

**The Certification Effect of New Legislation:
CEO Accountability for Misconduct After Sarbanes-Oxley**

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Abstract

We investigate how firms' responses to misconduct change when the institutional environment becomes more stringent. Organizational theory offers conflicting perspectives on whether new legislation will increase or decrease pressure on firms to take remedial action following misconduct. The dominant perspective posits that new legislation increases expectations of firm behavior, amplifying pressure on them to take remedial action after misconduct. A more recent perspective, however, suggests that the mere necessity to meet more stringent regulatory requirements certifies firms as legitimate to relevant audiences. This certification effect buffers firms, reducing the pressure for them to take remedial action after misconduct. Using a temporary, largely arbitrary exemption from a key provision of the Sarbanes-Oxley Act, we show that firms that were *not* required to meet all the regulatory standards of good governance it required became *45% more* likely to replace their CEOs following the announcement of an earnings restatement after Sarbanes-Oxley. On the other hand, those that were required to meet all of Sarbanes-Oxley's provisions became *26% less* likely to replace their CEOs following a restatement announcement. Ironically, CEOs at firms with a legislative mandate intended to increase accountability for corporate misconduct shoulder less blame than do CEOs at firms without such legislative demands.

Keywords: organizational misconduct; Sarbanes-Oxley; legislative certification; CEO turnover; symbolic management; regulation

Governments enact laws to govern firm behavior, often to prevent or minimize their wrongdoing. These laws specify appropriate rules of conduct, establish monitoring mechanisms, and outline punishments for deviations. A dominant theoretical perspective on the effect of legislation on firm behavior focuses on the various ways new laws increase pressure on firms to behave appropriately. These include imposing direct pressure on firms to comply with legal demands (DiMaggio & Powell, 1983), generating indirect pressure on them to signal their compliance through symbolic gestures (DiMaggio & Powell, 1983; Meyer & Rowan, 1977; Scott, 1995), and prompting them to take remedial action following suspicions that they have violated the new regulatory standards (Pfarrer et al., 2008; Pozner, 2008).

An alternative theoretical perspective, however, suggests that new legislation could have the opposite effect. Research has found that meeting stricter regulatory demands certifies firms as legitimate (Anderson, Daly, & Johnson, 1999; Rao, 1994; Sine, David, & Mitsuhashi, 2007), making it easier for them to manage stakeholder impressions and guide reactions to negative events (Elsbach & Sutton, 1992; Pfeffer, 1981). Ultimately, this may lead to less scrutiny of firm behavior and easier deflection of negative attention (Rao, 1994). In other words, firms can use the legal mandate to meet new regulatory requirements to signal that they are appropriate, valid, and desirable within a social system (Scott, 1987; Zucker, 1986). Thus, firms required to comply with new legislation are “certified” by it, increasing the likelihood that they will be endorsed by prominent institutional actors (Aldrich & Fiol, 1994; Hannan & Carroll, 1992), a key building block in both cognitive and sociopolitical legitimacy (Meyer & Rowan, 1977; Shane & Foo, 1999). This perspective suggests that firms subject to new legislative requirements may, ironically, be at least partially shielded from the consequences of violating them.

In this paper, we focus on the certification effect that the Sarbanes-Oxley Act of 2002 provided to firms that were required to fully comply with its new regulatory requirements. Sarbanes-Oxley defined corporate governance standards and mandated that Chief Executive Officers (CEOs) and Chief Financial Officers (CFOs) be personally liable for fraudulent financial statements. We theorize that the more stringent standards of corporate governance that Sarbanes-Oxley imposed on certain firms partially insulated them from the consequences of a restatement, lessening the pressure to take their senior executives to task. On the other hand, the subset of firms that were exempted from its most stringent provisions would not enjoy the same endorsement, forcing them to demonstrate fitness in other ways. Our fundamental argument is that, facing revelations of misconduct after announcing an earning restatement, firms that are not required to comply with legislative provisions pertinent to that misconduct substitute for the absence of legislative certification by engaging in visible remedial action (CEO replacement). We test whether exemption from Section 404, a key provision of the Sarbanes-Oxley Act requiring that firms strictly enumerate and defend the robustness of their internal controls, affected the likelihood that a firm replace its Chief Executive Officer (CEO) in the aftermath of misconduct. Replacing the CEO is a costly and highly visible decision (Gomulya & Boeker, 2014; Zavyalova et al., 2012) that heralds the seriousness of a firm's efforts to remedy the root causes of its misdeeds (Devers et al., 2009; Pozner, 2008). We find that firms that are exempt from Section 404 of Sarbanes-Oxley replace their CEOs following a restatement announcement at a significantly higher rate after Sarbanes-Oxley than they did before the enactment of the legislation, whereas firms that are similar on all relevant observable dimensions, but are required to comply with Section 404, are less likely to replace their CEOs after the legislative change. We provide detailed evidence that exempt firms are remarkably similar on observable financial

characteristics and governance variables to non-exempt firms. Within the bounds of archival studies, therefore, we are able to reasonably treat the firms just above and just below the threshold that triggers exemption as otherwise similar, and essentially randomly treated by the “certification effect” of the legislation.

The difference in remedial action between exempt firms and non-exempt firms holds for CEOs – whose replacement is highly visible (Gangloff, Connelly, & Shook, 2014), but whose connection to earnings restatements is arguably indirect – but not for CFOs, whose connection to earnings restatements is direct (Geiger & North, 2006), but whose replacement is less notable. That we find no effect for the likelihood of CFO replacement suggests that the increased rate of CEO replacement for firms exempt from Section 404 is driven by the importance of a symbolic act than it is by holding those most directly responsible for the financial statements to task.

THE CERTIFICATION EFFECT OF LEGISLATION

Much of the work on the effect of legislation on the institutional environment and firm behavior addresses how firms adapt to meet more demanding legislative standards (DiMaggio & Powell, 1983; Meyer & Rowan, 1977). A newer perspective suggests that firms may benefit from regulatory change: because many firm attributes are not directly observable, stakeholders rely on signals that they are behaving appropriately (Aldrich & Fiol, 1994; Rao, 1994), and the higher expectations that follow regulatory change provide a signal of organizational fitness to external audiences. One type of signal that external audiences interpret positively involves standards; meeting management standards such as ISO 9000 or ISO 1400, for example, certifies firms as competent in specific domains (Gray, Anand, & Roth, 2015; Montiel, Husted, & Christmann, 2012). Relatedly, Rao (1994) finds that victories in product competitions held by

respected and independent intermediaries certified winners, leading to presumptions of victors' competence and increasing their chances of survival.

Certification signals conformity with expectations but can be easily decoupled from underlying realities. Once certified, firms may comply only partially, if at all, with the criteria required for certification (Fiss & Zajac, 2004; Sine et al., 2007), yet still benefit from the certification effect. After receiving ISO9000 certification, for example, process compliance in medical device firms steadily deteriorated (Gray et al., 2015). The presence of the requirements signals firms' legitimacy, even if compliance with those requirements is only symbolic or even non-existent. We highlight how the mere enactment of new legislation certifies firms required to comply with its stricter regulatory requirements and term this benefit a *certification effect*.

Certification buffers firms from the external scrutiny that non-certified firms face, reducing the need to expend resources to demonstrate organizational fitness (Aldrich, 1999; Rao, 1994).

Absent a certification effect, firms must find other ways to demonstrate fitness.

Okhmatovskiy and David (2012) argue that organizations may substitute for compliance with a similar act, which they acknowledge can be either symbolic or substantive. This substitution response shifts stakeholder attention away from non-compliance and towards adherence to an alternate standard. Though firms ideally address misconduct in a substantive way, symbolic management—sending clear, visible signals that the firm is taking costly steps to meet stakeholders' expectations and ensure legitimacy — is also a common response (Fiss & Zajac, 2006; Gangloff et al., 2014; Westphal & Zajac, 1998). After misconduct, addressing its root causes quietly may not send a clear enough signal to external audiences that the firm is serious about change, slowing its path to restored legitimacy (Pfarrer et al., 2008). Building on this insight, we argue that firms exempt from legislative requirements designed to ensure integrity in

financial reporting may need stronger signals of reparative action than firms certified by the legislation. In contrast, firms required to comply with new standards will be buffered from negative evaluation, lessening their need to send such signals.

CEO Change After a Financial Restatement

A particularly visible form of organizational misconduct is the restatement of corporate earnings. Restatements are the correction of material errors in disclosures previously filed with the Securities and Exchange Commission (SEC) that can result from legitimate errors or “accounting irregularities,” fraudulent misapplication of accounting rules, or manipulation of facts, although it is often difficult to distinguish between intentional and unintentional misstatements. Regardless of whether they reflect fraud, most material earnings restatements are understood as admissions of negligence or misconduct. Restatements have meaningful consequences, including the loss of shareholder value (Akhigbe, Kudla, & Madura, 2005; Palmrose, Richardson, & Scholz, 2004), impaired credibility of future financial disclosures (Farber, 2005), diminished future earnings expectations, and an increase in the cost of capital (Hribar & Jenkins, 2004).

Given the potential of restatements to elicit negative organizational consequences, most restating firms take some form of remedial action to elicit positive interpretations from external audiences (Busenbark et al., 2019; Hersel et al., 2019). CEO replacement is a common remedial measure after restatements (Arthaud-Day et al., 2006; Gangloff et al., 2014). Stakeholders typically attribute responsibility for perceived wrongdoing to the chief executive (Hersel et al., 2019), making CEO replacement a salient signal of a firm’s commitment to redressing the wrongdoing. A CEO’s connection to earnings restatements is more symbolic than direct, however, and replacement may be more about signaling that the firm is taking reparative actions

rather than it is about enacting effective means of repairing the firm's corporate governance and financial reporting weaknesses (Boeker, 1992; Gangloff et al., 2014).

The Role of Legislative Certification in CEO Change After Misconduct

The period surrounding the passage of the Sarbanes-Oxley Act provides a rich context in which to study legislative certification effects. Also known as "Public Company Accounting Reform and Investor Protection Act," Sarbanes-Oxley was passed in 2002 following the corporate accounting and earnings manipulation scandals that dominated news cycles and policy debates during the preceding two years. Its goal was to increase the reliability of corporate financial statements by elaborating the responsibilities of corporate boards of directors, increasing internal accountability for the accuracy of financial disclosures, and enacting penalties for non-compliance. Among other provisions, the 64-page Act required the CEO and CFO to verify the accuracy of firm financial statements personally.

Not all firms were equally beholden to all of Sarbanes-Oxley's requirements, however. One of the most important aspects of this legislation is Section 404, which mandates that firms have external experts review their internal controls and disclose weaknesses that are discovered, generating external pressure to improve internal processes (Coates & Srinivasan, 2014). By creating an incentive to penalize senior leadership in the wake of misconduct and heightening expectations of appropriate corporate governance, Sarbanes-Oxley also increased the salience of CEO replacement as a signal of a firm's commitment to sound management and oversight (Gomulya & Boeker, 2016; Hillman et al., 2011). Thus, Sarbanes-Oxley increased the expectations of senior leadership accountability of all firms but provided only those certified by it with means to explain away mismanagement and restore legitimacy without taking meaningful action. We argue the certification effect of Sarbanes-Oxley will mean that firms required to

comply fully with the law will be less likely to replace their CEO after misconduct. In contrast, firms that are exempt from key Sarbanes-Oxley provisions will substitute for their comparative lack of legislative certification with the visible and costly signal of CEO replacement.

H1. After Sarbanes-Oxley, the rates of CEO replacement following a restatement announcement will (a) decrease for firms subject to its provisions, and (b) increase for firms exempt from them.

The Role of Legislative Certification in CFO Change After Misconduct

Research shows that, like CEOs, CFOs are likely to be replaced after an earnings restatement (Agrawal & Cooper, 2016; Feldmann, Read, & Abdolmohammadi, 2009). Though both are key members of the C-suite, the CFO controls the integrity of a firm's financial reporting in a more direct way than the CEO. If the firm's aim is truly to ensure the veracity and integrity of their financial reporting after misconduct, the replacement of a CFO is more relevant as a remedial act. But replacing a CFO is less visible than replacing a CEO, and thus is a weaker signal to external audiences, and carries less symbolic value. To explore whether the certification effect of legislation is primarily symbolic, we contrast the likelihood of replacing the CEO with the likelihood of replacing the CFO as a function of legislative change.

If replacing executives following revelations of wrongdoing were designed to address issues leading to the misreporting in the most direct way, we would expect CFO turnover trends to mirror those of CEO replacement. We would further expect this effect to be amplified following legislation specifically mandating C-suite accountability for the veracity of financial reports. Studies confirm that CFO turnover increased in the post-Sarbanes-Oxley period in response to non-fraudulent earnings restatements (Burks, 2010). Thus, the legislation did increase CFO accountability in at least some intended ways, though the remedial actions

required after fraudulent restatements likely differ from those required for more benign reasons. If CFO replacement rates were not to change appreciably after the legislation in parallel with CEO replacement, it would provide evidence that CEO replacement was necessary as a symbolic management strategy in response to misconduct.

We test whether the hypothesized changes with respect to CEO replacement after the enactment of Sarbanes-Oxley are also observable for CFOs. Support for this hypothesis would indicate that firms exempt from the legislation have a desire to improve their corporate governance substantively and address the root causes behind the need to restate. On the other hand, if we found support for increased rates of CEO replacement for exempt firms but no support for increased rates of CFO replacement for those firms, this would suggest that exempt firms are managing stakeholder impressions more symbolically than directly.

H2. After Sarbanes-Oxley, the rates of CFO replacement following a restatement announcement will (a) decrease for firms subject to its provisions, and (b) increase for firms exempt from them.

METHODS

Empirical Setting

We situate our study in the context of earnings restatements and the Sarbanes-Oxley Act. Section 404 requires external experts to review firms' internal controls and disclose any weaknesses, generating pressure to improve controls (Coates & Srinivasan, 2014), which is costly and places a considerable burden on small firms (Advisory Committee on Smaller Public Companies, 2006). To avoid imposing this disproportional cost on small firms, legislators

created a temporary exemption for firms with a public float¹ of \$75 million, which became permanent in September 2010 (SEC, 2010).

This “temporary” exemption allows us to establish an unusually clear causal argument. While arguments about the cost of compliance are related to firm size, the exemption is based on public float, which is only loosely coupled with size. The public float cutoff of \$75 million for Section 404 exemption was arbitrary, corresponding only to the SEC’s regulation around accelerated filing of annual statements,² suggesting that exempt firms are similar to non-exempt firms just above the compliance threshold. As the threshold was set in a largely arbitrary political process, we consider it to be exogenous and unknowable *a priori*, allowing us to contrast behavior of Section 404-exempt firms to those of non-exempt firms. The unpredictable sequence of extensions provides a quasi-natural experiment, providing an appropriate setting to assess the legislation’s impact on firm behavior (Arping & Sautner, 2013; Iliev, 2010).

Data

Our sample of restating firms was drawn from two databases issued by the U.S. Government Accountability Office (GAO): restatements announced between January 1, 1997 and June 30, 2002 (U.S. Government Accountability Office, January 17, 2003), and restatements

¹ Public float refers to the total market value of shares in the hands of public investors, excluding insiders and controlling interest stakeholders. This differs from market capitalization, which refers to the total value of shares outstanding, regardless of ownership.

² The temporary and arbitrary nature of the exemption means that it has been subject to regular debate. In a November 2002 final rule release, the following comment was entered into the published record:

“Comments were mixed on the proposed definition of accelerated filer. Several commenters believed all public companies should be subject to the same filing deadlines, regardless of a company's size or experience in preparing filings. Other commenters agreed with the notion of excluding smaller companies that may not have the necessary resources and infrastructure to report on an accelerated basis. Comments also were somewhat mixed on the proposed use of public float as a method to differentiate between companies. Several commenters thought the \$75 million public float threshold was too low...I believe that a public float test serves as a reasonable measure of size and market interest...” (McFarland, 2002).

Correspondingly, the Dodd-Frank Act of 2010 required the SEC to study whether \$300 million in public float might be a more reasonable cut-off for Section 404 exemption; proposals have suggested extending the exemption to firms of that size (SEC, 2016).

announced between July 1, 2002 and June 30, 2006 (U.S. Government Accountability Office, August 31, 2006). The GAO database only includes cases due to “so-called ‘aggressive’ accounting practices, intentional and unintentional misuse of facts applied to financial statements, oversight or misinterpretation of accounting rules, and fraud” (U.S. Government Accountability Office, 2002, p. 72). Of the population of 2,309 restatement announcements, complete data were available for 752 restatements from 410 firms. We include the first restatement for each firm to avoid inflating the number of observations.

Dependent Variables. We test firm responses to restatements using *CEO change* and *CFO change*, which take on a value of 1 if the CEO or CFO was replaced within the 365-days following the restatement announcement, and a value of 0 otherwise. These data were collected from media reports and corporate 8-K filings with the SEC.

Independent Variables. Our primary independent variable is an interaction of *Post-Sarbanes-Oxley* and *Exempt from Section 404*. *Post-Sarbanes-Oxley* takes a value of 1 if the restatement was announced after the Sarbanes-Oxley Act was signed on July 30, 2002, and 0 if the restatement preceded the legislation. *Exempt from Section 404* was assigned a value of 1 if the restating firm was below the \$75 million public float threshold for exemption from Section 404; a value of 0 indicates a requirement to comply with Section 404 during the quarter in which the restatement was announced. The interaction *Post-Sarbanes-Oxley X Exempt from Section 404* takes a value of 1 if the firm was exempt from Section 404 after Sarbanes-Oxley’s passage.

Control Variables. Media attention is an important driver of CEO turnover (Pozner, Mohliver, & Moore, 2019; Wiersema & Zhang, 2013) and may vary between exempt and non-exempt firms. We account for baseline levels of media attention with *Number of Articles about the Company*, a count of articles mentioning the focal firm in the year prior to restatement,

gathered from the *Lexis-Nexis* news service. We also measure the number of articles that included the word “restatement” for the 365-day period beginning with restatement announcement to proxy for external pressure, but exclude articles published after the CEO turnover announcement; we label this variable *Number of Articles about the Restatement*.

We also include controls for firm size and performance. *Market Capitalization* measures the value of stock outstanding, calculated 21 trading days before the restatement announcement. We also include *Return on Assets*, capturing average firm profitability for three years, collected using the *CRSP/Compustat* merged database.

To ensure that our independent variables capture only the variance attributable to the act of restating rather than characteristics of the restatement itself, we include four measures of restatement severity. The dummy variable *Increased net income* is included because restatements that raise net income generally elicit fewer penalties than those that do not (Akhigbe et al., 2005). This measure was drawn from public filings in the SEC’s *EDGAR* database. We include a dummy variable for restatements driven by *error involving fraud*, which engender particularly adverse outcomes (Hennes, Leone, & Miller, 2008). *Number of quarters restated*, a count of the number of reporting periods corrected in the restatement, captures the duration of the problem underlying restatement. Finally, we include *Restatement was prompted by the firm*; if a restatement was prompted by the SEC or external auditor, the focal firm failed either to find or to disclose its improper accounting, potentially increasing the importance of the firm to take remedial action.

Identification Strategy

Our empirical test relies on comparing two groups of firms that are similar along all dimensions except for Section 404. We thus compare firms below the \$75 million exemption

threshold to the rest of the firms in the “micro-cap” category, which includes firms with up to \$150 million public float and market capitalization no larger than \$1.5 billion, resulting in a final sample of 84 firms. We report the results of several robustness checks as well as models testing our hypotheses on broader samples, which increase our sample size to 210 firms. Generally, we err on the side of precision over sample size, which reduces statistical power but increases the chances that effects we identify can be attributed to the exemption rather than omitted variables. A firm with a public float of \$70 million is likely to differ on multiple dimensions from a firm with a public float of \$500 million, but not necessarily from a firm with a public float of \$100 million.

Our statistical identification relies on the similarity of two groups of firms (“Exempt from Section 404” and “Not Exempt from Section 404”) with respect to the characteristics associated with CEO replacement in the wake of restatements. Two attributes of the firms in our sample lend confidence to this assumption. First, although firm size correlates with CEO dismissal, public float is only weakly related to firm size (correlation of 0.4). Similarly, there is no difference in the mean rate of pre-Sarbanes Oxley CEO replacement between exempt and non-exempt firms; a two-sample t-test finds a similar rate across the two groups ($t=1.58$).

Second, our sample occupies a small part of the distribution of firm size within the population of publicly traded corporations. Public firms vary in value from a few million dollars to hundreds of billions of dollars. When the threshold of \$75 million free float was created, it represented the smallest 0.00018% of public firms; adding firms with free floats larger than \$75 million but smaller than \$150 million (our comparison group) increases the threshold by only 0.0003%. Figure 1a represents the entire distribution of firms as a function of market capitalization (y-axis) and free float (x-axis); the figure-in-figure represents the section of the

range from which our sample derives. In the entire distribution, the range of market capitalization extends to \$1 trillion, and the range of free float extends to \$60 billion. Figure 1b zooms in from Figure 1a, while Figure 1c documents the parts of the distribution where our sample lies; within this bracket, exempt and non-exempt firms should not differ systematically.

FIGURES 1a-c HERE

We further restrict our sample by excluding firms that with market caps higher than that of the largest exempt firm (the horizontal dashed line), as well as firms that have public float larger than \$150 million. Unreported analysis demonstrates that firms excluded from the comparison set are dissimilar, while firms included in the comparison set are similar in terms of profitability, number of quarters restated, number of analysts following, and network centrality in the network of directors, justifying this empirical choice.

Lastly, we test for differences between exempt and non-exempt firms (with a free float between \$75 and \$150 million) on observables in Table 1. The two groups are identical on all but two dimensions: non-exempt firms have higher market capitalization ($\mu=389$, $\mu=265$, $p=.06$) and replace their CEOs more quickly ($\mu=142$ days, $\mu=232$ days, $p=.09$) than exempt firms.³ The groups do not differ on the likelihood of CEO replacement ($p=.75$) or CFO replacement ($p=.19$), pre-restatement media attention ($p=.34$) or post-restatement media attention ($p=.96$). We control for systematic differences in size in our empirical analysis.

TABLE 1 HERE

Model. We use a linear probability model with robust standard errors, represented as:

³ In response to a reviewer's comment that non-exempt firms replacing their CEOs more quickly than exempt firms do suggests that CEOs from non-exempt firms are less protected by certification that we argue, we ran our primary models (which we report in Table 3), with a restricted time window for CEO replacement. When we restrict the time window for CEO replacement to 6 months rather than 12, our results are qualitatively identical in direction and significance.

$$y_i = \alpha + \beta_1 Exempt_i + \beta_2 SOX + \beta_3 (Exempt \times SOX) + \beta X_i + \varepsilon_i$$

in which y is a dummy variable taking the value 1 if the CEO changed after the restatement in models 1-2 and zero otherwise. In models 3-4 the variable takes the value 1 if the CFO changed after the restatement and zero otherwise. *Exempt from Section 404* is a dummy variable taking the value 1 if the firm is smaller than the threshold required to comply with Section 404 (\$75 million in public float on the last day of the second quarter), *Post-Sarbanes-Oxley* takes the value 1 if the restatement occurred after Sarbanes-Oxley's passage. X is a vector of controls. The coefficient of interest the interaction term *Post-Sarbanes-Oxley X Exempt from Section 404*, which captures the change in behavior of firms exempt from Section 404 after Sarbanes-Oxley passed compared to firms required to comply with the legislation. A one-unit increase in the independent variable translates to a $\beta \times 100$ percent change in the dependent variable (CEO replacement and CFO replacement). Replications of all analyses using both logit and probit models show stronger statistical significance for our variable of interest, but with less easily interpreted results.

Results

We report descriptive statistics and correlations in Table 2. Table 3 reports the results of a linear probability model of the likelihood of CEO and CFO turnover for the sub-sample of firms with free float smaller than \$150 million.

TABLES 2 AND 3 HERE

Model 1 reports the results for the linear probability models regressing CEO change on *Post-Sarbanes-Oxley* and *Exempt from Section 404*. In model 2, we add the interaction of *Post-Sarbanes-Oxley X Exempt from Section 404*. The coefficient for *Post-Sarbanes-Oxley*, representing the change in likelihood that non-exempt firms replace their CEO following a

restatement after Sarbanes-Oxley, is negative and significant, indicating firms subject to Sarbanes-Oxley's most stringent provisions were 26% *less* likely to replace their CEOs following a restatement announcement than before the legislation was enacted. The interaction term is positive and significant, indicating that exempt firms were 45% *more* likely to replace their CEOs following a restatement announcement than before Sarbanes-Oxley. In raw terms, before Sarbanes-Oxley, one of six CEOs at restating firms below the \$75M threshold was replaced (17%), as were six of 27 CEOs at firms above it (22%); after Sarbanes-Oxley, eight of 17 (47%) CEOs at exempt firms were replaced after restating, while only eight of 34 CEOs at non-exempt firms (24%) were dismissed. These results provide support for H1.

Models 3-4 replicate the analysis for CFOs. The results demonstrate that the likelihood of CFO replacement did not change as a function of the legislation for either exempt or non-exempt firms. This analysis fails to provide support for H2.

Robustness Checks. Our identification strategy is centered on the assumption that when Sarbanes-Oxley passed, exempt and non-exempt firms were equally likely to replace their CEO in response to announcing restatements. To address the concern that our choice of comparison group influences this result, we conducted a falsification test, re-running our models using additional arbitrarily defined comparison groups, to confirm that the results do not hold when we use thresholds different from \$75 million.

We reran all models to test how the significance of the primary independent variable changed as a function of two additional counterfactual threshold levels for exemption from Section 404 (\$60 million and \$90 million), and eight different threshold levels for inclusion in the comparison group, ranging from \$150 million to \$500 million in increments of \$50 million. We then chart how the z-score of the coefficient of *Sarbanes-Oxley X Exempt from Section 404*

varies for each of these permutations. The results are plotted in Figure 3. The lines represent how the z-score varies for different comparison groups and the real (\$75 million/solid line) and counterfactual (\$60 million/dotted line, and \$90 million/dashed line) thresholds.

FIGURE 2 HERE

If our effect were spurious, the \$75 million public float threshold would not be inherently meaningful and we would expect other thresholds to explain CEO departure. As the figure shows, however, \$75 million is the only consistently meaningful threshold, and our key independent variable remains significant for all comparisons, while no other public float thresholds reflects z-scores of 1.64 or higher. This strongly suggests that \$75 million is a meaningful threshold, supporting our assertion that exemption from Section 404 drives CEO replacement, rather than other omitted variables that correlate with public float. This provides further evidence that the \$75 million public float threshold, though arbitrary, became a meaningful threshold separating firm behavior in response to earnings restatements.

We also assume parallel trends between firms smaller than \$75 million public float and firms \$75-\$150 million with respect to CEO replacement. If exempt firms become more likely than firms just above the threshold for exemption to replace their CEO between 1996 and 2006, our identification of the effect of Sarbanes-Oxley may be an artifact of violating the parallel trends assumption of a difference-in-difference model. To address this, we test for a difference in the likelihood of CEO replacement before Sarbanes-Oxley between the two groups and find none ($t=1.56$).

We also compare firm characteristics and the likelihood of CEO replacement before and after Sarbanes-Oxley and find no systematic differences on a variety of metrics. Table 4 reports sample mean and t-statistic tests for equality of means between the firms that announced their

restatements before and after the passage of Sarbanes-Oxley. The two groups are statistically identical on all but one dimension that might predict both restatement and CEO replacement: more articles are written about the restatement after Sarbanes-Oxley than before ($p=.07$). No systematic difference is found for size ($p=0.42$), number of directors ($p=.71$), number of employees ($p=.31$), or replacement of the CFO ($p=.52$) or auditor ($p=.85$). Nevertheless, their treatment of top managers varies systematically before and after the legislation.

TABLES 4 AND 5 HERE

Third, we collected data on CEO replacement for non-restating firms. We found no significant differences in the likelihood of CEO replacement between firms exempt from Section 404 and firms just above the exemption threshold. The results are reported in Table 5.

Another concern is whether there is a difference between firms with a public float just above and just below the cutoff of \$75 million. Public float can be manipulated in ways that other measures of size cannot (Gao, 2016; Gao, Wu, & Zimmerman, 2009). We therefore constrained our sample to firms with public floats between \$50 million and \$125 million, yielding a sample of 50 firms, 19 with public floats of \$50-\$75 million, and 31 with public floats of \$75-\$125 million. Firms manipulating public float are more likely to be in this restricted sample than in our primary one. Even using this smaller sample, our results support H1 but not H2; results are reported in Table 6.

TABLE 6 HERE

DISCUSSION

We investigate the effect of legislative certification on executive turnover after organizational misconduct. We find a significant change in how firms respond to revelations of their misconduct after the enactment of Sarbanes-Oxley, such that firms *exempt* from key

provisions of the act become 45% *more* likely to dismiss their CEOs after announcing a restatement than they were before the legislation, while firms that become subject to the strictest provisions of Sarbanes-Oxley become 26% *less* likely to dismiss the CEO in the same situation. We find no similar difference across groups with respect to CFO dismissal. Our analysis suggests the certification effect of the legislation confers legitimacy on firms that are required to comply with it but leaves exempt firms without such cover. These results suggest that Sarbanes-Oxley, ironically, released some CEOs from having to take more accountability for wrongdoing.

Our results provide evidence for the certification effects of legislation. The literature on certification has focused on the legitimacy benefits gained by meeting a professional standard or performance threshold (Anderson et al., 1999; Gray et al., 2015; Rao, 1994). We broaden these findings by showing how the mere enactment of new legislation certifies firms required to comply with its stricter regulatory requirements. Indeed, these firms can be declared fit *despite* evidence they had *not* complied with the new legislative standards. In contrast, exemption from new regulatory requirements, while intended to reduce burdens on small firms, eliminates the certification effect and appears to lead them to take more substantial remedial action. Our results suggest that exempt firms felt compelled to substitute (Okhmatovskiy & David, 2012) for the absence of certification by enacting costly symbolic management strategies after misconduct.

Consistent with existing research, our results suggest that replacing the CEO following misconduct is a means to restore firm legitimacy (Arthaud-Day et al., 2006; Gangloff et al., 2014; Pozner & Harris, 2016). Firms exempt from Sarbanes-Oxley's most stringent requirements appear to select CEO replacement over CFO replacement as a remedial strategy, even though the CFO has a more direct and substantive role in verifying a firm's financial records than the CEO. It also underscores the importance for exempt firms to send strong signals to external audiences

that they will address misconduct, even if replacing the CFO might have done this more directly (Geiger & North, 2006).

Though we found that firms beholden to Sarbanes-Oxley's strictest provisions became less likely to replace their CEOs after announcing a restatement, it would be inaccurate to conclude that Sarbanes-Oxley had a universally negative effect on CEO accountability. In other work using a broader set of firms, we find that Sarbanes-Oxley increased the likelihood of CEO replacement after a restatement, amplified by increased media coverage of corporate misconduct in the post-Sarbanes-Oxley world (Pozner et al., 2019). Relatedly, Gomulya and Boeker (2016) found that Sarbanes-Oxley decreased the extent to which a CEO could count on close ties to board members as protection from dismissal in the aftermath of misconduct. Burks (2010), however, found that Sarbanes-Oxley shifted the disciplinary measures imposed on CEOs for misconduct away from dismissal towards bonus penalties, a change attributed to restatements becoming less severe after the legislation. These conflicting findings indicate that Sarbanes-Oxley had differential effects on CEO accountability depending, at minimum, on firm size, restatement severity, and media attention, as well as whether the firm is beholden to all of Sarbanes-Oxley's legislative requirements. True main effects are always rare; the effect of Sarbanes-Oxley on CEO accountability for misconduct is no exception.

A thought-provoking incidental finding was that non-exempt firms that replace their CEOs after misconduct do so more quickly, though at a lower rate, than exempt firms. Questions about how quickly firms engage in remedial action after misconduct are interesting, as are questions about whether that speed matters to stakeholder evaluations (Pfarrer et al., 2008). Our data, however, are not well-suited to addressing such questions. Future research would benefit

from exploring the predictors and consequences of different temporal choices for remedial action following firm misconduct, using a sample more appropriate to those questions.

Our findings also have implications for theory on organizational stigmatization (Devers et al., 2009; Pozner, 2008). While much research has explored what contributes to firm-level stigmatization as well as how firms react to potentially stigmatizing events (Gomulya & Boeker, 2016; Semadeni et al., 2008), our study sheds light on why otherwise similar firms may take different actions in light of potentially negative social evaluations. We show that institutional forces – the certification effect of new legislation – lowers stigma for firms beholden to new legislation but raises it for firms exempt from its provisions.

Relevant to this discussion is the fact that the size of a firm's free float is, to some degree, open to manipulation by the company (Gao et al., 2009). Some firms may reduce the magnitude of their free float to avoid having to comply with the demands of Section 404, facilitating shady activity with less scrutiny. This is consistent with the findings summarized in Table 6, which demonstrates that our hypotheses hold with even the most restricted sample. The fact that some may have manipulated free float to avoid regulatory oversight could mean that every firm in that category suffers from some level of stigma-by-association. In this case, accountability pressures from CEOs beholden to the legislation are lessened not only because of the certification effect, but also by "cleaning" the category of certified firms from those firms with a tendency toward misconduct who self-select into the non-certified category. Determining what portion of the effect is driven by which mechanism is an interesting direction for future research.

More generally, our findings bolster our understanding of the effect of legislative change on firm behavior. Although research has demonstrated the coercive impact of legislation on market structures and firm practices, ours is the one of the first studies to explore how legislative

change affects how firms that have already violated the law behave (Pozner et al., 2019, is another). Our results also show clearly that legislation affects the behavior of firms that are outside its circumscribed regulatory reach, as well as the behavior of firms beholden to it—though in ways opposite to the legislation’s intent.

While our results indicate that new legislation elicits certification effects that release CEOs from experiencing the same burden of accountability as those who are exempt from the legislation’s requirements, we do not want to suggest in any way that Sarbanes-Oxley was ineffective, nor are we claiming that legislative efforts to control misconduct are destined to backfire. Indeed, in other work on the effect of legislative change on firm responses to misconduct, we found that Sarbanes-Oxley increased CEO accountability, both directly and by amplifying the role of the media in spotlighting firms’ misdeeds and triggering CEO change (Pozner et al., 2019). Our study has practical implications for firms, particularly those that find themselves on the wrong side of the law, as well as policy makers who want to encourage as many firms as possible to stay on the right side of it. Our results here focus on a small subset of firms to tease out how new legislation may have unintended certification effects that release CEOs from the accountability they ought to shoulder for their firms’ misconduct. Policymakers should be sensitive to their potential.

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FIGURES AND TABLES

Figure 1a.

Distribution of firms in the full sample, by free float (up to \$60 billion) and market capitalization (up to \$1 trillion)

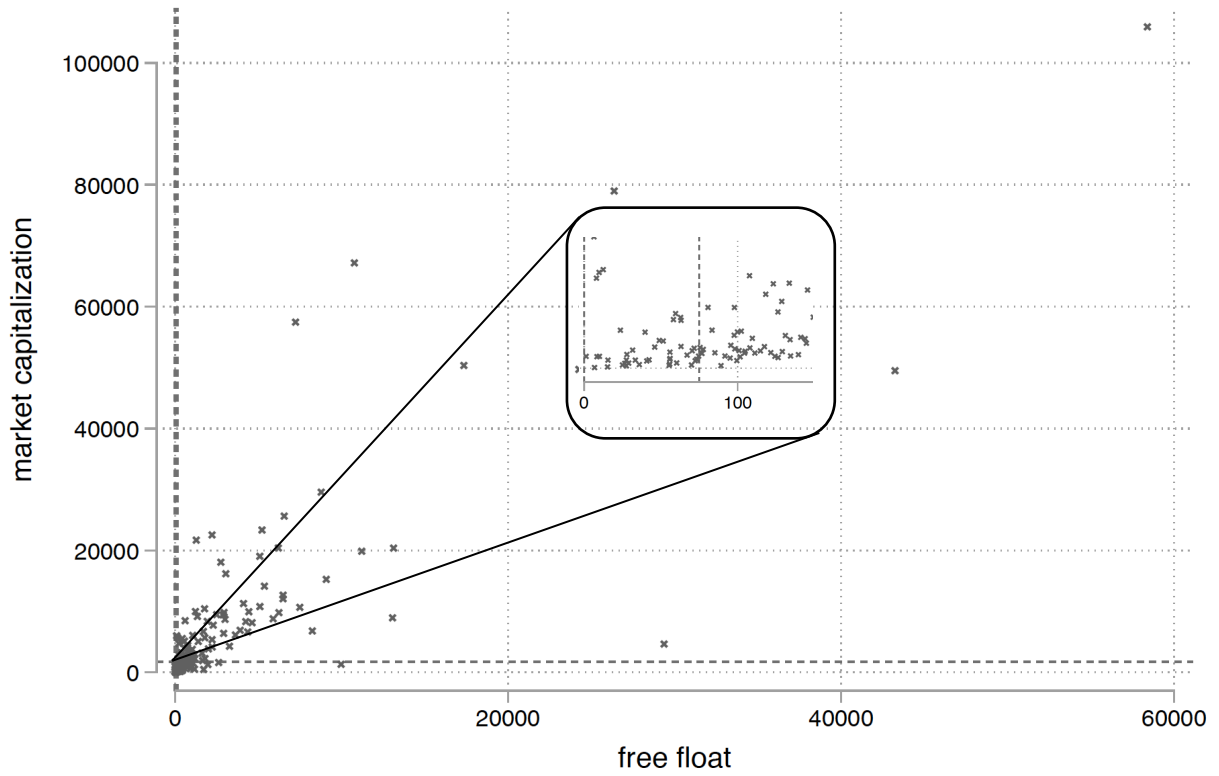


Figure 1b.

Distribution of firms included in the final sample by free float (up to \$500 million) and market capitalization (up to \$6 billion)

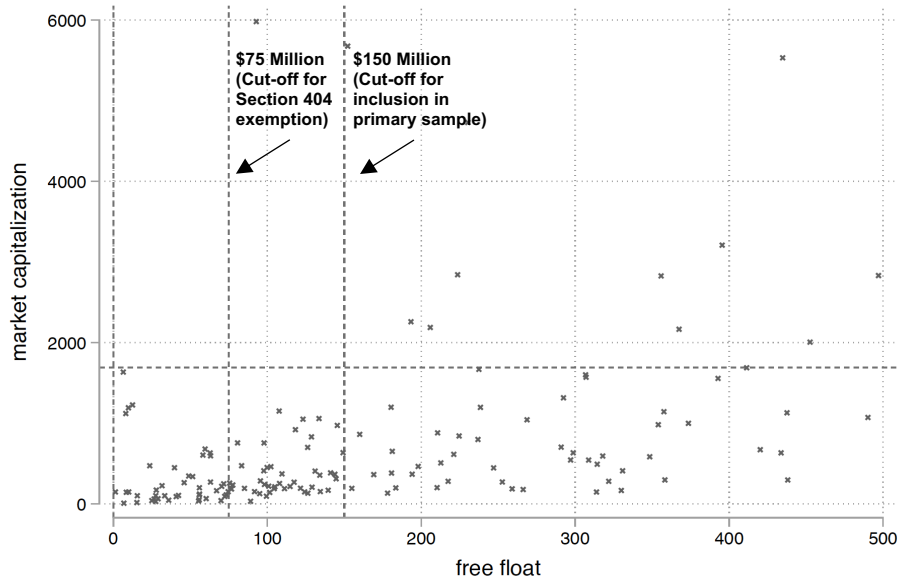


Figure 1c.

Firms excluded (unshaded) and those included (shaded) in the final sample

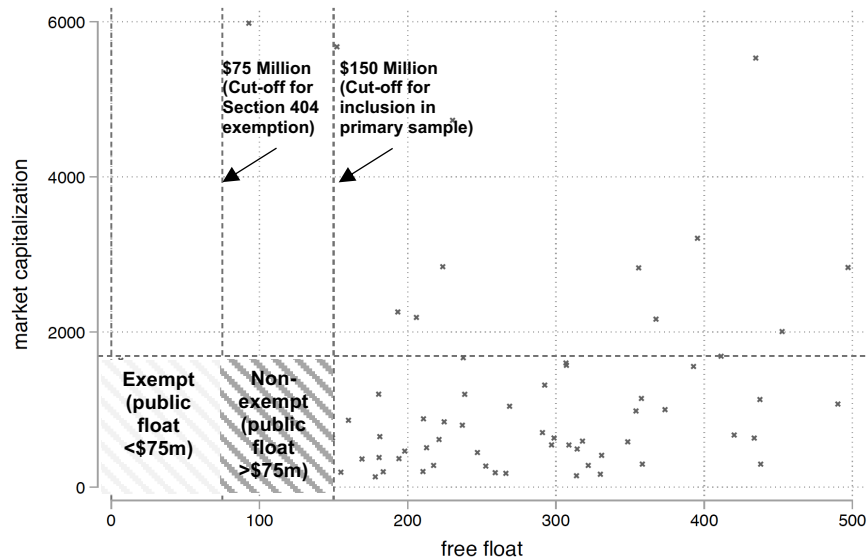


Figure 2.

Statistical significance (z-scores) of the coefficient on the interaction term for Sarbanes-Oxley X Exempt using different thresholds of public float

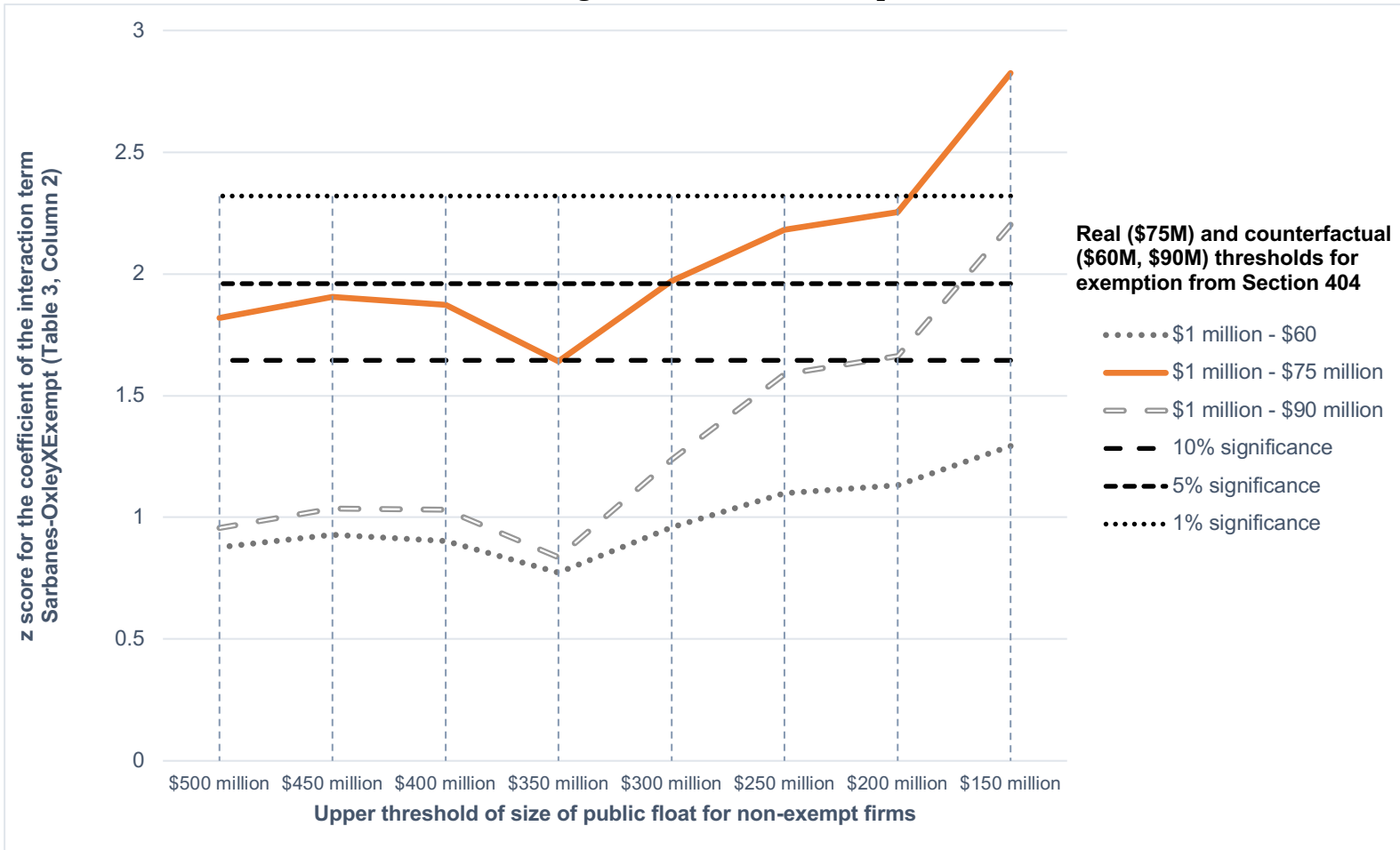


Table 1.

T-tests for equality of means between firms that are exempt from Section 404 (free float < \$75 million) and those that are not exempt (free float between \$75-150 million), N=84

	\$1m-\$75m float		\$75m-\$150m float		p value
	Mean	SD	Mean	SD	
Market capitalization*	265	309	389	295	0.06
Number of employees	2.9	4.0	4.4	7.1	0.21
Number of directors	11.7	4.1	13.2	4.3	0.10
Number of articles about the company one year before the restatement	62	175	130	430	0.34
Number of articles about the restatement	1.33	3.30	1.36	2.48	0.96
Number of analysts following the stock	0.92	0.82	1.10	0.56	0.22
Book to market ratio	0.86	0.60	0.70	0.54	0.19
Net profit margin	-0.15	0.52	-0.08	0.30	0.44
Return on equity	-0.08	0.75	-0.06	0.41	0.86
Return on assets	0.04	0.21	0.07	0.18	0.58
Return on assets (prior year)	0.09	0.21	0.11	0.08	0.60
Return on assets (two years ago)	0.14	0.19	0.11	0.09	0.34
Capital ratio	0.28	0.25	0.29	0.38	0.94
Cash ratio	1.02	1.57	0.65	0.79	0.17
Quick ratio	1.96	1.83	1.49	0.98	0.15
Current ratio	2.56	2.12	2.14	1.15	0.27
R&D to sales ratio	0.04	0.12	0.05	0.10	0.76
Amended quarterly reports	0.56	0.50	0.49	0.51	0.52
Replaced CEO	0.28	0.45	0.31	0.47	0.75
Replaced CFO	0.12	0.32	0.22	0.42	0.19
Replaced auditor	0.19	0.39	0.11	0.32	0.33
Days until CEO replacement	232	103	140	92	0.09
Days until CFO replacement	116	57	142	95	0.67

Table 2.

Descriptive statistics and correlations

		N	Mean	SD	1	2	3	4	5	6	7	8	9	10	11
1	CEO change	84	0.29	0.46											
2	CFO change	84	0.18	0.38	0.24**										
3	Exempt from Section 404	84	0.49	0.50	-0.07	-0.15+									
4	Post Sarbanes-Oxley	84	0.59	0.50	-0.04	0.07	0.30*								
5	No. articles 1 year prior to restatement	84	99.5	336.5	0.14	-0.01*	-0.11	0.06							
6	No. articles about the restatement	84	1.38	2.94	0.39**	0.29*	-0.01	0.20	0.11						
7	Market cap 21 days pre-restatement (\$M)	84	319	304	-0.04	-0.04	-0.18	0.10+	0.11	0.15					
8	Three-year average ROA	84	0.09	0.15	0.13	-0.14	-0.01	-0.11	-0.08	0.06	0.19				
9	Restatement increased income	84	0.24	0.43	-0.05	-0.11	-0.05	-0.04	0.03	-0.18	-0.24	-0.17			
10	Number of quarters restated	84	4.78	4.44	0.10	0.03	0.02	0.07	-0.01	0.15	-0.07	0.08	-0.15		
11	Restatement due to fraud	84	0.07	0.26	0.12	-0.01	-0.09	-0.33**	-0.07	-0.08	-0.07	0.09*	-0.04	0.14	
12	Restatement prompted by the firm	84	0.76	0.43	-0.13	-0.03	0.05	-0.13	0.05	-0.12	-0.05	-0.04	-0.02	0.15	0.04

Note. ** p<0.01, * p<0.05, + p<0.10.

Table 3.**Linear probability models predicting CEO change and CFO change after restatement**

	(1) CEO change	(2) CEO change	(3) CFO change	(4) CFO change
Exempt from Section 404	-0.043 (0.085)	-0.321* (0.137)	-0.163* (0.078)	-0.237* (0.098)
Sarbanes-Oxley	-0.058 (0.099)	-0.260* (0.129)	0.061 (0.088)	0.007 (0.135)
Sarbanes-Oxley X Exempt from Section 404		0.453** (0.167)		0.120 (0.153)
Number of articles about the company	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Number of articles about the restatement	0.062*** (0.0134)	0.061*** (0.015)	0.038** (0.013)	0.038** (0.013)
Market capitalization	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Three-year average return on assets	0.364 (0.289)	0.332 (0.255)	-0.377 (0.298)	-0.385 (0.313)
Restatement resulted in increased income	-0.000 (0.116)	-0.009 (0.111)	-0.118 (0.103)	-0.121 (0.103)
Number of quarters restated	0.003 (0.011)	0.008 (0.011)	-0.004 (0.009)	-0.002 (0.009)
Restatement due to fraud	0.216 (0.217)	0.203 (0.225)	0.030 (0.193)	0.027 (0.194)
Restatement prompted by the firm	-0.109 (0.115)	-0.080 (0.113)	0.018 (0.109)	0.025 (0.110)
Constant	0.338* (0.169)	0.381* (0.169)	0.297+ (0.153)	0.309+ (0.158)
Observations	84	84	84	84
R-squared	0.233	0.282	0.163	0.168

Note. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05, + p<0.10.

Table 4.**T-tests for equality of means before and after Sarbanes-Oxley**

	Before SOX		After SOX		<i>p</i> value
	Mean	SD	Mean	SD	
Market capitalization*	297	250	350	340	0.42
Number of employees	2.9	3.9	4.2	6.8	0.31
Number of directors	12.3	4.3	12.6	4.2	0.71
Number of articles about the company one year before the restatement	75	324	111	338	0.62
Number of articles about the restatement	0.67	1.60	1.81	3.46	0.07
Number of analysts following the stock	1.00	0.58	1.02	0.78	0.86
Book to market ratio	0.88	0.66	0.70	0.49	0.15
Net profit margin	-0.11	0.31	-0.11	0.49	0.93
Return on equity	-0.01	0.71	-0.11	0.53	0.46
Return on assets	0.09	0.08	0.03	0.24	0.11
Return on assets (prior year)	0.13	0.07	0.09	0.19	0.27
Return on assets (two years ago)	0.12	0.09	0.13	0.18	0.89
Capital ratio	0.31	0.28	0.27	0.34	0.57
Cash ratio	0.86	1.49	0.81	1.05	0.87
Quick ratio	1.88	1.70	1.61	1.28	0.42
Current ratio	2.61	1.85	2.16	1.56	0.24
R&D to sales ratio	0.06	0.09	0.04	0.12	0.38
Amended quarterly reports	0.44	0.50	0.58	0.50	0.23
Replaced CEO	0.31	0.47	0.29	0.46	0.86
Replaced CFO	0.14	0.35	0.19	0.40	0.52
Replaced auditor	0.14	0.35	0.15	0.36	0.85
Days until CEO replacement	148	98	225	105	0.16
Days until CFO replacement	179	81	110	81	0.21

Table 5.

Replication of the full model (Table 3, model 2) using non-restating firms

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	Full sample	Full sample	\$1m-\$150m	\$1m-\$150m	\$1m-\$150m
	CEO change	CEO change	CEO change	public float	public float	public float
				CEO change	CEO change	CEO change
Exempt from Section 404		-0.0121 (0.0125)	-0.026+ (0.013)		-0.047 (0.029)	-0.056+ (0.033)
Sarbanes-Oxley		0.00432 (0.0104)	-0.004 (0.013)		0.006 (0.015)	-0.008 (0.022)
Sarbanes-Oxley X Exempt from Section 404			0.029 (0.022)			0.029 (0.025)
Market capitalization		0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Public float	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Three-year average ROA		0.018 (0.0247)	0.017 (0.025)		0.039 (0.058)	0.039 (0.058)
Constant	0.0273*** (0.006)	0.032* (0.019)	0.036** (0.013)	0.029* (0.015)	0.088+ (0.048)	0.088+ (0.048)
Observations	1,211	1,093	1,093	551	509	509
R-squared	0.002	0.003	0.004	0.000	0.008	0.010

Robust standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table 6.**Replication of the full model (Table 3, model 2) using firms with public floats of \$50 - \$125 million**

VARIABLES	(1) CEO change	(2) CEO change	(3) CFO change	(4) CFO change
Exempt from Section 404	0.024 (0.134)	-0.297+ (0.152)	-0.096 (0.106)	-0.169 (0.113)
Sarbanes-Oxley	-0.035 (0.133)	-0.250 (0.151)	0.034 (0.133)	-0.015 (0.171)
Sarbanes-Oxley X Exempt from Section 404		0.579* (0.220)		0.131 (0.212)
Number of articles about the company	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Number of articles about the restatement	0.082* (0.035)	0.080* (0.035)	0.032 (0.0336)	0.032 (0.034)
Market capitalization	0.0001*** (0.000)	0.0001*** (0.000)	0.0001** (0.000)	0.0001** (0.000)
Three-year average return on assets	0.151 (0.241)	0.173 (0.184)	-0.255 (0.289)	-0.250 (0.309)
Restatement resulted in increased income	-0.010 (0.117)	-0.049 (0.121)	0.018 (0.147)	0.009 (0.146)
Number of quarters restated	0.0112 (0.018)	0.018 (0.018)	0.011 (0.016)	0.013 (0.017)
Restatement due to fraud	0.286 (0.274)	0.245 (0.245)	0.064 (0.202)	0.055 (0.206)
Restatement prompted by firm	-0.108 (0.163)	-0.099 (0.153)	-0.181 (0.187)	-0.179 (0.188)
Constant	0.099 (0.212)	0.174 (0.210)	0.245 (0.217)	0.262 (0.223)
Observations	50	50	50	50
R-squared	0.363	0.453	0.203	0.208

Robust standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1