The Effect of Financial Regulation on Nonfinancial Violations

Vincent Giese
University of Mannheim

Jonas Heese
Harvard Business School

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Abstract

This paper examines the effect of financial regulation on nonfinancial violations. Using differences in compliance requirements with Sarbanes–Oxley Act (SOX) Section 404, we find that adoption of Section 404 increased firms' propensity for nonfinancial violations. This effect is stronger for firms with greater external scrutiny toward their financial reporting, greater challenges in monitoring their operations, and limited resources. These results, together with an examination of changes in audit fees, conference call transcripts, and 10-K disclosures, suggest that the effects primarily stem from a shift in attention and resources toward SOX 404. Further, the effects are concentrated in employee-related violations and persist for approximately two years. Overall, our results suggest that financial reporting regulation can result in unintended consequences harming stakeholders, such as employees.

Keywords: corporate misconduct; SOX 404; compliance; financial regulation.

JEL Classifications: M40, M41

1. Introduction

The last two decades have seen an increasing focus on shareholder protection through strengthened financial regulation, most notably the Sarbanes-Oxley Act of 2002 (SOX). SOX has been described as the most far-reaching reform of American business since the 1934 Securities Exchange Act that ultimately aimed to improve firms' financial reporting and, more broadly, compliance with financial regulation.[1] At the same time, companies are also expected to comply with *nonfinancial* regulations designed to protect various other stakeholders, including customers (e.g., food safety), employees (e.g., workplace safety), or the environment (e.g., fuel economy regulations), among others (Heese 2024).[2] However, observers have long voiced concerns that SOX tied up substantial resources and management attention (Cohen et al. 2013), raising the question of whether this reform had unintended consequences for compliance with *nonfinancial* regulations (Coates and Srinivasan 2014). This paper addresses this question by examining the effect of SOX Section 404 adoption on firms' nonfinancial violations.

The introduction of SOX Section 404 is a particularly useful laboratory to study our research question for four main reasons. First, although primarily targeted at internal controls over financial reporting, SOX 404 plausibly affects the broader internal control environment (e.g., Imdieke et al. 2023; Schroeder and Shepardson 2016), underscoring the importance of analyzing consequences beyond financial compliance. Second, adoption of SOX Section 404 tied up substantial financial resources and management time, as its disclosure and auditing requirements were assessed to be the costliest part of SOX (e.g., Ge et al. 2017). Third, the rollout of SOX 404 is akin to a quasi-natural experiment, as firms below a certain size threshold did not have to comply with SOX 404 when it first became effective. This feature enables us to identify the effect of SOX 404 on firms' nonfinancial violations using a difference-in-

[1] Consistent with that objective, several studies document that firms improved their financial reporting following SOX (e.g., Bartov and Cohen 2009; Iliev 2010).

[2] In contrast, financial violations refer to violations of laws primarily protecting investors, such as securities laws (Heese 2024).

differences design. Fourth, Section 404 was implemented separately from the main body of the legislation, allowing us to separate the effect of SOX 404 from other SOX requirements, e.g., board independence rules.

Ex ante, the effect of SOX 404 on nonfinancial violations is unclear. On the one hand, adoption of SOX 404 may result in more nonfinancial violations for at least two reasons. First, SOX 404 tied up substantial financial resources and managerial time, with surveys of more than 200 public firms suggesting average expenditures of approximately $4 million for SOX 404 implementation in 2005 (FEI 2005). In particular, companies redesigned their internal controls or invested in information technology, among other changes (e.g., Alexander et al. 2013; Masli et al. 2010). Even after these initial investments, companies in the FEI survey reported to incur annual SOX compliance costs of approximately $2 million in the mid-2000s (FEI 2008).[3] Next to these direct expenditures, SOX 404 also required management to spend considerable time on SOX 404 compliance. In fact, executives have long complained that complying with SOX diverted their attention from running the business (e.g., Solomon and Bryan-Low 2004). The chief accounting officer of General Motors, for example, highlighted that "the real cost isn't the incremental dollars, it is having people that should be focused on the business focused instead on complying with […] the rules" (Solomon and Bryan-Low 2004).[4] The focus on SOX 404 might have taken attention away from compliance with other laws and regulations (Cohen et al. 2013; Leuz and Wysocki 2016), resulting in more nonfinancial violations.

Second, the passage of SOX was the response to a series of high-profile accounting scandals in the early 2000s. As a result, firms' accounting and reporting choices became subject to more scrutiny by regulators, the media, audit firms, investors, and, more broadly, the public at large. For instance, SOX allowed the SEC to impose larger penalties for accounting violations

[3] The public firms in the FEI surveys reported average sales of $5 billion, and are larger than our treatment firms, which reported average sales of $1.8 billion. Thus, the cost estimates from the FEI surveys may not be directly applicable to our sample firms, particularly non-accelerated filers or firms that adopted SOX 404 more recently.
[4] This anecdote is consistent with the concepts of bounded rationality (Simon 1955) and limited attention (Merton 1987), whereby managers focus their attention on the most pressing tasks.

on companies. Large firms, in particular, were subject to additional scrutiny. For example, in 2002, the SEC required that CEOs and CFOs of the largest publicly listed companies state under oath that the firm's financial reports are materially accurate pursuant to the Securities Exchange Act of 1934 (e.g., Chang et al. 2006; Heese et al. 2023). As a result, companies, particularly those subject to external scrutiny, had incentives to ensure careful adoption of SOX. Under this view, we would expect that adoption of SOX 404 induced firms to prioritize financial compliance, reducing resources and attention for compliance with nonfinancial regulation and ultimately leading to increases in firms' nonfinancial violations.

On the other hand, adoption of SOX 404 may also improve compliance with nonfinancial regulations as it strengthens firms' overall internal controls, decreasing nonfinancial violations (e.g., Alexander et al. 2013; Ashbaugh-Skaife et al. 2008; Down et al. 2023). In fact, the Committee of Sponsoring Organizations of the Treadway Commission (COSO 2013) emphasized in its revised framework the importance of controls over operations and compliance (in addition to financial reporting controls) as parts of internal controls. The COSO Chairman David Landsittel explained that "some people because of the implementation of our framework under SOX 404 and SOX think of it as a financial reporting framework that really relates to published financial statements. But it's broader than that. We want to have the reader recognize more vividly the relevance and opportunities to adopt the framework as it relates to operations and compliance" (Tysiac 2012). Indeed, a few studies suggest that stronger internal controls over financial reporting are associated with a better internal information environment, which may also affect nonfinancial outcomes (e.g., Feng et al. 2009; Schroeder and Shepardson 2016). Feng et al. (2015), for example, find a positive association between internal control quality and the efficacy of firms' inventory management. Cheng et al. (2013) show that weak internal controls are negatively related to investment efficiency. Focusing on FDA inspection outcomes, Down et al. (2023) show that more scrutiny of financial reporting controls positively affects operating control outcomes. Imdieke et al. (2023) test the impact of

an internal control audit mandate and find a positive relationship with operational efficiency. Practitioners also point towards broader benefits of SOX 404. For example, Wagner and Dittmar (2006) describe how SOX 404 helped firms to standardize their processes, which led to fewer errors in tasks such as manufacturing, order fulfillment, payroll, or human resources. Under this view, we would expect that SOX 404 adoption decreases nonfinancial violations.

Finally, SOX 404 adoption may have no effect on nonfinancial violations as firms might have adequate resources to ensure compliance with various regulations. In addition, regulatory oversight and the deterrence effect of looming penalties for nonfinancial misconduct might ensure that firms comply with nonfinancial regulations. Ultimately, the effect of SOX 404 on nonfinancial violations is an empirical question that we take to the data in this study.

To answer this question, we use a stacked difference-in-differences design, exploiting that firms with a public float below $75 million were initially not required to comply with Section 404.[5] Similar to prior studies (e.g., Ge et al. 2017; Schroeder and Shepardson 2016), we identify treated firms based on their SOX 404 adoption in 2004 and 2005. In our main analysis, we construct cohorts of treated and control firms for a six-year window around the adoption of Section 404 using either the universe of firms with available data (full sample) or those with a public float below $150 million (size-restricted sample).[6] To identify nonfinancial violations, we use data from Good Jobs First's Violation Tracker database that covers violations and penalties issued by over 400 federal, state, and local regulatory agencies. Our full sample includes 10,371 nonfinancial violations, corresponding to $14.8bn in penalties. In a given year, the average firm in our full (size-restricted) sample has an 8.7% (2.6%) propensity of a nonfinancial violation. We control for a set of firm-level determinants of nonfinancial violations identified in prior literature along with cohort-specific firm and year fixed effects.

[5] Public float is defined as the aggregate market value of the issuer's outstanding voting and nonvoting common equity held by nonaffiliates of the company (Securities and Exchange Act Rule 12b-2).
[6] Thus, our sample ends in 2007, which ensures that we hold attestation requirements constant. These requirements changed with the PCAOB's adoption of AS5 in 2007 (e.g., Schroeder and Shepardson 2016).

Our results suggest that adoption of SOX Section 404 increased the propensity of nonfinancial violations. Specifically, we find an increase of 1.8 (1.3) percentage points, or 21% (50%) of the sample mean, for firms adopting SOX 404 compared to those that did not, using the full (size-restricted) sample.[7] Next, we show that our results are primarily driven by employee-related violations, which represent approximately 76% of all nonfinancial violations in our sample.[8] For employee-related violations, we find an increase of 1.6 (1.1) percentage points, implying $29,240 ($3,001) in additional penalties and 0.07 (0.04) additional violations per firm year, or $71.2 million ($2.4 million) and 171 (25) violations annually across all treatment firms in the full (size-restricted) sample. Consistent with this, we also show that our main effect is driven by violations with relatively small penalty amounts, suggesting that SOX 404 adoption primarily reduced the monitoring of nonfinancial violations with smaller penalties, such as employee-related violations. In the interpretation of these results, it is important to note that this effect is based only on *detected* violations and that nonfinancial violations are associated with other costs besides regulatory penalties, including increased injury rates for employees.[9] We test these additional implications more explicitly by examining OSHA-reported workplace injuries. We find increases in workplace injuries of approximately 13.5% (11.9%) for the full (size-restricted) sample after SOX 404 adoption. These results suggest that SOX 404 adoption had unintended consequences for other stakeholders, particularly employees.

[7] This increase translates into 0.08 (0.04) violations and $113,230 ($27,018) in penalties per year for the average firm or 193 (31) violations and $275.7 million ($19.4 million) in annual penalties across all 2,435 (717) firms that adopted SOX 404 using the full (size-restricted) sample. The penalty estimates of $113,230 ($27,018) are based on a 0.018 (0.013) effect and conditional mean penalty of $6.3 million ($2.1 million) in the full (size-restricted) sample. The corresponding increase in violations is estimated at 0.08 (0.04) per firm year, using conditional means of 4.4 (3.3) for the full (size-restricted) sample.

[8] Given their prevalence, it is perhaps not surprising that our effects are primarily driven by the category with the greatest statistical power. While these results do not necessarily imply that the effect is unique to employee-related violations, we cannot rule out that other types of nonfinancial violations are less affected or that our setting is less suited to detect such effects. For instance, we find some significant effects in competitor-related violations.

[9] Workplace safety violations, for example, can also create costs related to litigation, wage demands, employee morale, exclusion from government contracts, and employee turnover (Caskey and Ozel 2017; Hope et al. 2022; Viscusi and O'Connor 1984). In the context of financial misconduct, Karpoff et al. (2008) estimate that these additional costs are over 7.5 times the sum of all penalties imposed through the legal and regulatory system.

As discussed, our main argument is that SOX 404 adoption ties managerial attention and resources, resulting in more nonfinancial violations. To examine this argument further, we conduct two sets of non-mutually exclusive cross-sectional tests that capture variation in managerial attention and firm resources. We expect SOX 404 to result in a higher propensity for nonfinancial violations (1) if firms face heightened external scrutiny of their financial reporting or (2) if firms are subject to more internal monitoring frictions and resource constraints. Our subsequent cross-sectional analyses explore the effects of these factors.

We first examine the role of external scrutiny regarding firms' compliance with SOX 404. Heightened external scrutiny may increase nonfinancial violations more strongly as management is particularly focused on SOX 404 compliance. To measure heightened external scrutiny, we use two proxies: firms with capital issuance and higher leverage. We find that our results are stronger among firms subject to higher external scrutiny.

Second, we examine whether firms with frictions related to internal monitoring and resource constraints are more prone to nonfinancial violations. To proxy for frictions in internal monitoring, we follow Bushman et al. (2004) and use the dispersion of segment sales to capture organizational complexity, which can limit management's ability to monitor local managers. We also use a bag-of-words measure of corporate integrity, similar to Li et al. (2021), to capture firms' integrity culture. Firms with monitoring frictions must invest relatively more to remedy control-related issues after SOX 404 adoption, and we thus expect a stronger effect on nonfinancial violations. We find that the effect is concentrated in firms with more dispersed segment sales and firms with low integrity culture. To proxy for resource constraints, we identify firms with low liquidity and find that the propensity of nonfinancial violations increases significantly more for firms with low liquidity after SOX 404 adoption, in line with the view that firms with limited resources must set priorities in their response to new regulations.

A limitation of these tests is that they only indirectly capture managerial attention. To examine it more directly, we also exploit changes in audit fees, earnings calls, and firms' 10-K

filings around SOX 404. The increase in audit fees following SOX 404 adoption reflects auditors'—and thus managers'—focus on SOX 404. We find that our results are concentrated in firms with larger increases in audit fees. Relatedly, the relative share of a topic in earnings calls captures managers' attention, as it reflects the priorities that managers set at a given point in time (e.g., Hassan et al. 2019). Following SOX 404, managers of treated firms talk significantly more about SOX and internal control topics during earnings calls. Similarly, 10-Ks include significantly more references to these topics after SOX 404 adoption. Moreover, the increase in nonfinancial-violation propensity is driven by firms that mention SOX-related topics more frequently in their earnings calls and 10-Ks. These results suggest that limited managerial attention is one explanation for the increase in nonfinancial violations following SOX 404.

A question that arises from our results is how long these effects persist and whether our findings generalize beyond the initial SOX 404 adopters. To address this, we conduct three tests. First, we show that our effects persist for approximately two years after the adoption of SOX 404, consistent with a temporary compliance shock, where firms initially face resource constraints or shifting priorities but subsequently adjust. This interpretation aligns with prior literature documenting short-term compliance burdens following SOX (e.g., Alexander et al. 2013). Second, we examine changes in the propensity of nonfinancial violations around firms' remediation of internal control weaknesses (ICWs). We find similar increases in the nonfinancial-violation propensity during the remediation phase of a material ICW. Third, we examine changes in the propensity of nonfinancial violations after SOX 404 adoption in the years 2004 to 2022. Again, we find an increase in the propensity of nonfinancial violations after SOX 404 adoption. These tests suggest that while the effects appear to be relatively short-lived, our findings generalize beyond the subset of firms that initially adopted SOX 404.

We also address alternative explanations for our findings through two sets of tests. First, following SOX 404 adoption, we find no change in inspection likelihood by the Occupational Safety and Health Administration (OSHA), which is the regulator enforcing compliance with

employee-related rules, alleviating the concern that changes in regulatory scrutiny explain our results. Second, we find no change in inspections triggered by accidents after SOX 404 adoption and obtain consistent results when excluding firms with accidents after SOX 404 adoption, alleviating the concern that SOX 404 adoption improves injury reporting, leading to more OSHA inspections and detected violations. Our results are also robust to alternative sampling choices, treatment definitions, dependent variables, fixed effects, and research designs.

We contribute to the literature in several ways. First, we contribute to the literature on the effects of SOX. This literature has documented the benefits (e.g., Bartov and Cohen 2009; Daniel et al. 2008) and costs of SOX (e.g., Coates 2007; Iliev 2010), as well as unintended consequences of SOX related to firms' stock listings (e.g., Leuz 2007; Leuz et al. 2008; Piotroski and Srinivasan 2008), the labor market for directors (Linck et al. 2009), and audit market concentration (DeFond and Lennox 2011). However, there is more to be learned about SOX's unintended consequences for firms' decisions, with a few studies showing effects on corporate investment (e.g., Kang et al. 2010) and risk-taking (Bargeron et al. 2010). Our study examines whether SOX 404 adoption increased nonfinancial violations across a comprehensive set of violation types. We find that SOX 404 leads to more *employee-related* violations using either all treated firms or only those with a public float of less than $150 million. Thus, our study provides a more complete understanding of SOX's unintended consequences.

Second, our study contributes to the literature on internal controls over financial reporting (e.g., Schroeder and Shepardson 2016). While several studies in this literature find a positive association between internal control quality and operational outcomes, such as inventory management (Feng et al. 2015), investment efficiency (Cheng et al. 2013), operational efficiency (Imdieke et al. 2023), FDA deficiencies (Down et al. 2023), or workplace injuries (Hope et al. 2022), our findings suggest that regulation shifting management focus to internal controls over financial reporting can also result in negative operational outcomes, such

as more violations. These effects are concentrated in employee-related violations and persist for approximately two years.

Third, our study also contributes to the literature on corporate misconduct. This literature has primarily focused on whether and how financial regulation improves firms' financial reporting and can therefore prevent future accounting scandals (e.g., Coates and Srinivasan 2014; Hail et al. 2018), but has rarely examined the effects of financial regulation on compliance with other regulations. While Hail et al. (2018) show that corporate scandals trigger regulation, which appears to be ineffective in preventing future scandals, we find that financial regulation can affect firms' compliance with *nonfinancial* regulations, contributing to a more complete cost-benefit analysis of financial regulation (Leuz and Wysocki 2016).[10]

Finally, our study also has implications for policymakers, regulators, firms, and employees. For policymakers, our findings suggest an intricate tradeoff between different policy goals, i.e., investor protection versus protection of other stakeholders. Our comprehensive analysis of nonfinancial violations reveals that SOX 404 can have unintended consequences for employees, as firms' compliance with employee-related laws decreases and workplace injuries increase. These findings help policymakers understand that employees can face negative externalities resulting from SOX 404. For regulators, our study shows that regulation outside of the regulator's scope can affect firms' compliance with regulation under the purview of a regulator.[11] For firms, our results suggest that the financial implications of penalties arising from employee-related violations may be financially immaterial. However, these violations can lead to other costs, including litigation expenses, increased wage demands, diminished employee morale, exclusion from government contracts, and higher employee

[10] Note that our study does not provide a comprehensive cost-benefit analysis of SOX 404 adoption.
[11] For example, a recent cost-benefit analysis by the European Financial Reporting Advisory Group estimated compliance costs of €600k (€320k thereof recurring) for the initial adoption of the European Sustainability Reporting Standards (EFRAG 2022). Our results could be suggestive of indirect costs of such regulation.

turnover—additional costs not captured in our study. In addition, we also find a substantial increase in the likelihood of workplace injuries, a material cost directly borne by employees.

2. Background and Related Literature

2.1. Financial Regulation

Under strict assumptions, the standard unraveling result suggests that regulation is unnecessary, and firms will optimally disclose (Grossman and Hart 1980). In practice, however, these assumptions are typically not fulfilled, and disclosure comes with considerable costs. Absent regulation, firms trade off costs and benefits in choosing their level of transparency (Beyer et al. 2010). One motivation for financial regulation is to ensure management conveys credible information to the market despite incentive conflicts with shareholders. The disclosure requirements under SOX Section 404 fall into this category and significantly altered the disclosure regime for U.S. companies (Leuz and Wysocki 2016).

Regulators often respond to corporate scandals with regulation (Hail et al. 2018). SOX, for example, was passed in 2002 following a series of scandals, most notably Enron in 2001. In such cases, financial regulation serves as a tool to address corporate reporting problems exposed by the scandal. The efficacy of regulation in addressing these problems is of particular interest to policymakers and regulators. As a result, the direct costs and benefits of financial regulation have been studied extensively (e.g., Christensen, Hail et al. 2016; La Porta et al. 2006).

A growing literature also studies the indirect effects of financial regulation. For example, it can benefit peer firms' financing (Shroff et al. 2017), investment (Ferracuti and Stubben 2019), and exports (Glaeser and Omartian 2022). Financial regulation can also crowd out unregulated disclosure (Breuer et al. 2022), reduce information production by information intermediaries (Breuer et al. 2018), and curb innovation (Breuer et al. 2025). Finally, and most closely related to our research question, regulation can shift misconduct from the newly regulated sphere into another, potentially becoming an antecedent to future scandals (Hail et al.

2018). In sum, while financial regulation is frequently used, conducting a thorough cost-benefit analysis remains difficult, and evidence on indirect effects is still scarce.

2.2. SOX and Section 404

SOX imposed additional governance and disclosure mandates on listed U.S. firms. It did not, however, directly mandate changes in control systems, but required more *transparency* about ICWs.[12] For accelerated filers, Section 404 required the attestation by an auditor and the inclusion of the management report on internal controls in the firms' 10-K. Section 404 does not render the existence of ICWs illegal, as long as firms report them. However, the expectation was that market pressure and litigation risk would reduce ICWs (Coates and Srinivasan 2014).

From 2007 onwards, nonaccelerated filers had to comply with the management report section, while the audit requirement was postponed several times until the Dodd-Frank Act made this exemption permanent. SEC release 33-8238 particularly points to Section 404 as an instrument intended to facilitate early internal detection of fraudulent reporting and thereby deter financial fraud and ultimately increase the reliability of financial statements and regain investor confidence. In light of these regulatory expectations, a large body of research has investigated the financial reporting effects of SOX generally, and Section 404 in particular.

Early evidence suggests that firms exhibit a lower tendency towards benchmark beating (Bartov and Cohen 2009) and accrual-based earnings management after SOX (Cohen et al. 2008; Daniel et al. 2008). Underscoring the relevance of transparency about ICWs, Ashbaugh-Skaife et al. (2008) suggest that earnings properties improve after the remediation of ICWs. Also, there is evidence that restatements have become less severe after SOX (Burks 2010).

Related to Section 404, Iliev (2010) shows that firms above the implementation threshold had significantly lower accruals and discretionary accruals in 2004, consistent with

[12] The 1977 Foreign Corrupt Practices Act required public firms to have controls giving "reasonable assurances" that "transactions are recorded as necessary to permit preparation of financial statements in conformity [with GAAP]."

more conservative reporting practices. Arping and Sautner (2013) find that analyst forecast error and dispersion decreased for firms after SOX 404. Schroeder and Shepardson (2016) provide evidence that internal control audits improve the quality of internal controls particularly during the first years after SOX 404 became effective.

2.3. Research Question

The more extensive disclosure requirements under SOX came with significant direct costs, well documented in the literature (Coates and Srinivasan 2014). Most notably, expenses for internal controls testing and reporting, as well as related audit fees, increased substantially (Coates 2007; Iliev 2010).[13] Evidence on indirect effects and potential unintended consequences of SOX regulations, however, remains inconclusive (Coates and Srinivasan 2014). Early studies have largely focused on (potentially) unintended capital-market effects. For example, Engel et al. (2007) find an increased propensity for small firms to go private after SOX. Bova et al. (2014) show a decrease in IPOs after SOX.[14] Leuz et al. (2008) find a rising tendency for firms to deregister and cease SEC reporting after SOX. There is also evidence that fewer firms enter U.S. equity (Piotroski and Srinivasan 2008) and bond markets (Gao 2011), and that foreign firms are more likely to delist from U.S. exchanges after SOX (Marosi and Massoud 2008).

Another stream of literature examines the effects of SOX on firms' investment and reporting practices. While early work suggests that SOX led to a decline in corporate investments (Kang et al. 2010) and risk-taking (Bargeron et al. 2010), these studies were contested by more recent results (Albuquerque and Zhu 2019). Studies also show that the positive financial reporting quality effects of SOX affect firms' remuneration policies (Carter et al. 2009). Overall, however, the literature on the effects of SOX is largely limited to financial

[13] DeFond and Zhang (2014) review the audit-related literature with respect to SOX.
[14] Leuz (2007) and Gao et al. (2013) argue that concurrent trends could have affected the results of some of the early studies investigating capital-market effects of SOX.

reporting and capital-market outcomes. We focus on indirect effects of SOX beyond financial outcomes by analyzing nonfinancial violations. This effect is unclear ex-ante.

Prior studies in the internal control literature show that improved internal controls are associated with a variety of outcomes, including more stable supply chain relationships (Bauer et al. 2018), lower risk premia requested by employees (Choi et al. 2023) and auditors (Hoitash et al. 2008), more precise management guidance (Feng et al. 2009), higher investment efficiency (Cheng et al. 2013), and less resource extraction by management (Ge et al. 2021). More closely related to our study, Hope et al. (2022) show a positive link between the internal information environment and workplace safety. Specifically for FDA inspection outcomes, Down et al. (2023) find that more scrutiny over financial reporting controls positively affects operating control outcomes. In sum, SOX-driven improvements in internal control systems could enhance operational monitoring and reduce the likelihood of nonfinancial violations.

Conversely, however, SOX 404 adoption tied up financial resources and managerial time. Managers often face more stimuli than they can process, forcing them to set priorities (Dessein et al. 2016). In response to SOX 404, companies redesigned internal controls, invested in information technology (Alexander et al. 2013; Masli et al. 2010) and human capital (Choi et al. 2013; Gao et al. 2023). SOX 404 also required management to focus on SOX 404 compliance, diverting their attention from running the business (Solomon and Bryan-Low 2004). These substantial resource commitments for SOX 404 compliance are also referenced by firms in their 10-K filings (see e.g., Example 1 in Internet Appendix 3). As SOX 404 takes attention and investment away from compliance with nonfinancial regulations, it could increase the likelihood of nonfinancial violations. SOX also likely increased external scrutiny of financial, but not nonfinancial compliance. As a result, SOX 404 could have induced firms to prioritize financial compliance, increasing firms' nonfinancial violations. Ultimately, the effect of SOX 404 on nonfinancial violations is an empirical question that we examine in this study.

3. Empirical Methodology and Data

3.1. Data

We obtain data on corporate violations from the Violation Tracker database, maintained by the non-profit Good Jobs First as part of its mission to promote corporate and government accountability.[15] The dataset contains violations starting in 2000 and imposed by more than 400 regulatory agencies (federal, state, and local) across a wide range of regulatory areas, including, among others, banking, consumer protection, environmental, health and safety, and workplace discrimination.[16] To create this repository, Good Jobs First downloads violations from agency websites and matches (often manually) violations to parent companies (Raghunandan 2024). The database is increasingly used in accounting and finance research (e.g., Heese and Pérez-Cavazos 2020; Heese et al. 2022; Raghunandan 2024; Raghunandan and Rajgopal 2022). For our primary analyses, we restrict the sample to violations occurring between 2001 and 2007, a tight window around SOX 404 implementation for accelerated filers. Appendix A provides a list of the type of nonfinancial violations used in our analyses, the top 5 regulatory agencies enforcing them, and our classification of violations into different stakeholder categories.[17]

We match the violation data to Audit Analytics data on SOX 404 adoption and drop observations from financial institutions and regulated utilities. For our main analyses, we focus on firms that adopt SOX 404 either in 2004 or 2005. Finally, we exclude firms that we cannot match to Compustat and those with missing data for our control variables, including firms' total assets (*Size*), the ratio of liabilities to total equity (*Leverage*), market-to-book ratio (*MTB*), asset tangibility (*PPE*), asset turnover (*Asset Turnover*), and capital expenditure (*CAPEX*). Control variables are winsorized at the 1st and 99th percentiles throughout all tests. For the treatment

[15] The Violation Tracker database is accessible at https://www.goodjobsfirst.org/violation-tracker. Further, the user guide containing a list of the agencies and the online locations of their data is available at https://violationtracker.goodjobsfirst.org/pages/user-guide.

[16] Violation Tracker's website offers a search interface that enables users to search case types using standardized categories. These online searches are free of charge; however, downloading search results and viewing several data fields requires a subscription. Violation Tracker provides a parent-subsidiary matching table (based on the current parent). The companies (or facilities) named in the violations are linked to more than 3,000 parent companies based on this table. We manually check the parent-subsidiary matching table for our sample of firms to ensure that subsidiaries are matched to the parent that owned the subsidiary in the year of a violation.

[17] Internet Appendix 1 provides a more detailed overview.

group, we either use all firms that adopted SOX 404 in 2004 or 2005 (full sample) or the subset of firms with a public float smaller than $150 million in the period before SOX 404 adoption (size-restricted sample).[18] Control firms are firms that either never adopt SOX 404 or firms that adopt SOX 404 after 2007. Our final full (size-restricted) sample includes 3,930 (2,212) unique firms and 26,999 (17,576) firm-year observations, as well as 10,371 (1,514) nonfinancial violations with more than $14.81 ($0.95) billion in penalties sanctioned.[19]

Table 1 describes our sample in more detail. Panel A summarizes the sample selection process. Panel B presents the number of nonfinancial violations and penalties by year and category for the full and size-restricted sample. There is considerable variation over time in both the number of violations and the associated penalties. Our data includes various offense types, such as workplace safety, environmental offenses, and consumer-related violations. Employee-related violations are the most common offenses, accounting for over 76% of total nonfinancial violations in the full sample. Government-related violations, however, account for the largest penalties, representing approximately 33% of all penalties in the full sample. Panel C presents a univariate analysis of the propensity of nonfinancial violations between treatment and control firms in both samples. The analysis shows a substantial increase in the propensity of nonfinancial violations—both economically and statistically—for treatment firms following SOX 404 adoption. In contrast, the propensity of nonfinancial violations for control firms remains rather stable.[20] This analysis provides initial evidence in support of our hypothesis.

[18] Both samples have strengths and weaknesses. While the full sample includes all firms, larger firms may systematically differ from smaller control firms, as they might face more regulatory scrutiny. In contrast, treatment and control firms are likely more similar in the size-restricted sample, but this research design choice limits the sample and may introduce noise and bias. As a result, we present results using both samples throughout the study.
[19] We use revised penalty amounts, rather than those initially proposed, to reflect negotiations or adjustments. Violations with missing penalty amounts or amounts below $5,000 do not enter the sample. The former can be the case for agencies such as the Food and Drug Administration (FDA), which generally does not impose penalties. However, the FDA refers serious cases to the Justice Department, which may impose penalties. Those penalties are included in Violation Tracker (Violation Tracker 2024).
[20] While the analysis also shows that the level of violation propensity is larger for treatment compared to control firms, please note that the average treatment firm is approximately 5x as large as a control firm. In addition, we conduct robustness tests using entropy-balanced samples to alleviate concerns related to this difference. A comparison of the nonfinancial-violation propensity between treatment and control firms in this sample shows a smaller gap: the mean violation propensity for treated firms is 0.067 compared to 0.034 for control firms.

3.2. Empirical Methodology

Our baseline regression model examines whether the adoption of SOX Section 404 affects firm-level nonfinancial-violation propensity using a stacked difference-in-differences design:

$$Y_{i,t,c} = \beta \text{SOX 404 Adoption}_{i,t,c} + \text{Controls}_{i,t} + \gamma_{i,c} + \delta_{t,c} + \varepsilon_{i,t,c}, \quad (1)$$

where *i* indexes a firm, *t* indicates a year, and *c* indexes a cohort. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. The variable *SOX 404 Adoption* is defined based on companies in our sample that adopted SOX 404 either in 2004 or 2005, and equals one (zero) for the three years following (before) the adoption.[21] We report results for both the full and the size-restricted sample (see Section 3.1 for sample definitions).

The regression specification includes the above-described control variables that capture time-varying firm factors that may influence violations of nonfinancial regulations (*Controls*) and are motivated by prior research (e.g., Caskey and Ozel 2017; Cohn and Wardlaw 2016; Heese and Pérez-Cavazos 2020; Hope et al. 2022). Our main specification includes cohort-firm fixed effects ($\gamma_{i,c}$) that account for time-invariant heterogeneity across firms and cohort-year fixed effects ($\delta_{t,c}$) that account for time trends. Standard errors are clustered at the cohort-firm level and Appendix B provides variable definitions. Our coefficient of interest is β. Figure 1 illustrates our research design using SOX 404 adoption by two companies as examples. Lufkin Industries, Inc. adopted SOX 404 in 2004, while TravelZoo Inc. adopted in 2005. Retractable Technologies, Inc., with a public float below $75 million, did not adopt SOX 404 during our sample period and serves as a control firm.

Table 1, Panel D provides descriptive statistics on our control variables for both the full and the size-restricted sample. In addition, it reports means and medians by treatment status and includes the p-values for t-tests of the differences in means between treatment and control firms.

[21] Thus, our sample ends in 2007, which ensures that we hold attestation requirements constant. These requirements changed with the PCAOB's adoption of AS5 in 2007 (e.g., Schroeder and Shepardson 2016).

Overall, treatment firms are larger than control firms. This difference decreases in the size-restricted sample, but it is still statistically significant. Across the other control variables, treatment and control firms are on average generally similar.

– Insert Table 1 and Figure 1 here –

4. Main Results

We begin our analyses with stacked difference-in-differences regressions that test the impact of SOX 404 adoption on nonfinancial misconduct. Table 2 provides the results from estimating equation (1). In Columns (1)-(2), we use the full sample and in Columns (3)-(4), we use the size-restricted sample. In Columns (1) and (3), we include cohort-specific firm and year fixed effects, and in Columns (2) and (4) we add firm-level control variables.[22]

Table 2 shows a positive and significant coefficient on *SOX 404 Adoption* across all specifications, with an effect size ranging from 0.019 (Column (1)) to 0.013 (Columns (3)-(4)). Based on the specification in Column (2), we document an economically meaningful increase in the propensity of nonfinancial violations by 1.8 percentage points or 21% of the sample mean propensity.[23]

Figure 2 plots the treatment coefficient from our main regressions for the full sample (Panel A) and the size-restricted sample (Panel B) in event time. The effect of SOX 404 adoption seems to be concentrated in the first two adoption years. In the third year, the coefficient reverts back to a level close to zero and is statistically insignificant, consistent with a temporary compliance shock, where firms initially face resource constraints or shifting priorities but subsequently adjust. This finding is consistent with prior literature documenting short-term compliance burdens following SOX (e.g., Alexander et al. 2013). Overall, these

[22] Our results hold if we include industry-by-year or state-by-year fixed effects (see Section 6).

[23] This increase corresponds to 0.08 additional violations (i.e., 0.018 x 4.4, which is the conditional annual mean violation count) and $113,230 in penalties (i.e., 0.018 x $6,290,566, which is the conditional annual mean penalty amount) per year for the average firm. Across all 2,435 treatment firms, this implies 193 additional violations and $275.7 million in penalties per year. In the size-restricted sample, the estimated 1.3 percentage point increase equals 50% of the sample mean and translates into $27,018 in additional penalties per year for the average adopting firm (or approximately $19.4 million across all 717 treatment firms).

results suggest that SOX 404 had unintended consequences for other stakeholders for approximately two years as the likelihood of nonfinancial misconduct increased after adoption.

– Insert Table 2 and Figure 2 here –

4.1. Types of Nonfinancial Violations

Next, we examine which violation categories drive our results to better understand which stakeholder is most affected by SOX 404. In Table 3, Panel A, we re-estimate our baseline model separately for each violation category. We show that the effect is driven primarily by *employee-related* violations, which are the most common offenses, representing more than 76% of nonfinancial violations in our sample. In terms of economic significance, the point estimate of 0.016 (see Column (3) of Table 3, Panel A) translates into $29,240 in additional penalties and 0.07 additional violations per firm year, or $71.2 million in additional penalties and 171 additional violations across all treatment firms. In the size-restricted sample, the point estimate of 0.011 is equivalent to $3,301 in additional penalties and 0.04 additional violations per year for the average firm. Across all treatment firms, this implies a total of 25 additional violations per year and approximately $2.4 million in additional annual penalties.

To corroborate these insights, we test if our main effect is driven by violations with relatively small penalties, such as employee-related violations (see Panel B). We classify nonfinancial violations as small or large based on penalties scaled by firms' public float, splitting them at the median into relatively small and relatively large penalties. We find that the effects are concentrated in relatively small penalties, suggesting that SOX 404 adoption primarily reduced monitoring of nonfinancial violations with smaller penalties.

In the interpretation of these results, it is important to note that these estimates are based only on *detected* violations. However, nonfinancial violations can also be associated with other costs besides regulatory penalties, including increased injury rates for employees. Thus, we next examine whether SOX 404 adoption can create harm for employees (see Panel C). Specifically, we collect data on workplace injuries from OSHA's Open Data Initiative (ODI)

and match them to public firms based on the linking table used in Caskey and Ozel (2017). This linking table relies on manually matching facilities in the ODI database to public firms in Compustat. The availability of a link within this dataset restricts our sample for the following tests. We find significant increases in workplace injuries of approximately 14% (12%) after SOX 404 adoption for the full (size-restricted) sample.[24] This increase corresponds to 25 (12) additional injuries per year for the average firm in the full (size-restricted) sample. We also find consistent results if we analyze injury cases relative to activity levels, focus on severe injury cases or use an indicator for above industry median injury cases as alternative dependent variables. Taken together, these results are consistent with SOX 404 adoption increasing workplace injuries and thereby creating harm for employees.

– Insert Table 3 here –

5. Mechanisms

We next conduct two sets of non-mutually exclusive cross-sectional tests to investigate the plausibility of our argument that SOX 404 ties managerial attention and resources, which drives the increase in nonfinancial-violation propensity. We test whether SOX 404 results in a larger increase (1) if firms face heightened external scrutiny towards their financial reporting or (2) if firms are subject to more internal monitoring frictions and resource constraints. Our subsequent cross-sectional analyses, which we discuss next, explore the effects of these factors.

5.1. External Scrutiny

Our first cross-sectional analysis investigates the role of two different types of external scrutiny vis-à-vis firms' financial reporting around the adoption of SOX 404. We expect that the adoption of SOX 404 ties more attention and resources in firms that are subject to higher levels of capital-market and creditor scrutiny regarding their financial reporting.

[24] This test also helps alleviate the concern that treatment and control firms may have different coverage of violations with small penalties (which are often workplace safety related), as we use actual injuries in this test.

Related to capital-market scrutiny, we expect an increased focus on compliance with financial regulation around capital issuances (Easterbrook 1984). We therefore split our sample based on an indicator for firms that issued either debt or equity in the pre-SOX 404 period based on SDC data. Finally, as prior research highlights the important role of financial reporting as a monitoring device after loan origination (e.g., Christensen, Nikolaev et al. 2016), we expect creditor scrutiny of firms' financial reporting to be particularly pronounced for high-leverage firms. Hence, we split our sample firms on the median into high vs. low creditor scrutiny based on pre-SOX 404 average leverage ratios.

For each of these tests, we bifurcate our treatment variable *(SOX 404 Adoption)* into two variables depending on the firm's exposure to external scrutiny. Specifically, we define *High (Low) External Scrutiny* as being equal to one if the firm is above (below) the sample median of the pre-treatment exposure to creditor scrutiny. For capital-market scrutiny, we define *High (Low) External Scrutiny* as being equal to one if the firm does (does not) issue capital in the pre-SOX 404 sample period. We then create two versions of our variable of interest: *SOX 404 Adoption* (*High External Scrutiny*) captures increases in nonfinancial violation propensity as a response to SOX 404 adoption for firms with high external scrutiny, whereas *SOX 404 Adoption* (*Low External Scrutiny*) captures the equivalent effect for firms with relatively low external scrutiny according to the respective dimension of scrutiny. The results are tabulated in Table 4. We find positive and significant coefficients on *SOX 404 Adoption* (*High External Scrutiny*) across both proxies for external scrutiny in the full sample. In the size-restricted sample, the coefficient on *SOX 404 Adoption* (*High External Scrutiny*) is highly significant for creditor scrutiny and similar in magnitude and close to statistically significant (p-value = 0.110) for capital-market scrutiny. Importantly, the coefficients on *SOX 404 Adoption* (*Low External Scrutiny*) are considerably smaller both in terms of economic and statistical significance. The differences in the increases between high-scrutiny and low-scrutiny adopters are statistically significant at conventional levels for both proxies in the full sample.

In the size-restricted sample, the differences are close to statistically significant for capital-market scrutiny and highly significant for creditor scrutiny. These results suggest that SOX 404 adoption triggers a particularly large increase in nonfinancial violations in the presence of high levels of scrutiny towards financial reporting.

– Insert Table 4 here –

5.2. Internal Monitoring Frictions and Resource Constraints

Our next cross-sectional analysis considers frictions related to internal monitoring and resource constraints. These analyses are motivated by the internal reasons for ICWs and changes triggered by SOX 404 that are disclosed in exemplary 10-K reports of affected firms. Companies refer to a "decentralized structure," an inadequate "tone at the top," and organizational culture (see Example 2 in Internet Appendix 3, as well as CFGI (2020)), in line with prior work on the determinants of ICWs (Doyle et al. 2007; Ge and McVay 2005).

We use the dispersion of firms' sales across segments to proxy for organizational complexity (e.g., Bushman et al. 2004). To capture corporate culture, we use a bag-of-words measure for corporate integrity culture following Li et al. (2021).[25] We then split firms based on the respective median into high vs. low complexity and high vs. low integrity.

Firms with monitoring frictions have to invest relatively more to remedy control-related issues after the adoption of SOX 404, and therefore we expect a stronger effect on nonfinancial violations. Lastly, we test the role of available resources for compliance outcomes. We use liquidity as a proxy for resources available for investments in internal control systems.

We tabulate the results in Table 5. We split our variable of interest *SOX 404 Adoption* into *SOX 404 Adoption* (*High Frictions*) and *SOX 404 Adoption* (*Low Frictions*) akin to the procedure used for external scrutiny. We find a positive and significant coefficient on *SOX 404*

[25] Li et al. (2021) use earnings call transcripts to train an algorithm with seed words related to integrity (e.g., "do the right thing") to develop a measure of a firm's integrity. They provide a list of the thirty most representative words for integrity in their Table 2, Panel A. We use this list and count the frequency of these words in firms' 10-K filings. Firms that mention these words more often are classified as high-integrity firms.

Adoption (*High Frictions*) across all three proxies for internal monitoring frictions both in the full and in the size-restricted sample. As expected, coefficients on *SOX 404 Adoption* (*Low Frictions*) are considerably smaller and mostly statistically insignificant. The differences between these coefficients are economically meaningful and (close to) statistically significant in most specifications. Overall, these results suggest that the likelihood of nonfinancial violations increases significantly more for firms with more internal monitoring frictions.

– Insert Table 5 here –

5.3. External Exposure and Managerial Attention

A potential limitation of our cross-sectional tests is that they only indirectly capture managerial attention and a firm's exposure to SOX 404, both of which are inherently difficult to measure. To better approximate these channels, we examine changes in audit fees, earnings calls, and 10-K disclosures. Based on the argument that audit fees reflect audit effort and client risk (e.g., Guo et al. 2018; Simunic and Stein 1996), we expect firms more exposed to SOX 404-related risks to exhibit relatively larger increases in audit fees around SOX 404 adoption. Further, in line with prior literature (e.g., Hassan et al. 2024), we use the relative share of a topic in earnings calls to proxy for managerial attention, as it reflects the priorities that managers (and potentially also external stakeholders) set at a given point in time.[26] Similarly, the prevalence of topics in 10-Ks is used to approximate managerial attention (Campello et al. 2022). Internet Appendix 2 summarizes our SOX-related word list. We expect nonfinancial violations to increase more for firms with larger audit fee increases and greater SOX focus in earnings calls and 10-Ks.

Results are presented in Table 6. In Columns (1)-(3), we validate our measures: We observe that following SOX 404 adoption, firms experience larger increases in audit fees, 10-Ks include significantly more SOX-related disclosures and managers of adopting firms talk

[26] Hassan et al. (2024, p. 415) expect "managers and analysts to devote more time to events of greater importance to the firm, in which case the time spent discussing an event is a powerful measure of a firm's exposure to it." Indeed, conference call discussions have been used to proxy for firms' political risk exposure (Hassan et al. 2019), climate change exposure (Sautner et al. 2023), or COVID-19 exposure (Hassan et al. 2023).

significantly more about SOX-related topics on earnings calls. We split our variable of interest *SOX 404 Adoption* into *SOX 404 Adoption* (*High SOX 404 Exposure*) and *SOX 404 Adoption* (*Low SOX 404 Exposure*) akin to the procedure used for external scrutiny. The results suggest that larger SOX exposure drives the increase in nonfinancial violation propensity observed in our main results. This is consistent with the interpretation that one explanation for the increase in nonfinancial-violation propensity following SOX 404 is limited managerial attention.[27]

– Insert Table 6 here –

6. Additional Tests

In this section, we present tests to examine the robustness of our results, to help rule out alternative explanations for our results, and to help alleviate identification concerns. We discuss each of these tests in more detail below.

6.1. Remediation of Internal Control Weaknesses and Additional SOX 404 Adoptions

An advantage of our main research design is that we focus on the initial adoption of SOX 404, which was akin to a quasi-natural experiment that created substantial opportunity cost between focusing on compliance with financial regulation versus nonfinancial regulation (Schroeder and Shepardson 2016). It is, however, possible that these effects do not generalize beyond the initial SOX 404 adopters. We conduct two tests to examine whether our findings generalize beyond the initial adoption of SOX 404. First, we exploit the remediation of severe ICWs as an alternative setting where firms face significant compliance demands. We define severe ICWs as instances where a firm discloses more than one ICW in a given year and where remediation takes longer than one year. We expect that the remediation of these ICWs ties up resources and management attention similar to the adoption of SOX 404. This expectation is based on prior literature and on anecdotal evidence from affected firms' 10-K disclosures: Johnstone et al

[27] Alternatively, our SOX 404 exposure measures may capture firms with low-quality internal controls. If so, the findings in Table 6 could suggest that firms with weak internal controls experience more nonfinancial violations after SOX 404 compliance. That said, additional tests show that only audit fees are significantly associated with internal control weaknesses (untabulated), while SOX-related words in 10-Ks and earnings calls are not. These findings suggest that the alternative interpretation may apply to audit fees but does not fully explain our results.

(2011) find that ICW disclosures are associated with structural changes in corporate governance, including board and executive turnover. Imdieke (2022) shows that more extensive remediation efforts increase the probability of successful remediation. Gao et al. (2023) document investments in (accounting-related) human resources following the disclosure of ICWs. Further, anecdotal evidence from firms' 10-Ks suggests that ICW remediation often involves investments in IT infrastructure and personnel, with firms explicitly noting concerns about diverted management attention (see Example 3 in Internet Appendix 3).

To test whether ICW remediation leads to higher nonfinancial violations, we follow our primary research design and study changes in nonfinancial violations during remediation periods. Our sample includes 345 severe ICWs across 332 firms. For each year starting in 2004, we construct treatment cohort datasets comprising firms disclosing a severe ICW and control firms that never report an ICW. Following Imdieke (2022), we identify the remediation period for each severe ICW to determine the length of the treatment and control period. For the stacked regressions, we retain observations up to a six-year window around the first-time disclosure of a severe ICW. The results are tabulated in Table 7, Panel A. Columns (1)-(3) contain the results from the stacked regressions. In Column (4), we tabulate results for a staggered difference-in-differences regression. Consistent with our main findings, we find a significant increase in the propensity of nonfinancial violations during the remediation period of a severe ICW. The magnitudes are similar to those reported in our main analyses.

Second, we extend our sample and examine SOX 404 adoptions until 2022. While our main sample focuses on the initial SOX 404 adopters in 2004 and 2005, this alternative test also considers SOX 404 adopters in later years. The results from this test are tabulated in Table 7, Panel B. Similar to our main findings, we find that the likelihood of nonfinancial violations increases after SOX 404 adoption.[28] Overall, these results alleviate external validity concerns.

[28] In untabulated tests, we use the adoption of SOX 404(a) by smaller companies (with a public float below $75 million) starting in 2007 as an alternative setting. Consistent with our main results, we find an increase in the

– Insert Table 7 here –

6.2. Alternative Explanations

Next, we examine whether improved injury reporting or changes in regulatory scrutiny could explain our results. For these tests, we focus on OSHA, the regulator responsible for the most prevalent type of violations in our sample. We conduct two sets of tests and report them in Table 8. First, we exclude firms that experienced accidents after SOX 404 adoption. We find consistent results (see Panel A), alleviating the concern that SOX 404 adoption improves companies' injury reporting, leading to increased OSHA inspections and detected violations. Second, we examine OSHA inspections directly and collect data on the number of inspections, as well as the efficiency of inspections (i.e., violations or penalties per inspection) and aggregate these variables at the state-industry(two-digit SIC)-year level. The independent variable of interest in these tests is *Post 2004*, an indicator set to one for years starting in 2004 and zero otherwise. We include state and industry fixed effects and cluster standard errors at the industry level. We omit year fixed effects in these specifications as they are collinear with *Post 2004*. In additional specifications, we focus on firms with high SOX 404 exposure by interacting *Post 2004* with an indicator for industries with high initial SOX 404 compliance (defined at the two-digit SIC level using a median split based on the share of firms complying with SOX 404 by 2006). These specifications include year fixed effects. The results are reported in Panel B of Table 8. Across all specifications, we find no significant changes in OSHA enforcement, suggesting that changes in enforcement are unlikely to explain our results. We also find no increases in inspections triggered by accidents, further suggesting that improved injury reporting and related OSHA enforcement are unlikely to explain our findings.

6.3. Sample Restrictions and Alternative Treatment Definition

occurrence of nonfinancial violations after the adoption of SOX 404(a) once we exclude large accelerated filers from the control group. These findings provide some evidence suggesting that the adoption of SOX 404(a) also resulted in an increase in nonfinancial violations. However, a limitation of this test is that the adoption of SOX 404(a) coincides with PCAOB AS5, which affected the control group in this test.

We test the robustness of our results using violation counts and penalties as alternative dependent variables.[29] Table 8, Panel C presents the results. We find significant increases in the number of nonfinancial violations (2.1% and 1.2%) and penalties (26.7% and 14.0%).

A potential concern with our baseline regression analysis is that treatment firms may not be comparable to control firms. For example, our descriptive statistics show significant size differences between treatment and control firms. In line with prior literature (e.g., Iliev 2010; Albuquerque and Zhu 2019), we vary the size cutoff for the restricted sample in additional tests to alleviate this concern. These results are presented in Table 8, Panel D. Columns (1) and (2) exclude firms with a public float larger than $120 million and $100 million in the pre-SOX 404 period, respectively. Columns (3) and (4) restrict the sample to firms with a public float between $25 million and $150 million and $50 million and $150 million, in line with prior literature (e.g., Albuquerque and Zhu 2019). Column (5) reports the results from our most restrictive sample, limited to firms with a pre-SOX 404 public float between $50 million and $100 million. Coefficient estimates are very close to our main results in Table 2. Overall, these results suggest that our findings are unlikely driven by size differences (or differences highly correlated with size) between treatment and control firms. Furthermore, regulatory scrutiny could differ over time by industry or location (e.g., different regulatory regimes across states). To alleviate the concern that such differences drive our results, Panel E shows that our results are robust to the inclusion of cohort-state-year and cohort-industry-year fixed effects.

Next, we show in Panel F that our results are robust to different research-design choices. In Columns (1) and (6), we run a staggered difference-in-differences regression and document coefficient sizes closely aligned with our main results. In Columns (2) and (7), we define treated firms based on their 2002 public float surpassing the regulatory threshold of $75 million (e.g.,

[29] For these tests, the dependent variable is the natural logarithm of one plus the number of violations per firm and year or the natural logarithm of one plus the dollar amount of penalties per firm and year. These variables are winsorized at the 1/99% level to mitigate the influence of outliers.

Iliev 2010; Albuquerque and Zhu 2019). The 2002 public float cannot be affected by firms' strategic behavior to manage their public float since the SEC published the $75 million threshold only in 2003. The results are similar to our main results.

Finally, we use the stacked samples again and present results using an entropy-balanced sample with weights calculated based on our control variables (Columns (3) and (8)). We find consistent results, helping to alleviate comparability concerns as the entropy balancing decreases the differences between treatment and control groups based on observable characteristics. Our findings are also robust to the exclusion of the years 2007 (Columns (4) and (9)) 2001 and 2007 (Columns (5) and (10)). Our sample includes disproportionately few observations in these years. Overall, these tests show that our main findings are robust to a range of alternative specifications.

– Insert Table 8 here –

7. Conclusion

This study investigates the effect of compliance with financial regulation on nonfinancial violations. We exploit the staggered adoption of SOX Section 404 and find significant increases in the propensity for nonfinancial violations after SOX 404 adoption. In line with explanations related to limited managerial attention and resources, these effects are stronger for firms with greater external scrutiny toward financial reporting, greater internal challenges in monitoring operations, limited resources, and more SOX focus as proxied by audit fees, conference call transcripts, and 10-K disclosures. Overall, our results suggest that financial reporting regulation can result in unintended consequences, creating harm for other stakeholders, such as employees. This paper contributes to the literature on SOX and financial regulation by documenting an intricate tradeoff between different policy goals, unintended consequences of regulation outside the original scope of the regulation, and the importance of balancing corporate resources to ensure compliance with an ever-increasing web of various types of regulations.

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Appendix A: Definitions of Nonfinancial Violations

Data on violations is obtained from Violation Tracker, which lists primary offenses for each violation. Violations are assigned to six stakeholder categories: competitors, consumers, employees, environment, government, and shareholders, based on the primary offense. The relative fraction of each primary offense type within the respective category (based on the violation count for the period 2000 to 2021) is provided. The tabulation of primary offense types and regulatory agencies is limited to the top five within each violation category. Agencies are marked as federal ("Fed") or state ("State"). 244 violations that are labeled miscellaneous are excluded. For a more extensive version of this table, please refer to Internet Appendix 1.

Type	Category of Violation	Primary Offenses (Top 5)	Regulatory Agencies (Top 5)
Nonfinancial	**Competitors**	Export control (24%), price-fixing or anti-competitive practices (19%), agribusiness (12%), foreign corrupt practices act (10%), maritime (9%)	Bureau of Industry and Security (22%, Fed); Grain Inspection, Packers & Stockyards Administration (11%, Fed); Federal Maritime Commission (9%, Fed); Justice Department Antitrust Division (9%, Fed); Securities and Exchange Commission (7%, Fed)
	Consumers	Consumer protection (46%), insurance (43%), privacy (3%), aviation consumer protection (3%), telecommunications (2%)	California Department of Managed Health Care (8%, State); Texas Department of Insurance (6%, State); Florida Office of Financial Regulation (4%, State); Virginia Bureau of Insurance (3%, State); New York Attorney General (3%, State)
	Employees	Workplace safety or health (55%), wage and hour (24%), motor vehicle safety (7%), railroad safety (4%), labor relations (4%)	Occupational Safety & Health Administration (48%, Fed), Labor Department Wage and Hour Division (21%, Fed); Mine Safety and Health Administration (8%, Fed); Federal Motor Carrier Safety Administration (7%, Fed); Federal Railroad Administration (4%, Fed)
	Environment	Environmental (99%), offshore drilling (1%)	Environmental Protection Agency (23%, Fed); Texas Commission on Environmental Quality (10%, State); Pennsylvania Department of Environmental Protection (6%, State); New Jersey Department of Environmental Protection (4%, State); Florida Department of Environmental Protection (3%, State)
	Government	False Claims Act and related (70%), HHS civil monetary penalties (15%), Controlled Substances Act (4%), Medicare parts C and D enforcement action (4%), federal leasing royalty (3%)	Justice Department Civil Division (23%, Fed); Health & Human Services Department of Inspector General (15%, Fed); Centers for Medicare & Medicaid Services (5%, Fed); Massachusetts Attorney General (5%, State); New York Attorney General (4%, State)
Financial	**Shareholders**	Investor protection (65%), banking (10%), tax (10%), economic sanction (6%), accounting fraud or deficiencies (4%)	Securities and Exchange Commission (21%, Fed); Financial Industry Regulatory Authority (7%, Fed); Federal Deposit Insurance Corporation (7%, Fed); Commodity Futures Trading Commission (6%, Fed); Office of Foreign Assets Control (5%, Fed)

Appendix B: Variable Definitions

The following variables are constructed using data from Violation Tracker's data set of corporate misconduct [VT], Compustat [C], Audit Analytics [AA], data on workplace injuries from OSHA's Open Data Initiative [OSHA ODI], other publicly available OSHA datasets [OSHA], data on public float from Ewens et al. (2023) [Ewens et al. 2023], data on corporate culture from Li et al. (2021) [Li et al. 2021], earnings call data from Hassan et al. (2019) [Hassan et al. 2019], and capital issuance data from SDC [SDC].

Variable	Description	Source
I. Dependent Variables		
A. Violation-Related Variables		
Competitor Violation Propensity	Indicator variable equal to one in case of the occurrence of at least one competitor-related violation for a given firm in the respective year, zero otherwise (see stakeholder mapping in Internet Appendix 1).	VT
Consumer Violation Propensity	Indicator variable for at least one consumer-related violation, defined akin to *Competitor Violation Propensity.*	VT
Employee Violation Propensity	Indicator variable for at least one employee-related violation, defined akin to *Competitor Violation Propensity.*	VT
Environment Violation Propensity	Indicator variable for at least one environment-related violation, defined akin to *Competitor Violation Propensity.*	VT
Government Violation Propensity	Indicator variable for at least one government-related violation, defined akin to *Competitor Violation Propensity.*	VT
Large Penalty Violation Propensity	Indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in the respective year with the associated penalty amount scaled by the firm's public float being larger than the median relative penalty amount, zero otherwise.	VT
Nonfinancial Violation Propensity	Indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in the respective year, zero otherwise.	VT
Nonfinancial Violation Count	Natural logarithm of one plus the number of total nonfinancial violations per firm and year.	VT
Nonfinancial Violation Penalty	Natural logarithm of one plus the penalties for total nonfinancial violations per firm and year.	VT
Small Penalty Violation Propensity	Indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in the respective year with the associated penalty amount scaled by the firm's public float being smaller than the median relative penalty amount, zero otherwise.	VT
B. SOX Exposure-Related Variables		
Audit Fees	Natural logarithm of the firm's audit fees for the period.	AA
Exposure (Earnings Calls)	SOX exposure in earnings calls. Measured based on the share of their quarterly earnings conference calls that they devote to SOX / SOX 404 related topics.	Hassan et al. 2019
Word Count	Natural logarithm of one plus the count of SOX / SOX 404 related expressions in a firm's 10-K reports.	Own calculation
C. Workplace-Safety Variables		

High Injury Firm	Indicator equal to one for firms that report above median number of injury cases, zero otherwise. Median defined within 2-digit SIC industry.	OSHA ODI
OSHA Accidents	The number of inspections triggered by accidents scaled by the total number of unplanned inspections in a state-industry (2-digit SIC level) year.	OSHA
OSHA Detections	The number of detected violations scaled by the number of inspections in a state-industry (2-digit SIC level) year.	OSHA
OSHA Injury Cases	Natural logarithm of the company-wide aggregate number of injury cases reported in the ODI data for a given year (published by OSHA).	OSHA ODI
OSHA Inspections	Natural logarithm of the number of OSHA inspections in a state-industry (2-digit SIC level) year.	OSHA
OSHA Penalties	The penalty amount assigned for the detected violations (in $k) scaled by the number of inspections in a state-industry (2-digit SIC level) year.	OSHA
Relative OSHA Injury Cases	Company-wide aggregate number of injury cases reported in the ODI data for a given year (published by OSHA) scaled by revenues (in $m).	OSHA ODI / C
Relative OSHA Injury Cases (severe)	Company-wide aggregate number of severe injury cases reported in the ODI data for a given year (published by OSHA) scaled by revenues (in $m). Severe cases are reported as either injuries (column INJ_M1) or fatalities (column DEATHS_G) in the ODI data.	OSHA ODI / C
II. Independent Variables		
High SOX404 Compliance	Indicator capturing 2-digit SIC industries with high initial SOX 404 compliance (median split based on the two-digit SIC-code level share of firms complying with SOX 404 by 2006), zero otherwise.	AA / Own calculation
ICW Remediation	Indicator equal to one if the firm-year is a remediation year after the discovery of a SOX 404 internal control weakness, and zero otherwise.	AA
Post 404	Indicator equal to one for years starting with 2004, zero otherwise.	Own calculation
SOX 404 Adoption	Indicator equal to one for firms that file both a management report and an auditor report on the effectiveness of their internal controls over financial reporting, zero otherwise. (Alternative definition for robustness tests: Indicator equal to one if a firm's public float in 2002 surpasses $75m based on Ewens et al. (2023)).	AA
III. Control Variables		
Asset Turnover	Revenues / Total Assets.	C
Capex	Capital Expenditures / Total Assets.	C
Leverage	(Debt in Current Liabilities + Long-Term Debt) / Total Assets.	C
MTB	((Shares outstanding * Closing stock price) + Preferred Stock + (Debt in Current Liabilities + Long-Term Debt) – Deferred Taxes) / Total Assets.	C
PPE	Net property plant and equipment / Total Assets.	C
Size	Natural logarithm of Total Assets.	C

IV. Cross-Sectional Variables		
High Capital Market Scrutiny	Indicator equal to one if the firm issues capital (debt and/or equity) in the pre-SOX 404 period, zero otherwise.	SDC
High Creditor Scrutiny	Indicator equal to one if the firm has an above median pre-SOX 404 average leverage ratio, zero otherwise.	C
High Exposure (Audit Fee Increase)	Indicator equal to one if the firm experiences an above median audit fee increase around the adoption of SOX 404 (split firms based on the difference between the average post-period fees and the average pre-period fees; Median defined separately for treatment vs. control firms), zero otherwise.	AA
High Exposure (Conference Calls)	Indicator based on a median split on the firm's total SOX-related earnings call exposure in the year before the first adoption of SOX 404 (see keyword list in Internet Appendix 2). Median defined separately for treatment vs. control firms. Equal to one for firms with high exposure, zero otherwise.	Hassan et al. 2019
High Exposure (Word Count)	Indicator based on a median split on the firm's total 10-K wordcount for SOX/ SOX 404 related expressions in the year before the first adoption of SOX 404 (see keyword list in Internet Appendix 2). Median defined separately for treatment vs. control firms. Equal to one for firms with high exposure, zero otherwise.	Own calculation
High Frictions (Low Integrity Culture)	Indicator equal to one if the firm has a below median sum of integrity-related words as defined by Li et al. (2021) in the pre-treatment period, zero otherwise. The word count is calculated based on firms' 10-K disclosures.	Li et al. 2021 / Own calculation
High Frictions (Low Liquidity)	Indicator equal to one if the firm's liquidity proxied by cash and cash equivalents scaled by total assets is below the median, zero otherwise.	C
High Frictions (Segments Dispersion)	Indicator equal to one if a firm is characterized by above median dispersion of sales across reported segments in the pre-SOX 404 period, zero otherwise. Dispersion is calculated based on Herfindahl-Hirschman indices measuring within firm industry and geographic concentration of sales.	C

Figure 1: Research Design

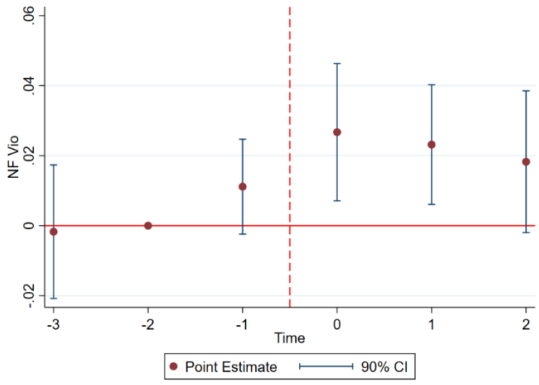
This figure illustrates our research design. Consider two firms that adopted SOX 404 either in 2004 or 2005. In 2004 (therefore 2004 is the first treatment year), Lufkin Industries, Inc. adopted SOX 404. A six-year window around the adoption year is used, meaning treated firms are included from three years before the treatment to three years after the treatment. Other firms that do not adopt SOX 404 form the control group. For example, Retractable Technologies, Inc. Each 0/1 coded cell (emphasized in bold) represents a firm-year observation included in the analysis. We also indicate the treatment cohort the respective observations are assigned to in the second column.

Firm	Cohort	2001	2002	2003	2004	2005	2006	2007
Lufkin Industries, Inc. adopted SOX 404 in 2004	**2004**	**0**	**0**	**0**	**1**	**1**	**1**	
Retractable Technologies, Inc. did not adopt SOX 404		**0**	**0**	**0**	**0**	**0**	**0**	
TravelZoo Inc. adopted SOX 404 in 2005	**2005**		**0**	**0**	**0**	**1**	**1**	**1**
Retractable Technologies, Inc. did not adopt SOX 404			**0**	**0**	**0**	**0**	**0**	**0**

Figure 2: Event-time Plots for Violation Patterns around SOX 404 Adoption

This figure reports the coefficient plots from the main results using a stacked linear regression design and an entropy-balanced sample with entropy weights calculated based on the firm-level control variables. Panel A uses the full sample and Panel B uses the size-restricted sample. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. The analysis includes cohort-specific firm and year fixed effects, as well as firm-level controls. Standard errors are clustered at the cohort-firm level. The horizontal axis records the periods in event time with t=0 defined to be the first year of SOX 404 adoption. The figure plots the coefficient estimates for each event period together with 90% confidence intervals. The indicator for the period t-2 is omitted in the estimation because t-2 is the last clean pre-period for both treatment cohorts (as period t-1 for the 2005 cohort is already a treatment year for the 2004 cohort). All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level.

Panel A: Full Sample



Panel B: Size-restricted Sample

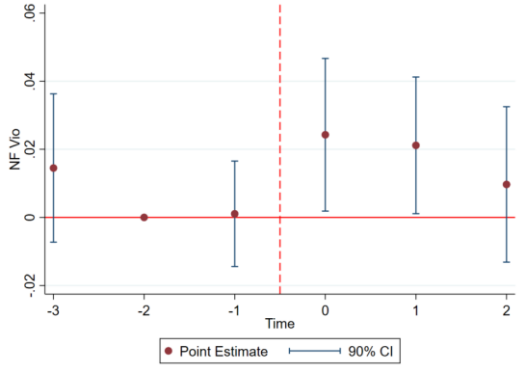


Table 1: Sample

Panel A: Sample Selection

This table reports the sample selection process.

Sample Construction Steps	# Firms	Firm-Years (2001-2007)
Audit Analytics data on SOX 404 adoption in 2004-2007	17,145	118,432
Less: No match with Compustat	4,604	35,654
Less: Financial Sector & Utilities	1,343	10,745
Less: Firms with missing controls	6,019	44,212
Sample before stacking	**5,179**	**27,821**
Final Sample (Full, cohorts 2004/05)	**3,930**	**26,999**
Less: Size restriction	1,718	9,423
Final Sample (Restricted, cohorts 2004/05)	**2,212**	**17,576**

Panel B: Descriptive Statistics of Nonfinancial Violations

This table reports the number of nonfinancial violations and related penalties (in $m) by year and stakeholder. Numbers in parentheses refer to the size-restricted sample. Numbers in front of the parentheses refer to the full sample. Stakeholders are mapped to violations based on the offense categories assigned by the Violation Tracker database. The mapping is displayed in detail in Internet Appendix 1.

	Number of violations					**Penalties ($m)**				
	Full Sample (Restricted Sample)					Full Sample (Restricted Sample)				
Year	Competitor	Consumer	Employee	Environment	Government	Competitor	Consumer	Employee	Environment	Government
2001	8 (1)	22 (1)	1,402 (237)	232 (41)	19 (0)	67.09 (0.21)	10.15 (0.1)	387.82 (4.92)	199.27 (5.08)	216.63 (0)
2002	8 (2)	34 (4)	964 (122)	294 (39)	7 (1)	45.61 (40)	37.19 (0.55)	784.01 (10.56)	306.18 (20.42)	64.99 (2.12)
2003	21 (4)	43 (3)	1,107 (107)	338 (66)	14 (3)	291.63 (70.97)	62.08 (21.19)	136.22 (3.84)	787.62 (408.62)	1,046.94 (46.41)
2004	27 (6)	39 (0)	1,286 (142)	366 (70)	17 (2)	887.55 (39)	288.64 (0)	340.11 (27.78)	116.52 (36.05)	843.83 (30.27)
2005	22 (4)	33 (4)	1,259 (150)	354 (59)	13 (1)	455.06 (25.41)	35.47 (8.48)	770.85 (35.41)	2625.55 (6.82)	418.45 (1.4)
2006	30 (5)	32 (8)	1,415 (180)	399 (73)	19 (2)	216.75 (3.85)	58.54 (21)	608.21 (11.22)	164.32 (8.41)	2164.70 (20)
2007	5 (0)	3 (1)	462 (158)	70 (16)	7 (2)	22.09 (0)	1.17 (0.21)	260.40 (8.58)	33.42 (18.19)	59.21 (16.86)
Total	**121 (22)**	**206 (21)**	**7,895 (1,096)**	**2,053 (364)**	**96 (11)**	**1,985.78 (179.44)**	**493.24 (51.52)**	**3,287.62 (102.32)**	**4,232.88 (503.59)**	**4,814.75 (117.06)**

Panel C: Univariate Analysis of Nonfinancial Violation Propensity

This table reports the pre- and post-SOX 404 adoption averages of the nonfinancial violation propensity (an indicator set to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise) for treatment and control firms. Averages for the full and size-restricted sample are reported. The last column reports the p-values of t-tests of the differences in pre- versus post-SOX 404 means. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

	Mean Nonfinancial Violation Propensity			
	Pre	Post	*Difference*	T-test (p-value)
Full Sample				
Treatment	0.1463	0.1777	*0.0314*	0.000***
Control	0.0119	0.0165	*0.0045*	0.0256**
Difference	*0.1344*	*0.1612*	***0.0268***	
Restricted Sample				
Treatment	0.0558	0.0789	*0.0231*	0.004***
Control	0.0119	0.0165	*0.0045*	0.0256**
Difference	*0.0438*	*0.0624*	***0.0186***	

Panel D: Descriptive Statistics of Main Variables

This table reports summary statistics for the full and size-restricted sample. Means and medians are separately reported for treatment (T) and control (C) firms. P-values for t-tests on the difference in means between treatment and control firms are reported. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers.

Variables		**Size**	**Leverage**	**MTB**	**PPE**	**Asset Turnover**	**CAPEX**
Full Sample (N = 26,999)	Mean	4.851	0.218	2.107	0.240	1.056	0.050
	Mean (T)	6.356	0.205	1.947	0.255	1.023	0.052
	Mean (C)	3.380	0.231	2.264	0.226	1.088	0.048
	T-test (p-value)	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
	Median	4.658	0.137	1.347	0.153	0.876	0.028
	Median (T)	6.235	0.159	1.394	0.176	0.865	0.033
	Median (C)	3.095	0.113	1.283	0.126	0.893	0.022
	SD	2.303	0.281	2.440	0.236	0.854	0.064
	p25	3.018	0.002	0.875	0.059	0.451	0.012
	p75	6.482	0.322	2.318	0.352	1.435	0.059
Restricted Sample (N = 17, 576)	Mean	3.770	0.223	2.233	0.229	1.071	0.049
	Mean (T)	5.127	0.196	2.125	0.239	1.012	0.052
	Mean (C)	3.380	0.231	2.264	0.226	1.088	0.048
	T-test (p-value)	*0.000*	*0.000*	*0.006*	*0.005*	*0.000*	*0.000*
	Median	3.517	0.109	1.308	0.132	0.888	0.024
	Median (T)	4.992	0.096	1.380	0.153	0.873	0.029
	Median (C)	3.095	0.113	1.283	0.126	0.893	0.022
	SD	1.844	0.313	2.768	0.239	0.910	0.069
	p25	2.451	0.000	0.819	0.049	0.391	0.010
	p75	4.753	0.322	2.410	0.338	1.501	0.055

Table 2: SOX Section 404 Adoption and Nonfinancial Violation Propensity

This table reports the main results using a stacked linear regression design. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. Columns 1 and 2 (3 and 4) use the full (size-restricted) sample. The tests include cohort-specific firm and year fixed effects. Columns 2 and 4 also include firm-level control variables. Standard errors are clustered at the cohort-firm level and are reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Nonfinancial Violation Propensity			
Sample	Full		Restricted	
Variables	(1)	(2)	(3)	(4)
SOX 404 Adoption	**0.019*** **	**0.018*** **	**0.013** **	**0.013** **
	(0.005)	**(0.005)**	**(0.006)**	**(0.006)**
Size		0.006**		0.000
		(0.002)		(0.001)
Leverage		-0.001		0.001
		(0.004)		(0.002)
MTB		-0.000		-0.000**
		(0.000)		(0.000)
PPE		0.016		0.007
		(0.011)		(0.006)
Asset Turnover		0.003		-0.001
		(0.003)		(0.001)
Capex		0.008		0.028*
		(0.024)		(0.015)
Cohort x Firm Fixed Effects	Yes	Yes	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	26,999	26,999	17,576	17,576
Adj. R-sq.	0.581	0.581	0.544	0.544

Table 3: Types of Nonfinancial Violations

Panel A: Violation Categories

This table reports results using a stacked linear regression design. The dependent variables are indicator variables equal to one in case of the occurrence of at least one nonfinancial violation related to the respective stakeholder for a given firm in a year, and zero otherwise. In Columns 1 and 6, the indicator variable captures cases in which a firm engages in competitor-related violations. Columns 2 and 7 focus on consumer-related violations. In Columns 3 and 8, the indicator captures violations related to employees. Columns 4 and 9 restrict the indicator to environment-related violations. Columns 5 and 10 focus on government-related violations. Stakeholders are mapped to violations based on the offense categories assigned by the Violation Tracker database. The mapping is summarized in Appendix A and displayed in detail in Internet Appendix 1. Columns 1-5 (6-10) use the full (size-restricted) sample. The tests include cohort-specific firm and year fixed effects, as well as firm-level control variables. Standard errors are clustered at the cohort-firm level and are reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Competitor	Consumer	Employee	Environment	Government	Competitor	Consumer	Employee	Environment	Government
Sample			Full					Restricted		
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SOX 404 Adoption	**0.003***	**-0.002**	**0.016*****	**0.004**	**0.002**	**0.001**	**-0.000**	**0.011****	**0.005**	**0.001**
	(0.001)	**(0.002)**	**(0.004)**	**(0.004)**	**(0.001)**	**(0.001)**	**(0.001)**	**(0.005)**	**(0.004)**	**(0.001)**
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	26,999	26,999	26,999	26,999	26,999	17,576	17,576	17,576	17,576	17,576
Adj. R-sq.	0.078	0.293	0.531	0.460	0.144	0.079	0.159	0.494	0.415	0.151

Panel B: Magnitude of Penalties

This table reports results using a stacked linear regression design. For the calculation of the dependent variable, penalty amounts are related to firms' public float and then split based on the median relative penalty size into relatively small and large penalties. In Columns 1 and 3, the dependent variable is an indicator variable equal to one in case of the occurrence of at least one small penalty nonfinancial violation for a given firm in a year, and zero otherwise. The dependent variable in Columns 2 and 4 is defined equivalently for the occurrence of large penalties. Columns 1 and 2 (3 and 4) use the full (size-restricted) sample. The tests include cohort-specific firm and year fixed effects, as well as firm-level control variables. Standard errors are clustered at the cohort-firm level and are reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Small Penalty	Large Penalty	Small Penalty	Large Penalty
Sample	Full		Restricted	
Variables	(1)	(2)	(3)	(4)
SOX 404 Adoption	**0.018*** **	**0.006**	**0.012*** **	**-0.001**
	(0.004)	**(0.004)**	**(0.003)**	**(0.005)**
Firm Controls	Yes	Yes	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	26,999	26,999	17,576	17,576
Adj. R-sq.	0.336	0.334	0.202	0.411

Panel C: Workplace Injuries

This table reports results using a stacked linear regression design. The dependent variable in Columns 1 and 5 is the natural logarithm of the number of injury cases reported for a firm-year in the ODI data published by OSHA. In Columns 2 and 6, the dependent variable is the number of injury cases scaled by contemporaneous revenues (in $m). In columns 3 and 7, the dependent variable is the number of severe injury cases scaled by contemporaneous revenues (in $m). The dependent variable in Columns 4 and 8 is an indicator variable that equals one for firms that report an above-median number of cases (split within 2-digit SIC industry). Columns 1-4 (5-8) use the full (size-restricted) sample. The tests include cohort-specific firm and year fixed effects, as well as firm-level controls. In addition, these tests include OSHA-specific control variables for the occurrence of a strike, the occurrence of a natural disaster, the average hours worked per employee, the share of covered employees in total employees, the average number of employees per covered facility and the number of facilities covered by the OSHA ODI data each year. The reporting requirements under the OSHA ODI initiative changed significantly in 2001, hence in line with prior literature (e.g., Caskey and Ozel 2017), the tests only include workplace injury data starting from 2002. Standard errors are clustered at the cohort-firm level and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	OSHA Injury Cases							
Sample	Full				Restricted			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SOX 404 Adoption	**0.126****	**0.034*****	**0.034*****	**0.086*****	**0.113**	**0.029***	**0.030****	**0.089****
	(0.062)	**(0.011)**	**(0.011)**	**(0.030)**	**(0.080)**	**(0.015)**	**(0.014)**	**(0.040)**
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,604	3,873	3,873	3,763	1,121	1,270	1,270	1,217
Adj. R-sq.	0.913	0.825	0.825	0.683	0.890	0.779	0.778	0.688

Table 4: Cross-Sectional Evidence on External Scrutiny around SOX 404

This table analyzes cross-sectional variation in the results tabulated in Table 2 using a stacked linear regression design. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. The sample is partitioned into two distinct groups (high vs. low external scrutiny) by interacting the independent variable of interest with a binary indicator variable for capital-market scrutiny (Columns 1 and 3, proxied by an indicator for whether the firm issues debt and/or equity in the pre-treatment period), and creditor scrutiny (Columns 2 and 4, proxied by a median split on the average pre-treatment period leverage ratio). Columns 1 and 2 (3 and 4) use the full (size-restricted) sample. All regressions include cohort-specific firm and year fixed effects, as well as firm-level controls. Standard errors are clustered at the cohort-firm level and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively. P-values (one-sided) from F-tests assessing the statistical significance of the differences across coefficients of interest are reported.

Dependent Variable	Nonfinancial Violation Propensity			
Sample	Full		Restricted	
Splitting Variable	Capital Issuance	Leverage	Capital Issuance	Leverage
Variables	(1)	(2)	(3)	(4)
SOX 404 Adoption [High External Scrutiny]	**0.030*** **	**0.024*** **	**0.024**	**0.029*** **
	(0.009)	**(0.008)**	**(0.015)**	**(0.011)**
SOX 404 Adoption [Low External Scrutiny]	0.012**	0.011**	0.010	-0.001
	(0.005)	(0.005)	(0.006)	(0.005)
p-value H_1: $\beta_{High} > \beta_{Low}$	*.044*	*.082*	*.195*	*.006*
Firm Controls	Yes	Yes	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	26,999	26,236	17,576	16,906
Adj. R-sq.	0.581	0.580	0.544	0.539

Table 5: Cross-Sectional Evidence on Internal Frictions

This table analyzes cross-sectional variation in the results tabulated in Table 2 using a stacked linear regression design. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. The sample is partitioned into two distinct groups (high vs. low internal frictions) by interacting the independent variable of interest with a binary indicator variable for low liquidity (Columns 1 and 4, proxied by a median split on the average pre-treatment cash ratio of the firm), low integrity culture (Columns 2 and 5, proxied by a median split on the average pre-treatment wordcount of integrity culture keywords (as identified by Li et al. (2021)) in the firms' 10-K reports), and firm dispersion (Columns 3 and 6, proxied by a median split on the firm-level pre-treatment dispersion of sales across reported segments). Columns 1-3 (4-6) use the full (size-restricted) sample. All regressions include cohort-specific firm and year fixed effects, as well as firm-level controls. Standard errors are clustered at the cohort-firm level and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively. P-values (one-sided) from F-tests assessing the statistical significance of the differences across coefficients of interest are reported.

Dependent Variable	Nonfinancial Violation Propensity					
Sample	Full			Restricted		
Splitting Variable	Liquidity	Integrity Culture	Segments Dispersion	Liquidity	Integrity Culture	Segments Dispersion
Variables	(1)	(2)	(3)	(4)	(5)	(6)
SOX 404 Adoption [High Frictions]	**0.027*** **	**0.021*** **	**0.021*** **	**0.026** **	**0.025*** **	**0.019** **
	(0.008)	**(0.008)**	**(0.006)**	**(0.012)**	**(0.009)**	**(0.008)**
SOX 404 Adoption [Low Frictions]	0.009*	0.019***	0.012*	0.002	0.007	0.006
	(0.004)	(0.006)	(0.007)	(0.004)	(0.007)	(0.008)
p-value H_1: $\beta_{High} > \beta_{Low}$	*.023*	*.426*	*.162*	*.027*	*.066*	*.117*
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	26,999	22,979	25,092	17,576	14,098	15,797
Adj. R-sq.	0.581	0.580	0.580	0.545	0.537	0.546

Table 6: Cross-Sectional Evidence on SOX 404 Exposure

This table analyzes cross-sectional variation in the results tabulated in Table 2 using a stacked linear regression design. The dependent variable is the logarithm of audit fees (Column 1), the logarithm of one plus the wordcount of SOX-related expressions from firms' 10-K filings (Column 2), the exposure to SOX-related topics measured based on earnings call discussions in accordance with Hassan et al. (2019) (Column 3), or an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise (Columns 4-9). The sample is partitioned into two distinct groups (high vs. low SOX404 exposure) by interacting the independent variable of interest with a binary indicator variable for a large increase in audit fees pre vs. post SOX 404 adoption (Columns 4 and 7), high SOX404 related 10-K disclosure (Columns 5 and 8, proxied by a median split on the pre-treatment wordcount), and large exposure to SOX-related topics measured based on earnings call discussions (Columns 6 and 9, proxied by a median split on the pre-treatment exposure). Columns 1-6 (7-9) use the full (size-restricted) sample. All regressions include cohort-specific firm and year fixed effects, as well as firm-level controls. Standard errors are clustered at the cohort-firm level and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively. P-values (one-sided) from F-tests assessing the statistical significance of the differences across coefficients of interest are reported.

Dependent Variable	Audit Fees	Wordcount	Earnings Call	Nonfinancial Violation Propensity					
Sample	Full			Full			Restricted		
Splitting Variable				Audit Fees	10-K	Earnings Call	Audit Fees	10-K	Earnings Call
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SOX 404 Adoption	0.650***	0.950***	0.004**						
	(0.014)	(0.024)	(0.002)						
SOX 404 Adoption [High SOX404 Exposure]				**0.026*** **	**0.031*** **	**0.024*** **	**0.021*** **	**0.010**	**0.023***
				(0.006)	**(0.007)**	**(0.009)**	**(0.008)**	**(0.008)**	**(0.012)**
SOX 404 Adoption [Low SOX404 Exposure]				0.001	0.002	0.005	-0.001	0.002	0.001
				(0.007)	(0.010)	(0.011)	(0.009)	(0.009)	(0.013)
p-value H_1: $\beta_{High} > \beta_{Low}$				*.002*	*.011*	*.070*	*.029*	*.272*	*.096*
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,194	18,021	14,308	23,617	17,163	12,633	15,197	9,130	5,856
Adj. R-sq.	0.937	0.772	0.598	0.583	0.557	0.551	0.547	0.512	0.519

Table 7: Internal Control Weaknesses and Additional SOX 404 Adoptions

Panel A: Remediation of Internal Control Weaknesses

This table reports the estimation results from linear regressions. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. Columns 1-3 report results from a stacked regression including cohort-specific firm fixed effects. Column 1 includes cohort-specific year fixed effects, while Column 2 (3) includes cohort-specific state-year (industry-year) fixed effects. Column 4 reports results from a staggered difference-in-differences regression and includes firm and year fixed effects. All regressions include firm-level control variables. Standard errors are clustered at the cohort-firm level (firm level in Column 4) and reported in parentheses below the coefficients. The sample is limited to a three-year window around the first disclosure of a material internal control weakness (the sample includes internal weakness disclosures that persist for more than one year and include more than one item). The independent variable of interest is an indicator variable that is equal to one if the firm-year is a remediation year and zero otherwise. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Nonfinancial Violation Propensity			
Variables	(1)	(2)	(3)	(4)
ICW Remediation	**0.030****	**0.028****	**0.030****	**0.024****
	(0.013)	**(0.013)**	**(0.013)**	**(0.012)**
Firm Controls	Yes	Yes	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes	Yes	No
Cohort x Year Fixed Effects	Yes	No	No	No
Cohort x State-Year Fixed Effects	No	Yes	No	No
Cohort x Industry-Year Fixed Effects	No	No	Yes	No
Firm Fixed Effects	No	No	No	Yes
Year Fixed Effects	No	No	No	Yes
Observations	157,661	157,661	157,661	35,123
Adj. R-sq.	0.534	0.534	0.537	0.500

Panel B: Additional SOX 404 Adopters

This table reports results using a stacked linear regression design. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. Column 1 includes all treatment cohorts from 2004 to 2022. Column 2 focuses on treatment cohorts after 2007. All models include cohort-specific firm and year fixed effects, as well as firm-level control variables. Standard errors are clustered at the cohort-firm level and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Nonfinancial Violation Propensity	
Sample	All cohorts	Cohorts >2007
Variables	(1)	(2)
SOX 404 Adoption	**0.010*****	**0.007***
	(0.003)	**(0.004)**
Firm Controls	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes
Observations	122,091	78,304
Adj. R-sq.	0.605	0.619

Table 8: Robustness Tests

Panel A: Excluding Accidents

This table reports results using a stacked linear regression design. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. Column 1 uses the full sample and Column 2 uses the size-restricted sample. Firms that report workplace injury cases or fatalities in the post adoption period, as well as firms for which we do not obtain data from the OSHA ODI database are excluded from the sample in this test. The tests include cohort-specific firm and year fixed effects, as well as firm-level control variables. Standard errors are clustered at the cohort-firm level and are reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Nonfinancial Violation Propensity	
Sample	Full	Restricted
Variables	(1)	(2)
SOX 404 Adoption	**0.029****	**0.038***
	(0.012)	**(0.019)**
Firm Controls	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes
Observations	6,336	3,141
Adj. R-sq.	0.590	0.593

Panel B: Changes in OSHA Inspections

This table reports results using a linear regression design. The number of OSHA inspections (as well as those triggered by accidents), detected violations in these inspections, and assigned penalties for these violations are aggregated by industry (two-digit SIC-code level), state and year. The dependent variable in Columns 1 and 2 is the natural logarithm of the number of inspections. In Columns 3 and 4, the dependent variable is the number of detected violations scaled by the number of inspections. The dependent variable in Columns 5 and 6 is the dollar amount (in $k) of penalties assigned for the detected violations scaled by the number of inspections, and in Columns 7 and 8 it is the number of inspections triggered by accidents scaled by total unplanned inspections. The sample period is limited to a six-year window around 2004. All regressions include state and industry fixed effects. In Columns 2, 4, 6, and 8, the *Post 2004* indicator is interacted with an indicator capturing industries with high initial SOX 404 compliance (median split based on the two-digit SIC-code level share of firms complying with SOX 404 by 2006). These regressions also include year fixed effects. Standard errors are clustered at the industry level and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Inspections		Detections		Penalties		Accidents	
Sample	State-Industry-Year							
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post 2004	**-0.003**		**0.013**		**-0.012**		**0.002**	
	(0.019)		**(0.019)**		**(0.044)**		**(0.003)**	
Post 2004 x High SOX404 Compliance		**0.021**		**-0.036**		**-0.059**		**0.008**
		(0.038)		**(0.037)**		**(0.087)**		**(0.006)**
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	13,873	13,873	13,873	13,873	13,873	13,873	13,873	13,873
Adj. R-sq.	0.760	0.760	0.249	0.249	0.205	0.205	0.220	0.220

Panel C: Count and Penalties

This table reports results using a stacked linear regression design. The dependent variable is the natural logarithm of one plus the number of violations per firm and year (Columns 1 and 3) or the natural logarithm of one plus the dollar amount of penalties per firm and year (Columns 2 and 4). Columns 1 and 2 (3 and 4) use the full (size-restricted) sample. The tests include cohort-specific firm and year fixed effects, as well as firm-level control variables. Standard errors are clustered at the cohort-firm level and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Count	Penalty	Count	Penalty
Sample	Full		Restricted	
Variables	(1)	(2)	(3)	(4)
SOX 404 Adoption	**0.021*** **	**0.237*** **	**0.012** **	**0.131** **
	(0.005)	**(0.053)**	**(0.005)**	**(0.059)**
Firm Controls	Yes	Yes	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	26,999	26,999	17,576	17,576
Adj. R-sq.	0.733	0.604	0.692	0.570

Panel D: Sample Restrictions around the Threshold

This table reports results using a stacked linear regression design. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. The test in Column 1 (2) excludes firms with a pre-treatment public float above $120 (100) million. In Column 3 (4), firms with a pre-treatment public float above $150 million and below $25 (50) million are excluded. The sample used in Column 5 excludes firms with a pre-treatment public float below $50 million and above $100 million. The tests include cohort-specific firm and year fixed effects, as well as firm-level control variables. Standard errors are clustered at the cohort-firm level and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Nonfinancial Violation Propensity				
Sample	PF<120	PF<100	25<PF<150	50<PF<150	50<PF<100
Variables	(1)	(2)	(3)	(4)	(5)
SOX 404 Adoption	**0.012***	**0.013***	**0.012** **	**0.017** **	**0.018** **
	(0.006)	**(0.007)**	**(0.006)**	**(0.007)**	**(0.008)**
Firm Controls	Yes	Yes	Yes	Yes	Yes
Cohort x Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Cohort x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	12,147	11,658	5,319	3,237	2,168
Adj. R-sq.	0.559	0.569	0.418	0.377	0.378

Panel E: Alternative Fixed Effects

This table reports results using a stacked linear regression design. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. Columns 1-4 (5-8) use the full (size-restricted) sample. Starting from Column 2 (6), the tests include firm-level control variables and cohort-specific firm fixed effects. The tests in Columns 2 and 6 (3 and 7) include state-year (industry-year) fixed effects. Columns 4 and 8 include both state-year and industry-year fixed effects. All these fixed effects interactions are cohort specific. Standard errors are clustered at the cohort-firm level and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Nonfinancial Violation Propensity							
Sample	Full				Restricted			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SOX 404 Adoption	**0.027*** (0.005)**	**0.020*** (0.005)**	**0.017*** (0.005)**	**0.017*** (0.005)**	**0.019*** (0.006)**	**0.013** (0.006)**	**0.012** (0.006)**	**0.012** (0.006)**
Firm Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Cohort x Firm Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Cohort x State-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Cohort x Industry-Year Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Observations	26,999	24,859	24,859	24,859	17,576	15,654	15,654	15,654
Adj. R-sq.	0.070	0.577	0.583	0.582	0.020	0.531	0.541	0.540

Panel F: Alternative Research Designs

This table reports results from linear regressions. The dependent variable is an indicator variable equal to one in case of the occurrence of at least one nonfinancial violation for a given firm in a year, and zero otherwise. Columns 1-5 (6-10) use the full (size-restricted) sample. Columns 1 and 6 report the results using a staggered difference-in-differences design, retaining firm-years for the period 2001 to 2006. Treatment firms are defined based on actual SOX 404 adoption in the years 2004 or 2005. In Columns 2 and 7, treatment status is assigned based on 2002 public float. In line with prior literature (e.g., Albuquerque and Zhu 2019; Iliev 2010), all firms whose public float in 2002 surpasses $75 million are defined as treated. The tests in Columns 2 and 7 are estimated based on a difference-in-differences regression with 2004 as first adoption year. These regressions include firm and year fixed effects, as well as firm-level controls. In the remaining columns, results are based on a stacked linear regression design, including cohort-specific firm and year fixed effects, as well as firm-level controls. Columns 3 and 8 use an entropy-balanced sample with entropy weights calculated based on the firm-level control variables. The tests in Columns 4 and 9 (5 and 10) exclude all observations from the year 2007 (the years 2001 and 2007). Standard errors are clustered at the firm level (cohort-firm level for stacked regressions) and reported in parentheses below the coefficients. All variables are defined in Appendix B. All continuous variables are winsorized at the 1/99% level to mitigate the influence of outliers. *, **, *** indicate significance at the two-tailed 10 percent, 5 percent, and 1 percent levels, respectively.

Dependent Variable	Nonfinancial Violation Propensity									
Sample	Full					Restricted				
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SOX 404 Adoption	**0.014*** (0.005)**	**0.027*** (0.005)**	**0.019*** (0.007)**	**0.018*** (0.005)**	**0.016*** (0.005)**	**0.017*** (0.006)**	**0.026** (0.013)**	**0.014* (0.008)**	**0.013** (0.006)**	**0.015** (0.007)**
Specification	Staggered	Public Float 2002	Stacked + EBW	Stacked + excl. 2007	Stacked + excl. 2007 & 2001	Staggered	Public Float 2002	Stacked + EBW	Stacked + excl. 2007	Stacked + excl. 2007 & 2001
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	No	No	No	Yes	Yes	No	No	No
Year Fixed Effects	Yes	Yes	No	No	No	Yes	Yes	No	No	No
Cohort x Firm Fixed Effects	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Cohort x Year Fixed Effects	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Observations	18,840	17,078	26,999	25,195	22,691	9,213	6,158	17,576	16,288	14,765
Adj. R-sq.	0.622	0.614	0.565	0.581	0.578	0.554	0.556	0.535	0.538	0.535