

What Does Tax Aggressiveness Signal?

Evidence from Stock Price Reactions to News About Tax Aggressiveness

Michelle Hanlon

Ross School of Business at the University of Michigan

Joel Slemrod

Ross School of Business at the University of Michigan

January 23, 2007

Abstract:

We study the stock price reaction to news about tax aggressiveness. We find that, on average, a company's stock price declines when there is news about its involvement in tax shelters, but the reaction is small relative to reactions to other corporate misdeeds. We find some limited evidence for cross-sectional variation in the reaction. For example, the stock price decline is smaller for firms that have good governance which is consistent with the idea that for these firms the news is less likely to trigger concerns about insiders' aggressiveness toward the investors themselves. The reaction is more negative for firms in the retail sector, suggesting that part of the reaction may be a consumer/taxpayer backlash. We also test whether the reaction varies with the market's perception of how tax-aggressive the firm is, using the firm's current effective tax rate as a proxy for the market's beliefs, and find mixed results.

We also explore the stock price reaction to reports of current effective tax rate calculations released by Citizens for Tax Justice. We hypothesize that these reports signal tax aggressiveness without the implications for tax penalties or illegal behavior that tax shelter news carries, and therefore any market reaction represents a pure reputation effect. We find no statistically significant stock price reaction to the reports, suggesting that the negative reaction to tax shelter news is not predominantly a reputation effect.

All in all, our analysis suggests that tax shelter news is viewed as a negative event by the market, although the stock price reaction is much smaller than the reaction to major accounting mishaps.

We are grateful to Estelle Dauchy and Kyle Peterson for able and tireless research assistance. We appreciate comments on earlier drafts from Joseph Bankman (discussant), Dhammika Dharmapala (discussant), Ed Outslay (discussant), Douglas Shackelford, Terry Shevlin, and workshop participants at the National Bureau of Economic Research Financial Accounting and Tax Policy Conference, and at the Sidney Winter Lecture Series at the University of Iowa. We thank the American Tax Policy Institute and the National Bureau of Economic Research for funding.

What Does Tax Aggressiveness Signal?

Evidence from Stock Price Reactions to News About Tax Aggressiveness

Lying to the IRS doesn't generate the same public outrage as lying to shareholders. In some quarters of the country, it is almost seen as a patriotic act.

---Alan Murray, *Wall Street Journal*, July 2, 2002

1. Introduction and Motivation

In order to maximize the value of the firm, shareholders would like to minimize corporate tax payments net of the private costs of doing so.¹ In other words, shareholders want a company they own part of to be optimally aggressive in their tax reporting. Marketing efforts and reports from tax shelter promoters are consistent with firms competing on tax minimization. For example, in a *Forbes* article on tax sheltering, a shelter promoter revealed that “A potential client once said that he would hire the firm if we could get their tax rate down, because it was higher than their competitors’ and they were embarrassed” (Novack and Saunders, 1998). Learning that a firm is in aggressive tax positions may make owning its shares more attractive. Thus, in this regard, information suggesting that a firm is tax aggressive or a low tax payer should be positive news to the market.

However, to the extent that tax aggressiveness is deemed by the Internal Revenue Service (IRS) and the tax courts to be noncompliance, it may be disallowed and be subject to penalties. Because the objective is to minimize tax payments net of cost, including expected penalties, news about penalties per se is almost certainly viewed negatively. In addition, if a firm is identified as a low taxpayer, or tax avoider, the firm may bear political costs of being labeled a “poor corporate citizen.”² Public statements attesting to firms’ tax payments support this hypothesis. For example,

¹ A value-maximizing firm should also strive to minimize (net-of-cost) personal tax liability related to stock ownership.

² One recent example is Stanley Works and their proposed move (reincorporation) to the tax haven country Bermuda (i.e., a corporate inversion). Stanley Works’ initial announcement increased firm value by \$199 million (Desai and Hines, 2002), but the company did not undertake the inversion transaction for several reasons including Congressional

after a 2004 Citizens for Tax Justice (CTJ) study that identified SBC as a low tax payer was released, the Vice President and Controller of SBC, John J. Stephens, asserted that “We carry an enormous tax burden for this country; for that not to be made clear seems very inappropriate” (Weisman, 2004). In the letter to shareholders that opens the 2004 Wal-Mart Annual Report, Lee Scott, President and CEO, tells shareholders

I will also report that Wal-Mart paid \$4 billion in U.S. federal income taxes in fiscal year 2004. To borrow a page from my friend Warren Buffet’s annual report letter to the shareholders of Berkshire Hathaway, this means that if 446 other taxpayers paid the same amount as Wal-Mart, no other business or individual in the United States would have had to pay federal taxes last year (based on 2003 U.S. federal tax receipts of \$1.782 trillion.)

One might infer from these statements that at least some firms perceive a political cost to being labeled as tax aggressive.³

In some instances, investors may interpret news about a firm’s tax aggressiveness as evidence that the firm is not only willing and/or adept at cheating the IRS, but is also willing and capable of cheating the investors as well (e.g., Desai et al., 2004). Indeed, this link to financial statement aggressiveness may be more direct than implied for tax shelter firms. In his recent testimony to the House Ways and Means Committee, Professor Douglas Shackelford explained that managers of public companies will often pass up a tax strategy if it has an adverse effect or even a non-positive effect on financial accounting earnings (Shackelford, 2006). Professor Mihir Desai is even more direct in his testimony asserting that “No corporate tax shelter was ever undertaken that reduced book income and, often, the primary benefit of a corporate tax shelter is the reported income it produces” (Desai, 2006). If shareholders suspect that managements who cheat the IRS are also aggressive in reporting their accounting earnings, then the market may grow suspicious of

Acts to penalize inverting corporations and the negative reaction of the public to migration of U.S. corporations offshore and by the perception that such transactions are unpatriotic (Boise and Koenig, 2002).

³ Throughout the paper we use the terms tax aggressive, tax cheating, tax cheat, illegal tax activity, lying to the IRS, and lying to shareholders in reference to the potential behavior of our tax shelter firm sample. We acknowledge, however, that some of the shelters were more aggressive than others, and that many might argue that at least some of the shelters were legal.

the company's financial statements. To the extent that this happens, the market reaction to the news that a firm is tax aggressive could be negative.

In theory, then, news about tax aggressiveness could either boost or depress a firm's stock price. Indeed, while the companies singled out by the CTJ for paying little or no tax generally respond defensively (e.g., the SBC example above), Whirlpool, which had the *highest* current effective tax rate on the list of firms named by the 1984 CTJ study, was also not pleased. Robert Kenney, the company's tax counsel, stated, "It's a double edged sword. We owe it to our shareholders to take legitimate and legal means to keep down taxes" (Birnbaum and Murray, 1987, page 12).

This paper examines how the market evaluates news of corporate tax aggressiveness by investigating the market reaction to an initial press mention that a firm was involved in a corporate tax shelter. We study these events both because they are currently under intense scrutiny and because there is very little research on the firms that engage in tax sheltering (for one example, see Graham and Tucker, 2005). Some view the fact that not all firms engage in tax shelters as surprising because of the widespread view that shelters, at least in the 1990s, were in expectation a positive net present value proposition. The potential negative public relations effect is often cited as one reason for this "undersheltering" phenomenon (Bankman, 2004).⁴

We construct our sample by searching press articles, announcements and newswires for the term "tax shelter" along with either "corporate" or "corporation", and then restrict the sample to cases where the news refers to some type of accusation or announcement in the press that the firm engaged in a tax shelter. We do this while fully recognizing that there is no universally accepted definition of a tax shelter and that some of the firms in our sample mentioned in the press as

⁴ The perception of bad publicity is also evident in a recent *Business Week* article which states that "While the promoters of these schemes [tax shelters] are some of the most highly paid people on Wall Street, they are not nearly as widely known as merger specialists, analysts, or fund managers. That's no accident. Well aware of the bad publicity engendered by successful tax avoidance, shelter dealers keep a low profile" (France et al., 2003).

engaging in a tax shelter did not in the end actually get sent a Notice of Deficiency by the IRS or from another tax authority regarding their participation in the mentioned shelter.⁵

One challenge of using this shelter sample is to disentangle the reputation effect of the firm being labeled tax aggressive and 1) the effect of the costs of foregoing the shelter and its related deductions once the shelter involvement is known, 2) potential tax penalties, and 3) the legal costs the firm may incur. As a result, we also examine the market reaction to a different kind of news about tax aggressiveness: the release of studies by Citizens for Tax Justice (CTJ) on October 5, 1984, October 19, 2000, and September 22, 2004 that each featured calculations of the current effective tax rates of over 200 firms.⁶ The 1984 study in particular was highly publicized, and has been cited as one of the instigating factors of the Tax Reform Act of 1986 closing many of corporate “loopholes,” based on the belief that many companies were not paying their “fair share” of corporate income tax.⁷ These reports did not link any of the firms to illegal activity, however, and thus should not have the confounding effects of expected legal costs or expected loss from not being able to use their tax avoidance methods in the future.

We find that, on average a company’s stock price declines when there is news about its involvement in tax shelters, but the reaction is much smaller than for other accounting missteps.⁸ We also find some limited evidence of cross-sectional variation in the reaction. For example, the stock price decline is smaller for firms that have good governance, which is consistent with the idea that for these firms the news is less likely to trigger concerns about insiders’ aggressiveness toward

⁵ See Appendix A for a list of tax shelters included in our sample.

⁶ These are the dates of the press releases about the studies, and constitute day “0” of our event window; thus, we examine the day prior to the press release, the day of the press release and the day following the press release.

⁷ For a summary of the response to the 1984 CTJ study, see Birnbaum and Murray, 1987.

⁸ This small reaction is consistent with some anecdotal evidence in recent news stories. For example, in 2004 *The Wall Street Journal* ran a story about Merck receiving a “preliminary notice of deficiency” from the IRS where the tax in dispute was \$2.04 billion which, by most tax experts’ estimation, was “staggering” compared with other IRS disagreements. The article also stated that Merck had not fully reserved the amount in its financial statements. In spite of these facts, the article noted that “Investors took the news in stride, with Merck shares ending the day barely changed, at \$47.71, down 40 cents or 0.8%...” (Martinez, 2004).

the investors themselves. The reaction is more negative for firms in the retail sector, suggesting that part of the reaction may be a consumer/taxpayer backlash. We note, however, that some of these cross-sectional results are sensitive to the proxy used, for example, to measure a company's governance quality or its vulnerability to a public backlash regarding (lack of) corporate citizenship. We also test whether the reaction varies with the market's perception of how tax-aggressive the firm is, using the firm's current effective tax rate as a proxy for the market's beliefs, and find mixed results.

We also explore the stock price reaction to reports of current effective tax rate calculations released by Citizens for Tax Justice, hypothesizing that these reports signal tax aggressiveness without the implications for tax penalties or illegal behavior that tax shelter news carries, and any market reaction is therefore a pure reputation effect. We find no statistically significant stock price reaction to the reports, suggesting that the negative reaction to tax shelter news is not predominantly a reputation effect.

One caveat to our study is that if the market learns about the shelter in advance of the news, or if the market can estimate which firms are tax aggressive or engaging in tax shelters prior to the release of the news, then we should not expect a reaction on the day the shelter involvement or news of tax aggressiveness is released in the press.⁹ We recognize that, as is the case with any short-window event study, we cannot be certain we are actually capturing the "event" which contains the news. For example, it is possible that in some cases the market knows the firm is involved in the tax shelter or perhaps is even in court before the story is picked up by the press.¹⁰ There also could

⁹ Other than a reaction related to news about penalties and loss of the use of the shelter.

¹⁰ An example was brought up by one of our discussants using Colgate's tax shelter case. Using a search similar to ours, the first article the discussant could find was a May, 1996 *Wall Street Journal* article about testimony brought up in the court case. The case was well known in most tax circles well before the article, however, because it was already in court. We checked our sample and noted that our search picked up an article dated August 17, 1990, also from the *Wall Street Journal*. Thus, we include an article prior to the case going to court and prior to at least that information being so publicly available prior to our event date. However, the overall point is still valid and one we cannot completely fix – information about the shelter could be in the market place before the date of the press article we obtain. We believe the press article methodology we employ may often result in an earlier date than would using court filings

be trading by insiders before the information becomes public in any meaningful sense. We test the market reaction to the first announcement in the press, but if the information about the shelter was already in the market than we have understated (or more generally misstated) the effect. We believe it is unlikely that the market can identify shelter firms in advance, at least not with a high degree of accuracy. It is well known that a firm's tax liability and tax status – much less their involvement with aggressive tax shelters - is very difficult to discern from publicly available information (see McGill and Outslay, 2002; Hanlon, 2003; and Desai, 2006). Although firms are required to record a tax contingency reserve, we do not think that the market could garner much, if any, information from this account.¹¹ During our sample period very little was required to be disclosed about the tax contingency reserve and the account was widely suspect of being used as a “cookie jar reserve” to manage financial accounting earnings.¹² Thus, it seems unlikely that the market could use information from the tax contingency reserve to determine a firm's aggressive tax positions. Finally, in our cross-sectional tests we examine whether there is a difference in market reaction based on what we estimate as the market's perception of the firm's tax status, thus alleviating some concern about this caveat.¹³

The paper proceeds as follows. Section 2 summarizes earlier research concerning the market reaction to corporate misdeeds. Section 3 discusses the potential sources for the market's response to tax noncompliance news. Section 4 provides hypothesis development while Section 5 describes our research design and data. Section 6 presents our empirical results with regard to our

(since many of our events are prior to the case going to court) or disclosures in financial statements. However, it is still possible that the news is in the market before our event date, weakening the power of our tests.

¹¹ During our sample period, when firms took an aggressive tax position that they thought may be disallowed in the future the firm was supposed to record the related (saved) tax expense when determining its financial accounting earnings in order to prevent overstating the accounting earnings. The reserve for the saved taxes was called the tax contingency reserve (and was also known as the tax cushion prior to recent regulatory scrutiny).

¹² See Gleason and Mills (2002) for a discussion of how little was disclosed about the tax contingency reserve. We note the recently released Financial Accounting Standards Board Interpretation No. 48 provides guidance on the Accounting for Uncertainty in Income Taxes. We are unaware of any other voluntary disclosure by firms that indicates involvement in tax shelters, and believe that this is unlikely because such disclosure would also inform the IRS.

¹³ We use the firm's current effective tax rate (current tax expense divided by pretax income) computed over the last two years as a proxy for the market's perception of the firm's tax aggressiveness.

tax shelter sample. Section 7 presents our analysis of the market reaction to the release of three reports issued by Citizens for Tax Justice that featured current effective tax rate calculations of major corporations. Section 8 concludes.

2. Related Literature

2.1. Event Studies of Corporate Misdeeds Unrelated to Tax Aggressiveness

There is a considerable literature that uses event-study methodology to examine the effects on stock market value of information revealed about corporate misdeeds other than tax noncompliance. For example, Ellert (1975) examines the market price effect in the indictment month of 566 firms indicted in horizontal price fixing conspiracy cases between 1935 and 1971. He reports that there is a statistically significant abnormal return of -1.1 percent in the indictment month for a sample of 566 firms in his sample. Strachan et al., (1983), look at 47 price-fixing firms in the 1970s and report that these firms have statistically significant negative abnormal returns on the reporting date and on the day before of about -0.8 percent and -0.6 percent, respectively. Garbade et al., (1982) studies daily returns to examine the impact of antitrust suits, including price fixing suits, on 34 firms between 1937 and 1974, and finds an average sample-wide 6 percent decline in share price. In addition, Bosch and Eckard (1991) examine the stock price reaction to federal indictments for price fixing for 127 firms during the period 1962 to 1980, and find an average abnormal return of -1.08 percent; they estimate that only 13 percent of that can be attributed to various legal costs and argue that the remainder is due to the present value of lost monopoly profits. Karpoff and Lott (1993) study the stock price reaction to announcements during 1978 to 1987 concerning corporate fraud cases in which the damaged party does business with the accused firm.¹⁴ They find that reports of regulation violations do not trigger significant abnormal returns,

¹⁴ Karpoff and Lott (1993) distinguish four kinds of fraud, including frauds of governments, but these refer to cases in which the accused firm cheated or was accused of cheating on implicit or explicit contracts with a government agency, such as overcharging on military contracts. Tax evasion cases are not included, because the focus of the analysis is cases in which the some or all of the costs of the fraud can be internalized by the firm through its repeated interactions

although reports of other frauds do. For example, they report that for initial press reports of corporate fraud against private parties, stock price declined by an average of 1.22 percent, for frauds against government agencies (non-tax) the loss in value is on average 1.67 percent, and for financial reporting frauds the negative reaction was 4.66 percent. Finally, when investigating allegations of environmental violations Karpoff et al., (1999) report that there is an average negative abnormal stock price reaction of 1.58 percent when the alleged violation is first announced and a negative 1.92 percent return when the firm is charged with or sued for a violation.

More recently, there have been studies that investigate the market reaction to specific financial accounting misdeeds such as being accused of a violation of Generally Accepted Accounting Principles (GAAP) by the Securities and Exchange Commission (SEC) and of announcing a restatement of the financial statements of the firms.¹⁵ For example, Dechow et al. (1996) provide evidence for a sample of 92 firms accused by the SEC of wrongdoing that, upon the announcement that the firm manipulated earnings, stock prices declined by an average of 8.8 percent. Consistent with these estimates, Palmrose et al., (2004) find negative average abnormal returns of about 9 percent over a two-day announcement window, and that the abnormal return is more negative with indications of fraud, the larger the size of the adjustment, and for restatements attributed to the auditors. In addition, they find that the reaction is greater, *ceteris paribus*, if no amount is given in the announcement. The authors hypothesize that the negative signal associated with fraud and auditor-initiated restatements is associated with an increase in investors' expected monitoring costs, while higher materiality is associated with the greater revisions of future performance expectations.

with customers, suppliers, employees, and investors. One of the other three sub-categories is financial reporting frauds, which included cases in which agents of the firm misrepresented or are accused of misrepresenting the firm's financial condition. Another is regulatory violations, such as failure to report currency transactions, in which it is not clear that the firm violated an implicit or explicit contract with an investor or stakeholder.

¹⁵ Restatements do not necessarily involve an accusation of wrongdoing by the SEC. However, the firm restates because the prior financial statements were incorrect and thus the restatement constitutes an "explicit acknowledgement that existing financial statements do not conform to GAAP" (Palmrose et al., (2004)).

In sum, an earlier literature has generally found (often large) negative stock market responses to corporate misdeeds other than tax noncompliance, including specific financial accounting misdeeds. Because it is impossible to quantitatively assess the magnitude of the misdeed, it is also impossible to evaluate whether the market response differs across kinds of misdeeds per “unit” of the misdeed.

2.2. Relevant Tax Research

For reasons we elaborate on below, tax evasion events share some, but not all, characteristics with the non-tax events discussed above. While there are no previous analyses of the stock market reaction to tax shelter events, there are several related studies. Desai and Dharmapala (2005) examine how investors value managerial actions designed solely to minimize corporate tax obligations by regressing over a cross-section of companies Tobin’s q (market value divided by the replacement cost of assets) on a proxy for tax avoidance (measured as an estimate of the book-tax differences of the firm less an estimate of the portion of the book-tax differences due to earnings management (i.e., the total accruals of the firm)). They find that their proxy for tax avoidance is positively related to firm value for well-governed firms, but insignificantly related to firm value for poorly-governed firms. The authors interpret their evidence as consistent with agency costs mitigating the benefits to shareholders of corporate tax avoidance. In other words, the managers’ tax sheltering decisions are related to their ability to divert value, so that in poorly-governed firms the tax sheltering is indicative of a higher likelihood of managerial wealth diversion and thus on net adds no value.

Graham and Tucker (2005) investigate a sample of 44 firms that received a Notice of Deficiency from the IRS for their involvement in a tax shelter to determine whether the firm’s tax shelter acted as a non-debt tax shield. They find that the shelter firms do appear to carry less debt during the years in which the tax shelter is in place (as well as years leading up to the shelter years).

Graham and Tucker (2005) do not, though, investigate the market's response to news of these shelters.

There is also research that examines the wealth effects of companies undertaking specific tax-favored transactions, or of changes in the tax treatment of specific transactions. For example, there are several studies that examine the market response to corporate inversions (i.e., when a corporation reorganizes so that the parent corporation is in a low tax jurisdiction). The results of these studies is somewhat mixed, however. Desai and Hines (2002) report a positive market response to the inversion announcement, Cloyd et al. (2002) report a negative to insignificant reaction, and Seida and Wempe (2004) report that firms have widely varying market responses to the announcement based on the firm's tax characteristics.

These studies all concern different types of events (i.e., either the stock price impact of changes in the tax law, or of tax-related company decisions, such as inversions) than we study below. Consider the studies investigating the market response to tax-related company decisions. We would expect *any* company decision to be value enhancing—otherwise why do it? A negative price response would occur only if the market disagreed with the decision-makers' assessment of its value. Our study of tax shelter news fits more closely with the studies of the stock price reaction to non-tax-related enforcement actions. The news concerns a press description or government enforcement action, not a company action. The press description or government action pertains to *past*, although possibly also ongoing, actions of the company. Because the event tested is not a firm action, one cannot presume that it is value maximizing. Holding constant other information about the firm, an enforcement action is bad news; however, it may provide previously difficult-to-discern insight into how aggressive the firm's taxpaying culture is. No previous study has addressed this set of issues.

3. The Market Reaction to Tax Noncompliance News: Potential Components

Strikingly, the earlier literature on the market reaction to corporate fraud announcements ignores the possibility that shareholders might want the company to be aggressive in their dealings with other companies and with the government. To be precise, it ignores the natural assumption that shareholders want the company to be *optimally* aggressive, taking into account the expected cost of being caught overstepping the line.

Although the earlier literature has analyzed the market price reaction to news regarding a wide range of corporate fraud, including fraud against the government, it has not addressed the market price reaction to press reports regarding tax avoidance. Such news of tax aggressiveness can indicate a number of different, but not mutually exclusive, things. Although some of the implications are similar to those addressed in prior literature, others are specific to news regarding tax avoidance.

The news may simply indicate that a firm is tax aggressive and nothing more. If this aggressiveness is not a surprise to the market, then the news may not move the market. However, if the firm was previously thought to be suboptimally pursuing tax-reduction strategies, then the news that the firm has now engaged in aggressive tax planning may indeed provide new, positive, information to the market.

Second, the news announcement may reveal that the firm's (known) aggressive tax position is being challenged by the IRS which, if upheld, could result in a tax judgment and penalties; this is bad news. The idea that future fines and penalties constitute part of the market's response has been studied previously in the contexts of other corporate misdeeds (see, for example, Karpoff and Lott (1993) and Bosch and Eckard (1991)).

Third, the news may reveal the final outcome of a tax shelter case, where the outcome may either be good or bad for the firm. This could have implications for the viability of future tax planning efforts. For example, in the prior non-tax fraud literature this aspect was labeled the loss

of case-related income stream. The case may reveal that a portion of the firm's expected after-tax profits is lost, assuming that the case-related activities will cease, be curtailed, or will be continued at a higher cost of concealment (Karpoff and Lott (1993)).

Fourth, the news could raise concerns that the company's management is engaged in cheating *everyone*, not just the IRS. As Bosch and Eckard (1991) discuss, this has at least two components. First, customers and suppliers might become wary of dealing with the firm, thereby increasing future transaction costs and perhaps causing customers and suppliers to deal with other companies (as in Klein and Leffler (1981)). Second, the case might signal that the firm might be engaging in other as yet unknown illegal activity that could lead to prosecution and associated costs. In addition, it may signal that the dishonesty extends to the financial accounting statements – in other words management is lying to the shareholders as well (see Desai and Dharmapala, 2005 and Desai et al., 2004).¹⁶

Finally, another component of the market reaction may be implied management incompetence. Accusations of wrongdoing might signal management incompetence: “assuming that smart managers wouldn't get caught” (Bosch and Eckard, 1991, p. 310) or reveal that the managers misestimated the probability of being caught.

We cannot empirically separate all of the effects listed above that may affect the market's response. However, we attempt to shed light on the nature of the stock market reaction by investigating cross-sectional differences in market reactions, as well as by examining the market

¹⁶ In an attempt to see whether institutions think poorly of firms engaged in tax shelters (or tax-aggressive behavior), we searched the web for articles that mentioned the California Public Employees' Retirement System (CalPERS) and tax shelters (or aggressiveness). We found several articles mentioning that CalPERS threatened to divest and block state purchases of stocks and bonds of U.S. companies in foreign tax havens. Although one article quoted Philip Angelides, California Treasurer, as saying foreign tax shelters "...are just another way to game the system" (SFGate.com, July 25, 2002), other articles present CalPERS's concern as being about shareholder rights rather than the avoidance of tax. For example, on the CalSTRS (California State Teachers' Retirement System) website is an article about institutional investors taking out a full page ad in the *USA Today* calling for Ingersoll-Rand (which had moved its headquarters to Bermuda (i.e., inverted) for tax reasons) to move back to the U.S. citing a "...substantial loss of shareholder rights that goes with off-shore incorporation" (<http://www.calstrs.com/Newsroom.What's%20New/callforvote.aspx>). Further, a *Tax Briefs* article from Levin and Weiser, LLC (March 3, 2003) states that "CalPERS believes Bermuda incorporation makes it more difficult for Tyco shareholders to hold the company, its officers and directors legally accountable in the event of wrongdoing" (page 6).

price reaction to news about tax aggressiveness that should not have the threat of legal sanctions or loss of future tax savings: firms listed as low tax payers in the CTJ studies.

4. Hypothesis Development

Because of the competing hypotheses mentioned above, we do not have a strong *ex ante* prediction concerning the market reaction upon the release of the news. We do, however, have some prior beliefs about what cross-sectional firm characteristics should affect the market's response to news of the tax aggressive behavior. First, we use the current effective tax rate from the firm's financial statements (calculated as the current tax expense divided by pretax income) as a proxy for the market's *ex ante* information about the tax aggressiveness of the firm.¹⁷ The idea is that, the higher the CETR, the less likely that the market would expect the firm to be tax aggressive, and the more likely it is to take the tax shelter news as a positive signal of aggressiveness. As a result, the higher the firm's CETR, the more positive, or less negative, the reaction upon the news announcement. We base this sign prediction upon the concept that there is some level of optimal tax aggressiveness, while recognizing (as stated above) that this is a double edged-sword.

Second, we hypothesize that poorly-governed firms will have a more negative market reaction relative to well-governed firms because of aroused suspicions that these firms are not simply cheating the IRS but they are cheating everyone, including the shareholders.

Finally, we predict that firms in the retail industry that deal directly with consumers will have a more negative reaction than other firms. These firms may be more concerned with public perception and the problems with appearing unconscionable or unpatriotic. We attempt to examine these cross-sectional predictions using both the tax shelter and the CTJ study samples.

¹⁷ Note that we do not use the current effective tax rate to infer the actual tax payments of the firm, but rather the market's perception of the tax payments/status of the firm.

5. Sample and Research Design

5.1. Sample Selection

To obtain our sample of firms, we first do a broad search of all sources in the Factiva database. We use the search terms “tax shelter” and “corporat*” to capture all articles or press releases that contain those terms. We conduct the search over the date range beginning January 1, 1990 and ending September 1, 2004 (the date we started the search process). From this search we obtained a beginning sample of 6,293 articles, newswire releases, or other types of press mentions. We then had two research assistants read through the articles, retaining only those that contained the name of a company associated in some way with having a tax shelter. The types of articles discarded by this process generally pertained to the topic of tax shelters generally, without mentioning any particular company. The remaining sample of potential observations contained 824 articles. We exclude articles about inversions, because these have been studied separately. We also exclude articles about transfer pricing, because these cases are not generally considered tax shelters although they are sometimes called such by the press. We choose to exclude articles about specific tax laws and the companies that take advantage of them (legally) which the press claimed was a tax shelter – such as the Foreign Sales Corporations and the Agricultural Land Tax provision. Finally, we exclude articles about taxes in other countries (e.g., the Petroleum Reserve Tax), articles about private companies, and non-income-tax “shelter” issues. After these exclusions, we retain a sample of 601 articles. Table 1 presents a summary our sample selection process.

We then reduce the sample by excluding multiple observations (366 articles) of the same firm-shelter, retaining only the first article and/or the first article in a major press source (defined as one of the major newswires (Dow Jones, AP, or Reuters), the *Wall Street Journal*, or the

Washington Post),¹⁸ We exclude observations with missing data in the CRSP database (45 articles) and for various other reasons spelled out in Table 1. This leaves us with a sample of 150 articles.

We create four samples from these remaining 150 articles. These samples are not exclusive, as they overlap to some extent. The first sample, which is the sample that we utilize for our main analyses, is the sample that contains the first article for every shelter that a firm is in. The articles in this sample do not have to be in a major press source, but can be in any press article or release. In addition, if a firm is in two different shelters that firm will have two different articles (dates) included in the sample. This sample contains 108 firm-shelter combinations.

The second sample includes only the major press mentions of the firm-shelter combinations in the first sample. Thus, this sample is a subset of the first sample and contains 70 firm-shelter observations.

The third sample is similar to the first sample in that it includes all articles, major press source and non-major press source. It differs by including press mentions only for the firm's first shelter. Thus, if a firm is in two tax shelters, only the first news about either shelter is included in the sample (N=97). We test the first shelter observation only in order to examine whether the effect is stronger for the *first* news that a firm is tax aggressive.

The fourth sample is then analogous to sample two – it includes only the major news source press mentions of the firm-shelter observations included in the third sample (N=63).

5.2. Event Date and Test Period

We use an event-study methodology to test the market reaction to news that a firm has engaged in tax-aggressive behavior. We examine the three-day window surrounding the press mention. We center the three days on the day of the press mention. For example, we label the day the story appeared in the press as day 0 and examine returns for the window of day -1 through day

¹⁸ We note that Factiva does not include full text of the *New York Times*. While we have one article from a *Times* abstract (which are included in Factiva), this same story was covered by the *Wall Street Journal*.

+1. We include the day prior to the release to capture any effect of news available to the market before the story and the day after to provide time for the market to react.

We use a market-adjusted model based on a value-weighted index (with dividends) to estimate abnormal returns. For the tax shelter tests, this model subtracts the CRSP market index return from a company's daily return to obtain the market adjusted abnormal return (AR) for each day and company.¹⁹ For the CTJ sample, the model subtracts the firm's own market index return from a company's daily return to obtain the market adjusted abnormal return (AR) for each day and company.²⁰ The daily abnormal returns are then summed to calculate the cumulative abnormal return (CAR) for a given time period. We report three measures of statistical significance. The first is the unadjusted t-statistic using the standard deviation of the sample CARs. The second is the statistic described in Patell (1976), t_{patell} , which estimates a separate standard error for each security-event and assumes cross-sectional independence. Third, we calculate a t-statistic, t_{cda} . Although the event dates in our study are not the same for all of our observations, we do have groups of observations with the same dates (e.g., one article may mention several firms). Because event-time clustering casts doubt on the independence assumption for abnormal returns in the cross-section (Kothari and Warner, 2004; Collins and Dent, 1984; and Bernard, 1987), we also report t_{cda} , which is constructed using a portfolio of event-firms to obtain a time series of abnormal returns on the portfolio for 180 days around the event date. The standard deviation of the portfolio returns is then used to assess the significance of the event-window average abnormal return. Using this approach

¹⁹ As a sensitivity analysis we also conduct our tests using the market model. We discuss the results of these analyses below.

²⁰ In particular, for NASDAQ traded firms we subtract from the stock's return the NASDAQ return for the day, and for NYSE firms we subtract the NYSE return for the day. Almost all of the firms in our CTJ samples are NYSE traded firms (198/199 in 1984, 204/214 in 2000, and 227/243 in 2004), but subtracting the CRSP return subtracts the total return from all the exchanges combined. While normally this is not an issue, we have the same event day for every observation and the NASDAQ had a large positive return on one of the days in the event window for the year 2000. Thus, using the CRSP index as the market benchmark resulted in significantly negative abnormal returns for the 2000 CTJ study but this was primarily because the NASDAQ had a large positive return and not because of anything related to news about tax aggressiveness. Using the CRSP index or the firm's own index does not make a difference in our CTJ event tests for the years 1984 or 2004 or for the tax shelter event tests. However, we present all CTJ study event tests using the firm's own market index to be consistent across the studies.

accounts for the cross-sectional dependence, because the variability of the portfolio returns through time incorporates whatever cross-dependence that exists among the returns on individual event securities (Kothari and Warner, 2004; Brown and Warner, 1980, 1985). Finally, we report t_{var} , which compensates for the possible variance increase during the event period.²¹ To the extent the event period is associated with increased uncertainty, i.e., greater return variability, the use of historical or post-event time series variability might understate the true variability of the event-period abnormal performance (Kothari and Warner, 2004).

We also report the results from a non-parametric generalized sign test. The null hypothesis for this test is that the fraction of positive returns is the same in the event period as in the estimation period. We label this statistic Z_{sign} .

6. Shelter Event Results

6.1. Overall and By-Category Averages

Panel A of Table 3 reports the results of the above tests. For the sample of observations which includes the first shelter news for each firm-shelter from all press sources (N=108), the mean CAR is -0.44%. This estimate is not significantly different from zero. If, however, we measure the abnormal returns for the sample including only major press mentions (and not mentions in smaller papers or news sources), the average CAR is -1.16%, which is significantly different from zero at a (two-tailed) p-value less than 0.05 for each of the t-tests we utilize. Thus, it seems that on average the market reacts negatively to news that a firm is in a tax shelter.²² However, relative to news of restatements of earnings (which brings a negative reaction on average of 9%), the negative reaction to news of corporate tax aggressiveness is much smaller.²³

²¹ This is the STDCECT option in Eventus.

²² The CAR day-by-day for the full sample (major press only sub-sample) is a return of -0.40% (-0.78%) on day -1, a return of -0.10% (-0.17%) on day 0, and a return of +0.06% (-0.21%) on day +1.

²³ Our results could be understated if firms whose auditor was previously accused of selling aggressive shelters experienced the majority of the market reaction upon the release of the news about the auditor. We investigate whether there is any difference in the market reaction to our press news for firms audited by KPMG as compared to other

We next report in Panel B of Table 3 the results for our sample after dividing the observations into those for firms which are relatively better governed and those firms which are relatively more poorly governed. As a measure of the quality of corporate governance, we use the Gompers et al. (2003) index of shareholder rights that ranges from 0-24, where a high value indicates low quality governance. We classify all firms in our sample with a governance score above the median score in our sample as poorly governed. If the variable is missing for a firm we include an indicator variable (NO GOV) set equal to one and do not include them in the poorly governed classification. In our sample, we have 39 observations which are for firms that are relatively well governed and 60 observations for firms with relatively poor governance.²⁴ The mean CAR for the well governed and poorly governed firms are -0.20% and -0.86%, respectively. The returns for the relatively poorly governed observations are significant using three out of the four test statistics and also using the non-parametric sign test; the returns for the well governed firms are not significant using any test. Thus, there is some support for the hypothesis that relatively more poorly governed firms will have a more negative response to the news about tax aggressiveness. However, when we test the difference in the means between the two groups, we find that they are not significantly different (p-value of 0.15, one-tailed). In untabulated data, we also conduct the same analysis for the sub-sample of observations with major press mentions (N=70) and find that the CAR for the well governed firms is -0.55% and for the poorly governed firms it is -1.69%; the difference between these is marginally significant at a p-value of 0.085, one-tailed.²⁵

auditors to see if there is evidence that this is the case. We find that the market reaction for the KPMG firms is in fact more negative for both the full sample and the major press sample (one-tailed p-values of 0.04 and 0.06, respectively). Thus, it does not appear that the stock price reaction of firms audited by KPMG is completely negated by any prior news about their auditor.

²⁴ There is not an equal number of observations on both sides of the median because of the mass of observations at a central number; these have been assigned to the poor governance category.

²⁵ We also investigate institutional holdings as an alternative measure of governance based on the idea that institutional investors monitor managerial performance more than individual owners, thus providing improved governance at the firm (similar to Desai and Dharmapala (2005)). We measure institutional holdings using data from Thomson Financial (Spectrum) by computing the fraction of total shares outstanding that are held by institutions (banks, insurance companies, mutual fund parent companies, pensions, endowments, and professional investment advisors). In the returns analysis, we find that firms with greater than the median (in our sample) institutional holdings have an abnormal return

In Panel C of Table 3, we divide the observations into those with a CETR at or above the sample median and those with observations below the sample median. We measure the CETR as the firm's average current effective tax rate (current tax expense (Compustat data #16-data #50)/total pre-tax book income (data #170)) over the two years prior to the press mention. We replace this measure with a substitute under two conditions. First, if the firm is in the banking industry (for which deferred taxes are unavailable), we replace the numerator of the CETR with total taxes (data #16) and then include a separate indicator (Bank) set equal to one for firms in the banking industry, and zero otherwise. Second, if this measure is not computable because the firm has a negative pre-tax book income, we include an indicator variable labeled LOSS set to 1 for these firms, and zero otherwise. For these loss firms, we also set the CETR continuous variable to zero. Finally, we reset any values of CETR greater than 50% to 50%, and any values less than zero to zero.

The firms with a CETR below the median have an average CAR of negative 1.07%, significantly negative at conventional levels. The firms with a CETR at or above the median value is -0.08% which is not significantly different from zero (the difference between the two returns is marginally significant at 0.09, one-tailed). Thus, there is some support for the hypothesis that firms with a higher CETR will have a less negative (more positive) reaction to news about tax aggressiveness. However, in untabulated results, we conduct this analysis for the sub-sample of 70 observations where the press mention is in a major press source. The returns are -0.013 for the high ETR group and -0.014 for the low ETR group; the difference between these is not statistically significant. Thus, we view the evidence on the CETR as mixed.

over the event window of -1.08% (-1.41% in the major press mention sample), and firms with less than the median institutional holdings have a return of 0.02% (-0.93% in the major press mention sample). For the full sample, the difference between the two groups has a two-tailed p-value of 0.1137. Thus, it seems that firms with high institutional holdings have a somewhat more negative return than firms with low holdings, which is apparently inconsistent with the monitoring story.

Panel D of Table 3 shows the sample divided into retail and non-retail firms. There are only eight observations from the retail sector which limits our ability to infer much from this division. However, the return for the retail firms is on average -1.27%, while the return for the non-retail firms is only -0.37%. Neither is significantly different from zero and the difference between them is not significant at conventional levels. Nevertheless, the point estimates are consistent with the hypothesis that the possibility of a negative consumer reaction to an indication of “bad” corporate citizenship makes retail firms relatively more vulnerable to news of their tax aggressiveness. When we repeat this test (not reported here) for the sub-sample of 70 observations where the press mention is in a major press source, the CAR for the retail firms is -0.026 and the CAR for the non-retail firms is -0.010. The difference between these is marginally statistically significant with a one-tailed p-value of 0.065. Thus, the inference is similar in the two samples.

We note that Karpoff and Lott (1993) distinguished four distinct types of press dates: the allegation date, the charges filed date, the settlement date, and the additional information date (the first date after the settlement date that contained any additional information about the settlement terms). In many cases, the first press date was an allegation date; in many others, the first press announcement was the date when charges were filed date, or was a settlement date.²⁶ For our shelter firms, there is not a press article at every stage of the process (e.g., when the IRS assesses deficiency, when the case goes to court, when the company loses or wins, when the other side appeals, etc.). In some cases, the article is simply the press itself uncovering the tax strategy and reporting on it. In other cases, the first mention in the press is the firm losing or winning a court case. Thus, a complete string of dates for each firm-shelter is not feasible for our study. Instead, we retain the first article for each firm-shelter and code the article by the type of event, in order to

²⁶ Karpoff and Lott (1993) noted that note that it was “very possible” that news of alleged fraud was publicly available before it appeared in the newspaper.

examine whether the market reaction varies by the type of events.²⁷ We classify our firms into seven different event types. We describe each of these event types below and indicate in Panel E of Table 3 how many observations from our primary sample (the 108 observations) are classified into each grouping.

PRESS ONLY is the group of firms mentioned by the press as being in a tax shelter but there is no mention of the IRS, Congress, or anyone else looking into these shelters or threatening to stop them. EXAMINATION is the group of firm-shelters that are in the press because the firm is accused of participating in the shelter and now the IRS, Congress, the SEC or some other regulatory body is examining these shelters. SHUTDOWN is the group of firm-shelters that are in the news because the shelter has been effectively stopped and the firms were supposed to unwind the transactions they engaged in to participate in the tax shelter. In our sample, all of these firms were in the step-down preferred stock tax shelter (described in Appendix A) that was made illegal retroactively. EARNWARN is the group of firms where the press mention concerns the firm announcing their involvement in the tax shelter as part of an overall earnings warning for the company. In these cases, the tax shelter and subsequent loss of the shelter is causing part of the earnings problems. COURT is the group of firm-shelters where the press mention concerns the shelter case going to or being in court. LOST is the group of firm-shelters where the press mention is about the firm losing a court case or settling out of court, agreeing to pay some amount of back taxes. WON is the group of firm-shelters where the press mention is about the firm winning a court case, the IRS deciding not to appeal a previous case, or for which the law just granted some exemption for the shelter.

²⁷ We recognize that different events in the sequence could have different market reactions. For example, one discussant pointed out that Qwest had no noticeable stock price reaction to the first article (in the *Wall Street Journal*) he could find, and no reaction upon the receipt of the Notice of Deficiency from the IRS. However, when the firm disclosed in their 10-k that they were taking an earnings charge related to the shelter, there was a negative reaction. In our tests, we are attempting to capture the reaction to the first news that the firm is in the shelter in order to infer whether there is any reputation effect to the firm for being tax aggressive. Thus, we retain the first article we can locate and categorize the articles by event type.

Panel E of Table 3 reports the mean CARs for our sample by type of event, so we can examine whether the estimated CAR varies by the type of shelter-related event the news concerns. Before discussing these results, we note a general caveat to this analysis. We do not investigate every event in our sample for other (i.e., non-tax shelter) news that might have been revealed on the same day, under the assumption that the occurrence of this other news should be randomly timed. However, when examining partitions of observations the sample size becomes quite small, increasing the likelihood that some other event could significantly affect the results by subgrouping.

Subject to this caveat, we offer some thoughts about how the stock market reaction might vary with the type of event. For example, we would expect the returns for observations in the PRESS ONLY category to reflect more “pure” reputation effects and not any market reaction related to losses due to penalties or expected legal costs. Indeed, the market returns for this group of firms are not significantly different from zero, perhaps indicating that news that a firm is tax aggressive--absent any indication that it is being challenged by the IRS about it--is not negative news to the market. For the 57 firm-shelter observations in our sample where there is a press article that mentions both that the firm was in a shelter *and* that the firm is being accused of wrongdoing by some authority (group labeled EXAMINATION) the data reveal a marginally negative market reaction of -0.62 percent, which is significant at less than 0.10 using a one-tailed p-value in two out of four of our t-tests and using the generalized sign test. This may indicate that charges against the firm and the resulting likelihood of legal costs, loss of the shelter, and possibility of penalties to induce the negative market reaction rather than the simple reputation effect of the firm being identified as being tax aggressive. Below we will attempt to investigate this distinction further using the CTJ studies, which contain a sample of firms that also have no formal charges assessed against them.

The classification SHUTDOWN is a little more difficult to interpret. For this group of firms the shelter in question was shut down by the government, and the transaction was supposed to be unwound (reversed) by the taxpayer immediately.²⁸ One might expect the market to interpret this negatively based upon the reasoning above – the firm will no longer be able to obtain the tax benefits available under this shelter. Indeed, estimates in the press suggested that approximately \$10 billion was involved in the deals in the month prior to the IRS ruling alone (Nyhan, 1997), and that nearly \$7 billion in Federal tax revenue would have been lost over ten years if the IRS had not shut down the deals already completed or in the works (Hershey, 1997). However, on the date of the news announcement these companies were not assessed penalties nor were they expected to incur legal costs because of the way these cases were resolved – the IRS simply issued rulings that eliminated the tax benefits to these transactions. Many of the issues had tax call provisions (otherwise known as “make whole” provisions) that protected the investors from losses should the issuer have to call the issues early. In addition, it is possible that these transactions were perceived as questionable to shareholders for non-tax reasons. Thus, when the ruling was issued and firms had to reverse their positions the market may have reacted positively. Further, the loss of the deduction was not expected to hurt issuers’ earnings per share (the shelter was entered into only in the month or two prior to the ruling for most firms) although there could have been minimal costs to unwind the transactions (Padgett, 1997). Finally, investors may have been positively surprised that these firms were taking tax aggressive positions. All of these things would lessen the likelihood of a negative market reaction (or increase the likelihood of a positive reaction), and could potentially explain the positive 1.56 percent abnormal return we observe for this group of observations.

The abnormal returns over the event window for the next three groupings (EARNWARN, COURT, and LOST) are generally negative in sign, as would be expected. However, only the

²⁸ Ex post, however, some firms did fight the ruling. For example, Freddie Mac’s case was not settled until 2005 (PR Newswire, May 12, 2005).

CARs of firms that lose a court case have even marginal statistical significance (using a one-tailed test).

The most unanticipated result is that the group WON has the largest average negative return of any of our groupings. We note, though, that there are only three firm-events in this sample, so it would be a mistake to place much weight on this result. Moreover, there is some reason to believe that, by chance, other significant events occurred for these firms--GE Capital Corp., Bank of America Corporation, and Crown Holdings--on the same days as the tax-shelter events. Our careful reading of the news about these three firms during their event window suggests that there were arguably some substantive coincidental factors that could have caused a negative CAR, and some stories that could have led the market to believe the company would not ultimately prevail in the tax case.²⁹ We recognize that this explanation is not completely compelling, in part because we did not carry out a similar search for non-tax news for the other events; however, it offers a plausible explanation for the counterintuitive result.³⁰

6.2. Cross-Sectional Analysis of Excess Returns

To address our hypotheses, we investigate the cross-sectional relation between firm characteristics and the event window returns. We conduct this examination via regression analysis of the abnormal returns during the event window on the firm's current effective tax rate (CETR), an indicator variable for whether the firm is considered poorly governed, and an indicator variable for whether the firm is in the retail industry.

We use the current effective tax rate (computed as described above) from the firm's financial statements as a proxy for the market's ex ante information about the tax aggressiveness of

²⁹ A detailed account of the news stories about these firms is available upon request.

³⁰ To examine the robustness of our results to the inclusion of these three events, we estimate our Table 3 Panel A results after eliminating these three firms. The overall CAR for the full sample of firms excluding the three events coded WON is -0.33%, and for the major-press-mention-only sub-sample the CAR is -1.12%. Thus, the removal of the three WON event observations does not change our overall inferences with regard to the average reaction to news of tax aggressiveness.

the firm. We conjecture that the higher the CETR, the less likely it is that the market would expect the firm to be tax aggressive. As a result we predict that the higher the firm's CETR, the more positive the reaction upon the news announcement.

Our second hypothesis is that poorly-governed firms will have a more negative market reaction relative to well-governed firms because of aroused suspicions that these firms are not simply cheating the IRS but they are cheating everyone, including the shareholders. We test this hypothesis by including an indicator set equal to one for poorly governed firms (POOR GOV) and zero for all other firms.

Finally, our final hypothesis is that firms that deal directly with consumers, proxied by being in the retail industry, will have a more negative reaction than other firms. Thus, we include an indicator variable set equal to one for firms in the retail sectors (NAICS codes 44-45) and set to zero otherwise.

The regression results are presented in Table 4, for the all-firm-shelter sample in Panel A and for the sample limited to only major press sources in Panel B. We present several specifications, but focus our attention on Column (4) of Panel A, the most complete model.

In Table 4, Panel A, for the full sample, the estimated coefficient on the CETR variable is, as our reasoning suggests, positive. The estimate of 0.1079 is also substantively large, because it suggests that that a one standard deviation (equal to 0.124) lower value of CETR is associated with a 1.34% lower value of the CAR surrounding a tax-shelter news release. The p-value for this estimate is 0.030, indicating statistical significance at standard confidence levels.

Also as prior reasoning suggests, the estimated coefficient on the dummy variable indicating poor corporate governance is negative. The coefficient suggests that a poorly-governed firm will, upon a news report of involvement in a tax shelter, have a market price reaction of 0.65% lower than a relatively well-governed firm. However, the standard error of this estimate is too high for us

to be able to, at standard levels for confidence, say that the negative coefficient is not due to chance.³¹

Finally, we hypothesized that firms in the retail sector might be more negatively impacted by news of a tax-shelter involvement, because of the possibility of a consumer reaction to perceptions of not being a good corporate “citizen.” This hypothesis is also supported, although weakly, by the data. The estimated coefficient on the dummy variable for being in the retail sector is -0.017, suggesting that the market reaction is 1.7% lower for a retail firm compared to non-retail firms.³² With the small number of firms in the retail sector, we cannot however assert with standard levels of confidence that this result is not due to chance; the p-value is equal to 0.1260.³³

In results not reported, we also investigate to what extent the market reaction varies by the type of tax-shelter event and with the type of event interacted with the mediating factors discussed above. The small number of events for each class precludes us from making any confident statements about these issues. However, we note that the interaction of POOR GOV and the shelter

³¹ When we include the measure of institutional holdings (see footnote 25) in the regression (analogous to column (4) in Table 4) as a proxy for governance (an indicator variable set equal to one for below the median institutional holdings) the coefficient on that variable is positive (0.008) in both samples—meaning firms with less institutional holdings have a higher return -- but is insignificant with two-tailed p-values of 0.327 for the full sample and 0.450 for the major press mention sample.

³² Because there is anecdotal evidence that firms engaged in the Corporate Owned Life Insurance (COLI) type of shelter are more likely to be retail firms, it is possible that the coefficient on the RETAIL variable is negative because the events refer to COLI shelter firms, not because they are in the retail industry. To investigate this possibility we include an indicator variable set equal to 1 for firms in the COLI tax shelter in addition to all the other variables in the column (4) specification. The coefficient on the RETAIL variable remains negative (equal to -0.0147 (p-value of 0.173) in Panel A and equal to -0.0164 (p-value of 0.095 in Panel B). The coefficient on the COLI variable is positive, but insignificant.

³³ We also tried substituting a measure of brand value for our RETAIL variable in order to capture a broader proxy of the vulnerability of companies to public perceptions of corporate citizenship. We set an indicator variable equal to 1 if the firm is listed as having one of the top 100 brand names as ranked in Business Week magazine in any of the years from 2001 to 2005 (the only years for which we could obtain the data). Business Week obtains the rankings from Interbrand, which ranks brands based upon the estimated amount the brand is likely to earn the firm in the future. Out of our 108 firms in the tax shelter sample, only 10 are on the Business Week brand list; out of our 70 observation sample of major press mentions, only 7 are included on the Business Week list in any of the years 2001 to 2005. When we include this indicator variable in the place of the RETAIL indicator (in the regression model in Table 4 column (4)) the variable is positive at 0.0042 in the full sample and 0.0089 in the sample including only major press mentions, however, both coefficients are not significantly different from zero with two-tailed p-values of 0.684 and 0.468, respectively.

event types is significantly negative in four out of the six cases, indicating an incrementally negative reaction for relatively poorly governed firms for certain events.

The analysis of the major press mentions only, shown in Column (4) of Panel B of Table 4, generates results that are broadly consistent with the results for the full sample. The impact of poor governance is about twice as large in absolute value, and becomes statistically different than zero at a 10% confidence level ($p=0.081$). The estimated coefficient on the retail sector becomes slightly smaller in absolute value, but now (barely) satisfies the 10% confidence level. The striking results concerning the CETR do, though, deteriorate when the sample is restricted to the major press source. The estimated coefficient falls from 0.1079 to 0.0286, and the estimate is no longer different than zero at standard confidence levels. Thus, as mentioned earlier, we view the results concerning the relation between the CETR and the market reaction as mixed.

All in all, the multivariate regression analysis is broadly consistent with the hypotheses this paper develops. News about tax shelter involvement on average affects a firm's stock price negatively. The impact, though, varies depending on the type of firm, being worse for firms with poor governance, in the retail sector, and, in the full sample, with a low current effective tax rate. Whether the impact varies by the type of event after controlling for these other factors is impossible to discern. The relatively small sample size means that these statements cannot be made with a high level of confidence.

The pattern of estimated coefficients implies that, for certain kinds of firms, we expect that the release of news of involvement in a tax shelter will cause the stock price to *increase*. For relatively well-governed companies that are not in the retail sector, this prediction applies to any firm with an CETR that exceeds 0.374 ($0.0404/0.1079$). This value is about one standard deviation above the CETR mean. The fact that we conclude from the regression analyses that a positive price response is not an extremely rare occurrence is consistent with the non-parametric examination of

the distribution of CARs that reveals that 44% of the abnormal return observations (47 out of 108 cases) are positive.³⁴

6.3. Additional Caveats and Limitations

There are several additional caveats and limitations to our tests and results. The first is that we cannot reliably decipher when, if ever, a firm recorded the tax savings in its financial statements. Thus, any observed negative market reaction could reflect that the firm booked the tax savings previously and now owes the taxes, requiring the firm to show that it made less after tax than it reported to shareholders; conceivably the price will adjust downward to reflect the lower amount of earnings. Although we use press articles about the shelters that often do not mention any effect on earnings, we cannot be certain that the potential earnings impact (rather than, or in addition to, the information that the firm is tax aggressive) is not affecting our results. However, we think it is unlikely that the market has information on whether the firm recorded the tax savings and thus, any market reaction due to this would be based on its perception of whether the firm booked the tax savings. Both the actual, and certainly, the perceived recording of the tax savings are not measurable, however, which is a potential limitation of the study.³⁵

Second, and related to the above, is that firm governance and how the firms account for the tax savings could be correlated. For example, it could be that firms with relatively poor governance

³⁴ As a sensitivity test, we conducted the entire analysis for the tax shelter sample using the market model to compute cumulative abnormal returns rather than the market adjusted model. The inferences of the analysis are very similar no matter which method of computing abnormal returns is used. For example, the overall CAR for the sample of 108 observations using the market model is -0.17 percent and the CAR for the sample of 70 observations (major press mentions) is -0.88 percent, with the -0.88 percent being statistically significant and the -0.17 not being statistically significant as in our reported results reported in Table 3 using the market adjusted returns. A notable difference from Table 3 is that the PRESS ONLY grouping of firms has a 1.05% CAR using the market model, which is significant at conventional levels. Otherwise, the signs and significance of the results are consistent with those reported in Table 3. The regression analysis from Table 4 also produces similar results. For example, the coefficient on the ETR variable in Column 4 becomes 0.1159 and remains significant at a one-tailed p-value of 0.03; the coefficient on the POOR GOV indicator variable becomes -0.0088, marginally significant at a one-tailed p-value of 0.09; and the RETAIL indicator variable has a coefficient of -0.0213 with a one-tailed p-value of 0.10. Thus, our results (or lack thereof) do not appear to be due to the method of computing the abnormal returns.

³⁵ We thank Joe Bankman for his mention of this issue, which he refers to as the “giveback hypothesis.”

book the tax savings (increasing after-tax earnings) in the year the shelter is utilized and firms with relatively good governance do not book the tax savings until the tax year has passed through IRS audit. While we have no evidence that this is the case and cannot test it reliably (because we do not have data on if or when the tax savings were recorded for all the firms in our sample) it is a potential correlation that could affect our tests of governance. In the presence of such a correlation, the more negative reaction observed for the more poorly governed firms in our results above could, in part, be attributable to the firm having in effect overstated prior earnings due to claimed tax savings that the firm cannot realize.

Finally, we recognize that our results are not necessarily generalizable to any other firm shelter. For example, the market's perception of tax aggressiveness may change over time so shelters in the future may receive a more or less severe reaction than we document in this study. The changing landscape in recent years, including the enactment and implementation of the Sarbanes-Oxley Act, the issuance of the Financial Accounting Standards Board Interpretation Number 48 (which provides guidance on the accounting for tax contingencies), the new M-3 disclosure rules, and the required disclosures of listed transactions will likely change both whether firms engage in tax shelters and how the market reacts to news that firms that have engaged in them.

7. The Stock Market Response to the CTJ Reports

As stated above, one challenge of studying news about companies' involvement in tax shelters is that it is difficult to disentangle the reputation effect of the firm being tax aggressive from the market effect of the potential future costs of losing the shelter and incurring legal costs. To help disentangle these distinct mechanisms of response, we turn next to analyze the market reaction to a different kind of tax news: the release of studies by Citizens for Tax Justice in 1984 (released on October 5, 1984), 2000 (released on October 19, 2000), and 2004 (released on September 22, 2004) that featured calculations of the current effective tax rates of over 200 firms. These reports do not

link any of the firms to illegal activity, and thus should not have the confounding effects of expected legal costs or expected loss from not being able to use their tax avoidance methods in the future.

7.1. The CTJ Studies

The Citizens for Tax Justice has released three major studies of corporate current effective tax rates – one in 1984 that covered 250 of the largest U.S. corporations over the years 1981-1983; one in 2000 that covered 250 of the largest U.S. corporations over the years 1996-1998; and one in 2004 that covered 275 of the largest U.S. corporations in the years 2001-2003. Each of these studies highlights the fact that many large, profitable, U.S. companies pay very little tax. Certainly the headlines in news stories after the studies were released stressed this angle: “128 Big Firms Paid No Federal Income Taxes,” (Los Angeles Herald Examiner, October 6, 1984) and “41 Big Firms Pay No Tax, Study Says,” (Reuters News Service, October 21, 2000). Indeed, the 2004 CTJ study begins as follows:

Following the tragic events of September 11, 2001, Americans joined together in grief and solidarity to support each other and our country. Corporate America also rallied to a cause, but it wasn't in support of our nation. On the contrary, it was tax avoidance.

If ever companies are accused of being poor corporate citizens with regard to their taxpaying behavior, it is in these studies. Thus, we use the CTJ studies as a setting to examine the “pure” reputation effects of news about being an aggressive tax avoider, not news related to being accused of wrongdoing by the IRS or the courts. Therefore the market reaction should be free of the potential loss of tax benefits or any expectation of future legal costs.

The CTJ studies include the largest U.S. firms that have positive accounting income (as adjusted by the CTJ) and have the data sufficient for the CTJ to compute the CETRs and

adjustments in each of the three years included in the study.³⁶ The CTJ then restricts the sample, generally based on size, but also in a fashion that maintains a representative sample of companies in each industry and a sample with between 250 and 275 firms.

Each CTJ study computes firm CETRs and then groups firms based on these computed CETRs. The CETRs the CTJ computes are based on the U.S. current tax expense divided by U.S. pre-tax earnings after some adjustments by the CTJ (discussed below). We conduct similar event study tests to those described above for our shelter sample, but this time using the groupings provided by the CTJ. The event-window returns are computed using the same methodologies as described above. As before, we center the CTJ studies on the day of the press coverage which is the day after the study is released. Thus our window includes the day the study is released, the first day it is mentioned in the press, and one day following the press mention. We locate the firms using the company name as listed in the CTJ study to match them into the Compustat/CRSP merged database, and use the CRSP historical data to locate firms that have changed names since the time of the study. After obtaining as many firms as possible, we have a sample of 199 firms that were listed in the 1984 study, 214 firms from the 2000 study, and 243 of the firms from the 2004 study.

In addition to computing the event-window returns for the firms listed as low tax payers in these studies, we investigate whether there is a differential market reaction based on the CETR disclosed in the financial statements relative to what the CTJ calculated. If there is a market response associated with being tax aggressive, we should observe it for the firms where the CTJ rate is much lower than the disclosed CETR in the firm's financial statements. We also investigate the effect of governance (as measured by the Gompers et al., 2003 governance score as above) on the market response to the firms being labeled as low tax payers by the CTJ.³⁷ If poorly governed firms

³⁶ For full detail on the calculations and adjustments made by the CTJ, see their studies on their web page www.ctj.org. Note that the 1984 study is no longer available on the web site, but will be provided by the CTJ if requested.

³⁷ Note, however, that the Gompers et al. (2003) governance score is not available in the 1980s. In our tests, we use the governance score for the firm from the nearest year to the year of the CTJ study. Thus, to the extent that the governance

are more suspect of cheating everyone when it becomes known that they are low tax payers, then we expect poorly governed firms to have a more negative market response than well governed firms.

To the extent that the information about these companies' tax positions is already public, one might argue that the market should not react. However, although the CTJ reports are based on public financial statements, the CETR_s that the CTJ reports are not easily calculated from the firms' financial statements. For example, the CTJ generally adds back expenses that are likely non-deductible for tax purposes (such as in-process research and development). In addition, the CTJ reduces the computed tax amount for a firm by the CTJ's computation of the firm's deductions related to stock option exercises.³⁸ Because the CTJ makes these adjustments (some of which require some judgment on their part) the CETR_s the CTJ uses are not easily available to the market.

7.2. Market Reaction by Firm Groupings

Table 5 provides the mean CARs and other statistics for each grouping of firms provided by the CTJ. In the 1984 study there were three major groupings of firms in the study consisting of 1) firms that, according to their calculations, paid no federal income taxes or received rebates in at least one of the three years of the study, 2) firms that paid less than the average individual taxpayer over the same years (less than 12%), and 3) companies called "High Tax Companies," that paid an average tax rate of 40% or more.³⁹ The 2000 and 2004 CTJ studies break out similar, but not identical, groupings. All three studies have a category for companies that paid no tax in at least one

at the firm changed substantially from the 1980s to the 1990s, the score we use for the 1984 study may not be an accurate measure of the firm's governance at the time the CTJ study was released.

³⁸ See Hanlon and Shevlin (2002) for more details on the accounting for the tax benefits of stock options.

³⁹ An interesting story about this set of firms is that one of the heads of the CTJ revealed to us that soon after the first CTJ study came out, one company labeled as a "high tax payer" in the study decided to set up a leasing subsidiary to cut its taxes (a la GE). The company's tax person had called the CTJ when the study came out to find out why they paid so much in taxes compared to GE. He called the CTJ back some months later to brag about how he'd cut their tax rate through leasing ("...did you see our latest annual report? Our effective tax rate is down by 6 points!").

of the previous three years and it is this group of companies that we focus on in the regression analyses below. We label this group NOTAX1 and examine these firms separately.

Table 5 reveals that the stock market reactions to the release of the three CTJ reports are generally insignificant or slightly negative. The NOTAX1 groups of companies had a cumulative abnormal return with a negative sign in response to two of the studies but in only one study (the year 2000) was the reaction significantly different from zero. None of the other average market reactions are significant in either direction. Thus, the “pure” reputation effects of being accused of being a low tax payer appear to be slightly negative, but on average very small.⁴⁰

7.3. Regression Analysis

In our regression analysis we examine the abnormal returns of those firms singled out as low taxpayers in the event window surrounding the report releases. We investigate some of the same explanatory variables investigated earlier. One additional explanatory variable is the difference between the CTJ measure and the disclosed CETR of the firm. We use U.S. current tax expense (data #63) divided by U.S. pre-tax income (data #272). If the firm does not break down domestic and foreign earnings and taxes, we assume all are domestic. We label this measure USCETR (this is the figure most analogous to the CTJ CETR). We then compute the difference between our computed CETR measure and the CTJ CETR measure to obtain a variable that should proxy for the difference between the investor expectations and the CTJ disclosures. We label this variable DIFF-USCETR.

⁴⁰ We conclude that there is little, if any, overall pure reputation effect to being labeled tax aggressive, although the reputation effect may be present for retail firms more than other firms. For example, when we examine firms that were mentioned in the press but with no accusation of wrongdoing (the PRESS ONLY sub-sample), we find a CAR with a positive sign (but which is insignificant). Further, there is no consistent reaction to the firms listed in the CTJ study as having not paid any tax over some period of time. We do, however, find a negative coefficient on the RETAIL indicator in Table 4 (but as stated above we cannot assert with standard levels of confidence that this is not due to chance). We conjecture that because retail firms can be more easily punished by consumers, this may cause these firms to have a greater reputation effect relative to other firms.

The results are displayed in the three panels of Table 7. We focus our attention on the columns (2) and (5) results, in which the basic explanatory variables are included without interaction terms. The first thing we note is that there are no significant results that are consistent across all three years. As we expected, the estimated coefficient on the RETAIL dummy variable is negative in all three years, but is only significantly different from zero in 1984. The estimated coefficient on the indicator for poor governance is positive in 1984 and 2000, but is negative in 2004. That this variable no longer “works” the way our prior reasoning suggest should not be surprising, though, because being categorized as a low-tax payer by Citizens for Tax Justice arguably does not raise the same kind of suspicions regarding ubiquitous double dealing as involvement in a tax shelter case might. In 1984 and 2000 our measure of the tax rate surprise attracts a negative coefficient, but is only significant in 1984. The data do not tell a consistent story about what a surprisingly low CTJ-measured tax rate implies about the value of a firm.

8. Conclusions

We study the stock price reaction to news about tax aggressiveness. We find that, on average a company’s stock price declines when there is news about its involvement in tax shelters. We find some limited evidence of cross-sectional variation in the returns. For example, the stock price decline is smaller for firms that have good governance, which is consistent with the idea that for these firms the news is less likely to trigger concern about insiders’ aggressiveness toward the investors themselves. The reaction is more negative for firms in the retail sector, suggesting that part of the reaction may be a consumer/taxpayer backlash. We also test whether the reaction varies with the market’s perception of how tax-aggressive the firm is, using the firm’s current effective tax rate as a proxy for the market’s beliefs and find mixed results.

We also explore the stock price reaction to reports of current effective tax rate calculations released by Citizens for Tax Justice, hypothesizing that these reports signal tax aggressiveness

without the implications for tax penalties or illegal behavior that tax shelter news carries, and is therefore a pure reputation effect. We find no statistically significant stock price reaction to the reports, suggesting that the negative reaction to tax shelter news is not predominantly a reputation effect.

All in all, our analysis suggests that tax shelter news is viewed as a negative event by the market, although the stock price reaction is much smaller than the reaction to major accounting mishaps.

Appendix A

Our sample consists of nine major types of tax shelters. We briefly describe each type of shelter below.

The first shelter involves the use of corporate owned life insurance (aka COLI). This shelter involves the purchase of life insurance policies on many employees of the company -- not just key-man policies but policies on even very low level employees (e.g., checkers in grocery stores and Wal-Mart). The firm then immediately borrows against these policies deducting the interest on these loans (which are usually loans from the life insurance company). The firm then retains the proceeds in the event of the employee's death even if that person no longer works for the company. Many times the employee did not know the insurance policy existed. In our sample of 108 observations there are 32 observations accused of this type of shelter.

The second type of tax shelter in our sample is one where the firm transfers its intangible assets (e.g., brand name) to a corporation organized in a state that does not have an income tax. The firm (more specifically its various entities for tax purposes) pay the new corporation for the use of the intangible. The firm then deducts these payments on its returns in taxable states reducing income subject to state tax. The new entity reports the income but because it does not have Nexus in a state with an income tax there is no state income tax due on this income, thus providing an income tax shelter from state taxes. Toys 'R Us is the most widely known example of this type of shelter -- they transferred Geoffrey the Giraffe as the intangible asset. In our sample of 108 observations, there are 16 observations accused of this type of shelter.

The third type of shelter in our sample is known as the Step-Down Preferred Stock tax shelter (or Fast-Pay Preferred). Under this shelter, preferred stock issues were sold to tax-exempt investors such as pension funds. This is a complex vehicle that in essence allows a company to issue equity and deduct interest and principal payments. The company engaging in the shelter creates a real estate investment trust (REIT) in partnership with a pension fund (which is tax

exempt). The company provides the real estate and receives common stock, the pension fund provides cash and receives preferred stock. The REIT then lends the company funds secured by its real estate holdings. When the company repays the loan, the cash goes to pay the dividends and some of the principal on the pension fund's preferred stock. Because the company is repaying a loan, both the dividend and principal payments are deductible as interest. In our sample of 108 observations, there are 12 observations accused of this type of shelter.

The fourth type of shelter in our sample is known as the Lease in Lease Out tax shelter (and the Sale in Lease Out) tax shelter (otherwise known as LILO or SILO). Although there are several methods by which this can be arranged, the concept behind this shelter is for a tax-exempt municipality to 'sell' depreciation tax deductions to a U.S. corporation. Generally, a U.S. corporation will lease or buy a real asset such as a power plant, a stadium, a subway system, etc. from a tax-exempt municipality (U.S. or foreign) and then sublease the property back to the same tax-exempt party. The corporation will generally prepay its lease obligation (if a LILO). The U.S. company engaging in the shelter then obtains the accelerated depreciation deductions on its tax return (but expenses the depreciation more slowly and over a longer time period for financial accounting purposes or only records rental expense for financial accounting purposes). In our sample of 108 observations, 9 observations are accused of this type of shelter.

The fifth type of shelter in our sample is known as the Contested Liability Acceleration Strategy tax shelter (also known as CLAS). This shelter was devised and marketed by KPMG and the IRS has said that it generated at least \$1.7 billion in tax savings for more than two dozen companies.⁴¹ The idea behind this shelter was to accelerate the timing of tax settlements of lawsuits and other claims. Deductions generally are not allowed for such liabilities until the claimants are paid but one exception under the tax code involves the transfer of money and/or other property to a contested liability trust before the claims are resolved. Thus, a company engaging in this shelter

⁴¹ See *The Wall Street Journal*, June 16, 2004, "KPMG Shelter Shaved \$1.7 Billion Off Taxes of 29 Large Companies."

strategy would establish a trust with itself as the beneficiary and then transfer noncash assets to the trust. The value of these items transferred (generally stock or an intercompany note) are to represent the amount the company is expected to pay to resolve the claims it is still contesting. By moving these noncash items to the trust, the company can take the deductions earlier than otherwise allowed, reducing taxable income. In our sample of 108 observations, 8 observations were accused of this type of shelter.

Another type of shelter in our sample is known as the Contingent Payment Installment Sale tax shelter. Generally, the goal of this shelter is to produce paper capital losses that can be used against real capital gains that the U.S. corporation has generated from some other source. Again, there are alternative methods to employ this strategy but a typical deal may work as follows. Three parties form a partnership to acquire a fixed-income security that does not trade publicly. The U.S. corporation seeking to engage in the tax shelter will put up approximately 9% of the funds, a tax-exempt (usually a foreign) entity will fund 90%, and another third party financier will put up the final 1%. The partnership will then sell the security in an installment sale, receiving most of the proceeds immediately and the remainder (contingent on the security's value at that time) in the third year. The partnership will show a large (paper) gain in the first year, most of which will be allocable to the tax-exempt party. Then, in the second year of the partnership's life, the tax-exempt will leave the partnership being bought out by the U.S. corporation. The partnership will then show (paper) losses in the second and third years, most of which will be allocable to the U.S. tax sheltering corporation, thus offsetting the capital gain the company generated from other sources. In our sample of 108 observations, 8 observations are accused of being involved in this type of shelter.

The seventh type of tax shelter in our sample is known as the Regulated Investment Trust tax shelter. This strategy was established to take advantage of a federal law that governs mutual funds. The U.S. company (usually a bank) would register a subsidiary with the SEC as a "regulated

investment company” under the Investment Company Act of 1940. The banks then transferred some of their loan portfolios and other assets into the funds and used the interest and other income they generated to pay themselves dividends. Most of the tax benefits were state tax benefits in states such as California which exempts money transferred between subsidiaries and corporate parents. Although the Wall Street Journal reported that, “...one bank fund, NBT Investment Co., said in an SEC filing that it was optimistic that its structure ‘will be sufficient to relieve it from all or substantially all federal and state income taxes’” (Simpson, 2003). In our sample of 108 observations, 6 observations are related to this type of tax shelter.

Another type of tax shelter is the Offshore Intellectual Property Haven tax shelter. Our sample of 108 observations only includes 5 observations related to this shelter. Under this strategy, firms transfer patents on intellectual property to a subsidiary established in a tax haven (generally somewhere like Bermuda or other low tax jurisdictions). Then royalties from sales of the products made outside the U.S. flow to the Bermuda subsidiary and can stay there tax-free until the U.S. parent company decides to repatriate these funds back to the U.S. at which time the repatriated earnings would be taxed.

OTHER is a collection of different tax shelters for which there are very few (usually only 1) observations. This grouping includes the Cross Border Dividend Capture (e.g., Compaq’s tax shelter involving ADRs), tax shelters involving sham partnerships, Money Market Principal Strips, subsidiary sales that generated double losses or were labeled reorganizations in order to obtain intracompany dividend treatment, and others. In our sample of 108 observations, 12 observations are included in this grouping.

References

- Alexander, Cindy R. and Mark A. Cohen. 1999. "Why Do Corporations Become Criminals? Ownership, Hidden Actions, and Crime as an Agency Cost", *Journal of Corporate Finance*, 5(1): 1-34
- Alexander, Cindy R. and Mark A. Cohen. 1996. "New Evidence on the Origins of Corporate Crime." *Managerial and Decision Economics* 17: 421-435.
- Bankman, J. 2004. "An Academic's View of the Tax Shelter Battle." In *The Crisis in Tax Administration*, edited by H.J. Aaron and J. Slemrod. Brookings Institution, Washington D.C.
- Bernard, V. 1987. "Cross-Sectional Dependence and Problems in Inference in Market-Based Accounting Research." *Journal of Accounting Research* (25): 1-48. .
- Birnbaum, Jeffrey H. and Alan S. Murray. 1987. *Showdown at Gucci Gulch: Lawmakers, Lobbyists, and the Unlikely Triumph of Tax Reform*. Random House, Inc. New York, New York.
- Boise, C. and J. Koenig. 2002. "Practical and Policy Considerations in Corporate Inversion Transactions." *Corporate Business Taxation Monthly* 3 (12): 3-20.
- Bosch, Jean-Claude and E. Woodrow Eckard, Jr. 1991. "The Profitability of Price Fixing: Evidence from Stock Market Reaction to Federal Indictments." *The Review of Economics and Statistics*: 309-17.
- Boucher, K. 2000. "Current Corporate Income Tax Developments (part I)." *Tax Adviser* (March 1): 164-172.
- Brown, S. and J. Warner. 1980. "Measuring Security Price Performance." *Journal of Financial Economics* (8): 205-258.
- Brown, S. and J. Warner. 1985. "Using Daily Stock Returns: The Case of Event Studies." *The Journal of Financial Economics* (14): 3-31.
- Browning, E.S., 1999. "Stocks Fall As Investors Take Profits." *The Wall Street Journal*. November, 24: C1.
- Cloyd, C. B., L. Mills, and C. Weaver. 2002. "Firm Valuation Effects of the Expatriation of U.S. Corporations to Tax Haven Countries." Working paper, University of Arizona.
- Collins, D. and W. Dent. 1984. "A Comparison of Alternative Testing Methodologies Used in Capital Market Research." *Journal of Accounting Research* (22): 48-84.
- Dechow, P, R. Sloan and A. Sweeney. 1996. "Causes and Consequences of Earnings Manipulations: An Analysis of Firms Subject to Enforcement Actions by the SEC." *Contemporary Accounting Research* 13(1): 1-36.

- Desai, M. 2006. "Testimony Before the Subcommittee on Select Revenue Measures of the House Committee on Ways and Means" May 9, 2006.
- Desai, M. and D. Dharmapala. 2005. "Corporate Tax Avoidance and Firm Value." NBER Working Paper No. 11241.
- Desai, M. A., A. Dyck, and L. Zingales. 2004. "Theft and Taxation." NBER Working Paper No. 10978.
- Desai, M.A. and J. Hines. 2002. "Expectations and Expatriations: Tracing the Causes and Consequences of Corporate Inversions." *The National Tax Journal*, September (55): 409-440.
- Ellert, James C. 1975. *Antitrust Enforcement and Stockholder Returns*. Unpublished doctoral dissertation, University of Chicago, Graduate School of Business.
- Federal Filings Newswire. 2000. "Holder Registers Crown Cork and Seal Co., Inc. Stock. February 29.
- France, M. with E. Thorton, H. Timmons, and L. Lavelle. 2003. "The Rise of Wall Street Tax Machine: The Shelter Game Has Been Hidden and Lucrative. Could it Be the Next Big Thing?" *Business Week* (March 31): 84.
- Garbade, Kenneth D., William L. Silber, and Lawrence J. White. 1982. "Market Reaction to the Filing of Antitrust Suits: An Aggregate and Cross-Section Analysis." *The Review of Economics and Statistics* 64 (November): 686-91.
- Gleason, C. and L. Mills. 2002. "Materiality and Contingent Tax Liability Reporting." *The Accounting Review*, 77 (2): 317-342.
- Gompers, P. A., J. Ishii, and A. Metrick. 2003. "Corporate Governance and Equity Prices." *Quarterly Journal of Economics*, 118: 107-155.
- Graham, John R. and Alan L. Tucker. 2005. "Tax Shelters and Corporate Debt Policy." Forthcoming, *Journal of Financial Economics*.
- Hanlon, M. and T. Shevlin. 2002. "Accounting for Tax Benefits of Employee Stock Options and Implications for Research." *Accounting Horizons*, 16(1): 1-16.
- Hershey, R. 1997. "IRS Closes Loophole that Let Firms Borrow Money Cheaply." *The New Orleans Time-Picayune*, February, 28.
- Iyer, Savita. 2002. "BoFA Launches \$1.25B AT&T Loan." Bank Loan Report, March 4, 2002.
- Karpoff, J. and L. J. Lott. 1993. "The Reputational Penalty Firms Bear for Committing Fraud," *Journal of Law and Economics*, 36(2), October: 757-803.

- Karpoff, J., J. Lott, and G. Rankine. 1999. "Environmental Violations, Legal Penalties, and Reputation Costs. Working paper, University of Chicago Law School.
- Klein, Benjamin and Keith B. Leffler. 1981. "The Role of Market Forces in Assuring Contractual Performance." *Journal of Political Economy*. 89 August: 615-641.
- Kothari, S.P. and J. Warner. 2004. "Econometrics of Event Studies." Working paper, final version to appear in the Handbook of Corporate Finance: Empirical Corporate Finance, edited by B. Espen Eckbo (Elsevier/North-Holland).
- Los Angeles Herald Examiner. 1984. "128 Big Firms Paid No Federal Income Taxes." October, 6.
- Martinez, B. 2004. "Merck Says It May Owe the IRS Up to \$2.04 Billion on Deductions." *The Wall Street Journal*, May 10.
- Maksimovic, Vojislav and Sheridan Titman. 1991. "Financial Policy and Reputation for Product Quality." *Review of Financial Studies*, 4(4): 175-200.
- McKinnon, J. D. 2000. "Treasury Seems to Ease Curbs on Tax Shelters." *The Wall Street Journal* (March 4): C1.
- Murray, Alan. 2002. "Inflated Profits in Corporate Books is Half the Story." *The Wall Street Journal* (July 2): A4.
- Novack, J. and L. Saunders. 1998. "The Hustling of X Rated Tax Shelters: Respectable Tax Professionals and Respectable Corporate Clients are Exploiting the Exotica of Modern Corporate Finance to Indulge in Extravagant Tax-Dodging Schemes." *Forbes*, (December 14): 198.
- Nyhan, P. 1997. "IRS Puts Stop on a REIT Tax Shelter." *The Journal Record*, Bloomberg News, February 28, 1997.
- Padgett, T. 1997. "IRS' Scuttling of Security Could Affect Some Banks Series." *American Banker*, March 5: 26.
- Palmrose, Z, V. Richardson, and S. Scholz. 2004. "Determinants of Market Reactions to Restatement Announcements." *Journal of Accounting and Economics* (37): 59-89.
- Patell, J. (1976). "Corporate Forecasts of Earnings per Share and Stock Price Behavior: Empirical Tests." *Journal of Accounting Research* (14): 246-276.
- PR Newswire. 2005. "Freddie Mac Announces Resolution to REIT Dividend Tax Issue." May 12, 2005.
- Reuters News Service. 2000. "41 big Firms Pay No Tax, Study Says." October 21.
- Shackelford, D. 2006. "Testimony Before the Subcommittee on Select Revenue Measures of the House Committee on Ways and Means" May 9, 2006.

Seida, J.A. and W.F.Wempe. 2004. "Investors' and Managers' Reactions to Corporate Inversions Transactions." Working paper, University of Notre Dame.

Simpson, G. 2003. "Banks Shifted Billions Into Funds Sheltering Income from Taxes." *The Wall Street Journal* (August 7): A1.

Strachan, James L., David B. Smith, and William L. Beedles. 1983. "The Price Reaction to (Alleged) Corporate Crime." *The Financial Review* 18 (May): 121-132.

The Express. 2002. "Wall Street." March 5, 2002.

U.S. Department of the Treasury. 1999. "The Problem with Corporate Tax Shelters: Discussion, Analysis, and Legislative Proposals." July. Washington, D.C.: Government Printing Office.

Wall Street Journal A News Roundup. 1999. "Murdoch Says GE Offered to Sell NBC to Time Warner." November 24, 1999.

Weisman, Jonathan. 2004. "Liberal Group Decries Corporate Tax Slide." *The Washington Post*, (September 23): E02.

Welch, W. 1999. "Tax Bill Fine Print Brings Good Thing to GE Subsidiary - Could Save Up to \$60M." *USA Today*, November 24.

Table 1
Sample Selection

	Articles	Firms
Search of Factiva 1/1/1990 - 9/1/2004 number of articles	6,293	
Potentially valid articles summarized by RAs	824	285
Less:		
Articles about inversions	39	14
Articles about transfer pricing	16	13
Articles about foreign sales corporations (FSCs)	12	7
Articles about Petroleum Reserve Tax	33	27
Articles about Agricultural Land Tax Break	6	6
Articles about private companies or non-income tax "shelter" issues ¹	117	85
	601	135 *
Less Observations:		
Subsequent articles about the same firm-shelter	366	
Observations with missing data	45	
Sales Leasebacks	6	
Vague mention of shelter activity (e.g., Enronesq)	10	
Other news in the same article	3	
City National/National City	3	
MIPS/Feline Pride	3	
Low Income Housing Credit	2	
Tax Credit Transfer	1	
Synthetic Fuels	1	
U.S. Division of Foreign Company	2	
Aircraft Lease Bonds	1	
Political after Heinz when Kerry running	1	
Transfer pricing/lobbying/foreign	2	
Enron -- too much other news	3	
WorldCom -- data not accessible	2	
Remaining observations for final sample-some have multiple articles though if first one is not major press	150	99
Less:		
Duplicate observations - retain only the very first article per firm	42	
Number of Observations -- First Press Article, All Shelters	108	97

Notes to Table 1:

¹ Articles about issues such as foreign tax acts (e.g. Stamp Duty Reserve Tax, property tax, tracking stock tax issues, reinsurance, Hong Kong Aircraft Leasing, Low Income Housing Credits. In addition, articles about the sellers of the tax shelters (e.g., Merrill Lynch) are excluded.

* Firms may have multiple shelters so the firm reduction column will not add up.

Table 2
Descriptive Statistics – Tax Shelter Sample

Panel A: All Firm-Shelters

	N	Mean	S.D.	Minimum	Median	Maximum
CAR	108	-0.004	0.037	-0.130	-0.004	0.203
CETR	108	0.310	0.124	0	0.326	0.5
LOSS	108	0.065	0.247	0	0	1
GOV SCORE	108	9.083	3.860	0	10	15
POOR GOV	108	0.556	0.499	0	1	1
NO GOV	108	0.083	0.278	0	0	1
RETAIL	108	0.074	0.263	0	0	1
BANK	108	0.259	0.440	0	0	1

Panel B: All Firm Shelters, Major News Source Only

	N	Mean	S.D.	Minimum	Median	Maximum
CAR	70	-0.012	0.033	-0.130	-0.008	0.060
CETR	70	0.298	0.128	0	0.318	0.5
LOSS	70	0.086	0.282	0	0	1
GOV SCORE	70	8.871	3.837	0	10	15
POOR GOV	70	0.514	0.503	0	1	1
NO GOV	70	0.086	0.282	0	0	1
RETAIL	70	0.071	0.259	0	0	1
BANK	70	0.229	0.423	0	0	1

Notes to Table 2: CETR is computed by taking the sum of the current worldwide tax expense over two years prior to the news of the shelter and dividing by the sum of worldwide pre-tax book income over the two years prior to the news of the shelter. If the denominator of this ratio is a loss we reset the CETR variable to zero. If the data are not available for two years prior to news of the shelter, we use only the prior year data to compute the CETR. We use the ratio of the sum and not the average of the ratios in order to minimize the number of loss observations and the effect of extreme one-year CETRs. Deferred taxes are not available on Compustat for banks (which means current tax expense is not calculatable) so we replace the numerator for banks with total tax expense and then create a separate indicator variable for banks (BANK) set equal to 1 for firms where this substitution has been made and zero otherwise. CETR is reset to zero for a minimum, and .5 for a maximum to eliminate the effect of outliers. LOSS is an indicator set to 1 for firms with negative pre-tax book income in the calculation of CETR, and zero otherwise. GOV SCORE is the continuous governance score from Gompers et al. (2003) as available on Compustat. NO GOV is an indicator variable set to one for firms where the GOV SCORE is not available. POOR GOV is an indicator variable set equal to 1 for firms with a GOV SCORE above the sample median which is indicative of relatively poor governance, and zero otherwise. RETAIL is an indicator variable set equal to 1 for firms in the retail industry and zero otherwise. The sample of all firm shelters (the 108 observations) includes the news of shelter involvement in all news sources available on Factiva and the sample of the major news source only (the 70 observations) includes only news in *The Wall Street Journal*, *The Washington Post*, or the major newswires (AP Online, Dow Jones News Wire, or Reuters).

Table 3
Cumulative Abnormal Returns
Tax Shelter Sample

Panel A: Cumulative Abnormal Returns Presented by Sub-Sample

	# obs	Mean CAR	Pos/Neg	t	t _{patell}	t _{cda}	t _{var}	Z _{sign}
Sample 1: All firm-shelters, first press mention	108	-0.44	47/61	-1.240	-1.296	-1.176	-1.33	-1.19
Sample 2: All firm-shelters, major press mention only	70	-1.16	27/43	-2.982**	-2.627**	-2.456**	-2.648**	-1.804*
Sample 3: First firm-shelter, first press mention	97	-0.34	41/56	-0.910	-1.021	-0.861	-1.05	-1.326
Sample 4: First firm-shelter, major press mention only	63	-1.04	24/39	-2.480**	-2.214**	-2.033**	-2.268**	-1.739*

Panel B: Cumulative Abnormal Returns for the First Press Mention Sub-Sample by Governance Group

	# obs	Mean CAR	Pos/Neg	t	t _{patell}	t _{cda}	t _{var}	Z _{sign}
Well Governed	39	-0.20	18/21	-0.460	-0.212	-0.338	-0.236	-0.361
Poorly Governed	60	-0.86	25/35	-0.541	-1.389*	-1.888**	-1.380*	-1.221*
No Governance Index	9	1.35	4/5	1.957*	0.461	0.729	0.404	-0.219
Total Sample	108							

Panel C: Cumulative Abnormal Returns for the First-Press Mention Sub-Sample – Above Median CETR and Below Median CETR

	# obs	Mean CAR	Pos/Neg	t	t _{patell}	t _{cda}	t _{var}	Z _{sign}
CETR above median	53	-0.08	26/27	-0.133	-0.085	-1.162	-0.077	-0.144
CETR below median	48	-1.07	18/30	-2.624**	-2.265**	-2.680***	-2.691***	-1.609*
No CETR (loss in denominator - LOSS firms)	7	1.17	3/4	1.154	0.609	0.395	1.158	-0.068
Total	108							

Panel D: Cumulative Abnormal Returns for the First-Press Mention Sub-Sample – Retail Industry compared to Non-retail industries

	# obs	Mean CAR	Pos/Neg	t	t _{patell}	t _{cda}	t _{var}	Z _{sign}
Retail	8	-1.27	3/5	-1.177	-0.763	-0.938	-0.898	-0.57
Non-Retail	100	-0.37	44/56	-0.997	-1.131	-0.946	-1.147	-1.076
Total	108							

Table 3 (continued)
Cumulative Abnormal Returns
Tax Shelter Sample

Panel E: Cumulative Abnormal Returns for the First Press Mention Sub-Sample by Event Group

	# obs	Mean CAR	Pos:Neg	t	t _{patell}	t _{cda}	t _{var}	Z _{sign}
PRESS ONLY	21	0.44	10/11	0.915	0.530	0.537	0.554	-0.039
EXAMINATION	57	-0.62	23/34	-1.140	*-1.426	-1.209	-1.579*	*-1.348
SHUTDOWN	10	1.56	7/3	2.845*	1.871*	1.795*	2.673**	1.357
EARNWARN	5	-1.25	1/4	-0.780	-1.224	-0.821	-1.260	-1.397
COURT	6	-0.24	5/1	-0.139	-0.102	-0.221	-0.080	*1.561
LOST	6	-2.76	1/5	-1.45	-1.819*	-1.004	-1.668*	-1.552
WON	3	-4.33	0/3	-2.449	-2.371*	-2.528**	-4.386**	-1.852*
Total sample	108							

Notes to Table 3: CARs are the cumulative abnormal returns calculated over a three day window centered on the date of the news and calculated using market adjusted returns (i.e., taking the raw return for the firm less the CRSP value weighted index return for the same day). We classify firms as well governed if their GOV SCORE is below the sample median and poorly governed if the GOV SCORE is above the sample median. We classify firms as RETAIL if they are in the retail sector (NAICS codes 44-45). We classify firms as having an above median CETR if their average current CETR ((data#16-data#50)/data#170) over the two years prior to the news is above the sample median of this measure. If the firm has negative total pre-tax book income (data #170) over the two years we code the firm as having No CETR (LOSS firm). We classify firms by event groups in Panel E based on the events described in Section 6.1 of the text. Significance levels are as follows: *** indicates significance at .01, ** at .05, and * at .10, two-tailed except for the governance groups which are calculated using one-tailed tests because we have a sign prediction.

Table 4
Regression Analysis

Panel A: All Firm-Shelters

	Pred. sign	1	2	3	4
Constant	+/-	-0.0353 [0.029]	-0.002 [0.644]	-0.0369 [0.049]	-0.0404 [0.031]
CETR	+	0.0898 [0.041]		0.0986 [0.036]	0.1079 [0.030]
LOSS	+/-	0.0469 [0.013]		0.0486 [0.013]	0.0523 [0.009]
NO GOV	+/-		0.0155 [0.523]	0.0206 [0.420]	0.0243 [0.348]
POOR GOV	-		-0.0066 [0.149]	-0.0052 [0.199]	-0.0065 [0.152]
RETAIL	-				-0.0171 [0.126]
BANK	+/-				0.0077 [0.196]
Observations		108	108	108	108
R-squared		0.063	0.028	0.099	0.124

Panel B: Only Major Press Sources

	Pred. sign	1	2	3	4
Constant	+/-	-0.0197 [0.204]	-0.0055 [0.340]	-0.0116 [0.412]	-0.0198 [0.175]
CETR	+	0.019 [0.339]		0.0133 [0.380]	0.0286 [0.267]
LOSS	+/-	0.0282 [0.136]		0.0264 [0.139]	0.0335 [0.074]
NO GOV	+/-		-0.0026 [0.830]	-0.0037 [0.769]	0.0027 [0.820]
POOR GOV	-		-0.0115 [0.088]	-0.0115 [0.081]	-0.0114 [0.081]
RETAIL	-				-0.0159 [0.095]
BANK	+/-				0.0156 [0.050]
Observations		70	70	70	70
R-squared		0.037	0.028	0.064	0.121

Notes to Table 4: P-values are in parentheses and are one-tailed where there is a prediction, two-tailed otherwise. The sample and the variables are as defined in Tables 2 and 3.

Table 5
Cumulative Abnormal Returns
Citizens for Tax Justice Samples

Panel A: CTJ 1984 Study

<i>Market adjusted returns</i>	# obs	%		t	t _{patell}	t _{cda}	t _{var}	Z _{sign}
		Mean CAR	Pos/Neg					
Total observable sample	199	0.11	109/90	0.69	0.905	0.513	1.067	1.45
128 of the 250 paid no federal income taxes in at least one of three years	97	0.25	56/41	1.18	1.156	0.708	1.480	1.645
129 of the 250 paid less than the average American family	103	0.27	58/45	1.26	1.089	0.794	1.352	1.407
High tax companies - average tax of over 40% in three years	10	-0.60	5/5	-0.47	-0.400	-0.615	-0.320	-0.025
In press for low tax - specifically mentioned	13	0.52	8/5	0.93	0.531	0.558	0.775	0.978

Panel B: CTJ 2000 Study

<i>Market adjusted returns</i>	# obs	%		t	t _{patell}	t _{cda}	t _{var}	Z _{sign}
		Mean CAR	Pos/Neg					
Total observable sample	214	0.03	96/118	0.06	1.08	0.033	0.852	-0.802
41 Companies that paid less than zero tax in one of last three years	31	-1.82	12/19	-2.00*	-2.237**	-1.561	-2.197**	-1.134
24 Companies that received tax rebates in 1998	19	-1.00	8/11	-0.97	-1.010	-0.722	-1.141	-0.681
133 of 250 companies paid half the statutory rate in one of three years	110	0.31	48/62	0.45	0.609	0.33	0.433	-0.821
Top 25 tax break recipients	22	0.37	11/11	0.25	0.390	0.330	0.239	0.199
In press for low tax - specifically mentioned	19	-0.80	8/11	0.51	-1.375	-0.695	-0.876	-0.612

Panel C: CTJ 2004 Study

<i>Market adjusted returns</i>	# obs	%		t	t _{patell}	t _{cda}	t _{var}	Z _{sign}
		Mean CAR	Pos/Neg					
Total observable sample	243	-0.08	121/122	-0.63	-0.246	-0.310	-0.290	0.237
82 Companies that paid no tax in one or more Bush years	65	-0.10	31/34	-0.48	-0.174	-0.278	-0.235	-0.336
28 Companies that had negative tax rate over entire period	21	-0.12	8/13	-0.38	-0.499	-0.223	-0.742	-1.086
46 Companies that paid zero tax in 2003 alone	36	-0.17	15/21	-0.70	-0.454	-0.365	-0.615	-0.967
Top 25 tax break recipients	21	-0.45	9/12	-1.57	-1.246	-1.318	-1.430	-0.664
In press for low tax - specifically mentioned	11	-0.15	6/5	-0.52	-0.536	-0.302	-0.782	0.308

Notes to Table 5: CARs for the CTJ studies are computed over the three days surrounding the news of the study (the day of the study release to the press, the day before and the day after the release to the press). The CARs are market adjusted abnormal returns and are computed in the same manner as the CARs for the shelter sample except that we use the firm's own market index return (e.g., the NASDAQ index for the NASDAQ firms, etc.) rather than the CRSP index return and subtract this from the firm's daily return to obtain the abnormal market return. Significance levels are as follows: *** indicates significance at .01, ** at .05, and * at .10, two-tailed except for the governance groups which are calculated using one-tailed tests because we have a sign prediction.

Table 6
Descriptive Statistics – CTJ Studies

Panel A: CTJ 1984 Study

	N	mean	sd	min	25%	50%	75%	max
CAR	199	0.001	0.023	-0.086	-0.010	0.001	0.014	0.074
CTJ CETR Average	199	0.108	0.200	-0.987	0.000	0.100	0.240	0.456
USCETR (US CETR)	189	0.218	0.235	-1.230	0.108	0.256	0.359	0.765
DIFF - USCETR	189	0.109	0.173	-0.391	0.016	0.068	0.164	0.729
NOTAX1	199	0.487	0.501	0.000	0.000	0.000	1.000	1.000
NO GOV	199	0.312	0.464	0.000	0.000	0.000	1.000	1.000
POOR GOV	199	0.563	0.497	0.000	0.000	1.000	1.000	1.000
GOV SCORE	137	9.934	2.613	4.000	8.000	10.000	12.000	16.000
BANK	199	0.055	0.229	0.000	0.000	0.000	0.000	1.000
RETAIL	199	0.015	0.122	0.000	0.000	0.000	0.000	1.000

Panel B: CTJ 2000 Study

	N	mean	sd	min	25%	50%	75%	max
CAR	214	0.0003	0.069	-0.367	-0.031	-0.007	0.025	0.405
CTJ CETR Average	214	0.216	0.092	-0.088	0.159	0.225	0.289	0.357
USCETR (US CETR)	196	0.274	0.131	-0.128	0.215	0.287	0.342	0.743
DIFF - USCETR	196	0.059	0.100	-0.147	0.005	0.039	0.084	0.579
NOTAX1	214	0.145	0.353	0.000	0.000	0.000	0.000	1.000
NO GOV	214	0.164	0.371	0.000	0.000	0.000	0.000	1.000
POOR GOV	214	0.542	0.499	0.000	0.000	1.000	1.000	1.000
GOV SCORE	179	10.123	2.496	3.000	9.000	10.000	12.000	16.000
BANK	214	0.131	0.338	0.000	0.000	0.000	0.000	1.000
RETAIL	214	0.047	0.212	0.000	0.000	0.000	0.000	1.000

Panel C: CTJ 2004 Study

	N	mean	sd	min	25%	50%	75%	max
CAR	243	-0.0008	0.0188	-0.1097	-0.0094	0.0000	0.0106	0.0833
CTJ CETR Average	243	0.168	0.128	-0.596	0.087	0.198	0.261	0.345
USCETR (US ETR)	227	0.231	0.183	-0.980	0.145	0.256	0.321	0.880
DIFF - USCETR	227	0.063	0.148	-0.792	0.014	0.042	0.081	0.900
NOTAX1	243	0.267	0.444	0.000	0.000	0.000	1.000	1.000
NO GOV	243	0.148	0.356	0.000	0.000	0.000	0.000	1.000
POOR GOV	243	0.506	0.501	0.000	0.000	1.000	1.000	1.000
GOV SCORE	207	10.019	2.434	3.000	8.000	10.000	11.000	16.000
BANK	243	0.165	0.372	0.000	0.000	0.000	0.000	1.000
RETAIL	243	0.049	0.217	0.000	0.000	0.000	0.000	1.000

Notes to Table 6: CAR is defined as in Table 5. CTJ CETR Average is the 3 year average CETR reported in the CTJ study. The USCETR is the U.S. current tax expense (data #16- data #50) divided by the firm's pre-tax book income (data #170) as disclosed by the firm and reported in Compustat. This is averaged over 3 years (sum of the numerator divided by the sum of the denominator) so as to be comparable to the rates computed by the CTJ. DIFF – USCETR is the difference between the computed USCETR and the rate provided in the CTJ study (CTJ CETR Average). NOTAX1 is an indicator variable set equal to 1 for firms in the first group of firms listed in Table 5 for each CTJ study and zero otherwise. More specifically, NOTAX1 is set to one if the firm paid zero or less in Federal income taxes in the three years covered by the CTJ as computed and reported by the CTJ, and zero otherwise. The remaining variables are as defined in Tables 2 and 3 for the tax shelter sample.

Table 7
Regression Analysis – CTJ Studies

Panel A: CTJ 1984 Study – NOTAX1 Firms Only

	Pred. sign	1	2	3	4	5	6
Constant	+/-	0.003 [0.248]	-0.011 [0.100]	-0.010 [0.034]	0.005 [0.089]	-0.009 [0.232]	-0.014 [0.195]
CTJ CETR Av	+/-	0.001 [0.976]	0.008 [0.664]	-0.014 [0.913]			
DIFF-USCETR	+/-				-0.018 [0.089]	-0.019 [0.063]	0.028 [0.495]
POOR GOV	+/-		0.018 [0.016]	0.017 [0.003]		0.018 [0.021]	0.024 [0.037]
NO GOV	+/-		0.013 [0.090]	0.011 [0.065]		0.014 [0.086]	0.018 [0.112]
RETAIL	+/-		-0.011 [0.001]	-0.011 [0.001]		-0.013 [0.001]	-0.013 [0.001]
BANK	+/-		-0.001 [0.899]	0.005 [0.367]		-0.001 [0.932]	-0.008 [0.836]
DIFF-USCETR*POOR GOV	+/-						-0.054 [0.209]
DIFF-USCETR*NO GOV	+/-						-0.044 [0.318]
DIFF-USCETR*BANK	+/-						0.071 [0.821]
CTJ CETR Av*POOR GOV	+/-			0.011 [0.931]			
CTJ CETR Av*NO GOV	+/-			0.023 [0.856]			
CTJ CETR Av*BANK	+/-			0.213 [0.000]			
Observations		97	97	97	89	89	89
R-squared		0.000	0.071	0.119	0.031	0.105	0.117

Table 7 (continued)
Regression Analysis – CTJ Studies

Panel B: CTJ 2000 Study – NOTAX1 Firms Only

	Pred. sign	1	2	3	4	5	6
Constant	+/-	-0.024 [0.044]	-0.047 [0.024]	-0.053 [0.084]	-0.015 [0.122]	-0.033 [0.013]	-0.031 [0.020]
CTJ CETR Av	+/-	0.081 [0.478]	0.117 [0.443]	0.184 [0.489]			
DIFF-USCETR	+/-				-0.051 [0.371]	-0.069 [0.307]	-0.131 [0.216]
POOR GOV	+/-		0.029 [0.268]	0.028 [0.412]		0.026 [0.261]	0.019 [0.467]
NO GOV	+/-		0.036 [0.164]	0.118 [0.004]		0.030 [0.197]	0.033 [0.201]
RETAIL	+/-		-0.024 [0.181]	-0.113 [0.000]		-0.025 [0.281]	-0.044 [0.018]
BANK	+/-		0.025 [0.392]	0.116 [0.056]		0.080 [0.000]	0.085 [0.000]
DIFF-USCETR*POOR GOV	+/-						0.105 [0.486]
DIFF-USCETR*NO GOV	+/-						0.033 [0.831]
DIFF-USCETR*RETAIL	+/-						0.412 [0.091]
CTJ CETR Av*POOR GOV	+/-			0.069 [0.856]			
CTJ CETR Av*NO GOV	+/-			-0.900 [0.006]			
CTJ CETR Av*RETAIL	+/-			0.978 [0.017]			
CTJ CETR Av*BANK	+/-			-0.594 [0.168]			
Observations		31	31	31	29	29	29
R-squared		0.011	0.088	0.200	0.010	0.125	0.149

Table 7 (continued)
Regression Analysis – CTJ Studies

Panel C: CTJ 2004 Study- NOTAX1 Firms Only

	Pred. sign	1	2	3	4	5	6
Constant	+/-	-0.001 [0.469]	0.003 [0.478]	0.003 [0.397]	-0.002 [0.293]	0.001 [0.799]	0.001 [0.831]
CTJ CETR Av	+/-	0.019 [0.282]	0.021 [0.264]	-0.034 [0.583]			
DIFF-USCETR	+/-				0.011 [0.203]	0.009 [0.416]	0.009 [0.588]
POOR GOV	+/-		-0.007 [0.156]	-0.008 [0.113]		-0.005 [0.362]	-0.005 [0.504]
NO GOV	+/-		-0.005 [0.388]	-0.006 [0.398]		-0.005 [0.529]	-0.004 [0.617]
RETAIL	+/-		-0.008 [0.322]	0.010 [0.017]		-0.004 [0.529]	-0.005 [0.395]
BANK	+/-		0.003 [0.442]	0.002 [0.574]		0.003 [0.548]	0.003 [0.645]
DIFF-USCETR*POOR GOV	+/-						-0.005 [0.839]
DIFF-USCETR*NO GOV	+/-						0.005 [0.780]
DIFF-USCETR*RETAIL	+/-						0.026 [0.127]
DIFF-USCETR*BANK	+/-						-0.003 [0.830]
CTJ CETR Av*POOR GOV	+/-			0.068 [0.320]			
CTJ CETR Av*NO GOV	+/-			0.067 [0.308]			
CTJ CETR Av*RETAIL	+/-			-0.065 [0.301]			
CTJ CETR Av*BANK	+/-			-0.004 [0.927]			
Observations		65	65	65	61	61	61
R-squared		0.014	0.052	0.082	0.027	0.046	0.053

Notes to Table 7: All p-values are in parentheses and are two-tailed. All variables are as defined in Table 6.