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Does SOX 404 Have Teeth? Consequences of the Failure to Report Existing Internal Control Weaknesses

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Does SOX 404 Have Teeth? Consequences of the Failure to Report Existing Internal Control Weaknesses

Abstract

We examine various penalties that could serve as enforcement mechanisms for SOX 404. We focus on firms with restatements, some of which had previously reported their control weaknesses as required and some of which acknowledged them only after announcing their restatement. We find no evidence that penalties are more likely for firms, managers, or auditors that fail to report existing control weaknesses. Instead, class action lawsuits, management turnover, and auditor turnover are all more likely in the wake of a restatement when control weaknesses had previously been reported. We find similar, though weaker, evidence for SEC sanctions. These results are consistent with disclosure of control weaknesses making it difficult for management to plausibly claim later that they were unaware of the underlying conditions that led to restatements. The results also suggest that the public and private enforcement mechanisms surrounding SOX 404 are unlikely to provide strong incentives for compliance and offer a potential explanation for why most restatements are issued by firms that previously claimed to have effective internal controls.

Keywords: Sarbanes-Oxley Act; internal controls; enforcement; restatements

I. INTRODUCTION

In this paper we examine several potential consequences of failing to report existing control weaknesses as required by Section 404 of the Sarbanes-Oxley Act of 2002 (hereafter SOX 404). Our investigation is motivated largely by recent concerns about the reliability of SOX 404 reports and related evidence of firms claiming to have effective internal controls over financial reporting when they instead have material weaknesses in those controls (e.g., Turner and Weirich 2006; Glass Lewis 2007; IMA 2008; SEC 2009; Plumlee and Yohn 2010; PCAOB 2012; Rice and Weber 2012). Understanding the consequences of such reporting failures is important because it bears on managers' and auditors' incentives to detect and disclose internal control weaknesses, and thus, on the effectiveness of SOX 404 in achieving its intended goal of boosting investor confidence in the reliability of financial reports. This importance is underscored by the high costs of control audits, which have made these requirements the most controversial aspect of SOX (Coates and Srinivasan 2014).

Under SOX 404, firms and their auditors are required to provide formal opinions on the effectiveness of internal controls over financial reporting within the annual 10-K filing.[1] Within those opinions, they must state whether internal controls are effective or ineffective. Internal controls are only to be deemed effective if there are no material weaknesses, which are defined as control deficiencies that result in the likelihood of a material misstatement being more than remote (PCAOB 2004). If material weaknesses exist, they are required to be reported along with a description of their nature. This requirement, which was promulgated in the aftermath of several high profile accounting scandals, is intended to enhance investor confidence in the

[1] We use "SOX 404" to refer collectively to the requirements under § 404(a) (management's report) and § 404(b) (the auditor's report); our empirical analyses also focus on firms with both management and auditor reports. Non-accelerated filers (firms with public float less than $75 million) are exempt from § 404(b) and are thus excluded.

reliability of financial reporting by providing an early warning of the possibility of impending accounting problems (PCAOB 2004; Cunningham 2004).[2]

However, concerns have begun to emerge about the reliability of SOX 404 reports, and the effectiveness of SOX 404 in providing advance warning of potential accounting problems remains unclear. For example, the SEC has suggested that the decrease in reported control weaknesses in recent years "could be due to material weaknesses not being identified or reported," as opposed to improvements in the underlying controls (SEC 2009; see also Whitehouse 2009, 2010). Consistent with that concern, the Public Company Accounting Oversight Board (PCAOB) recently reported that 22% of the internal control audits it reviewed for 2011 were deficient, as were 15% for 2010 (PCAOB 2012; see also Rapoport 2012). Practitioners have also questioned the vigor of enforcement, noting that the SEC eliminated its accounting fraud task force in a recent reorganization (e.g., McKenna 2012).[3]

Recent evidence from academic research highlights similar concerns. Rice and Weber (2012) study a sample of firms with restatements stemming from underlying control weaknesses and find that only a minority of these firms report their weaknesses prior to the related restatements. Thus, in many cases investors are not warned that the possibility of a material misstatement in the financial reports is more than remote until *after* the need to correct such a misstatement has been announced. In addition, Plumlee and Yohn (2010) document that restatements have generally outpaced reported internal control weaknesses in recent years and suggest that many weaknesses likely go unreported.

[2] The Securities and Exchange Commission (SEC) notes that "a central purpose of the assessment of internal control over financial reporting is to identify material weaknesses that have, by their very definition, more than a remote likelihood of leading to a material misstatement in the financial statements" (SEC 2005).

[3] For example, Jack Ciesielski, owner of research firm R.G. Associates and publisher of The Analyst's Accounting Observer, is quoted in McKenna (2012, 46) arguing, "SEC enforcement of Sarbanes-Oxley has been minimal. Sarbanes-Oxley may have brought us some peace for our time, but without vigilance through long-term enforcement, it can't last."

We extend this previous research by examining whether there are substantive consequences associated with failing to report existing internal control weaknesses as required by SOX 404. Understanding these consequences is important because the evidence from previous research suggests a potential disconnect between the intended goals of SOX 404 and the enforcement mechanisms that surround its implementation.[4] In our analyses we consider various regulatory, legal, and labor market-based penalties that could serve as potential enforcement mechanisms for SOX 404, including Accounting and Auditing Enforcement Releases (AAERs) issued by the SEC, class action lawsuits filed by investors, top management turnover, and auditor turnover. In doing so, we consider both public and private mechanisms as well as potential penalties against the firm, its managers, and its auditor.

Our analyses focus on a sample of firms that are subject to SOX 404 and that also have restatements. This sample has two important features. First, while all of our sample firms have misstatements, many firms acknowledge the control weaknesses underlying those misstatements only after announcing the need for a restatement. Because restatements provide a mechanism to (ex post) identify unreported control weaknesses, our design allows us to compare the consequences for firms that reported the existence of their control weaknesses in a timely manner with those that did not. Second, consequences are unlikely for firms without restatements because their failure to report existing weaknesses may never come to light. However, within our

[4] For example, the PCAOB has argued, "For the implementation of Section 404 of the Act to achieve its objectives, the public must have confidence that all material weaknesses that exist as of the company's year-end will be publicly reported" (PCAOB 2004). The findings of Rice and Weber (2012) suggest that this condition is violated, as the majority of restating firms previously claimed to have effective controls. One potential explanation for these reporting failures is a lack of substantive penalties.

sample unreported control weaknesses are revealed by the restatement; thus, to the extent that consequences exist, they are likely to be concentrated among firms with restatements.[5]

Our empirical strategy can be summarized as follows. We first identify a sample of firms with restatements, which are generally indicative of control weaknesses. We then examine whether various consequences of the restatements differ between those firms that reported control weaknesses prior to the restatement and those that did not, controlling for restatement severity and other relevant factors. We focus on consequences that could serve as potential enforcement mechanisms for SOX 404 (i.e., penalties faced by firms, their managers, and their auditors for failing to detect and disclose existing control weaknesses).[6] Because our main variable of interest (whether or not firms report their control weaknesses prior to the restatement) is potentially endogenous, in supplementary analyses we also use propensity score matching and bivariate probit estimation to capture observable and unobservable factors, respectively, that may affect firms' ex ante propensities to report existing weaknesses.

The economics literature has long recognized the importance of enforcement for incentivizing compliance with laws, regulations, and other prescribed behavior (e.g., Becker 1968; Stigler 1970). This literature is generally based on the assumption that agents consider the expected costs and benefits in deciding whether or not to comply with laws. The strength of enforcement, then, affects the expected costs of noncompliance. The accounting literature has also recently begun to document the important role of enforcement in determining the usefulness of various reporting standards and in reducing misreporting. For example, Kedia and Rajgopal

[5] In the absence of a restatement, the only direct public signal of control effectiveness is the control report itself. Thus, without a restatement, regulators and market participants will likely never discover the existence of unreported weaknesses or the associated reporting failures.

[6] As detailed in Section IV, we also conduct our tests on a subsample that is constrained to only those firms that explicitly acknowledge the existence of control weaknesses in the aftermath of their restatement, with generally similar results.

(2011) show that due to resource constraints the SEC is more likely to investigate firms located closer to its offices, and that these firms are, in turn, less likely to adopt aggressive accounting practices (see Leuz and Wysocki 2009 for a survey on reporting regulation and enforcement). If enforcement is stringent we expect penalties surrounding restatements to be more likely for firms that previously claimed their controls were effective.

However, there are also reasons to instead expect that compliance with SOX 404 through the timely acknowledgement of ineffective controls might actually *increase* the likelihood of penalties in the event of a later restatement. First, the public disclosure of control weaknesses likely brings firms to the attention of regulators and class action law firms as potential targets of investigations and litigation. These parties pursue targets where their chance of winning is highest, and the disclosure of control weaknesses is effectively a tacit admission that the firm is in violation of the internal control provisions of the Foreign Corrupt Practices Act (FCPA).[7] Second, the disclosure of control weaknesses serves to acknowledge managements' and auditors' awareness of the existence of those weaknesses, which makes it more difficult for them to plausibly claim later that they were unaware of the conditions in the control environment that led to the restatement. Therefore, whether the enforcement of SOX 404 is stringent enough to overcome these opposing forces is an empirical question.

We begin our empirical analysis with the public enforcement of SOX 404, focusing on AAERs issued by the SEC. For most specifications we find that the likelihood of receiving an AAER following a restatement is similar regardless of whether firms had reported their control weaknesses or instead claimed their controls were effective prior to the restatement. For our propensity score-matched sample, we find that the prior acknowledgment of control weaknesses

[7] The Foreign Corrupt Practices Act of 1977 requires public firms to devise and maintain a system of internal accounting controls sufficient to provide reasonable assurance that transactions are recorded in accordance with generally accepted accounting principles (15 U.S.C. § 78m(b)(2)(B)).

increases the likelihood of receiving an AAER by about 6 percent. Taken together, we find no evidence of vigorous public enforcement of SOX 404; instead, the evidence is suggestive of the opposite: that reported control weaknesses aid the SEC in identifying cases where potential enforcement actions are likely to succeed and make it difficult for management to claim they were unaware of the problems that led to the restatement.

Turning our attention to potential private enforcement mechanisms, after controlling for restatement severity and other relevant factors, we find that class action lawsuits are 5 to 10 percent more likely when firms report internal control weaknesses prior to restatements. This is true even when we remove lawsuits that are later dismissed, which is consistent with control weaknesses reported in advance of restatements lowering plaintiffs' burden of proof in showing that management was aware (or should have been aware) of the likelihood of misstatements.

We also find that top management turnover is 15 to 26 percent more likely at firms that report control weaknesses prior to their restatements. This result is consistent with firms seeking to improve the credibility of their financial reporting by replacing managers that were clearly aware of existing problems with controls over financial reporting, yet did not prevent them from manifesting in misstatements. This result holds for both CEOs and CFOs.

Likewise, auditor turnover is 6 to 9 percent more likely at firms that report control weaknesses prior to their restatements. This result, however, appears to be driven primarily by auditor resignations (rather than dismissals), which is consistent with auditors opting to resign from riskier clients in the event of a restatement (e.g., Huang and Scholz 2012). Auditors perhaps view such clients as higher risk because, despite being aware of the existing control weaknesses, the auditors remained unable to successfully prevent material misstatements from appearing in these clients' financial statements.

This study makes three primary contributions to the accounting literature. First, our evidence showing that all else equal, SEC sanctions following restatements are no more likely for firms that previously claimed to have effective internal controls (and in some cases are less likely) suggests that public enforcement of SOX 404 is unlikely to provide strong incentives to detect and disclose existing weaknesses. Second, our results showing that penalties stemming from various private mechanisms are *more* likely for firms that report their internal control weaknesses in advance of restatements suggests the existence of possible disincentives to detect and disclose existing weaknesses. Together these results offer a potential explanation for why the majority of restatements occur at firms that previously claimed to have effective controls. Finally, we also contribute to literature that investigates various consequences of restatements (e.g., Palmrose and Scholz 2004; Palmrose, Richardson, and Scholz 2004; Desai, Hogan, and Wilkins 2006; Kravet and Shevlin 2010). Our results demonstrate that prior disclosure of control weaknesses can be an important determinant of the associated consequences and should be considered by future research.

The remainder of the paper is structured as follows. Sections II and III provide institutional background and develop our research questions and design; Section IV describes our sample; Sections V and VI present our empirical results; Section VII concludes.

II. BACKGROUND AND RESEARCH QUESTIONS

SOX 404 requires a formal assessment of whether or not internal control over financial reporting is effective. Importantly, under the SEC's implementation regulations, "Management is not permitted to conclude that the registrant's internal control over financial reporting is effective if there are one or more material weaknesses" (17 C.F.R. § 229.308(a)(3)). Thus, a violation of SOX 404 occurs when ineffective controls are claimed to be effective. Such misreporting can

occur in several circumstances.[8] One possibility is that management is simply unaware that control weaknesses exist. That is, their control testing is insufficient to detect the weaknesses and consequently they do not report them.[9] A second, related possibility is that management is generally aware that a control deficiency exists, but misjudges its severity and fails to classify it as a material weakness. As shown by Bedard and Graham (2011), the classification of control deficiencies involves significant judgment and managers often underestimate severity.[10] Finally, a third possibility is that management is aware of the weakness, but deliberately chooses not to disclose it, perhaps hoping to avoid the negative consequences that typically follow such a disclosure. As discussed in Kinney and Shepardson (2011) and Kinney et al. (2013), material weaknesses are rarely reported unless misstatements are detected during the financial audit.

For our purposes, we note that each of the above circumstances is the result of managerial and/or auditor decision-making, which ultimately leads to inaccurate SOX 404 reporting, regardless of the level of intent. From a public policy perspective, it is not clear whether more aggressive enforcement against intentional (vs. unintentional) SOX 404 misreporting would be optimal, because managers and auditors influence the level of effort expended on detecting and assessing potential weaknesses. That is, if ignorance is a valid defense, managers and auditors have decreased incentives to detect control deficiencies or make good faith judgments about their severity. Thus, while not all SOX 404 misreporting is intentional, intent is ultimately

[8] For ease of exposition we frame this discussion around management, but because the auditor also issues a control opinion, it applies to auditors as well.

[9] See Kinney, Martin, and Shepardson (2013) for a discussion of the challenges inherent in control testing, particularly as they relate to the difference in testing the effectiveness of a *process* (internal control) as opposed to the accuracy of a numerical *statement* (financial statement item).

[10] Material weaknesses are the most severe category of control deficiency, based on both the likelihood of an associated misstatement occurring and the materiality of such a misstatement, and they are the only category required to be disclosed in SOX 404 reports.

unobservable, and therefore it is important to understand the consequences of misreporting more generally.[11]

Accounting and Auditing Enforcement Releases (AAERs)

We begin our investigation of potential penalties for SOX 404 reporting failures by considering public enforcement. Section 3 of SOX grants the SEC authority to issue related regulations and enforce its various provisions. Accordingly, we focus on AAERs, which are a designation the SEC assigns to enforcement actions for accounting-related infractions. Section 3 of SOX also establishes that violations of the Act are violations of the Securities Exchange Act of 1934. As such, internal control certifications under SOX 404 are subject to the same general regulations as financial statements and other disclosures issued by registrants. In particular, Section 10(b) of the Exchange Act (15 U.S.C. § 78j) and related SEC Rule 10b-5 (17 C.F.R. § 240.10b-5) prohibit registrants from making untrue or misleading statements about material facts. Certifying that controls are effective, when they are later revealed to be ineffective, could therefore be construed as a 10b-5 violation. Hence, among firms with restatements, failures to report the existence of control weaknesses until after the restatement could be viewed as additional violations of the Exchange Act and symptomatic of deeper reporting problems. Thus, we might expect SEC enforcement actions to be more likely for firms with restatements that previously claimed to have effective controls.

However, there are also reasons to expect that the previous revelation of internal control weaknesses might instead increase the likelihood of an SEC enforcement action among firms with restatements. In particular, the SEC often prosecutes accounting violations under the FCPA, which requires both that firms keep accurate books and records, and that they devise and

[11] While our primary focus is on the penalties related to misreporting, independent of the reasons for misreporting, in Section VI we conduct additional analyses on a subsample where the misreporting is more likely to be intentional.

maintain a system of internal controls sufficient to provide reasonable assurances that financial statements are prepared in accordance with generally accepted accounting principles. These are commonly referred to as the books and records provision and the internal controls provision of the FCPA, respectively (15 U.S.C. § 78m(b)(2)(A) and (B)). Most AAERs allege violations of one or both of these provisions.[12]

The SEC regularly reviews financial statements and other public disclosures and pursues investigations in those cases where the probability of success is highest (e.g., Feroz, Park, and Pastena 1991; Files 2012). Because the disclosure of material weakness in internal controls effectively acknowledges that the firm is in violation of the control provisions of the FCPA, and because control weaknesses often signal potential accounting problems, we might expect that they increase the likelihood of an SEC investigation and resulting AAER. Moreover, previously reported control weaknesses can undermine "plausible deniability" by making it more difficult for management to claim later that they were unaware of the underlying problems that resulted in misstatements. Thus, whether the timely reporting of control weaknesses under SOX 404 decreases or increases the likelihood of an AAER is an empirical question.

Class Action Lawsuits

The remainder of our investigation focuses on various private mechanisms that could potentially serve to enforce SOX 404. The first such mechanism we consider is class action lawsuits filed by investors against restating firms. These lawsuits generally center on allegations of 10b-5 violations. In these claims, the plaintiffs plead that the defendant made a material misstatement or omission and that the plaintiffs' reliance on that misstatement or omission led to injury in connection with the purchase or sale of securities. These conditions are typically

[12] Despite its name, the Foreign Corrupt Practices Act applies to all U.S. issuers of securities. Thus, firms need not have foreign operations for these provisions to apply. Of the 43 firms in our sample that received AAERs, 41 were cited for violating the internal control provisions of the FCPA.

evidenced by the restatement of prior financial statements and related stock price drop. However, in order to survive the inevitable motions to dismiss, plaintiffs must also show that the defendants acted with intent or reckless disregard (scienter). Establishing scienter is generally the most significant hurdle facing plaintiffs in accounting-related class actions (e.g., Pritchard and Sale 2005; Johnson, Nelson, and Pritchard 2006; Donelson, McInnis, and Mergenthaler 2012). If the plaintiffs can establish scienter and survive the motion to dismiss, these cases are almost always settled rather than go to trial.

A priori, whether timely control weakness reporting decreases or increases the likelihood of a class action lawsuit following a restatement is unclear. On one hand, failure to report an existing material weakness represents an additional misstatement that can potentially be used by plaintiffs as evidence of misleading reporting (in addition to the misstated financial statements). Because violations of SOX are considered violations of the Exchange Act, misreporting on the effectiveness of internal controls can subject firms to 10b-5 claims, which some legal scholars suggest could significantly increase the likelihood of litigation (e.g., Butler and Ribstein 2006 characterize the potential liability for failing to report control weaknesses as a "litigation time bomb").[13] Hence, timely reporting of control weaknesses may be viewed as a signal of management acting in good faith and help insulate the firm from claims that management knowingly misled investors. Likewise, reporting an existing control weakness could indirectly mitigate the risk of a related lawsuit by decreasing the amount of time the stock trades at inflated prices, thereby lessening the severity of a single large price drop at the restatement

[13] As an example, Weatherford International was sued in a class action following a 2011 restatement. Prior to the restatement, Weatherford had claimed its internal controls were effective, but after the restatement Weatherford acknowledged that material weaknesses did indeed exist. Investors sued alleging misrepresentations in both the accounting and the internal control reports. The judge dismissed the accounting allegations citing insufficient evidence of scienter, but upheld the internal control allegations. The internal control violations alone carried the suit. It was settled for $52.5 million.

announcement. These arguments suggest that firms are less likely to face class action lawsuits following a restatement if they have previously reported their internal control weaknesses.

Alternatively, there are at least two reasons to expect that reporting the existence of control weaknesses could instead expose firms to a greater likelihood of litigation in the event of a restatement. First, the previous disclosure of material weaknesses in controls could increase the likelihood of scienter being established by lowering plaintiffs' burden of proof in showing that management was aware (or should have been aware) of the likelihood of misstatements. By having already acknowledged material weaknesses in internal controls, it becomes more difficult for management to reasonably claim they were unaware of associated misstatements in the financial reports.[14] Therefore, if previous disclosure of control weaknesses increases plaintiffs' likelihood of succeeding in a class action, it is likely to encourage lawsuit filings.

Second, reported control weaknesses can bring the firm to the attention of class action litigators as a potential target. For example, Files, Swanson, and Tse (2009) argue that class action law firms typically have staff specifically assigned to searching for news stories and other disclosures to identify firms with lawsuit potential, and as a result prominent disclosure of restatement information increases the likelihood of restating firms becoming targets. Consistent with this argument, they show that, all else equal, firms that disclose restatements prominently in press releases are more likely to be sued than those with stealth restatements. In our setting, given the link between control weaknesses and potential accounting problems, adverse SOX 404

[14] For example, following a restatement in 2007, Shuffle Master, Inc., which had reported material weaknesses in its internal controls prior to the restatement, was sued in a class action. As part of the scienter allegations, the plaintiffs argued, "The Defendants knew or with deliberate and extreme recklessness disregarded the fact that the Company had not corrected the previously disclosed deficiencies in its internal controls and that the Company's internal controls therefore continued to suffer from systemic weaknesses which rendered the Company's financial accounting and reporting less reliable; thus, the Defendants knew of or deliberately disregarded the risk that accounting would be improper in order to 'meet the numbers.'" The court agreed that management's knowledge of the control weaknesses (as evidenced by their SOX 404 reports), and failure to prevent those weaknesses from manifesting in misstatements, created a strong inference of scienter and denied Shuffle Master's motion to dismiss.

opinions could also serve to draw the attention of law firms, thereby increasing the likelihood of litigation in the event of a subsequent restatement.

Given the opposing arguments outlined above, we do not predict the direction of the association between timely control weakness reporting and the likelihood of class action lawsuits against firms with restatements.

Management Turnover

We also consider employment-related consequences for managers. In particular, we examine whether the likelihood that a restatement leads to management turnover is affected by whether internal control weaknesses are reported in advance of the restatement. If the failure to report existing weaknesses is perceived as a sign of managerial incompetence or intent to deceive, we might expect management turnover to be more likely for firms that claimed to have effective controls prior to their restatement. Alternatively, Li, Sun, and Ettredge (2010) show that, in general, management turnover is more likely for firms with internal control weaknesses as boards seek to improve the perceived credibility of their financial reporting. While control problems exist for all of our sample firms, at those firms whose weaknesses were reported prior to their restatement, management was clearly aware of the control problems yet was unable to prevent them from manifesting in misstatements. As a result, we might expect management turnover to be more likely at firms with reported control weaknesses. Given the conflicting arguments, which effect dominates, if any, becomes an empirical question.

Auditor Turnover

Finally, we also consider consequences for auditors. In particular, we examine whether the likelihood of auditor turnover following a restatement is affected by the previous disclosure of control weaknesses. Because opining that a firm has effective internal controls when instead

material weaknesses exist suggests a failure on the part of the audit firm, we might expect auditor turnover to be higher at firms that previously received clean SOX 404 opinions. Alternatively, previous research suggests that reported control weaknesses increase the likelihood of auditor turnover as firms dismiss auditors to enhance the credibility of their financial reporting, and as auditors resign from their riskier clients (Ettredge, Heintz, Li, and Scholz 2011; Huang and Scholz 2012). We might expect both of these effects (dismissals and resignations) to be stronger in cases where auditors were aware of control weaknesses (as evidenced by their prior adverse SOX 404 opinions) yet were unable to prevent misstatements from occurring in the financial statements. Therefore, we make no prediction on the direction of the association between auditor turnover and the reporting of existing control weaknesses.

III. RESEARCH DESIGN

Our research design centers on estimating variants of the following basic model:

PENALTY = f(REPORT ICW, *RESTATEMENT SEVERITY*, *FIRM CHARACTERISTICS AND OTHER CONTROLS)* (1)

where *PENALTY* represents one of the potential enforcement mechanisms we examine (i.e., AAER, litigation, management turnover, or auditor turnover). The details for each specific response variable are explained more fully below (all variable definitions and data sources are also provided in Appendix A). Our primary independent variable of interest is *REPORT ICW*, which is an indicator for whether or not firms reported control weaknesses in advance of their restatement (coded 1 for those that did, 0 for those that did not). In our primary analyses we use probit regression to estimate (1).

The first set of controls in (1) relate to restatement severity. All else equal, we expect that more severe restatements increase the likelihood of the various penalties. Our controls for

restatement severity include the change in reported income (*REST MAGNITUDE*), the number of distinct accounts being restated (*REST COUNT*), the number of years being restated (*REST YEARS*), an indicator for whether or not reported revenue was restated (*REST REVENUE*), and the abnormal stock return at the restatement announcement (*CAR*). Following Hennes, Leone, and Miller (2008), we also control for whether restatements are attributable to unintentional errors or intentional irregularities. *IRREGULARITY* is coded 1 for restatements related to fraud or where SEC or board-instigated independent investigations occur, and 0 otherwise.

We also include controls for relevant firm characteristics and other factors in (1). These controls are specific to each individual penalty variable, as described below.[15]

AAERs

We collect AAER information from the SEC's website (www.sec.gov) and create the variable *AAER*, which we code as 1 for firms that were the subject of an enforcement action related to their restatement and 0 otherwise. In addition to the restatement severity controls, we also include control variables related to firm size, visibility, and the severity of related investor losses in the AAER model because SEC enforcement is likely to be affected by these factors (e.g., Karpoff, Lee, and Martin 2007).[16] Since larger firms are more likely to be targets of enforcement, we include *SIZE*, measured as the natural log of the market value of common equity. We control for investor losses leading up to the restatement with *PREVIOUS RETURN*, which is calculated as the buy-and-hold abnormal return using the CRSP equally-weighted market portfolio return measured over the window (-252, -2) relative to the restatement

[15] The specific controls included in each model are primarily motivated by previous literature and are based on the nature of the specific penalty measure in question. Thus, they vary somewhat depending on which dependent variable is being examined. In untabulated tests we include the full set of all controls in each model. The controls that are omitted from our tabulated results are generally insignificant and have little impact on our main results.

[16] We omit *IRREGULARITY* from the AAER model because it is mechanically related to the dependent variable in this particular version of (1). Recall that part of the Hennes et al. (2008) irregularity classification scheme is based on whether there is an SEC investigation, which is a precursor to an AAER.

announcement. We also control for how actively a firm's shares are traded using *SHARE TURNOVER*, measured as $[1 - \prod_t(1\text{-shares traded}_t/\text{total shares}_t)]$ over the one-year period ending on the second day prior to the restatement announcement date (Field, Lowry, and Shu 2005; Files et al. 2009), and for the growth in sales over the last year of the misstatement period (*SALES GROWTH*). We expect both *SHARE TURNOVER* and *SALES GROWTH* to be positively associated with the likelihood of an AAER.

Class Action Lawsuits

We collect lawsuit data from the Stanford Securities Class Action Clearing House and create the indicator variable *LITIGATION*, which we code as 1 if a class action lawsuit related to the restatement is filed against the firm, and 0 otherwise. Because many lawsuits are dismissed, we also create *LIT EXCL DISMISS*, which is coded as 1 only for those lawsuits that are not dismissed, and 0 otherwise.

Our litigation models include the controls for restatement severity as well as several factors that prior research suggests are related to the likelihood of litigation more generally (e.g., Kim and Skinner 2012; Rogers and Stocken 2005). As in the AAER model, we include *PREVIOUS RETURN*, *SHARE TURNOVER*, *SIZE*, and *SALES GROWTH*. We expect litigation to be negatively associated with *PREVIOUS RETURN* and positively associated with the others. We also control for the standard deviation and skewness of stock returns preceding the restatement, expecting that the likelihood of litigation is positively associated with standard deviation and negatively associated with skewness (Kim and Skinner 2012). Similar to *PREVIOUS RETURN*, we measure *RETURN STD DEV* and *RETURN SKEWNESS* over the window (-252, -2) relative to the restatement announcement. We include *BIG4*, an indicator for whether the firm's auditor is one of the four largest audit firms (Deloitte, Ernst & Young,

KPMG, and PricewaterhouseCoopers), to control for possible deep pockets effects related to the size of the auditor. Finally, we include an indicator variable, *LIT INDUSTRY*, for firms in industries identified by Francis, Philbrick, and Schipper (1994) as having higher litigation risk and expect it to be positively associated with the likelihood of litigation.

Management Turnover

We identify top management turnover using Audit Analytics and code *MGT TURN* as 1 if there is a change in either CEO or CFO during the one-year period following the restatement announcement and as 0 otherwise. We also consider CEO and CFO turnover separately. Along with the restatement severity controls, our management turnover models include controls for firm size (*SIZE*), stock returns preceding the restatement announcement (*PREVIOUS RETURN*), and return-on-assets for the year prior to the restatement as a proxy for recent operating performance (*ROA*). We expect management turnover to be more likely at firms with poor recent stock returns and operating performance, but do not make a prediction for firm size.

When we estimate (1) with *MGT TURN* as the dependent variable we remove observations with management changes in the year prior to the restatement. This eliminates cases where a restatement was potentially prompted by a new manager who disagrees with the firm's previous accounting practices, and also ensures that the managers in question had signed off on at least one set of previous, misstated financial statements (and associated SOX 404 reports).

Auditor Turnover

Similar to management turnover, we use Audit Analytics data to code *AUD TURN* as 1 if there is an auditor change during the one-year period following the restatement announcement, and 0 otherwise. We also consider auditor dismissals and resignations separately. In addition to the controls for restatement severity, we include *PREVIOUS RETURN* in the auditor turnover

models, and expect turnover to be more likely at firms with lower stock returns. We also include *SALES GROWTH*, and expect that higher growth firms are more likely to change auditors (Johnson and Lys 1990). Finally, because previous research suggests that the costs of switching auditors are greater for larger firms and those with Big 4 auditors (e.g., Hennes, Leone, and Miller 2014), we include *SIZE* and *BIG4* as controls.

Following Hennes et al. (2014), and similar to our approach with management turnover, we remove observations with auditor changes in the year prior to the restatement when estimating (1) with *AUD TURN* as the dependent variable. This eliminates cases where a restatement was prompted by an auditor switch (rather than vice versa) and ensures that the auditors in question have signed off on at least one set of previous, misstated financial statements and the associated SOX 404 reports.

Controlling for Self-Selection

The treatment in our research design (i.e., *REPORT ICW*) is not randomly assigned, and therefore we also acknowledge the possibility that the reporting of existing control weaknesses could be endogenous. If there are factors that affect the reporting of existing weaknesses that are also correlated with the various penalties we examine, then the coefficient estimates from our probit models could be biased. To address this possibility, we use propensity score matching to create an alternative sample where firms are matched on factors that affect the likelihood of reporting existing control weaknesses (Rosenbaum and Rubin 1983). We estimate propensities using the model of Rice and Weber (2012), which models the probability of reporting existing weaknesses as a function of financial distress, external capital needs, firm size, recent manager and auditor turnover, audit firm size, audit fees, non-audit fees paid to the audit firm, and previously reported control deficiencies and restatements. See their Table 4 (p. 829) for more

details. We limit matches to be within a caliper of 0.03 and perform the matching with replacement (Morgan and Harding 2006; Roberts and Whited 2011).

By first modeling firms' propensities to report their existing weaknesses and then matching firms based on those propensities, we control for the known determinants of *REPORT ICW*. This provides reasonable assurance that our results are not attributable to observable differences between firms that report existing weaknesses and those that do not.[17]

IV. SAMPLE

Sample Selection

Our sample selection centers on identifying firms that are subject to SOX 404 and that also have restatements. We then match the restatements with the SOX 404 reports issued by these firms during their misstatement periods (i.e., the periods for which misstatements appeared in their financial statements that are later corrected in their restatements). This enables us to determine whether or not control weaknesses were reported prior to the restatements.

Restatements and SOX 404 reports are both drawn from Audit Analytics. We begin by extracting all restatements for U.S.-incorporated firms announced by the end of 2010 that include annual reporting periods ending after the effective date of SOX 404 (November 14, 2004). For firms with multiple restatements announced during this period, we keep only the first instance. We then match the restatements with the SOX 404 reports issued during each firm's misstatement period.[18] This process results in 834 restatement observations with matching SOX 404 reports. From this initial sample, we eliminate 31 observations for which the restatement

[17] In Section VI, we also describe a bivariate probit approach that considers the possibility of endogeneity due to other, unobservable factors.

[18] We require the SOX 404 reports to be audited, a regulation that only applies to Accelerated Filers. Thus, all our sample firms are Accelerated Filers (public float in excess of $75 million).

announcement date precedes the end of the misstatement period in Audit Analytics.[19] We also eliminate 144 observations that are missing data necessary to construct the variables in our models, resulting in a usable sample of 659 observations. We refer to this as our full sample. The full sample includes 134 firms that reported the existence of material weaknesses prior to their restatements and 525 that did not.

While restatements are generally indicative of deficiencies in the control environment, in some less common situations the occurrence of a restatement may not strictly imply the existence of a material weakness.[20] Thus, while most of the analyses we present are based on the full sample, as a sensitivity check we also construct a subsample constrained to only those observations for which we can use the firms' own disclosures to unambiguously link their restatements to underlying material weaknesses. We refer to this as our constrained sample.

For the 525 firms from the full sample that claimed to have effective internal controls during their misstatement periods (i.e., did not report any material weaknesses prior to their restatement), we examine the control-related disclosures issued in the wake of their restatements. Of the firms in this group, 260 explicitly acknowledge the existence of material weaknesses in the immediate aftermath of their restatements. Another 54 firms report that material weaknesses existed during their misstatement period but have since been remediated. Despite initially reporting that their internal controls were effective, these 314 (260 + 54) firms explicitly acknowledge in hindsight that material weaknesses did indeed exist and we retain them for our

[19] These appear to be largely situations where firms first announce an internal investigation of some accounting issue that has the potential to ultimately result in a restatement. Audit Analytics marks this initial announcement as the restatement disclosure date. In some cases, however, the investigations are protracted and details of the restatement become available only with some considerable delay. Thus, we eliminate these observations.

[20] For example, the PCAOB allows that a restatement may not imply a material weakness if it "reflected the SEC's subsequent view of an accounting matter, when the auditor concluded that management had reasonable support for its original position" (PCAOB 2004, paragraph E99). Likewise, some restatements correct relatively minor clerical errors, which may not rise to the level of a material weakness in internal controls (Rice and Weber 2012).

constrained sample. We eliminate the other 211 firms that either do not mention (176 firms) or explicitly deny (35 firms) the existence of control weaknesses after their restatement.

We also examine the 134 firms from the full sample that reported control weaknesses in advance of their restatements. We first attempt to link the particular type of misstatements being corrected with the type of control weaknesses that were originally reported (e.g., a revenue restatement and a reported weakness in controls over revenue). For those where a link is not obvious, we read the control-related disclosures issued in the aftermath of the restatement and eliminate 14 cases where management either denies that the restatement is linked to a control weakness or does not mention such a link. Hence, our constrained subsample consists of 434 observations, including 120 firms that acknowledged control weaknesses in advance of their restatements and 314 that acknowledged them only in hindsight.

Descriptive Statistics

In Table 1 we provide descriptive statistics for our regression variables. Our sample firms are relatively large with a median value for *SIZE* of 6.470 (which translates to approximately $645 million in market capitalization), and a large majority (85 percent) are audited by one of the four largest audit firms. Approximately 10 percent of sample firms face litigation as a result of their restatement, though the mean value of *LIT EXCL DISMISS* (0.061) indicates that a large proportion of these cases are ultimately dismissed. About 7 percent of the sample is sanctioned by the SEC with an AAER, while the management and auditor turnover percentages are approximately 28 and 10 percent, respectively.

[INSERT TABLE 1 ABOUT HERE]

Table 1 also includes the mean values of our variables conditional on *REPORT ICW*. Turnover rates are higher for both managers and auditors of firms that previously reported their

control weaknesses (i.e., *REPORT ICW* = 1). The differences in litigation and AAER rates across groups are not significant. Taken together, there is no evidence from these univariate comparisons to suggest a higher likelihood of penalties for firms that claimed to have effective internal controls prior to their restatement. To the contrary, management and auditor turnover rates are instead higher for firms that report their control weaknesses in a timely manner. These simple comparisons, however, do not control for other potentially relevant factors and, accordingly, we base our inferences on the regression results in the next section.

Restatement severity appears to be reasonably similar across *REPORT ICW* groups, with insignificant differences for most of our proxies (*IRREGULARITY*, which is larger for *REPORT ICW* = 1 firms, is the only exception). Regarding firm characteristics, the *REPORT ICW* = 0 firms in our sample tend to be larger and more likely to have a Big 4 auditor. Other characteristics are similar across groups.

V. RESULTS

Our primary regression results are presented in Tables 2-5, with each table focused on a different type of penalty serving as the dependent variable in model (1). Along with coefficient estimates and standard errors, we also include estimated marginal effects for each variable in these tables. The reported marginal effects are averages across all observations in the sample (Bartus 2005; Greene 2003). For binary independent variables, they represent the average change in the probability of the penalty occurring for a change in the independent variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values, to ease interpretation and comparison across variables of different scales.

AAERs

In Table 2 we provide the results of our AAER regressions. The first two sets of results represent the full sample and the propensity score-matched sample, respectively. In both cases the estimated coefficients on *REPORT ICW* are positive, but only the coefficient for the propensity score-matched sample is statistically significant. The associated marginal effect indicates that, all else equal, firms in the propensity score-matched sample are about 6 percent more likely to receive an AAER following a restatement if they have also previously disclosed control weaknesses. The results for the control variables are largely in line with expectations. Firms with more severe restatements and more negative stock returns, and those that are larger, more heavily traded, and higher growth are generally more likely to receive an AAER.

[INSERT TABLE 2 ABOUT HERE]

Because the SEC is unlikely to pursue sanctions for relatively minor violations, regardless of *REPORT ICW*, we also estimate the AAER model on a subset of firms for which the potential damages to investors are particularly large. Following Files (2012), we calculate *DAMAGES* for each firm as the difference between market capitalization at its highest point during the misstatement period and its value on the day after the restatement announcement. This measure incorporates both the size of the firm and its stock market losses. The third set of results in Table 2 represent the AAER model estimated on the subsample of firms with above-median *DAMAGES*. The estimated coefficient on *REPORT ICW* is again insignificant. Therefore, even within this subsample, we find no evidence that the failure to report control weaknesses prior to a restatement increases the likelihood of SEC sanctions.

Finally, as a sensitivity check we estimate the AAER model using the constrained sample, which includes only those firms that explicitly acknowledge that their restatements are attributable to material control weaknesses. The results are presented in the final columns of

Table 2. Consistent with the full sample, the estimated coefficient on *REPORT ICW* is positive but not statistically significant.

Overall, there is no evidence of vigorous public enforcement of SOX 404, as restating firms are no more likely to face SEC sanctions if they also failed to report their existing control weaknesses until after the restatement. If anything, the evidence is weakly suggestive of the opposite: that reported control weaknesses aid the SEC in identifying cases where potential enforcement actions are likely to succeed and provide a tacit acknowledgement of FCPA violations that makes it more difficult for management to claim they were unaware of the problems that later led to a restatement.

Class Action Lawsuits

We present our analyses of class action lawsuits in Table 3. As a starting point, we begin with *LITIGATION* as the dependent variable for the first model, but to ensure that our results are not simply driven by lawsuits that are later dismissed, we then move to *LIT EXCL DISMISS* as the dependent variable for the remainder of the models.[21]

[INSERT TABLE 3 ABOUT HERE]

The first set of results in Table 3 are for the full sample with *LITIGATION* as the dependent variable, and the next two sets are for the full and propensity score-matched samples, respectively, with *LIT EXCL DISMISS* as the dependent variable. The estimated coefficients on *REPORT ICW* are positive and significant in each of these first three models, indicating that firms reporting control weaknesses prior to their restatements are more likely to face class action lawsuits. The estimated marginal effects suggest that *REPORT ICW* = 1 firms are roughly 5 to 7 percent more likely to face litigation. These effects are also economically significant, considering

[21] We omit *IRREGULARITY* from the models where *LIT EXCL DISMISS* is the dependent variable because every observation in our sample with *LIT EXCL DISMISS* = 1 also has *IRREGULARITY* = 1. In Section VI we perform sensitivity analysis using subsamples of only firms with irregularities.

that the sample averages for *LITIGATION* and *LIT EXCL DISMISS* are about 10 and 6 percent, respectively (see Table 1). Results for the control variables are generally consistent with expectations. Lawsuits are more likely following more severe restatements, for larger and more heavily traded firms, and when stock returns are more negative.

As with our AAER analysis, we also separately examine the subsample of firms with *DAMAGES* above the sample median, as these firms are most likely to be potential targets of class actions. The estimated coefficient on *REPORT ICW* is again positive and significant for this group, with an associated marginal effect of about 10 percent.

The final set of results in Table 3 is for the constrained sample. The estimated coefficient on *REPORT ICW* is again positive, but is not statistically significant (two-sided p value = 0.1009). We note however that the marginal effect is the same magnitude as for the full sample (4.8 percent), and if we limit the constrained sample to only those with *DAMAGES* over the median the coefficient on *REPORT ICW* is again positive and significant (untabulated).

As a whole, the Table 3 results are consistent with the notion that reported control weaknesses serve to raise the awareness of class action litigators as to the potential for a given firm to be a viable target, regardless of whether the nature of the reported weakness is directly tied to the restatement. Moreover, the fact that the results hold for suits that survive motions to dismiss (*LIT EXCL DISMISS*) is consistent with previously acknowledged control weaknesses lowering plaintiffs' burden in establishing scienter by making it more difficult for management to reasonably claim that they were unaware of the conditions that led to misstatements in the financial reports.[22]

[22] In a concurrent study, Hogan, Lambert, and Schmidt (2013) also examine whether management's internal control certifications affect the likelihood of litigation. Consistent with our results, they conclude that litigation is unlikely to provide strong incentives to disclose control weaknesses in a timely manner. Our study differs from Hogan et al.

Management Turnover

We present the management turnover results in Table 4. The first three sets of results all have *MGT TURN* as the dependent variable and are for the full, propensity score-matched, and constrained samples, respectively. In each case, the estimated coefficient on *REPORT ICW* is positive and significant at $p < 0.01$. The associated marginal effects indicate that management turnover is between 15 and 26 percent more likely at firms that report control weaknesses prior to their restatements. Regarding control variables, poorer performing firms and firms with more severe restatements are generally more likely to have management turnover.

[INSERT TABLE 4 ABOUT HERE]

The final results in Table 4 examine CEO and CFO turnover separately. In both cases the estimated coefficient on *REPORT ICW* is positive and statistically significant. The marginal effects for CEOs and CFOs are roughly 7 and 10 percent, respectively. Thus, the general results for *MGT TURN* apply to both CEOs and CFOs, with a slightly larger effect for CFOs.

Overall, the Table 4 results are consistent with reported control weaknesses leading to increased turnover as firms seek to improve the credibility of their financial reporting by replacing managers (both CEOs and CFOs) that were clearly aware of problems that existed in the control environment yet did not prevent them from manifesting in misstatements.

Auditor Turnover

The auditor turnover results are provided in Table 5. For both the full sample and the propensity score-matched sample the estimated coefficient on *REPORT ICW* is positive and significant. The corresponding marginal effects suggest that auditor turnover is 6 to 9 percent more likely at firms that reported their control weaknesses prior to their restatement. As

(2013) in several ways, most notably that they focus solely on class action lawsuits while we consider a range of potential consequences surrounding SOX 404 reporting.

expected, auditor size (*BIG4*) is negatively associated with auditor turnover, as is firm size for the full sample. The pervasiveness of misstatements, as measured by the number of distinct account types being restated (*REST COUNT*) increases the likelihood of auditor turnover. Auditor changes are more likely following irregularities in the full sample as well. For the propensity score-matched sample, there is also some evidence that auditor turnover is more likely at firms that have been underperforming, as measured by stock returns during the year prior to the restatement (*PREVIOUS RETURN*).[23]

[INSERT TABLE 5 ABOUT HERE]

The results so far provide no evidence that failing to detect and disclose existing internal control weaknesses prior to a restatement increases the likelihood of auditor turnover. To the contrary, they suggest that auditor turnover is instead more likely at firms that received adverse SOX 404 opinions prior to the restatement. This result is consistent with firms seeking to improve the credibility of their financial reporting after reporting the existence of control weaknesses (e.g., Ettredge et al. 2011) or with auditors opting to resign from riskier clients (e.g., Huang and Scholz 2012).[24] To explore these possible explanations, we separately examine auditor dismissals and resignations in the next two regressions presented in Table 5. The estimated coefficient on *REPORT ICW* is insignificant for auditor dismissals, but is positive and statistically significant for auditor resignations. The corresponding marginal effect indicates that, following a restatement, auditors are about 4 percent more likely to resign from engagements with clients that have previously received an adverse SOX 404 report.

[23] In untabulated tests we also include controls for book-to-market and bankruptcy risk (Altman Z). The additional controls are statistically insignificant and their inclusion does not affect our inferences.

[24] For example, auditors may view *REPORT ICW* =1 clients as more risky because, despite clearly being aware of the existence of control weaknesses, the auditors remained unable to successfully prevent material misstatements from appearing in these clients' financial statements.

The final regression in Table 5 employs the constrained sample. We focus on auditor resignations here because the previous results suggest resignations are driving the auditor change results. The estimated coefficient on *REPORT ICW* remains positive and significant for this sample, with a marginal effect again of 4 percent.

Taken together, the results in Table 5 provide no evidence that auditors are more likely to be dismissed following the failure to detect and disclose their clients' internal control weaknesses. Instead, the results suggest that auditor changes are more likely for clients that previously received adverse SOX 404 opinions and that these changes are driven primarily by auditor resignations. Overall, the evidence appears most consistent with auditors managing risk by resigning from riskier clients, as opposed to facing penalties in the form of dismissals for failing to report control weaknesses for clients that later have restatements.[25]

VI. ADDITIONAL ANALYSES AND SENSITIVITY TESTS

Penalty Timing and the Associations between Penalties

We next consider the associations between the various penalties in our analyses by examining their timing and whether certain penalties tend to precede or follow other penalties. Underlying this analysis is the question of whether the penalties are interrelated in such a way that perhaps one penalty is driving the others (e.g., does the SEC simply pile on with an AAER after observing that investors have filed a class action lawsuit?)

In Table 6 Panel A we tabulate distributional statistics on the time to enforcement (i.e., days between the initial restatement announcement and the subsequent penalty) for the various

[25] Our tests are not designed to evaluate risk directly and thus our results should be viewed as suggestive rather than conclusive about this particular explanation. We note, however, that untabulated tests reveal *REPORT ICW* = 1 firms are more likely than *REPORT ICW* = 0 firms to have additional restatements in subsequent years, which is consistent with heightened risk for these clients (the 3-year future restatement rates are 0.366 and 0.246, respectively; difference significant at $p < 0.01$). Moreover, our previous results suggest that reported control weaknesses can increase the likelihood of lawsuits and regulatory actions in the event of a restatement, which could also influence auditors' risk assesments. We thank an anonymous reviewer for suggesting this possibility.

penalties. Class action lawsuits tend to be filed fairly quickly in the aftermath of a restatement (median of 40 days; roughly a quarter occur within a week). AAERs, by contrast, tend to be issued much later (median of 862 days, which is roughly 2.4 years).[26] Management and auditor turnovers tend to occur later than lawsuits but earlier than AAERs (median times of 145 days and 135 days, respectively).

[INSERT TABLE 6 ABOUT HERE]

In Panel B of Table 6 we provide information on the relative timing for pairs of penalties. With the exception of AAERs, Panel B indicates that the vast majority of the penalties in our sample are not preceded by any of the other penalties. For AAERs, roughly half are preceded by lawsuits. AAERs are preceded by management turnover about half the time as well.

We next re-estimate our main regression models after including additional controls for the occurrence of other, earlier penalties. For example, for the AAER regression we include control variables indicating whether the AAERs were preceded by the filing of a class action lawsuit, management turnover, or auditor turnover. The results of these regressions are reported in Table 6 Panel C (for brevity we tabulate only the results for the *REPORT ICW* variable and summary statistics, but the models also include the full sets of corresponding controls, as in our main regressions). Consistent with the conclusions from Panel B, the additional controls for other penalties are generally insignificant (untabulated), with the exception of the AAER regression where the controls for prior litigation and prior management turnover are both positive and significant. More importantly, the inclusion of the additional controls does not alter

[26] Our search for AAERs and lawsuits was conducted through the end of 2013. Thus, we have at least three years after each restatement in our sample in which to observe AAERs and lawsuits. Because AAERs are occasionally issued more than three years after restatements, we conduct two additional sensitivity analyses. First, we re-estimate our AAER model using only pre-2009 restatements, thus ensuring at least five years in which to observe an AAER. Second, we adjust the coding of our *AAER* variable such that it is only set equal to 1 if the firm receives an AAER within three years of its restatement. Our inferences are unaffected in both cases.

our inferences with respect to *REPORT ICW*; it remains positive and significant in the litigation, management turnover, and auditor turnover models, and insignificant in the AAER model.[27]

As a whole, the results in Table 6 indicate that while there are some associations between the penalties we examine, the overlap is limited enough that no one penalty appears to be driven by the others.[28]

Endogeneity due to Unobservables

A limitation of propensity score matching as a treatment for endogeneity is that it only captures the observable factors included in the first stage matching model. We also consider here the potential for endogeneity due to omitted factors (unobservables). In our setting it is possible that unmodeled differences between *REPORT ICW* = 1 firms and *REPORT ICW* = 0 firms exist and that these differences are also correlated with the penalties we examine. We address this possibility by estimating each penalty model jointly with the *REPORT ICW* determinants model from Rice and Weber (2012) using bivariate probit estimation (Greene 2003).[29] This approach considers the correlation between the error terms of the simultaneously estimated models, which addresses the possibility of endogeneity due to unobservables. The general setup is:

$$y_1^* = \boldsymbol{\beta_1 x_1} + \gamma_1 y_2 + \varepsilon_1, \qquad y_1 = 1, (y_1^* > 0) \qquad \text{(Model 1)}$$

$$y_2^* = \boldsymbol{\beta_2 x_2} + \varepsilon_2, \qquad y_2 = 1, (y_2^* > 0) \qquad \text{(Model 2)}$$

[27] We also re-estimate the AAER model with the additional controls using the propensity score-matched sample (the only specification from Table 2 in which *REPORT ICW* is statistically significant) in an untabulated test. Similar to Table 2, *REPORT ICW* is positive and statistically significant in this specification, which reinforces the conclusion that controlling for the other penalties has little impact on our results.

[28] To investigate the possibility that consequences differ with the extent of non-reporting, we also decompose the *REPORT ICW* = 0 category into the four groups described in Section IV based on how control weaknesses are discussed in firms' post-restatement disclosures (acknowledge; acknowledge but claim to now be remediated; deny; no mention). We then re-estimate our main penalty models for these *REPORT ICW* = 0 firms, with 'acknowledge' as the reference group and indicator variables for the other groups. These indicators are generally insignificant, with the exception of 'no mention,' which is negative in the litigation and auditor turnover models.

[29] Because each of our penalty variables is binary, as is the potentially endogenous variable of interest (*REPORT ICW*), bivariate probit estimation is more appropriate in our setting than the common two-stage Inverse Mills Ratio approach, which requires the second stage to be a linear model (e.g., Tucker 2010).

where ε_1 and ε_2 are normally distributed error terms. Model 1 represents our various penalties models (i.e., such that the dependent variable is either *AAER*, *LITIGATION*, *MGT TURN*, or *AUD TURN*) and Model 2 represents the Rice and Weber (2012) model of determinants of *REPORT ICW*. The question is whether the errors from Model 1 are correlated with the errors from Model 2, where ρ represents the correlation parameter. If the errors are not correlated this suggests a low likelihood of omitted correlated variables, which would be captured in the error term of each model (Greene 1998). We use bivariate probit estimation, which estimates ρ using maximum likelihood, to test the null hypothesis of no correlation ($\rho = 0$).

Diagnostics from untabulated bivariate probit estimations reveal that for each of the four penalties, the correlation between the error terms from the *REPORT ICW* model and the error terms from the respective penalty model is insignificantly different from zero. Thus, the null hypotheses of $\rho = 0$ is not rejected in any of the cases, and single-equation estimation, as in our primary results, is preferred (Greene 1998). While we cannot completely rule out the possibility, these tests indicate that endogeneity is unlikely to be a significant concern.

Irregularities Restatements

Our primary results suggest a lack of penalties for SOX 404 reporting failures. Those failures, however, could stem from either failures to detect the weaknesses (i.e., unintentional oversights or misjudgments about materiality) or purposeful failures to report known weaknesses (i.e., intentional misreporting). Thus, a potential explanation for the lack of penalties we observe is that it is driven by unintentional failures to detect weaknesses. Put differently, if managers fail to report material weaknesses simply because they do not know about them, it is possible their actions may be viewed as less egregious and associated penalties unlikely.

We investigate this possibility by confining the sample to only those restatements attributed to irregularities. Irregularities represent "intentional misreporting" in the financial statements (Hennes et al. 2008). Rice and Weber (2012) argue that managers making such intentional misstatements in the financial statements are likely to be aware of the control weaknesses they are exploiting to do so. As such, irregularities restatements provide a subsample for which SOX 404 reporting failures are less likely to be the result of unintentional oversights.[30] If there are penalties specifically for intentional misreporting of internal control effectiveness, we are more likely to observe them for the irregularities subsample.

In untabulated analysis we re-estimate our main models after narrowing the sample to only irregularities restatements. Consistent with our main results, the estimated coefficient on *REPORT ICW* is insignificant in the AAER model, but is positive and significant when both *LIT EXCL DISMISS* and *MGT TURN* are the dependent variables, with marginal effects of 6 and 13 percent, respectively. The estimated coefficient on *REPORT ICW* is insignificant for auditor turnover overall, but is positive and significant with a marginal effect of 6 percent for auditor resignations. Thus, even for this subsample where there is a higher likelihood of intentional misreporting, we again find no evidence of enforcement of SOX 404. Instead, penalties are more likely for restating firms that have previously reported control weaknesses.

Monetary Penalties

Finally, we examine the amounts of monetary penalties for AAERs and class action lawsuits. While our primary results suggest that the *occurrence* of penalties is more likely for firms that previously reported their control weaknesses, it is possible that the *amounts* of the associated penalties may be lower if, for example, managers are more likely to be perceived as

[30] We acknowledge that intent is not observable and that irregularities are merely a proxy. Thus, while unintentional oversights are less likely in this subsample, we cannot rule out that they are still possible.

acting in good faith when they report control weaknesses. This does not appear to be the case. In untabulated analysis we find that the monetary amounts associated with AAERs and class action settlements are similar across *REPORT ICW* groups. For AAERs, the median amounts are $1,000,000 for *REPORT ICW* = 0 firms and $952,000 for *REPORT ICW* = 1 firms. The median settlements for class action lawsuits are $11,000,000 and $10,000,000.[31] Thus, there is no evidence that SOX 404 is enforced through the incurrence of larger monetary penalty amounts.

VII. SUMMARY AND CONCLUSION

In this paper we study a sample of firms that are subject to internal control reporting under SOX 404 and have restatements. Only a minority of these firms report their control weaknesses in a timely manner, as required by SOX 404; the majority acknowledge their weaknesses only after having announced the need for a related restatement. We investigate whether penalties surrounding the restatements differ for these two groups of firms and in doing so provide evidence on the consequences of failing to comply with SOX 404.

We examine several potential enforcement mechanisms for SOX 404, including both public mechanisms (SEC sanctions) and private mechanisms (class action lawsuits, top management turnover, and auditor turnover). We find no evidence that penalties following a restatement are more likely for firms that fail to detect and disclose their control weaknesses as required. Instead, firms that do report their control weaknesses in a timely manner are generally more likely to face each of these penalties in the event of a later restatement. These results are consistent with the disclosure of control weaknesses making it difficult for management to plausibly claim later they had been unaware of the underlying conditions in the control environment that led to their restatements.

[31] None of these differences are statistically significant. In untabulated analysis we also estimate regression models with monetary penalties as dependent variables and controls for firm size and damages. *REPORT ICW* is insignificant in all cases.

Taken as a whole, our results, along with those of Hogan et al. (2013), suggest the enforcement mechanisms surrounding SOX 404 are unlikely to provide strong incentives to detect and disclose existing control weaknesses. In some cases they may even create perverse incentives to avoid reporting control weaknesses until their revelation is forced by a restatement. These results offer a potential explanation for why the majority of restatements occur at firms that previously claimed their internal controls were effective. The very nature of control reporting, which involves a high degree of judgment and the inherent difficulty of assessing the effectiveness of processes, is likely to make stringent enforcement a challenge. From a public policy perspective, however, our results suggest that in the long run, without more stringent enforcement, SOX 404 may be unlikely to fulfill its underlying objective of enhancing investor confidence in the reliability of financial reporting, particularly if control weaknesses continue to be acknowledged only after the restatements they helped create.

APPENDIX A
Variable Definitions

Variable	Definition
REPORT ICW	1 if the firm reported the existence of a material weakness in internal control over financial reporting in any SOX 404 report during their misstatement period, 0 otherwise (source: Audit Analytics).
AAER	1 if the firm was subject to an Accounting and Auditing Enforcement Release from the SEC related to the restatement, 0 otherwise (source: SEC website, www.sec.gov)
LITIGATION	1 if a class action lawsuit related to the restatement was filed against the firm, 0 otherwise (source: Stanford Securities Class Action Clearinghouse).
LIT EXCL DISMISS	1 if a class action lawsuit related to the restatement was filed against the firm and was not later dismissed, 0 otherwise (source: Stanford Securities Class Action Clearinghouse).
MGT TURN	1 if the firm had a CEO or CFO change during the one year period following the restatement announcement, 0 otherwise (source: Audit Analytics).
AUD TURN	1 if the firm had an auditor change during the one year period following the restatement announcement, 0 otherwise (source: Audit Analytics).
REST MAGNITUDE	The cumulative change in reported earnings due to the restatement, scaled by total market value of common equity at the end of the misstatement period, winsorized at the 1st and 99th percentiles (source: Audit Analytics).
IRREGULARITY	1 for those restatements associated with fraud or where SEC or board-instigated independent investigations occur, 0 otherwise (source: Audit Analytics).
REST REVENUE	1 if the restatement involves revenue recognition, 0 otherwise (source: Audit Analytics).
REST COUNT	Number of distinct account types being restated (source: Audit Analytics).
REST YEARS	Length of the misstatement period, in years (source: Audit Analytics).
CAR	Cumulative abnormal return, over the days (0, +1) relative to the restatement announcement date, calculated as the raw stock return minus the CRSP equally weighted market portfolio return (source: CRSP).
LIT INDUSTRY	1 if the firm is in the biotech (SIC codes 2833-2836 and 8731-8734), computer (3570-3577 and 7370-7374), electronics (3600-3674), or retail (5200-5961) industry, 0 otherwise (source: Compustat).
PREVIOUS RETURN	Buy-and-hold abnormal return, based on the CRSP equally weighted market portfolio return, over the window (-252,-2) relative to the restatement announcement, winsorized at the 1st and 99th percentiles (source: CRSP).
RETURN STD DEV	Standard deviation of daily stock returns over the window (-252, -2) relative to the restatement announcement, winsorized at the 1st and 99th percentiles (source: CRSP).
RETURN SKEWNESS	Skewness of daily stock returns over the window (-252, -2) relative to the restatement announcement, winsorized at the 1st and 99th percentiles (source: CRSP).
SHARE TURNOVER	Probability that a share was traded within a given time period, calculated as: $[1-\Pi_t(1-\text{shares traded}_t/\text{total shares}_t)]$, accumulated over the one-year period ending on the second day prior to the restatement announcement (source: CRSP).
SIZE	Natural log of total market value of common equity at the end of the misstatement period, winsorized at the 1st and 99th percentiles (source: Compustat).
BIG4	1 if the firm received a SOX 404 audit opinion from Deloitte, PricewaterhouseCoopers, Ernst & Young, or KPMG during the misstatement period, 0 otherwise (source: Audit Analytics).
SALES GROWTH	Growth in sales over the last year of the misstatement period (source: Compustat).
ROA	Return-on-assets for the year prior to the restatement, calculated as operating income before interest and taxes divided by total assets (source: Compustat).
DAMAGES	The firm's market capitalization at its highest point during the misstatement period, minus its market capitalization on the day after the restatement announcement (or, if unavailable, on the day of the restatement announcement), truncated at zero (source: CRSP).

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TABLE 1
Descriptive Statistics

	Full Sample						*REPORT ICW* = 1		*REPORT ICW* = 0	
Variable	Mean	Std. Dev.	25th Pctl	Median	75th Pctl	N	Mean	N	Mean	N
Penalties										
AAER	0.065	0.247	0.000	0.000	0.000	659	0.060	134	0.067	525
LITIGATION	0.099	0.298	0.000	0.000	0.000	659	0.127	134	0.091	525
LIT EXCL DISMISS	0.061	0.239	0.000	0.000	0.000	659	0.082	134	0.055	525
MGT TURN	0.277	0.448	0.000	0.000	1.000	487	0.386	83	0.255**	404
AUD TURN	0.097	0.296	0.000	0.000	0.000	601	0.162	111	0.082***	490
Restatement severity controls										
REST MAGNITUDE	-0.015	0.044	-0.012	-0.002	0.000	659	-0.019	134	-0.014	525
IRREGULARITY	0.686	0.465	0.000	1.000	1.000	659	0.776	134	0.663**	525
REST REVENUE	0.153	0.361	0.000	0.000	0.000	659	0.157	134	0.152	525
REST COUNT	2.331	1.551	1.000	2.000	3.000	659	2.246	134	2.352	525
REST YEARS	3.242	2.500	1.496	2.745	4.000	659	3.474	134	3.182	525
CAR	-0.022	0.075	-0.042	-0.010	0.014	659	-0.025	134	-0.021	525
Firm characteristics and other controls										
LIT INDUSTRY	0.329	0.470	0.000	0.000	1.000	659	0.381	134	0.316	525
PREVIOUS RETURN	-0.100	0.447	-0.377	-0.139	0.111	659	-0.088	134	-0.103	525
RETURN STD DEV	0.031	0.018	0.019	0.026	0.035	659	0.033	134	0.031	525
RETURN SKEWNESS	0.267	1.063	-0.174	0.273	0.715	659	0.207	134	0.283	525
SHARE TURNOVER	0.804	0.201	0.689	0.885	0.963	659	0.800	134	0.805	525
SIZE	6.573	1.422	5.548	6.470	7.384	659	6.162	134	6.678***	525
BIG4	0.850	0.358	1.000	1.000	1.000	659	0.716	134	0.884***	525
SALES GROWTH	0.078	0.232	-0.014	0.035	0.154	659	0.087	134	0.076	525
ROA	-0.005	0.169	-0.016	0.020	0.066	659	-0.010	134	-0.003	525

Variables are as defined in Appendix A. ***, **, and * represent statistically significant differences between the *REPORT ICW* = 1 and *REPORT ICW* = 0 groups at the 0.01, 0.05, and 0.10 levels, respectively. The samples for *MGT TURN* and *AUD TURN* are reduced by the elimination of observations with management and auditor turnover, respectively, during the year preceding the restatement.

TABLE 2
AAER Regressions

		Dependent Variable: *AAER*							
		Full Sample		Propensity Score-Matched Sample		Subsample with above-median *DAMAGES*		Constrained Sample	
Parameter	Predicted Sign	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect
INTERCEPT	+/-	-4.413***		-20.976***		-4.221***		-5.335***	
		(0.737)		(7.056)		(1.120)		(1.029)	
REPORT ICW	+/-	0.012	0.001	1.461**	0.058	-0.233	-0.029	0.036	0.004
		(0.235)		(0.694)		(0.310)		(0.253)	
REST MAGNITUDE	-	-5.363***	-0.006	-13.895**	-0.007	-5.529***	-0.014	-4.999***	-0.010
		(1.662)		(6.954)		(2.021)		(1.683)	
REST REVENUE	+	0.765***	0.093	3.012***	0.198	1.002***	0.176	0.769***	0.103
		(0.218)		(1.171)		(0.281)		(0.247)	
REST COUNT	+	0.047	0.008	0.303*	0.023	0.051	0.013	0.020	0.004
		(0.056)		(0.190)		(0.070)		(0.065)	
REST YEARS	+	0.131***	0.025	0.300***	0.025	0.155***	0.058	0.110***	0.032
		(0.032)		(0.116)		(0.043)		(0.040)	
CAR	-	-3.268***	-0.015	-3.303	-0.007	-3.267***	-0.028	-4.161***	-0.027
		(0.961)		(2.782)		(1.141)		(1.075)	
PREVIOUS RETURN	-	-0.486**	-0.022	-0.362	-0.010	-0.391	-0.024	-0.669***	-0.035
		(0.243)		(0.600)		(0.322)		(0.285)	
SHARE TURNOVER	+	0.303	0.007	10.244**	0.072	0.290	0.006	1.074	0.031
		(0.617)		(5.146)		(0.900)		(0.869)	
SIZE	+	0.190***	0.030	0.688**	0.027	0.148*	0.035	0.241***	0.043
		(0.073)		(0.346)		(0.090)		(0.097)	
SALES GROWTH	+	1.028***	0.015	1.146	0.009	1.253***	0.026	0.836**	0.015
		(0.378)		(1.679)		(0.494)		(0.461)	
Observations		659		214		321		434	
Likelihood ratio chi-squared		88.52		43.14		56.43		72.56	
Pseudo R-squared		0.278		0.578		0.265		0.292	

This table presents the results of AAER regressions using probit estimation. Variables are as defined in Appendix A. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on one-tailed tests for variables with predicted signs and two-tailed otherwise. Reported marginal effects are averages across all observations. For binary independent variables, they represent the change in probability of an AAER occurring for a change in the variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values.

TABLE 3
Litigation Regressions

		Dependent Variable: *LITIGATION*		Dependent Variable: *LIT EXCL DISMISS*							
		Full Sample		Full Sample		Propensity Score-Matched Sample		Subsample with above-median *DAMAGES*		Constrained Sample	
Parameter	Predicted Sign	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect
INTERCEPT	+/-	-7.903***		-6.209***		-19.852***		-10.803***		-8.448***	
		(1.024)		(1.026)		(7.145)		(2.306)		(1.490)	
REPORT ICW	+/-	0.415*	0.050	0.491**	0.048	1.535*	0.073	0.829**	0.102	0.429	0.048
		(0.216)		(0.239)		(0.786)		(0.340)		(0.262)	
REST MAGNITUDE	-	-1.416	-0.002	-2.271	-0.002	-4.476	-0.004	-2.612	-0.005	-1.964	-0.004
		(1.686)		(1.887)		(4.938)		(2.547)		(1.963)	
IRREGULARITY	+	1.434***	0.106								
		(0.399)									
REST REVENUE	+	0.638***	0.085	0.428**	0.042	0.269	0.016	0.447	0.051	0.511**	0.060
		(0.215)		(0.244)		(0.713)		(0.351)		(0.277)	
REST COUNT	+	0.067*	0.014	0.115**	0.018	0.515**	0.046	0.218***	0.040	0.105*	0.020
		(0.051)		(0.057)		(0.224)		(0.084)		(0.065)	
REST YEARS	+	0.076**	0.019	0.116***	0.020	0.087	0.013	0.161***	0.047	0.129***	0.034
		(0.033)		(0.036)		(0.094)		(0.052)		(0.045)	
CAR	-	-5.398***	-0.032	-5.314***	-0.022	-9.461***	-0.022	-5.959***	-0.036	-6.032***	-0.036
		(1.033)		(1.048)		(3.108)		(1.381)		(1.224)	
LIT INDUSTRY	+	-0.189	-0.020	-0.295	-0.024	0.696	0.041	-0.456	-0.044	-0.345	-0.034
		(0.194)		(0.220)		(0.616)		(0.297)		(0.250)	
PREVIOUS RETURN	-	-0.604***	-0.033	-0.513**	-0.021	-0.975*	-0.037	-1.175***	-0.055	-0.460*	-0.023
		(0.228)		(0.253)		(0.692)		(0.408)		(0.289)	
RETURN STD DEV	+	11.879**	0.019	8.505	0.011	34.782**	0.038	10.497	0.019	17.277**	0.025
		(6.661)		(6.780)		(17.243)		(9.137)		(8.169)	
RETURN SKEWNESS	-	0.196	0.019	0.195	0.014	0.542	0.025	0.312	0.031	0.183	0.018
		(0.083)		(0.093)		(0.333)		(0.122)		(0.101)	
SHARE TURNOVER	+	1.478***	0.045	1.035*	0.024	7.899**	0.086	3.103**	0.053	2.129**	0.054
		(0.624)		(0.713)		(4.323)		(1.524)		(1.035)	
SIZE	+	0.305***	0.057	0.234***	0.033	0.740**	0.041	0.410***	0.070	0.371***	0.059
		(0.080)		(0.088)		(0.362)		(0.137)		(0.115)	
BIG4	+	0.611*	0.054	0.509	0.034	0.696	0.034	0.946	0.065	0.598	0.048
		(0.399)		(0.444)		(0.890)		(0.795)		(0.519)	
SALES GROWTH	+	0.292	0.005	0.792**	0.011	0.044	0.001	0.969**	0.015	0.701*	0.012
		(0.396)		(0.413)		(1.354)		(0.552)		(0.499)	

Observations	659	659	214	321	434
Likelihood ratio chi-squared	160.10	95.80	47.78	71.27	90.53
Pseudo R-squared	0.377	0.318	0.517	0.375	0.358

This table presents the results of litigation regressions using probit estimation. Variables are as defined in Appendix A. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on one-tailed tests for variables with predicted signs and two-tailed otherwise. Reported marginal effects are averages across all observations. For binary independent variables, they represent the change in probability of litigation occurring for a change in the variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values.

TABLE 4
Management Turnover Regressions

		Dependent Variable: *MGMT TURN*									
		Full Sample		Propensity Score-Matched Sample		Constrained Sample		CEO Turnover only		CFO Turnover only	
Parameter	Predicted Sign	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect
INTERCEPT	+/-	-1.858***		-0.846		-1.834***		-1.944***		-1.755***	
		(0.358)		(0.765)		(0.498)		(0.391)		(0.346)	
REPORT ICW	+/-	0.435***	0.145	0.887***	0.261	0.505***	0.166	0.300*	0.069	0.339**	0.103
		(0.162)		(0.295)		(0.183)		(0.161)		(0.159)	
REST MAGNITUDE	-	-0.134	0.000	4.099	0.021	0.539	0.003	-0.144	0.000	0.546	0.002
		(1.623)		(3.657)		(1.546)		(1.573)		(1.602)	
IRREGULARITY	+	0.102	0.031	-0.657	-0.207	0.122	0.037	0.106	0.022	-0.025	-0.007
		(0.145)		(0.317)		(0.255)		(0.159)		(0.141)	
REST REVENUE	+	-0.102	-0.031	0.541	0.169	0.002	0.001	-0.060	-0.012	0.240*	0.072
		(0.203)		(0.433)		(0.227)		(0.202)		(0.183)	
REST COUNT	+	0.057*	0.035	0.256***	0.149	0.064*	0.040	0.031	0.013	0.067**	0.037
		(0.041)		(0.088)		(0.046)		(0.044)		(0.040)	
REST YEARS	+	0.021	0.016	-0.001	-0.001	0.052*	0.048	-0.002	-0.001	0.047**	0.033
		(0.026)		(0.055)		(0.032)		(0.029)		(0.025)	
CAR	-	-3.064***	-0.053	-3.278*	-0.070	-3.921***	-0.081	-1.816**	-0.021	-1.420*	-0.020
		(0.981)		(2.232)		(1.125)		(0.904)		(0.877)	
PREVIOUS RETURN	-	-0.483***	-0.075	-0.802**	-0.098	-0.678***	-0.108	-0.543***	-0.057	-0.388***	-0.054
		(0.151)		(0.396)		(0.205)		(0.164)		(0.153)	
SIZE	+/-	0.120**	0.067	-0.063	-0.028	0.078	0.044	0.081	0.031	0.076	0.038
		(0.049)		(0.118)		(0.070)		(0.054)		(0.047)	
ROA	-	-0.648*	-0.015	1.236	0.034	-0.441	-0.011	-0.845**	-0.014	-0.219	-0.004
		(0.445)		(1.310)		(0.536)		(0.430)		(0.408)	
Observations		487		124		306		580		542	
Likelihood ratio chi-squared		40.39		27.72		38.18		27.86		27.62	
Pseudo R-squared		0.070		0.176		0.102		0.059		0.048	

This table presents the results of management turnover regressions using probit estimation. Variables are as defined in Appendix A. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on one-tailed tests for variables with predicted signs and two-tailed otherwise. Reported marginal effects are averages across all observations. For binary independent variables, they represent the change in probability of turnover occurring for a change in the variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values. The CEO (CFO) Turnover Only column excludes observations with CEO (CFO) turnover during the year preceding the restatement. When *MGMT TURN* is the dependent variable, observations with either CEO or CFO turnover in the year preceding the restatement are excluded.

TABLE 5
Auditor Turnover Regressions

		Dependent Variable: *AUD TURN*				Auditor Dismissals Only		Auditor Resignations Only			
		Full Sample		Propensity Score-Matched Sample		Full Sample		Full Sample		Constrained Sample	
Parameter	Predicted Sign	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect	Est. Coef. (Std. Error)	Marg. Effect
INTERCEPT	+/-	-0.579		-1.238		-0.853**		-1.240		-0.725	
		(0.389)		(0.947)		(0.408)		(0.833)		(0.927)	
REPORT ICW	+/-	0.309*	0.055	0.668**	0.093	0.176	0.026	0.705**	0.035	0.605*	0.040
		(0.174)		(0.329)		(0.189)		(0.307)		(0.318)	
REST MAGNITUDE	-	0.183	0.000	-0.756	-0.002	-0.270	0.000	0.829	0.001	1.000	0.001
		(1.589)		(3.164)		(1.658)		(2.830)		(2.560)	
IRREGULARITY	+	0.336**	0.050	-0.041	-0.006	0.214	0.028	0.794**	0.022	0.514	0.022
		(0.178)		(0.353)		(0.183)		(0.465)		(0.525)	
REST REVENUE	+	0.000	0.000	0.162	0.025	0.112	0.016	-0.577	-0.016	-0.629	-0.026
		(0.205)		(0.409)		(0.211)		(0.503)		(0.503)	
REST COUNT	+	0.068*	0.022	0.198**	0.053	0.018	0.005	0.207***	0.013	0.183***	0.017
		(0.045)		(0.108)		(0.050)		(0.077)		(0.078)	
REST YEARS	+	-0.030	-0.013	0.028	0.012	-0.021	-0.008	-0.053	-0.005	-0.071	-0.011
		(0.034)		(0.057)		(0.035)		(0.078)		(0.081)	
CAR	-	-0.449	-0.004	1.204	0.010	-0.622	-0.005	0.745	0.002	1.312	0.004
		(0.964)		(1.976)		(1.002)		(2.111)		(2.366)	
PREVIOUS RETURN	-	-0.036	-0.003	-0.627*	-0.049	-0.069	-0.005	0.235	0.004	0.275	0.007
		(0.168)		(0.434)		(0.180)		(0.306)		(0.332)	
SIZE	-	-0.143**	-0.042	-0.143	-0.030	-0.103*	-0.026	-0.291**	-0.017	-0.280**	-0.026
		(0.062)		(0.164)		(0.064)		(0.150)		(0.161)	
BIG4	-	-0.268*	-0.048	-0.499*	-0.083	-0.156	-0.023	-0.424	-0.019	-0.489*	-0.034
		(0.206)		(0.364)		(0.224)		(0.341)		(0.370)	
SALES GROWTH	+	0.181	0.004	0.699	0.019	-0.004	-0.001	0.796*	0.005	0.838*	0.008
		(0.302)		(0.628)		(0.325)		(0.514)		(0.546)	
Observations		601		174		601		601		388	
Likelihood ratio chi-squared		24.87		18.92		10.74		28.11		22.39	
Pseudo R-squared		0.065		0.170		0.033		0.239		0.209	

This table presents the results of auditor turnover regressions using probit estimation. Variables are as defined in Appendix A. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on one-tailed tests for variables with predicted signs and two-tailed otherwise. Reported marginal effects are averages across all observations. For binary independent variables, they represent the change in probability of turnover occurring for a change in the variable from 0 to 1. For continuous independent variables, they are based on changes from first to third quartile values. The samples exclude observations with auditor changes during the year preceding the restatement.

TABLE 6
Penalty Timing and Associations between Penalties

Panel A: Time to Enforcement (Days)

	Mean	Q1	Median	Q3
AAER	882	573	862	1,186
LITIGATION	119	8	40	138
MGT TURN	165	80	145	252
AUD TURN	160	64	135	253

Panel B: Proportion of Penalties Preceded by Other Penalties

	Proportion preceded by:			
	AAER	*LITIGATION*	*MGT TURN*	*AUD TURN*
AAER		51.1%	55.8%	4.7%
LITIGATION	1.5%		18.5%	3.1%
MGT TURN	0.5%	10.8%		6.2%
AUD TURN	0.0%	4.7%	17.2%	

Panel C: Regressions with Additional Controls for Other, Earlier Penalties

	AAER Est. Coef. (Std. Error) [Marg. Effect]	*LIT EXCL DISMISS* Est. Coef. (Std. Error) [Marg. Effect]	*MGT TURN* Est. Coef. (Std. Error) [Marg. Effect]	*AUD TURN* Est. Coef. (Std. Error) [Marg. Effect]
REPORT ICW	-0.105	0.477**	0.476***	0.369**
	(0.251)	(0.243)	(0.164)	(0.179)
	[-0.009]	[0.046]	[0.157]	[0.066]
Observations	659	659	487	601
Likelihood ratio chi-squared	109.9	99.41	50.3	31.87
Pseudo R-squared	0.346	0.330	0.088	0.083

Panel A presents distributional statistics on the time to enforcement (i.e., days between the initial restatement announcement and the subsequent penalty) for the various penalties. Panel B presents information on the relative timing for pairs of penalties. Panel C presents the results of re-estimating the various penalty regressions after including additional controls for the occurrence of other, earlier penalties. Those regressions also include the full set of control variables as in the primary specifications (results for control variables are untabulated for brevity). Reported marginal effects represent the average change in probability of the dependent variable being equal to 1 for a change in *REPORT ICW* from 0 to 1. The *MGT TURN* column excludes observations with management turnover (either CEO or CFO) during the year preceding the restatement. The *AUD TURN* column excludes observations with auditor changes during the year preceding the restatement. Variables are as defined in Appendix A. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively, based on two-tailed tests.