Accounting Complexity and Misreporting: Manipulation or Mistake?

by

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Dedication

I dedicate this dissertation to my dad, Dr. Gerald E. Peterson,
a great father and supporter of my education,
and a scholar I did not know very well.

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Abstract

I explore the effect of accounting complexity on misreporting using a setting of revenue restatements. I measure revenue recognition complexity using a factor score based on the number of words and revenue recognition methods from the revenue recognition disclosure in the 10-K just prior to the restatement announcement. Results are consistent with revenue recognition complexity increasing the probability of revenue restatements, after controlling for other determinants of misreporting revenue. These results are significant both statistically and economically and are robust to a number of different specifications. I also test whether misreporting for complex revenue recognition firms is the result of mistakes or manipulation. My tests provide no evidence consistent with complex revenue recognition being associated with manipulating revenue. However, there is evidence that firms that restate revenue and have more complex revenue recognition are less likely to receive an AAER from the SEC and have less negative restatement announcement returns than firms with less complex revenue recognition, suggesting mistakes are more likely for more complex firms.

Chapter 1. Introduction

Regulators' recent concern about complexity in financial reporting is predicated on the belief that complexity is costly to the financial markets. In regard to one particular cost, in December 2005 both Chairman Cox of the Securities and Exchange Commission (SEC) and Chairman Herz of the Financial Accounting Standards Board (FASB) suggested complex accounting and reporting was a major contributor to the increase in financial statement misreporting (Cox, 2005 and Herz, 2005). I investigate the effect of accounting complexity on financial statement misreporting, a question largely unexplored in the academic literature. I use revenue recognition as a setting to investigate this effect for three reasons. First, revenue recognition is a universal accounting issue that affects many, if not all, firms. In addition, prior research shows that revenue misreporting is a common type of restatement (Palmrose et al., 2004; GAO, 2002 and 2006), ensuring I can obtain a sufficiently large sample to test the effects of complexity on misreporting. Finally, anecdotal evidence suggests that revenue recognition can be complex for firms (Sondhi and Taub, 2006; Herz, 2007; Turner, 2001).

I define accounting complexity as the amount of uncertainty related to the mapping of transactions (or potential transactions) and standards into financial statements.[1] This definition incorporates complexity relevant to both preparers and users of financial statements. The uncertainty could result from unpredictable business

[1] Prior literature has not developed a formal definition of accounting complexity. While the SEC has been consistent in their discussion of complexity in SEC speeches and testimony, to my knowledge they have yet to formally define the concept.

environments, imperfect standards, or imperfect information about transactions. I conjecture that a description of the revenue recognition process captures aspects of this uncertainty. Therefore, I measure accounting complexity as it relates to revenue recognition using a factor score based on the number of words and number of revenue recognition methods from the firm's revenue recognition disclosure.

There are two competing (although not exclusive) theories about how accounting complexity might affect financial misreporting. The 'mistake theory', adapted from Dechow and Dichev (2002), argues that complexity causes managers to make more mistakes or errors in judgment. When accounting is complex, managers are more likely to err when applying standards to transactions, increasing the likelihood of misreporting due to mistakes. The 'manipulation theory' argues that managers take advantage of complex accounting to manipulate the financial statements. For example, prior research suggests that investors do not fully understand information found in pension footnotes, and that managers manage earnings through complex pension accounting (Picconi, 2004; Bergstresser et al., 2006). Therefore, the manipulation theory suggests complex accounting provides managers an opportunity to manage the financial statements more easily.

In the context of revenue, both theories suggest that revenue recognition complexity likely increases the propensity to misreport revenue. Assuming the probability of detection is similar across theories, I hypothesize that revenue recognition complexity increases the likelihood of revenue restatements. I then attempt to distinguish between the mistake theory and manipulation theory. The distinguishing feature between the two theories hinges on management's intent. I attempt to infer intent by testing certain attributes of the misreporting: (1) whether the misreporting caused the firm to meet a revenue benchmark, (2) whether the misreporting was an overstatement of revenue, and (3) whether the misreporting included multiple areas of the financial statements. To further test for intent, I examine the consequences of restating. Prior research provides evidence that misreporting costs are more severe if information related to the restatement calls into question the integrity of management (see Palmrose et al., 2004 and Hribar and Jenkins, 2004). Therefore, conditional on misreporting, the consequences of restatement should be more severe for intentional manipulation than for unintentional mistakes. An association between revenue recognition complexity and the consequences of misreporting provides an indication of intentional misreporting for complex firms. I examine three consequences associated with the restatement: (1) the likelihood of an SEC Accounting and Auditing Enforcement Release (AAER), (2) the restatement announcement returns, and (3) CEO turnover following the restatement.

I test my hypotheses on a sample of 348 revenue restatements from 1997-2005 identified by the Government Accountability Office (GAO) in their 2002 and 2006 reports to Congress on accounting restatements. In order to test whether revenue recognition complexity is an important determinant of restating revenue, I compare firms restating revenue to two sets of control, or comparison, firms: 1) firms that had a restatement during the sample period, but restate did not restate revenue (i.e., the firm restated expenses only; hereafter referred to as non-revenue restatements) and 2) a matched sample of firms that do not have a restatement of any kind during the sample period. I use both control samples because they offer complementary strengths and weaknesses in testing my hypotheses. I use non-revenue restatement firms for comparison principally because it provides an inherent control for determinants of restatement in general. I employ a matched sample design because it provides a more accurate estimate of the effect of complexity on misreporting relative to non-restating firms. Using these samples, I estimate a logistic regression model to test whether revenue recognition complexity increases the likelihood of a revenue restatement. All the logistic regression results provide evidence that firms with complex revenue recognition are more likely to restate revenue. Depending on the sample, a one standard deviation increase in revenue recognition complexity centered on the mean increases the probability of revenue misreporting by 8.8 to 21.7 percent. Relative to other determinants in the models, this marginal effect suggests complexity is one of the most important determinants of revenue restatements.

I then examine whether complex revenue recognition for firms that misreport revenue is associated with restatement attributes that suggest manipulation (missing benchmarks, overstatements, and restating multiple items). Results from these tests do not differentiate between the mistake and manipulation theories for complex revenue recognition firms. However, tests that examine the consequences of misreporting provide evidence consistent with the mistake theory. Regression results show that, given a revenue restatement, firms with more complex revenue recognition are less likely to receive an AAER and have less negative restatement announcement returns. However, the results show that revenue recognition complexity is not associated with CEO turnover, suggesting that boards may not distinguish between mistakes and manipulation in determining CEO departure when restatements occur. In sum, my results provide evidence that accounting complexity increases the probability of restatement in the case of revenue. While there is no evidence consistent with the manipulation theory, I do provide some evidence consistent with the mistake theory.

In additional analysis, I also perform a number of robustness checks. These include using alternative measures of revenue recognition complexity, testing for managerial discretion in disclosing revenue recognition procedures, and controlling for changes in the revenue recognition disclosure environment. These additional tests provide results that are generally consistent with those presented in the main analysis.

This study contributes to the literature in several ways. First, I present evidence that complex accounting increases the occurrence of misreporting, and most importantly, show that the magnitude of that effect is significant. Second, prior research (e.g. Bergstresser et al., 2006) suggests that complexity is associated with earnings management or manipulation; however, I find no evidence of an association between accounting complexity and manipulation in this setting of revenue recognition. Third, I provide a definition of accounting complexity and an associated empirical measure that can be applied in future research. These results should be informative to both the SEC and FASB as they attempt to reduce complexity in financial reporting, including revising revenue recognition standards in the near future.[2] This research should also be useful to investors, auditors, and firms to better understand the causes and consequences of revenue restatements.

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[2] Both the SEC and FASB have taken steps to address complexity. On June 27, 2007, the SEC announced the establishment of an advisory committee with a goal of reducing unnecessary complexity in financial reporting and making information more useful and understandable for investors. The FASB is readdressing specific accounting standards that are overly complex and has initiated an effort to develop an integrated codification of all existing accounting literature that would be available electronically. In February, 2007 the House of Representatives voted unanimously to require the SEC, PCAOB and FASB to report yearly on their efforts to reduce complexity in financial reporting (H.R. 755). The bill is currently in committee in the Senate.

A few recent studies also examine the role accounting standards and the application of those standards play in financial statement misreporting. Citing the need to understand the causes of restatements, Plumlee and Yohn (2008 WP) classify restatements as caused by either (1) a basic company error, (2) intentional manipulation, (3) transaction complexity or (4) some characteristic of the accounting standards. Using a sample of restatements from 2003 to 2006, their results suggest that 57 percent of restatements during this period are the result of basic internal company errors, while 37 percent were the result of some characteristic of the accounting standard. One drawback from using this classification approach is that the delineation of causes between company errors, transaction complexity, and accounting standards is likely a murky line that can be easily influenced by differences in firm restatement disclosures. Mergenthaler (2008 WP) examines whether the consequences of firms receiving SEC AAERs are affected when the misreporting is associated with a rules-based versus a principles-based standard. While Mergenthaler uses a standards-approach, I study the effect of accounting complexity on misreporting at the firm level. I believe this captures complexity more accurately because standards affect firms differently due to differences in contracts and transactions. Finally, compared to these two papers, my study attempts to look at the effect of complexity very broadly and attempts to measure it objectively.

The rest of the paper proceeds as follows. In the next chapter, I define accounting complexity and discuss the effect of complexity on misreporting. Chapter 3 discusses the empirical setting and develops my hypotheses. Chapter 4 discusses the sample, data, and empirical design. Results are presented in Chapter 5, with some additional analysis presented at the end. Chapter 6 concludes, including directions for future research.

Chapter 2. Accounting Complexity

2.1 Accounting Complexity Defined

To my knowledge, no formal definition of accounting complexity exists in the literature.[3] I define accounting complexity as the amount of uncertainty related to the mapping of transactions or potential transactions and standards into the financial statements.[4] This definition is intended to apply to both preparers and users of financial statements, and views accounting complexity as a scale or relation. I next discuss a few key points related to this definition to give some context.

Accounting is the confluence of transactions or potential transactions and standards. Preparers must take information about the firm's transactions and guidance from standards and map the two to determine the appropriate accounting. Users must also understand the mapping to interpret financial statements correctly. In many cases, this mapping is very straightforward, leading to a single, generally accepted and understood accounting choice. In other cases, there is uncertainty in the mapping, which can lead to potentially conflicting or erroneous accounting choices by preparers.

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[3] Prior research has examined firm or organization complexity (see Bushman et al. 2004), information complexity (see Plumlee, 2003), and information overload (see Schick et al., 1990 for a review), concepts not wholly unrelated to accounting complexity. I also recognize other definitions of accounting complexity likely exist; however, none has been explicitly stated.

[4] I believe this definition is in line with Congress' and the SEC's recent characterization of complexity, which encompasses both complexity as it relates to disclosure (which affects users) and standards (which affects users and preparers). For example, H.R. 755 passed by the House in February 2007, identifies 5 major areas that the SEC, PCAOB, and FASB need to address, encompassing both disclosure and standards issues. These are (1) reassessing complex and outdated accounting standards; (2) improving the understandability, consistency, and overall usability of the existing accounting and auditing literature; (3) developing principles-based accounting standards; (4) encouraging the use and acceptance of interactive data; and (5) promoting disclosures in 'plain English'.

Uncertainty also affects users since they must interpret how the mapping was performed based on limited disclosures. While some sources of uncertainty can be mutual for both preparers and users, uncertainty can also differ across preparers and users of financial statements. In some cases the mapping may be quite clear to preparers and auditors; however, uncertainty about the same transactions and standards can make accounting "appear" complex to users.[5] Increased transparency or disclosure can alleviate some uncertainty for users in terms of how the mapping *is* performed by preparers. However, increased transparency does not remove uncertainty regarding how the mapping *should be* performed by preparers.

Uncertainty in applying standards to transactions could come from many sources.[6] First, uncertainty could be the result of business environments that are not perfectly predictable. While accounting standards could require certainty of outcomes before recognition in the financial statements, most standards incorporate some aspect of this uncertainty, requiring managers to make estimates and judgments. Uncertainty could also result from flawed standards or deficient information about transactions. Unclear or ambiguous wording, inconsistencies across standards, or detailed rules-based standards can all cause uncertainty related to the standards.[7] Uncertainty could also stem from deficient information about transactions or contracts. Although the firm may have all

[5] While preparers and auditors are required to have some level of accounting expertise, there are no such requirements for investors. The FASB's SFAC 1 paragraph 36 acknowledges that users' understanding of financial information "may vary greatly", and that financial reporting should be accessible "to all—nonprofessionals as well as professionals—who are willing to learn to use it properly" (FASB, 1978).
[6] I provide a discussion on the potential sources of uncertainty to facilitate understanding, but my tests do not allow me to distinguish between the sources of uncertainty. However, understanding the precise causes of uncertainty may be important to regulators interested in eliminating avoidable sources of uncertainty.
[7] There is much discussion in the accounting literature on rules- v. principles-based accounting standards. While the intent of rules-based standards may be to remove uncertainty in the accounting, they can increase accounting complexity for users because of their inability to encompass all potential situations or their ability to obscure the original transaction's purpose beneath layers of rules.

available information related to contracts and transactions, the lack of systems to access that information could cause uncertainty to still persist for preparers. Uncertainty about contracts and transactions can increase for firms with numerous, customer-specific contracts or agreements documented by multiple contracts. Lengthy contracts and technical or legal wording in contracts may also cause uncertainty. In multi-division firms, uncertainty may increase because detailed information about contracts and transactions may be decentralized, while accounting expertise may be centralized.

Uncertainty is the result of, or is amplified by, limits to human cognitive function. Research shows that individuals have limits to cognitive processing, especially under uncertainty, which leads to simplification, heuristics, or biases (see Payne, 1976; Iselin, 1988; Bettman et al., 1990). As argued by Tversky and Kahneman (1974), this simplification can occasionally lead to errors in estimation or judgment. If uncertainty limits the efficient processing of information for preparers and/or users, this suggests that complexity can be costly to financial markets. I discuss one of these costs, misreporting, in the next section.

2.2 Accounting Complexity and Misreporting

I present two theories regarding the causes and consequences of accounting complexity on misreporting. One theory of accounting complexity suggests that complexity from the preparer's perspective causes unavoidable mistakes in financial reporting. This idea is adapted from Dechow and Dichev (2002), who write that "estimation accuracy [of accruals] depends on firm characteristics like complexity of transactions and predictability of the firm's environment." Although Dechow and Dichev focus on accruals, the idea applies more generally to all accounting and reporting.

Uncertainty in estimating accruals naturally leads to errors, which causes revision in future accruals and earnings. More generally, preparer uncertainty in mapping transactions and standards also leads to more errors and misreporting. However, unlike accruals, if the company makes errors when mapping transactions and standards (misinterpreting GAAP), the company must restate prior numbers. I term this the mistake theory of complexity.

Another theory of complexity suggests that managers opportunistically manage earnings when accounting is complex. In contrast to the mistake theory, which suggests that complexity affects the *preparer's* accuracy in financial reporting, the manipulation theory relies on complexity creating uncertainty for *investors* (and/or information intermediaries). For example, focusing on complex pension accounting, Picconi (2004) documents that investors and analysts do not understand the effect of changes in pension plan parameters on future earnings. He also shows that managers increase expected rates of return on pension assets to offset the effect of anticipated bad news in the future. Similarly, Bergstresser, Desai and Rauh (2006) show that managers increase rates of return assumptions on pension assets when the assumptions have a greater impact on earnings, or when managers are attempting to acquire other firms or exercise stock options. The findings on pensions suggest managers opportunistically alter financial reporting when accounting or reporting is complex. The manipulation theory argues that complexity increases uncertainty to outsiders, providing managers an opportunity to intentionally misreport more easily.[8]

[8] Although this theory suggests managers take advantage of complex accounting by managing the financial statements, complexity is not a necessary condition for manipulation. Many fraudulent practices are implemented using simple accounting settings (e.g., fictitious sales, bill-and-hold transactions, and capitalizing expenses).

Chapter 3. Setting and Hypotheses

3.1 Revenue Recognition Setting

I study the effect of accounting complexity on misreporting with respect to revenue recognition for three reasons. First, revenue recognition is a universal accounting issue; therefore, my findings will apply to a broad set of firms. Second, revenue misreporting is one of the most common types of restatement (see Palmrose et al., 2004; GAO, 2002 & 2006). This ensures that I can obtain a sufficiently large sample of revenue misreporting to test the effects of complexity on misreporting. Finally, anecdotal evidence suggests that revenue recognition can be complex for preparers and users of financial statements. I briefly discuss the evidence on the complexity of revenue recognition next.

Sondhi and Taub (2006) summarize the problems with revenue recognition when they write: "The lack of comprehensive guidance, in combination with the variety and complexity of revenue transactions, has resulted in a large number of financial reporting errors in the area of revenue recognition." Revenue recognition can be complex because of uncertainty about both standards and transactions. From 2001-2005, the FASB's advisory group named revenue recognition the top issue that should be addressed by the FASB (Schneider, 2005). The FASB states there are over 200 revenue recognition pronouncements by various standard setting bodies (Herz, 2007), and much of the authoritative guidance is industry- or transaction-specific. These issues can lead to inconsistencies across pronouncements or difficulties in applying multiple standards to a contract. In addition, complicated revenue transactions and contracts can increase uncertainty. Customer contracts can be lengthy, filled with legal wording, and include multiple clauses for customer acceptance, return policies, and payment terms. Companies with many customer-specific contracts can increase uncertainty and side agreements, whether written or oral, can also alter provisions in contracts leading to increased complexity (see Turner, 2001).

3.2 Research Questions and Hypothesis Development

Both the mistake and manipulation theory of complexity suggest that revenue recognition complexity increases the likelihood of misreporting revenue. Assuming that the probability of detecting the misreporting is similar across both theories, this leads me to the following hypothesis, stated in alternate form:

> *H1: Managers of firms with more complex revenue recognition are more likely to misreport revenue than managers of firms with less complex revenue recognition.*

Even though both the mistake and manipulation theories lead to the prediction in H1, the null hypothesis of no result could obtain if the effect of complexity on revenue misreporting were small or if misreporting is solely driven by managerial incentives and governance, as hypothesized in prior literature (see Zhang, 2006; Callen et al., 2005). Since I control for the incentives and governance related to misreporting using two approaches, these effects should not be reflected in the coefficient on complexity. In addition, testing H1 allows me to quantify the economic significance of the effect of complexity on the likelihood of misstating revenue.

I next attempt to distinguish between the two theories of misreporting. The research question I investigate is: Is misreporting revenue in a complex revenue recognition environment the result of intentional manipulation or unavoidable mistakes? I do not provide specific hypotheses regarding this research question, but I develop tests to distinguish between the two competing theories. The distinguishing feature between manipulation and mistakes is managerial intent. Although inferring intent is difficult in an empirical setting, I conduct tests on both the *attributes* and *consequences* of misreporting to infer which theory best explains revenue misreporting. I discuss these tests in the next section after a brief discussion of the sample.

Chapter 4. Sample Selection and Empirical Design

4.1 Data and Sample Selection

To examine the effect of complexity on misreporting, I use a sample of revenue restatement firms collected by the GAO for their reports to Congress in 2002 and 2006.[9] Included in the GAO database is the date of the restatement announcement, type of restatement, including whether the firm restated revenue, and who identified the misreporting (or source). The GAO study excludes certain types of restatements that are not due to "irregularities," including restatements from mergers and acquisitions, discontinued operations, and stock splits, among others. In the combined reports the GAO identified 738 firms that restated their revenue, covering the years 1997 to 2005. I exclude financial firms (SIC 6000-6999) as their revenue recognition is substantially different from other firms due to regulatory requirements. Firms may have multiple restatements over the sample period. I only include the first restatement for firms that restated more than once within a one year period.[10] I recategorize 18 revenue restatements identified by the GAO because they are categorized incorrectly. For example, the GAO categorizes restatements relating to non-operating gains on sale and other non-operating income (such as interest income) as revenue restatements. I also

[9] Restatement data from the GAO reports can be found at http://www.gao.gov/new.items/d03395r.pdf (2002 report) and http://www.gao.gov/special.pubs/gao-06-1079sp/toc.html (2006 report). Judson Caskey provides the data in one MS-Excel file here: http://personal.anderson.ucla.edu/judson.caskey/data.html.

[10] The GAO sample may have firms with multiple restatements within a one year period for two reasons. First, although extremely rare, the firm may have separately identified multiple misreporting violations during that one-year period. More commonly, the firm has multiple restatements because the GAO incorrectly included separate restatement announcements that are just updates of previously announced restatements.

exclude all revenue restatements in connection with SAB 101 or any EITF related to revenue issued during the sample period, as I consider these restatements as mandatory restatements caused by a change in accounting standard.[11] Missing variables from 10-K disclosures and *Compustat* and *CRSP* databases reduces the revenue restatement sample to 348 observations. Table 1 describes the attrition of the revenue restatement sample and the comparison samples described below.

4.1.1 Control Firms for H1

I test whether revenue recognition complexity increases the probability of misreporting revenue (H1) using two different small sample comparison groups.[12] This joint testing approach improves confidence in the combined results of the tests because of the unique strengths and weaknesses of each comparison sample.

For the first comparison sample, I compare the revenue recognition complexity of firms that restated revenue to firms that also had a restatement during the sample period, but restated something other than revenue. This design is advantageous because it inherently controls for incentives, governance effects, and other determinants of restatements, which are difficult to control for because they are hard to measure (e.g., governance and incentives). The main disadvantage of using non-revenue restatement firms for comparison is that results may not accurately estimate the full effect of complexity, incentives or governance on misreporting relative to non-restating firms. This comparison sample is also obtained from the GAO reports, with 1,567 non-revenue

[11] During the sample period, the Emerging Issues Task Force issued EITFs 99-19, 00-10, 00-14, 00-22, 00-25 to clarify revenue recognition issues such as recognizing gross v. net, shipping and handling costs, sales incentives, and other consideration from a vendor to a reseller.
[12] Although the ideal design would be to compare revenue restating firms to a broad cross section of firms, this approach is prohibitive because revenue recognition complexity requires some hand collection.

restatements obtained from the reports. As with the revenue restatement sample, I exclude financial firms and firms with more than one restatement in a one-year period. I also exclude any restatements for firms that have a revenue restatement over the sample period to ensure that a single firm cannot be in both samples. The final comparison sample is 840 restatements.

In addition to the non-revenue restatement firms, I also compare the revenue restatement firms to a matched control sample of firms that did not have a restatement over the sample period. A matched sample approach is advantageous because it allows me to estimate the full effect (i.e., magnitude) of complexity on misreporting. Of course, this depends on measuring all the control variables accurately and imposes additional data limitations on the sample. In addition, the matched sample design is not straightforward to implement because it is not clear how the match should be performed. For example, matching on industry introduces a noisy sort on revenue recognition complexity, potentially controlling for the effect being tested. I choose to match on fiscal year, assets, and the book-to-market ratio. Because my sample of revenue restatement firms are generally smaller firms, matching on assets and book-to-market ensures the firms are similar size and have similar growth prospects. I first identify all firms without any restatement during the sample period that have data coverage on *Compustat* and *Execucomp*. Firms with assets between 70% and 130% of the assets of the sample firm in the same fiscal year are chosen as potential matches. From this set of firms, I choose the matched firm with the book-to-market ratio closest to that of the sample firm. This process yields 338 matched sample firms. Missing financial and stock return data requirements reduces the matched sample to 316 firms. Finally, the research design using the matched sample ideally includes a measure of compensation incentives. The specifics of this measure are discussed in more detail in Section 4.3.2. Due to data limitations, including this measure in the model reduces the sample size, resulting in only 102 revenue restatements and 102 matched firms with necessary data.

All financial data is obtained from *Compustat*. I obtain stock returns from *CRSP* and analyst forecasts from I/B/E/S. Option compensation data is obtained from *Execucomp*. CEO turnover is also obtained from *Execucomp* where available and hand collected from the proxy filings where not available.

Table 2 displays the frequency of restatements by year for the type of restatement (Panel A), industry (Panel B), and source of the restatement (Panel C).[13] Panel A shows that revenue restatement firms are 29 percent of the total restatements in the sample and that 2000 and 2003 had especially high proportions of revenue restatements. Panel B displays the number of revenue/non-revenue restatements by year in each industry. Although not tabulated, the combined industry breakdown shown in Panel B is similar to the composition of all firms in Merged *CRSP*/*Compustat* database over the sample period, except my sample is overweighted in Wholesale/Retail and Technology and underweighted in Other.[14] Panel C presents information about who identified the misreporting (or source) broken out by the type of restatement. It is interesting to note that more than half of the restatements are initiated by the company for both revenue and non-revenue restatement firms.

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[13] Consistent with Palmrose et al. (2004), industries are defined by the following SIC codes: Mining & construction=0-1999, manufacturing=2000-3999 (except codes assigned to technology), technology=3570-3579 plus 7370-7379, transportation=4000-4799, communications=4800-4899, utilities=4900-4999, wholesale/retail=5000-5999, services=7000-8999 (except codes assigned to technology), and other=9000-9999.

[14] The overweighting in Wholesale/Retail is mostly explained by the large amount of lease-related restatements in 2005 for Wholesale/Retail firms.

4.2 Measuring Revenue Recognition Complexity

Because my definition of accounting complexity is built on the concept of uncertainty, this suggests that using a firm-level proxy of uncertainty might be appropriate for empirical testing. However, many of the well-documented empirical measures of uncertainty such as bid-ask spread, standard deviation of returns, and dispersion of analyst forecasts capture uncertainty relative to the whole firm and markets it engages in. Using a measure that captures this much uncertainty would almost assuredly drown out any uncertainty related to the mapping of revenue transactions and standards. However, a firm-level proxy is more appropriate than a standards-level proxy because standards apply to firms differently due to differences in transactions. I conjecture that a description of the revenue recognition practices captures uncertainty about recognizing revenue. To measure the complexity of revenue recognition at the firm level, I examine the firm's revenue recognition disclosures found in the summary of significant accounting policies contained in the notes to the financial statements.[15] I collect revenue recognition disclosures contained in the firm's most recent 10-K prior to the restatement announcement using the Edgar Company Search on the SEC website.[16] I use the Python programming language to obtain the revenue recognition disclosures where possible, personally checking for accuracy, and hand collecting the disclosures where Python fails. I measure revenue recognition complexity using a factor score (*RRC SCORE*) based on the number of words (*WORDS*) and a proxy for the number of

[15] Prior to SAB 101, firms had a choice to disclose their revenue recognition policy depending on whether they thought it was a significant policy; however, SAB 101, which became effective in 2001, required firms to disclose their revenue recognition policies in the notes to the financial statements. I discuss the effect of this change in disclosure requirements on my results in additional analysis in Chapter 5.
[16] Some restatements relate to quarterly filings only; however, revenue recognition disclosures are not found in 10-Q filings, so I also use the most recent 10-K filings for these firms.

methods (*METHODS*) obtained from the revenue recognition disclosure.[17,18] I use a factor score mainly for presentation purposes, but also to reduce noise relative to using each measure separately.

I believe *RRC SCORE* is a sufficient measure of revenue recognition complexity. Relative to simple disclosures, longer disclosures and more methods capture the preparer's need to incorporate a diverse set of transactions and standards and reflect the manager's need to explain more involved practices or methods.[19] These characteristics are evidence of increased uncertainty. To illustrate this, Appendix 1 includes a few sample revenue recognition disclosures. For example, A.C. Moore Arts & Crafts recognizes revenue at the point of retail sale, which is likely an automated process with no uncertainty, suggesting low complexity. The number of words in their revenue recognition disclosure is 8 and the number of methods is 1. On the other hand, ARI Networks recognizes revenue for maintenance fees, services, subscriptions, and software. The fees may not be fixed and the customer acceptance terms can differ across contracts. For ARI, the number of words is 158 and the number of methods is 7. Thus, relative to A.C Moore, ARI Networks' revenue recognition is more complex and the *RRC SCORE* will capture that increased complexity.

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[17] I measure the number of methods (*METHODS*) the firm employs by counting the number of occurrences of the words "recogn" and "record" found in the disclosure. Counting the occurrences of "recogn" and "record" overestimates the actual number of revenue recognition methods the firm employs. To alleviate concerns of bias in this measure, I physically read the recognition disclosures and counted the number of methods for a sub-sample of firms. The correlation between the two measures is .77, suggesting my proxy for the number of methods is sufficient.

[18] I use the principal components method of factor analysis, although results are very similar when I use the common factor method. Only one retained factor is available when using two individual variables and the eigenvalue of my retained factor is 1.7.

[19] As with any measure based on disclosures, managers have discretion as to how much to disclose. I conduct additional tests in Chapter 5 to determine if managers of revenue restating firms are manipulating disclosures to appear more or less complex.

Table 3 presents summary statistics on revenue recognition disclosures for my samples. Panel A provides revenue recognition disclosure summary statistics for the revenue restatement sample and both comparison samples. *RRC SCORE* is calculated for each combined sample and produces a score that is mean zero. The t-tests reveal that revenue restatement firms have higher *WORDS* (diff. of 81.1 and 93.3, t-stats of 5.26 and 5.70) and *METHODS* (diff. of 1.85 and 1.76, t-stats of 6.82 and 5.55) than both sets of comparison firms, resulting in higher *RRC SCORES* also (diff. of 0.63 and 0.61, t-stats of 7.92 and 5.99). These univariate results are consistent with H1. Panel B shows the mean and median *WORDS* and *METHODS* for the revenue and non-revenue restatement firms and matched sample firms by industry. These results reveal that revenue recognition disclosures vary by industry. As might be expected, technology, services, and communications firms appear to have longer disclosures than the other industries. The data in Panel B also shows the increased revenue recognition complexity for revenue restaters documented in Panel A does not apply equally to all industries. For example, revenue restatement technology firms have lower mean *WORDS* than non-revenue restatement technology firms, but a higher mean than the matched sample technology firms.

4.3 Empirical Design to test H1

I use two similar research designs (with slightly different control variables) to test H1 using the restatement and matched comparison samples. Both the restatement design and matched-sample design are based on the following generic model and estimated using a logistic regression:

$$P(\text{Revenue Restate}) = f(\alpha + \beta\,\text{RRC SCORE} + \sum \gamma\,\text{Controls}) \qquad (1)$$

The details of each model are discussed next, including control variables. Detailed variable definitions can also be found in Appendix 2.

4.3.1 Restatement Design

Using the generic model in equation (1), the dependent variable in the comparison of revenue and non-revenue restatement firms is one if the firm restated revenue and zero if the firm restated something other than revenue. Therefore, control variables need only measure any incremental determinants for why managers might misreport revenue. I organize my discussion of control variables into three categories based on prior research: (1) value relevance (2) governance and (3) other. I discuss them below.

Value-relevance of revenue

The two principal studies on revenue restatements, Zhang (2006) and Callen et al. (2005), have shown value relevance to be an important determinant of firms restating revenue. I use firm characteristics based on this prior research that suggest revenue has high value relevance and/or earnings has low value relevance. Ertimur et al. (2003) find the market reaction to revenue surprises to be greater for growth firms than value firms. Also, Ertimur and Stubben (2005) show that analysts are more likely to issue revenue forecasts for firms with higher growth prospects. Therefore, the existence of revenue forecasts should also increase the value relevance of revenue, since it provides the market a benchmark to evaluate revenue.[20]

Revenue may also be more important for valuation when net income is less value relevant. Hayn (1995), Collins et al. (1999) and others have shown that the returns-

[20] Zhang (2006) includes 3 other value relevance variables that I do not include in the paper to make the model more parsimonious. These include variables that measure the operating and gross margin of the firm and the R&D expense. Including these variables in the model does not change the results presented in the paper and are not significant in any of the regressions.

earnings relationship is weaker for loss firms than profit-making firms. Since loss firms have low value relevance of earnings, Callen et al. (2005) argue the market will substitute revenue for earnings in valuation. Zhang (2006) also argues that high earnings volatility is also likely to make earnings less value relevant, potentially increasing the value relevance of revenue.

In summary, growth prospects, analyst revenue forecasts, losses, and high earnings volatility all increase the value-relevance of revenue and the probability of revenue misreporting. Therefore, I include proxies in my model to control for these constructs. I use the book-to-market ratio of the firm at the fiscal year end just prior to the restatement (*BTM*) as a proxy for growth and an indicator equal to one if the firm has an analyst revenue forecast any time prior to the restatement announcement and zero otherwise (*SALEFCST*). Finally, I include the proportion of loss years to total years the firm has earnings data on *Compustat* (*LOSSPER*), and the 5-year average earnings volatility (*EARNVOL*) of the firm prior to the restatement announcement.[21]

Governance

Prior research provides some evidence on the effect of auditing and governance on the occurrence of misreporting the financial statements in general (see Defond and Jiambalvo, 1991 and Palmrose et al., 2004), but provides little insight to whether managers will specifically misreport revenue. It is more likely that the previously mentioned variables on the value-relevance of revenue already capture an increasing monitoring effect on revenue reporting by auditors. In addition, Kinney and McDaniel

[21] Zhang (2006) measures these variables relative to the initial period of misreporting. I measure the variables relative to the restatement announcement because I only have data on the misreporting period for a sub-sample of restatements. However, results are consistent with those presented in the paper when I measure these variables using Zhang's approach on the sub-sample.

(1989) find that firms correcting previously reported quarterly earnings are more likely to have negative stock returns leading up to the correction.   They argue that poor recent performance causes auditors to scrutinize financial statements and accounting choices. I include the stock returns for the 12 months prior to the restatement announcement (*PRERETURN*) to control for this effect. Modifying this same idea specifically for revenue restatements, recent sales declines may cause auditors to reexamine the revenue recognition of prior sales, increasing the likelihood of restatement. I control for deteriorating sales by using the average change in sales for the two years prior to the restatement (*CHSALES*). I control for other potential monitoring effects by including the logged market value of equity of the firm at the fiscal year end just prior to restatement (*LOGMVE*), an indicator equal to one if the firm is audited by a large accounting firm (*BIGN*), and an indicator equal to one if the restatement is attributed to the auditor (*AUDITOR*). However, I make no predictions regarding these effects.

Other determinants

Zhang (2006) also argues that large accounts receivable accruals allow managers more flexibility in managing revenue. Manipulating revenue when A/R accruals are already large decreases the likelihood of detection compared to small A/R accruals. I include the firm's 5-year average A/R Accrual prior to the restatement to control for high A/R accruals (*AR ACCRUAL*). Zhang makes a similar argument for unearned revenue accruals, but since data on unearned revenue accruals is only extensively available starting in 2002, I do not include unearned revenue accruals in the formal analysis.[22]

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[22] As a robustness check, I include in the test an indicator equal to one if the firm has unearned A/R accruals as of 2002 and zero otherwise. The results remain consistent with those presented in the main analysis.

Finally, I include industry and year indicators to control for industry and year effects that may affect the probability of restatements.

4.3.2 Matched Sample Design

The dependent variable using the matched sample design is one if the firm restated revenue and zero if the firm did not have a restatement. In contrast with the previous model, control variables in this model should also capture any additional determinants for why managers might misreport the financial statements. Prior literature identifies a number of determinants for why managers might misreport. Burns and Kedia (2006) provide a summary of these determinants in their study that examines the effect of CEO compensation on misreporting. Using a sample of S&P 500 firms, Burns and Kedia (2006) find evidence that the sensitivity of CEO option portfolios to stock price is higher for restatement firms than non-restatement firms, suggesting that compensation incentives matter to misreporting.

I begin by including the same control variables used in the restatement research design as described in 4.3.1. I include additional control variables from Burns and Kedia (2006) to capture incentives related to growth, external financing, violating debt covenants, and managerial equity incentives. Specifically, I include the earnings-to-price ratio (*EP*) as another proxy for growth, cash raised from issuing equity or debt (*DEBT ISSUE* and *EQUITY ISSUE*), and leverage as a proxy for closeness to violating debt covenants (*LEVERAGE*). I also include a measure of operating accruals (*OP ACC*), to accommodate the findings of prior research that misreporting firms have higher accruals (Dechow et al., 1996 and Richardson et al., 2003). Finally, I include a measure of CEO equity incentives using the pay-for-performance sensitivity of CEO stock options (*LOG*

PPS) as described in Burns and Kedia (2006). This variable measures the change in the value of stock options held for a percentage change in the value of the firm. For revenue restatement firms, these variables are all measured as of the fiscal year just prior to the restatement announcement. For matched firms, the variables are measured in the year in which the matched firm was matched on. Again, details about variable definitions can be found in Appendix 2.

4.4 Attributes of Misreporting Tests

I examine three attributes of the revenue misreporting itself to provide some evidence on the intent of managers. For each misreporting attribute indicator described below, I perform univariate logistic regressions to test whether revenue recognition complexity (*RRC SCORE*) is associated with the particular attribute.

The first attribute I examine is meeting revenue benchmarks, including analyst forecasts and prior period revenue. Meeting revenue benchmarks can be beneficial to the firm and provides incentives to manage revenue (see Ertimur et al., 2003; Rees and Sivaramakrishnan, 2007; Stubben, 2006). If the restatement caused the firm to miss a benchmark that the firm previously beat, this suggests the manager chose the recognition of revenue to manipulate revenue to beat the benchmark. Assuming the first period of misreporting was when the decision was made to recognize revenue in a particular way and the company maintained that policy in subsequent periods, the first period of misreporting is the period of interest. For each restatement I set *MISS GROWTH* to one if the firm had previously recorded positive sales growth in the first period of misreporting and the restatement caused the firm to have zero or negative sales growth for that period, and zero otherwise.[23] Similarly, for each restatement I set *MISS FCST* to one if the first period of the misreporting had an analyst revenue forecast and (1) the firm had previously beat the mean analyst revenue forecast for that period, and (2) the restatement caused the firm to miss the forecast for that period, and zero otherwise.

Second, if managers of firms with complex revenue recognition are manipulating revenue, there should be a greater likelihood that they overstate revenue compared to firms with less complex revenue recognition. In the extreme, one might suggest that evidence of mistakes should only exist if there are an equal number of over- and understatements. However, the probability of detection is not equal for over- and understatements because auditors and boards are more concerned with overstatements. In addition, this test attempts to determine if complex revenue recognition is more associated with unintentional mistakes, not solely driven by unintentional mistakes. I set an indicator equal to one if the sum of the restated revenue over the restating periods is less than the sum of the originally reported revenue over the restating period, and zero otherwise (*OVERSTATEMENT*).

Finally, the pervasiveness of the restatement may also give an indication of intent. If the company restates another area of the financial statements in addition to revenue, it suggests the misreporting is widespread and more likely intentional. In support of this, Palmrose et al. (2004) find that restatements involving multiple areas of the financial statements have more negative announcement returns, controlling for the magnitude of

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[23] Since companies can restate annual results and/or quarterly results, I determine sales growth differently for annual and quarterly periods. For restated annual results, sales growth is calculated as the annual difference in sales. For restated quarterly results, sales growth is calculated as the lagged 4-quarter difference in sales.

the restatement on net income. I set an indicator equal to one if the firm restated multiple areas of the financial statements, and zero otherwise (*MULTIPLE*).

4.5 Consequences of Misreporting Tests

In addition to the attributes of misreporting, I examine the negative consequences associated with the misreporting to partly infer whether stakeholders perceive there to be managerial intent associated with complexity. As Hribar and Jenkins (2004) argue, firms that announce restatements experience an increase in cost of capital partially because of uncertainty regarding managerial integrity. More generally, if managerial intent is important to stakeholders when they observe misreporting, then the theories suggest the consequences of misreporting will be more severe if managers are taking advantage of complexity and less severe if the complexity just results in more errors. I examine three reactions to misreporting that provide evidence of intent: SEC AAERs, restatement announcement returns, and CEO turnover following the restatement.[24]

First, I examine whether an SEC Enforcement Action (AAER) accompanies the revenue restatement.[25] While not all AAERs are accusations of fraud, the issuance of an AAER represents a greater likelihood of intentional actions.[26] For example, Karpoff et al. (2007), find that 622 of the 788 enforcement actions (79 percent) in their sample from 1978-2006 include charges of fraud. In addition, a 2007 Deloitte study finds that of

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[24] Using a different approach, Hennes, Leone, and Miller (2007) classify a restatement as intentional if the restatement disclosure discusses an irregularity, a board-initiated independent investigation, or an external regulatory inquiry. However, similar to my approach, they examine whether their classification is valid by examining the classification's association to announcement returns and class action lawsuits.

[25] I use the term 'accompanies' because the timing of restatements and AAERs can vary across firms. SEC investigations into misreporting, whether formal or informal, typically closely accompany restatement announcements. However, the complete resolution of restatements and AAERs can take years.

[26] Erickson, Hanlon and Maydew (2006) and Feroz et al. (1991) correctly argue that the SEC can issue administrative actions that do not imply charges of fraud or gross negligence. Generally, these administrative actions end with a settlement and an AAER, where the firm admits to no wrong-doing but agrees to avoid future securities violations.

revenue recognition AAERs, roughly half of the AAERs are issued for recording fictitious revenue, revenue swaps or round-tripping, or "bill and hold" transactions, which are all intentional manipulations (Deloitte Forensic Center, 2007). I obtain data on firms subject to SEC AAERs from the sample of AAERs that Dechow et al. (2007) use in their study. Using AAERs, Dechow et al. (2007) identify financial statement variables that predict financial manipulations and develop a Fraud Score based on their prediction model. Their sample contains AAERs from 1982 through 2004 and identifies if the AAER relates to revenue or receivables issues. Since my sample of restatements runs through 2005, I determine whether later restatements resulted in AAERs by searching the listings of AAERs on the SEC website through August 2007.

I use estimates from a logistic regression to test whether revenue recognition complexity affects the likelihood of receiving an AAER for revenue restatement firms. The dependent variable is one if the firm has an AAER associated with revenue or receivables within three years of the restatement announcement and zero otherwise. Equation (2) presents the model with a discussion of the variables following.

$$\begin{aligned} P(\text{AAER} \mid \text{Rev Restate}) = f\,(&\alpha + \gamma_1 RRC\,SCORE + \gamma_2 MULTIPLE + \gamma_3 AUDITOR \\ &+ \gamma_4 MISS\,FCST + \gamma_5 RESTLEN + \gamma_6 CHREV + \gamma_7 CHNI \\ &+ \gamma_8 LOGMVE + \gamma_9 BIGN + \gamma_{10-18} INDUSTRY\,) \end{aligned} \quad (2)$$

Generally, studies on AAERs (Dechow et al., 1996; Beniesh, 1999; Dechow et al., 2007) have compared AAER firms to either a large sample of public firms or to small matched-samples. These studies examine firm characteristics like governance, incentives, and financial statement characteristics to predict AAERs. In contrast, this test focuses on the likelihood of an AAER for a specific type of misreporting event; therefore, restatement characteristics are likely more important in determining if an AAER will be issued. I conjecture that the SEC is more likely to issue an AAER if managers had intent to manipulate revenue, if the misstatements are large, and if the SEC gets greater exposure from issuing the AAER. I include three variables to identify intent: (1) whether the firm restated more than just revenue (*MULTIPLE*); (2) whether the restatement is credited to the firm's auditor (*AUDITOR*); and (3) a dummy equal to one if the restatement caused the firm to miss the sales forecast for the first period of the restatement and zero otherwise (*MISS FCST*).[27] I include three measures of the magnitude of the restatement: (1) the number of periods the company is restating in quarters (*RESTLEN*); (2) the percentage change in revenue over all periods of the misreporting due to the restatement (*CHREV*); and (3) the percentage change in net income over all periods of the misreporting due to the restatement (*CHNI*).[28] Finally, the SEC may target large firms and firms audited by large accounting firms because it benefits from enforcement of those firms relative to smaller firms. To control for these effects, I include in the model both the log of the market value of equity for the fiscal year end prior to the restatement (*LOGMVE*) and whether the firm was audited by a large accounting firm (*BIGN*).

Recognizing that AAERs probably do not constitute a complete set of intentional misreporting violations, I examine two other events associated with the restatement,

[27] Although the purpose of the test is to infer intent using the association between revenue recognition complexity and AAERs, I control for other obvious indications of intent since the SEC likely uses this other information in conjunction with complexity to determine if the company was intentionally misreporting revenue.

[28] The last two measures of magnitude, *CHREV* and *CHNI* will not capture restatements that are solely timing issues, since there is no "change" in revenue or net income. However, few restatements can be categorized as solely revenue recognition timing problems because at the time of restatement the timing has not been fully resolved.

announcement returns and CEO turnover. If investors are sensitive to management integrity and are capable (through disclosures or inference) of identifying managerial intent when a restatement is announced, then examining the effect of complexity on returns provides the market's indication of intent. If complex revenue recognition firms have lower (higher) market returns it would suggest the market interprets revenue restatements for complex firms as more (less) intentional.

I test whether the market reaction to revenue restatement announcements differs based on revenue recognition complexity using OLS regression estimates of 5-day cumulative abnormal returns (*CAR*) centered on the announcement date regressed on complexity variables and other control variables. I measure the cumulative abnormal return using market adjusted returns, where the daily market returns are subtracted from the firm's raw returns and compounded over the period. The model is presented in equation (3), followed by a discussion of control variables.

$$\begin{aligned}\mathrm{CAR} = {} & \alpha + \gamma_1 RRC\,SCORE + \gamma_2 MULTIPLE + \gamma_3 AUDITOR + \gamma_4 AAER \\ & + \gamma_5 CHREV + \gamma_6 CHNI + \gamma_7 LOGMVE + \gamma_8 PRERETURN + \gamma_9 BTM \\ & + \gamma_{10-18} INDUSTRY + \varepsilon\end{aligned} \tag{3}$$

Palmrose et al. (2004) identify a number of restatement and firm characteristics that affect restatement announcement returns. They show that restatement announcement returns are negatively associated with restatements involving fraud, affecting multiple accounts, decreasing net income, and attributed to auditors or management. I control for these findings using *MULTIPLE* and *AUDITOR* as previously defined. In addition, I include an indicator for whether the restatement is eventually associated with an AAER

(*AAER*) to identify fraud. Since AAERs are not usually announced concurrently with accounting restatements, I include this variable to account for information the market infers or receives about fraud or SEC investigations from the announcement. I control for the magnitude of the restatement by including both *CHREV* and *CHNI* as previously defined. The model includes *LOGMVE* to control for size since adverse news is likely to be magnified for small firms, which typically have weak information environments (Collins et al., 1987 and Freeman, 1987). Since announcement returns are partially due to investors' revisions of future growth expectations, the returns are likely related to the book-to-market ratio of the firm just prior to the announcement (*BTM*) and recent stock performance (*PRERETURN*) as previously defined.

Finally, I examine evidence from CEO turnovers to infer intent. If corporate boards are more likely to dismiss CEOs that manipulate revenue, then an association between revenue recognition complexity and CEO turnover provides an indication of intent. However, it is possible that boards do not distinguish between manipulation, an indication of CEO integrity, and mistakes, an indication of CEO competence, when making turnover decisions. Therefore, I expect this test to be less powerful than the other consequences tests. Consistent with Desai, Hogan, and Wilkins (DHW, 2006), I test a logistic regression model where the dependent variable is one if the CEO resigned or was dismissed from the firm within two years following the restatement announcement and zero otherwise. The model, presented in Equation (4), includes a number of control variables that I discuss below.

$$P(\text{CEOTurn}|\text{Rev Restate}) = f(\alpha + \gamma_1 RRC\,SCORE + \gamma_2 AAER + \gamma_3 MULTIPLE \\ + \gamma_4 LOGMVE + \gamma_5 CHREV + \gamma_6 CHNI + \gamma_7 PRERETURN \\ + \gamma_8 POSTRETURN + \gamma_9 ROA + \gamma_{10} CAR + \gamma_{11-19} INDUSTRY) \quad (4)$$

Desai, Hogan, and Wilkins (2006) identify a number of variables that are associated with CEO turnover following restatements, many of which I include as control variables in my model. I include *MULTIPLE* and *AAER* as previously defined as partial controls for managerial culpability. I control for firm size by including *LOGMVE* as previously defined. I also include both *CHREV* and *CHNI* to capture the magnitude of the restatement. Prior stock return and operating performance are directly linked with CEO turnover decisions (see Warner et al., 1988 and Engel et al., 2003). Following DHW, I include both the stock returns for the year prior to (*PRERETURN*) and the year following (*POSTRETURN*) the restatement announcement to control for market-based performance. Consistent with DHW, I also include the return-on-equity (*ROA*) for the fiscal year prior to the restatement announcement to control for operating-based performance. Finally, I include the restatement announcement return (*CAR*) to capture the market's assessment of the restatement. DHW also provide some evidence that CEO age, tenure, stock ownership and occupying the Chairman position all contribute to the turnover. However, since over half of my sample firms are not covered by *Execucomp*, I exclude these variables from the model. I discuss the effect of this research design choice on my results in the next section.

Chapter 5. Results and Robustness Checks

5.1 Univariate Tests and Sample Correlations

Table 4 contains summary statistics for revenue restatement firms and both sets of comparison firms, with t-tests for the difference in means. The results for *RRC SCORE* are identical to those presented in Table 3 and were discussed in Section 4.2. The differences in means show that revenue restatement firms have lower book-to-market (*BTM*) than non-revenue restatement firms but are not different than the matched sample firms. This is expected since *BTM* was one of the match variables. Revenue restatement firms have incurred more losses (*LOSSPER*), have lower returns in the year leading up to a restatement announcement (*PRERETURN*), and have larger A/R accruals (*AR ACCRUAL*) prior to the restatement announcement compared to both sets of comparison firms. Revenue restatement firms have higher *CHSALES* compared to matched firms (0.21 vs. 0.09, t-stat 3.52), but there is no statistical difference in *CHSALES* relative to non-revenue restatement firms. Matched sample firms are more likely to have a sales forecast (*SALEFCST)* and be audited by a large accounting firm (*BIGN*) compared to revenue restatement firms; however, non-revenue restatement firms are less likely to have a sales forecast and have the same proportion of firms audited by large accounting firms. This is likely the result of matched sample firms being more established than non-revenue restatement firms because they are required to have data on *Execucomp*. Finally, in Panel B, it appears that revenue restatement firms are more likely to have issued debt or equity in the year prior to the restatement announcement compared to matched-sample firms.

Table 5 presents sample correlations for both research designs for selected variables listed in Table 4. As expected, *RRC SCORE* is positively correlated with the variable *REVENUE*, the dependent variable for both models, which is one if the firm restated revenue and zero otherwise. Revenue recognition complexity is also positively correlated with *LOSSPER* and *SALEFCST*, suggesting that firms with complex revenue recognition also have revenue that is more value relevant.

5.2 Results of Tests of H1

5.2.1 Results of Restatement Research Design

Table 6 presents results from the logistic estimation of the restatement research design. I calculate Z-statistics for marginal effects using robust standard errors with firm-level clustering to account for multiple observations for the same firm (93 cases).[29] The first observation from Table 6 is that *RRC SCORE* has a positive, statistically significant coefficient (.347, z-stat 4.71) indicating that revenue recognition complexity increases the likelihood that a firm will restate revenue relative to other restatement firms. This provides support for H1. In addition, the results indicate that firms with lower *BTM* and firms with an analyst sales forecast (*SALESFCST*) are more likely to restate revenue. Multivariate results also suggest that revenue restatement firms are experiencing a decline in sales (*CHSALES*) and have poor stock return performance (*PRERETURN*) prior to the restatement announcement, suggesting performance plays a monitoring role. Revenue restatement firms are less likely to be audited by a large accounting firm, which

[29] The z-statistics presented are for marginal effects only, but the z-statistics are almost identical for the coefficients in the model.

may suggest a lack of oversight of revenue restating firms' auditors (i.e., the non-Big N auditors are less likely to uncover revenue restatement problems). Finally, the coefficient of *AR ACCRUAL* is positive and significant (4.739, z-stat 3.05), indicating that revenue restating firms also have much larger A/R accruals prior to the restatement relative to other restatement firms.

I examine the economic significance of complexity relative to other determinants of revenue restatements by computing marginal effects. To facilitate comparing marginal effects across variables, marginal effects are calculated for each continuous variable as the change in the predicted probability as the variable moves one standard deviation centered at the mean, holding all other variables constant at their mean values. Marginal effects for indicator variables are similarly calculated, but with the change in the predicted probability calculated as the indicator moves from zero to one. A one standard deviation change in *RRC SCORE* (from -0.65 to 0.65) increases the probability of revenue restatement by 8.6 percent, which, in absolute terms, is greater than the marginal effect of any other continuous variable, and equal to or greater than all indicator variables. However, in untabulated results, the marginal effect of *RRC SCORE* is not statistically different from the marginal effects of *SALEFCST*, *PRERETURN*, *BIGN*, or *AR ACCRUAL*. Thus, while other determinants of misreporting are also important, revenue recognition complexity provides a significant effect on determining which firms will misreport revenue.

5.2.2 Results of Matched-Sample Research Design

Table 7 presents results from the logistic estimation of the matched-sample research design. Z-statistics and marginal effects are calculated the same as in Table 6. Three specifications are presented. First, I estimate the model using only the relevant variables from the restatement model to allow for comparison to those results.[30] The second specification includes additional controls for incentives that do not restrict the sample. Finally, I add *LOG PPS* to control for compensation incentives, which reduces the sample due to data restrictions. The first observation from Table 7 is that *RRC SCORE* has a positive, statistically significant coefficient for all three specifications, consistent with the findings in Table 6. Consistent with the findings in Table 6, *PRERETURN* and *BIGN* have negative and significant coefficients. However, *CHSALES* is now positive and significant, indicating that restating firms in general have higher sales growth than non-restating firms. The added control variables in the second specification appear to control for additional determinants of restatements. Both *DEBT ISSUE* and *EQUITY ISSUE* have positive and significant coefficients (0.95 and 0.59, z-stats of 2.05 and 1.67), suggesting that revenue restatement firms access the equity and debt markets prior to a restatement announcement compared to matched-sample control firms. The positive coefficient on *OP ACC* (1.745, z-stat 2.16) suggests that revenue restaters also have higher operating accruals than matched sample firms. Turning to the third specification, many of the significant results disappear when I add *LOG PPS* to the model, most likely an indication of losing power due to the sample being restricted. The estimates on marginal effects for the three specifications indicate that a one standard

[30] The indicator variable *AUDITOR* from the restatement model is only measured if the firm had a restatement. I exclude this variable from the matched-sample design because matched sample firms do not have a restatement.

deviation increase in complexity increases the probability of misreporting between 11.6 and 21.7 percent. As discussed in Section 4.1, comparing marginal effects for *RRC SCORE* in Tables 6 and 7 provides support for the contention that the effect of complexity on misreporting may not be estimated accurately using the restatement design. Marginal effects in Table 7 are higher than those presented in Table 6, indicating that restatement firms in general have more complex revenue recognition than non-restatement firms. Overall, the combined results of Tables 6 and 7 provide evidence consistent with revenue recognition complexity increasing the probability of misreporting revenue, both statistically and economically.

5.3 Attributes of Misreporting Results

The results of the attributes of misreporting tests are found in Table 8. The table presents univariate logistic regression estimates of each misreporting attribute (*MISS GROWTH*, *MISS FCST*, *OVERSTATEMENT*, and *MULTIPLE*). Z-statistics are presented below the coefficients, with statistical significance calculated using 2-tailed tests. Although the coefficients on *RRC SCORE* are positive for each model, none are statistically significant at conventional levels. Therefore, these results do not provide convincing evidence that managers of complex revenue recognition firms make more mistakes or are intentionally manipulating revenue. However, the lack of results for these tests may indicate weaknesses in my proxies for intentional manipulation. The attributes I use appear to be the most obvious, yet observable attributes related to intentional manipulation; however, it may be that more subtle or unobservable attributes of misreporting could provide better indications of manipulation.

5.4 Consequences of Misreporting Results

Table 9 contains descriptive statistics for the sample of 348 revenue restatement firms (Panel A) and consequence variables broken down by high and low revenue recognition complexity (Panel B). Panel A shows that 20 percent of all revenue restatements result in SEC AAERs and 31 percent of revenue restatement firms have CEO turnover in the two years following the restatement. The mean (median) announcement *CAR* is -10 percent (-5.4 percent), consistent with the findings in Palmrose et al. (2004). The mean stock return for the year prior to the restatement is -19.1 percent and the mean return for the year following the restatement is -21.3 percent. Restatement attributes show the mean number of quarters restated (*RESTLEN*) is 8.2, with a mean decrease in revenue (*CHREV*) of 5.7 percent and a mean decrease in earnings (*CHNI*) of 13.4 percent. In Panel B, the sample of revenue restatements are divided into high and low revenue recognition complexity based on *RRC SCORE* relative to the mean of *RRC SCORE* for the sample. The results in Panel B show that high complex revenue recognition firms are less likely to receive an AAER (0.16 vs. 0.24, t-test -1.82) and have less negative announcement returns (-0.08 vs. -0.119, t-test 1.78) than low complexity firms. The t-test shows no significant difference for CEO turnover.

Table 10 contains regression estimates for consequences of misreporting tests. The results on AAERs show *RRC SCORE* is negatively associated with AAERs (-0.385, z-stat -2.97), suggesting restatements involving complex revenue recognition are less likely intentional. The results also show the SEC targets firms with larger market values (0.207, z-stat of 2.34). *CHREV* has a negative coefficient (-3.475, z-stat of -3.08), which is expected if the SEC is more concerned with revenue overstatements. More surprising, the coefficient on *CHNI* is 0.157 (significant at 5%). However, this does not indicate that earnings increases are most associated with AAERs, but suggests that, given the change in revenue, an increase in earnings is more associated with AAERs. Therefore, these coefficients must be interpreted collectively.

The results for announcement returns in Table 10 also show that firms with complex revenue recognition have less negative announcement returns (0.023, t-stat 2.54). The economic effect on returns is also significant. Although not tabulated, a one standard deviation increase in *RRC SCORE* (1.22) increases announcement returns by 2.8 percent. With an average market capitalization of $1.9 billion prior to the restatement, the mean change in announcement return dollars is $52 million. The CAR results also show that understatements of revenue (*CHREV*) have higher announcement returns, and firms that eventually receive AAERs have much lower restatement announcement returns (-12.1 percent). As predicted, the coefficient on *PRERETURN* is also negative (-0.033, t-stat -3.63), suggesting the market must lower expectations of future growth to a greater degree for firms with good recent stock performance.

The final regression in Table 10 on CEO turnover also provides evidence generally consistent with the mistake hypothesis. However, while the coefficient for *RRC SCORE* is negative (-0.212), it is insignificant at conventional levels (z-stat of -1.61 or p-value of 0.108). This may be resulting from one of two different effects. First, the model may not be fully specified due to missing data on CEO characteristics like age, tenure and occupying the Chairman position as mentioned in Section 4.5; however, it seems unlikely that these variables are correlated with revenue recognition complexity, suggesting that the lack of specification may not influence the complexity coefficient. Second, as expected, it may be that complexity is not associated with CEO turnover decisions because boards may not distinguish between mistakes and manipulation in determining CEO departure. The regression results do indicate that CEO turnover is higher if the firm receives an AAER, has poor operating performance prior to the restatement (*ROA*) and poor stock returns following the restatement (*POSTRETURN*). Overall, the results in Table 10 provide evidence that revenue restatements resulting from complex revenue recognition have less severe consequences.

5.5 Robustness Checks

5.5.1 Other Measures of Revenue Recognition Complexity

To test the robustness of my proxy for revenue recognition complexity, I conduct all the previous tests using alternative proxies. First, to control for the effect of multi-division firms on the length of revenue recognition disclosures, I scale *RRC SCORE* by the number of operating segments obtained from the *Compustat* Segments Database. Firms with missing segment information are assumed to have a single business line. Results using this scaled complexity score are consistent with those presented in the main analysis. Also, I conduct the tests using the individual variables *WORDS* and *METHODS* and results are substantively similar. In addition, I develop a measure to capture the subjectivity of the revenue recognition methods employed by using key-word searches for the following practices: the percentage of completion method, providing multiple deliverables, vendor-specific objective evidence, barter or non-monetary exchange revenue, or fair valuing aspects of the contract. While using this measure provides support for H1, it is insignificant in any of the tests to determine intent (attributes or consequences). Finally, I conduct the analysis using a factor score obtained from

WORDS, *METHODS*, and the subjectivity measure. Again, the results remain substantially unchanged from those presented in the main analysis.[31]

5.5.2 Managerial Discretion and Revenue Recognition Disclosures

As with any measure based on firm disclosures, there is managerial discretion about how much to disclose with respect to revenue recognition. It may be that managers of firms use their discretion to disclose more to appear more complex, thereby making it more difficult to detect misreporting. To alleviate concerns that managers may be manipulating revenue recognition disclosures prior to the restatement, and therefore affecting inferences using my measure of revenue recognition complexity, I also collect the revenue recognition disclosures found in the most recent 10-K filing just following the restatement announcement. I conjecture that if managers are exhibiting discretion in their revenue recognition disclosures prior to the restatement, that the discretion will be reduced following the restatement due to auditor scrutiny accompanying the restatement. I collect these post-disclosures for both revenue restatement firms and non-revenue restatement firms so I can compare the changes in revenue recognition pre- and post-restatement for both sets of firms.

Table 11 presents the statistics for the pre- and post-restatement revenue recognition disclosures.[32] As shown in the table, the revenue restatement firms have more *WORDS* and *METHODS*, and higher *RRC SCORE* than non-revenue restatement

[31] Using this revenue recognition complexity factor score, the coefficient on RRC SCORE in Table 5 is 0.283, with a marginal effect of 0.079 and Z-statistic of 4.22. In Table 6, the coefficients for RRC SCORE are 0.308, 0.333, and 0.569 for the three specifications, with marginal effects of 0.114, 0.123, and 0.208 and Z-statistics of 4.16, 4.38, and 3.59, respectively.

[32] The number of revenue and non-revenue restatement firms in Table 4 differs from those presented in prior tables due to the requirement to have post-restatement revenue recognition disclosure.

firms in both the pre- and post- periods. This suggests that the difference in revenue recognition complexity for revenue restatement firms was not driven by managerial discretion and still exists post-restatement. It is also interesting to note that for both the revenue restaters and non-revenue restaters, the number of *WORDS* and *METHODS* increased in the post period, but the increase was greater for the revenue restaters (102.1 and 1.7 for revenue restaters; 38.7 and 0.5 for non-revenue restaters). Therefore, it appears that while the revenue restatement firms had longer disclosures than non-revenue restatement firms prior to the restatement, even these longer disclosures did not adequately explain how the firm should have recognized revenue recognition.

5.5.3 Regulations Affecting Revenue Recognition Disclosure

Effective in 2001, SAB 101 required disclosure of the firm's revenue recognition policies and gave more substantive guidance related to the content of those disclosures. Since my proxy for revenue recognition complexity relies upon these disclosures, a positive association between complexity and misreporting may be due to a disclosure change and not a change in revenue recognition complexity. I conduct all the tests splitting the sample into pre- and post-SAB 101 restatements. All results are consistent with the results presented in the paper except results for *RRC SCORE* coefficients are insignificant for the AAER and CAR regressions in the pre-SAB 101 period. Results remain consistent in the post-SAB 101 period: higher revenue recognition complexity is associated with fewer AAERs and less negative CARs. The difference in results pre- and post-SAB 101 may suggest that lack of guidance in the pre-SAB 101 period caused firm disclosures to be less reliable measures of the firm's real revenue recognition polices, increasing noise in the measure of revenue recognition complexity in the pre-period.

5.5.4 Exclusion of Lease-Related Restatements

Due to the large number of lease-related restatements in 2005 that some may consider a change in accounting policy, I re-estimate the logistic regression to test H1 excluding these restatements. I proxy for these lease restatements by excluding all restatements identified as "cost or expense" restatements in 2005 (198 cases). Again, results are consistent with those presented in the main analysis.

Chapter 6. Conclusion

I investigate the effect of accounting complexity on misreporting using a setting of revenue recognition complexity and revenue restatements. Using two complementary research designs, I find that revenue recognition complexity significantly increases the probability of revenue misreporting. Because complexity can lead to more mistakes and/or create opportunities for manipulation, I conduct two sets of tests to determine whether the increase in misreporting from complexity is likely the result of more mistakes or opportunistic behavior. These tests examine both the attributes of the misreporting and the consequences of misreporting. Tests on the attributes of misreporting do not provide clear evidence that managers of complex revenue firms are more likely to manipulate or make mistakes. However, results are consistent with the consequences of misreporting being less severe - firms with complex revenue recognition have less negative announcement returns and are less likely to receive AAERs. Finally, my analysis shows revenue recognition complexity is not associated with CEO turnover, suggesting that boards may not distinguish between mistakes and manipulation in determining CEO departure when restatements occur.

Collectively, the results suggest that in the case of revenue recognition, complexity is a major factor in the occurrence of misreporting. This provides evidence consistent with accounting complexity being costly to financial markets, which lends support to regulators' concerns about accounting complexity. While there appears to be no strong evidence of manipulation by firms with more complex revenue recognition, accounting complexity appears to be more associated with mistakes. More research is needed to determine if accounting complexity increases misreporting in other areas besides revenue and to understand other effects of accounting complexity besides misreporting.

Appendices

Appendix 1

Example Revenue Recognition Disclosures

A.C. Moore Arts & Crafts, 2005 10-K
Revenue is recognized at point of retail sale.

UStel, Inc., 1997 10-K
Revenue is recognized upon completion of the telephone call.

Brooks Automation, 2002 10-K
Revenue from product sales are recorded upon transfer of title and risk of loss to the customer provided there is evidence of an arrangement, fees are fixed or determinable, no significant obligations remain, collection of the related receivable is reasonably assured and customer acceptance criteria have been successfully demonstrated. Revenue from software licenses is recorded provided there is evidence of an arrangement, fees are fixed or determinable, no significant obligations remain, collection of the related receivable is reasonably assured and customer acceptance criteria have been successfully demonstrated. Costs incurred for shipping and handling are included in cost of sales. A provision for product warranty costs is recorded to estimate costs associated with such warranty liabilities. In the event significant post-shipment obligations or uncertainties remain, revenue is deferred and recognized when such obligations are fulfilled by the Company or the uncertainties are resolved.

Revenue from services is recognized as the services are rendered. Revenue from fixed fee application consulting contracts and long-term contracts are recognized using the percentage-of-completion method of contract accounting based on the ratio that costs incurred to date bear to estimated total costs at completion. Revisions in revenue and cost estimates are recorded in the periods in which the facts that require such revisions become known. Losses, if any, are provided for in the period in which such losses are first identified by management. Generally, the terms of long-term contracts provide for progress billing based on completion of certain phases of work. For maintenance contracts, service revenue is recognized ratably over the term of the maintenance contract.

In transactions that include multiple products and/or services, the Company allocates the sales value among each of the deliverables based on their relative fair values.

ARI Network Services, Inc., 2001 10-K
Revenue for use of the network and for information services is recognized in the period such services are utilized. Revenue from annual or periodic maintenance fees is recognized over the period the maintenance is provided. Revenue from catalog subscriptions is recognized over the subscription term.

The Company recognizes the revenue allocable to software licenses and specified upgrades upon delivery of the software product or upgrade to the end user, unless the fee is not fixed or determinable or collectibility is not probable. The Company considers all arrangements with payment terms extending beyond 12 months and other arrangements with payment terms longer than normal not to be fixed or determinable. If the fee is not fixed or determinable, revenue is recognized as payments become due from the customer. Arrangements that include acceptance terms beyond the Company's standard terms are not recognized until acceptance has occurred. If collectibility is not considered probable, revenue is recognized when the fee is collected.

Appendix 2

Variable Definitions

Variable Name	Source	Variable Definition
WORDS	Hand Collected	The number of words in the revenue recognition footnote disclosure in the most recent 10-K filing before the restatement announcement.
METHODS	Hand Collected	The number of times "recogn" or "record" is used in the revenue recognition footnote disclosure in the most recent 10-K filing before the restatement announcement.
RRC SCORE	Hand Collected	A factor score of *WORDS* and *METHODS* using the principal components method.
NODISC	Hand Collected	An indicator set to one if the firm does not have a revenue recognition disclosure in the most recent 10-K filing before the restatement announcement and zero otherwise.
LOSSPER	Compustat	The percentage of years that earnings before extraordinary items (data18) was negative since the company began coverage on *Compustat* through the restatement announcement.
CHSALES	Compustat	The average change in net sales of the firm (data12) for the two years prior to the restatement ((Sales – lag_2(Sales))/lag_2(Sales)).
BIGN	Compustat	An indicator variable equal to one if the firm was audited by a large accounting firm (data149) and zero otherwise.
SALESFCST	I/B/E/S	An indicator set to one if the firm has an analyst forecast of sales any time prior to the restatement announcement and zero otherwise.
BTM	Compustat	The firm's book-to-market ratio (data60 / [data25 * data199]) at the end of fiscal year just prior to the restatement announcement.
AR ACCRUAL	Compustat	The 5-year average A/R accrual scaled by sales (-data302 /data12) prior to the restatement announcement.
EARNVOL	Compustat	The standard deviation of earnings (NIBEI, data18) scaled by the absolute mean value of earnings using the 5 fiscal years prior to the restatement announcement.
PRERETURN	CRSP	The 12-month stock returns for the firm prior to the restatement announcement, including delisting returns.
LOGMVE	Compustat	The logged MVE (data25*data199) at the end of fiscal year just prior to the restatement announcement.
DEBT ISSUE	Compustat	The sum of long-term and short-term debt issued (data111+data114) divided by average total assets (data6) for the fiscal year prior to the restatement announcement.
EQUITY ISSUE	Compustat	Common and preferred stock issued (data108) divided by average total assets (data6) for the fiscal year prior to the restatement announcement.
LEVERAGE	Compustat	The ratio of short-term and long-term debt (data9+data104) divided by total assets (data6) at the end of the fiscal year just prior to the restatement announcement.
EP	Compustat	The ratio of earnings per share before extraordinary items (data58) to stock price (data199) at the end of the fiscal year just prior to the restatement announcement.
OP ACC	Compustat	Operating accruals defined as operating income after depreciation less cash flows from operations (data178-data308) divided by average total assets (data6) for the fiscal year prior to the restatement announcement.

Variable Name	Source	Variable Definition
LOG PPS	Execucomp	The change in the value of stock options held for a percentage change in the value of the firm (calculated as the option delta*1% of stock price*number of options held). Core and Guay (2002) and Burns and Kedia (2006) provide details on the calculation.
OVERSTATEMENT	Hand collected	An indicator equal to one if the sum of the restated revenue over the restating periods is less than the sum of the originally reported revenue over the restating period, and zero otherwise
MULTIPLE	GAO Database	An indicator equal to one if the firm's restatement included additional areas of restatement besides revenue, and zero otherwise.
MISS GROWTH	Compustat and Hand Collected	An indicator equal to one if first restating period had positive sales growth prior to the restatement and zero or negative sales growth after the restatement, and zero otherwise.
MISS FCST	I/B/E/S and Hand Collected	An indicator equal to one if for the first restating period (1) the firm had an analyst revenue forecast, (2) the firm had previously beat the analyst revenue forecast for that period, and (3) the restatement caused the firm to miss for that period, and zero otherwise.
AAER	Dechow et al. (2007)	AAER is an indicator set to one if the firm has an SEC AAER related to revenue or receivables within 2 years of the restatement announcement.
CAR	CRSP	The 5-day cumulative abnormal return centered on the restatement announcement date. Abnormal returns are market adjusted returns and calculated as the raw return for the firm less the market return for each day.
CEO LEFT	Execucomp & Hand Collected	An indicator set to one if the CEO resigns or is terminated within two years of the restatement announcement, but excludes CEO resignation where the former CEO retains a Chair or a Director position.
RESTLEN	Hand Collected	The number of quarters the firm restated.
CHREV	Hand Collected	The percentage change in revenue over all periods of the restatement due to the restatement.
CHNI	Hand Collected	The percentage change in income over all periods of the restatement due to the restatement.
ROA	Compustat	The return on assets (NIBEI/Assets – data18/data6) for the fiscal year just prior to the restatement announcement
POSTRETURN	CRSP	The 12-month stock returns for the firm following the restatement announcement, including delisting returns.
SUBJECTIVE	Hand Collected	A count variable equal to the number of subjective revenue recognition methods identified in the revenue recognition disclosure. I search for the following key words: “percentage of completion” or “percentage-of-completion”, “multiple deliverables”, “vendor-specific objective evidence” or “VSOE”, “barter” or “non-monetary”, and “fair value” or “fair-value.”

Tables

TABLE 1
Sample selection

Panel A: Revenue restatement sample selection

Revenue Restatement Sample	
Total GAO restatement firms (1997 - 2005)	738
Missing Compustat/CRSP data	(150)
Multiple restatements/year	(33)
Financial firms (SIC 6000-6999)	(53)
No filings available	(11)
Not revenue restaters	(39)
SAB101 and EITF firms	(104)
Revenue restatement sample firms	348

Panel B: Comparison sample selection (restatement and matched-sample)

Non-Revenue Restatement Sample	
Total GAO restatement firms	1567
Missing Compustat/CRSP data	(249)
Multiple restatements/year	(48)
Financial firms (SIC 6000-6999)	(221)
No filings available	(86)
In revenue sample	(123)
Other restatement firms	840

Matched Sample (non-restating)	
Total matched sample firms	338
Missing Compustat/CRSP data	(22)
Matched sample firms	316
Revenue restatement sample firms w/PPS data	102
Matched sample firms w/PPS data	102

This table presents the attrition of the revenue restatement sample (Panel A) and the comparison samples (Panel B). Restatements are obtained from the 2002 and 2006 GAO restatement reports and cover the years 1997-2005. Firms with missing Compustat/CRSP data necessary to run tests are removed from the sample. I keep only the first restatement for firms that have more than one restatement within a calendar year. Financial firms are removed from the sample (one-digit SIC=6) as these firms have revenue recognition that is substantially different from other firms. Firms with missing 10-K filings on the SEC Edgar website prior to the restatement announcement are removed from the sample. In Panel B, firms that have been identified as a revenue restatement firm are removed from the non-revenue restatement sample. The matched sample is selected by identifying a set of potential matches of the sample firm that are in the same fiscal year as the sample firm's fiscal year just prior to the restatement announcement, have data on EXECUCOMP, and have total assets (data6) within 70 and 130 percent of the sample firm's total assets. The matched firm is selected as the one with the book-to-market ratio closest to the sample firm to control for growth.

TABLE 2
Revenue and non-revenue restatement frequency

Panel A: Restatements by year and restatement type

	Year									
Restatement Type	1997	1998	1999	2000	2001	2002	2003	2004	2005	Total
Revenue	15	23	30	53	28	45	51	53	50	348
Non-Revenue	33	38	67	50	55	110	87	138	262	840
Total Restatements	48	61	97	103	83	155	138	191	312	1188

Panel B: Restatements (Revenue/Non-Revenue) by year and industry

	Year									
Industry[a]	1997	1998	1999	2000	2001	2002	2003	2004	2005	Total
Mining & construction	0/1	0/3	0/1	0/3	2/5	2/13	1/10	3/13	0/11	8/60
Communications	1/3	0/2	0/0	4/1	1/1	0/6	4/5	2/13	0/16	12/47
Manufacturing	5/17	9/16	10/28	16/20	11/24	9/42	18/36	15/48	22/75	115/306
Other	0/0	0/0	1/1	0/2	0/0	0/2	0/0	0/0	0/1	1/6
Services	2/3	1/5	0/8	7/6	2/6	7/6	8/6	10/18	12/23	49/81
Technology	6/5	12/6	8/19	22/10	9/7	21/6	11/8	18/13	8/25	115/99
Transportation	0/0	0/1	1/0	1/2	0/1	0/7	2/1	2/7	0/5	6/24
Utilities	1/1	0/0	2/4	0/2	0/3	2/11	2/9	1/10	1/10	9/50
Wholesale & retail	0/3	1/5	8/6	3/4	3/8	4/17	5/12	2/16	7/96	33/167
Total	15/33	23/38	30/67	53/50	28/55	45/110	51/87	53/138	50/262	348/840

Panel C: Restatements by source

	Source				
Restatement Type	Company[b]	Auditor	SEC	Other/Unknown	Total
Revenue	211	47	20	70	348
Non-Revenue	444	87	99	210	840
Total Restatements	655	134	119	280	1188

This table presents the frequency of restatements for each year in the sample. Panel A reports frequency by revenue restatements and non-revenue restatements. Panel B reports those same frequencies by industry; the first/second number is the number of revenue/non-revenue restatements during the year. Panel C presents the frequency of restatement type by source of the restatement according to the press release for the restatement. The sample of restatements is obtained from the 2002 and 2006 GAO restatement reports and contains restatements during the years 1997-2005. Financial firms are removed from the sample (one-digit SIC=6) as these firms have revenue recognition that is substantially different from other firms.

[a] Industry classification is from Palmrose et al. (2004) and defined by the following SIC codes: Mining & construction=0-1999, manufacturing=2000-3999 (except codes assigned to technology), technology=3570-3579 plus 7370-7379, transportation=4000-4799, communications=4800-4899, utilities=4900-4999, wholesale/retail=5000-5999, services=7000-8999 (except codes assigned to technology), and other=9000-9999.

[b] 72 observations have both company and another source for the restatement and are excluded as being identified by the company.

TABLE 3
Revenue recognition disclosure statistics

Panel A: Revenue recognition disclosure statistics

Revenue Restatments and Non-Revenue Restatement

	Combined			Revenue Restate			Non-Revenue Restate				
	N	Mean	Std.	N	Mean	Std.	N	Mean	Std.	Diff.	t-test
WORDS	1188	210.49	234.76	348	267.83	248.80	840	186.73	224.59	81.10	5.26
METHODS	1188	4.54	4.12	348	5.85	4.42	840	4.00	3.86	1.85	6.82
RRC SCORE	1188	0.00	1.31	348	0.45	1.22	840	-0.18	1.30	0.63	7.92

Revenue Restatements and Matched Sample

	Combined			Revenue Restate			Matched Sample				
	N	Mean	Std.	N	Mean	Std.	N	Mean	Std.	Diff.	t-test
WORDS	632	216.47	210.61	316	263.10	239.12	316	169.84	165.27	93.26	5.70
METHODS	632	4.92	4.07	316	5.80	4.40	316	4.04	3.51	1.76	5.55
RRC SCORE	632	0.00	1.31	316	0.30	1.31	316	-0.30	1.23	0.61	5.99

Panel B: Revenue recognition disclosure statistic means by industry and sample

	WORDS						*METHODS*					
	Revenue		Non-Revenue		Matched		Revenue		Non-Revenue		Matched	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Mining & constructioı	154.6	108.5	141.1	93.5	117.0	91.0	3.6	3.0	3.1	2.0	2.5	2.5
Communications	214.8	172.5	259.0	153.0	265.0	138.0	6.3	5.0	5.2	4.0	7.3	6.0
Manufacturing	173.6	136.0	142.3	86.5	138.9	99.0	4.1	3.0	3.4	2.0	3.5	3.0
Other	268.0	268.0	213.7	49.0	249.6	143.5	5.0	5.0	4.8	1.5	4.8	3.0
Services	332.4	259.0	224.6	153.0	234.9	172.0	6.9	6.0	4.1	3.0	4.2	3.0
Technology	391.9	291.0	419.9	342.0	245.6	196.0	8.3	7.0	7.9	7.0	5.9	5.0
Transportation	157.7	127.0	239.2	157.0	51.0	33.5	4.2	4.0	5.5	4.0	1.8	2.0
Utilities	109.2	95.0	204.8	132.5	551.5	551.5	3.0	3.0	3.6	3.0	8.0	8.0
Wholesale & retail	178.0	115.0	93.8	67.0	113.8	60.0	3.6	2.0	2.5	2.0	2.9	2.0
All Industries	267.8	188.0	186.7	104.0	169.8	113.0	5.9	5.0	4.0	3.0	4.0	3.0

This table contains revenue recognition disclosure statistics. The sample of interest is a sample of 348 revenue restatements. I use two comparison samples, a sample of non-revenue restatements occuring over the same sample period and a matched sample. More detail about the samples can be found in Table 1. Panel A presents descriptive statistics for the revenue recognition disclosure statistics (*WORDS* , *METHODS* , and *RRC SCORE*) by sample. Panel B presents both *WORDS* and *METHODS* sample means for each industry. Industry definitions can be found in Table 2 and detailed variable definitions can be found in Appendix 2. *WORDS* and *METHODS* are winsorized at the 1 and 99 percentiles. T-tests (2-tailed) are calculated on the difference in means.

TABLE 4
Descriptive statistics

Panel A: Restatement research design descriptive statistics

	Revenue Restatements						Non-Revenue Restatements							
Variable	N	Mean	Std.	25th	Med.	75th	N	Mean	Std.	25th	Med.	75th	Diff.	t-test
RRC SCORE	348	0.445	1.225	-0.373	0.316	1.138	840	-0.184	1.297	-0.847	-0.286	0.453	0.629	7.92
BTM	348	0.496	0.534	0.165	0.395	0.712	840	0.613	0.606	0.258	0.500	0.814	-0.117	-3.30
LOSSPER	348	0.408	0.337	0.125	0.333	0.667	840	0.321	0.319	0.043	0.222	0.500	0.087	4.11
SALEFCST	348	0.753	0.432	1	1	1	840	0.665	0.472	0	1	1	0.087	3.09
EARNVOL	348	5.751	13.643	0.963	1.715	3.914	840	5.106	12.234	0.904	1.482	3.737	0.645	0.76
CHSALES	348	0.212	0.481	0.012	0.123	0.318	840	0.175	0.496	-0.020	0.085	0.233	0.037	1.18
PRERETURN	348	-0.191	0.954	-0.704	-0.352	0.032	840	0.004	0.964	-0.436	-0.134	0.214	-0.194	-3.19
BIGN	348	0.876	0.330	1	1	1	840	0.880	0.325	1	1	1	-0.003	-0.16
LOGMVE	348	5.659	1.822	4.368	5.458	6.734	840	5.668	2.069	4.191	5.621	7.047	-0.009	-0.08
AR ACCRUAL	348	0.054	0.069	0.011	0.035	0.072	840	0.024	0.051	0.001	0.009	0.032	0.029	7.09

Panel B: Matched-sample research design descriptive statistics

	Revenue Restatements						Matched Sample							
Variable	N	Mean	Std.	25th	Med.	75th	N	Mean	Std.	25th	Med.	75th	Diff.	t-test
RRC SCORE	316	0.303	1.310	-0.589	0.181	1.032	316	-0.303	1.232	-0.930	-0.394	0.308	0.606	5.99
BTM	316	0.499	0.484	0.188	0.401	0.744	316	0.492	0.460	0.189	0.396	0.704	0.007	0.18
LOSSPER	316	0.400	0.339	0.119	0.304	0.667	316	0.336	0.354	0.033	0.176	0.575	0.064	2.32
SALEFCST	316	0.712	0.454	0	1	1	316	0.753	0.432	1	1	1	-0.041	-1.17
EARNVOL	316	5.979	15.237	0.950	1.597	3.906	316	3.799	11.922	0.822	1.323	2.122	2.180	2.00
CHSALES	316	0.209	0.427	0.018	0.124	0.303	316	0.088	0.440	-0.023	0.079	0.177	0.121	3.52
PRERETURN	316	-0.194	0.811	-0.683	-0.352	0.032	316	0.032	0.914	-0.498	-0.167	0.182	-0.226	-3.29
BIGN	316	0.896	0.306	1	1	1	316	0.953	0.213	1	1	1	-0.057	-2.71
LOGMVE	316	5.739	1.775	4.449	5.538	6.844	316	5.920	1.748	4.715	5.830	6.917	-0.181	-1.29
AR ACCRUAL	316	0.052	0.073	0.010	0.034	0.071	316	0.039	0.092	0.002	0.014	0.044	0.014	2.09
DEBT ISSUE	316	0.120	0.247	0.000	0.000	0.120	316	0.083	0.197	0.000	0.000	0.066	0.037	2.08
EQUITY ISSUE	316	0.149	0.350	0.002	0.012	0.056	316	0.093	0.254	0.002	0.009	0.033	0.056	2.32
LEVERAGE	316	0.157	0.214	0.001	0.053	0.249	316	0.160	0.216	0.000	0.076	0.245	-0.003	-0.18
EP	316	-0.126	0.361	-0.111	-0.006	0.040	316	-0.104	0.349	-0.102	0.018	0.052	-0.022	-0.79
OP ACC	316	-0.022	0.137	-0.082	-0.020	0.042	316	-0.040	0.120	-0.090	-0.023	0.023	0.018	1.74
LOG PPS	102	4.860	1.352	4.060	4.734	5.663	102	4.711	1.364	3.659	4.547	5.697	0.149	0.78

This table contains descriptive statistics for revenue and non-revenue restatements (Panel A) and revenue restatements and a matched sample (Panel B). The sample of restatements is obtained from the 2002 and 2006 GAO restatement reports and contains restatements during the years 1997-2005. All variable definitions can be found in Appendix 2. All variables except *LOSSPER, PRERETURN*, *LOGMVE, LOG PPS* and indicator variables are winsorized at the 1 and 99 percentiles for each combined sample. T-tests (2-tailed) are calculated on the difference in means for the revenue and non-revenue restatement observations.

TABLE 5
Sample correlations

Panel A: Selected Correlations for Revenue Restatement and Non-Revenue Restatement Firms
(Spearman rank correlations above diagonal, Pearson below diagonal)

	REVENUE	*RRC SCORE*	*BTM*	*LOSSPER*	*SALEFCST*	*EARNVOL*	*CHSALES*	*PRERETURN*	*BIGN*	*LOGMVE*	*AR ACCRUAL*
REVENUE	1	0.236	-0.104	0.124	0.086	-0.124	0.129	0.228	0.044	0.078	-0.183
RRC SCORE	0.219	1	-0.152	0.189	0.189	-0.080	0.142	0.319	0.104	0.020	-0.052
BTM	-0.091	-0.135	1	-0.154	-0.089	0.121	-0.087	-0.198	0.090	-0.218	-0.048
LOSSPER	0.121	0.152	-0.121	1	-0.201	-0.637	-0.087	0.383	0.282	0.055	-0.192
SALEFCST	0.086	0.193	-0.132	-0.184	1	0.207	0.081	0.002	-0.041	0.098	0.004
EARNVOL	0.010	-0.001	0.057	-0.351	0.103	1	0.340	-0.342	-0.013	-0.093	0.151
CHSALES	0.031	0.042	0.045	-0.262	0.085	0.859	1	0.205	0.040	0.004	0.011
PRERETURN	-0.002	0.032	-0.080	0.336	-0.054	-0.760	-0.739	1	0.044	0.055	-0.166
BIGN	0.023	-0.005	0.158	0.013	-0.055	0.040	0.024	-0.055	1	-0.169	-0.026
LOGMVE	0.034	0.009	-0.128	0.224	0.016	-0.241	-0.149	0.161	-0.064	1	-0.096
AR ACCRUAL	-0.092	-0.005	-0.065	-0.049	0.005	0.038	0.023	-0.036	0.014	-0.030	1

Panel B: Selected Correlations for Revenue Restatement and Matched Sample Firms
(Spearman rank correlations above diagonal, Pearson below diagonal)

	REVENUE	*RRC SCORE*	*LOSSPER*	*SALEFCST*	*EARNVOL*	*CHSALES*	*PRERETURN*	*BIGN*	*AR ACCRUAL*	*DEBT ISSUE*	*EQUITY ISSUE*	*LEVERAGE*
REVENUE	1	0.233	0.122	-0.046	0.140	0.153	-0.172	-0.108	0.205	0.090	0.058	0.006
RRC SCORE	0.232	1	0.289	0.139	0.179	-0.038	-0.055	0.014	0.142	-0.117	0.134	-0.036
LOSSPER	0.092	0.228	1	0.035	0.101	-0.222	-0.146	0.078	-0.137	0.043	-0.473	-0.015
SALEFCST	-0.047	0.144	-0.010	1	0.217	0.115	-0.167	-0.139	0.251	-0.168	0.356	-0.186
EARNVOL	0.080	0.001	0.107	-0.027	1	0.009	-0.073	0.083	0.018	0.002	0.005	0.037
CHSALES	0.139	-0.024	-0.131	0.203	-0.012	1	-0.067	-0.026	-0.063	-0.024	-0.092	-0.028
PRERETURN	-0.130	-0.010	-0.151	0.000	-0.049	0.038	1	-0.078	0.338	0.098	0.350	-0.001
BIGN	-0.108	0.034	0.039	-0.139	0.083	-0.025	0.040	1	-0.170	0.015	-0.043	0.105
AR ACCRUAL	0.083	0.034	-0.121	0.312	0.017	-0.013	0.465	-0.115	1	0.022	-0.056	0.044
DEBT ISSUE	0.083	-0.031	-0.054	0.004	-0.046	0.070	0.162	0.036	0.034	1	0.084	0.227
EQUITY ISSUE	0.092	0.008	-0.227	0.419	-0.033	-0.079	0.263	0.004	-0.078	-0.057	1	-0.182
LEVERAGE	-0.007	0.038	-0.189	-0.011	0.025	0.078	0.092	0.032	0.031	0.161	-0.098	1

This table contains Pearson and Spearman rank correlations for select variables using two sets of firms: 1) Revenue restatement and non-revenue restatement firms (Panel A) and 2) Revenue restatement firms and matched-sample firms (Panel B). *REVENUE* is an indicator equal to one if the firm restated revenue and zero if the firm restated something other than revenue. All of the other variables are calculated as explained in Appendix 2.

TABLE 6
Logistic regression estimates to test H1 using a restatement design

	Prediction	Coefficient	Marginal Effects	Z-statistic
RRC SCORE	+	0.347	0.086 ***	4.71
Value Relevance				
BTM	-	-0.264	-0.029 *	-1.77
LOSSPER	+	0.277	0.017	1.01
SALESFCST (d)	+	0.479	0.086 **	2.48
EARNVOL	+	0.007	0.016	1.31
Governance				
CHSALES	-	-0.307	-0.029 *	-1.78
PRERETURN	-	-0.263	-0.048 **	-2.29
BIGN (d)	?	-0.424	-0.086 *	-1.74
LOGMVE	?	0.039	0.015	0.78
AUDITOR (d)	?	0.252	0.050	1.20
Other				
AR ACCRUAL	+	4.739	0.052 ***	3.05
Intercept		-1.88		
Industry & Year (not presented)				
N		1188		
Chi^2		161.8 ***		
Psuedo R^2		0.157		
Correctly Classified		75.8%		

This table presents estimates of a logistic regression model where the dependent variable is one if the firm restated revenue and zero if the firm restated something other than revenue. All variables are as explained in Appendix 2. Z-statistics are presented using Huber/White robust standard errors with firm-level clustering to adjust standard errors for multiple restatements from the same firm. Marginal effects are calculated for each continuous variable as the change in the predicted probability as the variable moves one standard deviation centered at the mean, holding all other variables constant at their mean values. Marginal effects for indicator variables are similarly calculated, but with the change in the predicted probability calculated as the indicator variable moves from zero to one. Results for industry and year indicators are not shown but are included in the model. *, **, *** indicate statistical significance of the coefficient at the 10, 5, or 1 percent level.

TABLE 7
Logistic regression estimates to test H1 using a matched sample design

		Original			Additional Controls			Including PPS		
	Predict.	Coeff.	Marg. Eff.	Z	Coeff.	Marg. Eff.	Z	Coeff.	Marg. Eff.	Z
RRC SCORE	+	0.362	0.116 ***	4.33	0.388	0.125 ***	4.51	0.670	0.217 ***	3.63
BTM	-	0.194	0.022	0.91	0.299	0.034	1.35	0.128	0.018	0.33
LOSSPER	+	-0.247	-0.020	-0.75	-0.176	-0.014	-0.47	-0.589	-0.029	-0.53
SALESFCST (d)	+	-0.355	-0.037	-1.64	-0.359	-0.037	-1.62	0.370	0.035	0.78
EARNVOL	+	0.012	0.031	1.64	0.011	0.030	1.53	-0.003	-0.053	-0.70
CHSALES	-	0.924	0.096 ***	3.49	0.621	0.064 **	2.26	0.193	0.014	0.29
PRERETURN	-	-0.206	-0.049 *	-1.79	-0.220	-0.052 *	-1.87	-0.247	-0.040	-0.98
BIGN (d)	?	-1.151	-0.051 ***	-3.73	-1.171	-0.052 ***	-3.78	0.536	0.016	0.44
LOGMVE		-0.037	-0.017	-0.56	-0.027	-0.012	-0.40	-0.075	-0.030	-0.43
AR ACCRUAL	+	-0.092	-0.003	-0.07	-0.278	-0.010	-0.21	-0.196	-0.008	-0.14
DEBT ISSUE	+				0.950	0.064 **	2.05	2.258	0.157 *	1.71
EQUITY ISSUE	+				0.590	0.034 *	1.67	4.025	0.068	1.41
LEVERAGE	+				-0.214	-0.014	-0.44	0.144	0.007	0.14
EP	-				0.049	0.005	0.16	-0.500	-0.041	-0.95
OP ACC	+				1.745	0.051 **	2.16	2.827	0.058	1.13
LOG PPS	+							0.136	0.046	0.71
Intercept		1.510			1.401			-1.552		
Industry (not presented)										
N		632			632			204		
Chi^2		115.2 ***			128.1 ***			53.2 ***		
Psuedo R^2		0.132			0.146			0.188		

This table presents estimates of a logistic regression model using a matched sample where the dependent variable is one if the firm restated revenue and zero if the firm did not restate revenue. The first specification includes only relevant variables from the model in Table 6. The second and third specifications add additional control variables. All variables are as explained in Appendix 2. Z-statistics are presented using Huber/White robust standard errors with firm-level clustering to adjust standard errors for multiple restatements from the same firm. Marginal effects are calculated for each continuous variable as the change in the predicted probability as the variable moves one standard deviation centered at the mean, holding all other variables constant at their mean values. Marginal effects for indicator variables are similarly calculated, but with the change in the predicted probability calculated as the indicator variable moves from zero to one. Results for industry and year indicators are not shown but are included in the model. *, **, *** indicate statistical significance of the coefficient at the 10, 5, or 1 percent level.

TABLE 8
Attributes of misreporting univariate logistic regressions

	MISS GROWTH	*MISS FCST*	*OVERSTATEMENT*	*MULTIPLE*
RRC SCORE	0.318	0.041	0.007	0.071
	1.64	0.25	0.06	0.74
Intercept	-3.173 ***	-1.977 ***	1.412 ***	-0.913 ***
	-10.44	-8.52	9.83	-7.2
N	345	217	348	348
chi2	2.62	0.06	0.00	0.54
Pseudo R2	0.019	0.00	0.00	0.001

This table contains univariate logistic regression estimates to determine whether complex revenue recognition is associated with attributes of the restatement that indicate manipulation. The sample of restatements contains restatements during the years 1997-2005. *MISS GROWTH* is an indicator equal to one if first restating period had positive sales growth prior to the restatement and zero or negative sales growth after the restatement, and zero otherwise. *MISS FCST* is an indicator equal to one if for the first restating period (1) the firm had an analyst revenue forecast, (2) the firm had previously beat the analyst revenue forecast for that period, and (3) the restatement caused the firm to miss for that period, and zero otherwise. *OVERSTATEMENT* is an indicator equal to one if the net effect of the restatement over the restatement periods was an overstatement, and zero otherwise. *MULTIPLE* is an indicator equal to one if the firm's restatement included additional areas of restatement besides revenue, and zero otherwise. *, **, *** indicate statistical significance of the statistic at the 10, 5, or 1 percent level using 2-tailed tests.

TABLE 9
Consequences of misreporting descriptive statistics

Panel A: Descriptive Statistics

Variable	N	Mean	Std.	25th	Median	75th
RRC SCORE	348	0.445	1.22	-0.373	0.316	1.138
AAER	348	0.201	0.401	0	0	0
CAR	348	-0.100	0.205	-0.187	-0.054	0.013
PRERETURN	348	-0.191	0.954	-0.704	-0.352	0.032
POSTRETURN	348	-0.213	0.802	-0.623	-0.328	0.001
CEOLEFT	348	0.310	0.463	0	0	1
LOGMVE	348	5.659	1.822	4.368	5.458	6.734
RESTLEN	348	8.175	6.487	3	7	12
CHREV	348	-0.057	0.121	-0.068	-0.017	-0.002
CHNI	348	-0.134	1.493	-0.190	-0.008	0.115
BTM	348	0.496	0.534	0.165	0.395	0.712
ROA	348	-0.145	0.426	-0.147	-0.015	0.038
MISS FCST	348	0.078	0.268	0	0	0

Panel B: Consequences of Misreporting Variables by Revenue Recognition Complexity

	High Revenue Recognition Complexity						Low Revenue Recognition Complexity							
Variable	N	Mean	Std.	25th	Median	75th	N	Mean	Std.	25th	Median	75th	Diff	t-test
RRC SCORE	173	1.394	0.881	0.684	1.139	1.805	175	-0.492	0.667	-0.788	-0.364	0.015	1.886	22.50
AAER	173	0.162	0.369	0	0	0	175	0.240	0.428	0	0	0	-0.078	-1.82
CAR	173	-0.080	0.192	-0.153	-0.045	0.022	175	-0.119	0.216	-0.222	-0.064	0.001	0.039	1.78
CEO LEFT	173	0.301	0.460	0	0	1	175	0.320	0.468	0	0	1	-0.019	-0.39

This table contains descriptive statistics for the sample of revenue restatements (Panel A) and consequences of misreporting variables by revenue recognition complexity (Panel B). The sample of restatements is obtained from the 2002 and 2006 GAO restatement reports and contains restatements during the years 1997-2005. In Panel B, the sample is divided based on revenue recognition complexity relative to the mean revenue recognition complexity for the sample. T-tests are calculated using 2-tailed tests. All variables are previously defined in Appendix 2.

TABLE 10
Consequences of misreporting regression results

	Prediction	AAER	Prediction	CAR	Prediction	CEO Turnover
RRC SCORE	-	-0.39 ***	+	0.023 **	-	-0.212
		-2.97		2.54		-1.61
BIGN	+	0.908				
		1.48				
MISSFCST	+	0.058				
		0.12				
RESTLEN	+	0.011				
		0.5				
AUDITOR	+	0.137	-	0.028		
		0.34		0.85		
MULTIPLE	?	0.75 **	?	-0.014	?	0.376
		2.19		-0.65		1.30
LOGMVE	+	0.207 **	?	-0.003	?	-0.143 *
		2.34		-0.44		-1.75
CHREV	-	-3.48 ***	+	0.399 ***	-	-0.303
		-3.08		3.19		-0.29
CHNI	-	0.157 **	+	0.005	-	0.061
		1.97		0.85		0.76
AAER			-	-0.121 ***	+	0.607 *
				-3.96		1.80
BTM			+	0.003		
				0.14		
PRERETURN			-	-0.033 ***	-	-0.387
				-3.63		-1.15
POSTRETURN					-	-0.576 **
						-2.00
ROA					-	-0.872 **
						-2.13
CAR					-	-0.379
						-0.57
Intercept		-4.672 ***		-0.010		-1.791
		-3.78		-0.14		-1.45
Industry (not presented)						
N		347		348		347
Chi^2 / F		30.02 ***		.		37.621 ***
Pseudo R^2 / R^2		0.104		0.216		0.126

This table contains logistic and OLS regression estimates using a sample of 348 revenue restatement firms to determine if revenue recognition complexity affects the consequences of restatement to the firm/managers. The first model (AAER) estimates a logistic regression with the dependent variable set to one if the revenue restatement was accompanied by an SEC AAER and zero otherwise. The second model (CAR) is an OLS regression of 5-day cumulative abnormal annoucement returns on restatement and firm characteristics. Finally, the third (CEO Turnover) estimates a logistic regression with a dependent variable set to one if the CEO departs anytime in the two years following the restatement announcement and zero otherwise. All control variables are as explained in Appendix 2. Z-statistics (for Logistic) and t-statistics (for OLS) are listed below each coefficient. I use Huber/White Robust standard errors with firm-level clustering to control for multiple restatements by the same firm. *, **, *** indicate statistical significance of the coefficient at the 10, 5, or 1 percent level.

TABLE 11
Pre- and post-restatement revenue recognition disclosure statistics

	Revenue Restatements			Non-Revenue Restatements				
Variable	N	Mean	Std. dev.	N	Mean	Std. dev.	Diff.	t-test
WORDS	339	268.3	251.4	807	183.9	221.1	84.3	5.37
POST WORDS	339	370.3	319.1	807	222.6	250.1	147.7	7.60
Difference		102.1 ***			38.7 ***		63.4	5.17
t-test		6.51			6.43			
METHODS	339	5.81	4.44	807	3.95	3.80	1.86	6.74
POST METHODS	339	7.54	5.67	807	4.50	4.30	3.04	8.86
Difference		1.7 ***			0.5 ***		1.2	5.48
t-test		7.24			5.21			
RRC SCORE	339	0.434	1.23	807	-0.199	1.29	0.633	7.82
POST RRC SCORE	339	0.554	1.32	807	-0.233	1.23	0.787	9.42
Difference		0.120 **			-0.034		0.154	2.45
t-test		2.15			-1.02			

This table contains comparisons of revenue recognition disclosure statistics pre- and post-restatement for both revenue and non-revenue restatement firms. The number of firms differs from those presented in Table 1 due to the requirement to have post-restatement revenue recognition disclosures. Variable definitions can be found in Appendix 2. All t-tests are 2-tailed.

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