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Tax Aggressiveness and Accounting Fraud

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ABSTRACT

There are competing arguments and mixed prior evidence on whether firms that are aggressive in their financial reporting exhibit more or less tax aggressiveness. Our research contributes to resolving this issue by examining the association between aggressive tax reporting and the incidence of alleged accounting fraud. Relying on several proxies for tax aggressiveness to triangulate our evidence, we generally find that tax aggressive U.S. public firms are *less* likely to commit accounting fraud. However, we caution that our results are sensitive to how tax aggressiveness is measured. More specifically, four (two) of the five (three) proxies for firms' effective tax rates (book-tax differences) load positively (negatively) during the 1981–2001 period, implying that fraud firms are less tax aggressiveness. Our inferences persist when we isolate the 1995–2001 period in which accounting impropriety steeply rose and corporate tax compliance steeply fell. Moreover, we continue to find that tax aggressive firms are less apt to fraudulently manipulate their financial statements when we apply factor analysis to identify tax avoidance with a common factor extracted from the underlying proxies and match on propensity

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scores to ensure that the fraud and nonfraud samples have very similar non-tax characteristics.

1. Introduction

Recent research motivates our analysis of whether aggressive tax positions are associated with the incidence of accounting fraud allegedly perpetrated by U.S. public companies. Several trends spanning the mid-1990s to the early 2000s suggest that tax aggressiveness has begun to routinely accompany financial reporting aggressiveness. This apparent dynamic is evident in a steep rise in the frequency of accounting fraud and a fall in corporate tax compliance during this period. For example, the press highlights that over 50 major U.S. public firms were under investigation for accounting fraud or other financial irregularities in 2002 alone (Stoller [2002]). Although often less severe than accounting fraud, nearly 10% of all firms listed on the NYSE, NASDAQ, and Amex announced at least one earnings restatement between January 1997 and June 2002 with the number of companies restating their earnings climbing by 145% in this short time frame (General Accounting Office [2002]). In our own sample, the number of companies engaged in accounting fraud increases monotonically between 1995 and 2000.

In time-series evidence, Plesko [2000] finds that pretax book income grew at a faster rate than current or deferred tax expense from 1994 to 1998. He suggests that increased participation in tax shelters is one potential explanation for this divergence, although financial reporting aggressiveness may play a role as well. Similarly, Desai [2005] reports that in the mid-1990s actual book income began to seriously deviate from both taxable income and simulated book income—essentially, what book income should have been given genuine differences between financial accounting and tax reporting. Although actual marginal corporate tax rates were quite stable over this period, Yin [2003] estimates that the effective tax rates of companies in the S&P 500 slid from 28.9% in 1995 to 24.2% in 2000. In the same time frame, the GAO [2003] reports that the fraction of large companies paying no income taxes jumped from 32.7% to 45.3%. Finally, Desai [2003] finds that the explanatory power of annual regressions of book income on taxable income subsides over time.

Notwithstanding that aggregate time-series trends indicate that tax aggressiveness has begun to coincide with aggressive financial reporting, firm-level empirical research seldom examines this issue.[1] However, there are two important exceptions, albeit with conflicting results. First, in an extensive analysis of 27 firms censured by the Securities and Exchange Commission (SEC) for overstating earnings that were later restated,

[1] Shevlin [2002] cautions against drawing conclusions about aggregate trends in aggressive tax reporting stemming from book-tax differences, reinforcing the contribution of our firm-level evidence to extant research.

Erickson, Hanlon, and Maydew [2004] conclude that these firms, on average, deliberately overpaid their taxes by 11 cents to legitimize each dollar of fraudulently inflated earnings. Their small-sample evidence implies that some firms orchestrating accounting fraud rely on exaggerating their tax obligations to help disguise their deceit.[2]

In the other direction, Frank, Lynch, and Rego [2009] estimate a positive relation between financial reporting aggressiveness and tax aggressiveness, consistent with firms concurrently managing book income upward and taxable income downward. This evidence provides some support for arguments in Desai [2005] and Desai and Dharmapala [2006] that managers exploit complex tax avoidance strategies—under the pretext that lowering taxes benefits shareholders as the residual claimants—to divert corporate resources, which they later hide by distorting the firm's financial statements (e.g., La Porta et al. [1998], Dyck and Zingales [2004]). Despite that Frank, Lynch, and Rego [2009] highlight the recent watershed accounting scandals when developing their predictions, they quantify aggressive financial reporting with general earnings management, a construct that is notoriously difficult to measure (e.g., Dechow, Sloan, and Sweeney [1995], Dechow, Ge, and Schrand [2010], Guay, Kothari, and Watts [1996], Healy and Wahlen [1999], Hribar and Collins [2002], Wysocki [2004]).[3] Indeed, Ball [2009, p. 281] calls for more evidence on fraudulent financial reporting given the limitations inherent with earnings management research using discretionary accruals:

> The advantages of focusing on negligent and fraudulent reporting include: a proven case of negligent or fraudulent financial reporting is an institutional "fact," as distinct from an error-prone academic estimate; reporting negligence and fraud have been shown to have substantial adverse effects on firm values (Palmrose, Richardson, and Scholz [2004]); and the sight of executives being led away in handcuffs under indictment for reporting fraud created more scandal than a whole literature of Jones-model discretionary accrual estimates.

Similarly, DeFond [2010, p. 9] stresses that:

> Earnings restatements and SEC Accounting and Auditing Enforcement Releases (AAERs) are potentially attractive alternatives to abnormal

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[2] Ettredge et al. [2008] find that the disclosure of corporate tax details under SFAS No. 109 facilitates distinguishing firms that commit accounting fraud from a control sample of nonfraud firms, implying that some companies neglect to cover their tracks through book-tax conformity.

[3] Hanlon and Heitzman [2010] raise some concerns about the regression variables used in Frank, Lynch, and Rego [2009] that are derived from discretionary accruals models. Moreover, Blaylock, Shevlin, and Wilson [2012] provide evidence inconsistent with that of Frank, Lynch, and Rego [2009] that aggressive financial reporting firms are more likely to pursue aggressive tax reporting strategies.

accruals as a proxy for earnings quality. One perceived advantage of restatements and AAERs over abnormal accruals is that they appear to be more direct proxies for earnings quality. Restatements and AAERs are actual events, rather than error terms from a statistical model that cannot be validated.

We sharpen the analysis of this research question by isolating whether tax aggressiveness is associated with the probability that firms perpetrate fraudulent financial reporting in the 1981–2001 period.[4] For both types of behavior, we specify proxies that suffer from less measurement error than those employed in recent research. In particular, we identify accounting fraud by examining Accounting and Auditing Enforcement Releases (AAERs), which outline the SEC's actions to enforce financial reporting through civil litigation and administrative proceedings, leveled against companies.[5] This provides an opportune testing ground for our research given the broad consensus that AAERs amount to egregious violations of generally accepted accounting principles (GAAP) (e.g., Miller [2006]). Unlike earnings management that can comply with GAAP, financial accounting fraud that by construction involves severe deception certainly violates GAAP; e.g., deliberately misstating financial statements is the definition of fraud under SAS No. 99. Analyzing a large sample of AAERs extends both the small-sample tests in Erickson, Hanlon, and Maydew [2004] and examines extreme cases of financial reporting aggressiveness not specifically examined in Frank, Lynch, and Rego [2009].

Our research benefits from including several newly developed proxies for tax aggressiveness. Hanlon and Heitzman [2010] conceptualize tax minimization behavior as occurring along a continuum on which various proxies for tax aggressiveness may be more or less suited to a particular research question. The continuum ranges from tax avoidance (e.g., long-run cash effective tax rates developed by Dyreng, Hanlon, and Maydew [2008])

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[4] Our sample period ranges from 1981, the earliest year with accounting fraud data available, to 2001, the latest year for which we can reliably observe the incidence of accounting fraud given the lengthy lag between when a fraud occurs and its detection by the SEC according to AAER filings. An upside of focusing on this time frame is that we avoid shifts stemming from the major legislative and regulatory reforms to the U.S. capital markets in the aftermath of the highly visible financial reporting failures near the turn of the century. However, this is also a limitation in that our inferences on the link between tax aggressiveness and financial reporting aggressiveness do not apply to, for example, the period after the Sarbanes-Oxley Act of 2002 (SOX).

[5] SEC enforcement actions are informative according to investor perceptions. Dechow, Sloan, and Sweeney [1996] and Beneish [1999] document that firms incur a significant stock price penalty when the SEC pursues them for violating accounting standards. More recently, Johnson, Ryan, and Tian [2009] estimate that equity values fall about 20% when investors learn that the SEC has accused the firm of accounting fraud; similarly, Graham, Li, and Qiu [2008] report that debt financing becomes more expensive in these situations. Civil lawsuits brought by shareholders in companies that are also targets of such SEC investigations are more likely to succeed (Palmrose [1991]).

to tax aggressiveness (e.g., total book-tax differences developed by Mills [1998]; and discretionary permanent book-tax differences developed by Frank, Lynch, and Rego [2009]) to tax sheltering (e.g., probability of a firm engaging in a tax shelter developed by Wilson [2009] and Lisowsky [2010]). By applying recent innovations in the tax aggressiveness literature in a large sample, we evaluate whether various tax minimization activities are related to extreme financial reporting aggressiveness—namely, accounting fraud. To comprehensively analyze our research question to help resolve whether firms that are aggressive in their financial reporting are more (e.g., Frank, Lynch, and Rego [2009]) or less (e.g., Erickson, Hanlon, and Maydew [2004]) tax aggressive, we gauge firms' tax burdens with several proxies for both effective tax rates and book-tax differences.

Our analysis includes comparing fraud firms with firms not accused of fraud by the SEC. In probit regressions that control for unobservable firm-specific effects, we generally find that fraud firms are less tax aggressive.[6] This inference persists even after we narrow our analysis to the 1995–2001 timeframe in which book income began to drastically exceed taxable income and accounting fraud surged. However, we caution that our evidence is sensitive to how the tax aggressiveness constructs are measured. Consistent with fraud firms exhibiting *lower* tax aggressiveness, we find that for the entire 1981–2001 sample period four (two) of our five (three) proxies for firms' effective tax rates (book-tax differences) load positively (negatively).[7] Given our interest in helping to clarify the mixed evidence to date, it is important to stress that the only book-tax difference variable that has no perceptible association with accounting fraud likelihood is the DTAX measure of Frank, Lynch, and Rego [2009] that reflects discretionary permanent book-tax differences. However, some recent studies cast doubt on the suitability of this variable for our setting (e.g., Wilson [2009], Hanlon and Heitzman [2010], Lisowsky [2010], Lisowsky, Robinson, and Schmidt [2012]).

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[6] After Bonner, Palmrose, and Young [1998] and Erickson, Hanlon, and Maydew [2004, 2006], we label companies that the SEC sanctions for intentionally misstating their earnings as fraud companies in the rest of this paper, although these are technically fraud-accused companies. Prior research implies that evidence on the factors not affecting the frequency of corporate misreporting is valuable in its own right. For example, Kinney, Palmrose, and Scholz [2004] find no pervasive relation between non-audit services and restatements, while Erickson, Hanlon, and Maydew [2006] and Armstrong, Jagolinzer, and Larcker [2010] fail to find any consistent evidence that equity-based compensation is associated with accounting fraud. However, supporting theory that large equity incentives can induce corporate misreporting (e.g., Goldman and Slezak [2006]), other research implies a positive link between option-based executive compensation and earnings manipulation evident in, for example, the likelihood of restatements (e.g., Burns and Kedia [2006], Efendi, Srivastava, and Swanson [2007], Harris and Bromiley [2007]) and the magnitude of discretionary accruals (e.g., Bergstresser and Philippon [2006]). Armstrong, Jagolinzer, and Larcker [2010] comprehensively review recent evidence on the relation between executives' equity incentives and various types of accounting irregularities.

[7] All core results are nearly identical when we isolate the 1995–2001 period.

We continue to find in both the 1981–2001 and 1995–2001 periods that the likelihood that U.S. public firms commit accounting fraud declines with corporate tax avoidance when we apply factor analysis to identify tax aggressiveness with a common factor extracted from the underlying proxies. Our main results also persist when we use matched propensity scores to ensure that the fraud and nonfraud samples are similar with respect to the nontax independent variables that are associated with the likelihood of fraud. To provide some perspective on the economic magnitude of our coefficients, we estimate that the probability of fraud increases 17% (61%) as tax avoidance decreases in the inter-quartile range of GAAP effective tax rates during the 1981–2001 (1995–2001) period.

Collectively, we find that the incidence of accounting fraud falls with tax aggressiveness. Still, we stress the importance of interpreting our results with caution since two individual tax proxies fail to load and another is only statistically significant at the 10% level. However, we are in a stronger position to conclude that our analysis does not support arguments (e.g., Desai and Dharmapala [2006]) and evidence (e.g., Frank, Lynch, and Rego [2009]) that aggressive financial reporting tends to accompany aggressive tax reporting. Our mostly statistically significant results run in the opposite direction. In short, consistent with Erickson, Hanlon, and Maydew [2004], we provide compelling large-sample evidence that fraud, an extreme type of earnings management, is *not* associated with greater tax aggressiveness.[8]

The rest of this paper is organized as follows. Section 2 reviews prior theory and evidence to develop our research question. Section 3 outlines our empirical design and data. Section 4 reports our results. Section 5 concludes.

2. *Motivation*

Erickson, Hanlon, and Maydew [2006], Ball [2009], and Kedia and Philippon [2009], among many others, stress the importance of examining fraudulent financial reporting given that the economic and social fallout from these events can be massive.[9] We contribute to clarifying recent

[8] In a small-sample study focusing on income-increasing misstatements, Badertscher et al. [2009] analyze 34 fraud firms and find that those using high-quality auditors and engaging in accounting fraud have a higher chance of practicing conforming earnings management where earnings and taxable income are relatively close in value. We extend both Erickson, Hanlon, and Maydew [2004] and Badertscher et al. [2009] by employing a variety of tax aggressiveness measures and rigorously examining their relation to a large sample of fraudulent financial reporting.

[9] Companies subject to SEC enforcement for financial reporting violations lose, on average, $381 million in share value through legal and reputational penalties according to Karpoff, Lee, and Martin [2008]. They also estimate that firms implicated by the SEC incur, for every dollar in exaggerated earnings, $0.36 in fines and class action settlements and another $2.71 in reputational damage. Moreover, this analysis likely understates the overall impact of corporate misreporting by ignoring, for example, the major reputational damage suffered

research by examining large-sample evidence of tax aggressiveness in the specific context of accounting fraud committed by U.S. public companies. Besides the mixed prior evidence, there is another reason to analyze this issue: there are competing arguments on whether fraud firms are more or less tax aggressive.

In one direction, Desai [2005] and Desai and Dharmapala [2006] hold that timing the overstatement of accounting income to coincide with the understatement of taxable income provides cover that facilitates the diversion of corporate resources. This may involve firms arranging complex tax shelters—which they can readily justify since lowering the fraction of income that is shared with the government benefits investors—that they later exploit in pursuing their own interests. Tax aggressiveness under this argument widens the scope for managers to siphon resources at the expense of outside investors.

This agency perspective is rooted in the complementarities between aggressive tax avoidance and managerial expropriation. Desai, Hogan, and Wilkens [2006] suggest that complex tax transactions that conceal income from the government may, in turn, prevent investors from properly monitoring managers to constrain the extraction of corporate resources. In fact, several of the firms culpable in the recent high-profile financial reporting failures, including Dynegy, Enron, Tyco, and WorldCom, had implemented aggressive tax planning strategies that were conducive to suppressing information, ensuring that insiders' diversionary practices were kept hidden (e.g., Slemrod [2004], Desai [2005], Desai and Dharmapala [2006], and Graham and Tucker [2006]).[10] In short, undertaking aggressive tax positions under the guise of lowering corporate taxes can provide self-dealing managers with opportunities to manipulate earnings.

Indeed, Frank, Lynch, and Rego [2009] find that tax aggressiveness is positively related to financial reporting aggressiveness. In measuring financial reporting aggressiveness, they employ performance-matched discretionary accruals (see Kothari, Leone, and Wasley [2005]), which does

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by individual managers and directors (e.g., Desai, Hogan, and Wilkens [2006] and Fich and Shivdasani [2007]) as well as auditors (e.g., Carcello and Palmrose [1994] and Jones and Weingram [1996]). Finally, Dechow, Sloan, and Sweeney [1996] estimate that firms divulging that they have engaged in accounting fraud experience a 9% decline in their stock price, an increase in their bid-ask spreads, and a reduction in analyst coverage.

[10] Our sample includes all of these prominent fraud companies. The GAO [2003, p. 1] defines abusive shelters as "very complicated transactions promoted to corporations and wealthy individuals to exploit tax loopholes and provide large, unintended benefits." In small sample evidence, Graham and Tucker [2006] estimate that the average tax shelter deduction amounts to 9% of companies' asset value. In congressional testimony, Shackelford [2006, p. 3] stresses that: "...widely held public companies show little interest in tax reductions that adversely affect their financial reports. They sometimes even have limited interest in tax reductions that do not benefit accounting earnings." Similarly, Weisbach [2002] and McGill and Outslay [2002] highlight that tax shelters seldom lead to lower accounting income, reflecting that managers resist undertaking transactions that might cast the firm in a negative light in the capital markets.

not distinguish between fraud and nonfraud earnings management. In measuring tax aggressiveness, they use discretionary permanent book-tax differences, or the residual from a regression of permanent book-tax differences, estimated from the financial statements, on items known to create permanent book-tax differences, such as goodwill, consolidation accounting, state taxes, and change in net operating losses. The rationale behind this metric is that tax shelters derive their value through increasing (lowering) accounting earnings (taxable income), and reducing the reported effective tax rate. In other words, these outcomes stem from engaging in discretionary transactions that produce permanent book-tax differences.

Essentially, Frank, Lynch, and Rego [2009] focus on discretionary permanent differences to capture potential tax sheltering activity. In validating their measure using Graham and Tucker's [2006] sample of tax shelter firms, they find that the discretionary permanent book-tax differences variable (DTAX) is positively related to tax shelter incidence. However, Lisowsky [2010] finds no significant relation between DTAX and a large sample of tax shelter incidence from 2000 to 2004 obtained from the Internal Revenue Service's (IRS) Office of Tax Shelter Analysis (OTSA).[11] Since the discretionary permanent book-tax differences measure ranges toward the more aggressive side of the tax avoidance continuum (Hanlon and Heitzman [2010]), we estimate its association with accounting fraud to facilitate comparing evidence of Frank, Lynch, and Rego [2009] to ours. This analysis reflects that we are eager to comprehensively examine our research question by gauging tax aggressiveness in alternate ways in order to help empirically settle whether firms undertaking aggressive tax positions are more aggressive in their financial reporting given the mixed prior research on this issue. Importantly, we primarily focus on measures that are more toward the tax aggressiveness side of the Hanlon and Heitzman [2010] continuum given that fraud firms are very aggressive in their financial reporting; i.e., these measures better map into the intuition underlying our predictions.

Frank, Lynch, and Rego [2009] in additional tests find a significantly positive association between total book-tax differences and performance-matched discretionary accruals. However, not only may total book-tax differences indicate low financial earnings quality (Hanlon [2005]), restatements (Badertscher et al. [2009]), or fraud (Ettredge et al. [2008]), but also they may indicate tax risk (Mills [1998]), including tax shelters (Desai [2005], Wilson [2009], and Lisowsky [2010]).

In calling for more evidence on this issue, Hanlon and Heitzman [2010] conclude that the supposition that tax positions generating permanent

[11] In supplemental tests, Lisowsky [2010] also finds no significant relation between tax shelter usage and AAERs, net income restatements, and tax expense restatements, although sample sizes for all events are very small. For a more recent sample period, 2006–2009, Lisowsky, Robinson, and Schmidt [2012] also find no consistent evidence of a positive association between DTAX and tax shelter participation according to data obtained from OTSA.

differences reflect tax aggressiveness is difficult to square with recent research. Although Wilson [2009] finds that both temporary and permanent book-tax differences are statistically significant in predicting firms' participation in tax shelters, the temporary differences load more strongly. In fact, only the temporary book-tax differences remain significant when Wilson [2009] integrates a broad control sample into his analysis. In corroborating evidence using tax shelters reported to the IRS as reportable transactions, Lisowsky, Robinson, and Schmidt [2012] find that temporary book-tax differences are a more common feature of tax shelters than permanent book-tax differences.

In an alternative perspective to the arguments in Desai [2005] and Desai and Dharmapala [2006] and the evidence in Frank, Lynch, and Rego [2009], Erickson, Hanlon, and Maydew [2004, p. 388] suggest that: "managers may willingly have their firms pay taxes on the earnings overstatements to avoid raising the suspicion of savvy investors, the Securities and Exchange Commission (SEC), or the Internal Revenue Service (IRS)."[12] Given that managers tend to distort their firms' financial reporting when they are diverting more resources (La Porta et al. [1998], Dyck and Zingales [2004], Guedhami and Pittman [2006]), it follows that they will attempt to deflect attention from various external monitors in these situations.

For example, Mills [1998] and Mills and Sansing [2000] provide evidence from confidential corporate tax returns that the government tends to audit transactions that generate book-tax differences.[13] In fact, IRS guidelines specifically instruct their agents to reconcile differences between book and taxable income, consistent with both tax advisors' (Cloyd [1995]) and corporate managers' (Cloyd, Pratt, and Stock [1996]) perceptions that conformity leads to lower tax audit costs.[14] In fact, large book-tax differences even attract greater scrutiny from external auditors (Hanlon, Krishnan, and Mills [2012]). More generally, Shackelford and Shevlin [2001] and Hanlon and Heitzman [2010] comprehensively survey earlier research implying that firms experience tension in preferring to report higher book income to capital market participants and lower taxable income to tax authorities. The standard assumption had been that firms could not manage

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[12] Although the IRS is permitted to divulge confidential tax information to other federal agencies when there is evidence implicating the firm in criminal activities according to §6103(i)(3) of the Internal Revenue Code, the IRS did not detect any of the 27 accounting frauds under study in Erickson, Hanlon, and Maydew [2004], a result that Dyck, Morse, and Zingales [2010] corroborate in large-sample evidence.

[13] Prior theory (e.g., Desai, Dyck, and Zingales [2007]) and evidence (e.g., Guedhami and Pittman [2008]) implies that strict IRS monitoring improves corporate governance.

[14] In 2004, the IRS began requiring firms with $10 million or more in assets to file the Schedule M-3, which reconciles worldwide financial income to taxable income reported on the U.S. tax return. Book-tax differences related to both consolidation and income differences are enumerated there, presumably for tax risk assessment purposes. See discussions in Mills and Plesko [2003], Boynton and Mills [2004], and Boynton, DeFilippes, and Legel [2008].

book and taxable income in the opposite directions without arousing attention from, for example, the IRS, which could constrain the ability of managers to engage in accounting fraud.

In regressions that control for relevant firm-level characteristics, Dyreng, Hanlon, and Maydew [2010] find that executives affect corporate tax avoidance activities according to both GAAP and cash effective tax rates. They examine the importance of "tone at the top," that is, the executives' approach to corporate tax avoidance. They do not, however, analyze whether the "tone at the top" in the form of tax avoidance activities also affects the incidence of accounting fraud. Assuming firm tax and financial policies are set by the same managers, it is interesting to examine how firms combine their decisions on aggressive financial reporting and aggressive tax avoidance. Moreover, Hanlon and Heitzman [2010] stress that tax avoidance does not necessarily reflect agency problems. Consequently, our prediction focuses on clarifying the association between aggressive financial and tax reporting (the hypothesis is stated in the alternative form but without a signed prediction):

H1: Firms' tax aggressive behavior is associated with their probability of committing accounting fraud.

3. Sample and Research Design

3.1 SAMPLE

The SEC's [2002, p. 1] enforcement program strives to "promote the public interest by protecting investors and preserving the integrity and efficiency of the securities markets." Prosecution under the accounting fraud provisions of the Securities Act of 1934 occurs when the agency determines that there is sufficient evidence to warrant charging the company or its executives with the deliberate filing of materially inaccurate financial statements.[15] We identify frauds by collecting from the SEC Web site and Lexis-Nexis the AAERs, which outline the results of the SEC's investigations into alleged accounting violations, issued between January 1, 1981 and October 31, 2006, the date we finished compiling the sample.[16] A single

[15] Karpoff, Lee, and Martin [2008] comprehensively review the enforcement practices at the SEC and Department of Justice. For enforcement actions involving fraud allegations brought under the 1933 Securities Act or the 1934 Securities Exchange Act, they report that 67% charge the targeted company (or at least one related individual) with civil fraud; the remaining 33% involve criminal prosecution for fraud.

[16] We stress that it is plausible that the nonfraud (supposedly "innocent") firms in our control samples commit fraud that remains undetected by the SEC. For example, the SEC may have more targets for formal investigations than the agency can afford to pursue (Feroz, Park, and Pastena [1991], Dechow, Ge, and Schrand [2010]). Given their resource constraints, the SEC does not identify all reporting violations such that our sample almost certainly understates the incidence of accounting fraud (see discussions in DeFond and Francis [2005] and Karpoff, Lee, and Martin [2008]). However, it is difficult to conceal material financial misreporting for an extended period (Ball [2009], Kedia and Philippon [2009]). In any event,

TABLE 1
The Number of Fraudulent and Nonfraudulent Companies in Each Sample Year

Year	Fraud Obs. (*Fraud* = 1)	No Fraud Obs. (*Fraud* = 0)	Fraud %
1981	12	4,681	0.26%
1982	18	4,329	0.41%
1983	19	5,299	0.36%
1984	18	4,875	0.37%
1985	16	5,204	0.31%
1986	18	5,498	0.33%
1987	25	6,242	0.40%
1988	22	5,515	0.40%
1989	33	5,350	0.61%
1990	36	5,412	0.66%
1991	41	5,527	0.74%
1992	52	5,646	0.91%
1993	46	6,119	0.75%
1994	43	6,682	0.64%
1995	33	7,348	0.45%
1996	44	7,419	0.59%
1997	50	7,189	0.69%
1998	64	7,263	0.87%
1999	73	7,106	1.02%
2000	74	6,720	1.09%
2001	60	6,052	0.98%
Totals	797	125,476	0.63%

fraud can result in multiple AAERs as the SEC confronts different individuals implicated in the deception. For example, the Enron scandal led to the SEC filing 28 separate AAERs. Given that some AAERs stem from nonaccounting frauds that are irrelevant to our research question, we follow Erickson, Hanlon, and Maydew [2006] and Miller [2006] by excluding these from our sample. We determine the beginning and end of each accounting fraud by closely reviewing all of the AAERs issued against a particular company.[17]

Table 1 reports the number of fraud events per year from 1981 to 2001, which range from a low of 12 in 1981 to a high of 74 in 2000. Data inspection reveals that there is a sudden fall after 2001 in the number of frauds within our sample, reflecting the delay between the end of the fraud and

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this measurement bias that sacrifices power by injecting noise would work against our tests rejecting the null hypothesis that tax aggressiveness is not linked to accounting fraud likelihood. Analyzing actual SEC fraud allegations avoids classification bias from the researcher having to decide which accounting problems constitute fraud (Bonner, Palmrose, and Young [1998], Erickson, Hanlon, and Maydew [2004, 2006]). Importantly, AAERs better map into our research questions than restatements that routinely occur due to inadvertent reporting errors rather than an intent to deceive (Hennes, Leone, and Miller [2008]); e.g., in their sample of 492 restatements, Palmrose and Scholz [2004] find that only 11% led to the SEC filing an AAER against the firm.

[17] In a sensitivity test, we find that our core results are virtually identical when we focus on the first year of the fraud—the tipping point at which the manager initially resorts to perpetrating fraud—rather than all fraud years.

the first AAER issued against the company. Specifically, some of the more recent frauds are still under investigation by the SEC, so had not culminated in the filing of AAERs when we began our research. The attrition problem is material from 2002 onward, although there is also some evidence of attrition in 2001. We rely on a 2001 sample year cutoff in our main tests to minimize attrition bias in the observed incidence of fraud. This design choice also benefits from ensuring that we avoid shifts stemming from the watershed legislative and regulatory reforms to the U.S. capital markets precipitated by the high-profile financial reporting failures.[18] Accordingly, our sample period spans from 1981, the earliest year with accounting fraud data available, to 2001. Importantly, the accounting fraud frequency rises almost monotonically between 1995 and 2001, reinforcing prior evidence that firms had increasingly begun to fraudulently exaggerate their earnings in the years leading up to the major reforms. We return to this issue later in the paper by analyzing whether any links between financial reporting aggressiveness evident in accounting fraud and tax aggressiveness remain when we isolate the 1995–2001 period.

Table 2 summarizes the process we applied to assemble the sample. The fraud sample consists of 381 individual frauds and 797 fraud-years.[19] We code "fraud-years" by examining whether there is a fraud at any time during the calendar year. For example, we code both 1998 and 1999 as fraud-years if a company engaged in fraud from August 1998 to August 1999. Consequently, the mean number of fraud-years is 2.09 (381/797). We conserve power in our analysis by comparing the fraud sample to a nonfraud sample comprised of the rest of the COMPUSTAT population from 1981 to 2001, which includes 14,792 unique nonfraud firms and 125,476 nonfraud firm-years.

In unreported data inspection, we examine the composition of the fraud and nonfraud samples by industry according to the 12 categories in Fama and French [1997].[20] Some clustering is apparent as the fraud frequencies are highest in the "Business equipment," "Wholesale and retail," and

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[18] Applying a 2001 cutoff in our main tests avoids structural shifts in the broader monitoring environment that the ratification of the Sarbanes-Oxley Act on July 30, 2002 might engender. For example, SOX requires that a company's chief financial officer sign its federal income tax return. Similarly, restricting our sample to exclude frauds occurring after 2000 ensures that fallout from Enron's restatement of its earnings on October 15, 2001 is not spuriously behind our evidence. In short, our core inferences persist for these slightly different time frames.

[19] In reporting these descriptive statistics, we focus on the sample that results when the analysis includes the tax aggressiveness proxy that suffers from the least data attrition. The maximum number of fraud observations available for all of our tests is 1,109.

[20] The delegated monitoring of some financial firms by regulatory authorities moderates their asset substitution (Jensen and Meckling [1976]) and underinvestment (Myers [1977]) problems, which may affect the relation between accounting fraud and tax aggressiveness in these firms. Moreover, leverage—one of our firm-level controls—may be driven by explicit (or implicit) investor insurance schemes for banks and insurance companies (Rajan and Zingales [1995]). In fact, their debt-like liabilities are not strictly comparable to the debt issued by nonfinancial firms, while regulations such as minimum capital

TABLE 2
Description of Sample Selection Procedure

Panel A: Identifying the initial fraud and nonfraud samples

	Observations
Fraud Sample	1,109
AAERs issued against companies or their executives from January 1, 1981 through October 13, 2006. Each fraud event is counted only once as duplicate AAERs are sometimes issued for the same fraud. The fraud sample excludes: any companies not covered by COMPUSTAT, any AAERs issued to firms other than publicly traded companies (e.g., audit firms), AAERs that do not specify the year(s) of the alleged fraud, any frauds beginning after 2002, and any AAERs unrelated to fraudulent accounting.	
No-Fraud Sample	162,804
The no-fraud sample excludes: any companies not covered by COMPUSTAT and any companies that receive an AAER.	

Panel B: The fraud and nonfraud samples where data are available for the tax variables

	Fraud Sample	No-Fraud Sample	Total
ETR1	646	102,320	102,966
ETR2	386	65,749	66,135
ETR3	484	68,954	69,438
ETR4	461	63,204	63,665
ETR5	481	62,988	63,469
BTD1	797	125,476	126,273
BTD2	776	122,478	123,254
BTD3	394	61,668	62,062

"Consumer nondurables" sectors and lowest in the "Oil, gas, and coal," "Utilities," and "Finance" sectors. Still, similar to recent research (e.g., Erickson, Hanlon, and Maydew [2006] and Johnson, Ryan, and Tian [2009]), the accounting fraud frequency is fairly evenly spread across industries. In our primary empirical strategy, we control for firm-specific effects, implicitly handling industry-level variation in the incidence of fraudulent financial reporting.

3.2 TAX AGGRESSIVENESS VARIABLES

Given that no universally accepted definition for this construct has emerged in extant research and each measure has its advantages and disadvantages (Hanlon and Heitzman [2010]), we triangulate our evidence by relying on eight proxies for firms' tax aggressiveness: five representing effective tax rates (ETR) and another three for book-tax differences (BTD). Although we explain the choice and specification of each proxy below, we define all regression variables in the appendix. A low effective tax rate indicates tax aggressiveness (Chen et al. [2010]), while book-tax differences

requirements may affect their capital structures (Scholes, Wilson, and Wolfson [1990]). However, all of our main results are nearly identical when we simply remove financial firms from the sample.

are positively related to IRS audit adjustments, a proxy for tax risk (Mills [1998]).[21]

Omer, Molloy, and Ziebart [1991] stress the importance of researchers evaluating the sensitivity of their results to alternative ETR proxies.[22] *ETR1* reflects the traditional GAAP effective tax rate with total tax expense divided by pretax book income. This measure captures tax aggressiveness stemming from permanent book-tax differences that reduce the reported ETR. The next two ETR measures—*ETR2* (Porcano [1986]) and *ETR3* (Zimmerman [1983])—have total current tax expense in the numerator.[23] Current tax expense is intended to capture the firms' current period tax burden, similar to total tax based on taxable income in the corporate tax return (Lisowsky [2009]). The denominator in *ETR2* focuses on pretax worldwide financial income as the basis on which to compare the tax burden. In contrast, we specify *ETR3* by scaling current tax expense with operating cash flows. The first three proxies, *ETR1* to *ETR3*, are grounded in accrual accounting for tax positions.

We shift gears with our final two ETR measures, *ETR4* (Chen et al. [2010]) and *ETR5* (Dyreng, Hanlon, and Maydew [2008]), to consider cash-basis tax burdens. These proxies have cash taxes paid in the numerator, with *ETR4* (*ETR5*) using unadjusted (adjusted) pretax income in the denominator. Recent research argues that these measures may be cleaner as they capture the effects of both permanent and temporary differences, and are not overstated relative to current tax expense due to the accrual accounting for stock options during our pre-SFAS 123(R) sample period (Dyreng, Hanlon, and Maydew [2008], Chen et al. [2010]).

Our three measures for book-tax differences—*BTD1*, *BTD2*, and *BTD3*—capture total, permanent, and discretionary permanent book-tax differences, respectively (see Frank, Lynch, and Rego [2009]). In particular, *BTD1* is constructed to compare total pretax financial income with taxable income. However, taxable income must be estimated from the financial statements because IRS tax returns are not publicly available (and even if they were, they would only be informative of the U.S. tax burden).

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[21] Moreover, book-tax differences are larger for firms accused of participating in tax shelters according to Wilson [2009]. Regrettably, severe sample attrition prevents us from analyzing tax shelter data. Lisowsky [2010] provides more information on using financial statements to calculate the likelihood a firm engages in a tax shelter. However, the data inputs used in Lisowsky [2010] are only available for a small part of our sample period, again resulting in severe sample attrition.

[22] We closely follow prior research in specifying the ETR proxies. However, all of our core results remain when we scale each ETR proxy by the statutory tax rate for the year. Similarly, our evidence is virtually identical when we include the statutory tax rate as a control variable in the regressions. We thank an anonymous referee for suggesting this analysis.

[23] Our core evidence holds when we respecify GAAP ETR to follow the Joint Committee on Taxation [1984] and Shevlin [1987] in successive regressions; i.e., both consistently load positively in table 5 (for the 1981–2001 period) and table 7 (for the 1995–2001 period). Similarly, the factor analysis results that we report in table 6 (1981–2001) and table 8 (1995–2001) are materially insensitive to including these variables.

This data limitation forces researchers to estimate worldwide taxable income by grossing up current federal and foreign tax expense by the appropriate annual U.S. statutory tax rate. *BTD2* takes *BTD1* a step further by isolating the permanent portion of total book-tax differences as a measure for tax aggressiveness. This approach focuses on tax positions that reduce the effective tax rate, boosting financial earnings and earnings per share. Our final tax aggressiveness proxy, *BTD3*, isolates the discretionary portion of permanent book-tax differences. This measure, which exactly follows "DTAX" in Frank, Lynch, and Rego [2009], is intended to reflect tax shelter activities.[24]

Conceptually, our tax aggressiveness proxies are designed to capture the effects of nonconforming transactions; i.e., transactions that have a differential impact for financial versus tax reporting purposes. Accordingly, they map into our research question. Hanlon and Heitzman [2010] stress that the tax avoidance continuum places these measures from least to most aggressive due to the *financial reporting* implications of nonconforming tax avoidance strategies. GAAP ETRs and permanent BTDs are conceptually linked along the continuum because a strategy that generates a permanent BTD reduces the firm's ETR and increases its financial net income.[25] Notably, conforming measures would not capture the financial reporting implications of tax aggressiveness—and, in turn, would not suit our setting—because the income measures would move in lockstep; i.e., taxable income would *rise* with concurrently overstated earnings.

To the extent that a firm is being aggressive for financial reporting purposes (overstating earnings), then the Frank, Lynch, and Rego [2009] narrative implies that the same firm should also be minimizing its tax burden (understating ETRs). Consequently, the overstatement of earnings via fraud coupled with the understatement of taxable income stemming from tax aggressiveness would then translate into overstated BTDs. However, the Erickson, Hanlon, and Maydew [2004] evidence suggests that accounting fraud would be more difficult to perpetrate as the nonconformity of tax aggressive positions increases.

Given the conceptual foundation in Hanlon and Heitzman [2010], the various ETR and BTD measures should be linked empirically (albeit some distance from perfectly). GAAP ETRs capture tax avoidance strategies that reduce total tax expense (the numerator) through a reduction in its components (current and/or deferred tax expense). In other words, a tax strategy that simply defers taxes will not reduce the GAAP ETR because the

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[24] We cannot directly analyze tax shelters since prior research implies that the intersection between firms participating in tax shelters and firms committing accounting fraud is nearly an empty set (Lisowsky [2010]), which provides some coarse evidence that tax aggressiveness is incompatible with accounting fraud.

[25] In addition, book-tax difference measures are related to ETR measures because BTDs subtract taxable income from financial income and ETRs represent the share of income being consumed by taxes. In short, BTDs capture the income effects of tax avoidance strategies while ETRs capture their tax effects.

deferral will be included in the deferred tax expense component of total tax expense. However, permanent BTDs, which represent the income effect rather than tax effect of differences between book and taxable income, do not include avoidance strategies related to deferred positions. It follows that permanent BTDs can reduce GAAP ETRs if they reduce the current tax expense component of total tax expense. We also note that GAAP ETRs include the effect of financial accrual items related to tax expense, e.g., the valuation allowance, change in permanently reinvested earnings, and changes in the tax reserve. In fact, these three items are not permanent BTDs because they arise from financial accounting estimates rather than statutory differences between book and taxable income.

In contrast to permanent BTDs, total BTDs include the income effects of *both* temporary (deferral) and permanent differences. On one hand, the tax effects of such deferral strategies would affect the cash ETR, but not the current tax expense. On the other hand, the tax-related financial accruals (valuation allowance, reserve, permanently reinvested earnings) would affect current tax expense, but not cash ETR. However, permanent tax avoidance strategies affect both cash and current ETRs.

Finally, our *BTD3* variable (i.e., DTAX from Frank, Lynch, and Rego [2009]) is presumed to reflect more aggressive tax positions than permanent BTDs alone because the former highlights the discretionary portion of (nonconforming, nondeferral) tax planning. For example, permanent BTDs on their own capture the legal and benign exclusion of municipal bond interest income from U.S. taxable income, and given the related reduction in GAAP ETRs, might inappropriately suggest tax aggressiveness. *BTD3*, however, aims to ignore benign tax avoidance strategies that generate permanent BTD (and a reduction in GAAP ETRs) by focusing on the discretionary portion only.

Overall, by employing both ETRs and BTDs, we strive to balance a comprehensive empirical analysis with a clear conceptual linkage between accounting fraud and our tax aggressiveness proxies. In short, this puts us in a stronger position to help resolve whether firms that are aggressive in their financial reporting are more or less tax aggressive.

In panel A of table 3, we separately report the means of the tax aggressiveness variables for the fraud and nonfraud firms in the sample. In an initial pass at our research question, this analysis generally indicates that fraud firms exhibit lower tax aggressiveness than nonfraud firms, although there are some exceptions. More specifically, three of the five ETR measures are significantly larger for the fraud firms at the 5% level or better, while all three BTD measures are significantly smaller at the 10% level or better.[26] In the next section, we examine whether these preliminary

[26] In referring to book-tax differences, we interpret a positive (negative) BTD value as indicative of financial (taxable) income exceeding taxable (financial) income. Given that accounting fraud involves increasing financial earnings and tax aggressiveness involves

TABLE 3
Descriptive Statistics for the Effective Tax Rate (ETR) and Book-Tax Difference (BTD) Variables

Panel A: Variable means for the fraud and no-fraud samples

	Fraud Sample (*Fraud* = 1)		No-Fraud Sample (*Fraud* = 0)		Differences in Means
	Obs.	Mean	Obs.	Mean	*t*-Statistics
ETR1	646	0.342	102, 320	0.327	2.043**
ETR2	386	0.257	65, 749	0.227	2.938***
ETR3	484	0.298	68, 954	0.260	2.871***
ETR4	461	0.295	63, 204	0.277	1.558
ETR5	481	0.255	62, 988	0.268	–1.133
BTD1	797	0.258	125, 476	6.367	–2.281**
BTD2	776	–2.602	122, 478	2.419	–2.865***
BTD3	394	–0.027	61, 668	–0.007	–1.647*

Panel B: Correlation matrix

	1.	2.	3.	4.	5.	6.	7.
1. *ETR1*	1.000						
2. *ETR2*	0.520	1.000					
3. *ETR3*	0.383	0.342	1.000				
4. *ETR4*	0.453	0.509	0.248	1.000			
5. *ETR5*	0.411	0.470	0.275	0.907	1.000		
6. *BTD1*	−0.079	−0.151	−0.025	−0.201	−0.157	1.000	
7. *BTD2*	−0.206	−0.080	−0.034	−0.084	−0.069	0.578	1.000
8. *BTD3*	−0.249	−0.207	−0.108	−0.126	−0.113	0.034	0.078

***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.
All pair-wise correlations shown in the table are statistically significant at the 1% level (two-tailed). See the appendix for variable definitions.

inferences persist when we control for other accounting fraud determinants in a multivariate framework.

In panel B, we tabulate the Pearson correlation coefficients between the eight tax aggressiveness measures, which are all significantly different from zero at the 1% level. Reassuringly, both sets of proxies are positively correlated; i.e., like the ETR proxies, the BTD proxies are positively correlated with each other. Corroborating that these variables capture the underlying tax aggressiveness construct, the ETR and BTD proxies are all negatively correlated. These relations reflect that as temporary and permanent income differences increase due to a reduction in taxable income relative to financial income (i.e., BTDs increase), the tax burden (ETRs) decreases as well. Specifically, as temporary and permanent differences increase, cash taxes paid decrease, and, as permanent differences increase, the current ETR decreases. However, we concede that some of the correlations are small in magnitude, suggesting that the proxies may reflect different tax aggressiveness dimensions. Another reason is that the variables

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decreasing taxable income, it is interesting that we find that the excess of financial over taxable income is, in fact, smaller in fraud than nonfraud firms.

suffer from measurement error and reflect the various trimming rules and deflators that we apply to follow prior research. However, the correlations between the tax aggressiveness proxies are similar to recent research (e.g., Frischmann, Shevlin, and Wilson [2008], Chen et al. [2010], and Rego and Wilson [2012]).[27] Our sensitivity analysis includes a specification with tax aggressiveness identified with a common factor extracted from the various alternative proxies.

3.3 RESEARCH DESIGN

We examine the nondirectional prediction in H1 that the extent of tax aggressiveness is related to the likelihood that executives orchestrate accounting fraud. This involves estimating several versions of this probit model (subscripts omitted for notational convenience):

$$Fraud_{it} = \alpha_0 + \alpha_1 TaxAggressiveness_{it} + \sum Controls_{it} + u_i + \varepsilon_{it}. \quad (1)$$

The dependent variable, *Fraud*, equals one if the firm engages in fraudulent financial reporting, and zero otherwise. In successive regressions, we specify our treatment variable, *TaxAggressiveness*, with the proxies for firms' ETRs and their BTDs outlined earlier. We predict under H1 that tax aggressiveness is associated with the probability that firms will be accused of fraud by the SEC. If fraud firms are more tax aggressive, then this would be evident in a negative (positive) coefficient on *TaxAggressiveness* when we measure this construct with effective tax rates (book-tax differences). In contrast, we would expect opposite signs on these variables if tax aggressive firms are less likely to perpetrate accounting fraud.

Importantly, although the *TaxAggressiveness* variables are included as independent variables in the fraud model, we do not assume that they are exogenous. Relatedly, we are not attempting to identify a causal effect of *TaxAggressiveness* on *Fraud*. Instead, our objective is to determine how firms strategically combine these two actions given that each choice is expected to influence the payoff from choosing the other. One perspective is that engaging in fraud reduces the benefit from being tax aggressive (e.g., Erickson, Hanlon, and Maydew [2004]) because it is difficult to report high book incomes at the same time as reporting low taxable incomes since this may attract attention from the IRS. A firm's decision to report fraudulently and be tax aggressive are "strategic substitutes" under this view, implying a negative association between fraudulent financial reporting and tax aggressiveness.[28] An alternative perspective holds that both tax aggressiveness and fraud require reporting opacity, so these two decisions will tend to occur concurrently (e.g., Desai [2005]). Accordingly, the two decisions are

[27] Further, since *BTD3* is measured using the residual from a Jones-like model, it is understandably far from perfectly correlated with our other tax avoidance proxies.

[28] The "strategic substitutes" and "strategic complements" concepts are attributable to Bulow, Geanakoplos, and Klemperer [1985].

"strategic complements," implying that we should observe a positive association between fraudulent financial reporting and tax aggressiveness. In estimating the fraud prediction model, our objective is to determine how firms strategically combine these two decisions, rather than attempt to identify a causal effect of tax aggressiveness on fraud (or vice versa).

Some prior AAER research includes firm-specific controls for corporate governance structures (e.g., Erickson, Hanlon, and Maydew [2006] and Efendi, Srivastava, and Swanson [2007]). However, specifying controls for governance characteristics would lead to heavy attrition that would literally decimate our sample, so we do not directly control for corporate governance variables in our main regressions. Instead, we exploit the panel-data nature of our sample by controlling for unobserved firm-specific characteristics (the u_i in equation (1)).[29] In particular, we estimate equation (1) using a random effects probit model. The major advantage of the random effects probit model over a simple probit model is that the random effects model controls for unobservable time-invariant firm characteristics (u_i). The u_i are assumed to be normally distributed with mean zero and constant variance.[30] Because corporate governance characteristics are largely firm-specific, we expect that our panel data models help to control for this unobserved heterogeneity. Moreover, we account for any time-series dependence between yearly observations pertaining to the same company by clustering on each company (Rogers [1993], Petersen [2009]).

Our sample consists of 797 fraud-years, which is relatively large compared with the samples used in extant AAER research; e.g., Erickson, Hanlon, and Maydew [2006] and Johnson, Ryan, and Tian [2009]. For perspective, controlling for the corporate governance index of Gompers, Ishii, and Metrick [2003] would shrink our sample from 797 to 67 fraud observations. Despite these sample attrition complications, we analyze later in the paper

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[29] In unreported estimations, we follow prior research that does not capitalize on the panel-data nature of the sample by running alternative specifications that control for industry-specific effects, rather than firm-specific effects, since governance structures tend to vary across industries (Fich and Shivdasani [2007]). Moreover, extant research suggests that effective tax rates differ across industries (Chen et al. [2010]). Another reason for controlling for industry-wide variations in fraud frequencies is that some industries may be more susceptible to accounting misconduct stemming from differences in optimal customer, supplier, and employment contracts (Klein and Leffler [1981]). We find that these regression results with dummy variables representing the 12 Fama-French [1997] industries are very similar to our main results, implying that the omitted role of corporate governance or other firm-specific determinants are not spuriously behind our evidence on the impact of tax aggressiveness on the incidence of accounting fraud.

[30] Unfortunately, a fixed-effects model is not a practical alternative in our setting because it would drop all firms that lack any variation over time in the dependent variable (e.g., Baltagi [1995] and Greene [2001]). In other words, the fixed effects model would only use the companies that during the sample period switch from $Fraud_{it} = 0$ to $Fraud_{it} = 1$, or vice versa. For example, any company that has $Fraud_{it} = 0$ in every sample year is dropped from the fixed effects logit estimation. The reason for this is that, unlike the linear fixed effects model, the fixed effects logit model does not use a within transformation.

whether any evidence on the link between tax aggressiveness and the incidence of fraudulent financial reporting is sensitive to firm-level corporate governance quality.

Our selection and specification of controls for firm-level determinants closely resembles recent research on corporate misreporting; e.g., Burns and Kedia [2006], Efendi, Srivastava, and Swanson [2007], Erickson, Hanlon, and Maydew [2006], Johnson, Ryan, and Tian [2009], and Lennox and Pittman [2010]. Specifically, we control for auditor type and tenure, company size and age, negative book equity, mergers and acquisitions, the issuance of debt and equity, and the return on assets. *Big Five* has the value of one when the company appoints a Big Five public accounting firm (or one of its predecessors), and zero otherwise (data #149). *Company Size* equals the natural logarithm of total assets (data #6). We code *Company Age* as the natural logarithm of the number of years that the firm has been listed on COMPUSTAT. *Audit Firm Tenure* represents the number of years that the firm has retained the same auditor. We assign *Negative Equity* the value of one if total liabilities (data #181) exceed total assets (data #6), and zero otherwise (Graham [1996]).[31] The *M&A Indicator* equals one if the company has an acquisition that contributes to sales (data #249 > 0), and zero otherwise. The *Debt & Equity Issued* indicator variable takes the value of one if the sum of new long-term debt (data #111) plus new equity (data #108) exceeds 20% of total assets (data #6), and zero otherwise. We include *ROA* (return on assets), which stands for net income (data #172) divided by total assets (data #6), since more profitable firms naturally have higher effective tax rates. Except for *Company Age* and *Audit Firm Tenure*, which are not affected by outliers, we winsorize all continuous controls at the 1st and 99th percentiles.

Panel A of table 4 presents descriptive statistics for the control variables after splitting the sample into fraud and nonfraud firms. In univariate evidence broadly consistent with prior research, fraud firms tend to be larger, younger, and more likely to appoint lower-quality auditors (non–Big Five auditors with shorter tenure) and to have recently been active in the capital markets evident in arranging M&A transactions as well as in issuing debt or equity securities. Panel B tabulates correlations between the control variables, which are generally fairly small such that multicollinearity concerns are minimal.

We control for time using two alternative specifications. First, for parsimony, we include a time trend variable because, as shown earlier, the frequency of fraudulent financial reporting has steadily risen over time. Second, in unreported estimations, we find almost identical results when we replace the time trend with year fixed effects, which benefits from not imposing a uniform trend in accounting fraud over time.

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[31] Jiambalvo [1996] stresses the importance of controlling for financial distress in a regression that includes our ex ante external financing demand variable. Moreover, companies suffering financial distress are more liable to commit fraud (Maksimovic and Titman [1991]).

TABLE 4
Descriptive Statistics for the Control Variables

Panel A: Variable means for the fraud and no-fraud samples

	Fraud Sample (*Fraud* = 1)		No-Fraud Sample (*Fraud* = 0)		Differences in Means
	Obs.	Mean	Obs.	Mean	*t*-Statistics
Big Five	797	0.718	125,476	0.798	−5.587***
Company Size (Total Assets, $ Million)	797	904.974	125,476	572.900	4.598***
Company Size (Logs)	797	4.100	125,476	3.624	5.377***
Company Age (Years)	797	10.592	125,476	12.337	−4.294***
Company Age (Logs)	797	1.917	125,476	2.034	−3.117***
Audit Firm Tenure (Years)	797	6.138	125,476	6.794	−3.138***
Audit Firm Tenure (Logs)	797	1.416	125,476	1.522	−3.178***
Negative Equity	797	0.075	125,476	0.085	−0.976
M&A Indicator	797	0.166	125,476	0.083	8.348***
Debt & Equity Issued	797	0.442	125,476	0.334	6.413***
ROA	797	−0.152	125,476	−0.138	−0.733

Panel B: Correlation matrix

	1.	2.	3.	4.	5.	6.	7.	8.
1. *Big Five*	1.000							
2. *Company Size (Logs)*	**0.427**	1.000						
3. *Company Age (Logs)*	**0.099**	**0.366**	1.000					
4. *Audit Firm Tenure (Logs)*	**0.146**	**0.305**	**0.708**	1.000				
5. *Negative Equity*	**−0.120**	**−0.231**	**−0.078**	**−0.110**	1.000			
6. *M&A Indicator*	**0.057**	**0.075**	**−0.018**	0.002	**−0.051**	1.000		
7. *Debt & Equity Issued*	**−0.017**	**−0.132**	**−0.278**	**−0.209**	**0.077**	**0.098**	1.000	
8. *ROA*	**0.174**	**0.399**	**0.177**	**0.172**	**−0.421**	**0.040**	**−0.189**	1.000

***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.
All pair-wise correlations that are statistically significant at the 1% level (two-tailed) are shown in bold. See the appendix for variable definitions

4. Multivariate Results

4.1 PRIMARY ANALYSIS

In table 5, we report our main evidence on whether the likelihood of accounting fraud varies systematically with tax aggressiveness. The first five columns tabulate the results using effective tax rates; the remaining three columns measure tax aggressiveness using book-tax differences.

ETR1, which is the GAAP effective tax rate, loads positively and significantly at only the 10% level in a two-tailed test in column (1). The economic magnitude of this relation implies that increasing the effective tax rate from 25% (the 25th percentile in our data) to 42% (the 75th percentile) translates into the probability of fraudulent financial reporting increasing by 17% (from 0.58% to 0.68%), with the rest of the explanatory variables held

TABLE 5

The Association Between Accounting Fraud and the Tax Variables, H1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ETR1	0.34							
	(1.96)*							
ETR2		0.64						
		(3.22)***						
ETR3			0.26					
			(2.27)**					
ETR4				0.43				
				(3.05)***				
ETR5					0.21			
					(1.40)			
BTD1						−0.001		
						(−2.52)**		
BTD2							−0.002	
							(−3.89)***	
BTD3								−0.02
								(−0.15)
Big Five	−0.56	−0.52	−0.29	−0.33	−0.21	−0.59	−0.56	−0.45
	(−5.36)***	(−3.45)***	(−1.97)**	(−2.10)**	(−1.31)	(−6.99)***	(−6.56)***	(−3.48)***
Company Size	0.13	0.16	0.16	0.11	0.10	0.21	0.20	0.20
	(5.38)***	(4.80)***	(5.63)***	(3.81)***	(3.35)***	(10.02)***	(9.57)***	(6.40)***
Company Age	−0.10	−0.10	−0.06	−0.12	−0.11	−0.10	−0.08	−0.11
	(−1.90)*	(−1.42)	(−0.90)	(−1.78)*	(−1.52)	(−2.07)**	(−1.75)*	(−1.54)
Audit Firm Tenure	0.18	0.16	0.06	0.14	0.15	0.09	0.08	0.02
	(3.76)***	(2.40)**	(1.15)	(2.35)**	(2.62)***	(2.13)**	(1.96)*	(0.29)
Negative Equity	−0.17	−0.25	−0.15	0.09	−0.09	−0.28	−0.26	−0.21
	(−0.75)	(−0.79)	(−0.78)	(0.33)	(−0.35)	(−2.55)**	(−2.40)**	(−1.20)

(*Continued*)

TABLE 5—*Continued*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
M&A Indicator	0.26	0.12	0.33	0.28	0.37	0.29	0.28	0.11
	(3.24)***	(1.02)	(3.50)***	(2.87)***	(4.06)***	(3.93)***	(3.86)***	(1.01)
Debt & Equity Issued	0.24	0.27	0.04	0.19	0.18	0.17	0.16	0.17
	(3.74)***	(3.10)***	(0.44)	(2.36)**	(2.27)**	(3.10)***	(2.97)***	(2.05)**
ROA	−0.92	−0.76	−0.46	−1.18	−1.24	−0.18	−0.17	−0.29
	(−2.28)**	(−1.76)*	(−2.98)***	(−2.47)**	(−2.83)***	(−2.93)***	(−2.75)***	(−2.95)***
Time Trend	0.05	0.06	0.05	0.09	0.09	0.04	0.04	0.04
	(8.32)***	(6.94)***	(5.67)***	(8.00)***	(8.09)***	(6.86)	(6.86)***	(5.66)***
Company-Specific Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fraud Obs.	646	386	484	461	481	797	776	394
No-Fraud Obs.	102,320	65,749	68,928	63,204	62,988	125,476	122,478	61,668

The dependent variable equals one if the company engaged in accounting fraud during the year, and zero 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.

***Significant at the 1% level (two-tailed). **Significant at the 5% level (two-tailed). *Significant at the 10% level (two-tailed). See the appendix for variable definitions.

constant at their mean values.[32] After respecifying effective tax rates according to Porcano [1986] (*ETR2*) and Zimmerman [1983] (*ETR3*), we estimate positive coefficients that are significant at the 1% and 5% levels, respectively. Next, we consider whether this relation holds when we replace GAAP ETR measures with cash-based measures. In column (4), the cash ETR (*ETR4*) coefficient is positive and statistically significant at the 1% level when we follow Chen et al. [2010] in measuring this variable, although cash ETR (*ETR5*) fails to load under the Dyreng, Hanlon, and Maydew [2008] specification in column (5).[33] Altogether, the results from the effective tax rates suggest that accounting fraud is less prevalent among tax aggressive firms and, certainly, there is no evidence that it is more prevalent.

In the last three columns of table 5, we turn our attention to examining the associations between accounting fraud and firms' book-tax differences. Similar to the results for the effective tax rate proxies, we generally find that accounting fraud is less frequent among tax aggressive firms. More specifically, *BTD1* and *BTD2* both load negatively in columns (6) and (7), although the coefficient on *BTD3* that is equivalent to DTAX of Frank, Lynch, and Rego [2009]—in a considerably smaller sample—is statistically indistinguishable from zero in column (8).

Collectively, our research implies that tax aggressive firms are less likely to be accused of fraudulently exaggerating their earnings by the SEC. However, we stress that this evidence should be interpreted with caution given that two of our eight proxies for tax aggressiveness do not load. Despite that our analysis lends some empirical support to the intuition that firms that are aggressive for tax purposes are less likely to resort to accounting fraud, we are on more solid ground in interpreting our results as failing to support arguments (e.g., Desai and Dharmapala [2006]) and evidence (e.g., Frank, Lynch, and Rego [2009]) that aggressive financial reporting tends to accompany aggressive tax reporting. Consistent with Erickson, Hanlon, and Maydew [2004], we find some large-sample evidence that fraudulent financial reporting is associated with higher reported taxable income evident in higher effective tax rates and lower book-tax differences.

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[32] Although we gauge economic impact using the ETR coefficient from column (1), the point estimates in table 5 vary fairly widely from 0.21 to 0.64. The distributions of our ETR variables reflect fairly large shifts in U.S. statutory tax rates during our sample period, including that these rates were much higher in the earlier years under study. Specifically, the top corporate tax rate was 46% in 1981–1986, 40% in 1987, 34% in 1988–1992, and 35% in 1993–2001 according to the Tax Policy Center (http://www.taxpolicycenter.org/taxfacts/displayafact.cfm?Docid=64&Topic2id=70). We also compare descriptive statistics for our ETR proxies to recent research after aligning the sample periods to ensure that ours match theirs as much as possible; e.g., Rego [2003], Dyreng, Hanlon, and Maydew [2008], and Chen et al. [2010]. In all cases, we find that our ETR distributions are quite similar to prior research.

[33] *ETR5* remains statistically insignificant (z-statistic is 1.30) when we follow the advice of Hanlon and Heitzman [2010] by scaling by cashflow from operations, rather than pretax income minus special items.

Similar to most recent research on materially deficient financial statements (e.g., Burns and Kedia [2006], Erickson, Hanlon, and Maydew [2006], Efendi, Srivastava, and Swanson [2007], and Johnson, Ryan, and Tian [2009]), the coefficients on several control variables do not consistently load; however, the statistically significant results are in the expected directions. In all regressions, the time trend coefficient is positive and highly statistically significant, implying that the incidence of accounting fraud climbs over the 21 years under study. Similarly, *Company Size* always loads positively at the 1% level in table 5, reinforcing prior research that larger firms are more apt to engage in accounting fraud (e.g., Fich and Shivdasani [2007] and Lennox and Pittman [2010]). Except for the specification in column (5), *Big Five* enters negatively at the 5% level or better (z-statistics range from –1.31 to –6.99), consistent with Lennox and Pittman's [2010] evidence that brand-name auditors better discipline firms against fraudulently manipulating the financial statements. Corroborating Erickson, Hanlon, and Maydew [2006] and Efendi, Srivastava, and Swanson [2007], the mainly significantly positive coefficients on the *M&A Indicator* and *Debt & Equity Issued* variables suggest that fraud firms are more likely to participate in mergers and acquisitions and to issue securities, respectively.

4.2 FACTOR ANALYSIS

Given that we use a relatively large set of tax aggressiveness proxies, we follow Chen et al. [2010] by implementing factor analysis to identify a more parsimonious measure of the tax construct. This is another way that we bring clarity to extant research that remains mixed on whether firms that are more aggressive in their financial reporting exhibit more or less tax aggressiveness. In panel A of table 6, we report the factor loadings and communality percentage scores for several combinations of the eight tax variables. In selecting the variables to include in this analysis, we prefer to retain variables with higher communality while minimizing data attrition. Consistent with the correlations in table 3, the factor loadings in column (1) are all positive for the ETR proxies and all negative for the BTD proxies. However, keeping all eight tax aggressiveness proxies in column (1) brings heavy data attrition; i.e., we are left with only 96 fraud observations. In column (2), we exclude *BTD3*, which is responsible for extensive attrition and suffers from a low communality score of only 3% in column (1) (prior research implies that scores exceeding the 40% threshold are reasonable). In columns (3) and (4), we focus separately on the ETR (we omit *ETR3* given its considerably lower communality in comparison to the remaining ETR measures) and BTD proxies, respectively, since these variables have more in common with each other.

In panel B, we tabulate the results from re-estimating our models after integrating factor analysis, which provides evidence reinforcing the results in table 5. In particular, the common tax factors load positively in columns (1) to (3) at the 5% level or lower (z-statistics range from 2.34 to 2.95), which makes sense given that these variables capture positive (negative)

TABLE 6

Results from Using Factor Analyses on the Tax Variables

Panel A: Factor loadings and communality percentage scores for different combinations of the tax variables

	Factor Loadings (Communality %)	Factor Loadings (Communality %)	Factor Loadings (Communality %)	Factor Loadings (Communality %)
ETR1	0.73	0.67	0.58	
	(53%)	(44%)	(34%)	
ETR2	0.74	0.70	0.62	
	(55%)	(49%)	(39%)	
ETR3	0.47	0.44		
	(22%)	(19%)		
ETR4	0.89	0.89	0.92	
	(88%)	(79%)	(86%)	
ETR5	0.87	0.87	0.90	
	(75%)	(75%)	(81%)	
BTD1	−0.28	−0.28		0.68
	(8%)	(8%)		(46%)
BTD2	−0.27	−0.24		0.68
	(7%)	(6%)		(46%)
BTD3	−0.20			
	(3%)			
Fraud Obs.	96	157	261	776
No-Fraud Obs.	19,109	29,150	39,077	122,478

Panel B: Results using the common tax factor variables that are estimated using the factor loadings reported in panel A

	(1)	(2)	(3)	(4)
Common Tax Factor	0.24	0.17	0.15	−0.11
	(2.34)**	(2.35)**	(2.95)***	(−3.83)***
Big Five	−0.32	−0.16	−0.34	−0.56
	(−0.80)	(−0.53)	(−1.64)*	(−6.59)***
Company Size	0.17	0.14	0.10	0.20
	(2.38)**	(2.58)***	(2.43)**	(9.63)***
Company Age	0.13	−0.02	−0.05	−0.08
	(0.77)	(−0.21)	(−0.61)	(−1.75)*
Audit Firm Tenure	−0.09	−0.06	0.07	0.08
	(−0.67)	(−0.67)	(0.86)	(1.93)*
Negative Equity	0.51	−0.07	−0.13	−0.26
	(0.83)	(−0.13)	(−0.33)	(−2.38)**
M&A Indicator	0.20	0.30	0.25	0.28
	(0.85)	(1.85)*	(1.83)*	(3.86)***
Debt & Equity Issued	0.11	0.08	0.23	0.16
	(0.56)	(0.54)	(2.13)**	(2.98)***
ROA	0.05	0.51	−1.60	−0.17
	(0.03)	(0.41)	(−1.70)*	(−2.71)***
Time Trend	0.10	0.09	0.09	0.04
	(3.94)***	(4.65)***	(5.67)***	(6.86)***
Company-Specific Effects?	Yes	Yes	Yes	Yes
Fraud Obs.	96	157	261	776
No-Fraud Obs.	19,109	29,150	39,077	122,478

***Significant at the 1% level (two-tailed). **Significant at the 5% level (two-tailed). *Significant at the 10% level (two-tailed). *Common Tax Factor* = the common factor estimated from the four models reported in panel A. See the appendix for the definitions of other variables.

communality with ETR (BTD). Similarly, the common tax factor loads negatively at the 1% level in column (4), which makes sense given that this variable captures communality between the BTD proxies. Importantly, the sample sizes rise dramatically as we move across the columns, increasing the power of the tests, which predictably leads to more statistically significant results.

4.3 1995–2001 ANALYSIS

In the Introduction, we recount several trends covering the mid-1990s to the early 2000s indicating that tax aggressiveness has begun to coincide with financial reporting aggressiveness, at least in the aggregate. This prior research suggests that accounting income began to seriously diverge from taxable income and corporate misreporting soared during these years. For the 1995–2001 period, when tax aggressiveness and accounting fraud are more likely to be positively related, we analyze whether firms fraudulently manage book income upward and taxable income downward. In another upside, narrowing our focus to these years ensures that we cover a time frame that is closer to the 1996–2001 and 1991–2005 periods under study in Erickson, Hanlon, and Maydew [2004] and Frank, Lynch, and Rego [2009], respectively.[34]

Consistent with our full sample analysis in table 5, we continue to find some evidence that tax aggressive firms are less liable to commit accounting fraud when we isolate the 1995–2001 period in table 7, despite that this leads to nontrivial sample attrition; e.g., in column (1), the fraud (nonfraud) sample drops from 646 (102,320) to 331 (37,856) observations. Although the statistical significance of the five ETR (i.e., the same four load positively) and the three BTD (i.e., the same two load negatively) proxies is very similar to the primary regression results in table 5, the coefficient estimates are generally larger when we concentrate on the years in which financial reporting failures surged in the United States. The economic magnitude of this relation implies that increasing the effective tax rate from 20% (the 25th percentile in our data) to 40% (the 75th percentile) translates into the probability of fraudulent financial reporting increasing from 0.67% to 1.08%, with the rest of the explanatory variables evaluated at their mean values. From an economic standpoint, the *ETR1* coefficient in column (1) implies that the SEC becomes about 61% more likely to allege accounting fraud when GAAP ETR rises over the inter-quartile range.

In another robustness check, we report regression results for the 1995–2001 period in table 8 after replacing the individual tax proxies with a single common tax factor. This analysis is consistent with our results in tables 6 and 7 in that less tax aggressive firms are more likely to engage in accounting fraud.

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[34] All results reported in this section are virtually identical when we widen the sample period to 1991–2001, which is even closer to the years that Frank, Lynch, and Rego [2009] examine.

TABLE 7

The Association Between Accounting Fraud and the Tax Variables in the Post-1994 Period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ETR1	0.90							
	(3.01)***							
ETR2		1.26						
		(3.62)***						
ETR3			0.61					
			(3.61)***					
ETR4				0.42				
				(2.06)**				
ETR5					–0.07			
					(−0.32)			
BTD1						−0.001		
						(−2.30)**		
BTD2							−0.002	
							(−2.82)***	
BTD3								0.23
								(1.01)
Big Five	−0.39	−0.45	−0.29	−0.28	−0.25	−0.75	−0.73	−0.78
	(−1.75)*	(−1.42)	(−1.20)	(−1.14)	(−1.07)	(−4.47)**	(−4.33)***	(−2.80)***
Company Size	0.13	0.21	0.23	0.14	0.16	0.29	0.29	0.33
	(3.30)***	(3.69)***	(5.39)***	(3.36)***	(3.73)***	(7.90)***	(7.74)***	(5.71)***
Company Age	−0.05	−0.07	−0.06	−0.05	−0.06	−0.15	−0.12	−0.27
	(−0.49)	(−0.56)	(−0.61)	(−0.56)	(−0.62)	(−1.81)*	(−1.50)*	(−1.96)**
Audit Firm Tenure	0.13	0.13	0.06	0.11	0.12	0.12	0.13	0.11
	(1.70)*	(1.25)	(0.77)	(1.30)	(1.57)	(1.80)*	(1.81)*	(0.99)
Negative Equity	−0.66	−1.19	−0.91	−0.45	−0.87	−0.91	−0.90	−0.62
	(−1.30)	(−1.59)	(−2.24)**	(−0.88)	(−1.67)*	(−3.99)***	(−3.93)***	(−1.80)*

(*Continued*)

TABLE 7—*Continued*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
M&A Indicator	0.36	0.32	0.47	0.39	0.49	0.45	0.44	0.15
	(2.78)***	(1.76)*	(3.62)***	(3.02)***	(3.98)***	(4.09)***	(3.96)***	(0.83)
Debt & Equity Issued	0.20	0.26	0.10	0.18	0.20	0.18	0.18	0.12
	(1.83)*	(1.78)*	(0.89)	(1.57)	(1.86)*	(1.98)**	(1.89)*	(0.83)
ROA	−1.13	−0.89	−0.45	−1.43	−1.68	−0.41	−0.40	−0.52
	(−1.38)	(−1.11)	(−1.51)	(−1.70)*	(−2.40)**	(−4.21)***	(−4.09)***	(−3.51)***
Time Trend	0.20	0.22	0.16	0.20	0.20	0.11	0.10	0.15
	(7.43)***	(5.72)***	(6.08)***	(7.23)***	(7.33)***	(4.82)***	(4.59)***	(4.12)***
Company-Specific Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fraud Obs.	331	193	323	304	327	398	390	192
No-Fraud Obs.	37,856	23,704	37,699	33,540	34,297	49,097	48,218	22,403

The dependent variable equals one if the company engaged in accounting fraud during the year, and zero 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.

***Significant at the 1% level (two-tailed). **Significant at the 5% level (two-tailed). *Significant at the 10% level (two-tailed). See the appendix for variable definitions.

TABLE 8

Results from Using Factor Analyses on the Tax Variables in the Post-1994 Period

Panel A: Factor loadings and communality percentage scores for different combinations of the tax variables

	Factor Loadings (Communality %)	Factor Loadings (Communality %)	Factor Loadings (Communality %)	Factor Loadings (Communality %)
ETR1	0.75	0.69	0.61	
	(56%)	(47%)	(39%)	
ETR2	0.76	0.72	0.64	
	(58%)	(52%)	(41%)	
ETR3	0.49	0.45		
	(24%)	(80%)		
ETR4	0.87	0.87	0.92	
	(76%)	(77%)	(85%)	
ETR5	0.85	0.85	0.88	
	(72%)	(72%)	(88%)	
BTD1	−0.31	−0.30		0.66
	(9%)	(9%)		(43%)
BTD2	−0.32	−0.28		0.66
	(10%)	(8%)		(43%)
BTD3	−0.16			
	(3%)			
Fraud Obs.	61	111	171	390
No-Fraud Obs.	9,756	15,936	20,771	48,218

Panel B: Results using the common tax factor variables that are estimated using the factor loadings reported in panel A

	(1)	(2)	(3)	(4)
Common Tax Factor	0.33	0.23	0.18	−0.14
	(2.17)**	(2.42)**	(2.43)**	(−3.02)***
Big Five	0.08	0.27	−0.13	−0.73
	(0.13)	(0.48)	(−0.36)	(−4.34)***
Company Size	0.23	0.19	0.17	0.29
	(2.17)**	(2.53)**	(2.77)***	(7.78)***
Company Age	0.38	0.09	−0.01	−0.12
	(1.55)	(0.58)	(−0.11)	(−1.47)
Audit Firm Tenure	−0.22	−0.14	0.07	0.12
	(−1.12)	(−1.12)	(0.70)	(1.79)*
Negative Equity	0.70	−0.12	−0.86	−0.89
	(0.80)	(−0.16)	(−1.13)	(−3.89)***
M&A Indicator	0.44	0.47	0.40	0.44
	(1.39)	(2.29)**	(2.18)**	(3.96)***
Debt & Equity Issued	−0.03	0.12	0.16	0.18
	(−0.09)	(0.63)	(1.00)	(1.92)*
ROA	−0.34	−0.29	−1.99	−0.39
	(−0.13)	(−0.16)	(−1.32)	(−3.99)***
Time Trend	0.25	0.18	0.21	0.10
	(3.43)***	(2.53)***	(5.42)***	(4.59)***
Company-Specific Effects?	Yes	Yes	Yes	Yes
Fraud Obs.	61	111	171	390
No-Fraud Obs.	9,756	15,936	20,771	48,218

***Significant at the 1% level (two-tailed). **Significant at the 5% level (two-tailed). *Significant at the 10% level (two-tailed). *Common Tax Factor* = the common factor estimated from the four models reported in panel A. See the appendix for the definitions of other variables.

4.4 MATCHED PROPENSITY SCORES ANALYSIS

A possible limitation of our analysis is that the fraud and nonfraud samples could be systematically different in ways that are correlated with our tax variables, generating spuriously significant results. This is potentially an important concern because the nonfraud sample is much larger than the fraud sample (e.g., there are 646 fraud observations and 102,320 nonfraud observations in column (1) of table 5). As a robustness check, we isolate the nonfraud observations that are most closely matched to the fraud sample on the fraud predictor variables other than tax. This involves eliminating from the sample any nonfraud observations that are systematically different from the fraud sample to ensure that the fraud and nonfraud companies have very similar characteristics.

We implement the matching analysis in two steps. First, we estimate a probit model of fraud in which the independent variables are: *Big Five*, *Company Age*, *Audit Firm Tenure*, *Negative Equity*, *M&A Indicator*, *Debt & Equity Issued*, *ROA*, and *Time Trend*. The estimates from this model are used to generate the fraud propensity score for every observation in the sample. In the second stage, we match each fraud observation to the 10 nonfraud observations that have the closest matches and we re-estimate the fraud models on these matched samples using the tax variables as independent variables. We follow Lisowsky [2010] by using one-to-ten rather than one-to-one matching because there are relatively few fraud observations and our tests require adequate statistical power. The matched and fraud samples have extremely close propensity scores because the nonfraud samples are very large and therefore offer many potential matches. After removing the nonfraud observations that are not closely matched to the fraud sample, the resulting samples are much smaller. For example, there are 646 fraud observations in column (1) of table 5, so matching causes the nonfraud sample to drop from 102,320 observations to just 6,460 ($=10 \times 646$).

In table 9, we report the results of the matched-sample analysis. Consistent with table 5, we find that the *ETR1* to *ETR3* variables have positive and statistically significant coefficients. Moreover, the magnitudes of these coefficients are very similar to those reported in table 5. Accordingly, the evidence from this matching analysis suggests that our results are not attributable to systematic differences between the fraud and nonfraud samples. The *ETR5* and *BTD3* tax variables are statistically insignificant in both tables 5 and 9, which further supports our inference that the results are similar in both the matched and unmatched tests. Finally, the *ETR4*, *BTD1*, and *BTD2* tax variables are no longer statistically significant in table 9, although the coefficient signs are the same as in table 5. The drop in statistical significance is unsurprising given that the nonfraud samples are much smaller.

4.5 CORPORATE GOVERNANCE ANALYSIS

Next, we examine whether the relation between tax aggressiveness and the incidence of fraudulent financial reporting hinges on firm-level

TABLE 9
The Association Between Accounting Fraud and the Tax Variables Using Matched Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ETR1	0.34							
	(2.35)**							
ETR2		0.45						
		(2.78)***						
ETR3			0.22					
			(2.18)**					
ETR4				0.16				
				(1.30)				
ETR5					−0.07			
					(−0.55)			
BTD1						−0.001		
						(−1.20)		
BTD2							−0.001	
							(−1.15)	
BTD3								−0.13
								(−1.08)
Fraud Obs.	646	386	484	461	481	797	776	394
Matched No-Fraud Obs.	6,460	3,860	4,840	4,610	4,810	7,970	7,760	3,940

To derive the matched samples we first estimate a probit model of fraud in which the independent variables are: *Big Five, Company Age, Audit Firm Tenure, Negative Equity, M&A Indicator, Debt & Equity Issued, ROA,* and *Time Trend*. Using the estimates from this model, we calculate the fraud propensity score for each observation in the sample. We then match each fraud observation to the 10 no-fraud observations that have the closest matches. The fraud models are then re-estimated on these matched samples using the tax variables as independent variables. These matched-sample results are reported in columns (1) to (10). *Z*-statistics are reported in parentheses and standard errors are adjusted for clustering by company.

***Significant at the 1% level (two-tailed). **Significant at the 5% level (two-tailed). *Significant at the 10% level (two-tailed).

governance. Some recent research implies that managers intent on depriving outside investors of their fair share of corporate earnings resort to exploiting sophisticated tax planning—under the pretext that reducing taxes benefits all shareholders as the residual claimants—to conceal their diversionary practices by suppressing information necessary for monitoring purposes (e.g., Desai and Dharmapala [2006], Graham and Tucker [2006], Desai, Dyck, and Zingales [2007]). It follows that firms undertaking aggressive tax positions may be more apt to commit accounting fraud when their governance structures are poor; i.e., in these situations, managers have wider scope to misbehave. This argument is grounded in the intuition that lax governance leaves firms more susceptible to managerial diversion hidden through inherently complex tax strategies and, later, fraudulent financial statements. Still, it is important to stress that this issue is far from settled. Some research supports that investors' reactions to corporate tax aggressiveness impound their concerns that managers are more eager to extract private benefits when firm governance is worse (e.g., Desai and Dharmapala [2006, 2009], Guedhami and Pittman [2008], Wilson [2009]). However, more recent evidence calls into question whether

tax sheltering and the diversion of corporate resources are genuinely complementary activities (e.g., Seidman and Stomberg [2011], Rego and Wilson [2012]).

We follow extensive prior research by using the index, *GIndex*, of Gompers, Ishii, and Metrick [2003] to measure corporate governance when analyzing whether the role that tax aggressiveness plays in shaping accounting fraud is sensitive to governance quality (e.g., Desai and Dharmapala [2006], El Ghoul, Guedhami, and Pittman [2011], Kim, Li, and Zhang [2011], Hoopes, Mescall, and Pittman [2012]). The index amounts to a count of the presence of 24 anti-takeover provisions that capture when defensive tactics that entrench incumbent managers marginalize the market for corporate control. We specify *GIndex* so that higher scores reflect that governance is stronger when the market for corporate control exerts more discipline on managers. Regrettably, focusing on *GIndex*, which is only available for the 1990–2000 period, brings major attrition; for example, the number of fraud observations under study in column (1) of table 5 shrinks from 646 to just 67. We re-estimate the models in table 5 after adding the *GIndex* conditioning variable and its interaction with each of the tax proxies. In these low-power tests, we find no evidence that corporate governance moderates the link between tax aggressiveness and accounting fraud.

Finally, we estimate the eight regressions after replacing *GIndex* with the presence of a Big Five auditor, *Big Five*, which is a hallmark of good corporate governance according to extensive prior research; e.g., Francis [2004] and Piotroski and Srinivasan [2008]. Relevant to our purposes, Lennox and Pittman [2010] document that, although the Big Five were heavily criticized in the aftermath of the surge in major financial reporting failures by their clients in the late 1900s and the early 2000s, they continue to constrain firms for perpetrating accounting fraud; i.e., Big Five audit quality has remained stable over time. In an important upside, gauging governance with *Big Five* avoids sample attrition—accordingly, we control for auditor choice in all of the regressions—although we find no evidence that the impact of firms' tax aggressiveness on the frequency of accounting fraud turns on whether they appoint a Big Five auditor known to closely monitor the financial reporting process.

5. *Conclusions*

Although research on taxation and corporate governance has begun to converge, Desai and Dharmapala [2008] call for more evidence on the role that tax avoidance plays in financial reporting. Desai [2005] argues that timing the overstatement of accounting income to coincide with the understatement of taxable income enables managers to deny outside investors their fair share of corporate profits. Consistent with accounting transparency constraining expropriation, which managers can disguise as tax planning that ostensibly benefits shareholders, Frank, Lynch, and Rego [2009] document that aggressive financial reporting tends to accompany

aggressive tax reporting. In contrast, Erickson, Hanlon, and Maydew [2004] attribute their evidence that fraud firms overpay their taxes to managers trying to avoid arousing suspicion from government agencies, including the SEC and the IRS, and outside investors.

There are competing arguments and mixed evidence on whether firms that are aggressive in their financial reporting exhibit more tax aggressiveness. Our research contributes to empirically resolving this issue by estimating the relation between aggressive tax planning and the frequency of alleged accounting fraud. After relying on a comprehensive set of effective tax rate and book-tax difference proxies—as well as a common factor extracted from these measures—to triangulate our evidence, we generally find that tax aggressive U.S. public firms are less likely to fraudulently manipulate their financial statements. Our results are very similar when we isolate the 1995–2001 period in which accounting impropriety steeply rose and corporate tax compliance steeply fell according to prior research. The coefficient estimates imply that increasing the GAAP effective tax rate from the 25th to the 75th percentile in our data, with the rest of the explanatory variables evaluated at their mean values, translates into the probability of fraudulent financial reporting rising by 17% (61%) during the 1981–2001 (1995–2001) period. Our sample period covers 1981, the earliest year for which data on fraudulent financial reporting is available, to 2001, the latest year for which we can reliably measure the incidence of accounting fraud. Accordingly, a limitation of our analysis is that we do not examine more recent years when major regulatory and legislative reforms to the U.S. capital markets occurred.

Moreover, we caution that our results are sensitive to how we measure tax aggressiveness; i.e., in our main tests, four (two) of our five (three) effective tax rate (book-tax difference) proxies load positively (negatively). Consequently, although we mainly find that tax aggressive firms are less apt to commit accounting fraud, we are in a stronger position to conclude that our analysis fails to support arguments (e.g., Desai and Dharmapala [2006]) and evidence (e.g., Frank, Lynch, and Rego [2009]) that aggressive financial reporting coincides with aggressive tax reporting. In addition, it is important to stress that we do not attempt to establish whether there is a causal relationship running from tax aggressiveness to fraud, or from fraud to tax aggressiveness. Instead, our objective is to analyze how firms strategically *combine* these two actions given that each choice is expected to affect the payoff from choosing the other. Committing fraud reduces the benefit from undertaking aggressive tax positions under one perspective because it is hard to report high book incomes when reporting low taxable incomes since this would be a red flag to the IRS (e.g., Erickson, Hanlon, and Maydew [2004]). In contrast, another perspective holds that both tax aggressiveness and fraud require reporting opacity, implying that these two decisions will tend to occur concurrently (e.g., Desai [2005]). Our evidence is more consistent with the former argument than the latter.

APPENDIX

Variable Names and Definitions

Name	Definition
Panel A: Dependent variable	
Fraud	One if the company is alleged by an Accounting and Enforcement Release to have engaged in an accounting fraud during the year, zero otherwise.
Panel B: Tax variables	
ETR1	Total tax expense (#16) / Pretax income (#170).
ETR2	Current federal tax expense (#63) / (Income before extraordinary items (#18) + Current federal tax expense (#63) + Minority interest (#49) – Extraordinary items (#48) – Equity in earnings (#55)). Based on Porcano [1986].
ETR3	Total tax expense (#16) – Change in deferred tax (#35) / Operating cash flows (#308). Based on Zimmerman [1983].
ETR4	Cash taxes paid (#317) / Pretax income (#170). Based on Chen et al. [2010].
ETR5	Cash taxes paid (#317) / (Pretax income (#170) – Special items (#17)). Based on Dyreng, Hanlon, and Maydew [2008].
BTD1	Pretax income (#170) – ((Current federal tax expense (#63) + foreign tax expense (#64)) / statutory marginal tax rate). Based on Frank, Lynch, and Rego [2009].
BTD2	BTD1 – (Total deferred tax expense (#50) / statutory marginal tax rate). Based on Frank, Lynch, and Rego [2009].
BTD3	Residuals from estimating equation (1) by two-digit SIC code and fiscal year, where all variables including the intercept are scaled by beginning of year total assets (#6): $BTD2 = \alpha_0 + \alpha_1\ INTANG + \alpha_2\ UNCON + \alpha_3\ MI + \alpha_4\ CSTE + \alpha_5\ \Delta NOL + \alpha_6\ BTD2\_lag + e$ (1) where *INTANG* = goodwill and other intangibles (#33),; *UNCON* = income reported under the equity method (#55),; *MI* = minority interest (#49),; *CSTE* = current state income tax expense (#173),; ΔNOL = change in net operating loss carryforward (#52),; and *BTD2_lag* = the one-year lagged value of *BTD2*. Based on Frank, Lynch, and Rego [2009].
Panel C: Control variables	
Big Five	One if the company is audited by one of the Big Five firms or their predecessors, zero otherwise.
Company Size	Log of total assets (#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 Equity	One if total liabilities (#181) > total assets (#6), zero otherwise.
M&A Indicator	One if the company had an acquisition that contributed to sales (#249 > 0), zero otherwise.
Debt & Equity Issued	One if the sum of new long-term debt (#111) plus new equity (#108) exceeds 20% of total assets (data #6), zero otherwise.
ROA	Net income (#172) divided by total assets (#6).
Time Trend	One in 1981, two in 1982, . . . , twenty in 2000, twenty-one in 2001, etc.

Following Chen et al. [2010], each ETR variable is set as missing when the denominator is zero or negative and we winsorize each ETR to the range [0, 1]. Except for the company's age and audit firm tenure, the remaining continuous variables are winsorized at the top 1% and bottom 99% to address problems of outliers.

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