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The Effect of Governance Reforms on Financial Reporting Fraud

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ABSTRACT

In response to financial reporting scandals, Congress and the securities exchanges mandated increases in board and audit committee independence and banned most non-audit services. We exploit these exogenous shocks to examine whether these governance reforms reduced financial reporting fraud. Comparing firms forced to comply with the reforms to firms already in compliance, we find that mandated increases in overall board independence significantly reduced the rate of fraud, while mandating a fully independent audit committee had a weaker effect. Further, banning non-audit services did not reduce the incidence of fraud.

Keywords: Fraud, sarbanes-oxley, corporate governance, board independence.

JEL Codes: K22, K41, M41.

1 Introduction

Financial reporting scandals such as Enron and WorldCom ushered in a wave of corporate governance reforms in the early 2000s. These reforms, mandated by the Sarbanes-Oxley Act of 2002 (SOX) and major U.S. stock exchanges, required (among other things) independence for all audit committee

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members and a majority of directors on the full board. In addition, most non-audit services provided by auditors to their audit clients were banned. These exogenous regulatory shocks allow us to investigate whether increased board independence and decreased non-audit fees reduce the risk of financial reporting fraud. We focus on fraud because several high-profile frauds led to these regulatory reforms (e.g., Klein, 2002a; Lucas, 2004).

Our analysis is important for three reasons. First, the reforms we study remain controversial. Supporters argue that a lack of independent boards and auditors contributed to financial reporting scandals in the late 1990s and early 2000s, and the reforms were therefore necessary to increase oversight of firm managers (e.g., Faleye *et al.*, 2011). Others argue the reforms were largely "window dressing" (e.g., Romano, 2005).

Second, pre-reform studies find mixed evidence on the relation between board or auditor independence and financial reporting quality, including fraud. Some studies find that more independent boards are associated with higher reporting quality (e.g., Klein, 2002b) and lower fraud risk (e.g., Beasley, 1996; Dechow *et al.*, 1996), while others do not (e.g., Larcker *et al.*, 2007; Abbott *et al.*, 2000; Persons, 2005). Results also conflict regarding audit committee independence and non-audit fees (see, e.g., Farber, 2005; Abbott *et al.*, 2000; Park and Shin, 2004; Kinney *et al.*, 2004; Ferguson *et al.*, 2004, Frankel *et al.*, 2002). Importantly, no studies investigate whether the reforms we examine actually curbed financial reporting fraud. Given the mixed findings from prior work, it is not clear one should expect the reforms to have been effective in this regard.

Third, prior studies correlate endogenous variation in board structure and non-audit fees with the incidence of financial reporting fraud. The endogenous nature of governance characteristics makes it difficult to establish causal inferences (Armstrong *et al.*, 2010). For example, if one finds that independent boards are associated with lower fraud risk, it could mean that independent directors deter financial reporting fraud, or it could mean that firms with good reporting quality select or even attract more independent directors. Our study largely avoids these concerns because the shifts in board structure and non-audit fees that we examine were mandatory and thus exogenous.

We follow the approach of Duchin *et al.* (2010) and focus on firms that were not already in compliance with the reforms when they were passed. These firms were the target of the reforms and either: (a) did not have a fully independent audit committee, (b) did not have a majority independent board, (c) procured non-audit services from their auditor, or (d) had some combination of all three prior to the reforms. Changes in the above factors for these firms after the reform enactment were material on average and non-voluntary (i.e., exogenous) in nature. We examine whether these exogenous shifts are associated with a

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decrease in financial reporting fraud from the pre-reform period of 1996–1999 to the post-reform period of 2004–2007 among firms that were not already in compliance. We refer to these firms as "non-compliers."

Because the incidence of fraud may have changed due to factors unrelated to the reforms, our analysis requires a benchmark sample for comparison. Again, similar to Duchin *et al.* (2010), we select benchmark firms that were already in compliance with the mandates prior to the reforms (e.g., firms that already had a fully independent audit committee). These firms had no substantive changes in board independence or non-audit service procurement after the reforms. We refer to these firms as "compliers."

We conduct a "differences in differences" analysis by comparing the change in the rate of financial reporting fraud for non-compliers to that of compliers. If the lack of board and auditor independence contributes to financial reporting fraud then non-compliers should experience an incremental reduction in the incidence of financial reporting fraud (a "treatment effect") from the pre-reform to the post-reform period compared to compliers.

We identify compliers and non-compliers using governance data from RiskMetrics and non-audit service fee data from Audit Analytics. Our proxy for financial reporting fraud includes Accounting and Auditing Enforcement Releases (AAERs) that allege fraud, and settled class action securities lawsuits that allege fraudulent GAAP violations. If the financial statements for a firm-year are alleged to be fraudulently misstated by an AAER or a settled shareholder suit, we classify that firm-year as an instance of financial reporting fraud.

In univariate tests, for the full board and the audit committee, we find that the decrease in the rate of fraud from the pre-reform to the post-reform period is significantly greater for non-compliers firms than it is for compliers. The reduction in the rate of fraud from the pre-reform to the post-reform period is 2.66 percentage points greater for firms that were forced to adopt a majority independent board than it was for firms that had a majority independent board prior to the reform. Similarly, the reduction in the rate of fraud between the pre- and post-reform periods is 1.90 percentage points more for firms that were forced to have a fully independent audit committee than it was for firms that already had a fully independent audit committee prior to the reform. However, we find no significant difference for firms that purchased non-audit services. In multivariate tests that jointly examine the incremental effect of all three non-compliance measures and control for shifts in fraud incentives, we find that requiring the majority of the board to be independent is the dominant factor in fraud reduction. This result is robust to a variety of alternative specifications, including firm-level regressions, panel regressions with fixed effects, and a propensity matching approach to pair similar complier and non-complier firms. Overall, the full board reforms seem to have reduced fraud, while the other reforms either had weaker or no effect.

It is unlikely that other contemporaneous factors such as financial statement certification, increased audit and regulatory scrutiny, or internal control audits drive the above findings. We assess the threat of other contemporaneous factors by examining a set of firms that did not comply with the board independence requirements in the pre-reform period *and* the post-reform period. These firms were either exempt from the independence requirements, used "gray" directors of questionable independence, or had staggered boards, so they did not receive a "treatment." Although these firms were subject to financial statement certification, internal control audits, and potential scrutiny from being "non-compliers," these firms did not experience a drop in fraud. This finding suggests that the exogenous, mandated shift in board independence, not other contemporaneous reforms, led to the fraud reduction we observe for non-compliers.

We also perform a battery of additional tests to strengthen inferences for the board results. We find no evidence that internal control audits, mean reversion in fraud or changes in detection levels drive our results. To save space, we report the results of these additional tests in an Internet Appendix.

Our study contributes to the literature on independent directors and audit committees. Recent work exploiting the exogenous changes to the level of independent directors finds that independent directors improve firm performance in strong information environments (Duchin *et al.*, 2010) and improve corporate transparency, as measured by changes in information asymmetry, disclosure, and information intermediation (Armstrong *et al.*, 2014). To our knowledge, we provide the first evidence that an exogenous increase in board independence is associated with a reduction in financial reporting fraud. Given some of the mixed results from prior studies in this area, as well as concerns about endogeneity and causal inferences raised by some scholars, the evidence we provide is important. In addition, our results highlight the importance of overall board independence vis-à-vis audit committee independence in reducing fraud. This study also contributes to the literature on non-audit services and financial reporting fraud. Our tests indicate that the banning of most non-audit services under SOX did little to reduce the risk of financial reporting fraud.

From a policy perspective, our findings inform policymakers and regulators about the effectiveness of these reforms. Our findings on board independence are somewhat encouraging. However, we caution that we provide only one aspect of the story. To the extent that having potentially more knowledgeable, non-independent directors on the board yielded net benefits to firms, the reforms eliminated such benefits.

2 Prior Literature and Background

A number of prior studies (e.g., Fama and Jensen, 1983) recognize independent directors as important monitors of management. Specifically, independent

directors (on the full board and/or the audit committee) "have incentives to carry out their monitoring tasks and not to collude with top managers to expropriate stockholder wealth, so the inclusion of [independent] directors increases the board's ability to monitor top management effectively in agency settings arising from the separation of corporate ownership and control" (Beasley, 1996, p. 444). Similar arguments are made for auditors with respect to constraining management (Jensen and Meckling, 1976), as shown by the long history of required independent audits under U.S. securities laws.

2.1 Studies on Fraud and Board and Audit Committee Independence

Prior studies reach mixed conclusions regarding the effect of board independence and audit committees on fraud (e.g., Armstrong *et al.*, 2010). Using a matched-pair design with financial reporting fraud firms and non-fraud firms, Beasley (1996) finds that the proportion of independent directors is negatively associated with the likelihood of financial reporting fraud. Further, he finds that the presence of an audit committee does not incrementally affect the incidence of fraud, nor does it affect the relation between board independence and fraud.[1] Dechow *et al.* (1996) and Farber (2005) also examine fraud firms and matched non-fraud firms and find that board independence is negatively associated with financial reporting fraud. However, unlike Beasley (1996), Dechow *et al.* (1996) find that the presence of an audit committee is negatively associated with financial reporting fraud, while Farber (2005) finds that the number of independent directors on the audit committee is not. Abbott *et al.* (2000) find no relation between overall board independence and financial reporting fraud, but they do find that independent audit committees that meet frequently are associated with lower fraud likelihood.[2]

[1] Firms in Beasley's sample period were not required to have an audit committee.

[2] Studies also reach mixed conclusions using other proxies for financial reporting quality. Klein (2002b) examines the association between board and audit committee independence and the magnitude of abnormal accruals. She finds, using both a levels and changes analysis, that more board and audit committee independence is associated with lower abnormal accruals. She also finds that the presence of a *fully independent* audit committee has no effect on the magnitude of abnormal accruals. In a related study, Larcker *et al.* (2007) examine the association between earnings quality and 14 principal components extracted from governance variables. Two of these factors are related to the influence and presence of board insiders on both the full board and audit committee. Inconsistent with the findings of Klein (2002b), Larcker *et al.* (2007) find no significant association between these independence components and abnormal accruals. Using Canadian data, Park and Shin (2004) also find no significant association between abnormal accruals and board independence. Further, Larcker *et al.* (2007) find no relation between these independence components and restatements, although they do not distinguish between errors and frauds.

2.2 Studies on Fraud and Non-Audit Services

There is less evidence relating non-audit services and fraud. Ferguson *et al.* (2004) use UK data and find that non-audit services are linked to higher rates of restatements and regulatory investigations related to fraud. Other studies investigate whether restatements or regulatory investigations are linked to non-audit services. Both Kinney *et al.* (2004) and Raghunandan *et al.* (2003) find no positive link between restatements and non-audit services. However, these studies do not distinguish between error-based and fraud-based restatements.[3]

2.3 Overview of Regulatory Reforms

In the late 1990s, the Securities and Exchange Commission (SEC) became very concerned with financial reporting in the United States (see Levitt, 1998). The SEC attempted several reforms to curb financial reporting misconduct, including banning non-audit services provided by the firm's primary auditor, but were not wholly successful. However, these efforts did lead to initial audit committee reforms for firms that traded on the NYSE and NASDAQ stock exchanges in December 1999, requiring these firms to have fully independent audit committees (Klein, 2002b). In proposing the reform, the NYSE specifically noted that the rule was consistent with its requirement to "prevent fraudulent and manipulative acts and practices" (SEC, 1999).[4]

Not long after this initial reform, some of the largest financial reporting scandals in U.S. history were revealed. Enron's collapse led to market turmoil, and the ensuing collapse of WorldCom contributed to passage of SOX (Fass, 2003). In 2002, SOX codified the requirement that firms have a fully independent audit committee and banned many types of non-audit services provided by a firm's auditor.[5] Senator Sarbanes stated in an interview after SOX was passed that a series of hearings by the Senate Banking Committee yielded a consensus that a "lack of auditor independence" and "weak corporate governance procedures" contributed to the publicized financial reporting scandals (Lucas, 2004).

[3] A number of studies examine the relation between non-audit fees and abnormal accruals. Some studies, such as Frankel *et al.* (2002) and Larcker and Richardson (2004), find a positive association between the *proportion* of non-audit fees paid by firms and the magnitude of abnormal accruals. However, Ashbaugh *et al.* (2003) and Larcker and Richardson (2004) do not find a positive association between abnormal accruals and the *level* of non-audit fees. Further, Chung and Kallapur (2003) find no association between the magnitude of abnormal accruals and the importance of non-audit fees to an auditor at the national or local level.

[4] This rule also contained an exception if the board believed it was in the firm's best interests to not have a fully independent audit committee (e.g., Klein, 2002a).

[5] Other SOX-era reforms included other changes, such as the requirement for executive certification of financial statements and internal control reporting. We focus on these reforms because, as we explain more fully in the next section, our empirical tests require a set of benchmark firms already in compliance with the reforms.

In 2003, at the request of the then SEC Chairman, Harvey Pitt, the major stock exchanges passed more stringent audit committee guidelines and a new requirement that the majority of the board be comprised of independent directors, with the NYSE again noting its anti-fraud goals (SEC, 2003b). The clear goal of these independence reforms was to curb financial reporting fraud. However, many argued that the reforms were largely symbolic and a form of "window dressing." Romano (2005), for example, calls SOX "quack corporate governance" resulting from a desire by lawmakers to act quickly and appear responsive to upcoming elections in the face of increasing public concern over financial reporting scandals. Romano (2005) characterizes many of the governance and auditing reforms as more than just harmless window dressing, as they may impose substantial costs on firms. Other legal scholars (e.g., Clark, 2005; Ribstein, 2005) expressed similar views. In the auditing literature, DeFond and Francis (2005) were highly critical of the audit-related reforms, particularly the ban on certain non-audit services.

2.4 Studies on the Effect of SOX on Reporting Quality

Surprisingly, there is no work directly examining the relation between the reforms above and financial reporting fraud. However, two recent studies that examine the effect of SOX on financial reporting are relevant. Cohen *et al.* (2008) find that accrual-based earnings management decreased following SOX, while real activities management increased. In the study most similar to ours, Chen *et al.* (2015) examine whether the mandated increase in board independence among non-compliers reduced the magnitude of abnormal accruals. Inconsistent with what one might expect from the Cohen *et al.* (2008) findings, Chen *et al.* (2015) find no significant reduction in earnings management among firms forced to increase their board independence. However, they do find some reduction among the subset of non-compliers with low information acquisition costs (e.g., firms with high analyst coverage).

2.5 Summary

In general, the result of prior studies examining the link between earnings manipulation and board independence, audit committee independence, non-audit services, and SOX-era reforms are mixed and appear to be sensitive to research design choices. Against this backdrop, there are two important reasons this study is needed. First, the shifts in board independence, audit committee independence, and non-audit service provision in our study are largely exogenous since they were mandated by either Congress or the securities exchanges. In contrast, prior studies (with the exception of Chen *et al.*, 2015), relate financial reporting quality to endogenous variation in board independence, audit committee independence, and non-audit service provision.

As Armstrong *et al.* (2010, p. 192) note in their recent review of the corporate governance literature, the endogenous nature of these characteristics makes it difficult to establish causal inferences in prior research. We do not know from these studies, for example, whether independent directors constrain manipulative financial reporting choices, or whether firms with good financial reporting quality select or even perhaps attract independent directors.

Relatedly, it is also not clear that a majority of independent directors is an efficient choice for all firms, and thus it is possible that forcing firms into a "one size fits all" regime may actually be harmful. This could be possible if, for instance, certain firms have operating environments that require more specialized skills to understand, making affiliated directors better monitors. Prior studies are subject to endogeneity and thus cannot establish whether the effects of some firms' governance choices in equilibrium can be generalized to other firms that made different choices. We thus do not know whether firms that chose not to have a majority independent board would experience the same benefits from board independence (i.e., a reduced rate of fraud) as firms that voluntarily chose to have majority independent boards. As noted, one reason such firms may not have majority independent boards is because the benefits of doing so may be small or even non-existent. Our study largely avoids these endogeneity concerns by utilizing the exogenous shock provided by the reforms surrounding SOX.

Second, unlike prior SOX-related studies, we focus on observable financial reporting fraud to measure financial reporting manipulation. We do so because the reforms we examine were passed specifically in response to publicized financial reporting frauds. Our findings thus speak more directly to the root cause of the reforms, than do the findings of other studies that use measures like abnormal accruals.[6]

3 Sample and Primary Empirical Tests

3.1 Sample

We use RiskMetrics for director independence and audit committee data, Audit Analytics for audit and non-audit fees, Compustat for financial data, and CRSP for stock price data. We break the sample period into two sub-samples. The pre-reform period includes fiscal years 1996–1999, while the post-reform period includes 2004–2007. We omit the years 2000–2003 when the reforms were implemented. We require director independence and audit committee data in

[6]Kothari *et al.* (2005) and Dechow *et al.* (2012) and Hennes *et al.* (2008), among others, point out that it is difficult to parse out the effect of earnings management on accruals from growth, performance, and other factors. We avoid these issues by using the incidence of fraud rather than less direct proxies of earnings management.

1999 and for each year a firm is in the sample in the post-reform period, data on audit and non-audit fees in 2001, as well as financial and market data to construct the requisite control variables. Since we need measures of compliance and fraud incidence in the post-reform period, we exclude all firms that do not survive from the pre-reform to the post-reform period.[7] In addition, we exclude firm-years in the post-reform period that do not have a majority of independent directors on the board or do not have a fully independent audit committee.[8] The final sample includes 877 firms with requisite data in both pre- and post-reform periods.

Data availability and our definition of fraudulent financial reporting determine the sample period. Measuring fraud is inexact because firms rarely admit to fraud. Thus, we use two measures from prior research to capture financial reporting fraud. First, we use AAERs by the SEC, as coded by Dechow *et al.* (2011) to involve GAAP violations.[9] Because not all AAERs allege intentional misrepresentations, we read each AAER and include only cases that specifically mention fraud or Rule 10b-5. Second, we use securities class action suits that include allegations of Rule 10b-5 violations involving accounting and were settled under the legal standards of the Private Securities Litigation Reform Act of 1995 (PSLRA) as coded by RiskMetrics in their Securities Class Action Services database. Settlement indicates that the case passed a motion to dismiss, meaning a judge determined that the factual al-

[7]Due to outcome unobservability for non-survivors, it is difficult assess the effect of this survivorship requirement on our main inferences below. However, when we retain non-surviving compliers and non-compliers in the pre-period and assume they have the same average fraud rates as their counterparts in the post-reform period, board non-compliers still experience an incremental drop in fraud ($t = 2.04$, $p < 0.05$; untabulated).

[8]There are at least three potential reasons firms might not be reported as conforming to the requirements in the post-reform period. First, "controlled companies" are exempt from the exchange listing requirement for a majority independent board. Controlled companies are those "of which more than 50% of the voting power is held by an individual, a group or another" (NYSE, 2003). Second, firms with staggered boards were given additional time to comply with the independent director reform. Finally, the RiskMetrics data is coded with a more stringent definition of independence than the legal/exchange definitions. For firms with directors that meet the legal/exchange definition but not the RiskMetrics definition, we can be less sure that they experienced an exogenous shock because their pre-reform data is also likely subject to such bias.

The RiskMetrics database classifies a director as independent when the director is neither affiliated with nor currently employed by the company. As noted by Chhaochharia and Grinstein (2009), RiskMetrics defines an affiliated director as "a former employee of the company or of a majority-owned subsidiary; a provider of professional services—such as legal, consulting, or financial—to the company or an executive; a customer of or supplier to the company; a designee, such as a significant shareholder, under a documented agreement between the company and a group; a director who controls more than 50% of the company's voting power (and thus would not be considered to represent the broader interests of minority shareholders); a family member of an employee; or an employee of an organization or institution that receives charitable gifts from the company."

[9]The Dechow *et al.* (2011) co-author team updated the AAER dataset through the end of calendar year 2010.

legations in the plaintiffs' complaint yielded a strong inference of fraud. Thus, the settlement of the case provides more assurance that a financial reporting irregularity occurred over and above the mere filing of a case (see Donelson *et al.*, 2013).[10]

Under each measure, we use the fiscal years alleged to be misstated in a financial reporting fraud. The sample period starts in 1996 due to the passage of the PSLRA in December 1995. The sample period ends in 2007 in order to allow a lag for SEC investigations to be completed, and for securities litigation cases to be resolved.

3.2 Research Design

3.2.1 Identification Strategy—Measurement of Exogenous Shock

Our research design exploits the exogenous shock provided by the mandated reforms. To measure the exogenous shock, we classify firms as either being in compliance with the reforms prior to their implementation ("compliers") or not in compliance with the reforms prior to their implementation ("non-compliers"). Identifying these two groups of firms is critical in our analysis, as the compliers serve as a "control" sample of firms that did not experience an exogenous shift in board independence, audit committee independence, and non-audit services consumption. Without this control sample, our tests cannot identify the incremental treatment effect experienced by non-compliers as a result of the reforms. For this reason, we cannot evaluate the effectiveness of SOX provisions like the requirement for internal control assessments, because we cannot construct an adequate control sample of firms who complied with this reform prior to its enactment.

We measure firms that were in compliance with the three primary independence requirements prior to their implementation as follows: the majority of the board was composed of independent directors; the entire audit committee was independent; and the firm did not purchase non-audit services that were banned under SOX, such as certain types of information technology design or internal audit outsourcing. Board variables are from RiskMetrics, which codes independent directors and committee membership. We code the indicator variable Non-Comply-Board as one when the majority of a firm's board of directors was not independent in 1999, and zero when the majority of a firm's board directors was independent. We code the independent variable Non-Comply-Audit as one when a firm did not have a fully independent

[10]To ensure that non-meritorious lawsuit outcomes do not drive our results, we re-run primary tests with additional screens for the lawsuits in our sample. Results with respect to independent directors are somewhat stronger when we screen out suits that settled for less than 0.5% of market value of equity, a common measure of meritorious securities litigation outcomes (e.g., Johnson *et al.*, 2007). In addition, primary results hold if we use only SEC enforcement actions as a proxy for fraud.

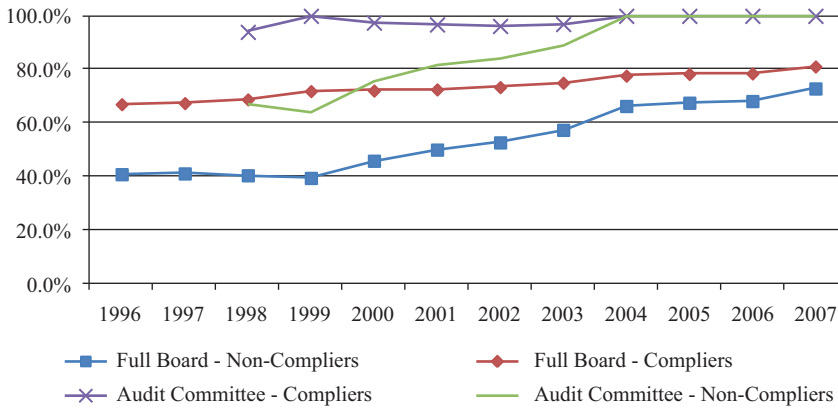

Figure 1: Board Member Independence by Year

Note: This figure provides a graphical depiction of the percentage of independent directors and audit committee members for groups of firms based on whether they were in compliance with the identified corporate governance reforms in 1999. Audit committee data is not available for the full sample period.

audit committee in 1999, and zero when a firm had a fully independent audit committee.

Figure 1 provides a graphical summary of the evolution of the mean full board and audit committee independence separately for compliers and non-compliers between 1996 and 2007. As expected, the firms that were not in compliance with the full board and audit committee independence requirements in 1999 (our measurement year for non-compliance) had a sharp increase in the percentage of independent directors beginning in the year 2000. Some prior studies, such as Duchin and Sosyura (2014), use 2000 as the measurement year for non-compliance because most firms were not required to comply until 2001 based on the timing of their board election. However, the single biggest shift in audit committee independence occurs in 2000, consistent with many firms responding to the new requirements as soon as they were passed. Similarly, the shift in board independence for board non-compliers begins in 2000, consistent with audit committee shifts affecting the entire board, after being largely flat from 1996–1999. These shifts appear to be responses to regulation (rather than voluntary choices by firms) and we thus use the 1999 as our base in order to capture firm's choices prior to regulatory intervention.

The shift in independence is not nearly as noticeable for firms that already had a majority of independent directors on the full board, while firms that had a fully independent audit committee in 1999 show very little variation. This supports the use of the governance reforms as an exogenous shock for non-compliers, coupled with very little change for compliers. Further, the lack

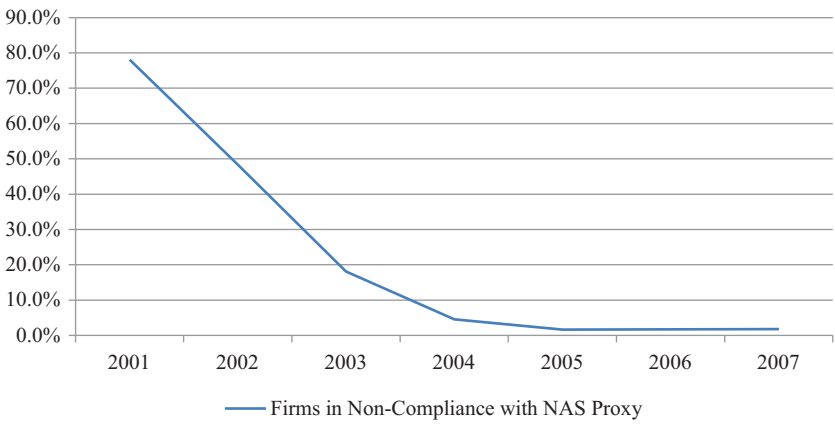

Figure 2: Non-Audit Services Ratio over Time

Note: This figure provides a graphical depiction of the number of firms that purchased financial information systems design and implementation related fees and other/miscellaneous categories of non-audit services of at least 10% of the firm's audit and audit-related fees in a given year, as coded by Audit Analytics. We use the 10% cutoff as a proxy for services that were not banned by the Sarbanes-Oxley Act of 2002.

of an increase in the percentage of independence prior to 1999 reinforces that the reforms caused the increase in board and audit committee independence.[11]

Measuring pre-reform compliance with the non-audit services ban is more challenging because firms did not report non-audit service fees in direct relation to services that were later banned by SOX. However, due to the smaller effect of SOX prohibitions on areas such as tax and benefit consulting, we use the coding by Audit Analytics of the financial information systems design and implementation related fees and other/miscellaneous categories of non-audit services ("IT/Other") to isolate firms that were likely purchasing services that were later banned.

Figure 2 provides a graphical summary of the proportion of firms that were purchasing IT/Other non-audit services equal to at least 10% of the firm's audit and audit-related fees. In 2001, our primary year of measurement prior to the reforms being passed in 2002, 78.1% of firms purchased IT/Other non-audit services equal to at least 10% of audit and audit-related fees. This proportion began to fall in 2002, and was less than 2% by 2005, where it remained in 2006 and 2007. Thus, we implement this 10% cutoff to estimate

[11] Audit committee data is not available for the early years of the sample period (Duchin *et al.*, 2010). However, the 1998 and 1999 data points are very similar for non-compliers with respect to the audit committee, again consistent with the reforms as a shock to firm governance. We measure board variables in 1999 (rather than each year from 1996–1999) to preserve sample size. If we exclude the relatively few firms who changed their board structure between 1996 and 1999 (because these firms are not clearly compliers or non-compliers), results are slightly stronger.

which firms were purchasing any non-audit services from their auditor that were later banned. We code the independent variable Non-Comply-NAS as one when a firm purchased IT/Other non-audit services of at least 10% of audit and audit-related fees in 2001, and zero otherwise.[12]

3.2.2 Empirical Strategy

We use two complementary designs, both of which are fundamentally differences-in-differences specifications. First, we use changes between the pre-reform period and the post-reform period at the firm level ("changes design"). Second, we use a panel data design. The changes design allows presentation of univariate correlations that are more easily interpretable, and thus aids in the presentation of univariate relations. The panel data design is more amenable to regression specifications, although results are similar with both methods. We describe the changes design first as it involves more variable measurement issues.

Our primary measure for the changes design is the change in fraud rate from the pre- to the post-reform period, calculated by performing the following steps. First, we identify fiscal years in which firms allegedly committed fraud by identifying when an AAER alleges that fraud occurred in that year or when the fiscal year is part of the class period of a settled financial reporting-related securities class action lawsuit.[13] Second, after identifying the years in which firms did and did not commit fraud, we calculate the pre- and post-reform fraud rate for each firm. For example, a complier firm with financial reporting fraud in 1996 and 1997 and no fraud in 1998 and 1999 would have a fraud rate of 50% for the pre-reform period, with two of the four possible years coded as fraud years and the other two years coded as non-fraud years.

Third, we measure changes in financial reporting fraud rates from the pre- to the post-reform time period for each firm. Finally, we compare the change in the rate of fraud from the pre- to the post-reform time period among compliers and non-compliers in both univariate and multivariate tests. Our univariate tests use a differences-in-differences design. We measure financial reporting fraud rates for both the compliers and non-compliers in each period, and calculate whether there is a significant difference in the change of financial reporting fraud rates between the two groups from the pre- to the post-reform period. Each firm acts as its own control in the changes design because we require data for each firm in each period.

[12] Because this variable is subject to measurement error, we repeat analyses using alternative cutoffs (50% and 100% of audit fees) and find very similar results. These thresholds are arguably more consistent with intent to ban material non-audit service purchasers.

[13] In addition, this definition allows two effects of the governance changes. First, they could stop frauds from occurring initially. Second, they could shorten the period of the fraud. These two effects were specifically noted by the SEC in approving the exchange rule updates (SEC, 2003a).

3.3 Descriptive Statistics, Correlations and Univariate Results

3.3.1 Descriptive Statistics and Correlations

Table 1 provides descriptive statistics, divided into the pre- versus post-reform periods and compliance versus non-compliance firms. The overall rate of fraud in the whole sample is low at 2.79% in the pre-reform period and 2.64% in the post-reform period. Our sample contains a total of 86 unique frauds (53 in the pre-reform period and 33 in the post-reform period) and 160 fraud-years (97 in the pre-reform period and 63 in the post-reform period).[14]

In addition to our governance and fraud variables, we tabulate results for determinants and incentives to commit fraud, including securities offerings, firm growth, firm size, and leverage (see Dechow *et al.*, 2011; Kim and Skinner, 2012). Offering is an indicator variable set to one if a firm sold debt or equity securities equal to at least 10% of total assets in the current or prior year, and is equal to zero otherwise. BTM is the beginning of the year book-to-market value of equity ratio. Size is beginning of the year total assets. Leverage is long term debt scaled by total assets. Like Fraud, all control variables are averaged across the pre- and post-periods for each firm. Offering, Size and Leverage should be positively associated with fraud, while BTM should be negatively related (see Dechow *et al.*, 2011; Kim and Skinner, 2012). Detailed variable descriptions are in the Appendix. The firms in our sample are large, with mean assets of $5.031 billion ($11.546 billion) in the pre-reform (post-reform) period.[15] The BTM (book-to-market ratio), Offering, and Leverage variables are all similar in both periods.

The mean of the Non-Comply-NAS variable is 0.78, indicating that 78% of firms were not in compliance with our proxy for the Sarbanes-Oxley non-audit services restrictions prior to the legislation passing. The mean of the Non-Comply-Audit variable is 0.43, indicating that 43% of firms did not have fully independent audit committee prior to the mandated governance reforms. Finally, the mean of the Non-Comply-Board variable is 0.27, indicating that 27% of the firms in our sample did not have a majority of independent directors prior to the mandated governance reforms. This is similar to the percentage of firms found to be in compliance in prior work (e.g., Duchin *et al.* (2010) found that 24% of firms were not in compliance in 2000).

Parsing on compliance, most firm characteristics do not significantly differ between compliers and non-compliers. However, firms that had a minority

[14]There are 877 total firms and 6,080 firm-years in the sample (an average of about 7 years each, split between the pre-reform and post-reform periods). The overall fraud rate in the sample is 2.6%, which equals the total fraud-years (160) divided by the total number of firm-years (6,080).

[15]Cooper *et al.* (2008) find that the mean (median) asset growth rate for companies between 1968 and 2002 is 19% (8%). Assuming a growth rate of 10% each year over an eight year period, firms' size would grow by a factor of 2.14. Thus, the change in firm size between our pre- and post-reform periods seems to be reasonable.

	Pre-Reform				Post-Reform				
Variable	N	Mean	Median	Std. Dev.	N	Mean		Median		Std. Dev.
Panel A: Descriptive Statistics for the Pre- and Post-Reform Periods										
Fraud	877	2.79%	0.00%	0.12	877	2.64%		0.00%		0.15
#hline of Unique Frauds	53				33					
# of Fraud Years	97				63					
Size	877	5,031	1,302	8,838	877	11,546	***	2,831	***	21,677
LnSize	877	7.31	7.15	1.57	877	8.13	***	7.94	***	1.56
BTM	877	0.45	0.41	0.29	877	0.47	*	0.44	***	0.27
LnBTM	877	−1.02	−0.95	0.63	877	−0.91	***	−0.83	***	0.58
Offering	877	0.40	0.50	0.38	877	0.36	**	0.25	***	0.39
Leverage	877	0.17	0.16	0.13	877	0.18		0.17		0.13
Non-Comply-NAS	877	0.78	1.00	0.41	877	.		.		.
Non-Comply-Board	877	0.27	0.00	0.45	877	.		.		.
Non-Comply-Audit	877	0.43	0.00	0.50	877	.		.		.

Table 1: Descriptive Statistics

	Comply-Audit				Non-Comply-Audit N			
Variable	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.
Panel B: Descriptive Statistics Partitioned on Compliance with the Non-Audit Service Reforms Prior to their Enactment								
Fraud	192	1.82%	0.00%	0.09	685	3.07%	0.00%	0.13
Size	192	4,183	1,213	7,729	685	5,269	1,341	9,116
LnSize	192	7.19	7.08	1.49	685	7.34	7.15	1.59
BTM	192	0.46	0.41	0.32	685	0.45	0.41	0.28
LnBTM	192	−1.00	−0.94	0.63	685	−1.02	−0.96	0.63
Offering	192	0.41	0.33	0.40	685	0.40	0.50	0.38
Leverage	192	0.17	0.16	0.14	685	0.17	0.16	0.13
	Comply-NAS				Non-Comply-NAS			
Variable	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.
Panel C: Descriptive Statistics Partitioned on Compliance with Full Audit Committee Independence Reforms Prior to their Enactment								
Fraud	499	2.64%	0.00%	0.11	378	3.00%	0.00%	0.14
Size	499	4,835	1,371	8,424	378	5,289	1,228	9,361
LnSize	499	7.30	7.20	1.56	378	7.32	7.08	1.57
BTM	499	0.45	0.41	0.26	378	0.46	0.40	0.33
LnBTM	499	−1.01	−0.94	0.62	378	−1.02	−0.95	0.65
Offering	499	0.42	0.50	0.38	378	0.39	0.25	0.39
Leverage	499	0.18	0.16	0.13	378	0.17	0.15	0.13

Table 1: Continued

Note: This table provides descriptive statistics for the primary variables used in the study, in both the pre-reform period (1996–1999) and the post-reform period (2004–2007). Further, this table provides descriptive statistics from the Pre-Reform period for firms that are and are not in compliance with majority board independence, full audit committee independence, and non-audit services. Variables are defined in the Appendix.

independent board in the pre-reform period are smaller, less levered, and more likely to have a debt or equity offering.

Table 2 provides correlations among the major change variables that are used in following analyses. Notably, Fraud is significantly correlated with ΔLnSize, Non-Comply-Board, and Non-Comply-Audit on a univariate basis. Non-Comply-NAS is not significantly related to either of the board reform variables. However, Non-Comply-Board and Non-Comply-Audit have a correlation coefficient of 0.32, indicating that they are highly correlated and that firms that did not have a majority of independent directors were less likely to have a fully independent audit committee. Other relations are as would be expected. For example, ΔLnSize is positively correlated with ΔLnBTM and ΔLeverage, and negatively correlated with ΔOffering.

3.3.2 Univariate Results

Table 3 provides results of univariate tests regarding the change in fraud rates among firms that were forced to change their governance structure versus firms that were already in compliance with the governance reforms. Panel A provides results regarding non-audit services, Panel B provides results regarding the full board containing a majority of independent directors, and Panel C provides results regarding a fully independent audit committee.

In Panel A, the fraud rate for NAS non-compliers falls from 3.07% in the pre-reform period to 2.96% in the post-reform period, for a decrease of 0.10 points. For the control group of NAS compliers, the fraud rate falls from 1.82% to 1.48%, a decrease of 0.35 points. The difference-in-differences is −0.25 points, which is insignificant (t − stat = −0.21). In Panel B, the fraud rate for audit committee non-compliers falls from 3.00% in the pre-reform period to 1.76% in the post-reform period, for a decrease of 1.23 points. For the control group of audit committee compliers, the fraud rate increases from 2.64% to 3.31%, an increase of 0.67 points. The difference-in-differences equates to a 1.90 point decrease in the rate of fraud, which is significant at the 10% (5%) level on a two-sided (one-sided) basis (t-stat = 1.73). Given the relatively low base rate of fraud, this difference is economically significant.

In Panel C, the fraud rate for full board non-compliers falls from 3.89% in the pre-reform period to 1.81% in the post-reform period, for a decrease of 2.08 points. For the control group of full board compliers, the fraud rate increases from 2.38% to 2.96%, an increase of 0.58 points. The difference-in-differences equates to a 2.66 point decrease in the rate of fraud, which is significant at the 5% level (t − stat = 2.37). Given the relatively low base rate of fraud, this difference is economically significant as well.

Finally, we conduct a falsification test to help rule out the possibility that other SOX-era factors (e.g., disproportionate regulatory enforcement or

	ΔFraud	ΔLnSize	ΔLnBTM	ΔOffering	ΔLeverage	Non-Comply-NAS	Non-Comply-Audit
ΔLnSize	0.17						
	(<0.0001)						
ΔLnBTM	−0.03	0.30					
	(0.31)	(<0.0001)					
ΔOffering	0.03	−0.08	−0.12				
	(0.40)	(0.01)	(0.00)				
ΔLeverage	0.03	0.11	−0.002	0.20			
	(0.31)	(0.00)	(0.64)	(<0.0001)			
Non-Comply-NAS	0.01	0.04	0.01	0.02	0.08		
	(0.86)	(0.29)	(0.81)	(0.53)	(0.01)		
Non-Comply-Audit	−0.06	0.07	0.01	0.04	0.03	0.05	
	(0.10)	(0.03)	(0.87)	(0.28)	(0.32)	(0.15)	
Non-Comply-Board	−0.07	0.12	0.05	−0.04	0.01	−0.02	0.32
	(0.04)	(0.00)	(0.16)	(0.28)	(0.84)	(0.53)	(<0.0001)

Table 2: Pearson Correlations

Note: This table provides Pearson correlation coefficients for primary variables, with p-values in parentheses. Variables are defined in the Appendix.

Variable	Non-Comply-NAS	Comply-NAS	Difference (t-stat)
Panel A: Differences in the Frequency of Fraud—Non-Audit-Services (NAS)			
Pre-Reform	3.07%	1.82%	1.24%
			(−1.51)
Post-Reform	2.96%	1.48%	1.49%
			(−1.51)
Difference	0.10%	0.35%	−0.25%
(t-stat)	(−0.14)	(−0.36)	(−0.21)
Panel B: Differences in the Frequency of Fraud—Independent Audit Committee			
Pre-Reform	3.00%	2.64%	0.36%
			(0.41)
Post-Reform	1.76%	3.31%	−1.54%
			(−1.62)
Difference	1.23%	−0.67%	1.90%
(t-stat)	(1.86)	(−0.76)	(1.73)
Panel C: Differences in the Frequency of Fraud—Independent Board			
Pre-Reform	3.89%	2.38%	1.51%
			(1.39)
Post-Reform	1.81%	2.96%	−1.15%
			(−1.17)
Difference	2.08%	−0.58%	2.66%
(t-stat)	(2.42)	(−0.80)	(2.37)
Panel D: Differences in the Frequency of Fraud—Post-Reform Non-Independent Board			
Pre-Reform	1.11%	2.38%	−1.27%
			(1.42)
Post-Reform	3.61%	2.96%	0.65%
			(0.35)
Difference	−2.50%	−0.58%	1.92%
(t-stat)	(1.29)	(0.80)	(0.93)

Table 3: Univariate Differences in the Rate of Fraud - Non-Compliers and Compliers

Note: This table provides univariate differences in the frequency of fraud between firms that were or were not in compliance with three different governance regulations prior to their passage (panels A-C), and for firms that did not immediately comply with the board reforms (panel D). Variables are defined in the Appendix.

scrutiny against non-compliers in the post-reform period that deterred fraud commission) contribute to our main findings. We perform this test only with the majority independent board provision because it is the only regulatory requirement that was subject to an exception. We thus examine whether firm-years that did not receive the "treatment" in the post-reform period, which we label "post-reform non-compliance" (PRNC) firms, experience a differential level of fraud.

This sample includes 90 firms that did not comply with the majority independent board requirement in the pre-period *and* did not comply for at

least a portion of the post-reform period. These firms either were not required to comply with the full board majority independence requirement because they were controlled companies, appointed "gray" directors whose independence was not clear, or were allowed to postpone implementation of the reforms due to a staggered board. If other SOX-era factors drive our results then one would expect PRNC firms, which were non-compliers in the pre-reform period but did not experience a shift in board independence, to experience a drop in financial reporting fraud in the post-reform era. If however receiving the regulatory shock of greater board independence drives our findings, PRNC firms should not experience a drop in financial reporting fraud in the post-reform era.

Panel D contains the same differences-in-differences analysis as panel C, except we substitute PRNC firms (which did not receive the compliance shock) for the non-compliers (which did actually receive the compliance shock in the post-reform period). Unlike the non-compliers in Panel C, PRNC firms did not experience a significant decline in financial reporting fraud. Their fraud rate actually increased 2.50 percentage points, but this change is not statistically different from either zero ($t = -1.29$) or the fraud rate change for compliers ($t = 0.93$). Overall, this evidence suggests that the mandated shift in board independence, not other SOX-era reforms or contemporaneous factors, led to the reduction in the rate of financial reporting fraud.[16]

Thus, on a univariate basis, it appears that both types of board-related governance reforms affected the rate of financial reporting fraud, while reforms related to non-audit services do not. However, these specifications do not control for other factors that may be related to fraud, such as firm-level incentives. In addition, given the high correlation between Non-Comply-Board and Non-Comply-Audit, it is not clear whether both variables are affecting the rate of fraud independently. We thus turn to our multivariate analysis.

3.4 Multivariate Design and Results

Our multivariate tests use a similar intuition as our univariate tests, while controlling for fraud determinants and incentives. The prior literature uses two types of measures to predict fraud. The first type includes incentives to commit fraud, such as securities issuance. The second type includes indicators that fraud is occurring (such as accrual magnitude) that provide a red flag regarding unrevealed fraud. We purposely do not control for variables that indicate fraud is occurring because doing so could remove some of the effect of interest (i.e., whether the reforms reduced the occurrence of fraud). However, in later sensitivity tests, we compare changes in a "red flag" fraud indicator variable for compliers and non-compliers to corroborate our findings. We therefore

[16]Of the 90 firms in this sample, 58 eventually adopted a majority independent board, while 32 firms never did so. The increase in fraud rate is concentrated in the 32 firms that never adopted a majority independent board.

include controls for securities offerings, firm growth, firm size, and leverage (see Dechow *et al.* (2011) and Kim and Skinner (2012)). We control for these factors to guard against the possibility that they have affected compliers and non-compliers differentially over time.

We measure the change in fraud rates at the firm level and test reforms jointly:

$$\begin{aligned}\Delta\text{Fraud}_i = {} & \alpha + \beta_1\text{Non-Comply-NAS}_i + \beta_2\text{Non-Comply-Audit}_i \\ & + \beta_3\text{Non-Comply-Board}_i + \beta_4\Delta\text{LnSize}_i + \beta_5\Delta\text{LnBTM}_i \\ & + \beta_6\Delta\text{Offering}_i + \beta_7\Delta\text{Leverage}_i + \varepsilon_i \end{aligned} \tag{1}$$

If all three shifts in governance variables were incrementally effective in reducing the rate of financial reporting fraud, we expect β_1 to β_3 be significantly negative in model (1).

For variable measurement in the panel data tests, we simply measure each variable (e.g., fraud) by fiscal year. Terminology is thus very similar, but does not involve changes. Thus, this model requires use of time (Post) and interaction terms. Our primary regression specification using panel data, which produces results parallel to model (1), is thus:

$$\begin{aligned}\text{Fraud}_{i,t} = {} & \alpha + \beta_1\text{Post} + \beta_2\text{Non-Comply-NAS}_i \\ & + \beta_3\text{Post*Non-Comply-NAS}_i + \text{B}_4\text{Non-Comply-Audit}_i \\ & + \text{B}_5\text{Post*Non-Comply-Audit}_i + \beta_6\text{Non-Comply-Board}_i \\ & + \text{B}_7\text{Post*Non-Comply-Board}_i + \beta_8\text{LnSize}_{i,t} + \beta_{10}\text{LnBTM}_{i,t} \\ & + \beta_{11}\text{Offering}_{i,t} + \beta_{12}\text{Leverage}_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{2}$$

In addition, we often use firm fixed effects in the panel data tests. The firm fixed effects subsume the primary non-compliance measures, and thus only the interaction terms survive:

$$\begin{aligned}\text{Fraud}_{i,t} = {} & \alpha_i + \beta_1\text{Post} + \beta_2\text{Post*Non-Comply-NAS}_i \\ & + \text{B}_3\text{Post*Non-Comply-Audit}_i \\ & + \text{B}_4\text{Post*Non-Comply-Board}_i + \beta_5\text{LnSize}_{i,t} \\ & + \beta_6\text{LnBTM}_{i,t} + \beta_7\text{Offering}_{i,t} \\ & + \beta_8\text{Leverage}_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{3}$$

3.4.1 Multivariate Results—Changes Design

Column 1 of Table 4 provides regression results for model (1), where the dependent variable is ΔFraud, calculated as the fraud rate in the post-reform period less the fraud rate in the pre-reform period. Again, to the extent that

the governance reforms were effective, each non-compliance indicator variable should have a negative coefficient, indicating a higher decrease in the fraud rate for firms that were forced to comply with the new governance requirements. The only governance reform variable that is statistically significant is Non-Comply-Board ($t-stat = -2.23$). The ΔSize and ΔBTM variables are significant in the predicted direction. We next turn to our panel data design to provide further insight and examine the robustness of this relation.

3.4.2 Multivariate Results—Panel Data

In the panel data tests, we use a linear probability model due to the inclusion of firm fixed effects and interactions terms. As detailed in Duchin and Sosyura (2014), non-linear binary response models tend to produce biased coefficient estimates in these situations.

Table 4, column 2 presents results of estimating model (2). Overall, the results are very similar to those of the changes design. In particular, the coefficient on Post*Non-Comply-Board (the equivalent to the Non-Comply-Board variable in the changes design) is negative (−0.02) and significant ($t = -1.84$). Next, we control for unobservable firm characteristics by using firm fixed effects.[17] Table 4, column 3 presents results. The interaction of Non-Comply-Board and Post is negative and significant ($t = -2.26$). Finally, column 4 uses model (2) with firm random effects, and the coefficient on Post*Non-Comply-Board is again negative and significant ($t = -1.93$).[18]

The lone significance of variables related to board independence may seem somewhat surprising, given that the audit committee and non-audit services seem more closely related to financial reporting than does the composition

[17]We also estimated specifications with year fixed effects (with the *Post* main effects dropped) and both year and fixed effects (with both *Post* and *Non-Comply-Board* main effects dropped). Results are inferentially unchanged.

[18]One potential issue with our board reform measure is that, because it is simply an indicator variable, it does not capture all information regarding reforms. We thus use two alternate measures of board independence, both measured at the firm level (rather than the firm-year) level. First, we use the percentage change in each firm's average level of independent directors between the post- and pre-period (ΔBoard_Independence). This captures the *extent* of the actual change in independence, but is subject to firm choice. Second, we use the firm's average level of board independence during the pre-reform period (Initial_Indep) as a proxy for the required change in independence (i.e., to achieve at least 51% independence). We multiply this variable by negative one so that it increases by the extent to which firms would have to increase their board independence. If mandated increases in board independence really drive fraud reduction, we expect larger increases to be associated with a greater fraud reduction.
In model 2 (omitting audit committee and NAS measures), the interaction of ΔBoard_Independence and Post is negative and significant ($t = -3.68$, untabulated). In addition, the interaction of Initial_Indep and Post is negative and significant ($t = -2.29$, untabulated), consistent with firms with lower independence in the pre-reform period having a greater reduction in fraud in the post-reform period relative to higher independence firms.

Variable	Pred.	(1)	(2)	(3)	(4)
Intercept		−0.03***	−0.05***	−0.24***	−0.09***
		(−1.92)	(−2.89)	(−4.28)	(−4.19)
Post	−		−0.01	−0.02**	−0.01
			(−0.54)	(−2.32)	(−0.74)
Non-Comply-NAS	−	0.00	0.01		0.01
		(−0.17)	(−1.19)		(1.19)
Non-Comply-NAS*Post	−		0.00	0.00	0.00
			(0.23)	(−0.05)	(0.07)
Non-Comply-Audit	−	−0.02	0.00		0.00
		(−1.40)	(−0.01)		(−0.09)
Non-Comply-Audit*Post	−		0.00	0.00	0.00
			(0.35)	(−0.37)	(−0.32)
Non-Comply-Board	−	−0.03**	0.01		0.02
		(−2.23)	(1.24)		(1.41)
Non-Comply-Board*Post	−		−0.02*	−0.03**	−0.02*
			(−1.84)	(−2.26)	(−1.93)
ΔLnSize (1)/LnSize (2-4)	+	0.06***	0.03***	0.03***	0.01***
		(4.21)	(4.49)	(4.49)	(3.73)
ΔLnBTM (1)/LnBTM (2-4)	−	−0.03***	−0.01***	−0.02	−0.02***
		(−2.84)	(−2.77)	(−3.77)	(−3.30)
ΔOffering (1)/Offering (2-4)	+	0.02	0.02***	0.01	0.01*
		(−0.87)	(2.60)	(1.24)	(1.94)
ΔLeverage (1)/Leverage (2-4)	+	0.00	0.02	−0.02	0.00
		(−0.07)	(0.64)	(−0.77)	(0.19)
Industry Fixed Effects		Yes	Yes		Yes
Firm Fixed Effects				Yes	
Firm Random Effects					Yes
R^2		4.20%	2.84%	0.40%	2.62%
N		877	6,080	6,080	6,080

Table 4: Regression Results—The Effect of Governance Reforms on Fraud

Note: This table presents the results of the following regressions. Model (3) includes firm fixed effects.

$$\begin{aligned}\Delta Fraud_i = {} & \alpha + \beta_1 \text{Non-Comply-NAS}_i + \beta_2 \text{Non-Comply-Board}_i + \beta_3 \text{Non-Comply-Audit}_i \\ & + \beta_4 \Delta\text{LnSize}_i + \beta_5 \Delta\text{LnBTM}_i + \beta_6 \Delta\text{Offering}_i + \beta_7 \Delta\text{Leverage}_i + \varepsilon_i \qquad (4)\end{aligned}$$

$$\begin{aligned}Fraud_{i,t} = {} & \alpha + \beta_1 \text{Post} + \beta_2 \text{Non-Comply-NAS}_i + \beta_3 \text{Post} * \text{Non-Comply-NAS}_i \\ & + \text{B}_4 \text{Non-Comply-Audit}_i + \text{B}_5 \text{Post} * \text{Non-Comply-Audit}_i \\ & + \beta_6 \text{Non-Comply-Board}_i + \text{B}_7 \text{Post} * \text{Non-Comply-Board}_i + \beta_8 \text{LnSize}_{i,t} \\ & + \beta_{10} \text{LnBTM}_{i,t} + \beta_{11} \text{Offering}_{i,t} + \beta_{12} \text{Leverage}_{i,t} + \varepsilon_{i,t} \qquad (5)\end{aligned}$$

$$\begin{aligned}Fraud_{i,t} = {} & \alpha_i + \beta_1 \text{Post} + \beta_2 \text{Post} * \text{Non-Comply-NAS}_i + \text{B}_3 \text{Post} * \text{Non-Comply-Audit}_i \\ & + \text{B}_4 \text{Post} * \text{Non-Comply- Board}_i + \beta_5 \text{LnSize}_{i,t} + \beta_6 \text{LnBTM}_{i,t} + \beta_7 \text{Offering}_{i,t} \\ & + \text{B}_8 \text{Leverage}_{i,t} + \varepsilon_{i,t} \qquad (6)\end{aligned}$$

Variables are defined in the Appendix. *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively, in a two-tailed test. t-statistics based on standard errors corrected for heteroscedasticity (column 1) or clustered at the firm level (columns 2–4) are in parentheses below coefficients.

of the full board. However, the results with respect to non-audit services are largely in line with prior research (Kinney *et al.*, 2004).

With respect to the audit committee appearing to be less important than the full board, it is important to note that the requirements for these two bodies are not symmetric in the reforms. It is plausible that moving from an already largely independent audit committee to a fully independent audit committee could have less of an effect than moving from a minority to a majority of independent directors on the full board, the latter of which affects a variety of monitoring sub-committees (as we demonstrate later). Notably, DeFond and Francis (2005, p. 7) indicate that it is possible that moving to fully independent audit committees may not improve governance beyond audit committees with "a high proportion of independent members."

In fact, our results are consistent with Klein (2002b) with respect to audit committees and earnings management prior to the mandated governance reforms. While Klein (2002b) found a negative relation between additional independent audit committee members and discretionary accruals, she found no difference when firms moved to fully independent audit committees. Due to the insignificant of the NAS and audit committee variables, we do not explore these issues further in the main analysis, but provide additional results in the Internet Appendix.

4 Additional Analysis—Full Board Independence Tests

In this section, we perform a variety of additional tests to assess the reliability of the association between the full board independence reforms and financial reporting fraud. We focus on the full board independence reforms since this variable is the dominant factor in our prior tests. Specifically, we explore: 1) the use of propensity score matching, 2) the effect of any pre-existing trends in fraud in the pre-reform era, and 3) various alternate explanations.

4.1 Propensity Score Matching

We use propensity score matching to negate observable differences in firm characteristics between compliers and non-compliers. We use logistic regression to model the probability that a firm has a majority of independent directors in 1999, prior to the passage of the first governance reform. In the first matching model (the "base model"), we include prior variables representing determinants or incentives to commit fraud firm size, book-to-market, offering, and leverage as of 1999, as well as trends in these same variables over the pre-reform period from 1996 to 1999, as control variables (Atanasov and Black, 2015). We also

include industry indicators:

$$\begin{aligned}&\text{Prob(Non-Comply-Board} = 1)\\&\quad = \text{F}(= \alpha_0 + \beta_1 \text{LnSize}_{\text{it}} + \beta_2 \text{LnSize\_Trend}_{\text{it}} + \beta_3 \text{LnBTM}_{\text{it}}\\&\qquad + \beta_4 \text{LnBTM\_Trend}_{\text{it}} + \beta_5 \text{Offering}_{\text{it}} + \beta_6 \text{Offering\_Trend}_{\text{it}}\\&\qquad + \beta_7 \text{Leverage}_{\text{it}} + \beta_8 \text{Leverage\_Trend}_{\text{it}}) \qquad (7)\end{aligned}$$

Panel A of Table 5 presents the results of the logistic regression. The levels of LnSize, LnBTM, and Leverage and the change in LnSize are significantly associated with the Non-Comply variable. The model has reasonable explanatory power with a pseudo r-squared of 0.127. Based on these results, we drop matches with an absolute difference in predicted probability greater than 0.01. This produces a panel data set of 2,942 firm-year observations from 214 non-complier firms matched to 214 complier firms. Notably, even with this relatively simple model, we lose over half of the initial panel data sample.

We next estimate a more complete model (the "full model") that also includes controls for audit committee non-compliance and the actual percentage of audit committee independence in the pre-reform period; NASDAQ listing; residual audit fees from the first available year for the firm; and whether the firm's auditor reported an internal control weakness when SOX section 404(b) went into effect. All of these variables are likely to be related to the firms' governance and control environment. For instance, the firm's audit committee measures are highly correlated with its governance environment as well as full board independence (Duchin *et al.*, 2010). Similarly, exchange-based governance rules differ somewhat, with NASDAQ rules generally being slightly less strict. Audit fees represent auditor scrutiny and are highly related to financial reporting quality (Kravet *et al.* 2014).[19] Internal control reporting by the auditor under section 404(b) of SOX reflects both the firm's control environment, may have led to a decrease in the rate of fraud and may also disproportionately affect firms with weak internal controls (i.e., non-compliers may have had worse controls and experienced more remediation). We assume that audit fees and internal controls are relatively stable firm characteristics, and that they reflect the firm's overall governance structure even though these variables are actually drawn from future time periods. The model is specified

[19]To measure audit scrutiny, we estimate an audit fee model similar to Hogan and Wilkins (2008), controlling for client firm size (measure by the log of total assets), sales growth, leverage, return on assets, negative earnings, foreign operations, merger activity, calendar fiscal year end, and whether the audit report contained any non-standard language. The audit fee regressions have an adjusted r-squared ranging from 0.71 to 0.77.

Variable	Pred.	(1) Base	(2) Full	(3) 40–60%
Panel A: Propensity Score Match Modeling the Probability of a Majority Independent Board				
Intercept		1.16**	9.14***	6.62***
		(5.32)	(44.35)	(11.00)
LnSize	+	−0.21***	−0.20**	−0.14**
		(10.66)	(5.99)	(1.32)
LnSize_Trend	+	0.81***	0.79***	0.51
		(18.33)	(12.33)	(2.28)
LnBTM		−0.32**	0.39**	0.50**
		(4.68)	(4.85)	(3.03)
LnBTM_Trend	−	−0.16	−0.12	−0.20
		(0.83)	(0.29)	(0.34)
Offering	+	0.09	0.16	0.15
		(0.14)	(0.30)	(0.12)
Offering_Trend	+	0.15	0.22	0.34
		(0.63)	(0.90)	(0.88)
Leverage	+	−1.59**	−2.47***	−2.70*
		(4.01)	(6.47)	(3.45)
Leverage_Trend	+	0.09	0.62	0.31
		(0.01)	(0.36)	(0.05)
Non-Comply-Audit	+		−1.43***	−1.31**
			(9.95)	(4.06)
Audit Ind. %	−		−8.57***	−4.32**
			(52.91)	(6.94)
NASDAQ	+		−0.43	−0.03
			(2.70)	(0.01)
ICW	+		−0.12	0.39
			(0.12)	(0.41)
Residual Audit Fee	+		−0.47***	−0.38
			(7.71)	(2.55)
Industry Fixed Effects		Yes	Yes	Yes
Pseudo R^2		12.70%	28.83%	17.41%
N		871	809	256

Table 5: Propensity Score Match Results—The Impact of Governance Regulations on the Incidence of Fraud

Note: Panel A presents the results of the logistic regressions used to obtain our propensity score match control samples:

$$\begin{aligned}\text{Prob(Non-Comply-Board} = 1) = \text{F}(= \alpha_0 &+ \beta_1 \text{LnSize}_{it} + \beta_2 \text{LnSize\_Trend}_{it} + \beta_3 \text{LnBTM}_{it} \\ &+ \beta_4 \text{LnBTM\_Trend}_{it} + \beta_5 \text{Offering}_{it} \\ &+ \beta_6 \text{Offering\_Trend}_{it} + \beta_7 \text{Leverage}_{it} \\ &+ \beta_8 \text{Leverage\_Trend}_{it}) \end{aligned} \tag{8}$$

$$\begin{aligned}\text{Prob(Non-Comply-Board} = 1) = \text{F}(= \alpha_0 &+ \beta_1 \text{LnSize}_{it} + \beta_2 \text{LnSize\_Trend}_{it} + \beta_3 \text{LnBTM}_{it} \\ &+ \beta_4 \text{LnBTM\_Trend}_{it} + \beta_5 \text{Offering}_{it} \\ &+ \beta_6 \text{Offering\_Trend}_{it} + \beta_7 \text{Leverage}_{it} \\ &+ \beta_8 \text{Leverage\_Trend}_{it} + \beta_9 \text{Audit Ind.\%}_{it} \\ &+ \beta_{10} \text{Non-Comply-Audit}_i + \beta_{11} \text{NASDAQ}_i \\ &+ \beta_{12} \text{ICW}_i + \beta_{13} \text{Residual\_Fee}_i) \end{aligned} \tag{9}$$

Variable	Mean Treatment	Mean Control	Diff. t-stat	Median Treatment	Median Control	Diff. chi-sq.
Panel B: Covariate Balance in Matched Samples						
Base Model:						
LnSize	7.09	7.13	−0.17	6.93	6.88	0.15
LnSize_Trend	0.50	0.54	−0.66	0.41	0.44	0.60
LnBTM	−1.01	−1.04	0.28	−0.92	−0.99	0.60
LnBTM_Trend	−0.01	−0.03	0.50	−0.01	−0.04	0.93
Offering	0.46	0.43	0.48	0.00	0.00	0.24
Offering_Trend	0.05	0.00	0.65	0.00	0.00	0.46
Leverage	0.17	0.17	0.17	0.15	0.15	0.15
Leverage_Trend	0.03	0.03	0.43	0.00	0.00	0.00
Full Model:						
LnSize	7.19	7.33	0.78	6.95	7.10	0.37
LnSize_Trend	0.51	0.51	0.12	0.40	0.39	0.01
LnBTM	−0.99	−0.98	0.03	−0.92	−0.89	0.01
LnBTM_Trend	−0.02	0.00	0.22	−0.02	0.00	0.13
Offering	0.46	0.47	0.24	0.00	0.00	0.06
Offering_Trend	0.04	0.05	0.10	0.00	0.00	0.01
Leverage	0.17	0.17	0.02	0.17	0.15	1.20
Leverage_Trend	0.03	0.03	0.24	0.00	0.01	1.79
Audit Ind. %	0.82	0.81	−0.11	0.83	0.80	0.05
Non-Comply-Audit	0.52	0.53	0.24	1.00	1.00	0.06
NASDAQ	0.28	0.27	−0.14	0.00	0.00	0.02
ICW	0.07	0.07	−0.03	0.00	0.00	0.00
Residual_Fee	−0.04	−0.03	0.21	0.01	−0.05	0.37
Full Model/P60–90:						
LnSize	7.28	7.22	−0.22	7.14	6.97	0.12
LnSize_Trend	0.47	0.48	0.16	0.36	0.39	0.12
LnBTM	−0.95	−0.99	−0.30	−0.87	−0.89	0.00
LnBTM_Trend	0.03	0.01	−0.15	−0.01	0.02	0.12
Offering	0.46	0.57	1.37	0.00	1.00	1.87
Offering_Trend	0.01	0.12	1.05	0.00	0.00	1.03
Leverage	0.16	0.17	0.29	0.17	0.14	0.47
Leverage_Trend	0.00	0.03	1.42	0.00	0.01	1.82
Audit Ind. %	0.90	0.91	0.44	1.00	1.00	0.03
Non-Comply-Audit	0.34	0.32	−0.18	0.00	0.00	0.03
NASDAQ	0.31	0.26	−0.57	0.00	0.00	0.32
ICW	0.06	0.06	0.00	0.00	0.00	0.00
Residual_Fee	−0.01	−0.07	−0.60	0.04	−0.07	1.87
Full Model/40–60% Ind.:						
LnSize	7.23	7.36	0.53	7.14	7.33	0.03
LnSize_Trend	0.52	0.51	−0.15	0.44	0.41	0.03
LnBTM	−1.01	−1.04	−0.32	−1.00	−1.03	0.03
LnBTM_Trend	−0.03	−0.03	0.00	−0.06	−0.11	0.03
Offering	**0.55**	**0.39**	**−1.89**	**1.00**	**0.00**	**3.49**
Offering_Trend	0.10	−0.03	−1.31	0.00	0.00	1.76
Leverage	0.16	0.16	0.08	0.14	0.15	0.26
Leverage_Trend	0.01	0.02	0.66	0.00	0.00	1.54
Audit Ind. %	0.83	0.79	−1.24	0.83	0.75	0.62
Non-Comply-Audit	0.51	0.59	1.02	1.00	1.00	1.05
NASDAQ	0.26	0.30	0.56	0.00	0.00	0.32
ICW	0.03	0.06	0.81	0.00	0.00	0.66
Residual_Fee	−0.03	−0.11	−0.77	0.01	−0.09	0.26

Table 5: Continued

Matched Sample: Variable	Pred.	Base Coeff.	Full Coeff.	Full P60–P90 Coeff.	Full 40%–60% Ind. Coeff.
Panel C: Linear Probability Model Results- The Impact of Board Independence on the Incidence of Fraud Using a Propensity Match Sample					
Intercept		−0.22***	−0.20*	−0.33**	−0.26**
		(−2.99)	(−1.91)	(−2.39)	(−2.20)
Post		−0.02	−0.01	−0.01	−0.01
		(−1.40)	(−0.33)	(−0.38)	(−0.71)
Non-Comply-Board*Post	−	−0.03**	−0.04***	−0.04*	−0.05**
		(−2.44)	(−2.63)	(−1.79)	(−2.07)
LnSize	+	0.03***	0.03**	0.05**	0.04**
		(3.21)	(2.05)	(2.42)	(2.26)
LnBTM	−	−0.03***	0.00	0.01	−0.01
		(−2.84)	(−0.32)	(1.28)	(−0.94)
Offering	+	0.00	0.01	0.03*	0.01
		(0.61)	(1.24)	(1.93)	(0.37)
Leverage	+	−0.01	0.00	−0.03	0.02
		(−0.36)	(−0.08)	(−0.51)	(0.30)
Firm Fixed Effects		Yes	Yes	Yes	Yes
R^2		1.00%	0.88%	1.25%	2.05%
N		2,942	1,846	928	950

Table 5: Continued

Note: Panel B presents summary statistics regarding the covariate balance between our treatment and control samples.
Panel C presents the propensity score match results of estimating a linear probability model for the following model using the various matched samples:

$$\begin{aligned}\text{Fraud}_{it} = {} & \alpha_0 + \beta_1 \text{Post}_{it} + \beta_2 \text{Non-Comply-Board}_{it} * \text{Post}_{it} + \beta_3 \text{LnSize}_{it} + \beta_4 \text{LnBTM}_{it} \\ & + \beta_5 \text{Offering}_{it} + \beta_6 \text{Leverage}_{it} + \varepsilon_{it} \end{aligned} \tag{10}$$

Variables are defined in the Appendix. *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively, in a two-tailed test. Chi-square statistics are in parentheses below coefficient estimates in Panel A, and t-statistics based on heteroskedasticity-consistent standard errors clustered by firm are in parentheses below the coefficients in Panel C.

as follows:

$$\begin{aligned}&\text{Prob(Non-Comply-Board} = 1) \\ &= \text{F}(= \alpha_0 + \beta_1 \text{LnSize}_{\text{it}} + \beta_2 \text{LnSize\_Trend}_{\text{it}} + \beta_3 \text{LnBTM}_{\text{it}} \\ &\quad + \beta_4 \text{LnBTM\_Trend}_{\text{it}} + \beta_5 \text{Offering}_{\text{it}} + \beta_6 \text{Offering\_Trend}_{\text{it}} + \beta_7 \text{Leverage}_{\text{it}} \\ &\quad + \beta_8 \text{Leverage\_Trend}_{\text{it}} + \beta_9 \text{Audit Ind. \%}_{\text{it}} + \beta_{10} \text{Non-Comply\_Audit}_i \\ &\quad + \beta_{11} \text{NASDAQ}_i + \beta_{12} \text{ICW}_i + \beta_{13} \text{Residual\_Fee}_i) \end{aligned} \tag{11}$$

Using the full model (column 2 of panel A in Table 5), the number of matched firms declines substantially, resulting in 1,821 firm-years. We use two additional techniques to further refine matches and improve covariate balance. First, we use only firms with a predicted probability of non-compliance between 60% and 90% from the model above. We set these boundaries based on where the separate kernel density estimates for compliers and non-compliers provide

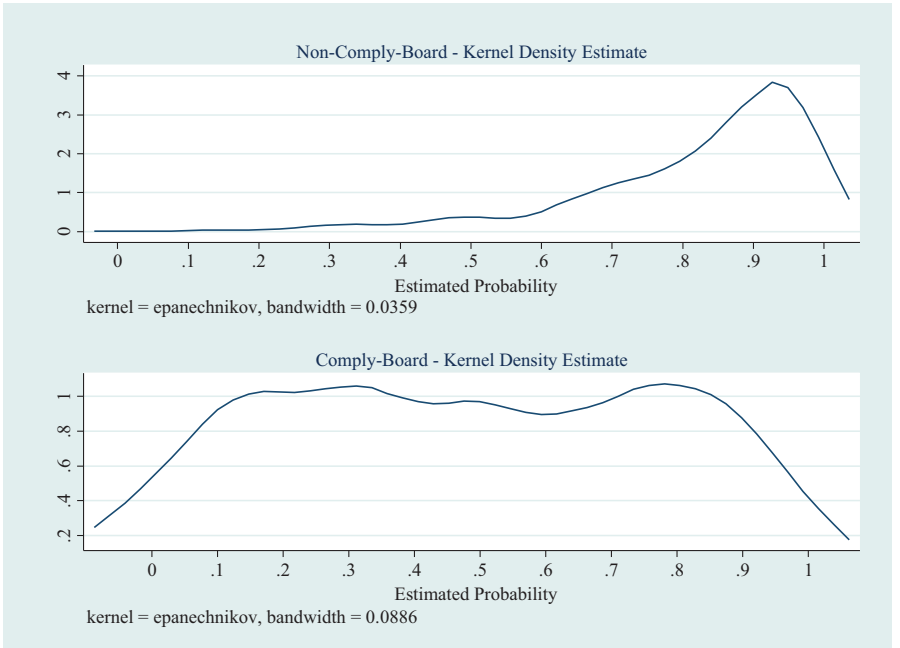

Figure 3: Kernel Density Estimates for Board Compliers and Non-Compliers

Note: This figure provides kernel density estimates of the compliers and non-compliers based on the full model propensity-matched sample in Table 5.

meaningful overlap in the two groups. For reference, we provide the kernel density function estimates (Figure 3), which shows that there are few board compliers with an estimated probability over 90%, while there are relatively few non-compliers with an estimated probability below 60%. This produces a sample of 928 firm-years. Second, we re-estimate the full model over only those firms with a pre-reform board independence percentage between 40% and 60%, which also makes firms more comparable. This produces a sample of 917 firm-years.

Panel B of Table 5 provides summary statistics regarding covariate balance for each of our four matched samples. Overall, the matches are very good. The Offering variable in the sample using the full model, limited to firms with 40% to 60% board independence, is the only variable across the four matched samples that shows a statistically significant difference.

Regression results based on the propensity-matched samples are presented in Panel C of Table 5 using a panel specifications similar to model (3), including firm fixed effects:

$$\begin{aligned}\text{Fraud}_{i,t} = \alpha_i + \beta_1 \text{Post} + \beta_2 \text{Post*Non-Comply-Board}_i + \beta_3 \text{LnSize}_{i,t} \\ + \beta_4 \text{LnBTM}_{i,t} + \beta_5 \text{Offering}_{i,t} + \beta_6 \text{Leverage}_{i,t} + \varepsilon_{i,t} \qquad (12)\end{aligned}$$

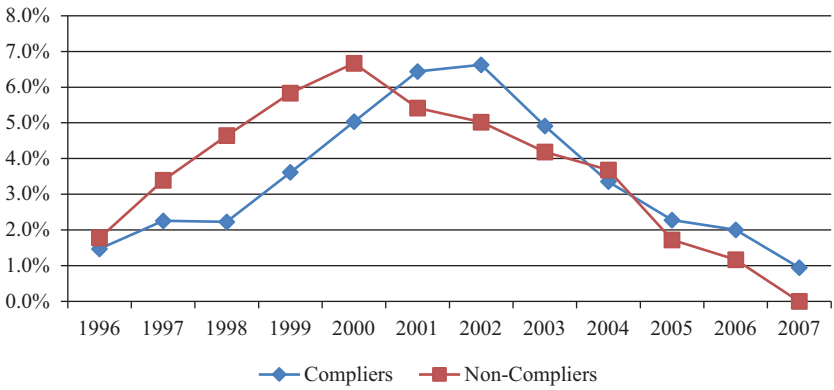

Figure 4: Fraud Rate by Year

Note: This figure provides a graphical depiction of the fraud rate by year for firms based on whether they had a majority independent board during the pre-reform period.

In column 1, the interaction of Non-Comply-Board and Post is negative and significant ($t = -2.44$) when the base model matches (n = 2,942) are used. Similarly, the interaction of Non-Comply-Board and Post is negative and significant (t = −2.63) in column 2 when we use the full model without further refinements (n = 1,846). When we restrict to matches in the 60% to 90% probability range (n = 928), the statistical significance is somewhat weaker (t = −1.79), but the economic significance is nearly identical (coefficients of −0.04 in both columns 2 and 3). Finally, when we restrict to firms that had board independence percentages between 40% and 60% in the pre-period (n = 950), the interaction of Non-Comply-Board and Post is again negative and significant (t = −2.07), and the economic significance is marginally higher (coefficient of −0.05 in column 4). Overall, these results are consistent with earlier results.

4.2 Pre-Existing Trends in Fraud

One concern with our findings is that perhaps non-compliers were already experiencing a disproportionate downward trend in fraud relative to compliers that began prior to the reforms. Figure 4 plots the fraud rate by year for compliers and non-compliers in the pre-reform (1996–1999), reform (2000–2003) and post-reform (2004–2007) periods. Inconsistent with a pre-existing downward trend for non-compliers, fraud actually *increased* for both compliers and non-compliers similarly pre-reform. For non-compliers, fraud began dropping in 2001 shortly after board independence began to increase for these firms (see also Figure 1). In contrast, the fraud rate for compliers rose through 2003 and only began to fall in 2004. Thus, it does not appear from Figure 3 that a

pre-existing downward trend in fraud contributes to our results.

To evaluate the effect of pre-existing trends more formally, we use a specification similar to Atanasov and Black (2015, p. 39), where we insert pre-reform year dummies for both compliers and non-compliers into equation (6) as follows:

$$\begin{aligned}\text{Fraud}_{it} = {} & \alpha_0 + \alpha_i + \beta_1 1997 + \beta_2 1998 + \beta_3 1999 \\ & + \beta_4 1997^{*}\text{Non-Comply-Board}_{it} \\ & + \beta_5 1998^{*}\text{Non-Comply-Board}_{it} \\ & + \beta_6 1999^{*}\text{Non-Comply-Board}_{it} + \beta_7 \text{LnSize}_{it} + \beta_8 \text{LnBTM}_{it} \\ & + \beta_9 \text{Offering}_{it} + \beta_{10} \text{Leverage}_{it} + \beta_{11} \text{Post}_{it} \\ & + \beta_{12} \text{Non-Comply-Board}_{it}{}^{*}\text{Post}_{it} + \varepsilon_{it} \end{aligned} \tag{13}$$

This regression allows both compliers and non-compliers to have different pre-reform trends in fraud rates. Table 6 presents the results of this regression. There is no obvious significant differential trend in the pre-reform dummies (β_4 to β_6) for non-compliers. The coefficient on Non-Comply-Board*Post (β_{12}) captures the average differential decrease in fraud for non-compliers relative to 1996, which is included in the intercept. To arrive at a true "diff-in-diff" comparison for non-compliers from the pre-reform era to the post-reform era, we conduct an F-test that compares β_{12} to an average of β_4 through β_6.[20] This difference is -0.026 ($F = 6.38$, $p < 0.05$), which is consistent with a differential decline in fraud for non-compliers after allowing for differential trends in fraud rates in the pre-reform period.

Atanasov and Black (2015) also recommend a "leads and lags" plot to illustrate the effect of reform shocks (see their equation 21 and the surrounding discussion). To generate the relevant coefficients, we use a model similar to model (7) while including the reform period years (2000–2003) in order to show any potential immediate effects as soon as firms began to move toward independent boards (the increase in board independence began in this period with the mandate for fully independent audit committees):

$$\begin{aligned}&\text{Fraud}_{it} \\ &\quad = \alpha_0 + \alpha_i + \beta_1 1997 + \beta_2 1998 + \beta_3 1999 \\ &\qquad + \beta_4 1997^{*}\text{Non-Comply-Board}_{it} + \beta_5 1998^{*}\text{Non-Comply-Board}_{it}\end{aligned}$$

[20] To see why, label the average rate of fraud for a complier in the pre-period as $\text{Cpre} = 0.25(4\alpha_0 + 4\alpha_i + \beta_1 + \beta_2 + \beta_3)$, assuming the continuous covariates equal zero. Likewise, $\text{Cpost} = \alpha_0 + \alpha_i + \beta_{11}$. For a non-complier in the pre-period, $\text{NCpre} = 0.25(4\alpha_0 + 4\alpha_i + \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5 + \beta_6)$ and $\text{NCpost} = \alpha_0 + \alpha_i + \beta_{11} + \beta_{12}$. Thus, $(\text{NCpost} - \text{NCpre}) - (\text{Cpost-Cpre}) = \beta_{11} + \beta_{12} - 0.25(\beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5 + \beta_6) - (\beta_{11} - 0.25(\beta_1 + \beta_2 + \beta_3)) = \beta_{12} - 0.25(\beta_4 + \beta_5 + \beta_6)$.

$$
\begin{aligned}
&+ \beta_6 1999^{*}\text{Non-Comply-Board}_{it} + \beta_7 \text{LnSize}_{it} + \beta_8 \text{LnBTM}_{it} \\
&+ \beta_9 \text{Offering}_{it} + \beta_{10} \text{Leverage}_{it} + \beta_{11} 2000 + \beta_{12} 2001 + \beta_{13} 2002 \\
&+ \beta_{14} 20003 + \beta_{15} 2004 + \beta_{16} 2005 + \beta_{17} 2006 + \beta_{18} 2007 \\
&+ \beta_{19} 2000^{*}\text{Non-Comply-Board}_{it} + \beta_{20} 2001^{*}\text{Non-Comply-Board}_{it} \\
&+ \beta_{21} 2002^{*}\text{Non-Comply-Board}_{yit} + \beta_{22} 2003^{*}\text{Non-Comply-Board}_{it} \\
&+ \beta_{23} 2004^{*}\text{Non-Comply-Board}_{it} + \beta_{24} 2005^{*}\text{Non-Comply-Board}_{it} \\
&+ \beta_{25} 2006^{*}\text{Non-Comply-Board}_{it} + \beta_{26} 2007^{*}\text{Non-Comply-Board}_{it} \\
&+ \varepsilon_{it} \qquad (14)
\end{aligned}
$$

We plot the coefficients on the year-Non-Comply-Board interaction terms (along with confidence intervals) in Figure 5. The effect of increased board independence on fraud appears to have occurred quickly, with a dip beginning in 2000, which becomes more pronounced in 2001. While each individual year in not significant as compared to the omitted year of 1996, there is a clear downward shift in the interaction terms beginning with the reform period.[21] Overall, it does not appear that differential trends in pre-reform period contribute significantly to our findings.

In another test of pre-reform fraud rates, we estimate a firm-level trend in fraud from the pre-period and use this coefficient as an additional control variable (Fit Trend, which is set to zero for firms with no variation in the pre-reform period), interacted with the firm fixed effect (see Atanasov and Black, 2015, p. 37):

$$
\begin{aligned}
\text{Fraud}_{it} = {} & \alpha_0 + \beta_1 \text{LnSize}_{it} + \beta_2 \text{LnBTM}_{it} + \beta_3 \text{Offering}_{it} + \beta_4 \text{Leverage}_{it} \\
& + \beta_5 \text{Post}_{it} + \text{B}_6 \text{Non-Comply-Board}_{it} * \text{Post}_{it} \\
& + \text{B}_7 \text{FE\_Fit Trend} + \varepsilon_{it} \qquad (17)
\end{aligned}
$$

Even after this control, the coefficient on the interaction of Post and Non-Comply-Board is still negative and significant ($t = -2.57$, untabulated). Finally, there is an additional concern related to the pre-reform period. Returning to Figure 4, non-compliers had a higher rate of fraud in the pre-period and both non-compliers and compliers experienced a decrease in fraud in the post-period. It is therefore possible that mean reversion contributes to the main results. In other words, perhaps non-compliers had a bigger drop in fraud relative to compliers simply because they had a higher rate of fraud in the pre-reform era. Results are robust to controlling for the effects of this mean reversion, as detailed in the Internet Appendix.

[21] Confidence intervals include zero, and significance is only achieved when multiple years are summarized.

Variable	Pred. Sign	Coeff.	Coeff.
Intercept		−0.22***	−0.21***
		(−4.00)	(−4.21)
1997	?	0.00	
		(0.30)	
1998	?	−0.01	
		(−0.87)	
1999	?	0.00	
		(0.52)	
1997*Non-Comply-Board	?	0.01	
		(0.93)	
1998*Non-Comply-Board	?	0.02	
		(1.34)	
1999*Non-Comply-Board	?	0.02	
		(1.10)	
LnSize	+	−0.25***	0.03***
		(−4.13)	(4.16)
LnBTM	−	−0.02***	−0.02***
		(−3.65)	(−3.49)
Offering	+	0.01	0.01
		(1.20)	(1.13)
Leverage	+	−0.02	−0.02
		(−0.84)	(−0.73)
Post	−	−0.02**	−0.02**
		(−2.51)	(−2.86)
Non-Comply-Board*Post	−	−0.01	−0.03**
		(−1.33)	(−2.37)
Column 1: $H_0 : \beta_{12} 0.25(\beta_4 + \beta_5 + \beta_6) = 0; F = 6.38^{**}$			
Firm Fixed Effects		Yes	Yes
Firm-Specific Pre-Reform Trend			Yes
R^2		0.50%	42.35%
N		6,080	6,080

Table 6: Linear Probability Model Results—The Impact of Pre-Existing Trends in Fraud on the Relation between Board Independence and Fraud Frequency

Note: This table presents the results that examine the effect of pre-existing trends in fraud. In column 1, we allow compliers and non-compliers to have different pre-reform trends in fraud rates. The model we estimate is as follows:

$$\begin{aligned}\text{Fraud}_{it} = {} & \alpha_0 + \beta_1 1997 + \beta_2 1998 + \beta_3 1999 + \beta_4 1997 * \text{Non-Comply-Board}_{it} \\ & + \beta_5 1998 * \text{Non-Comply-Board}_{it} + \beta_6 1999 * \text{Non-Comply-Board}_{it} + \beta_7 \text{LnSize}_{it} \\ & + \beta_8 \text{LnBTM}_{it} + \beta_9 \text{Offering}_{it} + \beta_{10} \text{Leverage}_{it} + \beta_{11} \text{Post}_{it} \\ & + \beta_{12} \text{Non-Comply-Board}_{it} * \text{Post}_{it} + \varepsilon_{it} \end{aligned} \tag{15}$$

In column 2, we use firm-specific trends from the pre-reform period interacted with firm fixed effects:

$$\begin{aligned} Fraud_{it} = {} & \alpha_0 + \beta_1 LnSize_{it} + \beta_2 LnBTM_{it} + \beta_3 Offering_{it} + \beta_4 Leverage_{it} + \beta_5 Post_{it} \\ & + \text{B}_6 Non - Comply - Board_{it} * Post_{it} + \text{B}_7 FE\_FitTrend + \varepsilon_{it} \end{aligned} \tag{16}$$

Variables are defined in the Appendix. *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively, in a two-tailed test. t-statistics based on heteroskedasticity-consistent standard errors clustered by firm are in parentheses below the coefficients.

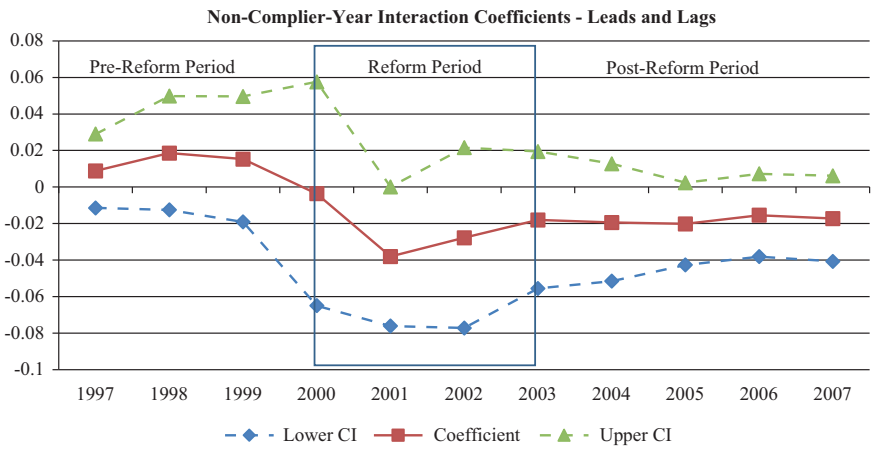

Figure 5: Leads and Lags Regression Coefficients

Note: This figure provides the coefficients on interaction terms (along with confidence intervals) for the following panel data regression, including firm fixed effects, for a dataset including all years from 1996 to 2007:

$$\begin{aligned}\text{Fraud}_{it} = {} & \alpha_0 + \beta_1 1997 + \beta_2 1998 + \beta_3 1999 + \beta_4 1997^{*}\text{Non-Comply-Board}_{it} \\ & + \beta_5 1998^{*}\text{Non-Comply-Board}_{it} + \beta_6 1999^{*}\text{Non-Comply-Board}_{it} + \beta_7 \text{LnSize}_{it} \\ & + \beta_8 \text{LnBTM}_{it} + \beta_9 \text{Offering}_{it} + \beta_{10} \text{Leverage}_{it} + \beta_{11} 2000 + \beta_{12} 2001 + \beta_{13} 2002 \\ & + \beta_{14} 20003 + \beta_{15} 2004 + \beta_{16} 2005 + \beta_{17} 2006 + \beta_{18} 2007 \\ & + \beta_{19} 2000^{*}\text{Non-Comply-Board}_{it} + \beta_{20} 2001^{*}\text{Non-Comply-Board}_{it} \\ & + \beta_{21} 2002^{*}\text{Non-Comply-Board}_{it} + \beta_{22} 2003^{*}\text{Non-Comply-Board}_{it} \\ & + \beta_{23} 2004^{*}\text{Non-Comply-Board}_{it} + \beta_{24} 2005^{*}\text{Non-Comply-Board}_{it} \\ & + \beta_{25} 2006^{*}\text{Non-Comply-Board}_{it} + \beta_{26} 2007^{*}\text{Non-Comply-Board}_{it} + \varepsilon_{it}\end{aligned}$$

4.3 Alternative Explanations

As with any study, a correlated omitted variable could be at least partly responsible for the results. While the relatively exogenous nature of the change we measure helps alleviate this concern, it cannot be completely eliminated. To be credible, however, any such omitted variable would have to (1) be correlated with the drop in the rate of fraud from the pre-reform period to the post-reform period, and (2) disproportionately affect non-compliance firms. The most credible explanation along these lines is that the SEC (or perhaps plaintiff attorneys) could have targeted firms with weak governance (i.e., minority independent boards) in the pre-reform period if they believed such firms had higher financial reporting risk, and that such targeting stopped when all firms were forced to adopt majority independent boards. Such a scenario could cause the relatively sharper decline in detected fraud among the Non-Comply-Board firms.

We thus use a measure of fraud (F-Score) that does not rely on filed securities lawsuits or SEC investigations. The F-Score was developed by Dechow *et al.* (2011) using financial statement data (like changes in receivables and inventory) and is intended to proxy for underlying fraud commission. Higher F-Scores are a "red flag" that reporting fraud is occurring. Results using F-Score corroborate our main results (see the Internet Appendix for details). Thus, it appears a decrease in the *underlying* rate of fraud for non-compliers, not a change in detection patterns, drives our main findings.[22]

5 Conclusion

This study examines whether the SOX-era requirements to increase board and audit committee independence and to ban non-audit services for audit clients reduced the incidence of financial reporting fraud. Our empirical tests are modeled after Duchin *et al.* (2010) and identify the incremental impact of these reforms by comparing the change in fraud incidence among firms forced to comply with the new reforms to the change in fraud incidence for firms already in compliance with the reforms upon enactment.

We find that forcing firms to appoint a majority of independent directors to the full board significantly reduced the incidence of financial reporting fraud. On the other hand, we find that banning the provision of non-audit services for audit clients had no effect on financial reporting fraud. Our findings with respect to the requirement that firms adopt a fully independent audit committee are more nuanced. We find no incremental reduction in financial reporting fraud from the adoption of this mandate after controlling for majority board independence.

One limitation is that our tests do not identify the incremental effect on financial reporting fraud (if any) experienced by voluntarily-in-compliance firms when they originally selected higher levels of board independence or abstained from purchasing non-audit services. However, from a policy perspective, the effects experienced by firms that were forced to comply with the SOX-era reforms are of more importance, as these firms were the direct target of the reforms. For these firms, our results are somewhat encouraging to lawmakers and regulators in the sense that the mandate to increase board independence appears to have had a significant effect in reducing financial reporting fraud. Ironically, however, this was the one mandate which was not required by Congress under SOX, but was instead required by the securities exchanges.

[22]In untabulated tests, we also examine whether independent directors serve as a deterrent to financial reporting fraud or simply discover it more quickly than their non-independent counterparts. We do not find evidence that the exogenous increase in board independence significantly affected the duration of financial reporting fraud. This evidence suggests that the primary effect of increased board independence is in reducing fraud incidence.

A Appendix: Primary Variable Definitions

Analyst-Following	An indicator variable coded as one if a firm had analyst following at any point in the 1996–1999 (pre-reform) period, from the I/B/E/S summary file, zero otherwise.
BTM	Book-to-market ratio, measured as common stockholder's equity divided by market capitalization at the beginning of the year. LnBTM is the natural log of this variable.
Fraud	An indicator variable coded as one when a credible allegation of financial reporting fraud exists, measured as the presence of an Accounting and Auditing Enforcement Release (AAER) that includes the firm year, or the presence of a settled securities class action case with a financial reporting allegation, where the class period included the firm-year, and zero otherwise.
ICW	An indicator variable coded as one when the auditor's internal control opinion notes a material weakness.
Leverage	The ratio of long term debt to total assets.
NASDAQ	An indicator variable set to one if a firm is listed on the NASDAQ, zero otherwise.
Non-Comply-Audit	An indicator variable coded one if a firm that did not have a fully independent audit committee in the year 1999, zero otherwise.
Non-Comply-Board	An indicator variable coded one if a firm that did not have a majority of independent directors in the year 1999, zero otherwise.
Non-Comply-NAS	An indicator variable coded one if a firm purchased IT and Other non-audit services equal to 10% or more of its audit and audit-related fees (as coded by Audit Analytics) in the year 2001, zero otherwise.
Offering	An indicator variable coded as one if the firm sold debt or equity in the current or prior year of at least 10% of total assets, zero otherwise.
Post-Non-Comply	An indicator variable coded one if a firm did not adopt a majority independent board during a year in the post-reform period.

Predicted Fraud Score	The output of Dechow *et al.* (2011, pp. 55–57) model 1 using the coefficients as reported from those authors.
Residual Audit Fee	The difference in residual audit fees between the pre-period (measures for this variable as 2000–2001) versus the post-period (2004–2007) from a regression of the natural logarithm of audit fees on the natural logarithm of total assets, sales growth, leverage, return on assets, negative earnings, foreign operations, merger activity, calendar fiscal year end, and whether the audit report contained any non-standard language.
Size	Total assets at the beginning of the year. LnSize is the natural log of this variable.

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