Big Five Audits and Accounting Fraud*

CLIVE LENNOX, *Nanyang Technological University*

JEFFREY A. PITTMAN, *Memorial University of Newfoundland*

1. Introduction

Empirical studies of earnings management, audit pricing, and audit reporting provide extensive evidence that the Big Five public accounting firms are associated with higher quality financial statements (Francis 2004).[1] Nevertheless, the recent high-profile financial reporting failures that roiled the U.S. capital markets cast doubt on whether this proposition remains valid. Indeed, many commentators interpret the steep upward trend during the late 1990s and early 2000s in accounting misstatements by companies with Big Five auditors as almost conclusive evidence that their assurance services have deteriorated over time (e.g., Coffee 2002; Imhoff 2003; Zeff 2003). Among other explanations, some observers blame these watershed events on the more lenient litigation landscape having a disproportionate impact on Big Five quality as well as market conditions that led them to increasingly pursue lucrative consulting contracts to the detriment of their independence on audit engagements (e.g., Earley, Odabashian, and Willenborg 2003).

Measuring trends in absolute audit quality is typically infeasible, so we more narrowly focus on discriminating between the *relative* performance of the Big Five and non-Big Five audit firms in preventing companies from orchestrating accounting fraud. Several commentators motivate arguments on the underlying reasons for the apparent erosion in Big Five audit quality by highlighting the well-known cases of fraudulent financial reporting by their clients (e.g., Coffee 2002; Cox 2003). However, rigorous evidence on whether companies are less likely to engage in accounting fraud if they are

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1. For expositional convenience, we refer to the set of Big Five auditors and their predecessors as the Big Five auditors in this paper since our 1981–2001 sample period precedes the demise of Arthur Andersen. Similarly, we follow convention by describing companies subject to an SEC Accounting and Auditing Enforcement Release as fraud companies when they are technically "fraud-accused" companies.

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audited by Big Five firms or whether the relative quality of financial statements audited by Big Five firms has declined over time remains elusive. Against this backdrop, we test whether Big Five audits are associated with a lower incidence of accounting fraud. Conditional on observing this negative relation, we proceed to examine two additional questions that provide our main contribution to extant research: (1) Did the negative relation between the presence of a Big Five auditor and fraud likelihood change in the years leading up to the Sarbanes-Oxley Act of 2002 (SOX)? (2) Is the negative relation explained by Big Five auditors supplying higher quality audits, or by the endogenous effects of screening by auditors and selection by their clients?

Analyzing accounting frauds suits our purposes since they were the catalysts for recent major legislative and regulatory changes largely aimed at improving the quality of audited financial statements. In other words, this is an opportune testing ground for our research on the link between Big Five audits and accounting impropriety. We collect the Accounting and Auditing Enforcement Releases (AAERs) issued by the Securities and Exchange Commission (SEC) for accounting frauds committed by companies between 1981 and 2001, which provides a fraud sample of 1,109 company-years. The control sample consists of 162,804 company-years in which there were no allegations of accounting fraud.

We begin by providing univariate evidence on the association between Big Five audits and accounting fraud. For the entire sample period, the frequency of accounting fraud is 0.61 percent in Big Five clients and 0.92 percent in non-Big Five clients. The difference between these frequencies is highly significant, reinforcing prior research that brand-name auditors are associated with higher quality financial statements (Francis 2004). We then examine whether the negative association between Big Five audits and accounting fraud remains stable over time. Univariate tests reveal that the associations are negative in every year between 1981 and 1995 and they are statistically significant at the 0.05 level or better in most years. However, there is a sudden change after 1995 with the relation between Big Five audits and fraud becoming insignificant between 1996 and 2000. Moreover, the association becomes significantly positive in 2001, suggesting that the clients of Big Five firms are *more* likely to commit fraud than are the clients of non-Big Five firms in the year prior to the passage of SOX. Altogether, the univariate evidence corroborates claims that Big Five audits were no longer associated with a lower incidence of accounting fraud in the years immediately before 2002.

Next, we report multivariate evidence on the relation between Big Five audits and accounting fraud. Consistent with the univariate tests, we find strong, robust evidence in both unmatched and matched samples that companies with Big Five auditors are less apt to engage in fraudulent financial reporting over the full sample period. Our probit coefficient estimates imply

that hiring a Big Five auditor translates into the client being about four times less likely to engage in accounting fraud, reflecting the first-order economic impact on audit quality.

We also report multivariate evidence on the stability of the negative association between Big Five audits and accounting fraud over time. Reinforcing the univariate analysis, we find significant negative associations in the period from 1981 to 1995. More importantly, the multivariate results include significant negative associations in the period from 1996 and 2001, which is inconsistent with both the univariate analysis and with claims that the Big Five quality differential had fallen in the years leading up to SOX. The negative Big Five coefficients are also similar in magnitude in the two periods, −0.63 (z-statistic = −7.73) in 1981–1995 and −0.69 (z-statistic = −5.10) in 1996–2001. Overall, the univariate results suggest that the negative relation between Big Five audits and accounting frauds vanishes after 1995 whereas the multivariate results indicate the opposite (i.e., that there is no such structural break). Additional tests demonstrate that the apparent structural break in the univariate results stems from the lack of a control for company size. Corroborating prior studies (e.g., Desai 2005), we find that larger companies were increasingly likely to engage in accounting fraud after 1995. Consequently, the apparent diminution in Big Five audit quality after 1995 actually reflects the increasing propensity for large companies to commit accounting fraud.

Since the multivariate evidence indicates a stable negative relation between Big Five audits and the likelihood of accounting fraud, we next examine whether this finding reflects the causal effect of Big Five firms' superior external monitoring. One alternative explanation is that these auditors are more adept at screening out the companies that are most likely to commit fraud. Another explanation is that companies planning to commit fraud are less likely to appoint Big Five audit firms. We shed light on these issues by examining auditor changes before and during the fraud years. Our results fail to support either of these alternative explanations. Specifically, we find no evidence that Big Five audit firms are more likely to resign from clients that engage in fraud, which is inconsistent with the screening argument. Similarly, the results do not suggest that fraud companies are less likely to switch to Big Five audit firms, which is inconsistent with the selection argument.

Contrary to recent criticisms of the Big Five firms, we provide compelling evidence that they were *consistently* associated with a lower incidence of accounting fraud, even in the years shortly prior to the sweeping corporate governance reforms. Moreover, this finding is robust to controlling for the endogenous effects of screening by audit firms and selection by their clients. Although the incidence of accounting fraud is lower for Big Five

clients, the economic and social fallout is more severe for frauds involving the Big Five since their clients are typically much larger.[2]

This paper continues as follows. Section 2 develops our hypotheses by recounting the evolution in legislative, regulatory, and competitive conditions that may have affected the relative quality of Big Five audits over time. Section 3 describes our research design and data. Section 4 reports our evidence and Section 5 concludes.

2. Testable predictions

The Big Five audit firms have motives to provide stricter external monitoring to avoid ruining their reputations and becoming embroiled in costly litigation (e.g., DeAngelo 1981a, 1981b; Dye 1993). It follows that a Big Five audit firm will be more eager to identify accounting misstatements and to resist client pressure to waive their correction. Indeed, Francis (2004) reviews the extensive prior evidence that the Big Five firms provide better assurance services to their clients, translating into our first prediction (all hypotheses are stated in alternate form):

HYPOTHESIS 1. *Ceteris paribus, companies with Big Five auditors are less likely to engage in accounting fraud.*

The rest of our analysis hinges on initially finding evidence consistent with Hypothesis 1 as a necessary condition to justify examining our next two research questions, starting with whether differential Big Five audit quality persists over time. Some argue that developments in the past 25 years led to a degradation in Big Five audits long before the recent deluge in financial reporting failures. Although their perspectives diverge on the main causes, several commentators share the perception that Big Five assurance services have eroded over time relative to smaller auditing firms (e.g, Coffee 2002; Imhoff 2003; Zeff 2003). In another common denominator, commentators almost invariably rely on the surge in accounting frauds committed by U.S. public companies with Big Five auditors to motivate their prescriptions for improving financial reporting quality.

2. Although our results imply that these events are relatively scarce, we concede that accounting fraud by Big Five clients can have catastrophic economic consequences. For example, Karpoff, Lee, and Martin (2008) estimate that the average company subject to SEC enforcement for financial reporting violations loses about $381 million in share value through legal and reputational penalties. They also report that firms implicated by the SEC lose, for every misrepresented dollar in its financial statements, $0.36 in fines and class action settlements and another $2.71 in reputational damage. In fact, this analysis is conservative since it ignores the serious reputational penalties incurred by individual managers and directors (e.g., Desai, Hogan, and Wilkens 2006; Fich and Shivdasani 2007) as well as auditors (e.g., Carcello and Palmrose 1994). Data limitations prevent us from estimating the economic impact of frauds perpetrated by Big Five and non-Big Five clients.

Coffee (2002), among others, argues that relaxing the severity of private enforcement against the Big Five auditors may have been partly behind the recent litany of financial reporting failures. He contends that the prospect of civil litigation can have a sobering impact on auditors' incentives to limit managers' discretion over the choice of accounting policies and estimates. However, legislative changes throughout the 1990s may have diluted the incentives for partners in the Big Five public accounting firms to monitor each other's work.[3] Coffee (2002) holds that Big Five audit quality largely began to fall with the passage of the Private Securities Litigation Reform Act of 1995 (PSLRA) that replaced joint and several liability with proportionate liability, which restricts investors' recourse against external auditors.[4] In evidence corroborating that this legislation narrowed incremental audit quality, Lee and Mande (2003) find that the income-increasing discretionary accruals of Big Five clients rose more than those of non-Big Five clients after the PSLRA was enacted.

Although the PSLRA was its most visible lobbying success, the Big Five used their political clout on Capitol Hill to initially deflect the SEC's proposals to seriously restrict non-audit services to audit clients (Mayer 2002). In fact, another standard explanation for the supposed decline in Big Five audit quality is their quest for more consulting revenues at the expense of auditor independence. The rise in non-audit services may matter more to the Big Five that generate a larger fraction of their revenues from this source (e.g., Ruddock, Taylor, and Taylor 2006). Indeed, the SEC (2001) highlights that its proposals to ban non-audit services would primarily affect the Big Five that increasingly relied on these revenues in the period leading up to SOX.

In a climate of fierce competition among the large public accounting firms for consulting contracts, internal controls over their audit practices may have begun to unravel with the Big Five becoming more accommodating over time toward aggressive financial reporting by their clients. In short, this argument suggests that the Big Five were so eager to secure lucrative consulting contracts that they succumbed to pressure from clients. Coffee (2002, 14) echoes Healy and Palepu 2003 and Imhoff 2003 in linking

3. There is evidence that the audit engagement partner — whose compensation is directly tied to the revenues generated from the partner's client portfolio — has stronger motives than other partners to tolerate aggressive earnings manipulation (Kinney 1999; SEC 2000).

4. Coffee (2001) reviews other legal developments in the 1990s that collectively made it more difficult for investors to recover damages in the event of audit failure. Several other legal scholars attribute the rise in accounting frauds by Big Five clients to the PSLRA and other legislation that softened private enforcement against auditors over this decade (e.g., Macey and Sale 2003). Supporting Dye's (1993) theory, relaxing auditor liability would disproportionally benefit the Big Five that provide greater implicit insurance coverage to shareholders (e.g., Willenborg 1999) and lenders (e.g., Pittman and Fortin 2004) stemming from audit failure.

the apparent slide in Big Five audit quality to the growth in consulting activities:

> The benefits of acquiescence to auditors rose over this same period, as the Big Five learned during the 1990s how to cross-sell consulting services and to treat the auditing function principally as a portal of entry into a lucrative client. Prior to the mid-1990s, the provision of consulting services to audit clients was infrequent and insubstantial in the aggregate … Not only did [the Big Five] see more profit potential in consulting than in auditing, but they began during the 1990s to compete based on a strategy of "low balling" under which auditing services were offered at rates that were marginal to arguably below cost. The rationale for such a strategy was that the auditing function was essentially a loss leader by which more lucrative services could be marketed.[5]

Indeed, the Panel on Audit Effectiveness, appointed by the Public Oversight Board (POB) in 1999 at the behest of the SEC, reports that 80 percent of Big Five clients in 1990 paid no consulting fees to their auditors. This situation changed dramatically with the Big Five's revenues from consulting practices growing from 13 percent in 1981 to 33 percent in 1993 to 51 percent in 1999 (SEC 2000). Moreover, the POB (2002) charges that the Big Five strongly opposed its plans to introduce steps to improve oversight of audit practices. Finally, in describing the Panel's hearings, Zeff (2003, 279) comments:

> The accumulated testimony about the change in character of the big firms in the 1980s and 1990s suggests the evolution toward a climate in which audit partners felt less secure in resisting clients' insistent arguments that marginal or even illicit accounting interpretations be applied in their financial statements. In the increasingly business-dominated climate of the big audit firms, one can raise serious questions about whether audit engagement partners, and indeed the firms themselves, were steadfastly resisting these pressures.

5. DeAngelo (1981a) models that incumbent auditors generate quasi-rents from existing clients which subsidize the high startup costs on new audits. Craswell and Francis (1999) only find initial engagement discounting for Australian firms upgrading from non-Big Five to Big Five auditors. Cox (2003), among many others, criticizes the Big Five for positioning the provision of audit services as a "loss leader" for getting a foot in the door to market consulting services to these same clients. For example, Ernst & Young penalized audit engagement partners ten percent of their salary for failing to meet revenue targets for non-audit services. Reinforcing the Big Five's focus on expanding their consulting practices, Andersen in 1998 developed a compensation system — labeled the "2X Strategy"—that sought to motivate audit partners to ensure that non-audit fees were twice as large as their audit fees (Earley et al. 2003).

Although these commentators identify somewhat different potential explanations, Francis (2004) explains that they all conclude that the relative quality of financial statements audited by the Big Five firms began to falter in recent years. However, extant research neglects to provide rigorous evidence on the validity of this argument. In our second prediction, we focus on helping to resolve whether any negative relation between Big Five audits and the incidence of accounting fraud subsides over time:

HYPOTHESIS 2. *Ceteris paribus, the negative association between Big Five audits and accounting fraud is stable over time.*

Conditional on finding that companies with Big Five auditors are less likely to engage in accounting fraud, we turn our attention toward unraveling the underlying reasons behind this negative relation. Specifically, we isolate whether the provision of higher quality audits by the Big Five firms narrows the scope for companies to fraudulently exaggerate their earnings. This causal inference would be premature without considering whether endogeneity in auditor choice is responsible for the negative association between Big Five audits and fraud.

More specifically, two competing explanations preclude concluding that Big Five auditors actually lower the incidence of accounting fraud. First, an audit firm can refuse to accept an engagement for a company that is likely to violate generally accepted accounting principles, or it can resign from an engagement when a high risk of material misstatements emerges (e.g., DeFond, Ettredge, and Smith 1997). Accordingly, Big Five firms may "screen out" clients that are more apt to commit accounting fraud. Second, there may be a "selection" effect with companies that are intent on engaging in fraudulent financial reporting being reluctant to appoint Big Five audit firms. The potential role of screening and selection in auditor choice motivates our third research question:

HYPOTHESIS 3. *Ceteris paribus, the negative association between Big Five audits and accounting fraud stems from endogeneity in auditor choice.*

3. Research design and data

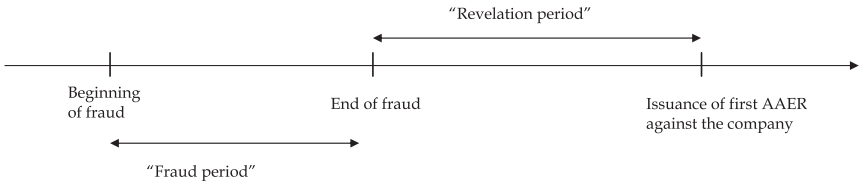
Model specifications

Our inquiry begins by testing Hypothesis 1 that companies with Big Five auditors are less likely to engage in accounting fraud. This involves estimating several variants of the following probit model (subscripts are omitted for notational convenience):

$$Fraud = \alpha_0 + \alpha_1 Big\ Five + Year\ effects + Company\ effects + \varepsilon \quad (1).$$

The dependent variable, *Fraud*, equals 1 if the company engaged in accounting fraud, and 0 otherwise. The treatment variable, *Big Five*, equals

Figure 1 The time line for fraudulent accounting and the Securities and Exchange Commission's issuance of Accounting and Auditing Enforcement Releases (AAERs)



1 if the company is audited by one of the Big Five public accounting firms, and 0 otherwise. Under Hypothesis 1, companies are less likely to be accused of fraudulent reporting if they are audited by a Big Five firm ($\alpha_1 < 0$). Conditional upon finding that α_1 is negative, we test Hypothesis 2 by analyzing whether α_1 becomes less negative over time.

To identify frauds, we collect all the AAERs issued by the SEC that are available from its website and Lexis-Nexis up to October 31, 2006, the date that our data collection ended. Using AAERs to identify fraudulent financial reporting helps avoid any coding biases from the researcher in evaluating whether a certain event constitutes an accounting fraud (Erickson, Hanlon, and Maydew 2006).[6] Since we are concerned with frauds that involve financial accounting, we follow Erickson et al. 2006 and Miller 2006 by removing any non-accounting frauds from the sample. Figure 1 illustrates a time line for fraudulent accounting and the subsequent filing of an AAER against the company by the SEC. We pinpoint the beginning and end of each accounting fraud by reading every AAER leveled against a particular company. Because it can take several years for the SEC to complete its investigation, there is typically a delay between the end of the fraud and the date that the SEC issues its first AAER.

We control for year effects using two alternative specifications. First, we include year dummy variables, which benefits from not imposing a uniform trend in accounting fraud over time. Second, we include a time trend variable since, as we explain in more detail later, the frequency of accounting fraud has been rising, particularly since 1995.

Company-specific control variables

The choice and specification of control variables closely follows recent research on accounting misstatements (e.g., Myers, Myers, Palmrose, and

6. We concede that our design for identifying companies that engage in accounting fraud is necessarily incomplete since, for example, the SEC understandably does not detect every reporting violation (see discussions in DeFond and Francis 2005 and Karpoff et al. 2008). SEC investigations routinely stem from the following activities: (a) the market surveillance programs of the American and New York Stock Exchanges and the National Association of Securities Dealers; (b) public complaints, tips, referrals from other law enforcement agencies, and financial press information; and (c) review of company filings (Pincus, Holder, and Mock 1988).

Scholz 2005; Burns and Kedia 2006; Erickson et al. 2006; Efendi, Srivasta, and Swanson 2007; Dechow, Ge, Larson, and Sloan 2009). *Company Size* equals the natural logarithm of total assets. *Company Age* represents the natural logarithm of the number of years that the company has been listed on COMPUSTAT. *Audit Firm Tenure* is the number of years that the company has retained the same audit firm. Because companies suffering financial distress are more likely to commit fraud (Maksimovic and Titman 1991), we control for this determinant by setting *Negative Book Equity* equal to 1 if liabilities exceed assets, and 0 otherwise. We assign the *M&A Indicator* the value 1 if the company has an acquisition that contributes to sales, and 0 otherwise. We code an indicator variable for the issuance of debt or equity, *Debt & Equity Issued Indicator*, to take the value 1 if the sum of new long-term debt and equity exceeds 20 percent of total assets, and 0 otherwise. Finally, the *Loss* dummy variable equals 1 if net income is negative, and 0 otherwise. Apart from *Company Age* and *Audit Firm Tenure*, which are not affected by outliers, we winsorize all of the continuous control variables at the 1st and 99th percentiles.

In additional analysis, we include the following control variables that have some missing observations. *Inverse Interest Coverage* is interest expense divided by operating income before depreciation. We follow Efendi et al. 2007 by capping the interest coverage ratio at 2.00 and assigning a value of 2.00 if operating income before depreciation is negative. *Sales Growth* equals the percentage change in sales from the prior year to the current year. The *Book-to-Market* ratio equals the book value of equity divided by the market value of equity. The *Working Capital Accruals* variable equals the change in non-cash current assets minus the change in current liabilities (excluding short-term debt and taxes) minus depreciation, scaled by average assets (Dechow et al. 2009). *Return volatility* is the standard deviation of daily stock returns during the calendar year. *External Financing Demand* is a dummy variable that we set to one if *Freecash* < -0.5, where *Freecash* equals cash from operations minus average lagged capital expenditure divided by lagged current assets.

Prior AAER research also examines the association between fraud and corporate governance characteristics (e.g., Beasley 1996; Beasley, Carcello, Hermanson, and Lapides 2000). To avoid the severe attrition that would literally decimate our sample, we do not directly control for corporate governance variables in our main regressions. Instead, we report supplementary regressions using a much smaller sample that includes the corporate governance variables. We begin by constructing the following variables from the Investor Responsibility Research Center (IRRC) database: *%Board independence* (*%Audit committee independence*), which measures the percentage of board (audit committee) members that are outside rather than executive directors; *Board size*, which equals the number of directors that sit on the board; *CEO-Chair duality*, which takes the value 1 if the company's chief executive officer (CEO) is also Chairman of the Board, and 0 otherwise; and *Board meetings* (*Audit committee meetings*), which denotes the number of

meetings held by the board (audit committee) during the year. These variables are available in the IRRC database for the period 1996–2001 except for board meetings (1997–2001) and audit committee characteristics (1998–2001). Given that some of the fraud companies are not covered in the IRRC database, we hand collect the above corporate governance variables from proxy statements for the fraud companies that have missing data.

Importantly, our sample consists of 508 fraud companies. Some frauds last more than one year, which means that our fraud sample consists of 1,109 company-years. The control sample consists of 162,804 years for which companies are not accused of committing fraud. Supplementary regressions are conducted using smaller samples for the variables that have missing data. Because the sample is pooled across company-year observations, the annual observations of a given company might not be drawn independently, so we address this statistical problem by adjusting the coefficients' standard errors by "clustering" on each company (Petersen 2009). Further, we exploit the panel-data nature of our sample by controlling for company-specific effects.

Descriptive statistics

Our sample consists of frauds committed between 1981 and 2001. Some recent frauds under investigation by the SEC had not culminated in the filing of AAERs when we ended our data collection at October 31, 2006. Since this delay causes severe attrition in the frequency of alleged accounting frauds after 2001 and, importantly, our research questions focus on the link between fraud and Big Five audits in the years preceding the Sarbanes-Oxley Act of 2002, we choose a sample period ending in 2001. Table 1 reports the number of annual audits performed by the Big Five and non-Big Five public accounting firms and the frequency of fraud in those audits. The average frequency of accounting fraud is 0.61 percent in Big Five clients compared with 0.92 percent in non-Big Five clients; the difference is statistically significant at under the 0.01 level (t-statistic = −6.14).

In Table 1, we bisect the fraud frequencies for each sample year from 1981 to 2001 by audit firm size. Importantly, these frequencies are lower for Big Five clients in every year from 1981 to 1998. Moreover, the differences in Big Five and non-Big Five fraud frequencies are statistically significant at the 0.05 level or better in 12 of the 15 sample years from 1981 to 1995. In contrast, the Big Five and non-Big Five fraud frequencies are not significantly different between 1996 and 2000, while the sign of the difference switches from negative to positive in 2000. In 2001, the frequency is significantly *higher* in Big Five audits relative to non-Big Five audits (t-statistic = 2.27).

These findings corroborate the prevailing perception that external monitoring by Big Five auditors began to deteriorate in the years leading up to the recent legislative and regulatory reforms (e.g., Coffee 2002; Imhoff 2003; Earley et al. 2003). However, as will be shown later, it is vital to rigorously control for company size when testing Hypothesis 2 because there is an upward trend in fraudulent financial reporting by large companies around

TABLE 1
The frequency of accounting frauds over time: Big Five versus non-Big Five audits

	Big Five audits			Non-Big Five audits			Big Five Fraud % minus Non-Big Five Fraud %	
			Fraud %			Fraud %		
Years	Obs.	Frauds	(1)	Obs.	Frauds	(2)	(1)–(2)	*t*-stat.
1981–2001	130,642	802	0.61	33,271	307	0.92	−0.31	−6.14*
1981	4,116	10	0.24	1,503	9	0.60	−0.36	−2.03†
1982	4,660	18	0.39	1,555	11	0.71	−0.32	−1.61‡
1983	4,876	12	0.25	1,590	12	0.75	−0.51	−2.90*
1984	4,925	13	0.26	1,606	13	0.81	−0.55	−3.02*
1985	5,252	18	0.34	1,687	12	0.71	−0.37	−2.01†
1986	5,537	21	0.38	1,647	7	0.43	−0.05	−0.26
1987	5,763	22	0.38	1,513	7	0.46	−0.08	−0.44
1988	5,653	19	0.34	1,438	13	0.90	−0.57	−2.87*
1989	5,605	29	0.52	1,379	16	1.16	−0.64	−2.67*
1990	5,725	30	0.52	1,277	14	1.10	−0.57	−2.34†
1991	5,872	38	0.65	1,273	20	1.57	−0.92	−3.33*
1992	6,008	44	0.73	1,320	23	1.74	−1.01	−3.49*
1993	6,458	41	0.63	1,424	27	1.90	−1.26	−4.66*
1994	7,086	39	0.55	1,484	15	1.01	−0.46	−2.04†
1995	7,893	30	0.38	1,579	14	0.89	−0.51	−2.70
1996	8,077	47	0.58	1,581	13	0.82	−0.24	−1.11
1997	7,897	53	0.67	1,535	11	0.72	−0.05	−0.20
1998	7,981	69	0.86	1,686	17	1.01	−0.14	−0.57
1999	7,612	84	1.10	1,991	22	1.10	0.00	−0.01
2000	7,104	91	1.28	2,090	19	0.91	+0.37	+1.37
2001	6,542	74	1.13	2,113	12	0.57	+0.56	+2.27†

Notes:

* Significant at the 0.01 level (two-tailed).

† Significant at the 0.05 level (two-tailed).

‡ Significant at the 0.10 level (two-tailed).

the turn of the century (Desai 2005; Lennox and Pittman 2007). In other words, the apparent diminution in Big Five audit quality could actually reflect the increasing propensity for large companies to commit accounting fraud, a competing explanation that we explore in the multivariate analysis.

Table 2 provides tests for significant differences in the means of the independent variables between the fraud sample and the no-fraud sample. Because the sample covers 21 years from 1981 to 2001, we first deflate the variables to 1980 prices using the annual rates of general price inflation.

TABLE 2
Descriptive statistics for the fraud and no-fraud samples

Variable	Sample period	Fraud sample (*Fraud* = 1) Obs.	Fraud sample (*Fraud* = 1) Variable mean	No-fraud sample (*Fraud* = 0) Obs.	No-fraud sample (*Fraud* = 0) Variable mean	Differences in means *t*-statistics
Big Five	1981–2001	1,109	0.72	162,804	0.80	−6.14*
Company Size (total assets)	1981–2001	1,109	1,212.50	162,804	670.90	7.89*
Company Size (log of total assets)	1981–2001	1,109	4.26	162,804	3.79	6.32*
Company Age (years)	1981–2001	1,109	11.02	162,804	12.36	−3.90*
Company Age (log of years)	1981–2001	1,109	1.94	162,804	2.04	−3.28
Audit Firm Tenure (years)	1981–2001	1,109	6.18	162,804	6.82	−3.62*
Audit Firm Tenure (log of years)	1981–2001	1,109	1.42	162,804	1.53	−3.95*
Negative Book Equity Indicator	1981–2001	1,109	0.07	162,804	0.08	−0.95
M&A Indicator	1981–2001	1,109	0.17	162,804	0.08	10.32*
Debt & Equity Issued Indicator	1981–2001	1,109	0.43	162,804	0.33	7.74*
Loss	1981–2001	1,109	0.43	162,804	0.37	4.00*
Leverage	1981–2001	1,108	0.31	162,193	0.30	0.50
Inverse Interest Coverage	1981–2001	1,011	0.74	152,551	0.67	2.84*
Sales Growth	1981–2001	1,040	0.48	145,185	0.30	5.25*
Book-to-Market	1981–2001	1,043	0.50	132,455	0.68	−6.19*
Working Capital Accruals	1981–2001	961	−0.01	132,227	−0.03	5.84*
Return volatility	1981–2001	995	0.05	119,121	0.04	5.34*
External Financing Demand	1981–2001	789	0.19	93,275	0.16	2.18†
Board size	1996–2001	511	6.23	8,854	9.44	−23.14*
%Board independence	1996–2001	511	0.53	8,854	0.77	−35.83*
CEO-Chair duality	1996–2001	511	0.54	8,854	0.66	−5.81*
Board meetings	1997–2001	302	6.73	5,473	7.02	−1.66‡
%Audit committee independence	1998–2001	249	0.93	4,432	0.99	−16.08*
Audit committee meetings	1998–2001	249	2.59	4,432	3.58	−8.65*

(The table is continued on the next page.)

TABLE 2 (Continued)

Notes:

Except for the company's age and audit firm tenure, the continuous variables are winsorized at the top 1 percent and bottom 99 percent percentiles to address outliers. The accounting variables are stated at 1980 prices after adjusting for the effects of general price inflation during the sample period.

Fraud	= 1 if the company is engaged in an accounting fraud during the year, 0 otherwise.
Big Five	= 1 if the company is audited by one of the Big Five firms or their predecessors, 0 otherwise.
Company Size	= log of total assets (data #6).
Company Age	= log of the number of years that the company is listed on COMPUSTAT.
Audit Firm Tenure	= log of the number of years that the company is audited by the same audit firm on COMPUSTAT.
Negative Book Equity Indicator	= 1 if total liabilities (data #181) > total assets (data #6), 0 otherwise.
M&A Indicator	= 1 if the company had an acquisition that contributed to sales (data #249 > 0), 0 otherwise.
Debt & Equity Issued Indicator	= 1 if the sum of new long-term debt (data #111) plus new equity (data #108) exceeds 2percent of total assets (data #6), 0 otherwise.
Loss	= 1 if net income (data #172) is negative, 0 otherwise.
Leverage	= total debt (data #34 + data #9) / total assets (data #6).
Inverse Interest Coverage	= interest expense (data #15) divided by operating income before depreciation (data #13). The ratio is capped at 2.00 and we assign a value of 2.00 if operating income before depreciation is negative.
Sales growth	= percentage change in sales (data #12) from the prior year to the current year.
Book-to-market	= book value of equity (data #216) / market value of equity (data #199 * data #25).
Working Capital Accruals	= ([ΔCurrent assets (data #4) – ΔCash and short-term investments (data #1)] – [ΔCurrent liabilities (data #5) – ΔDebt in current liabilities (data #34) – ΔTaxes payable (data #71)] – Depreciation (data #14)) / Average total assets (data (#6).
Return volatility	= standard deviation of daily stock returns during the calendar year.

(The table is continued on the next page.)

TABLE 2 (Continued)

External Financing Demand	= 1 if *Freecash* < −0.5, 0 otherwise.
Freecash	= (Cash from operations (data #308) – average lagged capital expenditure (data #128) / lagged current assets (data #4). Capital expenditures are averaged over the preceding three years ($t - 3$ to $t - 1$) if data #128 is available in each year. Capital expenditures are averaged over the preceding two years ($t - 2$ to $t - 1$) if data #128 is unavailable in year $t - 3$. Capital expenditures are lagged by one year ($t - 1$) if data #128 is unavailable in year $t - 2$.
Board size	= the number of directors who sit on the board.
%Board independence	= the percentage of board members who are outside rather than executive directors.
CEO-Chair duality	= 1 if the company's CEO is also Chairman of the board, 0 otherwise.
Board meetings	= the number of meetings held by the board of directors during the year.
%Audit committee independence	= the percentage of audit committee members who are outside rather than executive directors.
Audit committee meetings	= the number of meetings held by the audit committee during the year.

* Significant at the 0.01 level (two-tailed).

† Significant at the 0.05 level (two-tailed).

‡ Significant at the 0.10 level (two-tailed).

Table 2 reveals that frauds are less likely to be committed by Big Five clients (t-statistic = −6.14) and more likely by large companies (t-statistics = 7.89, 6.32). The different signs for audit firm size and company size imply that the *Big Five* variable is not spuriously capturing a client size effect, which might otherwise be a concern since larger companies tend to choose Big Five auditors. The relations for the control variables are broadly consistent with prior research (Myers, Myers, and Omer 2003; Burns and Kedia 2006; Efendi et al. 2007; Dechow et al. 2009).

4. Multivariate results

Tests of Hypothesis 1

Table 3 reports evidence on whether the likelihood of accounting fraud varies systematically between Big Five and non-Big Five audits

TABLE 3
The association between accounting fraud and Big Five audits, Hypothesis 1 $Fraud = \alpha_0 + \alpha_1 Big\ Five + Year\ effects + Company\ effects + \varepsilon$

	1	2	3	4	5	6	7	8	9
Big Five	−0.57 (−9.08)*	−0.56 (−8.80)*	−0.64 (−5.88)*	−0.56 (−6.04)*	−0.74 (−7.86)*		−0.48 (−4.48)*		
Arthur Andersen								−0.68 (−7.17)*	−0.85 (−7.47)*
Deloitte & Touche								−0.39 (−4.58)*	−0.39 (−3.69)*
Ernst & Young								−0.64 (−7.50)*	−0.57 (−5.47)*
KPMG								−0.62 (−6.53)*	−0.64 (−5.73)*
Pricewaterhouse Coopers								−0.55 (−6.89)*	−0.53 (−5.24)*
BDO Seidman									−0.01 (−0.07)
Grant Thornton									−0.19 (−1.01)
Time Trend	0.04 (9.78)*								
Company Size	0.11 (8.82)*	0.11 (8.65)*	0.13 (5.72)*	0.16 (8.25)*	0.23 (7.39)*	0.02 (3.47)*	0.02 (0.99)	0.11 (8.81)*	0.13 (8.40)*
Company Age	−0.10 (−3.03)*	−0.11 (−3.13)*	−0.24 (−4.22)*	−0.18 (−3.68)*	−0.04 (−0.63)	−0.08 (−3.74)*	0.03 (0.54)	−0.11 (−3.19)*	−0.10 (−2.53)†
Audit Firm Tenure	0.06 (1.77)‡	0.06 (1.90)‡	0.07 (1.4(4)	−0.01 (−0.20)	−0.01 (−0.09)	−0.05 (−1.96)†	−0.00 (−0.05)	0.07 (2.09)†	0.04 (1.23)
Negative Book Equity Indicator	−0.23 (−2.86)*	−0.24 (−2.97)*	−0.48 (−2.46)†	−0.50 (−3.18)*	−0.12 (−0.92)	−0.18 (−2.65)*	−0.01 (−0.04)	−0.24 (−2.95)*	0.28 (−3.00)*
M&A Indicator	0.30 (5.09)*	0.33 (5.50)*	0.30 (3.56)*	0.27 (3.53)*	0.55 (4.16)*	0.26 (6.21)*	−0.05 (−0.45)	0.33 (5.49)*	0.35 (5.05)*
Debt & Equity Issued Indicator	0.20 (4.47)*	0.21 (4.77)*	0.10 (1.50)	0.10 (1.64)*	0.31 (3.85)*	0.16 (5.11)*	0.08 (0.90)	0.22 (4.83)*	0.21 (4.00)*
Loss	0.20 (4.42)*	0.19 (4.14)*	0.11 (1.16)	0.27 (4.06)*	0.05 (0.58)	0.01 (0.43)	0.02 (0.24)	0.20 (4.24)*	0.19 (3.49)*
Working Capital Accruals			0.83 (2.32)†	0.60 (2.23)†					
Leverage			0.46 (2.43)†	0.14 (1.00)					
Inverse Interest Coverage			0.07 (1.12)						
Sales Growth			−0.05 (−1.51)						
Book-to-Market			−0.16 (−3.09)*	−0.13 (−3.16)*					
Return volatility			−0.65 (−0.51)						

(The table is continued on the next page.)

TABLE 3 (Continued)

	1	2	3	4	5	6	7	8	9
External Financing Demand			0.19 (1.77)‡	0.18 (1.98)†					
Year dummy variables?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Company-specific effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fraud company-years	1,109	1,109	643	748	418	508	508	1,109	924

Notes:

The dependent variable equals 1 if the company engaged in accounting fraud during the year, and 0 otherwise. We control for company-specific effects by estimating random effects probit models. *Z*-statistics are reported in parentheses. Standard errors are adjusted for clustering by company.

Time Trend = one in 1981, two in 1982, …, 21 in 2001, etc. See Table 2 for the definitions of the regression variables. The intercept and year dummy coefficients are unreported for brevity.

In column 5, we use the propensity score matching approach to control for differences in the nature of Big Five and non-Big Five clients. To obtain a matched sample, we first estimate an auditor choice model that predicts whether the company is audited by a Big Five or non-Big Five firm (the untabulated results for the auditor choice model are available from the authors). Each non-Big Five client is then matched to a Big Five client that has the closest propensity score. The results in column 5 are obtained by estimating the fraud prediction model on the non-Big Five and matched Big Five groups.

In columns 6 and 7, we use the propensity score matching approach to control for differences between non-fraud and fraud companies in their last clean year. To obtain a matched sample, we first estimate a fraud prediction model where the dependent variable indicates whether the company is about to commit a fraud (column 6). Each fraud company is then matched to a non-fraud company that has the closest propensity score. We then re-estimate the fraud model on the matched groups, with the Big Five variable included as an independent variable (column 7).

* Significant at the 0.01 level (two-tailed).

† Significant at the 0.05 level (two-tailed).

‡ Significant at the 0.10 level (two-tailed).

(Hypothesis 1). The models are estimated using random effects probit in order to control for unobserved company-specific effects. We do not implement a fixed-effects probit model because it is known to provide inconsistent parameter estimates (Baltagi 1995; Greene 2001). Moreover, the fixed-effects specification would dummy out all companies that experience no variation in the dependent variable over time (Baltagi 1995; Greene 2001), causing all of the no-fraud companies to be dropped from the estimation sample.

In the first five columns, the *Big Five* coefficients are negative ($\alpha_1 < 0$), reinforcing the univariate evidence consistent with Hypothesis 1 that Big Five clients are less likely to be accused of fraud. Indeed, the α_1 coefficients are all statistically significant at under the 0.01 level, with the *z*-statistics ranging from −5.88 (column 3) to −9.08 (column 1). To calibrate the economic magnitude of this association, we predict the probabilities of accounting fraud when the Big Five variable is "switched on" (*Big Five* = 1) and "switched off" (*Big Five* = 0) and the other variables are evaluated at their sample means.[7] Re-writing the fraud model as

$$Fraud = aX + bBigFive + u,$$

we estimate that

$$\hat{b} = -0.57$$

and

$$\hat{a}\bar{X} = -2.015,$$

where

$$\bar{X}$$

is a vector containing the mean values of all independent variables except for *Big Five*. The expected probability of fraud conditional on appointing a non-Big Five auditor is therefore

$$\Pr(Fraud = 1 | Big\ Five = 0) = \Phi(-2.015) = 0.0218,$$

where

$$\Phi(.)$$

is the cumulative normal distribution function for the probit model. In contrast, the expected probability of fraud conditional on appointing a Big Five auditor is

$$\Pr(Fraud = 1 | Big\ Five = 1) = \Phi(-2.015 - 0.57) = 0.0049.$$

7. Although we interpret economic importance using the *Big Five* coefficient from column 1 of Table 3, the coefficients are similar in columns 2 to 7, ranging from −0.48 to −0.74.

Therefore, appointing a Big Five auditor translates into the client becoming more than four times less likely to commit accounting fraud (0.49 percent versus 2.18 percent). The *Company Size* coefficients are all positive and highly significant, reflecting recent evidence that large companies are more likely to engage in accounting fraud (e.g., Fich and Shivdasani 2007). This is consistent with agency conflicts stemming from the separation between managers and outside investors increasing in company size (Berle and Means 1932; Jensen and Meckling 1976). The *Company Age* coefficients load negatively, indicating that fraud companies tend to be younger, although the age coefficients are not statistically significant in column 5 where we use the propensity-score matched control sample. The results do not provide consistent evidence that accounting fraud is associated with the duration of *Audit Firm Tenure*, corroborating recent evidence that long tenure may not undermine audit quality (Carcello and Nagy 2004; Myers et al. 2003). Consistent with Erickson et al. 2006, fraud companies are more likely to be involved in mergers and acquisitions. Similar to Efendi et al. 2007, the issuance of debt and equity is positively related to the probability of accounting fraud, suggesting that companies have more incentive to deliberately misstate their financial statements when they are raising external capital. Although the fraud companies are more likely to report losses, they are less likely to report negative book equity, indicating mixed findings as to the relation between fraud and reported financial condition; however, these variables fail to load when the model is estimated on the matched control sample (column 5).

In column 1, the *Time Trend* coefficient is positive and significant (z-statistic = 9.78), corroborating our univariate evidence that the frequency of accounting frauds rose over the sample period. Since Table 1 indicates that the increase in fraud is not strictly linear over time, we replace *Time Trend* with year dummy variables in columns 2 to 7. In column 3, we add the control variables for working capital accruals, leverage, inverse interest coverage, sales growth, book-to-market, return volatility, and external financing demand. Incorporating these variables causes considerable sample attrition with the number of fraud (no-fraud) observations falling from 1,109 (162,804) to 643 (66,572), although only *Working Capital Accruals*, *Leverage*, *Book-to-Market*, and *External Financing Demand* load. The positive coefficients on *Working Capital Accruals* and *Leverage* imply that the fraud companies are more likely to aggressively manage their earnings and suffer from financial distress, respectively. The negative coefficient on the *Book-to-Market* ratio suggests that fraud companies are perceived by the market to have greater growth opportunities (Erickson et al. 2006), consistent with investors being misled by fraudulent accounting. Finally, the positive coefficient on *External Financing Demand* indicates that fraud companies have stronger incentives to raise external finance. We find nearly identical results in column 4 after recovering some observations by removing the controls that fail to load in column 3.

In column 5, we use the propensity score matching approach to control for differences in the nature of Big Five and non-Big Five clients.[8] To obtain a matched sample, we first estimate an auditor choice model that predicts whether the company is audited by a Big Five or non-Big Five firm. Each non-Big Five client is then matched to a Big Five client that has the closest propensity score. The results in column 5 stem from estimating the fraud prediction model on the non-Big Five and matched Big Five groups.

We also use the propensity score matching approach to control for differences between fraud and non-fraud clients in the last clean year. To obtain a matched sample, we first estimate a fraud prediction model where the dependent variable indicates whether the company is about to commit a fraud (*Fraud_start*). The results from this first-stage model are shown in column 6. In the second stage, we match each fraud company to a non-fraud company based on the closest propensity score. We then estimate the fraud model on the matched groups after including the *Big Five* variable. As shown in column 7, the *Big Five* coefficient remains negative and highly significant (z-statistic = −4.48). Accordingly, this helps dispel the concern that our results spuriously reflect fundamentally different characteristics between fraud and non-fraud companies in the last clean year. The other independent variables are generally insignificant in column 7 which is unsurprising given that the fraud and non-fraud observations are matched on these characteristics in the first stage model estimated in column 6.

In column 8, we replace the *Big Five* dummy variable with dummies for each of the Big Five audit firms to investigate whether the negative relation between fraud and audit firm size extends to all of these firms. The coefficients on each audit firm dummy variable is negative and highly significant, implying that all five firms are less likely to be associated with fraud compared with the non-Big Five. Interestingly, the *Andersen* coefficient is the most negative of the five and an untabulated test reveals that it is significantly more negative at the 0.10 level. This is contrary to the view that Andersen was associated with a higher incidence of financial reporting failures. The *Deloitte & Touche* coefficient is the least negative of the five and an untabulated test reveals that the difference is significant at the 0.01 level.

In column 9, we include dummy variables for the two mid-tier audit firms, BDO Seidman and Grant Thornton, in addition to each of the Big Five audit firms. Audits by the two mid-tier firms are not identified in COMPUSTAT during the period prior to 1988, so the model in column 9 is estimated for the period 1988–2001. In this regression set-up, audit firms other than the largest seven are included in the intercept. The coefficients are statistically indistinguishable from zero for both BDO Seidman and Grant Thornton. In contrast, the coefficients are more negative and highly

8. Similarly, we continue to find strong evidence in size-industry-year matched samples that companies with Big Five auditors are less apt to commit accounting fraud.

significant for each of the Big Five. These results imply that the Big Five firms, unlike BDO Seidman and Grant Thornton, are each less likely to be associated with fraud compared with the other non-Big Five firms. Accordingly, we conclude that the key distinction is between the Big Five and non-Big Five audit firms rather than the mid-tier firms.

Table 4 reports results for the fraud prediction model after inclusion of the corporate governance variables, which substantially reduces the size of

TABLE 4
The association between accounting fraud and Big Five audits after controlling for corporate governance characteristics

	1	2	3
Big Five	−2.33 (−5.26)*	−2.97 (−4.24)*	−4.57 (−4.31)*
%Board independence	−3.73 (−7.40)*	−6.59 (−3.92)*	−0.32 (−0.25)
Board size	−0.17 (−5.17)*	−0.02 (−0.25)	
CEO-Chair duality	0.03 (0.22)	0.37 (1.53)	0.01 (0.02)
Board meetings		−0.05 (−1.14)	
%Audit committee independence			−14.33 (−5.09)*
Audit committee meetings			−0.38 (−3.90)*
Company Size	−0.32 (−5.02)*	−0.41 (−3.23)*	−0.64 (−5.36)*
Company Age	−1.10 (−7.83)*	−1.74 (−6.52)*	−2.60 (−6.38)*
Audit Firm Tenure	−0.09 (−1.14)	0.04 (0.22)	0.42 (2.01)†
Negative Book Equity Indicator	−0.48 (−1.44)	1.97 (2.75)*	4.62 (4.90)*
M&A Indicator	0.37 (2.41)†	0.67 (2.70)*	0.44 (1.40)
Debt & Equity Issued Indicator	0.32 (2.31)†	0.26 (0.70)	0.24 (0.83)
Loss	0.31 (2.17)†	0.54 (2.31)†	0.83 (2.55)†
Year dummy variables?	Yes	Yes	Yes
Company-specific effects?	Yes	Yes	Yes
Sample period	1996–2001	1997–2001	1998–2001
Fraud company-years	511	302	249
No-fraud company-years	8,854	5,473	4,432

Notes:

The dependent variable equals 1 if the company engaged in accounting fraud during the year, and 0 otherwise. We control for company-specific effects by estimating random effects probit models. *Z*-statistics are reported in parentheses. Standard errors are adjusted for clustering by company. See Table 2 for the definitions of the regression variables.

* Significant at the 0.01 level (two-tailed).

† Significant at the 0.05 level (two-tailed).

our fraud and no-fraud samples. Column 1 is estimated for the period 1996–2001 and reveals that *%Board independence* and *Board size* are negatively and significantly related to the likelihood of fraud (z-statistics = −7.40, −5.17, respectively), although CEO-chair duality does not load. More relevant to our research questions, we continue to find that the *Big Five* coefficient is negative and significant at less than the 0.01 level (z-statistic = −5.26).

Despite that this sacrifices power by shrinking the sample size considerably, column 2 adds the *Board meetings* variable and is estimated for the period 1997–2001. Although the number of board meetings is not significantly different between the fraud and no-fraud samples, the *Big Five* variable still loads negatively (z-statistic = −4.24). Finally, after including in column 3 the variables for *%Audit committee independence* and *Audit committee meetings*, we estimate the model for the period 1998–2001. Both of the audit committee variables have significant negative coefficients, implying that fraud is less common when companies have independent audit committees that meet more frequently. Interestingly, the *%Board independence* variable ceases to load when we control for audit committee independence (board independence and audit committee independence are, of course, positively correlated).[9] More important for our purposes, the *Big Five* variable continues to load negatively (z-statistic = −4.31).[10]

Tests of Hypothesis 2

In a preliminary pass at our second research question, we document in Table 1 that the significant negative association between Big Five audits and accounting fraud began to subside over time and even became positive after 1999. These univariate findings are consistent with critics' arguments that the Big Five quality differential started to narrow during the

9. This evidence reconciles with extensive prior research implying that audit committee independence plays a major role in strengthening oversight of the financial reporting process (e.g., Carcello and Neal 2000; Klein 2002; Abbott, Parker, and Peters 2004).

10. Our data collection process for the corporate governance analysis is behind the company size coefficient switching from loading positively in Table 3 to loading negatively in Table 4. Because the IRRC database does not include many of the fraud companies, we resorted to hand collecting the missing corporate governance data from proxy statements to avoid severe sample attrition for this set of companies. However, given that larger companies dominate the IRRC coverage, the fraud companies for which we hand collect data are typically considerably smaller. Accordingly, the companies in the non-fraud sample (comprised strictly of IRRC-covered companies) are, on average, larger than those in the fraud sample, which includes companies for which we recovered through hand collection observations that were missing in the IRRC database. Predictably, we report a significantly negative coefficient on *Company Size* in Table 3, although this variable consistently loads positively in the rest of our regressions.

late 1990s, which was at the root of the rise in accounting fraud frequency.

We now focus on whether the univariate evidence in Table 1 of a structural break persists in a multivariate framework. Specifically, we estimate the accounting fraud models using rolling regressions covering three-year periods. We choose three years because, as shown in Table 1, there are insufficient frauds in individual years to reliably estimate the Big Five coefficients in annual multivariate regressions. For example, there are only 19 frauds in 1981, which includes 10 (9) by Big Five (non-Big Five) clients. Pooling the estimation samples over rolling three-year periods increases the precision of our estimated coefficients, although at the cost of making it more difficult to identify any individual year in which a structural break may occur.[11] Similar to columns 1 and 2 of Table 3, the regressions include controls for *Company Size*, *Company Age*, *Audit Firm Tenure*, *Negative Book Equity Indicator*, *M&A Indicator*, *Debt & Equity Issued Indicator*, and *Loss*. However, for brevity, we only report the coefficients and *z*-statistics for *Big Five*.

Table 5 reveals that the *Big Five* coefficients are negative for each of the 19 rolling regressions that extend from 1981–1983 to 1999–2001. These negative coefficients are statistically significant at under the 0.01 level in 16 out of the 19 regressions, with *z*-statistics ranging from −2.62 to −4.63. In the other three regressions, the negative coefficients are either significant at the 0.05 level (1984–1986), the 10 percent level (1986–1988), or insignificant (1985–1987). Importantly, the *Big Five* coefficients remain negative and significant at the 0.01 level in the years immediately preceding SOX (i.e., from 1996–1998 through 1999–2001), implying that Big Five clients were significantly less likely to engage in fraud.[12] This evidence stands in stark contrast to the univariate analysis in Table 1 where the frequency of accounting frauds is *higher* for Big Five clients than it is for non-Big Five clients in 2001 and statistically insignificant between 1996 and 2000.

To shed light on why these multivariate results reveal a very different pattern from that in Table 1, it is constructive to consider how neglecting to control for company size affects the multivariate inferences. We re-estimate the 19 three-year rolling regressions after dropping *Company Size* and report these results in the second set of columns in Table 5. In the absence of a control for client size, the association between Big Five audits and accounting fraud closely resembles Table 1. Specifically, the *Big Five*

11. In a sensitivity test, we also estimate annual regressions and rolling regressions of two-year periods. The *Big Five* coefficients have a similar pattern to the three-year rolling regressions, but the standard errors of the coefficients are larger, indicating less precise estimation.

12. The pairwise correlation between the *Big Five* coefficients reported in Table 5 and a time vector for the 19 three-year intervals is statistically insignificant, reinforcing the absence of evidence that incremental Big Five audit quality has eroded over time.

TABLE 5
The association between accounting fraud and Big Five audits using rolling regressions of three-year periods

	Multivariate regressions that control for *Company Size*		Multivariate regressions that do not control for *Company Size*	
	Big Five variable		*Big Five* variable	
Years	Coefft.	*z*-stat.	Coefft.	*z*-stat.
1981–1983	−0.40	−3.30*	−0.28	−2.57*
1982–1984	−0.40	−3.54*	−0.30	−2.85*
1983–1985	−0.40	−3.09*	−0.30	−2.73*
1984–1986	−0.28	−2.26†	−0.21	−1.95‡
1985–1987	−0.16	−1.47	−0.11	−1.06
1986–1988	−0.19	−1.86‡	−0.14	−1.37
1987–1989	−0.25	−2.62*	−0.22	−2.26†
1988–1990	−0.29	−2.67*	−0.25	−2.41†
1989–1991	−0.30	−2.89*	−0.25	−2.68*
1990–1992	−0.33	−3.25*	−0.26	−2.95*
1991–1993	−0.38	−4.28*	−0.31	−3.93*
1992–1994	−0.35	−3.97*	−0.30	−3.79*
1993–1995	−0.41	−4.63*	−0.33	−4.23*
1994–1996	−0.31	−3.35*	−0.24	−2.70*
1995–1997	−0.31	−3.08*	−0.17	−1.92‡
1996–1998	−0.25	−2.64*	−0.11	−1.27
1997–1999	−0.29	−3.23*	−0.08	−0.97
1998–2000	−0.29	−3.02*	−0.03	−0.35
1999–2001	−0.25	−2.68*	+0.07	+0.85

Notes:

The dependent variable equals 1 if the company engaged in accounting fraud during the year, and 0 otherwise. We include the same set of control variables as in model 1 of Table 3 (i.e., *Company Age*, *Audit Firm Tenure*, *Negative Book Equity Indicator*, *M&A Indicator*, *Debt & Equity Issued Indicator* and *Loss*). For brevity, the coefficients on the control variables are not reported. See Table 2 for the definitions of the regression variables.

* Significant at the 0.01 level (two-tailed).

† Significant at the 0.05 level (two-tailed).

‡ Significant at the 0.10 level (two-tailed).

coefficients are statistically insignificant in the four rolling regressions from 1996–1998 onwards and the *Big Five* coefficient even becomes positive in the most recent period, 1999–2001. In comparison, the *Big Five* coefficients

are negative and significant at the 5 percent level or better in 11 out of the 15 regressions from 1981–1983 to 1995–1997, confirming that Big Five clients are less likely to be accused of accounting fraud in the earlier years. Accordingly, a failure to account for the influence of company size results in a misleading inference that the negative association between Big Five audits and accounting fraud disappeared in the years leading up to SOX.

To demonstrate this more rigorously, we re-estimate the fraud models after adding interactions between: (a) Big Five audits and time, and (b) company size and time. Table 1 suggests that any structural break in the relation between Big Five audits and accounting fraud occurred in the second half of the 1990s, so our time variable is an indicator that takes the value of 1 in 1996–2001 and 0 in 1981–1995. We include the interaction variable *Big Five * Time Dummy (1996–2001)* in order to test whether the negative association between Big Five audits and fraud is stable over the sample period (Hypothesis 2). A relative decline in Big Five audit quality around the turn of the century would be evident in a positive coefficient on the *Big Five * Time Dummy (1996–2001)* interaction variable. However, we would expect the coefficient to be insignificant if the negative link between Big Five audits and fraud remains stable. We add the interaction *Company Size * Time Dummy (1996–2001)* because recent research finds that large companies were increasingly likely to engage in accounting fraud in the pre-SOX years (Desai 2005).

In column 1 of Table 6, the *Big Five * Time Dummy (1996–2001)* coefficient is positive and statistically significant at under the 1 percent level (z-statistic = 4.41). This corroborates the Table 1 results by suggesting a structural break in the association between Big Five audits and financial misstatements in the period 1996–2001 compared with 1981–1995. However, this apparent structural break disappears once we control for the changing association between company size and fraud. In particular, the *Big Five * Time Dummy (1996–2001)* coefficient becomes much smaller in magnitude as well as statistically insignificant in column 2 after we add the *Company Size * Time Dummy (1996–2001)* interaction. The coefficient for the *Company Size * Time Dummy (1996–2001)* variable is positive and highly significant (z-statistic = 5.33), implying that large companies were increasingly more likely to engage in fraud after 1995.

In columns 3 and 4, we estimate the fraud models after partitioning the sample into two time periods, 1996–2001 and 1981–1995. Column 3 reveals that the *Big Five* coefficient is −0.69 and highly significant (z-statistic = −5.10) during the 1996–2001 period. In column 4, the *Big Five* coefficient has a similar magnitude during the 1981–1995 period, −0.63, and is also highly significant (z-statistic = −7.73). On the other hand, the magnitude of the *Company Size* coefficient is considerably larger between 1996 and 2001 (0.23) compared with the 1981–1995 period (0.11), which confirms that large companies were increasingly likely to engage in accounting fraud in recent years. Unlike the univariate results in Table 1, the multivariate regressions provide strong, robust evidence dispelling the claim that there was a relative

TABLE 6
Changes in the association between accounting fraud and Big Five audits during the sample period, Hypothesis 2

	1	2	3	4	5	6
Big Five	−0.69 (−9.96)*	−0.58 (−8.06)*	−0.69 (−5.10)*	−0.63 (−7.73)*	−0.91 (−7.29)*	−0.73 (−5.33)*
Company Size	0.11 (8.24)*	0.06 (4.04)*	0.23 (8.18)*	0.11 (5.13)*	0.11 (8.54)*	0.04 (1.56)
Time Dummy (1996–2001)	−0.03 (−0.37)	−0.27 (−2.60)*				
Time Trend					0.02 (3.03)*	0.01 (1.18)
*Big Five * Time Dummy (1996–2001)*	0.46 (4.41)*	0.16 (1.33)				
*Big Five * Time Trend*					0.03 (3.12)*	0.01 (1.34)
*Company Size * Time Dummy (1996–2001)*		0.10 (5.33)*				
*Company Size * Time Trend*						0.01 (2.94)*
Company Age	−0.05 (−1.50)	−0.05 (−1.36)	−0.09 (−1.24)	0.01 (0.29)	−0.09 (−2.69)*	−0.08 (−2.50)†
Audit Firm Tenure	0.04 (1.13)	0.04 (1.30)	0.14 (2.39)†	0.03 (0.61)	0.04 (1.39)	0.04 (1.43)
Negative Book Equity Indicator	−0.19 (−2.29)†	−0.17 (−2.03)†	−0.34 (−2.12)†	−0.09 (−0.77)	−0.22 (−2.71)*	−0.21 (−2.52)†
M&A Indicator	0.31 (5.10)*	0.32 (5.19)*	0.36 (3.62)*	0.23 (2.46)†	0.30 (5.06)*	0.31 (5.16)*

(The table is continued on the next page.)

TABLE 6 (Continued)

	1	2	3	4	5	6
Debt & Equity Issued Indicator	0.20 (4.48)*	0.19 (4.43)*	0.15 (1.90)‡	0.29 (4.64)*	0.20 (4.47)*	0.19 (4.41)*
Loss	0.22 (4.95)*	0.22 (4.95)*	0.26 (3.14)*	0.19 (3.03)*	0.20 (4.39)*	0.20 (4.39)*
Company-specific effects?	Yes	Yes	Yes	Yes	Yes	Yes
Sample period	1981–2001	1981–2001	1996–2001	1981–1995	1981–2001	1981–2001
Fraud company-years	1,109	1,109	512	597	1,109	1,109
No-fraud company-years	162,804	162,804	55,697	107,107	162,804	162,804

Notes:

The dependent variable equals 1 if the company engaged in accounting fraud during the year, and 0 otherwise. We control for company-specific effects by estimating random effects probit models. *z*-statistics are reported in parentheses. Standard errors are adjusted for clustering by company. The intercept coefficients are unreported for brevity. *Time Trend* = one in 1981, two in 1982, …, 21 in 2001, etc. See Table 2 for the definitions of the rest of the regression variables.

* Significant at the 0.01 level (two-tailed).

† Significant at the 0.05 level (two-tailed).

‡ Significant at the 0.10 level (two-tailed).

decline in the quality of financial statements audited by the Big Five firms around the turn of the century. In columns 5 and 6, we replace the *Time Dummy (1996–2001)* variable with the *Time Trend* to determine whether our results are sensitive to the definition of time effects. We find that the *Big Five * Time Trend* coefficient loads positively (z-statistic = 3.12) before we add the *Company Size * Time Trend* interaction whereas it is insignificantly different from zero when we control for the evolving association between company size and fraud.[13]

Collectively, these multivariate results imply that the univariate findings in Table 1 actually reflect a structural break in the relation between company size and fraud, rather than a break in the relation between Big Five audits and fraud. In other words, large companies were increasingly likely to engage in fraud in the years leading up to SOX and a failure to consider this phenomenon points to a misleading inference that Big Five audits were no longer associated with less frequent accounting fraud after 1995.

Test of Hypothesis 3

One explanation for our evidence that Big Five audits are negatively associated with fraudulent financial reporting is that the Big Five firms supply better audits stemming from their superior monitoring in the form of discovering and correcting material errors in the financial statements (DeAngelo 1981a). This explanation maps into our research design, which implicitly treats auditor choice as pre-determined. However, given that auditor choice may be endogenous, there are two alternative explanations for the observed negative relation between Big Five audits and accounting fraud. First, the Big Five firms may refuse to audit companies with a high propensity to engage in fraud. Second, clients that have a high propensity to commit fraud may prefer to select non-Big Five audit firms, especially if they perceive that these auditors are less likely to prevent earnings overstatements. In the presence of either of these ''screening'' and ''selection'' phenomena, there is a potential endogeneity problem of unknown severity because Big Five audits are assumed to be pre-determined in our earlier analysis.

To the extent that screening and selection engender endogeneity, it should be more apparent when audit firm tenure is short. For example, suppose that two companies commit accounting fraud in 2000 and both are audited by the same non-Big Five firm. Suppose also that Company A initially hired this auditor in 1990 whereas Company B waited until 1999. It follows that any bias in the coefficient estimates arising from endogeneity is likely to be worse for Company B because its auditor choice decision occurred shortly before it began to perpetrate accounting fraud. For example, Company B may have been audited prior to 1999 by a Big Five firm, which may have resigned after concluding that Company B had become a

13. The low variance-inflation-factors (unreported) for all of the independent variables and interaction terms helps alleviate concerns about multicollinearity.

high-risk client (the screening explanation). Alternatively, Company B may have dismissed its incumbent auditor in 1999 in favor of appointing a lower quality non-Big Five firm during the fraud period (the selection explanation). In either case, endogeneity is more likely to be serious for Company B because there is a shorter lag between its choice of auditor and its decision to orchestrate accounting fraud. Accordingly, treating the Big Five variable as pre-determined is more defensible when audit firm tenure is longer (Myers et al. 2003; Caramanis and Lennox 2008). If the negative association between Big Five audits and fraud is driven by either screening or selection, we would expect the results to be weaker in a long-tenure sample where the audit firm's appointment is essentially pre-determined.[14] Consequently, we re-estimate the fraud prediction models after bisecting the sample at the median duration of audit firm tenure (i.e., five years).

The results reported in Table 7 reveal that the *Big Five* coefficients are negative and statistically significant in both the short and long tenure samples (z-statistics = −6.68, −3.64). More important for our purposes, the *Big Five* coefficients are virtually identical in these samples with values of −0.61 and −0.60, respectively; predictably, the difference is statistically insignificant. This evidence helps dispel the concern that the negative link with Big Five audits reflects endogeneity. Rather, these results imply that Big Five audits have a causal effect in terms of reducing the likelihood of accounting fraud.

Next, we attempt to isolate whether the evidence supports the endogeneity explanation by examining the direction of auditor switching between Big Five and non-Big Five audit firms. For example, it could be that Big Five firms are more likely to resign from companies that are currently committing fraud or that are likely to commit fraud in the near future in order to moderate their exposure to litigation risk (Johnstone 2000). Further, companies could dismiss (appoint) Big Five (non-Big Five) audit firms either during the fraud period or in the years just beforehand in order to reduce the likelihood of detection. Therefore, we test the endogeneity explanation by examining whether fraud companies are more (less) likely to switch from Big Five to non-Big Five firms rather than in the opposite direction, compared with non-fraud companies.

14. In theory, it is possible to test the direction of causality by estimating a Heckman-type model, although we choose not to follow this approach because it is extremely susceptible to econometric problems (see Puhani (2000) for a review). Larcker and Rusticus (2010) emphasize that instrumental variable methods such as the Heckman correction are typically unreliable because the chosen instruments are invalid or weak. In the context of auditor choice, a researcher who wishes to use the Heckman model faces the often intractable task of identifying an independent variable that meets the following conditions: (a) it is exogenous, (b) it is a very powerful predictor of auditor choice in the first stage model, and (c) it does not affect the dependent variable in the second stage model. Francis and Lennox (2009) demonstrate that, in the absence of such a variable, the results from a Heckman model of auditor choice are extremely sensitive to minor changes in model specification.

TABLE 7
The association between accounting fraud and Big Five audits, after partitioning the sample into short and long audit firm tenure

	Short audit firm tenure (≤ 5 years)		Long audit firm tenure (> 5 years)	
	Coefft.	*z*-stat.	Coefft.	*z*-stat.
Big Five	−0.61	−6.68*	−0.60	−3.64*
Company Size	0.09	3.81*	0.11	3.75*
Time dummy (1996–2001)	−0.43	−3.22*	0.18	0.67
*Big Five * Time dummy (1996–2001)*	0.14	0.92	0.05	0.17
*Company Size * Time dummy (1996–2001)*	0.09	2.79*	0.08	2.55†
Company Age	0.10	2.51†	−0.37	−3.75*
Audit Firm Tenure	0.08	1.70‡	0.20	1.67‡
Negative Book Equity Indicator	−0.17	−1.69*	−0.29	−1.43
M&A Indicator	0.41	5.03*	0.14	1.29
Debt & Equity Issued Indicator	0.32	5.38*	−0.00	−0.00
Loss	0.21	3.31*	0.20	2.42†
Company-specific effects?	Yes		Yes	
Sample period	1981–2001		1981–2001	
Fraud company-years	669		440	
No-fraud company-years	87,508		75,296	

Notes:

The dependent variable equals 1 if the company engaged in accounting fraud during the year, and 0 otherwise. We control for company-specific effects by estimating random effects probit models. *Z*-statistics are reported in parentheses. Standard errors are adjusted for clustering by company. See Table 2 for the definitions of the regression variables.

* Significant at the 0.01 level (two-tailed).

† Significant at the 0.05 level (two-tailed).

‡ Significant at the 0.10 level (two-tailed).

Our treatment variable (*Fraud*) equals 1 if the auditor change occurs during a fraud year or during the three-year period prior to its initiation, and it takes the value 0 if the auditor change involves a company that does not commit fraud. We choose a window that extends before the fraud period because auditor switches may occur in anticipation of the actual start of the fraud. However, untabulated results indicate that the conclusions are unchanged if the treatment variable takes the value one only during a fraud

year rather than the pre-fraud period, or if we change the length of the pre-fraud window.

The coverage of auditor changes in the Audit Analytics and Auditor-Trak databases begins in 1992 and 2000, respectively. We merge the two databases in order to identify audit firm changes in the period from January 1, 1992 to December 31, 2001. For inclusion in the sample, we require that data are available on both the identities of the outgoing and incoming audit firms and that the company discloses whether the auditor change stems from a dismissal or a resignation. We drop auditor changes involving Andersen after October 1, 2001 since these were likely prompted by the fall-out from the Enron scandal. Finally, we require that financial statement data are available from COMPUSTAT in order to construct the control variables.[15]

The no-fraud sample comprises 3,478 auditor changes experienced by companies that do not engage in fraud. Among the fraud firms, there are just 46 auditor changes during the fraud year or during the three preceding years. In Table 8, we examine whether the frequency of "upgrades" ("downgrades") from a non-Big Five to a Big Five audit firm (Big Five to non-Big Five) varies systematically between the fraud and no-fraud samples. Panel A reports the univariate results. In both the fraud and no-fraud samples, the majority of auditor changes involve lateral switches from either one Big Five firm to another, or from one non-Big Five firm to another. Specifically, there are 35 (76.1 percent) lateral switches in the fraud sample compared with 2,228 (64.1 percent) in the no-fraud sample. However, these lateral switches cannot explain our core evidence since they do not affect the Big Five dummy variable. There are only 11 non-lateral switches in the fraud sample, which involve seven downgrades and four upgrades. The frequency of both upgrades and downgrades is slightly lower in the fraud sample than in the no-fraud sample. More formally, we are unable to reject the hypothesis that fraud companies are more (less) likely to switch from Big Five to non-Big Five (Big Five to non-Big Five) compared with the no-fraud firms (p-value = 0.896), contradicting the explanation that endogeneity is behind the negative link between Big Five audits and fraud.

Multivariate evidence on the direction of auditor switching is presented in panel B of Table 8. We control for whether the auditor change occurs as a result of an audit firm resignation (*Resign* = 1) or the audit firm is dismissed by the company (*Resign* = 0). Prior studies show that audit firms

15. Regrettably, data are unavailable in COMPUSTAT for many of the SEC registrants covered in the Auditor-Trak and Audit Analytics databases. The reason is that COMPUSTAT covers only publicly traded companies whereas auditor changes are disclosed by many non-traded registrants that file 8-K forms. In an untabulated test, we do not impose the requirement to have data on the control variables and we examine the univariate relation between fraud and the direction of auditor switching. The univariate results with this larger sample are very similar to those tabulated.

TABLE 8
Accounting fraud and audit firm changes

Panel A: Univariate tests

	Non-Big Five to Big Five	Big Five to Non-Big Five	Non-Big Five to Non-Big Five	Big Five to Big Five	Total
Fraud sample	4 (8.7%)	7 (15.2%)	17 (37.0%)	18 (39.1%)	46 (100.0%)
No-fraud sample	431 (12.4%)	819 (23.6%)	859 (24.7%)	1,369 (39.4%)	3,478 (100.0%)

Note:

Test of the hypothesis that fraud companies are more (less) likely to switch from Big Five to non-Big Five auditors (non-Big Five to Big Five auditors) compared with the no-fraud companies ($\chi^2 = 0.017$; p-value = 0.896).

Panel B: Multivariate results

The dependent variable indicates whether the auditor switch is from a non-Big Five to Big Five audit firm, from a Big Five to non-Big Five audit firm, or a lateral switch (i.e., non-Big Five to non-Big Five or Big Five to Big Five). For comparison purposes, the base outcome is lateral switches in columns 1 and 2 and Big Five to non-Big Five switches in column 3.

(The table is continued on the next page.)

TABLE 8 (Continued)

	Non-Big Five to Big Five vs. lateral switch		Big Five to Non-Big Five vs. lateral switch		Non-Big Five to Big Five vs. Big Five to Non-Big Five	
	1		2		3	
	Coefft.	*z*-stat.	Coefft.	*z*-stat.	Coefft.	*z*-stat.
Fraud	−0.38	−0.69	−0.47	−1.10	0.09	0.14
Resign	−0.88	−4.82*	0.52	5.18*	−1.40	−7.50*
GC opinion	−0.39	−2.58*	0.42	4.11*	−0.81	−5.07*
Company Size	−0.14	−6.03*	−0.17	−8.48*	0.03	1.21
Company Growth	0.03	2.88*	−0.12	−3.17*	0.15	4.03*
M&A Indicator	0.30	1.86*	−0.47	−2.66*	0.77	3.60*
Debt & Equity Issued Indicator	0.50	4.49*	0.03	0.31	0.48	3.70*
Negative Book Equity Indicator	−0.66	−3.48*	−0.25	−1.91‡	−0.41	−2.07†
Loss	0.18	1.45	0.15	1.45	0.03	0.22
Fraud sample	46		46		46	
No-fraud sample	3,478		3,478		3,478	

(The table is continued on the next page.)

TABLE 8 (Continued)

Notes:

The fraud sample comprises auditor changes during the fraud years and during the three years prior to commencement of the fraud. The no-fraud sample comprises the auditor changes of companies that do not engage in fraud.

Fraud	= 1 if the auditor change occurs during a fraud year or during the three year period prior to commencement of the fraud, 0 if the auditor change involves a company that does not commit fraud.
Resign	= 1 if the auditor change occurs as a result of an audit firm resignation, 0 one if the auditor change occurs as a result of the audit firm being dismissed.
GC opinion	= 1 if the audit opinion issued prior to the auditor change states that there is an uncertainty with respect to the company's ability to remain a going concern, 0 otherwise.
Company Size	= log of total assets (data #6).
Company growth	= percentage change in total assets (data #6) from the prior year to the current year.
M&A Indicator	= 1 if the company had an acquisition that contributed to sales (data #249 > 0), 0 otherwise.
Debt & Equity Issued Indicator	= 1 if the sum of new long-term debt (data #111) plus new equity (data #108) exceeds 20% of total assets (data #6), 0 otherwise.
Negative Book Equity Indicator	= 1 if total liabilities (data #181) > total assets (data #6), 0 otherwise.
Loss	= 1 if net income (data #172) is negative, 0 otherwise.

* Significant at the 0.01 level (two-tailed).

† Significant at the 0.05 level (two-tailed).

‡ Significant at the 0.10 level (two-tailed).

tend to resign from clients that present high litigation risk and that Big Five audit firms are more sensitive than the non-Big Five to litigation risk (Krishnan and Krishnan 1997; Shu 2000). Consequently, we expect that *Resign* is negatively associated with switches from non-Big Five to Big Five auditors and positively associated with switches in the opposite direction. Distressed companies pose higher litigation risks, so we control for *GC opinion* which equals 1 if the audit opinion issued prior to the auditor change states that there is an uncertainty with respect to the company's ability to remain a going-concern, and 0 otherwise. Similar to the *Resign* variable, we expect that companies are more likely to switch away from Big Five auditors to the non-Big Five when they have received going-concern audit opinions. We control for *Company Size* since large companies tend to appoint Big Five rather than non-Big Five auditors. However, we do not form a prediction as to how the company's size level affects the direction of switching between large and small auditors. We expect that companies increasing in size are more likely to upgrade from non-Big Five to Big Five auditors, while companies in decline would downgrade in the other direction. We control for the change in size using *Company Growth*, which is the percentage change in total assets from the prior year to the current year. Companies grow more quickly if they engage in merger and acquisition activity, so we also include the *M&A Indicator* dummy variable. As with the *Company Growth* variable, we expect positive coefficients for upgrades to larger auditors and negative coefficients for downgrades. Numerous studies find that companies raising external finance benefit from appointing higher quality auditors (Balvers, McDonald, and Miller 1988; Beatty 1989; Willenborg 1999), so we include *Debt & Equity Issued Indicator* as a control variable. Finally, we control for the company's financial health using the *Negative Book Equity Indicator* and *Loss* variables.

Besides that the controls generally load in the predicted directions, panel B of Table 8 indicates that the *Fraud* variable is insignificant in explaining the propensity for auditor upgrades and downgrades.[16] Collectively, we fail to find that fraud companies are switching away from the Big Five audit firms or toward the non-Big Five firms, which is inconsistent with the argument that endogeneity is responsible for our primary results. Instead, these results reinforce our evidence in Table 7 that the Big Five effect is causal with these audit firms helping to prevent fraudulent financial reporting. In a nutshell, the evidence in this section enables us to more

16. We continue to find that *Fraud* consistently fails to load when we add controls to reflect changes in operating cash flows scaled by assets, changes in new financing scaled by assets, and changes in times interest earned scaled by assets after Johnson and Lys (1990), although this leads to the fraud (no-fraud) sample falling to 2,147 (27) observations. The core evidence in Table 8 also remains when re-estimate the models after dropping all the control variables, which increases the number of auditor changes to 18,011 (98) in the fraud (no-fraud) samples.

narrowly interpret the negative relation between Big Five audits and accounting fraud as empirically validating theory (e.g., DeAngelo 1981a) that these auditors provide higher-quality detection and correction against materially inaccurate financial statements.

5. Conclusion

In this paper, we rely on the incidence of accounting fraud to analyze whether the relative quality of financial statements audited by the Big Five firms began to slide in the years leading up to SOX. Our empirical strategy involves examining a specific event, fraudulent financial reporting, that has become a major focal point for justifying sweeping legislative and regulatory changes after high-profile corporate governance failures like Enron and Worldcom. Many interpret the sudden surge in fraudulent financial reporting as smoking-gun evidence that the relatively superior quality of Big Five audits has fallen in recent years. We shed light on whether this evidence persists in a multivariate framework that controls for company size, which is crucial since larger companies tend to retain Big Five auditors.

In both unmatched and propensity score matched samples, our analysis reveals that the steep rise in accounting frauds by Big Five clients — that has spawned many papers that strive to explain the reasons behind the apparent erosion of Big Five quality — is simply an artifact of company size. After properly controlling for company size and other determinants, we find that the incidence of fraudulent financial reporting is consistently lower for Big Five clients. Reflecting the first-order economic importance of audit quality, our probit coefficient estimates translate into Big Five clients being, on average, about four times less likely to commit accounting fraud.

In 18 of 19 rolling regressions covering each three-year period within 1981 to 2001, we document a statistically significant negative relation between the presence of a Big Five auditor and the incidence of alleged accounting fraud. The time-series evidence indicates that this relation was stable over time, including the 1996–2001 period in which the univariate evidence would suggest a structural break in the association between Big Five audits and accounting fraud. Importantly, we also report results corroborating that this evidence likely stems from Big Five firms genuinely conducting better audits, rather than from endogeneity.

Collectively, our research implies that the Big Five public accounting firms consistently supplied higher-quality external monitoring from 1981 to 2001, including the last five years in this timeframe when fraudulent financial reporting by U.S. public companies spiked. Still, although we provide strong, robust evidence that Big Five clients are less likely to orchestrate accounting fraud, it is important to stress that the economic fallout from fraud can be dire in these situations since the Big Five tend to audit larger companies. In addition, we analyze the *relative* quality of Big Five audits compared with the non-Big Five and the results should not be

interpreted in terms of trends in absolute audit quality. Finally, we focus on the link between auditor choice and the incidence of accounting fraud given that criticism of the Big Five stresses their apparent role in the prominent financial reporting failures, although future research could complement our analysis by examining whether our evidence extends to other settings.

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