of the Effects of Earnings Restatements



* Our measures of increased conservatism, for companies that correct prior overstatements, are based on Basu (1997), Ball and Shivakumar (1995), and LaFond and Roychowdhury (2008).

-  Indicates a contribution in the main analyses.
-  Indicates an expected positive association between improved governance and increased earnings conservatism.
-  Indicates a contribution in the additional analyses.

Figure 1.2 Sampling Approaches for Detecting Greater Conservatism Following Restatements



——— A horizontal solid line indicates the level of test (restating) company conservatism.
 - - - - - A horizontal dashed line indicates the level of control portfolio conservatism.
 Variable POST is coded = 0 in the pre- period and = 1 in the post- period.

Table 1.1 Sample Distribution by Calendar Year

Calendar year	Restatement Sample
	Number of observations
1999	77
2000	106
2001	280
2002	273
2003	338
2004	417
2005	641
Total	2,132

Table 1.2 Sample Descriptive Statistics for Within-Sample Models

Panel A: Sample for Earnings Metric Model of Equation (2)

	Mean	STD	Median	Q1	Q3
$POST_{it}$	0.277	0.447	0.000	0.000	1.000
NI_{it}	0.015	0.139	0.045	-0.011	0.078
NEG_{it}	0.423	0.494	0.000	0.000	1.000
RET_{it}	0.182	0.629	0.082	-0.194	0.385
$MVEBOOK_{it-1}$	3.702	23.228	1.979	1.273	3.275
$LEVERAGE_{t-1}$	0.225	0.194	0.204	0.044	0.351
MVE_{it-1}	5.756	1.912	5.652	4.367	6.939
LIT_{it-1}	0.181	0.385	0.000	0.000	0.000

Panel B: Sample for Earnings Change Metric Model of Equation (4)

	Mean	STD	Median	Q1	Q3
$POST_{it}$	0.270	0.444	0.000	0.000	1.000
ΔNI_{it+1}	0.011	0.119	0.004	-0.024	0.038
$D\Delta NI_{it}$	0.412	0.492	0.000	0.000	1.000
ΔNI_{it}	0.012	0.218	0.004	-0.026	0.039
$TOTAL\ ASSET_{it}$	4,140	2,0735	290	57	1,429

Variable Definitions:

$POST_{it}$ is an indicator variable that equals one if firm i has a restatement in the sample period and year t is in the post-restatement periods, and zero if year t is in the pre-restatement periods. NI_{it} is year t net income before extraordinary items, scaled by the beginning of fiscal year t market value of equity. RET_{it} is annual buy-and-hold return, calculated as returns from three months after fiscal year end in year $t-1$ to three months after fiscal year end in year t . NEG_{it} is an indicator variable that equals one if RET_{it} is

negative, and zero otherwise. $MVEBOOK_{it-1}$ is the market to book ratio at the beginning of the fiscal year t . $LEVERAGE_{it-1}$ is the total debt divided by total assets at the beginning of the fiscal year t . MVE_{it-1} is natural log market value of equity at the beginning of the fiscal year t . LIT_{it-1} is an indicator variable that equals one if a firm is in a litigious industry (SIC codes 2833 to 2836; 3570 to 3577; 3600 to 3674; 5200 to 5961, and 7370), and zero otherwise. ΔNI_{it} is change in net income before extraordinary items from year $t-1$ to year t , scaled by total assets at the beginning of year t . $D\Delta NI_{it}$ is an indicator variable that equals one if $\Delta NI_{it} < 0$, and zero otherwise. $TOTAL_ASSET_{it}$ is total assets of firm i at end of year t .

Table 1.3 Use Earnings Metric Model to Compare Conservatism between Post- and Pre-period

Dependent Variable = NI_{it}	Excluding Misstated Periods			Including Misstated Periods		
	Predicted Sign	Coef.	t-stat.	Coef.	t-stat.	
<i>INTERCEPT</i>	?	0.0056	0.41	0.0114	0.90	
<i>NEG_{it}</i>	?	0.0303	1.41	0.0223	1.11	
<i>POST_{it}</i>	?	-0.0114	-1.56	-0.0149	-2.17	**
<i>MTB_{it-1}</i>	?	0.0046	0.31	-0.0015	-0.11	
<i>LEV_{it-1}</i>	?	0.0159	1.17	0.0255	1.97	**
<i>SIZE_{it-1}</i>	?	0.0622	4.46	0.0503	3.89	***
<i>LIT_{it-1}</i>	?	-0.0357	-2.49	-0.0329	-2.48	***
<i>NEG_{it} × POST_{it}</i>	?	0.0050	0.34	0.0065	0.46	
<i>NEG_{it} × MTB_{it-1}</i>	?	-0.0702	-3.05	-0.0573	-2.53	***
<i>NEG_{it} × LEV_{it-1}</i>	?	-0.0075	-0.36	-0.0168	-0.88	
<i>NEG_{it} × SIZE_{it-1}</i>	?	0.0321	1.53	0.0403	2.00	**
<i>NEG_{it} × LIT_{it-1}</i>	?	-0.0150	-0.85	-0.0073	-0.47	
<i>RET_{it}</i>	+	0.0097	0.52	0.0032	0.18	
<i>RET_{it} × POST_{it}</i>	?	-0.0003	-0.02	0.0047	0.33	
<i>RET_{it} × MTB_{it-1}</i>	+	0.0037	0.14	0.0051	0.22	
<i>RET_{it} × LEV_{it-1}</i>	-	0.0218	0.88	0.0110	0.47	
<i>RET_{it} × SIZE_{it-1}</i>	+	-0.0454	-1.76	-0.0248	-1.09	*
<i>RET_{it} × LIT_{it-1}</i>	-	-0.0235	-1.36	-0.0195	-1.33	
<i>RET_{it} × NEG_{it}</i>	+	0.3322	4.77	0.3699	5.33	***
<i>RET_{it} × NEG_{it} × POST_{it}</i>	+	0.1379	2.83	0.1168	2.46	***
<i>RET_{it} × NEG_{it} × MTB_{it-1}</i>	-	-0.3798	-5.21	-0.3849	-5.11	***
<i>RET_{it} × NEG_{it} × LEV_{it-1}</i>	+	-0.0778	-1.26	-0.0893	-1.43	
<i>RET_{it} × NEG_{it} × SIZE_{it-1}</i>	-	0.1565	2.23	0.1299	1.89	*
<i>RET_{it} × NEG_{it} × LIT_{it-1}</i>	+	-0.0096	-0.21	-0.0040	-0.10	
Adj. R ²			0.1432		0.1450	
Number of Observations			4,021		4,681	

The sample period is from 1996 to 2006.

The models are estimated using the Huber-White procedure with clustering by companies.

POST_{it} is an indicator variable that equals one if firm *i* has only one overstatement in the sample period and year *t* is after the overstatement in the time-serial sample, and zero otherwise. *NI_{it}* is year *t* net income before extraordinary items scaled by the beginning of fiscal year *t* market value of equity. *RET_{it}* is annual return, calculated as returns from three months after fiscal year end date in year *t-1* to three months after fiscal year end date in year *t*. *NEG_{it}* is an indicator variable that equals one if *RET_{it}* is negative, and zero otherwise. *MTB_{it-1}* is the scaled decile rank of the market to book ratio at the beginning of the fiscal year *t*. *LEV_{it-1}* is the scaled decile rank of total debt divided by total assets at the beginning of the fiscal year *t*. *SIZE_{it-1}* is the scaled decile rank of market value of equity at the beginning of the fiscal year *t*. *LIT_{it-1}* is an indicator variable that equals one if a firm was in a litigious industry (SIC codes 2833 to 2836; 3570 to 3577; 3600 to 3674; 5200 to 5961, and 7370), and zero otherwise.

T-tests are two-tailed. *p*-values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise. ***, **, * indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Table 1.4
Use Earnings Change Metric to Compare Conservatism between Post- and Pre-period

Dependent Variable = ΔNI_{it+1}	<i>Excluding Misstated Periods</i>			<i>Including Misstated Periods</i>			
	Predicted Sign	Coef.	t-stat.		Coef.	t-stat.	
<i>INTERCEPT</i>	?	-0.0020	-0.30		-0.0022	-0.38	
$D\Delta NI_{it}$?	0.0116	1.12		0.0086	0.93	
ΔNI_{it}	?	0.0332	0.93		0.0366	1.23	
$D\Delta NI_{it} \times \Delta NI_{it}$	-	-0.1710	-3.15	***	-0.2277	-4.88	***
<i>POST</i> _{it}	?	0.0091	2.28	**	0.0095	2.61	***
$POST_{it} \times D\Delta NI_{it}$?	-0.0118	-1.57		-0.0111	-1.68	*
$POST_{it} \times \Delta NI_{it}$?	-0.0216	-0.61		-0.0273	-0.87	
$POST_{it} \times D\Delta NI_{it} \times \Delta NI_{it}$	-	-0.1465	-2.07	**	-0.1566	-2.46	***
<i>SIZED</i> _{it}	?	-0.0031	-0.34		-0.0042	-0.54	
$SIZED_{it} \times D\Delta NI_{it}$?	-0.0121	-0.84		-0.0081	-0.63	
$SIZED_{it} \times \Delta NI_{it}$?	-0.1007	-1.25		-0.0655	-0.92	
$SIZED_{it} \times D\Delta NI_{it} \times \Delta NI_{it}$?	-0.0605	-0.44		-0.0150	-0.12	
Adj. R ²		0.0708			0.0467		
Number of Observations		5,635			7,181		

The sample period is from 1996 to 2006.

The models are estimated using the Huber-White procedure with clustering by firms.

*POST*_{it} is an indicator variable that equals one if firm *i* has only one overstatement in the sample period and year *t* is after the overstatement in the time-serial sample, and zero otherwise. ΔNI_{it} is change in net income before extraordinary items from year *t-1* to year *t*, scaled by total assets at the beginning of year *t*. $D\Delta NI_{it}$ is an indicator variable that equals one if $\Delta NI_{it} < 0$, and zero otherwise. *SIZED*_{it} is rank of total assets at end of year *t*, standardized to vary between zero and one.

T-tests are two-tailed. *p*-values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise. ***, **, * indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Table 1.5 Sample Descriptive Statistics for Control-Sample Models

Panel A: Sample for Earnings Metric Model of Equation (5)

	Mean	STD	Median	Q1	Q3
$OVERSTAT_{it}$	0.073	0.260	0.000	0.000	0.000
NI_{it}	0.040	0.110	0.056	0.027	0.080
NEG_{it}	0.323	0.468	0.000	0.000	1.000
RET_{it}	0.190	0.457	0.130	-0.067	0.355
$MVEBOOK_{it-1}$	7.499	167.886	1.984	1.313	3.108
$LEVERAGE_{it-1}$	0.209	0.174	0.191	0.052	0.324
MVE_{it-1}	6.671	2.230	6.665	5.030	8.225
LIT_{it-1}	0.179	0.383	0.000	0.000	0.000

Panel B: Sample for Earnings Change Metric Model of Equation (6)

	Mean	STD	Median	Q1	Q3
$OVERSTAT_{it}$	0.071	0.256	0.000	0.000	0.000
ΔNI_{it+1}	0.010	0.093	0.006	-0.006	0.026
$D\Delta NI_{it}$	0.349	0.477	0.000	0.000	1.000
ΔNI_{it}	0.010	0.097	0.006	-0.007	0.026
$TOTAL_ASSET_{it}$	13,595	71,588	1,091	232	5,535

$OVERSTAT_{it}$ is an indicator variable that equals one if firm i has a restatement in the sample period and year t is in the post-restatement periods, and zero if firm i is a non-restating company.

Variable Definitions:

$POST_{it}$ is an indicator variable that equals one if firm i has a restatement in the sample period and year t is in the post-restatement periods, and zero if year t is in the pre-restatement periods. NI_{it} is year t net income before extraordinary items, scaled by the beginning of fiscal year t market value of equity. RET_{it} is annual buy-and-hold return, calculated as returns from three months after fiscal year end in year $t-1$ to three months after fiscal year end in year t . NEG_{it} is an indicator variable that equals one if RET_{it} is negative, and zero otherwise. $MVEBOOK_{it-1}$ is the market to book ratio at the beginning of the fiscal year t . $LEVERAGE_{it-1}$ is the total debt divided by total assets at the beginning of the fiscal year t . MVE_{it-1} is natural log market value of equity at the beginning of the fiscal year t . LIT_{it-1} is an indicator variable that equals one if a firm is in a litigious industry (SIC codes 2833 to 2836; 3570 to 3577; 3600 to 3674; 5200 to 5961, and 7370), and zero otherwise. ΔNI_{it} is change in net income before extraordinary items from year $t-1$ to year t , scaled by total assets at the beginning of year t . $D\Delta NI_{it}$ is an indicator variable that equals one if $\Delta NI_{it} < 0$, and zero otherwise. $TOTAL_ASSET_{it}$ is total assets of firm i at end of year t .

**Table 1.6 Use Earnings Metric to Compare Conservatism
between Overstating Firms and Non-overstating Peer Firms**

Dependent Variable = NI_{it}	Excluding Misstated Periods			Including Misstated Periods		
	Predicted Sign	Coef.	t-stat.		Coef.	t-stat.
<i>INTERCEPT</i>	?	0.0444	8.36 ***		0.0456	8.62 ***
<i>NEG_{it}</i>	?	0.0067	0.65		0.0045	0.44
<i>OVERSTAT_{it}</i>	?	-0.0144	-1.81 *		-0.0152	-1.92 *
<i>MTB_{it}</i>	?	-0.0132	-1.80 *		-0.0139	-1.90 *
<i>LEV_{it}</i>	?	0.0049	0.75		0.0078	1.21
<i>SIZE_{it}</i>	?	0.0346	4.61 ***		0.0310	4.13 ***
<i>LIT_{it}</i>	?	-0.0221	-3.90 ***		-0.0205	-3.61 ***
<i>NEG_{it} × OVERSTAT_{it}</i>	?	0.0134	0.84		0.0138	0.87
<i>NEG_{it} × MTB_{it-1}</i>	?	-0.0056	-0.38		0.0021	0.15
<i>NEG_{it} × LEV_{it-1}</i>	?	0.0010	0.08		-0.0013	-0.10
<i>NEG_{it} × SIZE_{it-1}</i>	?	-0.0150	-1.03		-0.0158	-1.09
<i>NEG_{it} × LIT_{it-1}</i>	?	0.0129	1.23		0.0097	0.93
<i>RET_{it}</i>	+	0.0147	1.72		0.0146	1.71 *
<i>RET_{it} × OVERSTAT_{it}</i>	?	-0.0094	-0.69		-0.0088	-0.64
<i>RET_{it} × MTB_{it-1}</i>	+	0.0225	1.89		0.0242	2.04 ***
<i>RET_{it} × LEV_{it-1}</i>	-	-0.0012	-0.11		-0.0045	-0.41
<i>RET_{it} × SIZE_{it-1}</i>	+	-0.0173	-1.20		-0.0153	-1.07
<i>RET_{it} × LIT_{it-1}</i>	-	-0.0431	-5.04 ***		-0.0460	-5.39 ***
<i>RET_{it} × NEG_{it}</i>	+	0.4588	13.94 ***		0.4484	13.81 ***
<i>RET_{it} × NEG_{it} × OVERSTAT_{it}</i>	+	0.1628	3.06 ***		0.1653	3.10 ***
<i>RET_{it} × NEG_{it} × MTB_{it-1}</i>	-	-0.4678	-10.65 ***		-0.4527	-10.53 ***
<i>RET_{it} × NEG_{it} × LEV_{it-1}</i>	+	-0.0668	-1.59		-0.0492	-1.20
<i>RET_{it} × NEG_{it} × SIZE_{it-1}</i>	-	-0.0188	-0.40		-0.0264	-0.57
<i>RET_{it} × NEG_{it} × LIT_{it-1}</i>	+	0.0933	3.28 ***		0.0928	3.32 ***
Adj. R ²			0.1683			0.1651
Number of Observations			6,676			6,835

The sample period is from 1996 to 2006.

Variable Definitions:

OVERSTAT_{it} is an indicator variable that equals one if firm *i* has only one overstatement in the sample period and year *t* is after the overstatement year in the cross-sectional sample, and zero otherwise. ***NI_{it}*** is year *t* net income before extraordinary items scaled by the beginning of fiscal year *t* market value of equity. ***RET_{it}*** is annual return, calculated as returns from three months after fiscal year end date in year *t-1* to three months after fiscal year end date in year *t*. ***NEG_{it}*** is an indicator variable that equals one if ***RET_{it}*** is negative, and zero otherwise. ***MTB_{it-1}*** is the scaled decile rank of the market to book ratio at the beginning of the fiscal year *t*. ***LEV_{it-1}*** is the scaled decile rank of total debt divided by total assets at the beginning of the fiscal year *t*. ***SIZE_{it-1}*** is the scaled decile rank of market value of equity at the beginning of the fiscal year *t*. ***LIT_{it-1}*** is an indicator variable that equals one if a firm was in a litigious industry (SIC codes 2833 to 2836; 3570 to 3577; 3600 to 3674; 5200 to 5961, and 7370), and zero otherwise.

T-tests are two-tailed. *p*-values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise. ***, **, * indicate significance levels of 0.01, 0.05, and 0.10, respectively.

**Table 1.7 Use Earnings Change Metric to Compare Conservatism
between Overstating Firms and Non-Overstating Peer Firms**

Dependent Variable = ΔNI_{it+1}	Excluding Misstated Periods			Including Misstated Periods		
	Predicted Sign	Coef.	t-stat.	Coef.	t-stat.	
<i>INTERCEPT</i>	?	0.0115	5.12 ***	0.0113	4.95 ***	***
<i>DANI_{it}</i>	?	-0.0133	-3.67 ***	-0.0170	-4.55 ***	***
<i>ANI_{it}</i>	?	-0.1516	-7.93 ***	-0.1664	-8.78 ***	***
<i>DANI_{it} × ANI_{it}</i>	–	-0.2369	-7.21 ***	-0.2664	-7.63 ***	***
<i>OVERSTAT_{it}</i>	?	-0.0051	-1.23	-0.0043	-1.02	
<i>OVERSTAT_{it} × DANI_{it}</i>	?	0.0155	2.20 **	0.0164	2.28 **	**
<i>OVERSTAT_{it} × ANI_{it}</i>	?	0.1186	2.64 ***	0.1348	2.93 ***	***
<i>OVERSTAT_{it} × DANI_{it} × ANI_{it}</i>	–	-0.2281	-2.98 ***	-0.2026	-2.62 ***	***
<i>SIZED_{it}</i>	?	-0.0076	-2.06 **	-0.0073	-1.96 **	**
<i>SIZED_{it} × DANI_{it}</i>	?	0.0124	1.98 **	0.0174	2.72 ***	***
<i>SIZED_{it} × ANI_{it}</i>	?	0.2516	5.06 ***	0.2388	4.79 ***	***
<i>SIZED_{it} × DANI_{it} × ANI_{it}</i>	?	-0.2333	-2.97 ***	-0.1158	-1.42	
Adj. R ²			0.0974		0.0953	
Number of Observations			6,293		6,422	

The sample period is from 1996 to 2006.

Variable Definitions:

OVERSTAT_{it} is an indicator variable that equals one if firm *i* has only one restatement in the sample period and year *t* is after the restatement year in the cross-sectional sample, and zero otherwise. *ANI_{it}* is change in net income before extraordinary items from year *t-1* to year *t*, scaled by total assets at the beginning of year *t*. *DANI_{it}* is an indicator variable that equals one if *ANI_{it}* < 0, and zero otherwise. *SIZED_{it}* is rank of total assets at end of year *t*, standardized to vary between zero and one.

T-tests are two-tailed. *p*-values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise. ***, **, * indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Table 1.8 Comparison of Conservatism in Subsamples Partitioned on w and w/o Corporate Governance Improvement

Dependent Variable = NI

	% of Insider Holding				% of Insiders in Board				Board Size			
	Improved		Unimproved		Improved		Unimproved		Improved		Unimproved	
	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.
$POST_{it}$	0.004	0.84	-0.021	-1.88	-0.007	-1.02	0.036	1.87	-0.008	-0.84	0.010	0.83
$NEG_{it} \times POST_{it}$	-0.002	-0.24	-0.004	-0.16	0.008	0.56	-0.088	-2.02	0.016	0.80	-0.039	-1.66
$RET_{it} \times POST_{it}$	-0.010	-0.93	0.032	1.46	0.017	1.35	-0.192	-4.74	0.018	1.19	-0.039	-1.42
$RET_{it} \times NEG_{it} \times POST_{it}$	0.086	2.19	0.018	0.21	0.103	2.15	-0.038	-0.22	0.139	2.12	0.002	0.02
Adj. R2	0.2111		0.2016		0.1332		0.2866		0.1446		0.1220	
Number of Observations	784		555		1,051		288		654		685	

The sample period is from 1996 to 2006.

% of Insider Holding is the percentage of shares held by insiders. **% of Insiders in Board** is the percentage of board directors being insiders. **Board Size** is the number of directors in the board. Improved subsamples consist of companies that exhibit (1) decreased insider holdings, (2) decreased percentages of insiders on the board of directors, and (3) increased board sizes.

For brevity, this table only reports the results on interest variable. For these results, we run the regression of Equation (2).

T-tests are two-tailed. p -values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise. ***, **, * indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Table 1.9 Use Earnings Metric to Compare Conservatism between Overstating Firms and Non-restating Firms in the Pre-restatement period

Dependent Variable = NI_{it}	Predicted				p-value	
	Sign	Coefficient	t-statistics			
<i>INTERCEPT</i>	?	0.040	12.23	0.000	***	
<i>NEG_{it}</i>	?	0.007	1.06	0.290		
<i>OVERSTAT_{it}</i>	?	-0.010	-2.98	0.003	***	
<i>MTB_{it}</i>	?	-0.025	-5.52	0.000	***	
<i>LEV_{it}</i>	?	0.016	3.99	0.000	***	
<i>SIZE_{it}</i>	?	0.041	9.40	0.000	***	
<i>LIT_{it}</i>	?	-0.055	-16.95	0.000	***	
<i>NEG_{it} × OVERSTAT_{it}</i>	?	0.010	1.54	0.124		
<i>NEG_{it} × MTB_{it-1}</i>	?	-0.018	-2.12	0.034	**	
<i>NEG_{it} × LEV_{it-1}</i>	?	-0.002	-0.26	0.800		
<i>NEG_{it} × SIZE_{it-1}</i>	?	0.014	1.69	0.091	*	
<i>NEG_{it} × LIT_{it-1}</i>	?	0.023	3.83	0.000	***	
<i>RET_{it}</i>	+	-0.019	-6.58	0.000	***	
<i>RET_{it} × OVERSTAT_{it}</i>	?	0.010	3.36	0.001	***	
<i>RET_{it} × MTB_{it-1}</i>	+	0.006	1.55	0.061	*	
<i>RET_{it} × LEV_{it-1}</i>	-	0.004	1.14	0.127		
<i>RET_{it} × SIZE_{it-1}</i>	+	0.015	3.18	0.000	***	
<i>RET_{it} × LIT_{it-1}</i>	-	-0.002	-0.71	0.239		
<i>RET_{it} × NEG_{it}</i>	+	0.496	32.06	0.000	***	
<i>RET_{it} × NEG_{it} × OVERSTAT_{it}</i>	-	-0.028	-2.08	0.019	**	
<i>RET_{it} × NEG_{it} × MTB_{it-1}</i>	-	-0.356	-19.51	0.000	***	
<i>RET_{it} × NEG_{it} × LEV_{it-1}</i>	+	0.011	0.67	0.253		
<i>RET_{it} × NEG_{it} × SIZE_{it-1}</i>	-	-0.106	-5.60	0.000	***	
<i>RET_{it} × NEG_{it} × LIT_{it-1}</i>	+	0.004	0.32	0.375		
Adj. R ²			13.16%			
Number of Observations			42,975			

The sample period is from 1996 to 2006.

OVERSTAT_{it} is an indicator variable that equals one if firm *i* has only one overstatement in the sample period and year *t* is before the overstatement year in the cross-sectional sample, and zero otherwise. *NI_{it}* is year *t* net income before extraordinary items scaled by the beginning of fiscal year *t* market value of equity. *RET_{it}* is annual return, calculated as returns from three months after fiscal year end date in year *t-1* to three months after fiscal year end date in year *t*. *NEG_{it}* is an indicator variable that equals one if *RET_{it}* is negative, and zero otherwise. *MTB_{it-1}* is the scaled decile rank of the market to book ratio at the beginning of the fiscal year *t*. *LEV_{it-1}* is the scaled decile rank of total debt divided by total assets at the beginning of the fiscal year *t*. *SIZE_{it-1}* is the scaled decile rank of market value of equity at the beginning of the fiscal year *t*. *LIT_{it-1}* is an indicator variable that equals one if a firm was in a litigious industry (SIC codes 2833 to 2836; 3570 to 3577; 3600 to 3674; 5200 to 5961, and 7370), and zero otherwise. T-tests are two-tailed. *p*-values are one-tailed when the sign of the coefficient is predicted, two-tailed otherwise. ***, **, * indicate significance levels of 0.01, 0.05, and 0.10, respectively.

**STUDY TWO: AN ANALYSIS OF WHEN AND WHY MARKET
REACTIONS TO RESTATEMENT ANNOUNCEMENTS ATTENUATE**

ABSTRACT

I investigate the market reactions to 3,520 restatement announcements during 1995-2006, and find an 85 percent reduction in the average market reaction beginning in the first quarter of 2001, and continuing through the end of the sample period. I analyze whether the reduction is due to changes in the frequencies of specific restatement characteristics, changes in the way the market reacts to the characteristics, or both. My results suggest that reduced market reactions are mainly associated with a change in the frequencies of restatement characteristics, including the severity of the restatements, changes in the way they are disclosed, and changes in the disclosures of parties that initiate restatements. Nearly half of the reduction in market reactions appears to be due to an increase in the frequency of less severe restatements.

Key Words: market reaction, misstatement, restatement, disclosure

2.1 Introduction

This study investigates the timing of and reasons for a dramatic change in market reactions to restatement announcements. Although prior literature has documented sharp stock price declines as well as other adverse consequences following restatement announcements (Palmrose et al. 2004; GAO 2003), recent studies have noted that the magnitude of market reactions to financial restatements has become much less negative in recent years than in earlier years (Hranaiova and Byers 2007, Burks 2009, Scholz 2008, Wang and Yu 2008, Audit Analytics 2009). This recent evidence raises questions of when and why market reactions to restatement announcements attenuate. Do investors believe restatements are not as severe as before, or that the reporting environment in general and related restatement characteristics have changed, as news media and other studies have argued (Scholz 2008, Smith and Hirschey 2009, Wang and Yu 2008)?

This study provides new empirical evidence about when and why the change in market reactions happened. Several recent studies arguably imply that the reduced market reactions to restatement announcements can be attributed to the passage of Sarbanes-Oxley (SOX) (Burks 2009, Smith and Hirschey 2009, Wang and Yu 2008). However, no real evidence is found in those studies to support the SOX argument. Burks (2009) and Smith and Hirschey (2009) assume that the reduction happened after the passage of SOX. Burks (2009) investigates post-announcement drift and trading volume of restatement companies before versus after SOX. He finds that neither post-announcement drift nor trading volume explains the reduced market reactions. Similarly, Smith and Hirschey (2009) investigate a wide range of possible explanations surrounding the SOX year and conclude that none of them is the cause

of the reduced market reactions.

In contrast, prior studies investigating the timing of the reduced market reactions provide different evidence that the shift might have happened before the passage of SOX. Hranaiova and Byers (2007) find that market reactions to restatements become much less negative in magnitude after the Justice Department announced a criminal investigation of Enron (January 2002). Although their study does not investigate the periods before the Enron investigation, it suggests a possibility that the reduction could have happened earlier. However, Scholz (2008) suggests the turning point of the decrease is the year of 2001 and finds that market reactions decrease from -10 percent for years before 2001 to -1 percent for years after 2001.

Because of the mixed evidence in prior studies, this study does not presume that the shift in market reactions is due to SOX or any other events, but rather begins with an investigation of when the shift occurred. Using a sample of restatement announcements across a twelve-year period from 1995 to 2006, I find that a reduction in market reactions occurred in the first calendar quarter of year 2001. This finding is consistent with Scholz (2008) which provides the first evidence that the shift happened in 2001. My study extends Scholz (2008) by investigating 2001 quarterly market reactions, and finds that the shift happened in the first calendar quarter of 2001, and that smaller reactions continue within the year and persist from 2001 through 2006. On average, there is an 85 percent reduction in the magnitude of market reactions in 2001-2006 relative to 1995-2000. Focusing on the years surrounding the change, average market returns in 2000 are -10.6 percent, similar to returns during the years 1995-1999. Average market returns in 2001 are -1.6 percent, similar to those during 2002-2006.

Thus, the shift occurs nearly a year before Enron announced its restatement in November 2001, and about a year and a half before the passage of SOX in July 2002. Therefore, the change does not appear to be directly associated with either SOX or its precipitating accounting scandals. Although Scholz (2008) finds decreased market reactions happened in 2001, that study leaves several questions open. For example, why do market reactions to restatement attenuate? What is driving reduced reactions? Do less material restatement dollar amounts impact the decreased reactions? Does the way restatements are disclosed impact the decreased reactions? Is the change in attribution parties of restatements related to the dampened market reactions? After determining the timing of the shift is the first quarter of 2001, my paper extends Scholz (2008) by investigating these unanswered questions.

Specifically, I examine the association between the reduced reaction and changes in restatement characteristics by employing three groups of restatement characteristics that have been found to be determinants of announcement returns in prior literature: severity of the error(s) restated (whether fraud was involved, etc.), restatement disclosure venues (whether the restatement was announced in a press release and whether or not the restatement amount was quantified), and who initiated the restatements (i.e. companies, auditors, etc.).

I investigate two possible explanations for the reduced market reactions. First, the average reduction could result from changes in the frequencies of specific restatement characteristics as discussed above. Second, the reduced reactions could be due to changes in the market's response to those characteristics. For example, concurrent studies posit that smaller market reactions occurred because investors have developed "restatement fatigue" due to the large number of restatements in recent years (i.e.

Plumlee and Yohn 2010). Investors might have revised upward their estimated probability that additional companies would disclose restatements in the future. This could have led to dampened market reactions when additional restatements were announced (Plumlee and Yohn 2010).

My analysis suggests the overall reduction in market reactions is driven mainly by a change in the mixture of restatement characteristics, rather than a change in market's response to specific characteristics. Specifically, there is a greater proportion of non-severe restatements revealed in 2001, compared to 2000. The attenuated market returns are also due to changes in the parties to whom restatements are attributed and to disclosure venues.

In additional analyses, I explore other possible explanations by investigating pre-announcement returns and market volatility. Investors' expectation of future restatements might cause the stock price to decline prior to the restatement announcement day (Plumlee and Yohn 2010). Therefore, increased pre-announcement negative market returns may drive the reduced announcement effects. Additional analyses suggest that the dampened stock reactions are neither due to changes in investors' anticipation of restatements prior to announcement dates, nor due to stock market volatility.

My study contributes to the restatement literature in three ways. First, recent studies provide mixed evidence on the timing of the dampened market reactions to restatement announcements. Scholz (2008) indicates the turning point is in the year of 2001; Hranaiova and Byers (2007) suggest the shift happened since the investigation of Enron; other recent studies arguably imply that the turning point is the passage of SOX in 2002 (Wang and Yu 2008, Smith and Hirschey 2009, Burks 2009). Based on

the empirical analysis of market reactions over the twelve year window from 1995 to 2006, my study provides new evidence that the turning point at which the average market reactions to restatements decreased dramatically is the first calendar quarter of 2001. The turning point of the negative reactions identified in this study is consistent with Scholz (2008) but extends her study by examining the shift point *within* 2001 and providing evidence on the persistence in quarterly and yearly returns since the turning point.

Second, prior studies document the determinants of market reactions to restatement announcements (Palmrose et al. 2004, Anderson and Yohn 2002). This study extends prior literature on the determinants of market reactions by investigating what is causing market reactions to attenuate in recent years. Extant studies only provide evidence on what factors are *not* the cause of reduced market reactions (Burks 2009, Smith and Hirschey 2009). These non-factors include changes in industry mix, restatement type, economic environment, stock-market environment, contagion effects, post-announcement drift and trading volume (Burks 2009, Smith and Hirschey 2009). None of the prior studies explain what changes *are* associated with the shift in market reactions to restatements. My study fills this gap by investigating the underlying cause of the shift in market reactions to restatement announcements. My results suggest that the shift in market reactions is mainly associated with the change in the mix of restatement characteristics, rather than the change in the market's response to characteristics. In additional analyses, I investigate investor anticipation of restatements before announcements (measured by pre-announcement returns) and market volatility, and find neither of those two factors is associated with the reduced market reactions.

Third, my study is the first to disaggregate the shift in average market reactions into the impact of three groups of restatement characteristics. This disaggregation quantitatively adds to our understanding of this shift in market reactions. I find that nearly half of the reduction in market reactions is due to an increase in the frequency of less severe restatements. In order of magnitude, the reduction in market reactions is also associated with a change in attributing parties and disclosure venues.

The rest of this paper is organized as follows. Section 2 discusses related studies. Section 3 describes the sample, the timing of the reduced market reactions, and the research design including the proxies for restatement characteristics. The univariate results are presented in Section 4. Section 5 reports the multivariate results and Section 6 discusses additional tests. Conclusions are provided in section 7.

2.2 Background and Related Literature

Companies are required to restate their financial results when material errors are found in financial statements previously filed with the SEC (Security and Exchange Commission). As the number of restatements has increased dramatically in recent years, the large number and their serious implications have raised concerns among investors, analysts and regulators (Wilson 2008). Prior literature has documented that financial restatements impose a large cost on the U.S. capital market and on investors (Levitt 1998, GAO 2003). Studies of restatements announced during the 1990s report average market reactions of about -10 percent (GAO 2003, Palmrose et al. 2004). These studies find that the price declines are associated with restatement severity, including the presence of fraud and more negative effects on reported income.

However, studies also find that market reactions to restatement announcements are much less negative in recent years (Scholz 2008, Hranaiova and Byers 2007, Burks 2009, Wang and Yu 2008, Audit Analytics 2009). Similar to these studies, I focus on two important issues: the timing and the underlying cause of the change in market reactions. Several of the recent studies find market reactions to restatements differ before and after SOX, and attribute the reduced market reactions to SOX-related effects. For example, Burks (2009) finds that the market returns over restatement announcement windows decreased from -9.7 percent before SOX to -3.0 percent after SOX, based on the GAO sample of 1,387 restatements announced from 1997 to September 2005. He posits that the reduced market reactions mean that investors are not confused by the high frequency of restatements after the passage of SOX. However, after investigating post-announcement drifts and trading volumes in pre- versus post-SOX periods, Burks (2009) finds no evidence that either price drift or volume changes are associated with the reduced market reactions.

Similar to Burks (2009), Smith and Hirschey (2009) find market reactions decrease from about -7 percent before SOX to about -1 percent after the passage of SOX (pre- vs. post-July 2002). Interestingly, they find a sharp decrease in market reactions to restatement announcements beginning early in 2001. However, they attribute this timing of the decrease to an effect of SOX. They find the reduced market effect is not associated with change in restating firms' industries, restatement types, economic environment, stock-market environment, or contagion effects. Therefore, they speculate an increasingly conservative reporting environment in the post-SOX era may be responsible for the dramatic decline in market reactions.

Another contemporary study by Wang and Yu (2008) also attributes the reduced

market reactions to restatements to SOX effects. They argue that SOX increases the possibility of detecting errors in previously issued financial statements. Investigating a sample of voluntary restatements (i.e. restatements initiated by management) before versus after SOX, they find the shift in market reactions is associated with an increase in restatements of quarterly financial reports to correct errors that occurred before SOX.

Hranaiova and Byers (2007) report that the market announcement return is reduced from -9 percent pre-SOX (before July 30, 2002) to -1.2 percent post-SOX (after July 30, 2002) using a sample of 1,728 hand collected restatements from 1998 to 2005. They also attribute some of the shift to the Enron scandal, noting there is a reduction from -10 percent for years preceding the Justice Department's investigation of Enron beginning January 10, 2002, to -1.4 percent after that date. They attribute the shift to the effects of SOX, and argue that investors might perceive information conveyed by restated financials as more timely and of higher quality.

Scholz (2008) finds that restatement frequencies begin to accelerate in 2001; however, market reactions to them decrease from -10 percent for years before 2001 to -1 percent for restatements after 2001. Her study identifies a turning point of market reactions in 2001, and documents changes in several restatement characteristics and in the reactions to them. However, Scholz (2008) leaves several questions open. For example, her study does not directly examine what is driving the shift. Also, it does not provide evidence on whether the magnitude of the restatement is related to dampened market reactions, or how initiating parties and disclosure venues are associated with the shift. In addition, Scholz (2008) does not examine whether the association between the restatement characteristics and the market reactions changed

at the turning point. This study extends Scholz (2008) by investigating these open questions.

My study differs from extant studies in three ways. First, I identify the timing of the shift based on empirical analyses, rather than assume it is due to a specific event. The Enron investigation, passage of SOX, or SOX 404 compliance requirements, may or may not affect market reactions to restatements. Second, I explicitly analyze whether reduced market reactions are due to the change in the market's response to specific restatement characteristics at the identified turning point. Third, I disaggregate the mean market reactions to determine the effects of three categories of restatement characteristic proxies. Thus, I quantitatively examine the relations between the reduction in mean market reactions and the impact of each proxy respectively.

2.3 Sample and Research Design

2.3.1 Restatement Sample

My initial restatement sample consists of restatements to correct errors or irregularities announced by U.S. companies from January 1, 1995 through December 31, 2006. I identify the restatement sample from three sources. (1) I use the Palmrose and Scholz (2004) sample of restatements from 1995-2000, identified via key-word searches in Lexis-Nexis Library and SEC Filing Library for terms such as restat, revis, adjust and error. (2) Restatements announced from 2000 to 2006 are obtained from Audit Analytics (AA). (3) I conduct key-word searches in Lexis-Nexis Library and SEC Filing Library for both 2000 and 2001 to validate my previous two sources. When multiple disclosures about the same restatement event exist, I compare the

dates of the disclosures and use the earliest disclosure as the restatement announcement date. After excluding announcements for which CRSP market return data are unavailable, the initial sample includes 3,520 restatements announced over the twelve years from 1995 to 2006.

2.3.2 Timing of the Reduced Market Reactions

Figure 1 presents the distribution of the 3,520 restatement announcements with market return data from 1995 through 2006. Consistent with extant studies, the frequency of restatements in my sample increased dramatically over the sample period (Burks 2009, Hranaiova and Byers 2007, Scholz 2008). The frequency is generally increasing each year, from 38 announcements in 1995 to 751 in 2005, before decreasing somewhat in 2006.³⁸ While investigation of restatement frequency is beyond the scope of this study, it is important to note that the increasing number of restatements began well before the scandal/SOX year of 2002, nearly doubling from 140 announcements in 2000, to 259 in 2001.

The lower part of Figure 1 (the solid lines connecting dots) shows average market reactions using cumulative abnormal returns (CAR) for the two-day announcement window, days [0, 1].³⁹ From 1995-1999 CARs are meaningfully negative, -9.5 percent on average. These returns are consistent with prior studies of the earlier period.⁴⁰ Figure 1 indicates a significant reduction in average market reactions beginning in year 2001, when average returns are only -1.6 percent. The reduced

³⁸ The unusually high number of restatements in 1999 is mainly due to a wave of SEC mandated adjustments of In-Process Research and Development. 1999 has 75 restatements involving only IPR&D corrections. Without these restatements, 1999 has 113 restatements, only slightly greater than the number of restatements in 1998. Scholz (2008) provides a comprehensive timeline of restatement frequency and related events.

³⁹ CARs on both day 0 and day 1 are significantly negative. P-values are 0.000 in two-tailed t-tests.

⁴⁰ For example, Palmrose et al. (2004) study restatements announced from 1995 to 1999, and report average market-adjusted announcement returns of -9.2 percent. The GAO (2003) uses a sample of 689 restatements announced from 1997 to March 2002 and finds an average return of -10.0 percent.

reactions persist from 2001 through the end of the sample period. Although small, returns during the latter period are statistically negative in every year except 2003, when the average annual return is just -0.2 percent. The most negative is 3.6 percent in the scandal/SOX year of 2002. However, market returns in 2002 do not differ significantly from those in 2001. Again, the signs and magnitudes of returns over this period are consistent with studies based on restatements announced in more recent years (Scholz 2008, Hranaiova and Byers 2008, Wang and Yu 2008, Audit Analytics 2009).

The timing of the turning point found in this study is similar to the finding of Scholz (2008). To further verify that year 2001 was an inflection point, I performed two additional analyses beyond Scholz (2008). I divide the entire sample period into two sub-samples; panel A of table 1 reports the comparison of CARs between the two sub-samples: 1995-1999 versus 2000 and 2002-2006 versus 2001. It shows that on average, CARs in 2000 are statistically indistinguishable from CARs in 1995-1999 (t-statistic = 0.60). Similarly, on average, CARs in 2001 do not differ significantly from the average market reactions during 2002-2006 (t-statistic = 0.52). In addition, the year 2000 CARs do not differ statistically from any individual year from 1995 to 1999, and the year 2001 CARs do not differ statistically from any individual year from 2002 through 2006.

Second, to further identify the timing of the reduction in market reactions and to study whether the reduced reactions are consistent *within* 2001, I examine quarterly CARs in 2000 and 2001 in Figure 2. Figure 2 shows a similar quarterly pattern across both years. That is, announcement returns are more negative in the first and fourth quarters and less negative in the second and third quarters in both years. However,

average returns in each quarter of 2001 are significantly smaller than in the corresponding quarter of 2000 (t-statistic ranges from -3.38 to -2.18), except the second quarter (t-statistic = -1.41).⁴¹

In summary, there is an 85 percent reduction in the magnitude of the market reaction from 2000 to 2001, and from 1995-2000 to 2001-2006. Data indicate the shift occurred in the first quarter of year 2001, nearly a year before Enron announced its restatement in November 2001, and a year and a half before SOX passed in July 2002. The bursting of the “Tech Bubble” happened at about this time in March 2000. After the bubble, the more stringent reporting environment might have led auditors and companies to be more conservative on reported earnings and materiality decisions, leading to many corrections of old errors with small amounts. Thus, the reduction may result from a change in the mixture of restatement characteristics, a change in the associations between market reactions and restatement characteristics, or a combination of both.

To identify the factors that are associated with the shift in average market reactions, I focus on the years 2000 and 2001. As shown in panel A of table 1, the mean market reactions for 2000 and 2001 are representative of the longer pre- and post- shift periods. I focus on these years and not the full sample period for three reasons. First, the factors that influence the reporting environment and associated restatement characteristics could change from year to year. These factors include the passage of SOX in 2002, lease accounting restatements in 2005, and stock option backdating in 2006. The effects of these factors could dilute the 2001 market reactions change, if I use a longer window comparison of 1995-2000 versus

⁴¹ T-statistic = -3.38 for Q2, -1.41 for Q2, -2.18 for Q3, -2.50 for Q4.

2001-2006. Second, some have argued that the reduced reaction is because of “restatement fatigue”, which results from more conservative auditing (Plumlee and Yohn 2010). Auditors have become more conservative in their decisions about whether errors are material and need to be corrected. Therefore, there are more restatements correcting smaller earnings amounts. Focusing on the comparison between 2000 and 2001 enables me to refute this argument to some degree because the number of restatements in year 2001 is not much larger than in 1999-2000 but the reduction in market reactions is already evident. Third, my analysis requires some hand-collected data, which can be obtained at reasonable cost for the restatements in 2000 and 2001, but not for all 3,520 restatements across the longer period.⁴²

2.3.3 Measures of Change in Market Reactions

I use the following model to examine change in mean market reactions to restatement announcements from 2000 to 2001. Model (1) tests whether the change in year from 2000 to 2001 has an incremental effect on the magnitude of market reactions, controlling for other factors thought to be associated with announcement returns.

$$CAR = \beta_0 + \beta_{1a-1f} \text{ Severity Proxies} + \beta_{2a-2b} \text{ Disclosure Attributes} + \beta_{3a-3d} \text{ Attribution Parties} + \beta_4 Y2001 \dots \dots \dots (1)$$

Following prior studies, I include three categories of restatement characteristics: severity of error(s) restated, disclosure venues, and parties to whom the restatements are attributed. Variable definitions are summarized in the Appendix. This model

⁴² I hand collect data including dollar amounts of restatements (restated net income minus original net income), the initiating parties of restatements, the number of account groups restated, the number of years restated, and the indicator of quantification effects in the initial disclosures. Audit Analytics provides part of those data for later years.

constrains the coefficient estimates to be the same for each restatement characteristic in both years 2000 and 2001, except for the intercept shift. While the three groups of proxies for characteristics of restatements likely capture the associations with announcement market returns, the intercept shift that is not captured by the three groups of proxies will be reflected by the indicator variable *Y2001*, which equals “one” if one restatement is announced in calendar year 2001. I expect the coefficient of the variable *Y2001*, β_4 , to be positive, consistent with the reduced negative market reactions to restatement announcements beginning in 2001.

While model (1) captures the intercept shift between 2000 and 2001, an extended model allows for variation in the association between market returns and restatement characteristics for each variable. Thus, in model (2), I interact *Y2001* with the restatement characteristic variable to allow differential associations between market returns and restatement characteristics from 2000 to 2001. The stand alone variables capture the associations between each proxy and market returns in 2000. The interactions capture the *change* in the associations from 2000 to 2001.

$$\begin{aligned} \text{CAR} = & \beta_0 + \beta_1 \text{Severity Proxies} + \beta_2 \text{Disclosure Attributes} + \beta_3 \text{Attribution Parties} \\ & + \beta_4 \text{Severity Proxies} \times \text{Y2001} + \beta_5 \text{Disclosure Attributes} \times \text{Y2001} \\ & + \beta_6 \text{Attribution Parties} \times \text{Y2001} + \beta_7 \text{Y2001} \dots \dots \dots \quad (2) \end{aligned}$$

Next, I describe the three categories of restatement characteristics in detail. Prior studies show a negative relation between the severity of errors and market announcement returns (Anderson and Yohn 2002, Wu 2002, Palmrose et al. 2004). I expect that restatements to correct severe errors are associated with more negative market reactions. The six proxies for severity of errors include: *Fraud involved, Revenue restated, Income impact, Income to loss, No. accounts, and No. years.*

A restatement is serious if the errors involve fraudulent reporting (*Fraud involved*), restate previously recognized revenue (*Revenue restated*), correct a greater dollar amount (*Income impact*)⁴³, reverse a net income to a net loss (*Income to loss*), restate more accounts (*No. accounts*) and affect longer periods (*No. years*). I expect each proxy to be negatively associated with the market returns except for *No. years*. Evidence on *No. years* is mixed. Using a sample of 438 restatements over the period of 1997 to 2005, Wang and Yu (2008) find that the duration of errors over time is positively associated with market reactions to restatements. The positive relation is stronger in the period before the passage of SOX. They interpret their findings as evidence that market participants perceive restatements with longer duration as a signal that companies improve their quality of reporting more thoroughly. Investors, therefore, react favorably to those restatements. In contrast, using a larger sample of 403 restatements from 1995 to 1999, Palmrose et al. (2004) find no evidence of a relation between the number of years being restated and market reactions. In this study, I do not specify an expected sign for *No. years*.

In addition to the proxies for severity of errors, I use two proxies to capture characteristics of restatement disclosures: whether managers disclose the quantitative impact of a restatement on net income in initial announcements (*Quantification disclosed*) and whether managers use press releases (versus SEC filings) to disclose a restatement (*Press release*). Providing the quantitative effect of a restatement reveals more complete information to the market at the time of announcement. Palmrose et al. (2004) find that market reactions are more negative when the initial revelation does

⁴³ Following Wang and Yu (2008) and Palmrose et al. (2004), I calculate *Income impact* by subtracting the restated net income from originally reported net income during all restated periods and then scaling the difference by the total assets reported at the year-end prior to restatement announcements.

not include a quantified effect on net income. The market penalizes the initial disclosures that are incomplete, and therefore involve uncertainty. Therefore, I expect a positive relation between *Quantification disclosed* and market reactions because the market participants should react *less* negatively to more complete disclosure on restatements. *Press release* captures the effect of disclosure media on market reactions. Myers et al. (2008) find that restatements disclosed by press release are usually associated with more negative market reactions.

The third group of proxies consists of dummy variables proxying for the parties initiating restatements. Errors can be identified by external auditors, SEC staff, or the company (including managers, board of directors, or internal audit committee). I refer to whichever party identifies or reveals the need for a restatement as the attribution party. Press releases or amended filings are used to attribute the discovery of the misstatement to auditors (*Auditor*), the SEC staff (*SEC*), company management (*Company*) or jointly to auditors and the company (*Auditor_Company*).

The benchmark condition is unattributed restatements, those without clear information about which party initiated the restatement. Attribution disclosure is not mandatory; it is at the discretion of management. The proxies for attribution parties capture additional information that may not be captured by the other two groups of proxies. Auditor-attributed restatements (*Auditor*) are generally associated with more negative market reactions because they indicate management's inability to properly monitor and control the company (Palmrose et al. 2004). I do not specify a sign for SEC attribution (*SEC*), since prior studies find no evidence that the SEC attribution

has an incremental negative impact on market returns (e.g. Palmrose et al. 2004).⁴⁴ A restatement attributed to company management provides a mixed signal to market. On one hand, revelation of errors by management provides a positive signal to the market that companies have detected errors and are willing to correct them (Wang and Yu 2008). On the other hand, investors may view restatements as a bad signal because managers did not prevent them in the first place. For the above reason, I do not designate a sign for the association between the indicator of management attribution (*Company*) and market returns.⁴⁵ In some cases, restatement disclosures indicate that errors are identified by the “joint work” between auditors and companies’ managers. As previously discussed, the effect of management- and auditor-attribution on market returns can be different. The effect of this joint effort might also differ from that of either management- or auditor-attribution. I thus use a separate indicator variable to proxy for the cooperative work between companies and auditors (*Auditor_Company*). Again, I do not designate a sign for this indicator variable.

2.4 Univariate Results

Panel B of table 1 provides descriptive statistics for variables used in the regression models. It reveals that the composition of restatements changed from 2000 to 2001 in all the three categories of restatement characteristics. The frequency of restatements with severe characteristics decreased from 2000 to 2001. For example, while the number of restatements increased 85 percent from 140 to 259, the number

⁴⁴ Palmrose et al. (2004) interpret their results as evidence that investors view SEC intervention probably as technical compliance issues, rather than fundamental financial reporting violations.

⁴⁵ Palmrose et al. (2004) find that restatements attributed to the company are usually associated with more negative returns. They provide several possible explanations for this result.

of fraudulent restatements declined slightly, from 23 to 16, or 16 percent of all restatements in 2000 compared to 6 percent in 2001 (Chi-square p-value = 0.019). There is a similar shift in the proportion of restatements revising revenue, from 47 percent in 2000 to 28 percent in 2001, (Chi-square p-value = 0.003). Similarly, fewer restatements reverse net income to net loss in 2001 (p-value < 0.001).

Panel B of table 1 also shows shifts in the average reaction to categorical restatement characteristics from 2000 to 2001. The average market reactions for each category are the mean cumulative adjusted-returns (CARs) for all firms having that characteristic, without controlling for other characteristics that the firm may also have.⁴⁶ In general, the market reaction abated for restatements with both severe and non-severe characteristics. For example, the average market return for restatements involving fraud eased from -28 percent to -19 percent (p-value = 0.07), while the average non-fraud reaction lessened from -7 percent to -1 percent (p-value < 0.01). Similarly, the average return for revenue-related restatements decreased from -17 percent to -6 percent (p-value < 0.01), while non-revenue restatements declined from -5 percent to 0.1 percent (p-value = 0.01). Despite this easing, on average, fraud, revenue and income-to-loss restatements elicit significantly negative reactions in both years. However, their non-severe counterparts generate negative and significant reactions in 2000, but in 2001 the average reactions do not differ from zero.

The average number of accounts restated increases from 1.5 per restatement to 1.9 per restatement (p-value < 0.01). The number of years restated also increases from

⁴⁶ In a sensitivity test, I use the traditional market-model approach with equally weighted index returns. Similar to DeFond et al. (2005), the market returns are calculated as cumulative market returns over the two-day event windows using 239 days (day -244 through day -6) in the estimation period to derive the standard deviation. I restrict the analysis to companies with at least 120 daily returns in the estimation period. This process thus decreases the sample size by 11 percent (from 399 to 357). Coefficient estimates and significance levels are similar to results reported in Tables 3 and 4. Also consistent with Table 5, the reduced average returns are primarily due to the decreased severity of errors restated. Results are not tabulated.

1.2 years to 1.4 years, (p-value < 0.10), indicating that later restatements tend to be more extensive in terms of both financial statement scope and longer time periods.⁴⁷

In summary, these statistics are generally consistent with companies beginning to correct more but less material accounting issues, resulting in a reduction in the average severity of errors being restated. However, the reactions associated with more severe characteristics also abate from 2000 to 2001, so the overall shift cannot be attributed entirely to a change in the mix of restatement characteristics.

Announcement characteristics and the related market reactions also shift from 2000 to 2001. By 2001, managers are more likely to quantify the income effect of the restatement in the initial announcement (91 percent vs. 74 percent, p-value < 0.01) and to disclose the restatement in SEC filings rather than press releases (p-value < 0.01).⁴⁸ Palmrose et al. (2004) find that quantified announcements have less negative market reactions, and Myers et. al. (2008) and Plumlee and Yohn (2010) find that less severe restatements are likely to be reported in SEC filings (other than 8-K). Thus, both of these changes are consistent with the tendency toward generally less severe restatement characteristics noted above.

Market returns for the less severe disclosure venues also abated. The mean market return for quantified announcements became less negative (-6 percent to -0.3 percent, p-value < 0.01), as did the mean return for disclosures in SEC filings (-5 percent to 0.4 percent, p-value < 0.01). In contrast, changes in average CARs for non-quantified announcements (-24 percent vs. -16 percent) and press disclosures

⁴⁷ A further examination reveals that the time lapse between the dates when errors happened and when restatements are announced is shorter when the number of restated periods is greater. This suggests that relatively more recent errors are detected and restated in 2001. This finding is consistent with Wang and Yu (2008) who find that market reactions to older errors are less negative.

⁴⁸ PRESS equals one only if management chooses to use press rather than SEC filings to make initial revelation of restatements.

(-14 percent vs. -11 percent) are insignificant.

Lastly, although it is not mandatory that companies' managers disclose which parties identified the need for a restatement, about 81 percent of my sample provides reasonably clear attribution to press release or SEC filings. There are more restatements providing attribution in 2001 than in 2000 (84 percent versus 77 percent), although the difference is insignificant (p-value = 0.17). Among those with attribution, the increase is mainly due to company-attributed restatements, which increased from 49 percent to 66 percent of all announcements (p-value < 0.01). Further, the average reaction to both company- and auditor-attributed restatements eased (respectively, -14 percent to -1.2 percent, p-value < 0.01, and -28 to -14 percent, p-value = 0.08).⁴⁹

Table 2 presents correlation tables for the variables used in model 1. Panel A provides correlations for 2000, and panel B shows 2001. The correlation estimates between all the explanatory variables and market reactions, and their significance levels are essentially the same in 2000 and 2001, except for *No. accounts*, which is significant in 2001, but not in 2000. All are in the same direction as the univariate tests in Table 1. *Fraud involved* exhibits the largest correlation with market returns, in both years. Tests of multi-collinearity in OLS regressions for all variables in table 2 reveal the highest variance inflation factor is for *Company* at 1.8 in 2000 and 1.7 in 2001, both of which are well below 10.0, the empirical level conventionally accepted as cause for concern.

⁴⁹ Studies of restatements announced in earlier periods indicate that attribution to managers and auditors was typically associated with more negative returns (Palmrose et al. 2004.)

2.5 Multivariate Results

2.5.1 Regression Results

Table 3 presents the OLS regression model for the relationship between market reactions and restatement characteristics estimated using model (1). Year 2000 results are presented in Panel A, year 2001 results in Panel B, and the two years combined are in Panel C. In both 2000 and 2001, the overall models are highly significant ($p\text{-value} < 0.01$), and the results are essentially similar.

Specifically, restatements involving fraud, revenue, and greater effect on net income (net income changes/assets) are associated with more negative market reactions in both years. Also as expected, announcements made in press releases and lacking initial quantification are associated with more negative reactions. Finally, the results indicate that restatements attributed to auditors or management have incrementally negative effects on market returns. These findings are generally consistent with univariate results in Table 1, and with prior research (Anderson and Yohn 2002, Wu 2002, Palmrose et al. 2004). Most coefficients and significance levels are similar in Panels A and B, except *Income_impact*, which has a smaller, but somewhat more significant coefficient in 2001.

To test for an intercept change in market reactions to restatement announcements from 2000 to 2001, Panel C includes observations from both years and adds an indicator variable, *Y2001*, to capture the overall change in market reactions. The model is again highly significant ($p\text{-value} < 0.01$). The positive relationship between *Y2001* and announcement returns is consistent with the intercept shift in mean market reactions to restatements, as observed in Figure 2 and in univariate tests in Table 1. Results for all other variables are similar to those for individual years. Thus Panel C

indicates that after controlling for significant determinants of market reactions to restatement announcements, there was a temporal shift from 2000 to 2001.

To further explore whether the reduction can be attributed to a change in the market's response to specific restatement characteristics, Table 4 shows the results of an interaction model, which allows the explanatory variables to have different coefficients in 2001 compared to 2000. For parsimony, I include only variables and interactions for characteristics that gain significance in Table 3.⁵⁰

In the interaction model, the non-interacted variable coefficients capture the association between announcement characteristics and market reactions during 2000. As in Table 3, all are significant in the expected directions. The coefficient of the *Y2001* indicator variable is insignificant in the interaction model, as are nearly all of the interaction variables, indicating that the market reaction for each of these characteristics did not change significantly from 2000 to 2001. The one exception is *Company_Y2001* which is positive and marginally significant (p-value = 0.089), indicating that the market reacted somewhat less negatively to restatements attributed to management during 2001, relative to 2000.

To ascertain that market reactions remain significantly associated with each of these announcement characteristics during 2001, I perform a joint test of the sum of each variable coefficient and the coefficient of its related interaction (results not tabulated). The joint tests indicate the associations between each characteristic and market responses in 2001 are significant in the expected direction, with p-values ranging from less than 0.01 to 0.10, the latter for company-attributed restatements.

⁵⁰ The coefficient estimates and their corresponding p-values in Table 4 are essentially the same if the regression includes the interactions between *Y2001* and all twelve explanatory variables. However, the model suffers from multi-collinearity problems, as there are five interaction variables with VIF coefficients of about 10.

Overall, these results suggest that the reduction in the average market reaction is not due to a change in the market reaction to specific restatement characteristics (see Table 4). Instead, the reduction is mainly associated with a change in the composition of restatements (see Panel B of Table 1). This change in composition is associated with an increase in the frequency of restatements that correct less severe errors. It thus increases the proportion of restatements with non-severe characteristics that have less negative reactions, such as non-fraud, non-revenue, and non-reversal restatements, and therefore, shifted the average CARs in 2001. Second, these non-severe restatements are more likely to be attributed to management. Managers of companies making restatements are inclined to use SEC filings (including Form 8-K) rather than press releases to reveal less severe restatements, beginning in 2001 (Myers et al. 2008).⁵¹ There are two incentives for this tendency. First, management can avoid attracting public attention when they use SEC filings. Studies show that litigation risk is higher when management makes highly prominent disclosures of restatements (Files et al. 2009). Second, management is more likely to provide more comprehensive information in SEC filings than in press releases, so that their companies can avoid adverse market penalties for incomplete disclosures.

2.5.2 Explanation of the Reduction in Market Reactions Using Regression Results and Descriptive Statistics

⁵¹ SEC Rule 33-8400 (“Additional Form 8-K Reporting Requirements and Acceleration of Filing Date”) became effective on August 23, 2004. The rule requires a company to file Form 8-K if and when its board of directors, a committee of the board of directors, or an authorized officer, concludes that any of the company's previously issued financial statements covering one or more years or interim periods no longer should be relied upon because of an error in such financial statements. Though the rule is not directly relevant to my analysis of restatements announced in 2000 and 2001, the evidence provided by Myers et al. (2008) is consistent with the findings of this study.

To quantitatively examine how the change in mean market reactions is explained by changes in the frequencies and market response to specific restatement characteristics, I disaggregate the overall mean market reaction by each specific characteristic. Table 5 displays characteristics' frequencies or means and their associations with the overall return, as captured by coefficient estimates from the regression results, to disaggregate the overall 9 percent shift in average CARs (from -10.6 percent in 2000 to -1.6 percent in 2001) into the shift attributable to each restatement characteristic.

Column 1 shows the percent of all 2000 restatements that involved each characteristic. For example, fraud was involved in 16 percent of the year 2000 restatements. Column 2 shows the Table 4 coefficient for the association between fraud restatements and CARs in 2000. That is, the coefficient for fraud in Table 4 is -.145. Column 3 is the product of Columns 1 and 2, combining the effects of frequency and association with market returns per occurrence. Thus, in 2000, fraud restatements contribute -0.024 to the mean CAR of -0.106.

Columns 4 to 6 repeat the calculation using 2001 variable frequencies/means, and the 2001 coefficients from Table 4. The 2001 coefficient value is the sum of the coefficients for the variable and its interaction with *Y2001*, and the intercept is the sum of the Table 4 intercept and the *Y2001* coefficient. Thus, in 2001, fraud restatements contributed -0.008 (-.135 x 6%) to the overall mean CAR of -0.016,

The difference between Columns 3 and 6, reported in Column 7, represents how much of the change in mean CAR is due to each characteristic. Since Table 4

indicates that the market reaction per unit of characteristic does not differ significantly between the two years, these differences are mainly attributable to changes in the frequencies/means of the restatement, disclosure and attribution characteristics.

Of the overall difference in mean CARs of 9.0 percent (-10.6 percent to -.106 percent), 98 percent of the change is explained by the model in Table 4. The unexplained change, represented by the change in the intercept, is 0.002, or 2 percent of the total shift. Forty-nine percent of the reduction is due to the decreased severity of errors restated. Changes in disclosure characteristics account for a net decrease of 22 percent, and 28 percent is explained by changes in attribution.

The primary reason for the reduced market reaction is the decreased severity of errors being restated, which accounts for almost half of the shift in mean CARs. The generally decreased severity probably results from an increase in the proportion of restatements with non-severe characteristics. This increase in non-fraud, non-revenue, and non-reversal restatements reduced the average CARs in 2001. *Fraud involved* and *Revenue restated* explain 17 percent and 24 percent of the change in mean CARs, respectively.

The changes in disclosures and attribution of restatements explain almost another half of the reduction in mean CARs. Management identified (or is described to have identified) more errors in 2001 versus 2000. They are more inclined to use SEC filings (including Form 8-K) rather than press releases to reveal less severe restatements.

2.6 Additional Tests: Pre-announcement Returns and Market Volatility

Plumlee and Yohn (2010) suggest that investor anticipation might adjust as the number of restatements increases. Thus, another possible explanation for the attenuated announcement reactions might be that investors began to better anticipate future restatements in 2001. Prior literature either assumes no early market anticipation (Wang and Yu 2008; Scholz 2008), or argues that preceding price declines are small (Palmrose et al. 2004). To test this explanation, I examine the association between the market reactions at the announcement date and the pre-announcement returns during various windows. If there is a systematic shift in the price reaction from the restatement announcement to the pre-announcement period, we should observe an increase in negative market returns preceding the announcements in 2001, and a negative association between announcement returns and pre-announcement returns in 2001 compared to 2000.

I find that the pre-announcement return, on day -1 is small in 2000 (-2.0 percent, p-value = 0.04), and decreases further in 2001 (-0.8 percent, p-value < 0.01). The day -1 returns do not significantly differ between the two years (p-value = 0.123). Results for both years are consistent with prior studies. Moreover, the pre-announcement returns are not significantly associated with announcement CARs either in 2000 or in 2001 (results are not tabulated).

I extend the pre-announcement window to longer periods, examining CARs over 10-, 20- and 30- day windows ending on day -1. CARs are not significantly different from zero for any window in 2001. Further, there is no significant association between announcement returns and any pre-announcement CARs. The small magnitude of pre-announcement returns and the insignificant association with

announcement reactions suggest that the reduced market reactions in 2001 are not due to better investor anticipation of restatements.

Next, I examine whether the dampened reaction is due to a decrease in the overall volatility of stock prices from 2000 to 2001. I use the volatility of stock prices (measured by the standard deviation of stock prices) during 10-, 20- and 30- day windows ending on day -1, to test whether the volatility of pre-announcement returns changed from 2000 to 2001 and whether the association between pre-announcement volatility and market returns at announcement dates has changed.

The pre-announcement stock price volatility does not differ between 2000 and 2001 for any of the pre-10, -20 or -30 days windows.⁵² None of the stock price volatility during the three pre-announcement windows is associated with the announcement returns. Combined, the reduced market reactions in 2001 are not due to a greater volatility of stock prices.

2.7 Conclusions

This study investigates the timing of and reasons for reduced market reactions to restatement announcements in the 1990s compared with those announced more recently. Research has documented average returns of about -10 percent in earlier years (GAO 2003, Palmrose et al. 2004), while more recent studies have reported much less negative reactions (Hranaiova and Byers 2007, Burks 2009, Scholz 2008, Wang and Yu 2008, Audit Analytics 2009). As the number of restatements has dramatically increased in recent years, it is important to understand this shift, as restatements and the market response to them have important implications for the

⁵² P-value = 0.45 for (-10, -1) window, p-value = 0.58 for (-20, -1) window, and p-value = 0.98 for (-30, -1) window.

quality of financial reporting and auditing and the efficient functioning of capital markets.

My study has at least three main findings. First, using a comprehensive sample of restatement announcements over a twelve-year period, from 1995 to 2006, I provide new evidence on an 85 percent reduction in average market reactions between the years 1995-2000 and the years 2001-2006. I find that the shift takes place in the first quarter of 2001, as the magnitude of returns is comparable between 1995-1999 and 2000, and between 2002-2006 and 2001.

Second, using a model of significant determinants of market reactions to restatement announcements, I find that the decreased market reactions appear to be driven by a change in the composition of restatement characteristics. The associations between market returns and specific restatement characteristics do not change between year 2000 and 2001.

Third, I disaggregate the mean market reactions in 2000 and 2001 into the changes in frequency and market associations among the three categories of restatement characteristics. I find that the reduction is primarily associated with a decline in the severity of restatements in 2001. These non-severe restatements are more likely to be attributed to the management. There are also important disclosure changes, as managers are more likely to disclose a restatement via SEC filings (versus press releases) in 2001, and to provide quantification of the restatements to avoid a market penalty for incomplete disclosure.

One implication of this study is that the market reaction did not shift in response to the scandals leading to SOX or SOX itself. Rather, the change is observed nearly one year prior to Enron's restatement announcement in November 2001, and a year

and a half before SOX was passed in July 2002. The results suggest that well before these events, companies had begun restating less severe items with greater frequency, leading to a reduction in the average market response. Further, while the market response attenuated somewhat for both severe and non-severe restatement characteristics, statistically, the market's response to specific characteristics does not differ between the two years.

This study does not investigate the reasons underlying the increased frequency of less significant restatements about this time. Historically, this period comes at the end of the Technology Bubble, which reached its peak in April 2000. Anecdotally, the Bubble was marked by a subtle disdain for accounting rules and concepts. Revenue is arguably the accounting concept that retained the most relevance during the Bubble period, and this study reports that revenue restatements account for an important decrease in the average market reaction. This pattern is consistent with a post-bubble shift to a more comprehensive respect for accounting results.

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Figure 2.1
The Number of Restatements and Market Reactions to
Restatement Announcements 1995-2006

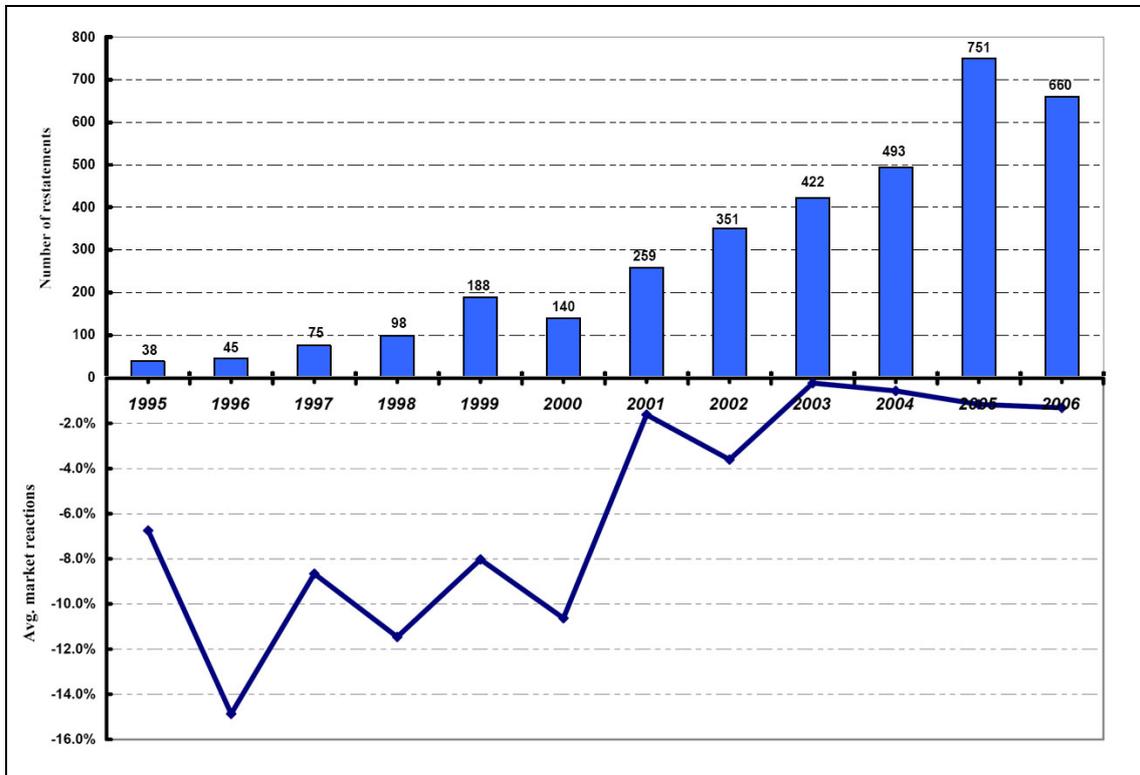
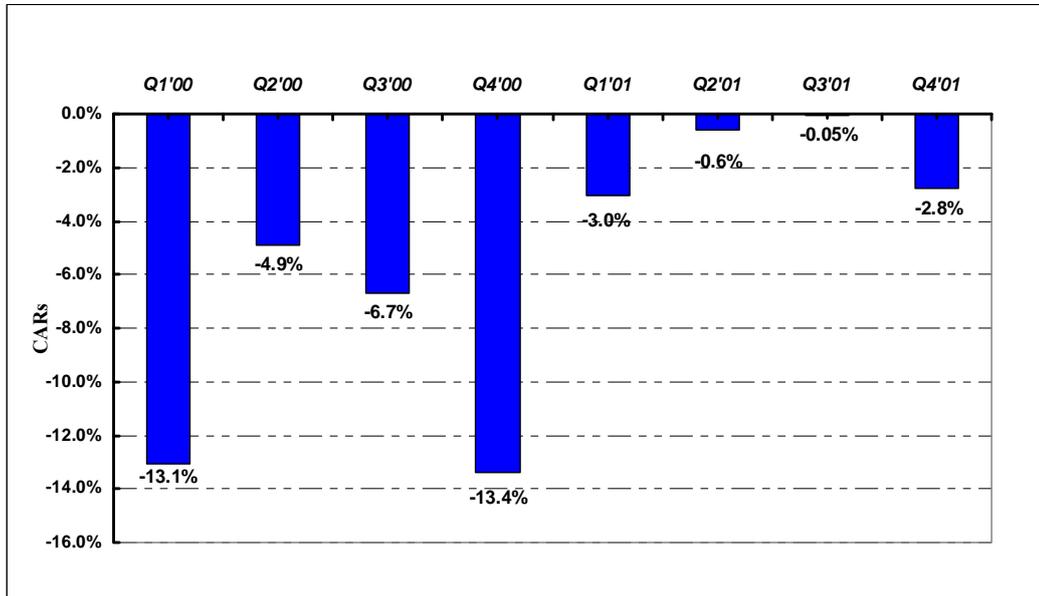


Figure 2.2
Quarterly CARs 2000-2001



Appendix

Definitions of explanatory variables

Variable	Variable Definitions
<i>Fraud involved</i>	=1 if a company admits to fraud or irregularities in its filings or press releases, and 0 otherwise.
<i>Revenue restated</i>	=1 if a restatement involves revenue recognition issues and 0 otherwise.
<i>Income impact</i>	= Restated net income (loss) minus originally reported net income (loss) over restated period, scaled by total assets reported at fiscal year-end prior to restatement announcements.
<i>Income to loss</i>	= 1 if a restatement changes originally reported net income to a loss, and 0 otherwise.
<i>No. accounts</i>	=Number of account groups involved in a restatement. The seven account groups are revenue, cost of sales, operating expenses, one-time/special items, merger-related, non-operating expenses, and other items.
<i>No. years</i>	=Number of years restated, where a fiscal year =1 and each additional quarter = 0.25.
<i>Quantification disclosed</i>	=1 if a company quantifies the impact of a restatement on its net income in an initial disclosure and 0 otherwise.
<i>Press release</i>	=1 if a restatement is disclosed in press releases, and 0 otherwise
<i>Auditor</i>	=1 if a restatement is attributed to company auditors, specified in press releases or company filings, and the benchmark condition is unattributed.
<i>Company</i>	=1 if a restatement is attributed to company management, directors, or internal audit committee, specified in press releases or company filings, and the benchmark condition is unattributed.
<i>SEC</i>	=1 if a restatement is attributed to SEC, specified in press releases or company filings, and the benchmark condition is unattributed.
<i>Auditor_Company</i>	=1 if a restatement is attributed to co-work of company auditors and management, directors or internal audit committee, specified in press releases or company filings, and the benchmark condition is unattributed.

Table 2.1 Descriptive Statistics

Panel A

<u>Periods</u>	<u>N</u>	<u>CAR (%)</u>	<u>Periods</u>	<u>N</u>	<u>CAR (%)</u>	<u>(t-stat.)</u>
1995-1999	444	-9.5	2000	140	-10.6	(0.60)
2002-2006	2,677	-1.3	2001	259	-1.6	(0.52)

Panel B

<u>Variables</u>	<u>Year 2000</u>			<u>Year 2001</u>			<u>Diff. in % or Mean¹</u>
	n = 140			n = 259			
	<u>Freq.</u>	<u>% or mean</u>	<u>CARs</u>	<u>Freq.</u>	<u>% or mean</u>	<u>CARs</u>	<u>chi-sqr./t-test</u>
<i>CAR</i>			-0.106			-0.016	
Severity of Errors Restated:							
<i>Fraud involved</i>	23	16%	-28%	16	6%	-19%	0.019**
no fraud	117	84%	-7%	243	94%	-1%	
<i>Revenue restated</i>	66	47%	-17%	73	28%	-6.0%	0.003***
no revenue	74	53%	-5%	186	72%	0.1%	
<i>Income impact</i>		-0.029			-0.010		0.185
<i>Income to loss</i>	20	14%	-20%	13	5%	-8%	0.003***
no reversal	120	86%	-9%	246	95%	-1%	
<i>No. accounts</i>		1.507			1.938		0.000***
<i>No. years</i>		1.188			1.411		0.056*
Disclosure of Restatements:							
<i>Quantification disclosed</i>	103	74%	-6%	236	91%	-0.3%	0.000***
no quantification	37	26%	-24%	23	9%	-16%	
<i>Press release</i>	85	61%	-14%	46	18%	-11%	0.000***
no press	55	39%	-5%	213	82%	0.4%	
Parties Initiating Restatements:							
<i>Auditor</i>	10	7%	-28%	11	4%	-11.8%	0.29
<i>SEC</i>	11	8%	-4%	16	6%	0.639	0.666
<i>Company</i>	68	49%	-14%	172	66%	-1.2%	0.008***
<i>Auditor_company</i>	19	14%	-12%	19	7%	-5.3%	0.088*
<i>Unattributed</i>	32	23%	3%	41	16%	2.5%	0.983

Note: ¹ p-values are for t-tests or chi-squares on differences of mean values for corresponding characteristic.

² p-values are for t-tests on differences of mean CARs. ***, **, * indicate statistical significance level at 0.01, 0.05 or 0.10 respectively, based on chi-square or t-statistics as appropriate. All p-values are two-tailed.

Table 2.2 Correlation Tables
Panel A: Correlation table for Year 2000

	B	C	D	E	F	G	H	I	J	K	L	M		
CAR	A	-0.45 ***	-0.31 ***	0.29 ***	-0.20 **	-0.12	-0.14 *	0.41 ***	-0.22 ***	-0.24 ***	0.09	-0.17 *	-0.04	
Fraud involved	B		0.11	-0.29 ***	0.24 ***	0.12	0.25 ***	-0.31 ***	0.04	0.12	0.03	0.01	0.14	
Revenue restated	C			-0.18 **	0.06	0.12	-0.01	-0.18 **	0.14 *	0.13	0.04	0.03	0.04	
Income impact	D				-0.26 ***	-0.09	-0.02	0.16 *	0.03	-0.02	-0.09	-0.12	0.09	
Income to loss	E					0.12	-0.07	-0.17 **	0.16 *	0.20 **	-0.12	0.09	0.02	
No. accounts	F						0.19 **	0.00	-0.09	0.07	0.05	-0.06	0.09	
No. years	G							-0.20 **	-0.06	0.06	0.34 ***	-0.04	0.01	
Quantification	H								-0.22 **	-0.21 **	-0.07	-0.03	0.00	
Press release	I									0.11	0.13	-0.07	-0.02	
Auditor	J										-0.08	-0.27 ***	-0.11	
SEC	K											-0.28 ***	-0.12	
Company	L												-0.39 ***	
Auditor_company	M													1.00

(Continued)

Table 2.2 (Continued)
Panel B. Correlation table for Year 2001

	B	C	D	E	F	G	H	I	J	K	L	M		
CAR	A	-0.37 ***	-0.23 ***	0.23 ***	-0.13 **	-0.16 **	-0.08	0.36 ***	-0.36 ***	-0.18 ***	-0.01	0.05	-0.09	
Fraud involved	B		0.12 **	-0.08	0.09	0.19 ***	0.07	-0.26 ***	0.30 ***	0.11 *	0.00	-0.22 ***	0.30 **	
Revenue restated	C			-0.07	-0.03	0.21 ***	0.02	-0.05	0.18 ***	-0.09	-0.02	-0.05	0.12 *	
Income impact	D				-0.12 *	-0.08	-0.01	0.22 ***	-0.16 ***	0.00	0.01	0.05	-0.02	
Income to loss	E					-0.06	-0.03	-0.05	0.12 **	-0.05	0.01	-0.10	0.14 **	
No. accounts	F						0.16 **	-0.15 **	0.17 ***	0.03	0.07	-0.06	0.03	
No. years	G							-0.03	0.08	0.26 ***	0.19 ***	-0.20 ***	0.01	
Quantification	H								-0.49 ***	-0.27 ***	-0.03	0.09	-0.07	
Press release	I									0.20 ***	0.13 **	-0.23 ***	0.18 **	
Auditor	J										-0.05	-0.30 ***	-0.06	
SEC	K											-0.36 ***	-0.07	
Company	L												-0.36 **	
Auditor_company	M													1.00

Note: The correlations are based on Pearson Correlations. Spearman Correlations yield the similar results. P-values are *** significant at 0.01 level or better, ** significant at 0.05, * significant at 0.10. All-p-values are two-tailed. See Appendix for variable definitions.

**Table 2.3 OLS Regression for Model (1).
Dependent Variable is CAR Over Days (0, 1)**

	Pred.	Panel A. Year 2000		Panel B. Year 2001		Panel C 2000 and 2001				
Variable:	Sign	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value			
Intercept	+/-	0.029	0.605	-0.007	0.846	-0.029	0.337			
Severity of Errors Restated:										
<i>Fraud involved</i>	-	-0.127	0.002	***	-0.134	0.000	***	-0.136	0.000	***
<i>Revenue restated</i>	-	-0.067	0.021	**	-0.046	0.003	***	-0.058	0.000	***
<i>Income impact</i>	+	0.330	0.034	**	0.090	0.017	**	0.109	0.009	***
<i>Income to loss</i>	-	0.022	0.608		-0.049	0.113		-0.031	0.205	
<i>No. accounts</i>	-	-0.015	0.400		-0.002	0.799		-0.003	0.642	
<i>No. years</i>	+/-	-0.014	0.370		-0.003	0.580		-0.006	0.337	
Disclosure of Restatements:										
<i>Quantification disclosed</i>	+	0.089	0.010	**	0.066	0.019	**	0.082	0.000	***
<i>Press release</i>	-	-0.074	0.013	**	-0.043	0.043	**	-0.047	0.005	***
Parties Initiating Restatements:										
<i>Auditor</i>	-	-0.148	0.019	**	-0.081	0.037	**	-0.108	0.001	***
<i>SEC</i>	+/-	0.069	0.267		-0.020	0.535		0.003	0.929	
<i>Company</i>	+/-	-0.091	0.012	**	-0.036	0.045	**	-0.059	0.001	***
<i>Auditor_company</i>	+/-	-0.067	0.161		0.001	0.973		-0.028	0.283	
Change Across Years:										
<i>Y2001</i>	+							0.034	0.029	**
N =		140			259			399		
F-test		7.56	0.000	***	8.890	0.000	***	18.88	0.000	***
Adjusted R-square		0.362			0.268			0.369		

Table 2.4 OLS Regression for the Interaction Model

		Interaction Model		
		Coef.	p-value	
		-0.024	0.480	
Severity of Errors Restated:				
<i>Fraud involved</i>	—	-0.145	0.000	***
<i>Revenue restated</i>	—	-0.073	0.001	***
<i>Income impact</i>	+	0.264	0.025	**
Disclosure of Restatements:				
<i>Quantification disclosed</i>	+	0.092	0.001	***
<i>Press release</i>	—	-0.060	0.009	***
Parties Initiating Restatements:				
<i>Auditor</i>	—	-0.136	0.002	***
<i>Company</i>	+/-	-0.078	0.001	***
Change Across Years:				
<i>Y2001</i>	+	0.002	0.970	
<i>Fraud_involved_Y2001</i>	+/-	0.009	0.840	
<i>Revenue_restated_Y2001</i>	+/-	0.028	0.322	
<i>Income_impact_Y2001</i>	+/-	-0.169	0.180	
<i>Quantification_Y2001</i>	+/-	-0.027	0.527	
<i>Press_Y2001</i>	+/-	0.012	0.722	
<i>Auditor_Y2001</i>	+/-	0.060	0.328	
<i>Company_Y2001</i>	+/-	0.049	0.089	*
F-statistic			16.80	***
Adjusted R2			0.373	
Sample size			399	

Note: *, **, *** Denotes a statistically significant two-tailed t-test of the null hypothesis that the coefficient estimate is different from zero at the 0.10, 0.05, 0.01 levels, respectively. The dependent variable is *CAR*, the cumulative market-adjusted abnormal returns over two days 0 and 1 using an equally weighted index, where day 0 is the restatement announcement date. See Appendix for other variable definitions.

Table 2.5 Disaggregation of the Reduction in Mean Market Reactions from 2000 to 2001 Using Interaction Regression Results (Table 2.4) and Descriptive Statistics (Table 2.1)

Variables	2000		(1) x		2001		(4) x		(6)-(3)	
	(1)	(2)	(2)	(4)	(5)	(5)	(6)	(7)		
	% or mean	Mkt returns per unit		% or mean	Mkt returns per unit					
Intercept			-0.024			-0.022	0.002	2%	2%	Unexplained
Severity of Errors Restated:										
<i>Fraud involved Revenue restated Income impact</i>										
	16%	-0.145	-0.024	6%	-0.135	-0.008	0.015	17%		
	47%	-0.073	-0.034	28%	-0.044	-0.013	0.022	24%		
	-0.029	0.264	-0.008	-0.010	0.095	-0.001	0.007	7%		
									49%	
Disclosure of Restatements:										
<i>Quantification disclosed Press release</i>										
	74%	0.092	0.068	91%	0.065	0.059	-0.008	-9%		
	61%	-0.060	-0.036	18%	-0.048	-0.008	0.028	31%		
									22%	
Parties Initiating Restatements:										
<i>Auditor Company</i>										
	7%	-0.136	-0.010	4%	-0.076	-0.003	0.006	7%		
	49%	-0.078	-0.038	66%	-0.029	-0.019	0.019	21%		
			-0.106			-0.016	0.088		28%	
			≈			≈				
Means from Table 1			-0.106			-0.016				
									98%	Explained

Note: Column (1) and (4) are percent or mean values from Table 1 for year 2000 and 2001, respectively. Column (2) and (5) present market returns per unit of each variable for 2000 and 2001, respectively. Column (6) presents market returns per unit of each variable, the joint effects for year 2001. Column (3) is the product of column (1) and column (2). Column (6) is the product of column (4) and column (5). Column (7) is the difference between column (6) and column (3).

STUDY THREE: AN ANALYSIS OF THE ASSOCIATION BETWEEN RESTATEMENTS AND AUDITOR RESIGNATIONS

ABSTRACT

This paper examines auditor resignations at the time when clients publicly disclosed financial accounting failures. I hypothesize and find that auditor resignations are determined both by increased audit risks conveyed by restatements and by increased economic benefits proxied by audit fees.

Using companies that announce restatements during 2003-2007, I find that auditors are more likely to resign from clients whose restatements involve fraud, reverse income to loss and affect more accounts. Correcting more recent errors and the use of press release (versus amended filings) are also associated with increased likelihood of resignations. Among characteristics of restatements, the restated dollar impact appears to be the most important proxy for increased audit risk in the resignation decisions. Auditors' economic incentives to retain clients mitigate their resignations when auditors expect a higher increase in audit fees. The inferences remain the same after controlling for other determinants of resignations and using extended control groups and alternative resignation windows.

Key Words: restatements, accounting failure, auditor resignation, audit fee

3.1 Introduction

Restatements are disclosed to correct material errors in financial statements previously filed with the SEC (Security and Exchange Committee).⁵³ Recent research has largely documented the consequences of accounting restatements, such as sharp stock price declines (Scholz 2008; Palmrose et al. 2004; GAO 2003), increased cost of equity capital (Hribar and Jenkins 2004), increased incidence of lawsuits (Palmrose and Scholz 2004), top management turnovers (Desai et al. 2006) and outside director turnovers (Srinivasan 2005). High turnover rates have been observed following restatements for top management around at 60 percent (Desai et al. 2006) and at about 48 percent for outside directors (Srinivasan 2005). However, there currently is no academic study of external auditor turnovers related to restatements. This paper addresses this gap in the literature.

In this paper I investigate voluntary turnover decisions by external auditors, auditor resignations, in cases where their clients have detected material errors and publicly disclosed that they had financial reporting failures. Auditors face the trade-off between risks and benefits in their decisions on whether to resign or not. The risks mainly come from auditors' litigation costs, which are the primary factor driving auditor resignations.⁵⁴ Prior literature argues that auditors strategically manage clients' audit risk by selectively refusing to accept new, risky engagements or by declining to continue with engagements where audit risk has increased (Pratt

⁵³ This type of restatements, which I focus on in this study, is limited to error corrections to financial statements in the course of companies applying U.S. GAAP. In contrast, other studies may include sample of restatements on changes in accounting principles, re-disclosures following U.S. GAAP, accounting adjustment at year-end, etc. See section 3 for a detailed discussion of data selection.

⁵⁴ See MacDonald, E. More accounting firms are dumping risky clients. *The Wall Street Journal*: April 25, 1997.

and Stice 1994). Shu (2000) and Krishnan and Krishnan (1997) also find empirical evidence that auditor resignation is positively associated with the likelihood of litigation.

Along these lines, I examine whether external auditors' resignation choices are sensitive to the information conveyed by a restatement disclosure on the client's risks. Restatement reveals that a client has weak corporate governance and internal control, and raises questions about managerial competence and/or integrity, and the reliability of the client's past financial reporting. Thus, I hypothesize that external auditors are more likely to resign from a company that reports severe accounting failures or indicates impairment of reliability of financial reporting.

Besides the risks driving auditor to resign, the incentives to retain clients and make a profit motive auditors to continue engagements. Bockus and Gigler (1998) utilize an analytical model to demonstrate that auditors are more likely to resign from engagements where they perceive that the probability of hidden audit risk is not sufficiently compensated by their proceeds. Therefore, I test whether a greater audit fee increase has mitigating effects on auditors' resignation decisions because it is costly for an auditor to resign from clients. If an auditor resigns from an engagement, he/she will not only incur switching costs from the current engagement but also forego the discounted values of possible future audit fees. The auditor is only likely to sacrifice these economic benefits because of concerns about being associated with the current client. These concerns include doubts about the accounting information quality of the companies that announce restatements, potential future litigation through being associated with an accounting failure client, or loss of reputations with regards to Arthur Anderson's failure. The purpose of this

paper is to explore the effect of audit fees and the information conveyed by clients' restatement disclosures on auditors' resignation decisions.

I utilize a sample of 1,232 companies that announced restatements from January 1, 2003 throughout December 31, 2007 to provide evidence on external auditors' resignation decisions when dealing with companies' previous financial reporting failures. I find that auditors are more likely to resign from the clients with severe restatements that indicate weak corporate governance and internal control. Specifically, clients revealing fraudulent accounting are more likely to experience a voluntary turnover by auditors. This is consistent with auditors' decisions to refuse association with clients whose management has questionable integrity and who also have been detected violating professional ethics. Auditors are more likely to resign from current engagements when disclosed restatements reverse positive net income to a loss, and when restatements are material in dollar impact and pervasive in account groups.

Furthermore, my analysis indicates that auditors are more likely to resign from clients that reveal more recent errors, even the errors last over a shorter period of time. Auditor resignations are more likely when companies disclose restatements by press releases (versus SEC filings). Other measures of severity of restatements, such as whether or not restatements affect revenue, are not significantly associated with auditors' decision to resign.

Auditors' risk-based incentives to resign are balanced by their economic incentives to retain clients. I find that companies experiencing greater increases in audit fees from the year before to the year of restatement announcements are *less* likely to have audit resignations. The results are consistent with a scenario in which

auditors who expect increased economic benefits continue with the engagements, likely because they perceive that increased fees are sufficient to compensate for the increased audit risk.

Overall, I interpret the evidence as suggesting that hidden audit risk conveyed by restatement disclosures is a major consideration in auditor resignation decisions. This is consistent with the effects of increased litigation costs on resignations documented by prior literature (Shu 2000; Krishnan and Krishnan 1997). At the same time, I illustrate that increased economic benefits exceed current and expected future litigation costs for many auditors who continue engagements with companies disclosing restatements. These results are helpful in understanding costs and benefits weighed by auditors in their resignation decisions.

Finally, I supplement my analysis by examining the type of an auditor hired following a resignation. I find that the majority of companies experiencing auditor resignations subsequently hire non-Big 4 auditors. All the sample companies that admit fraudulent accounting, or restate a profit to a loss, switch to non-Big 4 auditors. This evidence is consistent with risk aversion of Big 4 auditors when selecting new clients (Ettredge et al. 2007a).

The rest of the study proceeds as follows. Section 2 discusses the relevant prior literature, outlines the hypotheses, and develops the research design. Section 3 summarizes my sample selection and results. Section 4 discusses additional and robustness tests and section 5 concludes.

3.2 Prior Literature, Hypotheses Development, and Research Design

3.2.1 Prior Literature and Hypotheses Development

Factors affecting auditor changes are well investigated in prior literature. Topics studied include auditor switching given client financial distress (Chen et al. 2004; Johnson and Lys 1990), auditor-client disagreements (Turner et al. 2005; DeFond et al. 1997; Dye 1991; Antle and Nalebuff 1991), litigation risk (Shu 2000; Krishnan and Krishnan 1997), audit fees (Ettredge et al. 2007a; Griffin and Lont 2005; Gregory and Collier 1996), agency costs (DeFond 1992), corporate take-over (Anderson et al. 1993), reporting lags (Schwartz and Soo 1996), corporate governance (Lee et al. 2004), discretionary accruals (DeFond and Subramnyam 1998), going-concern (Chen et al. 2004; Carcello and Neal 2003), audit opinion shopping (Gomez-Auliar and Ruiz-Barbadillo 2003) and auditors' internal control opinion (Ettredge et al. 2007b). Extant studies suggest that auditors increasingly tend to avoid clients who expose them to greater litigation risks (Shu 2000; Krishnan and Krishnan 1997), rather than adjusting fees (Bockus and Gigler 1998). My study contributes to the stream of research on auditor switches by examining the interplay of factors determining auditor resignations given companies have detected material accounting failures and consequently disclosed financial restatements.

Financial restatements provide an ideal setting to examine auditor resignation decisions under increased audit risk. Financial restatements are made to correct material errors in companies' past financial statements. A restatement disclosure is a public admission that the client has had accounting failures, which are associated with higher litigation risk (Palmrose and Scholz 2004). I expect that auditors are more likely to resign from their clients if restatement disclosures convey information about high audit risk in restating companies.

H1: The greater the increase in audit risk among companies disclosing restatements, the more likely an auditor will resign from the current engagement, *ceteris paribus*.

On the other hand, if auditors consider the expected audit fees as sufficient to compensate for increased litigation risk and to cover current and future potential costs (Johnstone and Bedard 2003), they may continue engagements with the companies. Therefore, I test whether a greater fee increase has mitigating effects on auditor resignation decisions, if all else is equal.

H2: The greater the increase in audit fees among companies disclosing restatements, the more likely an auditor will continue the current engagement, *ceteris paribus*.

3.2.2 Research Design

I model auditors' decisions to resign from companies disclosing financial restatements as a function of variables that proxy for restatement characteristics that affect audit risk, and of change in audit fees. To examine this decision, I employ factors suggested in prior literature on financial restatements, auditor resignations, auditor switches, and audit fees. The primary model is:

$$\begin{aligned}
 RESIGN = & \alpha_1 + \beta_1 FRAUD + \beta_2 REVENUE + \beta_3 NOACCT + \beta_4 REVERSE + \beta_5 AMOUNT \\
 & + \beta_6 RESTYRS + \beta_7 PRESS + \beta_8 CHGFEE \\
 & + \beta_9 SIZE + \beta_{10} LOSS + \beta_{11} LEVERAGE + \beta_{12} CHGMGMT \\
 & + \beta_{13} INCTR + \beta_{14} GOCERN + \beta_{15} SAMEAU + \beta_{16} TENTURE + \beta_{17} FEEEXP + \beta_{18} CLTEXP + \varepsilon
 \end{aligned}$$

All the variables are defined in Table 1.

I classify an incumbent auditor as resigning from a company making a financial restatement ($RESIGN = 1$) if the auditor resigns within a period from 90

days prior to, up to one year subsequent to the restatement announcement. The control sample consists of restating companies neither having auditor resignations nor dismissals throughout the sample period ($RESIGN = 0$). Use of a within-sample design avoids matching control sample of non-restatement companies to restatement companies. Thus the auditor's resignation decision is affected by the severity of restatements. Estimated coefficients are not affected by systematic differences between restatements and non-restatements companies.

3.2.3 Test Variables

Fraud means intentional, deliberate violating GAAP reporting. It not only indicates weak corporate governance and internal monitoring, but more importantly, implies a lack of management integrity and credibility. Prior research provides evidence that market participants react more negatively to restatements related to fraud than to restatements related to other errors (Hennes et al. 2008; Palmrose et al. 2004). I expect auditors more likely to resign from companies announcing restatements that involve fraudulent reporting because it indicates increased likelihood of costly litigation and regulatory actions (Palmrose and Scholz 2004). I classify fraud restatements based on companies' admission of frauds or irregularities in their disclosures. Given the litigation risk and penalties on reputations, I believe these words are reliably selected when they are used.⁵⁵

I include test variables *REVENUE* and *NOACCT* to capture the severity and

⁵⁵ Prior research also uses restatements related to SEC AAER to proxy for fraud (Hennes et al. 2008; Palmrose et al. 2004). However, many investigations involve external auditors and take several years to eventuate. The effect of AAER on auditor resignations could be mixed and not the focus of my study. In untabulated analysis, I include an indicator of SEC or FED investigation on restatements and find that the coefficient estimate for the indicator is insignificant, and the coefficient for *FRAUD* becomes marginally significant. The other inferences remain unaffected.

pervasiveness of the effects of restatements on companies' income statements. Prior literature uses the indicator whether restatements correct revenue account (*REVENUE*) as proxy for severity (Anderson and Yohn 2002) and the number of account groups affected (*NOACCT*) to proxy for pervasiveness of restatements (Palmrose and Scholz 2004). *NOACCT* range from one to seven, presenting the account groups restated: revenue, cost of goods sold, operating expenses, special items, merge and acquisitions, non-operating accounts and other items. Some studies use an indicator of core accounts (revenue, cost of goods sold and operating expense), rather than *REVENUE*, as proxy for severity (Palmrose and Scholz 2004; Palmrose et al. 2004; Hribar and Jenkins 2004). However, Palmrose and Scholz (2004) suggest that it is revenue restatements drive core results and argue that revenue (not core) and pervasive restatements are significantly associated with higher payments from the lawsuits. Therefore, I expect that revenue and more pervasive restatements to be associated with a higher likelihood of auditor resignations. And, I replicate my results using an indicator of core accounts.

Next, I include two variables *REVERSE* and *AMOUNT* that capture restatements' impact on the trend of net income and relative size of errors being restated, respectively. Wang and Yu (2008) and Palmrose et al. (2004) find that the market participants react more negatively to more material restatements in dollar impacts. I expect auditors' proclivity to resign from restating companies to increase as the size of errors. Follow Wang and Yu (2008), Palmrose et al. (2004) and Feroz et al. (1991), I compute *AMOUNT* by subtracting restated net income from originally reported income over all restated periods and then scaling the difference by totals assets of companies at the fiscal year-end prior to restatements. To

supplement dollar amount impact, I also include an indicator *REVERSE* when restatements reverse a positive profit to a net loss. I expect *REVERSE* to be positively related to the likelihood of auditor resignations.

An additional test variable, *RETYRS*, measures the duration, persistence of the errors by computing the number of years' financial statements restated. Every additional quarter restated is defined as 0.25. Prior literature finds mixed effects of the number years' financials restated on market reactions. Palmrose et al. (2004) find the market reactions to restatements that correct longer periods of erroneous financials are not significantly more negative. Wang and Yu (2008) find restatements with longer duration are associated with more positive reactions. They interpret their results as evidence that companies take "big bath" to improve the quality of historical financial statements. Given the mixed evidence on *RETYRS* to proxy for materiality of restatements in duration from prior literature, I do not make prediction on the direction of the effects of *RETYRS* on auditor resignations.

I utilize variable *PRESS RELEASE* to capture whether the media by which management chooses to disclose restatement announcements affects auditors' resignations decisions. Once material misstatements are discovered in previous filings, management has a duty to make public announcements to inform financial statement users who are continuing to rely on the financial statements (Skinner 1997). Management has choices to decide whether to employ a press release (with or without 8-K filing) or to use an amended filing to announce restatements until August 2004 when SEC regulation requires 8-K filings. I expect a positive association between the likelihood of auditor resignations and the use of press release (or 8-K) to announce restatements. Companies are strategic in their decision

to employ 8-K filings and tend to use amended filings to make initial announcements if restatements involve lower uncertainty (Plumlee and Yohn 2008). The choice of use of press release (over amended filings) captures increased risks that result from uncertainty related to restatements.

The proxy for increased economic benefits for auditors is measured as percentage increase in audit fees from the preceding year to the year during which restatements are announced (*CHGFEE*). If all else is equal, we expect a higher increase in audit fees mitigate the proclivity of auditor resignations. Therefore, the likelihood of auditors' decisions to resign will decrease as the change in audit fees increases.

3.2.4 Control Variables

In addition to the test variables, my resignation model also controls for the effects of other factors that may affect the likelihood of an auditor resignation, including client and auditor/engagement characteristics. The client characteristics include their size, financial condition, leverage ratio, and an indicator of top management turnovers. I control for size of clients (*SIZE*) because resignations are less likely for larger clients (Johnstone and Bedard 2004; Lee et al. 2004). I also employ *LOSS* as control variable. Blouin et al. (2007), Shu (2000), Krishnan and Krishnan (1997) and Palmrose (1987) argue that auditors are more likely to resign from companies with poor financial performance. *LEVERAGE* captures the agency costs between shareholders and debt holders in the restating company. DeFond et al. (1997) find that auditor-resigned companies are more highly leveraged. Furthermore, prior studies suggest that a change in top management (*CHGMGMT*)

is often associated with a change in auditors (Carcello and Neal 2003; Williams 1988; Schwartz and Menon 1985; Chow and Rice 1982). Therefore, I include an indicator (1 = CEO or CFO change, 0 = no change) to measure top management change during a three-year window surrounding the date when the restatement was disclosed, provided the change in top management predates auditor resignations.

With regard to audit or engagement affects, I control for audit tenure, expertise, and the type of audit opinions received in the periods during which restatements are disclosed. Auditors are more likely to resign if they issue or expect to issue adverse SOX 404 internal control opinions (Ettredge et al. 2007b), or going-concern opinions (Krishnan and Krishnan 1997). I utilize *INTCTR* and *GOCERN* to control for adverse internal control and going concern reports, respectively. I expect that auditors with longer tenures are less likely to resign (*TENURE*) (Krishnan and Krishnan 1997). To supplement tenure effects, I also use an indicator to capture whether a resigning auditor was engaged when misstatements occurred (*SAMEAU*). If auditors have failed to detect material errors which are subsequently restated, I expect the same auditors *less* likely to resign. In addition, I employ two proxies for the auditor's expertise in the client's primary industry. I expect *FEEEXP* (auditors with the greatest audit fees in the client's industry) and *CLTEXP* (auditors with the greatest number of clients in the client's industry) to be negatively associated with resignations, suggesting that auditors are *less* likely to resign if they are industry leader.

3.3 Sample Selection and Results

3.3.1 Sample Selection

In constructing my sample, the primary databases I use are Audit Analytics (AA) and Compustat Fundamental Annual. First, I utilize AA to identify U.S. companies that disclosed financial restatements during 2003-2007. Next, using AA again, I focus on auditors resignations happening from October 2002 through November 2008 (the cut-off period of auditor resignations in AA) to identify restating companies with auditor resignations during the period from 90 days prior through one year subsequent to the restatement announcements. Then, I require my sample companies to have all necessary restatements data in AA and financial data from Compustat, so I analyze a final sample of 1,232 restatements. A total of 133 companies are classified as having resignations surrounding restatement announcements, and 1,099 are classified as continuing engagements, in which auditors neither resign nor are dismissed during the whole sample period. The majority of resignations occur after restatement announcements. Among the 133 resignations, 115 auditors resign following restatements and 18 resignations predate restatement announcements by 90 days or less.⁵⁶

Table 2 Panel A shows the year distribution of total restatements, Resign versus Non-resign companies. On average, about 11 percent of companies that announce financial restatements experience auditor resignations. Resign companies and non-resign companies have similar yearly distribution across 2003-2007. The industry composition for the sample is illustrated in Panel B of Table 2. Panel B illustrates that restatements are most common in the Manufacturing industry. This result is consistent with prior studies describing restatement industry compositions

⁵⁶ Auditors resign on average 54 days prior to the restatements, among which 16 percent (3 out of 18) resign in the preceding one month, 50 percent (9 out of 18) resign in the preceding one to two months, and 33 percent (6 out of) resign in the preceding two to three months. In untabulated analysis, I run the logistics regression by excluding the 18 resignations that predate restatements and find the main results remain unaffected.

(Scholz 2008). Compared to non-resignation sample companies, resignation companies have similar industry distribution but are more focused in the Manufacturing and Technology industries, with 35 percent and 20 percent of the resignation samples, respectively, coming from these two industries.

3.3.2 Results

3.3.2.1 Univariate

Table 3 provides descriptive statistics for Resign and Non-resign companies. Auditors are more likely to resign from companies that restate, on average, about one year and a half financials than from those that restate more than two years (t-stat. 5.96). The percentage change in audit fees is significantly lower for companies with auditors resigning than for those with auditors continuing engagements (t-stat. 2.00). In addition, companies with auditor resignations tend to be smaller in size, report more incidence of loss and be less leveraged. Auditor-resign companies are more likely to receive going-concern opinions (chi-square 3.04). Furthermore, auditors with longer tenure with the engagement are *less* likely to resign (t-stat. 3.84). Univariate significance tests do not control for correlations among variables. Results for some variable differ in multivariate models.

In Table 4 correlation analysis, I find significant correlations between *RESIGN* and the variables *FRUAD*, *RETYRS*, *SIZE*, *LOSS*, *INTCTR*, *GOCERN*, *SAMEAU*, *TENURE*, *FEEEXP* and *CLTEXP*, using both Pearson and Spearman correlation tests. All are in the same direction as the univariate tests in Table 3, with *SIZE* exhibiting the largest correlation with *RESIGN* (0.26 Pearson) in absolute

magnitude. The largest correlations are between *RETTYRS* and *SAMEAU* (0.53 Spearman) and between *SIZE* and *LOSS* (0.36 Pearson). Tests of multi-collinearity for all variables in Table 4 reveal the highest variance inflation factor is 1.3 for *SIZE*, which is below 10.0, the empirical level conventionally accepted as cause for concern.

3.3.2.2 Multivariate

Table 5 presents logistic regression results for the Resign/Non-resign model. Coefficient estimates are generally consistent with the univariate results and also with the expectations about the effects of restatements and audit fees on the likelihood of auditor resignations. The coefficients on *FRAUD*, *NOACCT*, *REVERSE*, and *PRESS RELEASE* are significant, and are consistent with the argument that auditors refuse to continue with engagements having increased audit risk, as presented in H1. More specifically, auditors are more likely to resign from companies that have higher incidences of financial frauds (*FRAUD*). My finding indicates that the presence of a fraudulent accounting in clients' financial statements increases in the odds of auditor resignations by 362 percent (p-value 0.012).⁵⁷

The significant positive coefficient on *NOACCT* indicates that companies disclosing more pervasive restatements are more likely to experience auditor resignations, which is consistent with notion that more pervasive restatements are associated with a higher likelihood of litigation (Palmrose and Scholz 2004). One standard deviation increase in *NOACCT* results in a 128 percent increase in the

⁵⁷ The unconditional odds of audit resignations is 0.12-to-1, which is obtained by dividing the frequency of resignations documented in Table 3 (133) by the frequency of not resignations (1,099).

odds of auditor resignations. For test variables *REVENUE*, given the significance of *NOACCT*, its lack of significance implies that auditors are concerned more about the pervasiveness of the restatements on overall income statements rather than on a particular account such as revenue.

Resignations are positively associated with restatements that reverse positive net income to a loss (*REVERSE*). The Δ odds statistics indicate that companies restating originally reported positive profit to a net loss increase their chances to have auditor resignations by 379 percent. The restated dollar amount is also marginally associated with a company's proclivity to have auditor resignations. One standard deviation increase in *AMOUNT* results in a 771 percent increase in the odds of auditor resignations. The dollar impact appears to be the most important proxy for high audit risk when auditors make their resignation decisions. Combined, the marginal significance for *AMOUNT* and significance for *REVERSE* indicate that auditors are not only concerned about precise restated dollar amounts, but also concerned about whether restatements reverse the trend of net income.

The significant negative coefficient on *RETYRS* means that companies restating earnings for a short period (i.e. fewer years) are more likely to have auditor resignations. *RETYRS* appears to capture the relevance of errors being corrected to companies' current financial situations. In untabulated analysis, I find highly positive and significant correlations between *RETYRS* and the time lapse from when misstatements occur to when they are detected and disclosed (0.86 Pearson). When I replace *RETYRS* in the model with *LAPSE* (natural log of days from initiation of misstatements until restatements are announced), the coefficient of *LAPSE* is positive and significant. Combined, these results suggest that auditors

are more likely to resign when restatements reveal more recent errors, even if the errors endured over fewer periods.

The coefficient on *PRESS RELEASE* is positive and significant, suggesting that restatements that involve uncertainty more likely cause auditors to resign. *PRESS RELEASE* is capturing a high uncertainty related to restatements. In many cases, companies are unclear about the specific qualification effects of restatements on their financial statements until they file amended filings (Palmrose et al. 2004). The use of news press to disclose a restatement implies that restated dollar amount are unknown and the effects could be more material. The variable *PRESS RELEASE* is capturing an increased audit risk, providing impetus for auditors to resign. The results in Table 5 indicate that the odds of auditor resignations in companies disclosing restatements by news press increase by 200 percent.

The negative and significant coefficient for *CHGFEE* is consistent with mitigating effects of increased audit fees on resignation decisions as presented in H2. This implies that, all else equal, companies with a greater fee increase to pay their auditors, have decreased odds of auditor resignations by 59 percent.

Finally, the results on my control variables are consistent with prior literature. Auditor are more likely to resign from smaller companies, which reinforces the arguments in prior literature that small companies account for a relatively smaller proportion of audit firms' revenue (Lee et al. 2004; DeAngelo 1981). The significantly positive coefficients for *INTCTR* and *GOCERN* illustrate that auditor are more likely to resign if they expect to give an adverse SOX 404 internal control opinion (Ettredge et al. 2007b) or a going-concern opinion (Krishnan and Krishnan 1997). In addition, a one standard deviation decrease in *TENTURE* results in an 88

percent increase in adds of auditor resignations. This finding implies that auditor tenure over a shorter period increases the likelihood of auditor resignations because as the tenure of the auditor-client relationship lengthens, auditors develop client-specific knowledge that is *less* likely to lead to an audit failure (Stice 1991). The coefficient on *FEEEXP* is negative and statistically significant, which suggests that auditors, who are the industry leader with the greatest total audit fees in the industry, are more likely not to resign.

The coefficients for remaining control variables do not significantly differ from zero. For variable *LOSS*, *LEVERAGE*, *CHGMGMT* and *SAMEAU*, the lack of significance indicates that company performance, leverage ratio, the likelihood of top management turnovers and the incidence of the same auditors are equally distributed across the Resign and Non-resign samples.

At an overall level, my model appears to appropriately capture variation in auditor resignation decisions as evidenced by the inability to reject the null that the model is an appropriate fit, as indicated by the Hosmer-Lemeshow statistics (p-value 0.15). Similarly, the Receiver Operating Characteristics (ROC) curve analysis, with a test-value of 0.81, provides support that my model exhibits adequate ability to discriminate between the resignation and non-resignation two different situations, as a statistic of 0.70 or better indicates acceptable performance suggested by Hosmer and Lemeshow (2000).

3.4 Additional Tests

3.4.1 New Hires of Auditors

In this section, I analyze the type of auditors that companies hire after their auditors resign. Given that these auditor-resignation companies tend to be risky clients and spend less on auditors, we should observe a meaningful number of those clients switching to non-Big 4 auditors. There are two reasons related to this switch direction. On one hand, Big 4 audit firms strategically decline high audit risk clients. On the other hand, clients with weaker internal control also spend less on auditors. To test this switching pattern, I examine the newly hired auditors for those auditor-resignation companies, and I find that a majority of restating companies that have auditor resignations subsequently are engaged with non-Big 4 auditors, with a proportion of 80 percent (107 out of 133, p-value of chi-square = 0.000).

As shown in Panel A of Table 6, among the 133 auditor-resignation companies in my samples, 63 percent (78) resigning auditors are Big 4 auditors, and 67 percent of those switch to a non-Big 4 successor. More importantly, among companies whose resigning auditors are non-Big 4 auditors, none of them hire (may not have alternatives to hire) Big 4 auditors as successors, and therefore in my context, companies switch to other Big 4 auditors only if their predecessor auditors are Big 4 auditors.

Next, I make a closer comparison between companies switching to Big 4 auditors and those switching to non-Big 4 auditors. This comparison provides evidence that the factors determining auditor resignations also influence companies' choice of new auditors. Specifically, from Panel B of Table 6, I find that companies revealing frauds, or restating net income from a profit to a loss, inclusively switch to non-Big 4 auditors and none to Big 4 auditors. These results

reinforce my conclusion that clients disclosing restatements that reveal frauds or reverse the trend of earnings, are perceived as risky clients and drive auditors to resign.

Companies switching to non-Big 4 auditors pay less on auditors than before, compared to those switching to Big 4 paying a 36 percent increase in audit fees, which is consistent with high premium charged by Big 4 auditors (Ettredge et al. 2007a; Rama and Read 2006; Sankaraguruswamy and Whisenant 2004; Ireland and Lennox 2002; Craswell et al. 1995). In addition, companies switching to non-Big 4 auditors tend to be smaller in size, loss-reporting firms, have lower incidence to replace top management, and more likely receive an adverse SOX 404 internal control report. Companies that hire other Big 4 auditors as successors more likely hire an industry leader prior to restatements (the greatest number of clients in a given industry), with a proportion of 42 percent, compared to those hiring non-Big 4 auditors with a mean of 12 percent of hiring an industry leader before restatements. Pair-wised correlation tests are consistent with main conclusion of univariate tests.

3.4.2 Considering Auditor Dismissals

Recent research argues that resignations and dismissals are driven by different interests (Calderon and Ofobike 2008). However, there is a grey area where resignations and dismissals are intertwined. Managers may dismiss incumbent auditors to pre-empt an explicit resignation because investors respond more negatively to resignations than to dismissals (Griffin and Lont 2007). Auditors can de-facto resign by increasing fees (Ettredge et al 2007a). The logistic analysis in

Table 5 allows us only to study the variation in the dichotomous auditors' resigning or continuing engagement decisions. Nonetheless, Table 5 does not take into account the other type of auditor switches, auditor dismissals. In this section, I expand the scope of reference category groups (where *RESIGN* = 0) by using extended reference groups including auditor dismissals. Including dismissals into the reference category groups will work against finding results consistent with my hypotheses if it is the case that auditor dismissals are a grey area between resignations and continuing engagements. Alternatively, if resignation decisions do not vary between continuing and dismissal auditors, I should expect no differences between the two and find similar implications as indicated the main analysis in Table 5.

Table 7 presents the coefficient estimates and p-values for logistic regression with an extended reference category where *RESIGN* equals "zero" if auditors continue engagements during the whole sample period or are dismissed during the period from 90 days prior through one year subsequent to restatement announcements.⁵⁸ Where coefficient estimates and p-values for other variables are essentially similar to those in Table 5, the results in Table 7 differ from those in Table 5 mainly on *REVERSE* and *AMOUNT*. Compared to Table 5, *AMOUNT* is positively and significantly associated with auditors' proclivity to resign (p-value of 0.016) and however, *REVERSE* loses its significance. These results suggest that resignations decisions are particularly sensitive to restated dollar impact rather than the trend of net income, relative to the extended reference category group. Overall,

⁵⁸ In untabulated logistic regression, I include an indicator (1 = dismissals and = 0 if otherwise) and find that coefficient estimate for the indicator is insignificant (p-value of 0.957) and other results are qualitatively similar to those presented in Table 7. This analysis suggests that there is no systematic difference between auditors continuing engagements and being dismissed, relatively compared to auditors resigning.

the results in Table 7 are consistent with Table 5 and help illustrate that restatement features and change in audit fees played a role in determining auditor resignations regardless of the differences between dismissals or continuing engagements.

3.4.3 Robustness Tests

In this section, I summarize the results of two main sensitivity tests that examine the robustness of the primary results in Table 5.

3.4.3.1 Alternative Resignation Windows

In the above tests, auditor resignation window includes resignations that occur 90 days prior to restatement announcements. An underlying assumption is that auditor resignation that happen in the preceding three months prior to restatements are related to restatement issues.⁵⁹ In this section, I investigate the sensitivity of the results to using an alternative window to allow auditor resignations in the preceding six months prior to restatement announcements. Also, I extend the examined auditor resignation periods to July 2002 through November 2008 (extend the front cut-off period forward for three months; November 2008 is the cut-off period of auditor resignations in AA). Table 8 shows the coefficient estimates, p-values and Δ odds statistics where *RESIGN* equals “one” if auditor resigns during the period 180 days prior through one year subsequent to restatement announcements and “zero” if company neither has auditor resignations nor dismissals throughout the whole sample period.

The main inferences remain unchanged. The extended resignation windows

⁵⁹ When I exclude the samples of resignations before restatement announcements, the results in Table 5 remain unaffected.

increase the power of the model by exhibiting greater “goodness-of-fit” (higher R-square, higher Hosmer-Lemeshow p-values and higher ROC curve statistics). The restated dollar impact is still the most important proxy for increased audit risk when auditors make their resignation decisions. In addition to the findings in Table 5, the results in Table 8 suggest that auditor resignations more likely to happen in companies that replace CEO or CFO and in cases where auditors are not engaged during the misstatement periods. Contrary to my expectation, restatements that correct revenue account are associated with a lower likelihood of auditor resignations, but this association is marginally significant.

3.4.3.2 Alternative Control Groups

In the main tests reported in Table 5, I utilize a reference group where the *RESIGN* equals “zero” if companies neither have auditor resignations nor dismissals during the entire period. In this section, I test the sensitivity of the reference category group by including auditor switches outside this timeline. In this expanded reference group, if a company has auditor realignments 90 days earlier or one year later than the restatement announcement. I consider companies that have auditor realignments outside this timeline as non-switching companies, given an underlying assumption that auditor switches beyond certain timeline are perceived as unrelated to restatement issues. Therefore, in Table 9, *RESIGN* equals “zero” if a company (1) has neither resignations nor dismissals throughout the whole sample period, or (2) auditor resigns or is dismissed 90 days earlier or one year later than restatement announcements.⁶⁰

⁶⁰ In untabulated logistic regression, I include an indicator (1 = switch and = 0 if otherwise) and find that the

When we include the additional auditor switching companies into the reference category group, the coefficient estimates for test variable *REVERSE* and *AMOUNT* lose their significances. The control variables *SIZE*, *GOCERN*, and *TENURE* become insignificant. In addition, the overall explanatory power of the model reduced. Max-rescaled R-square significantly decreases from about 25 percent to 11 percent. All combined, these results indicate that the inclusion of auditor switching companies in the control group mix the effects on restatements on auditor resignation decisions.

3.5 Conclusions

In this paper, I examine voluntary external auditor turnovers related to financial restatements. My study is motivated by the lack of evidence in prior literature about the auditors' resignation decisions in the firms that have had accounting failures or even revealed financial reporting frauds. I find that, on average, among 1,232 companies that reveal financial restatements during 2003-2007 in our sample, 11 percent (133) experience auditor resignations. This results is consistent with the notion that auditor resignations happen infrequently, even if in revealed accounting failures clients which indicate increased audit risk.

I find that restatements involving frauds are associated with a higher likelihood of auditor resignations as they call into question management competence and integrity, which is consistent with auditors strategically refusing association with clients whose management has violated ethics and has questionable credibility.

coefficient for the indicator is insignificant (p-value of 0.932) and other results are qualitatively similar to those presented in Table 9. This result suggests that there are no systematic differences between auditors continuing engagements and switching outside the timeline, compared to auditors resigning.

Auditors are more likely to resign in cases where restatements reverse positive earnings to loss, affect more accounts and have more negative change in net income. The restated dollar impact appears to be the most important proxy for increased audit risk driving auditors to resign.

I also conjecture that restatements that affect more years are more associated with auditor resignations. Additional analysis of the duration and lapse of restatements suggests that auditors are more likely to resign when restatements correcting more recent errors, even if the errors affect a longer periods of financials. I document a positive association between auditor resignations and the use of press releases to reveal restatements. This provides evidence the choices in the media of restatement disclosures affect auditor resignation decisions, implying that the choice of press release proxying for uncertainty.

Auditor resignation decisions are driven by risk aversion incentives, however, a majority of auditors in companies that reveal financial restatements, continue engagements with their restating clients. I conjecture that a greater increase in audit fees will balance out the risks and is associated with a higher likelihood for auditor to stay. Resignations occur *less* as increases in audit fees from the prior year to the year of restatements are high. This result is consistent with the notion that an increase in auditors' economic benefits mitigates their resignation decisions.

Overall, my results indicate that auditors trade-off risks and benefits in their resignation decisions. The characteristics of the restatement and increase in audit fees are both important in determining auditor resignation decisions. Additional analysis suggests that these factors also influence the type of auditors subsequently hired following resignations. My results are robustness to the inclusion of auditor

dismissals in the reference category group and the use of alternative resignations windows.

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Table 3.1 Definitions of Variables

Variable	Definition
Dependent Variable	
RESIGN	= 1 if auditors resign, 90 days prior to or one year subsequent to restatement announcements, = 0 if company neither has auditor resignations nor dismissals throughout the whole sample period.
Restatement features	
FRAUD	= 1 if company discloses that the misstatements are due to fraud or irregularities in its filings or press release and 0 otherwise.
REVENUE	= 1 if a restatement corrects revenue account, and 0 otherwise.
REVERSE	= 1 if a restatement changes originally reported net income to a loss, and 0 otherwise.
NOACCT	= Number of account groups involved in a restatement. The seven account groups are revenue, cost of sales, operating expenses, one-time special items, merger-related, non-operating expenses and other items.
AMOUNT	= Restated net income (loss) minus originally reported net income (loss) over restated period, scaled by total assets reported at fiscal year-end prior to restatement announcements.
RETYRS	= Number of years restated, where a fiscal year = 1 and each additional quarter = 0.25.
PRESS RELEASE	= 1 if a restatement is disclosed by news releases or 8-K, and 0 otherwise
H2: CHGFEE	= Percent change in audit fees from year t-1 to year t, year t is the restatement year.
Control variables	
Client characteristics:	
SIZE	= Natural logarithm of total assets at the fiscal year-end prior to a restatement announcement.
LOSS	= 1 if ROA in restating firm is less than 0 in the fiscal year-end prior to restatement announcements, and 0 otherwise. ROA, Return on Assets = Operating Income Before Depreciation/[(Total assets in year t + Total assets in year t-1)/2]
Client characteristics:	
LEVERAGE	= Total debt / total assets at the fiscal year-end prior to a restatement announcement.
CHGMGMT	= 1 if company changes CEO or CFO during a three-year window, from one year before to one year after a restatement announcement, provided the management change preceded the dates of auditor resignation and 0 otherwise.
INTCTR	= 1 if company receives an adverse internal control opinion, in the restatement year, and 0 otherwise.
GOCERN	= 1 if company receives a going-concern opinion in the restatement year, and 0 otherwise
SAMEAU	=1 if resigning auditors are engaged during the misstatement periods and 0 otherwise. Exclude the situation where the resigning auditor came in during a misstated quarter, detect and fix the errors in the audit of annual results.
TENTURE	= Number of consecutive years the auditors performed the audit per Compustat by fiscal year-end prior to a restatement announcement.
FEEEXP	=1 if the auditor has the greatest total audit fees in the industry at the fiscal year-end prior to a restatement announcement and 0 otherwise.
CLTEXP	= 1 if the auditor has the greatest number of clients in an industry at the fiscal year-end prior to a restatement announcement and 0 otherwise.

Table 3.2
Sample Yearly and Industry Composition

Panel A:

	<u>Non-resign sample</u>		<u>Resign sample</u>		<u>Total restatements</u>	
	Number	Freq.	Number	Freq.	Number	resign/all
2003	120	10.9	12	9.0	132	10.0
2004	169	15.4	26	19.5	195	15.4
2005	343	31.2	44	33.1	387	12.8
2006	279	25.4	40	30.1	319	14.3
2007	188	17.1	11	8.3	199	5.9
	1,099	100.0	133	100.0	1,232	10.8

Panel B:

Industry	<u>Non-resign sample</u>		<u>Resign sample</u>		<u>Total restatements</u>	
	Number	Freq.	Number	Freq.	Number	Freq.
Agriculture, construction, mining	76	6.9	11	8.3	87	7.1
Manufacturing	291	26.5	47	35.3	338	27.4
Technology	124	11.3	26	19.5	150	12.2
Transportation	30	2.7	0	0.0	30	2.4
Communication	62	5.6	3	2.3	65	5.3
Utilities	51	4.6	4	3.0	55	4.5
Wholesale/Retail	164	14.9	11	8.3	175	14.2
Financial	175	15.9	18	13.5	193	15.7
Services	104	9.5	11	8.3	115	9.3
Unknown	22	2.0	2	1.5	24	1.9
	1,099	100	133	100	1,232	100

Industry membership is determined by primary SIC code as follows: Agriculture, construction, mining (0000-1999), Manufacturing (2000-3999 except Technology), Technology (2834-2836, 3570-3579, 7370-7379), Transportation (4000-4799), Communication (4800-4899), Utilities (4900-4999), Wholesale/Retail (5000-5999), Financial (6000-6999), and Services (7000-8999 except Technology).

Table 3.3
Descriptive Statistics of Regression Variables

Test Variable	Non-resign sample (n=1,099)			Resign sample (n=133)			Test of Differences	
	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	<u>t-test/chi-sqr.</u>	<u>z-score</u>
Restatement features (H1)								
<i>FRAUD</i>	0.017	0.000	0.130	0.045	0.000	0.208	0.80	
<i>REVENUE</i>	0.190	0.000	0.393	0.195	0.000	0.398	0.00	
<i>NOACCT</i>	1.962	2.000	0.980	2.038	2.000	1.111	-0.75	0.51
<i>REVERSE</i>	0.008	0.000	0.090	0.023	0.000	0.149	0.47	
<i>AMOUNT</i>	-0.030	0.000	0.417	-0.008	-0.001	0.075	-1.53	-0.44
<i>RETYRS</i>	2.304	2.000	1.857	1.620	1.250	1.154	5.96	*** -3.85 **
<i>PRESS RELEASE</i>	0.117	0.000	0.322	0.150	0.000	0.359	0.19	
Mitigating effects of higher fees (H2)								
<i>CHGFEE</i>	0.126	0.123	0.417	0.009	0.115	0.657	2.00	** -0.27
Client characteristics:								
<i>SIZE</i>	6.731	6.608	2.184	4.875	4.557	2.195	9.26	*** -9.21 **
<i>LOSS</i>	0.156	0.000	0.363	0.353	0.000	0.480	4.11	**
<i>LEVERAGE</i>	0.266	0.211	0.285	0.217	0.091	0.256	1.90	* -2.81 **
<i>CHGMGMT</i>	0.081	0.000	0.273	0.060	0.000	0.239	0.09	
Auditor and audit engagement characteristics:								
<i>INTCTR</i>	0.158	0.000	0.365	0.241	0.000	0.429	0.79	
<i>GOCERN</i>	0.029	0.000	0.168	0.105	0.000	0.308	3.04	*
<i>SAMEAU</i>	0.854	1.000	0.354	0.729	1.000	0.446	1.64	
<i>TENTURE</i>	4.088	4.000	1.759	3.474	4.000	1.593	3.84	*** -3.81 **
<i>FEEEXP</i>	0.289	0.000	0.454	0.143	0.000	0.351	1.43	
<i>CLTEXP</i>	0.299	0.000	0.458	0.180	0.000	0.386	0.91	

Variables are defined in Table 1.

Test of differences present t-statistics (chi-squares if appropriate) or z-scores from the comparison of Non-resign and Resign companies' mean or median values.

**Table 3.4
Correlations**

	RESIGN	FRAUD	REVENUE	REVERSE	NOACCT	AMOUNT	RETYRS	PRESS	CHGFEE	SIZE	LOSS	LEVERAGE
SIGN	1.000	0.061 (0.032)	0.004 (0.883)	0.045 (0.111)	0.024 (0.407)	0.017 (0.549)	-0.118 (0.000)	0.031 (0.271)	-0.081 (0.005)	-0.255 (0.000)	0.161 (0.000)	-0.054 (0.058)
AUD	0.061 (0.032)	1.000	0.121 (0.000)	-0.014 (0.617)	0.195 (0.000)	0.005 (0.861)	0.141 (0.000)	0.088 (0.002)	0.029 (0.303)	0.060 (0.034)	-0.006 (0.823)	-0.012 (0.676)
VENUE	0.004 (0.883)	0.121 (0.000)	1.000	0.015 (0.600)	0.274 (0.000)	0.009 (0.760)	0.053 (0.063)	0.086 (0.003)	0.038 (0.181)	0.004 (0.902)	0.056 (0.048)	-0.048 (0.089)
VERSE	0.045 (0.111)	-0.014 (0.617)	0.015 (0.600)	1.000	0.036 (0.204)	-0.013 (0.654)	-0.024 (0.398)	-0.037 (0.197)	0.016 (0.577)	-0.031 (0.273)	-0.046 (0.107)	-0.030 (0.288)
ACCT	0.015 (0.611)	0.134 (0.000)	0.226 (0.000)	0.023 (0.430)	1.000	-0.019 (0.510)	0.178 (0.000)	0.094 (0.001)	-0.039 (0.172)	0.126 (0.000)	-0.065 (0.022)	0.098 (0.001)
OUNT	-0.013 (0.659)	-0.104 (0.000)	-0.118 (0.000)	-0.122 (0.000)	-0.167 (0.000)	1.000	-0.018 (0.530)	0.010 (0.737)	0.048 (0.092)	0.042 (0.142)	-0.025 (0.379)	-0.024 (0.405)
STYRS	-0.110 (0.000)	0.130 (0.000)	0.053 (0.064)	-0.013 (0.640)	0.219 (0.000)	-0.160 (0.000)	1.000	0.093 (0.001)	0.002 (0.957)	0.212 (0.000)	-0.142 (0.000)	-0.003 (0.924)
ESS	0.031 (0.271)	0.088 (0.002)	0.086 (0.003)	-0.037 (0.197)	0.090 (0.002)	-0.117 (0.000)	0.086 (0.003)	1.000	0.028 (0.319)	0.043 (0.129)	-0.042 (0.145)	0.004 (0.883)
GFEE	-0.008 (0.791)	0.053 (0.062)	0.065 (0.022)	0.007 (0.803)	-0.010 (0.730)	-0.006 (0.821)	-0.004 (0.889)	0.049 (0.088)	1.000	0.017 (0.540)	-0.043 (0.129)	0.002 (0.932)
E	-0.263 (0.000)	0.052 (0.067)	0.004 (0.891)	-0.043 (0.130)	0.121 (0.000)	0.101 (0.000)	0.218 (0.000)	0.056 (0.048)	-0.004 (0.898)	1.000	-0.357 (0.000)	0.149 (0.000)
SS	0.161 (0.000)	-0.006 (0.823)	0.056 (0.048)	-0.046 (0.107)	-0.080 (0.005)	-0.013 (0.657)	-0.154 (0.000)	-0.042 (0.145)	0.006 (0.823)	-0.357 (0.000)	1.000	-0.020 (0.478)
VERAGE	-0.080 (0.005)	-0.006 (0.828)	-0.053 (0.063)	-0.028 (0.319)	0.088 (0.002)	0.038 (0.186)	0.020 (0.485)	0.009 (0.746)	0.005 (0.870)	0.324 (0.000)	-0.083 (0.004)	1.000

To be continued

	RESIGN	FRAUD	REVENUE	REVERSE	NOACCT	AMOUNT	RETYRS	PRESS	CHGFEE	SIZE	LOSS	LEVERAGE
GMGMT	-0.024 (0.400)	0.043 (0.128)	-0.027 (0.346)	0.032 (0.256)	-0.018 (0.538)	-0.029 (0.316)	0.044 (0.122)	0.021 (0.462)	-0.090 (0.002)	0.110 (0.000)	-0.112 (0.000)	-0.057 (0.044)
CTR	0.068 (0.016)	0.151 (0.000)	0.131 (0.000)	-0.022 (0.434)	0.143 (0.000)	-0.127 (0.000)	0.137 (0.000)	0.114 (0.000)	0.064 (0.026)	0.098 (0.001)	-0.031 (0.276)	-0.023 (0.414)
CERN	0.125 (0.000)	-0.028 (0.320)	0.024 (0.395)	-0.020 (0.493)	-0.043 (0.135)	-0.048 (0.094)	-0.067 (0.019)	-0.034 (0.238)	-0.044 (0.123)	-0.123 (0.000)	0.223 (0.000)	0.009 (0.754)
MEAU	-0.105 (0.000)	0.047 (0.099)	0.003 (0.909)	0.021 (0.468)	0.143 (0.000)	-0.085 (0.003)	0.528 (0.000)	0.019 (0.501)	-0.037 (0.191)	0.111 (0.000)	-0.111 (0.000)	-0.017 (0.556)
NURE	-0.109 (0.000)	0.002 (0.945)	-0.035 (0.216)	0.017 (0.551)	-0.047 (0.101)	0.018 (0.525)	0.098 (0.001)	0.032 (0.257)	-0.169 (0.000)	0.136 (0.000)	-0.128 (0.000)	-0.084 (0.003)
EEXP	-0.102 (0.000)	0.028 (0.328)	0.064 (0.026)	-0.024 (0.404)	0.087 (0.002)	-0.032 (0.267)	0.089 (0.002)	0.074 (0.009)	-0.037 (0.195)	0.206 (0.000)	-0.075 (0.009)	0.069 (0.015)
IEXP	-0.082 (0.004)	-0.002 (0.942)	0.062 (0.028)	-0.045 (0.118)	0.032 (0.264)	-0.008 (0.782)	0.062 (0.031)	0.046 (0.109)	-0.093 (0.001)	0.166 (0.000)	-0.021 (0.462)	0.039 (0.172)

To be continued

	CHGMGMT	INTCTR	GOCERN	SAMEAU	TENURE	FEEEXP	CLTEXP
RESIGN	-0.024 (0.400)	0.068 (0.016)	0.125 (0.000)	-0.105 (0.000)	-0.109 (0.000)	-0.102 (0.000)	-0.082 (0.004)
FRAUD	0.043 (0.128)	0.151 (0.000)	-0.028 (0.320)	0.047 (0.099)	0.005 (0.867)	0.028 (0.328)	-0.002 (0.942)
REVENUE	-0.027 (0.346)	0.131 (0.000)	0.024 (0.395)	0.003 (0.909)	-0.037 (0.197)	0.064 (0.026)	0.062 (0.028)
REVERSE	0.032 (0.256)	-0.022 (0.434)	-0.020 (0.493)	0.021 (0.468)	0.022 (0.433)	-0.024 (0.404)	-0.045 (0.118)
NOACCT	-0.015 (0.589)	0.160 (0.000)	-0.041 (0.146)	0.141 (0.000)	-0.035 (0.219)	0.083 (0.004)	0.028 (0.323)
AMOUNT	0.016 (0.576)	0.018 (0.527)	-0.089 (0.002)	0.001 (0.982)	0.047 (0.098)	0.017 (0.550)	0.009 (0.749)
RETYRS	0.075 (0.008)	0.169 (0.000)	-0.060 (0.037)	0.390 (0.000)	0.105 (0.000)	0.084 (0.003)	0.069 (0.016)
PRESS	0.021 (0.462)	0.114 (0.000)	-0.034 (0.238)	0.019 (0.501)	0.031 (0.278)	0.074 (0.009)	0.046 (0.109)
CHGFEE	-0.084 (0.003)	0.052 (0.068)	-0.091 (0.002)	-0.010 (0.729)	-0.082 (0.004)	-0.008 (0.782)	-0.063 (0.026)
SIZE	0.097 (0.001)	0.112 (0.000)	-0.138 (0.000)	0.115 (0.000)	0.143 (0.000)	0.202 (0.000)	0.154 (0.000)
LOSS	-0.112 (0.000)	-0.031 (0.276)	0.223 (0.000)	-0.111 (0.000)	-0.129 (0.000)	-0.075 (0.009)	-0.021 (0.462)
LEVERAGE	-0.061 (0.034)	-0.026 (0.362)	0.005 (0.861)	-0.005 (0.871)	-0.098 (0.001)	0.039 (0.174)	0.022 (0.441)
CHGMGMT	1.000	0.063 (0.027)	-0.042 (0.144)	0.012 (0.663)	0.153 (0.000)	0.010 (0.728)	-0.005 (0.853)
INTCTR	0.063 (0.027)	1.000	0.026 (0.353)	0.006 (0.845)	0.135 (0.000)	0.062 (0.030)	-0.019 (0.497)
GOCERN	-0.042 (0.144)	0.026 (0.353)	1.000	-0.078 (0.006)	-0.081 (0.005)	-0.034 (0.228)	-0.068 (0.017)
SAMEAU	0.012 (0.663)	0.006 (0.845)	-0.078 (0.006)	1.000	0.114 (0.000)	0.064 (0.025)	0.071 (0.013)
TENURE	0.158 (0.000)	0.136 (0.000)	-0.076 (0.008)	0.116 (0.000)	1.000	0.094 (0.001)	0.065 (0.023)
FEEEXP	0.010 (0.728)	0.062 (0.030)	-0.034 (0.228)	0.064 (0.025)	0.087 (0.002)	1.000	0.425 (0.000)
CLTEXP	-0.005 (0.853)	-0.019 (0.497)	-0.068 (0.017)	0.071 (0.013)	0.061 (0.031)	0.425 (0.000)	1.000

Bottom triangle presents Spearman correlations and top triangle presents Pearson correlations. P-values are two-tailed and in parenthesis. See Table 1 for variable definitions.

Table 3.5
Logistic Regression of Auditor Resignations

<u>Variable</u>	<u>Sign</u>	<u>Coeff. Est.</u>	<u>p-value</u>	<u>ΔOdds</u>	
Restatement features (H1):					
<i>FRAUD</i>	+	1.287	0.012	**	3.62
<i>REVENUE</i>	+	-0.221	0.205		0.80
<i>REVERSE</i>	+	1.333	0.033	**	3.79
<i>NOACCT</i>	+	0.246	0.013	**	1.28
<i>AMOUNT</i>	+	2.043	0.070	*	7.71
<i>RETYRS</i>	+/-	-0.225	0.009	***	0.80
<i>PRESS RELEASE</i>	+	0.693	0.009	***	2.00
Mitigating effects of higher fees (H2):					
<i>CHGFEE</i>	-	-0.535	0.002	***	0.59
Client characteristics:					
<i>SIZE</i>	-	-0.442	0.000	***	0.64
<i>LOSS</i>	+	0.081	0.375		1.08
<i>LEVERAGE</i>	+	-0.285	0.381		0.75
<i>CHGMGMT</i>	+	0.151	0.718		1.16
Auditor and audit engagement characteristics:					
<i>INTCTR</i>	+	1.096	0.000	***	2.99
<i>GOCERN</i>	+	0.768	0.027	**	2.16
<i>SAMEAU</i>	-	-0.308	0.120		0.74
<i>TENTURE</i>	-	-0.133	0.012	**	0.88
<i>FEEEXP</i>	-	-0.506	0.044	**	0.60
<i>CLTEXP</i>	-	-0.056	0.419		0.95
<i>Intercept</i>		0.948	0.041	**	
n Resign					133
n Non-Resign					1,099
Max-rescaled R-Square					0.25
Hosmer-Lemeshow p-value					0.15
ROC curve statistic					0.81

This table presents binary results modeling the probability that an auditor resigns from companies with restatements (*RESIGN=1*) versus the reference category of continuing engagements with restating companies (*RESIGN=0*).⁶¹

The variable definitions are presented in Table 1.

P-values are one-tailed when the sign of the coefficient is predicted, two-tailed tests otherwise. ΔOdds represents the change in odds of auditor resignations given a standard deviation change in the independent variable of interest for continuous variables and relative to the “zero” category for the indicator variables. The unconditional odds of auditor resignations is 0.12-to-1.

⁶¹ RESIGN =1 if auditors resign, during the period from 90 days prior though one year subsequent to restatement announcements, = 0 if company neither has auditor resignations nor dismissals throughout the whole sample period.

The Hosmer-Lemeshow statistic is a measure of the “goodness of fit” of the model that is developed by comparing the observed versus expected frequencies across intervals that are determined using the estimated probabilities obtained from the model (Hosmer and Lemeshow 2000). The null hypothesis is that the model has an appropriate fit. Small p-values for the statistics indicate a lack of model fit.

The Receiver Operating Characteristics (ROC) curve statistic measures the area under the ROC curve, which provides an assessment of the model’s ability to discriminate between those subjects who experience the event of interest versus those who do not. The area under an ROC curve ranges from 0.5, no discrimination of the outcomes, to 1, a perfect discrimination. Hosmer and Lemeshow (2000) suggest a statistic of 0.70 or greater indicates acceptable model discrimination.

Table 3.6
Type of New Auditors

Panel A:

Switch Direction	n	%
Big 4 to Big 4	26	33%
Big 4 to non-Big 4	52	67%
Total resigning Big 4 firms	78	63%
Non-Big 4 to Big 4	0	0%
Non-Big 4 to non-Big 4	55	100%
Total resigning Non-big 4 firms	55	37%
Total resignation sample	133	100%
Chi-Square (p-value)	25.30	0.000

Panel B:

Test Variable	NEWNB4=0 (n=26)			NEWNB4=1 (n=107)			Test of Differences	
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	t-test/chi-sq	z-score
Restatement features (H1)								
<i>FRAUD</i>	0.000	0.000	0.000	0.056	0.000	0.231	259.07	***
<i>REVENUE</i>	0.192	0.000	0.402	0.196	0.000	0.399	0.01	
<i>ACCOUNT</i>	1.808	2	0.749	2.093	2	1.178	-1.54	-0.89
<i>REVERSE</i>	0	0	0	0.028	0	0.166	61.86	***
<i>AMOUNT</i>	-0.002	-0.001	0.008	-0.010	-0.001	0.084	0.95	0.30
<i>RESTYRS</i>	1.644	1.250	1.188	1.614	1.250	1.151	0.12	0.23
<i>PRESS RELEASE</i>	0.231	0.000	0.430	0.131	0.000	0.339	4.87	**
Mitigating effects of higher fees (H2)								
<i>CHGFEE</i>	0.361	0.433	0.429	-0.076	0.058	0.675	4.10	*** 3.29 ***
Client characteristics:								
<i>SIZE</i>	6.927	6.872	2.162	4.376	4.215	1.900	5.98	*** 5.09 ***
<i>LOSS</i>	0.077	0.000	0.272	0.421	0.000	0.496	142.95	***
<i>LEVERAGE</i>	0.286	0.159	0.305	0.200	0.076	0.241	1.55	1.70 *
<i>CHGMGMT</i>	0.115	0.000	0.326	0.047	0.000	0.212	3.93	**
Auditor and audit engagement characteristics:								
<i>INTCTR</i>	0.385	0.000	0.496	0.206	0.000	0.406	11.71	***
<i>GOCERN</i>	0.038	0.000	0.196	0.121	0.000	0.328	16.46	***
<i>SAMEAU</i>	0.769	1.000	0.430	0.720	1.000	0.451	1.18	
<i>TENTURE</i>	3.923	4.000	1.093	3.364	3.000	1.679	2.08	1.51
<i>FEEEXP</i>	0.231	0.000	0.430	0.121	0.000	0.328	5.82	
<i>CLTEXP</i>	0.423	0.000	0.504	0.121	0.000	0.328	32.06	***

NEWNB4 = 1 if new auditor is not one of the Big 4; 0 otherwise. All the other variables are defined in Table 1.

Test of differences present t-statistics (chi-squares if appropriate) or z-scores from the comparison of mean or median values for companies hiring new Big 4 and new non-Big 4 auditors.

Panel C:

	NOACCT	AMOUNT	RETYRS	CHGFEE	SIZE	LEVERAGE	TENURE
NEWNB4	0.102 (0.241)	-0.042 (0.635)	-0.010 (0.907)	-0.265 (0.002)	-0.463 (0.000)	-0.134 (0.124)	-0.140 (0.109)
NOACCT		-0.019 (0.828)	0.218 (0.012)	-0.069 (0.431)	0.083 (0.342)	0.063 (0.474)	-0.062 (0.482)
AMOUNT			0.011 (0.899)	-0.128 (0.141)	0.116 (0.185)	0.163 (0.060)	0.032 (0.712)
RETYRS				0.063 (0.474)	0.214 (0.014)	0.006 (0.947)	0.105 (0.230)
CHGFEE					0.127 (0.145)	0.021 (0.815)	0.111 (0.205)
SIZE						0.182 (0.036)	0.202 (0.020)
LEVERAGE							-0.261 (0.002)

**Table 3.7: Additional test-----Including auditor dismissals
Multinomial Logistic Regression of the Resign Decisions**

<u>Variable</u>	<u>Sign</u>	<u>Coeff. Est.</u>	<u>p-value</u>	<u>ΔOdds</u>
Restatement features (H1):				
<i>FRAUD</i>	+	1.333	0.007 ***	3.79
<i>REVENUE</i>	+	-0.136	0.299	0.87
<i>NOACCT</i>	+	0.224	0.013 **	1.25
<i>REVERSE</i>	+	0.392	0.276	1.48
<i>AMOUNT</i>	+	2.679	0.016 **	14.57
<i>RETYRS</i>	+/-	-0.253	0.003 ***	0.78
<i>PRESS RELEASE</i>	+	0.637	0.011 **	1.89
Mitigating effects of higher fees (H2):				
<i>CHGFEE</i>	-	-0.334	0.015 **	0.72
Client characteristics:				
<i>SIZE</i>	-	-0.362	0.000 ***	0.70
<i>LOSS</i>	+	-0.069	0.386	0.93
<i>LEVERAGE</i>	+	-0.386	0.165	0.68
<i>CHGMGMT</i>	+	0.254	0.535	1.29
Auditor and audit engagement characteristics:				
<i>INTCTR</i>	+	0.921	0.000 ***	2.51
<i>GOCERN</i>	+	0.893	0.008 ***	2.44
<i>SAMEAU</i>	-	-0.104	0.337	0.90
<i>TENTURE</i>	-	-0.097	0.044 **	0.91
<i>FEEEXP</i>	-	-0.647	0.013 **	0.52
<i>CLTEXP</i>	-	0.024	0.464	1.03
<i>Intercept</i>		0.082	0.847	
n Resign				133
n Non-Resign				1,396
Max-rescaled R-Square				0.18
Hosmer-Lemeshow p-value				0.58
ROC curve statistic				0.79

This table presents binary results modeling the probability that an auditor resigns from companies with restatements ($RESIGN = 1$) versus the reference category of continuing engagements or being dismissed from restating companies ($RESIGN = 0$).⁶² P-values are one-tailed when the sign of the coefficient is predicted, two-tailed tests otherwise. ΔOdds represents the change in odds of auditor resignations given a standard deviation change in the independent variable of interest for continuous variables and relative to the “zero” category for the indicator variables. The unconditional odds of auditor resignations is 0.10-to-1. The definitions for other variables are presented in Table 1.

⁶² $RESIGN = 1$ if auditor resigns during the period from 90 prior up to one year subsequent to restatement announcements, = 0 if company 1. neither has auditor resignations nor dismissals throughout the whole sample period, or 2. auditor is dismissed during the period from 90 days prior up to one year subsequent to restatement announcements.

**Table 3.8: Robustness Tests-----Alternative Resignation Windows
Multinomial Logistic Regression of the Resign Decisions**

<u>Variable</u>	<u>Sign</u>	<u>Coeff. Est.</u>	<u>p-value</u>	<u>ΔOdds</u>
Restatement features (H1):				
<i>FRAUD</i>	+	0.964	0.050 **	2.62
<i>REVENUE</i>	+	-0.352	0.092 *	0.70
<i>NOACCT</i>	+	0.268	0.006 ***	1.31
<i>REVERSE</i>	+	1.305	0.041 **	3.69
<i>AMOUNT</i>	+	1.918	0.053 *	6.81
<i>RETYRS</i>	+/-	-0.189	0.014 **	0.83
<i>PRESS</i>	+	0.446	0.068 *	1.56
<i>RELEASE</i>				
Mitigating effects of higher fees (H2):				
<i>CHGFEE</i>	-	-0.473	0.005 ***	0.62
Client characteristics:				
<i>SIZE</i>	-	-0.360	0.000 ***	0.70
<i>LOSS</i>	+	0.193	0.218	1.21
<i>LEVERAGE</i>	+	-0.391	0.223	0.68
<i>CHGMGMT</i>	+	0.947	0.003 ***	2.58
Auditor and audit engagement characteristics:				
<i>INTCTR</i>	+	1.275	0.000 ***	3.58
<i>GOCERN</i>	+	0.943	0.007 ***	2.57
<i>SAMEAU</i>	-	-0.454	0.033 **	0.64
<i>TENTURE</i>	-	-0.259	0.000 ***	0.77
<i>FEEEXP</i>	-	-0.651	0.014 **	0.52
<i>CLTEXP</i>	-	-0.246	0.180	0.78
<i>Intercept</i>		1.170	0.008	
n Resign				156
n Non-Resign				1,055
Max-rescaled R-Square				0.29
Hosmer-Lemeshow p-value				0.80
ROC curve statistic				0.82

This table presents binary results modeling the probability that an auditor resigns from companies with restatements (*RESIGN = 1*) versus the reference category of continuing engagements with restating companies. (*RESIGN = 0*).⁶³ P-values are one-tailed when the sign of the coefficient is predicted, two-tailed tests otherwise. ΔOdds represents the change in odds of auditor resignations given a standard deviation change in the independent variable of interest for continuous variables and relative to the “zero” category for the indicator variables. The unconditional odds of auditor resignations is 0.15-to-1. The definitions for other variables are presented in Table 1.

⁶³ RESIGN = 1 if auditors resign, during the period from 180 days prior up to one year subsequent to restatement announcements, = 0 if company neither has auditor resignations nor dismissals throughout the whole sample period.

**Table 3.9: Robustness Tests-----Alternative Control Groups
Multinomial Logistic Regression of the Resign Decisions**

<u>Variable</u>	<u>Sign</u>	<u>Coeff. Est.</u>	<u>p-value</u>	<u>ΔOdds</u>	
Restatement features (H1):					
<i>FRAUD</i>	+	1.187	0.007	***	3.28
<i>REVENUE</i>	+	-0.063	0.398		0.94
<i>NOACCT</i>	+	0.128	0.09	*	1.14
<i>REVERSE</i>	+	0.448	0.242		1.57
<i>AMOUNT</i>	+	0.592	0.116		1.81
<i>RETYRS</i>	+/-	-0.221	0.006	***	0.80
<i>PRESS</i>	+	0.407	0.064	*	1.50
<i>RELEASE</i>					
Mitigating effects of higher fees (H2):					
<i>CHGFEE</i>	—	-0.422	0.007	***	0.66
Client characteristics:					
<i>SIZE</i>	—	-0.262	0.911		0.77
<i>LOSS</i>	+	0.026	0.515		1.03
<i>LEVERAGE</i>	+	-0.204	0.792		0.82
<i>CHGMGMT</i>	+	-0.105	0.002	***	0.90
Auditor and audit engagement characteristics:					
<i>INTCTR</i>	+	0.753	0.001	***	2.12
<i>GOCERN</i>	+	0.370	0.141		1.45
<i>SAMEAU</i>	—	0.125	0.299		1.13
<i>TENTURE</i>	—	0.058	0.143		1.06
<i>FEEEXP</i>	—	-0.450	0.061	*	0.64
<i>CLTEXP</i>	—	0.022	0.467		1.02
<i>Intercept</i>		-1.460	0.000	***	
n					133
Resign					
n Non-Resign					1,913
Max-rescaled R-Square					0.11
Hosmer-Lemeshow p-value					0.86
ROC curve statistic					0.74

This table presents binary results modeling the probability that an auditor resigns from companies with restatements (*RESIGN* = 1) versus the reference category of resigning or being dismissed earlier than 90 days prior to or later than one year subsequent to restatement announcements (*RESIGN* = 0).⁶⁴ P-values are one-tailed when the sign of the coefficient is predicted, two-tailed tests otherwise. ΔOdds represents the change in odds of auditor resignations given a standard deviation change in the independent variable of interest for continuous variables and relative to the “zero” category for the indicator variables. The unconditional odds of auditor resignations is 0.10-to-1. The definitions for other variables are presented in Table 1.

⁶⁴ RESIGN = 1 if auditor resigns during the period from 90 days prior up to one year subsequent to restatement announcements, = 0 if auditor resigns or is dismissed 90 days earlier than or one year later than restatement announcements.