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Can Paying "Too Much" or "Too Little" Tax Contribute to Forced CEO Turnover?

Can Paying "Too Much" or "Too Little" Tax Contribute to Forced CEO Turnover?

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ABSTRACT

Our study examines the effect of corporate tax outcomes on forced CEO turnover. While prior research argues that firms often do not engage in tax avoidance due to reputational concerns, the empirical evidence suggesting the existence of reputational costs is scarce. In a broad sample of firms, we find evidence of a relation between the payment of low taxes and forced turnover. We also find that forced CEO turnover is more likely when the firm pays a high tax rate relative to its peers. Our results are consistent with the existence of previously unexplored individual reputational costs for not engaging in tax avoidance.

Keywords: CEO turnover, tax avoidance
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I. INTRODUCTION

In this paper we examine the effects of tax avoidance on forced CEO turnover. A commonly held belief in the tax literature is that reputational costs are a limiting factor in the extent to which firms and managers are willing to minimize their effective tax rates (Desai and Dharmapala 2006; Hanlon and Slemrod 2009; Chen, Huang, Li, and Stanfield 2012; Graham, Hanlon, Shevlin, and Shroff 2013). The reputational cost assumption implies that CEOs should experience forced turnover at higher rates when their firms' tax rates are low. Contrary to this notion, Gallemore, Maydew, and Thornock (2014) find no evidence of increased CEO turnover rates following the revelation of tax shelter participation. In this paper we consider both the traditional reputational cost prediction as well as the opposite prediction: the possibility that CEOs are more likely to be fired for paying high (rather than low) effective tax rates. Because taxes represent a wealth transfer from shareholders to government authorities, we predict CEOs are more likely to be terminated when their firms pay high taxes. Evidence consistent with this effect would suggest a different kind of reputational effect, one where the CEO is implicitly motivated to avoid rather than not avoid taxes.

To examine this possibility, we study the relation between both GAAP and cash effective tax rates and forced CEO turnover. Our effective tax rate measures are benchmarked by industry, year, and size; consistent with the approach in Armstrong, Blouin, Jagolinzer, and Larcker (2015). We use effective tax rates in our primary analysis for a number of reasons. First, effective tax rates are readily available and relatively easy to understand summary measures of tax policy choices that boards can monitor and evaluate (Dyreng, Hanlon, and Maydew 2010; Armstrong et al. 2015). Second, the choice of benchmarked effective tax rates is supported by our conversations with a former tax director for a large publicly traded firm, who emphasized

that boards not only focus on effective tax rates but also regularly compare these rates to those of their peers. Third, groups like Citizens for Tax Justice and US Uncut tend to focus on effective tax rate measures when commenting on corporate tax policy.[1] Fourth, effective tax rates allow for a common measure of relatively high or relatively low tax rates that simplifies our research design and the interpretations of our results. Fifth, in many instances using effective tax rates minimizes sample attrition, thus increasing the generalizability of our results.

We examine forced CEO turnover because it represents a deliberate action by the board to modify the firm's direction, strategy, and leadership (Fee, Hadlock, and Pierce 2013).[2] We focus on CEOs rather than tax directors (who are directly responsible for the tax function of the firm) for three primary reasons.[3] First, focusing on CEOs makes the task of identifying forced turnover possible. Second, empirical results showing that tax director turnover increases in the firm's tax rate is unlikely to revise prior expectations. The extent to which CEOs are held accountable for tax outcomes is less certain. However, there is at least some anecdotal support for the view that CEOs are held accountable for corporate tax outcomes. For example, former Xerox CEO Richard Thoman was replaced as CEO following a string of disappointing financial results, particularly with respect to taxes. Mismanagement of the firm's tax strategy during Thoman's tenure resulted in a seven percentage point increase in Xerox's reported effective tax rate.[4] A growing stream of the literature also investigates the link between CEOs and corporate taxes including Dyreng et al. (2010), Rego and Wilson (2012), Chyz (2013), Gaertner (2014),

[1] News and Analysis – Economic Analysis. February 13, 2012.
[2] We do not examine changes in CEO compensation, as there exists a relatively deep literature examining the relation between CEO compensation and corporate tax outcomes (e.g., Phillips 2003; Desai and Dharmapala 2006; Armstrong, Blouin, and Larker 2012; Rego and Wilson 2012; Gaertner 2014; and Powers, Robinson, and Stomberg 2016).
[3] These arguments also apply to CFOs, albeit to a lesser extent than the tax director. Because CFOs are often held responsible for the tax function of the firm, we also expect them to be more likely to be fired when the firm's tax rate is significantly higher than the tax rate of their peers. As part of our empirical analysis we examine CFO turnover and find qualitatively similar conclusions as those obtained from our CEO analysis (see footnote 19).
[4] The Wall Street Journal. April, 17 2001, "How a Xerox Plan to Reduce Taxes and Boost Profits Backfired."

Olsen and Stekelberg (2015), and Chyz, Gaertner, Kausar, and Watson (2015). Despite anecdotal evidence and the findings in these papers, skepticism remains about the potential role for CEOs in corporate tax policy because CEOs are almost never tax experts (Dyreng et al. 2010). Third, we are interested in tax outcomes that motivate boards to change firm leadership. While CEO dismissal is a board decision, the dismissal of a tax director is largely a CEO decision. In this respect, CEOs' selection and retention of tax directors capture an aspect of their corporate tax management strategy.

Using a large firm-year panel, we find in univariate tests that forced CEO turnover is highest for observations in the first and fifth quintiles of benchmarked GAAP and cash effective tax rates. We extend this analysis into multivariate models that estimate the linear probability of forced CEO turnover for any given firm-year as a function of belonging to either the lowest or highest benchmarked GAAP or cash effective tax rate quintile. These regressions control for industry and year effects, turnover determinants from prior literature, and proxies for organizational crises. Our multivariate results confirm univariate findings, indicating that CEOs are indeed more likely to experience forced turnover when benchmarked tax rates are relatively high and relatively low.

As part of our empirical analysis we perform a falsification test, examining the association between taxes and unforced CEO turnover (i.e., turnover due to death or natural retirement). Unforced turnover provides us with a strong falsification test because these events are less likely to result from board intervention. If there is a spurious positive relation between higher or lower effective tax rate treatment firms and turnover then we should find similar results using unforced turnover events. When we examine unforced CEO turnovers we find no evidence of a positive association between turnover and either high or low effective tax rate treatment

firms. This falsification test provides further support for the inferences documented in our primary tests.

Our primary results continue to hold after controlling for alternative explanations including variation in corporate governance, competition, CEO geography, and pretax accounting performance. We address pretax accounting performance as an alternative explanation because of the well-known relation between accounting performance and CEO turnover (see for example; Coughlan and Schmidt 1985; Warner, Watts, and Wruck 1990; Engel, Hayes, and Wang 2003; Farrell and Whidbee 2003). Consistent with extant research, our primary effective tax rate measures use pretax income as a tax expense scalar. Scaling by pretax income could introduce measurement error to the extent differences in effective tax rates arise from variation in pretax book income as opposed to variation in tax avoidance. We perform a number of robustness tests to address these concerns including scaling tax preference proxies by the market value of equity consistent with Henry and Sansing (2015), dropping firms in the bottom half of our sample's return on assets, and dropping firms in the bottom half of our sample's *change* in return on assets. In all but one case (i.e., low tax rate quintile using the approach in Henry and Sansing 2015) we continue to document support for our two hypotheses. We also document consistent results after dropping firms with extreme effective tax rate realizations and firms with more than one instance of CEO turnover. We examine the sensitivity of our results to the distributional assumptions of our empirical models (i.e., logit instead of LPM), and continue to find consistent results. Our results are also unchanged after controlling for common determinants of tax avoidance. Finally, we use propensity score weighting as an alternate approach to controlling for tax avoidance determinants and continue to document support for our hypotheses.

In additional tests we explore whether the link between forced CEO turnover and benchmarked effective tax rates varies over time. Specifically, we use the passage of the Sarbanes-Oxley Act (SOX) to capture a period when firms were under relatively more pressure to be less tax aggressive. The post-SOX period coincided with increased IRS scrutiny of aggressive tax positions and legislation that led to increased regulatory scrutiny over the tax function. Consistent with increased pressures to be less tax aggressive, we find that being in the lowest quintile of benchmarked tax rates is only influential in predicting CEO turnover in the post-SOX period. This suggests that the relation between paying relatively too little tax and CEO turnover obtains only in periods with high regulatory scrutiny on tax aggressiveness. In contrast, we find that the positive relation between relatively too much tax and forced CEO turnover in the pre-SOX period does not change following the passage of Sarbanes-Oxley. This latter result is consistent with our expectations, as the likelihood that boards hold CEOs accountable for paying high tax rates should not change in periods where regulatory scrutiny is focused on firms paying low tax rates.

Our findings yield a number of important insights. We document a statistically and economically significant relation between paying higher benchmarked taxes and forced CEO turnover. Given the unconditional probability of CEO turnover in our sample of 4.91 percent, the coefficient magnitudes in our regression analyses suggest that firms paying high taxes relative to peers have turnover rates of 20 to 22 percent higher than non-treatment firms. These results show that CEOs are more likely to be terminated when their firms pay relatively high taxes. This is opposite to the commonly held reputational cost view of tax avoidance advanced in the extant literature.

We also document a statistically and economically significant relation between paying lower benchmarked taxes and forced CEO turnover. Given the unconditional probability of CEO turnover in our sample of 4.91 percent, the coefficient magnitudes in our regression analyses suggest that firms paying lower taxes relative to peers have turnover rates 13 to 16 percent higher than non-treatment firms. To our knowledge this is the first empirical support in the literature for the commonly held reputational cost view of tax avoidance with respect to CEO labor market reputations. Gallemore et al. (2014) are unable to find evidence of increased likelihoods of CEO turnover following the revelation of tax shelter participation. The authors cite their small sample and a resulting low test power as a possible reason they do not document statistically significant results. Using broader measures of benchmarked tax avoidance and forced turnover as we do in our study potentially allows for the identification and increased test power necessary to document a statistically significant result.

Our study adds to the literature that examines the role of CEOs on corporate tax outcomes (Dyreng et al. 2010; Rego and Wilson 2012; Chyz 2013; Gaertner 2014; Olsen and Stekelberg 2015; Chyz et al. 2015; Koester, Shevlin, and Wangerin 2016). A general criticism in this literature is that CEOs are almost never tax experts and are unlikely to understand the details of common tax strategies, thus calling into question their role in corporate taxes. Our results document a relation between forced turnover and tax avoidance, suggesting that boards appear to hold CEOs accountable for firms' corporate tax outcomes at least to some extent.

Our study also adds to the literature examining determinants of CEO turnover. In particular, we add to the literature that examines the relation between firm performance and CEO turnover (see for example; Coughlin and Schmidt 1985; Murphy and Zimmerman, 1993; Denis and Denis 1995; Engel et al. 2003; Farrell and Whidbee 2003; Yonker 2016). Controlling for

other aspects of firm performance, effective tax rates can be an incremental performance metric that has not been considered in prior work. The marginal effect of paying too much or too little tax that we document appears economically significant both in isolation and relative to the prior literature in this area.

We structure the remainder of this paper as follows. In Section 2, we place our study in the context of the existing literature and develop hypotheses. In Section 3, we describe the data, our variables of interest, and the empirical design. In Section 4, we present results from empirical tests. We summarize our findings and conclude in Section 5.

II. RELATED LITERATURE AND HYPOTHESIS DEVELOPMENT

Recent studies present evidence of substantial variation in effective tax rates (Dyreng, Hanlon, and Maydew 2008; Blouin 2014). For example, Dyreng et al. (2008) document that some firms sustain low effective tax rates over a long period of time while other firms exhibit consistently high effective tax rates that meet or exceed the statutory rate. At least part of this variation has been attributed to CEOs. For example, Dyreng et al. (2010) document a significant CEO effect on corporate tax rates. Similarly, Chyz (2013), Olsen and Stekelberg (2015), and Chyz et al. (2015) find that variation in corporate taxes is affected by CEOs' personal tax aggressiveness, narcissism, and overconfidence respectively. Koester et al. (2016) find that managers possessing superior ability tend to maintain lower effective tax rates. Rego and Wilson (2012) find that CEO equity risk incentives are an important determinant of tax aggressiveness. Gaertner (2014) and Powers et al. (2016) find that effective tax rates are lower for firms that use

after-tax performance incentives relative to firms that use before-tax performance incentives.[5] Prior literature also suggests that CEOs' tax policy choices impound the non-tax costs of tax avoidance. Consistent with the Scholes and Wolfson all-taxes paradigm (Scholes et al. 2013) the maintained view in much of the empirical tax research is that cross-sectional variation in non-tax costs partially explains this observed variation in effective tax rates.[6] Among other things, this framework suggests that while avoiding taxes generates tax savings, doing so is not costless and managers must trade-off non-tax costs with expected benefits from tax avoidance. Reputational costs are frequently posited as an important non-tax cost that could limit the extent to which CEOs could avoid taxes (Desai and Dharmapala 2006; Hanlon and Slemrod 2009; Chen et al. 2012; Graham et al. 2013).

While studies vary in whether they view reputational costs as impacting executives individually or the firm as a whole, in all cases the assumption is that reputational penalties follow "too much" tax avoidance. For example, Desai and Dharmapala (2006) suggest that possible sanctions imposed upon managers that increase the costs of tax avoidance include criminal, civil, or reputational sanctions. Austin and Wilson (2015) cite both firms' reputation with customers and managers' individual reputations as limiting tax avoidance. Finally, underscoring the role of executive reputation, Crocker and Slemrod (2005) suggest that tax enforcement sanctions are typically optimal when levied against firms' management.

To date, empirical evidence supporting significant reputational effects at both the firm and executive level is scant. Some limited exceptions include Hanlon and Slemrod (2009) and

[5] Armstrong et al. (2012) do not differentiate between before and after tax performance incentives and find statistically significant relations between effective tax rates and tax director compensation but not with respect to CEO compensation.

[6] See Hanlon and Heitzman (2010), Maydew (2001), and Shackelford and Shevlin (2001) for comprehensive summaries on the related literature.

Graham et al. (2013).[7] Both studies document evidence supporting reputational costs for tax avoidance. Hanlon and Slemrod (2009) find that stock prices are negatively impacted upon the news of accusations that firms participated in tax shelter transactions. Graham et al. (2013)'s survey results suggest that 69 percent of executives cite "potential harm to firm reputation" as a reason for not adopting a particular tax avoidance strategy. Reputational concerns is second only to concerns of getting caught as the most cited reason for not participating in a tax shelter. Unlike our study, the evidence in Hanlon and Slemrod (2009) measures changes in total firm value and thus cannot isolate labor market reputational costs. In addition, it is unclear whether the survey evidence in Graham et al. (2013) captures broader firm-level reputational costs or executive-specific reputational costs.

In the study most closely related to ours, Gallemore et al. (2014) specifically test for CEO labor market reputational effects following announcements of tax shelter participation. Among empirical tests aimed at quantifying both firm and executive level reputational effects, the authors examine the likelihood of CEO turnover following public revelation of tax shelter involvement. Gallemore et al. (2014) find no evidence of increased CEO turnover following announcements of firms' tax shelter participation. This result suggests that CEOs do not bear labor market reputational costs from aggressive tax policy choices. Unlike Gallemore et al. (2014) we do not focus on the revelation of a particular tax strategy. Rather, we examine firms in the bottom of the tax distribution relative to peers as a proxy for what boards might consider paying "too little" tax. In addition, our study extends prior literature by distinguishing between forced and unforced CEO turnover. The null result in Gallemore et al. (2014) suggests we might not be able to document an association between relatively low effective tax rates and CEO

[7] Dyreng, Hoopes, and Wilde (2015) document decreases in tax rates for large U.K. firms following public scrutiny of firm subsidiary locations.

turnover even in our alternative setting. However, Fee et al. (2013) suggest that failing to distinguish between forced and unforced turnover could introduce noise or even bias in empirical tests. Because forced turnover represents a deliberate action by the board to modify the firm's direction, strategy, and leadership (Fee et al. 2013) we are better able to reduce noise and bias in our tests thus improving our chances at documenting a relation between paying relatively too little tax and CEO turnover.

As noted above, shareholders (and boards) will want managers to engage in optimal tax planning – balancing the benefits of tax avoidance against the costs (Hanlon and Slemrod 2009). Costs increase with the risk that taxing authorities will challenge tax positions and when paying too little tax leads to the political and reputational costs of being label a "poor corporate citizen" (Hanlon and Slemrod 2009). Because boards and groups like Citizens for Tax Justice regularly compare firms' effective tax rates to those of their peers, we are lead to our first hypothesis stated in alternative form.

Hypothesis 1: *The probability of forced CEO turnover increases when effective tax rates relative to peer firms are low.*

As is evident from the discussion above, prior research has not visited, at least empirically, the possibility that CEOs bear reputational penalties for paying "too much" rather than too little taxes. Tax planning is typically viewed as beneficial to shareholders since it results in higher cash flows and net income (Blouin 2014). Blouin (2014) concludes that firms have a responsibility to structure corporate transactions in a tax efficient manner. Thus, risk-neutral shareholders likely expect managers to pursue opportunities to reduce tax liabilities (Hanlon and Heitzman 2010). High effective tax rates could signal managers' unwillingness or inability to pursue such opportunities. Given that taxes represent a wealth transfer from shareholders to

taxing authorities, it is possible that CEOs are held responsible for perceived decreases in shareholder wealth attributable to tax outcomes. As a result, as effective tax rates increase, boards and shareholders could reasonably question CEOs' stewardship of firm resources.

CEOs should only bear labor market consequences from corporate tax outcomes if boards believe CEOs can impact these outcomes. Prior research has documented evidence consistent with links between individual CEOs and corporate tax outcomes (Dyreng et al. 2010; Rego and Wilson 2012; Chyz 2013; Gaertner 2014; Chyz et al. 2015; and Olsen and Stekelberg 2015). This notion is supported by non-academic evidence. For example, a recent practitioner survey finds that CEOs are playing an increasingly active role in setting and evaluating their firms' tax policies (Ernst & Young 2004). In another example, when asked by a 60 Minutes reporter whether he was judged as a CEO on issues like taxes, John Chambers (CEO of Cisco) responded "Absolutely."[8] Finally, former Xerox CEO Richard Thoman was forced out as CEO in part because of a failed tax strategy that he led.[9]

While we recognize that CEOs are unlikely to directly oversee the tax function of the firm, we believe that failure to engage in tax avoidance could prevent CEOs from achieving after-tax earnings targets, thus increasing the likelihood of CEO underperformance, which would in turn increase their likelihood of termination. To the extent firms' optimal level of tax planning balances benefits against perceived costs as theory would suggest, firms' tax rates relative to peers could simply reflect these costs and not an unwillingness or inability on the part of a CEO to obtain the benefits. In addition, if boards prioritize pretax performance at the expense of tax policy management or do not hold CEOs responsible for taxes, we might not be able to

[8] Abbreviated transcript available at http://www.cbsnews.com/news/a-look-at-the-worlds-new-corporate-tax-havens-25-03-2011/ (last accessed January 30, 2017).
[9] The Wall Street Journal. April 17, 2001. "How a Xerox Plan to Reduce Taxes and Boost Profits Backfired".

document a relation between paying relatively high taxes and CEO turnover. This leads to our second hypotheses stated in alternative form.

Hypothesis 2: *The probability of forced CEO turnover increases when effective tax rates relative to peers are high.*

Our hypotheses refer to effective tax rates relative to peer firms. To capture this construct we estimate industry benchmarked GAAP and cash effective tax rates consistent with Armstrong et al. (2015). Through conversations with a former tax director at a large publicly traded firm we were able to confirm that boards view effective tax rates relative to peer firms as an important metric in evaluating management performance. This is consistent with the view in Armstrong et al. (2015) that peer adjusted taxes capture the cross-sectional comparisons boards could make to determine whether the CEO is effectively managing the firms' taxes.

Focusing on CEOs as we do and not tax directors is consistent with several CEO related studies, including Dyreng, et al. (2010), Rego and Wilson (2012), Chyz (2013), Gaertner (2014), Olsen and Stekelberg (2015), and Chyz et al. (2015). These studies suggest CEOs have a significant impact on corporate policies and decision-making, including tax planning (even if they are not directly involved in the tax-planning process). This approach is also consistent with the "upper echelons" perspective introduced by Hambrick and Mason (1984). An alternative view is that CEOs do not influence taxes, given that they are rarely if ever tax experts and may not be involved in the selection and implementation of tax strategies. If this alternative view is descriptive and boards do not hold CEOs accountable for the tax outcomes of the firm then we should not find support for either of our hypotheses. However, finding support for either of our hypotheses suggests that boards believe CEOs can impact tax outcomes and at times hold them accountable for the tax performance of the firms they manage.

As discussed earlier, we are also cognizant of the role played by tax directors in corporate tax outcomes. In addition to data availability issues with tax directors, we choose to focus on CEOs because we are interested the tax outcomes that motivate boards to change firm leadership. Furthermore, boards hire and fire CEOs while CEOs are more likely to hire and fire tax directors. We view tax director hiring and firing decisions as a component of the CEO's corporate tax management.[10]

III. DATA, MEASURES, AND RESEARCH DESIGN

Data

We begin with a sample of firms experiencing forced CEO departures from Fee et al. (2013). Fee et al. (2013) note that in many settings, CEO departure is endogenously related to organizational crisis that drives board action to deliberately change its leader and/or firm strategy. While their study focuses on unforced turnover to avoid attributing personal characteristics to organizational change, they also compile data for forced turnover. Our study focuses on forced CEO turnover, as we predict that boards are more likely to remove the current CEO when taxes are either high or low. In later tests, however, we use data on unforced turnover as a falsification test.

To arrive at forced CEO turnover, Fee et al. (2013) use Compustat Research Insight CDs and Factiva to search for articles surrounding CEO turnover events containing key words that would indicate a forced departure such as "fired", "ousted", "under pressure", etc. Using this procedure Fee et al. (2013) identify 533 forced turnover events. The authors add to this sample a number of "suspected forced departures" which are not identified as unforced and that relate to a

[10] Given our setting and research design (described in Section 3), tax director turnover would have to be the cause of forced CEO turnover for any tax director effect to be a correlated omitted variable that would impact our inferences. We believe that the chances of this are quite low.

departing CEO that is (1) under the age of 60 at the start of his/her last year in office, and (2) does not immediately resurface as a CEO of another firm. This method adds an additional 4,087 turnover events. Fee et al. (2013) perform validity testing which indicates that both approaches are effective in capturing forced CEO turnover.[11]

[Insert Table 1 Here]

Table 1, Panel A provides detail on our sample selection criteria. We obtain annual financial data from Compustat and turnover data from Fee et al. (2013). Consistent with extant tax-accounting research we begin our study in the post FAS 109 (now ASC 740) period. Our sample ends in 2006 because 2007 is the last year we have turnover data from Fee et al. (2013). We then make data cuts consistent with Fee et al. (2013): deleting foreign firms, financial firms (SIC codes 6000-6999), utilities (SIC codes 4900-4949), and firms with less than $10 million in book assets. We also require firm-year observations to have positive pretax income, as effective tax rates are difficult to interpret for loss firms. Finally, we delete observations without sufficient data to compute the variables in our model.[12] This procedure yields a final sample of 29,703 firm-years (5,108 firms) from 1993 to 2006 for our full multivariate model (see Table 4).[13] Our sample covers a period with consistent accounting for income taxes (i.e., post FAS 109/ASC 740 implementation) and with an unchanged top U.S. corporate statutory tax rate of 35 percent. Table 1, Panel B summarizes the annual distribution of forced turnover events. Our study examines

[11] As a sensitivity check we perform our main tests using only firms where CEOs were overtly fired (i.e., excluding "suspected forced departures") and find that our sample is severely reduced by this exclusion (i.e., after sample cuts we are left with only 211 turnover events under this approach). After limiting our sample we find the predicted coefficient loadings on all four tax indicators, although in all but one case these coefficients are not statistically significant.

[12] We set *Meet/Beat indicator* to zero in cases when a firm is not covered by IBES, as eliminating firms without analyst following would decrease our sample by 34 percent. Given our results, this choice implies that CEOs of firms without analyst coverage face less financial reporting pressure. However, when we limit our sample to firms covered by IBES we document a *Meet/Beat indicator* mean of 0.63 (i.e., 63 percent of the firms in the sample meet or beat analyst forecasts). Our multivariate results are robust to eliminating observations with missing IBES data.

[13] To reduce the impact of outliers, we winsorize all continuous variables at the 1st and 99th percentiles by year.

1,459 forced turnover events. The frequency of forced turnover is highest in 1999 with 158 events and lowest in 1993 with 77 events. The mean annual turnover over our sample period is approximately 104.

Measures

Effective Tax Rates

We are interested in examining the role of tax avoidance and effective tax rates on forced CEO turnover. Consistent with Dyreng et al. (2010) we use two broad and easy to understand measures: the GAAP effective tax rate (*ETR*) and the cash effective tax rate (*Cash ETR*). Both measures, computed over a three-year period, capture the amount of tax firms pay relative to their pretax accounting income. For each firm-year we construct benchmarked *ETR* and *Cash ETR* measures, consistent with Armstrong et al. (2015). These effective tax rate measures capture cross-sectional variation in firms' tax planning and benchmarks a given firm's tax avoidance relative to that of similar-sized firms in the same industry and year.

Cash ETR reflects firms' actual cash tax payments for a given level of pretax income. *ETR* reflects firms' GAAP tax expense for a given level of pretax income. *ETR* includes tax accruals for financial reporting purposes, while the numerator of *Cash ETR* includes only cash flows and should not be affected by accruals including changes in firms' tax contingencies (tax cushion) (Dyreng et al. 2008). We examine both GAAP and cash effective tax rates because Graham, Hanlon, Shevlin, and Shroff (2015) cite survey results suggesting that there is potential variation amongst top management regarding how they value the GAAP effective tax rate relative to cash taxes paid. This could reflect boards' preferences or it could be that boards do not vary in how they value cash taxes paid relative to GAAP tax expense. Research has also shown that public companies are primarily concerned with their GAAP effective tax rate (Blouin 2014).

Thus, whether our results differ when examining *Cash ETR* or *ETR* is an empirical question. Because the *ETR* is reported on the financial statements and commonly referred to in the financial press, it is potentially more visible than the *Cash ETR*.

We select effective tax rates instead of other measures thought to capture tax avoidance for a number of reasons. First, many common tax avoidance measures, including book-tax-differences, are closely related to effective tax rates (Guenther 2014). Second, in part because of their simple interpretation, visibility in the financial statements, and ease of calculation we expect effective tax rates to be relatively more useful for boards in their CEO evaluation and decision making (Armstrong et al. 2015). Third, anecdotal evidence suggests tax watch dog groups like Citizens for Tax Justice focus on effective tax rates.[14] To the extent boards hold CEOs accountable for damage done to the firms' reputation because of attention from tax watch dog groups, effective tax rates are the most appropriate measure. Fourth, Graham et al. (2015) suggest effective tax rates (GAAP effective tax rates specifically) are important in incremental decision making. Finally, as noted above, conversations with a former tax director at a large publicly traded firm confirm boards are in constant review of firms' effective tax rates.

ETR is estimated as the three-year sum of total tax expense divided by the three-year sum of pretax income. We set *ETR* equal to zero in the case of net tax refunds and equal to one when *ETR* is greater than one. *Cash ETR* is the three-year sum of total cash paid for taxes divided by the three-year sum of pretax income. Similar to *ETR* we set *Cash ETR* equal to zero for net tax refunds and equal to one when *Cash ETR* is greater than one. Both measures are set to missing when the three-year sum of pretax income is non-positive. Lastly, we normalize *ETR* and *Cash ETR* by size, industry, and year so that our measures of tax performance are benchmarked to

[14] News and Analysis – Economic Analysis. February 13, 2012.

other firms of similar size in the same industry consistent with the approach in Armstrong et al. (2015).[15]

Both effective tax rates only capture non-conforming tax rate avoidance. Effective tax rate variants are also unable to distinguish between real activities that are tax favored, activities specifically targeted to reduce taxes, and targeted tax benefits from lobbying activities (Hanlon and Heitzman 2010). If our measures of tax avoidance are understated, then we would expect our empirical results to be understated as well.

Research Design

We examine whether tax avoidance impacts the probability of a forced CEO turnover event using the following linear probability model:[16]

$$\begin{aligned} \textit{Forced CEO turnover}_{i,t+1} = {} & \alpha + \beta_1 \textit{Low tax indicator}_{i,t} + \beta_2 \textit{High tax indicator}_{i,t} \\ & + \beta_k \textit{Controls}_{i,t} + I_i + T_t + \varepsilon_{i,t} \end{aligned} \quad (1)$$

Linear probability models generate OLS estimates of a binary dependent variable. This design choice aids in the economic interpretation of our coefficients of interest.[17] Variable definitions

[15] We normalize by subtracting by a benchmark effective tax rate, where benchmarks are the annual averages of effective tax rates calculated the same way as each individual firms' effective tax rate measure for the each firm's size and industry peer group. Size peers are firms within the same quintile of total assets and industry peers are firms within the same Fama-French 17 industry classification. In sensitivity checks we use Fama-French 30 and Fama-French 48 industry classifications and find similar results.

[16] We use a linear probability model (LPM) to simplify the interpretation of our coefficients. According to Wooldridge (2002) "the LPM should be seen as a convenient approximation to the underlying response probability." Wooldridge suggests the key drawback to using LPM is that it may produce fitted values outside the unit interval (i.e., outside of 0 to 1 range). According to Wooldridge, however, "even with these weaknesses, the LPM often seems to give good estimates of the partial effects of the response probability." Further, Wooldridge (2002) suggests LPMs are especially suited to discrete variables of interest. Given that (1) we do not require fitted values as part of our analysis, and (2) our variables of interest are discrete, we believe our setting is particularly suited to a linear probability approach. We also note LPMs have been shown to perform as well on categorical variables as specifications that do not assume linearity and have been used in prior accounting studies (Shi 2003; Hanlon and Hoopes 2014). However, as reported later in our discussion of robustness tests, we find that our inferences are unchanged using a logit specification.

[17] Using a logit model does not impact our inference. See sensitivity analysis summarized in Table 6.

for equation (1) are as follows:[18] *Forced CEO turnover* is an indicator variable capturing forced CEO turnover and is equal to one for firm-years where Fee et al. (2013) code the turnover event as an overt firing or a suspected force out, and 0 otherwise.[19,20] *Low tax indicator* is an indicator variable equal to 1 for observations in the lowest quintile of *ETR* / *Cash ETR*, and 0 otherwise; *High tax indicator* is an indicator equal to 1 for observations in the highest annual quintile of *ETR* / *Cash ETR*, and 0 otherwise, *I* is a vector of industry fixed effects, *T* is a vector of year fixed effects, α is an intercept, and ε is a disturbance term with mean zero. Our inclusion of year fixed effects removes the effects of macroeconomic conditions that might be associated with forced CEO turnover, while our inclusion of industry effects removes cross-sectional effects of industry on turnover. We estimate model (1) and present results separately for *ETR* and *Cash ETR*. By including indicator variables for the highest (*High tax indicator*) and lowest (*Low tax indicator*) peer-adjusted tax rate quintiles, our variables of interest capture differences in the probability of being forced out relative to the middle three peer-adjusted tax rate quintiles. In this respect, the middle three quintiles act as our control group while the top and bottom tax quintiles are our treatment groups.

Hypothesis 1 predicts that the probability of a forced turnover increases for firms with relatively low effective tax rates. In equation (1) this would be supported by a positive and

[18] Subscript *i* denotes firm, *j* denotes industry, and *t* denotes year. Unless stated otherwise, all input data for control variables are calculated as of time *t*.

[19] For any unforced turnover event in our panel of firm-years, we code the variable *Forced CEO turnover* as zero. For example, if Firm A experiences a forced turnover event in 1996, *Forced CEO turnover* = 1 for that year. If the same firm experiences an unforced turnover event in 2001 *Forced CEO turnover* = 0 for that year and all other years except 1996.

[20] Our data on forced turnover only includes CEOs. For completeness we examine CFO turnover using turnover data from Execucomp. This reduces our sample by 68% percent (i.e., from 29,703 to 9,578). In this analysis we find a coefficient of 0.01 on both *Low tax indicator* and *High tax indicator* when tax indicators are based on *ETR*. The coefficient on *Low tax indicator* is significant at the 0.10 level while the coefficient on *High tax indicator* is significant at the 0.05 level. When tax indicators are based on *Cash ETR* we continue to find positive coefficient on both low- and high-tax indicators, although neither is statistically significant. Overall, our CFO results are somewhat consistent with our CEO results despite the large reduction in statistical power due to the restricting our sample to Execucomp and the downward bias inherent to using general CFO turnover that does not distinguish between forced and unforced.

significant β_1 coefficient. Hypothesis 2 predicts that the probability of a forced turnover increases for firms with relatively high effective tax rates. In equation (1) this would be supported by a positive and significant β_2 coefficient. Because equation (1) is a linear probability model we can interpret coefficients on β_1 and β_2 as the percentage change in probability of a forced CEO turnover for a one-unit change (i.e., 0 to 1) in the *Low tax indicator* and *High tax indicator* respectively.

We add the set of control variables included in the model used by Gallemore et al. (2014), which captures variables shown in the prior accounting research to influence executive turnover (see for example Gilson 1989; Engel et al. 2003; Hennes, Leone, and Miller 2008; Menon and Williams 2008). We supplement the Gallemore et al. (2014) set of controls to include additional determinants of turnover from prior literature as well as controls intended to capture organizational crisis. Our full set of control variables includes *Size*, *Abnormal stock returns*, *ΔReturn on assets*, *Leverage*, *CEO ownership*, *ΔSales*, *σAbnormal stock returns*, *σReturn on assets*, *ΔNOL*, *Prior losses indicator*, *Managerial ability*, and *Meet/Beat indicator*. *Size* is the natural log of market value of equity. *Abnormal stock returns* is the annual stock return for the fiscal year minus the value-weighted return for the S&P 500. *ΔReturn on assets* is current minus lagged pretax return on assets. *Leverage* is long-term debt (including the current portion) divided by total assets. *CEO ownership* is the number of shares owned by the CEO divided by total shares outstanding. *ΔSales* is the one-year change in sales divided by total assets. *σAbnormal stock returns* is the three-year standard deviation of *Abnormal stock returns*. *σReturn on assets* is the three-year standard deviation of pretax earnings divided by total assets. *ΔNOL* is the one-year change in tax loss carryforwards divided by total assets. *Prior losses indicator* is a binary variable indicating a loss in either the current or prior year. *Managerial ability* is a proxy for

managerial ability from Demerjian, Lev, and McVay (2012). Finally, *Meet/Beat Indicator* is 1 if the firm meets or beats the consensus IBES forecast and 0 otherwise.

IV. EMPIRICAL RESULTS

Descriptive Statistics

[Insert Table 2 Here]

Table 2 presents descriptive statistics conditioned on tax quintiles. Panel A presents results for the bottom, middle three, and top quintiles of *ETR*. Raw *ETR* reports *ETR* before benchmarking by industry, year, and size; while *ETR* reports the benchmarked measure. Beneath the summary of descriptive statistics we also present tests for the differences in means between the control groups of firms (i.e., Q2, Q3 and Q4) and Q1 and Q5 respectively. Further, we summarize the difference in means between Q1 and Q5. Many of our variables exhibit statistically significant differences between the quintiles, underscoring the need to include them as controls in our multivariate analysis.[21]

On average, firms in the bottom *ETR* quintile have tax rates that are 19.8 percentage points lower than their peers. On average, firms in the top *ETR* quintile have tax rates that are 19.4 percentage points higher than their peers. In Table 2 we see also that *Forced CEO turnover* is highest in the first and fifth quintiles of *ETR*, consistent with H1 and H2. Firms in the extreme quintiles of *ETR* tend to be smaller than firms in the middle quintiles. *Abnormal stock returns* are highest for observations in the first quintile and lowest for observations in the fifth quintile. *ΔReturn on assets* tends to be negative throughout the sample, and is least negative for

[21] Results summarized in Table 7 and described below employ a propensity score weighting technique to improve the covariate balance between our treated and nontreated samples.

observations in the first quintile of *ETR* and most negative for observations in the fifth quintile; although differences between the three partitions are trivial. *Leverage* is highest for observations in the top quintile of *ETR*. *CEO ownership* is highest for observations in the middle three quintiles. *ΔSales* is lowest in the bottom quintile of *ETR*. Volatility in stock returns is highest for firms in the bottom quintile of *ETR* while volatility in pretax accounting returns is lowest for firms in the bottom and top quintiles. *ΔNOL* is lowest for the bottom quintile of *ETR*. Firms in the top quintile of *ETR* tend to experience more losses. Finally, observations in the middle quintiles of *ETR* have higher managerial ability and are more likely to meet or beat earnings targets.

Panel B of Table 2 presents results for the bottom, middle three, and top quintiles of *Cash ETR*. Again we see that *Forced CEO turnover* is highest in the first and fifth tax quintiles, consistent with H1 and H2. With a few exceptions we observe similar patterns between partitions as observed in Table 2 Panel A.

Overall both panels of Table 2 support H1 and H2. However, univariate results should be interpreted with caution as they do not control for firm and industry characteristics or general time trends.[22]

Correlations

[Insert Table 3 Here]

[22] Because there is some evidence in Table 2 of higher forced turnover rates for CEOs in Q5 relative to Q1, our results potentially support the assertion that costs faced by CEOs in Q5 are relatively higher than costs faced by CEOs in Q1. Additional analysis summarized in Table 8 examines periods before and after the passage of SOX, when the costs faced by CEOs in the first quintile changed while the costs faced by CEOs in the fifth quintile were unlikely to have changed. Consistent with our results in those tests, in untabulated analysis we find that the univariate difference in forced CEO turnover for Q1 relative to the control quintiles only holds post-SOX while the difference between control quintiles and Q5 holds both before and after SOX.

Table 3 presents Pearson correlation coefficients for the main variables used in our study. Correlation coefficients presented in bold are statistically significant at the 5 percent level. Consistent with H2 we find high tax indicators (based on both *ETR* and *Cash ETR*) are positively and significantly correlated with *Forced CEO turnover*. Simple correlations of low tax indicators and Forced CEO turnover are insignificant.

Δ*Return on assets* and *Abnormal stock returns* are negatively and significantly correlated with *Forced CEO turnover*, suggesting that turnover is more likely when performance is weak as we would expect given prior research. Correlations between *Forced CEO turnover* and all other variables tend to be small, consistent with the low predictive power of models predicting turnover in prior literature.

Forced Turnover by Tax Quintiles

[Insert Figure 1 Here]

In Figure 1 we plot the likelihood of forced CEO turnover by tax quintiles. Panel A reports turnover likelihoods by *ETR* quintiles, while Panel B reports turnover likelihoods by *Cash ETR* quintiles. Because *Forced CEO turnover* is an indicator variable, the quintile-specific mean realizations capture the probability of experiencing a forced turnover conditional on the effective tax rate quintile assignment (i.e., quintiles 1 through 5). Under H1 we expect the mean realizations of forced CEO turnover to be higher in Q1 (when benchmarked effective tax rates are lowest). Under H2 we expect mean realizations of forced CEO turnover to be higher in Q5 (when benchmarked effective tax rates are highest). Thus, we expect a U-shaped relation between forced CEO turnover and taxes. Consistent with our earlier results, Figure 1 shows the probability of forced turnover is lowest in the control group where firms' tax rates are closest to

their peers' and highest in the top and bottom quintiles. This U-shaped relation between tax rates and forced turnover offers support for both H1 and H2.[23]

Multivariate Results

[Insert Table 4 Here]

Table 4 reports two sets of results for estimates of equation (1). The first for tax indicators based on *ETR* and the second for tax indicators based on *Cash ETR*. The variable *Low tax indicator* captures the sub-sample of CEOs at firms with relatively low effective tax rates. If CEOs of firms paying little tax relative to peers tend to experience reputational costs then we expect the coefficient on *Low tax indicator* to be positive and significant. This would support H1. The coefficient on the variable *High tax indicator* captures the sub-sample of CEOs at firms with relatively high effective tax rates. If CEOs of firms paying high effective tax rates relative to peers tend to experience higher turnover rates then we expect the coefficient on *High tax indicator* to be positive and significant. This would support H2.

In both *ETR* and *Cash ETR* models we find a positive and significant coefficient on *Low tax indicator*, suggesting CEOs of low-tax-paying firms are more likely to be fired relative to CEOs of firms with tax rates in the middle three quintiles, holding the remaining variables constant. Thus, we find multivariate support for H1. In addition, we find a positive and significant coefficient on *High tax indicator* in both *ETR* and *Cash ETR* models. Both results

[23] Figure 1 also presents the p-value on the test of the difference in the mean realizations of CEO turnover in Q5 relative to Q1. In both Panels A and B, the mean realization of turnover is higher in Q5, though this difference is statistically significant in Panel B only.

suggest CEOs are more likely to be fired when firms pay high tax rates, holding the remaining variables constant.[24]

Because we estimate the effect of taxes on forced CEO turnover using a linear probability model, the coefficients on both tax indicators represent marginal effects of belonging to each specific sub-sample, holding the other variables in the model constant. Economic estimates of *Low tax indicator*, where tax indicators are based on *ETR* (*Cash ETR*), imply firms paying low tax rates experience incrementally higher CEO turnover rates by 0.6 percent (0.8 percent). Given the unconditional probability of CEO turnover in our sample of 4.9 percent, these marginal effects suggest that firms paying much lower taxes relative to peers have turnover rates that are anywhere from 13 to 16 percent higher than the control group of firms. Economic estimates of *High tax indicator*, where tax indicators are based on *ETR* (*Cash ETR*), imply firms paying high tax rates experience incrementally higher CEO turnover rates by 0.8 percent (1 percent). These marginal effects suggest that firms paying higher taxes relative to peers have turnover rates that are anywhere from 20 to 22 percent higher than the control group of firms.[25] Economic magnitudes derived from our multivariate results are generally consistent with those implied in

[24] Unreported F-statistics for our *ETR* and *Cash ETR* model are 8.62 and 8.83, respectively (i.e., both models are significantly different from 0 with p-value < .0001). The addition of our tax indicators to our forced CEO turnover model increases the R-squared from 2.02 to 3.13 percent depending on the effective tax rate measure. This increase appears to be in line with recent research including Koester et al. (2016). While the R-squared is an important statistic in measuring overall model explanatory power, it is unrelated to our ability of drawing conclusions about the relation between X and Y. Because we are interested in the relation between tax indicators and forced CEO turnover rather than finding the best model of forced CEO turnover, we believe our model is appropriate for our research question.

[25] In untabulated results we use standardized coefficients to better understand the economic magnitude of our results relative to other control variables. We find that a one standard deviation increase in *Low tax indicator* results in about a 0.015 standard deviation increase in *Forced CEO turnover*. This effect is about 42 percent of the *Size* effect, 29 percent of the *Abnormal stock returns* effect, and 39 percent of the *ΔReturn on assets effect*. In fact, of the statistically significant coefficients, the effect of *Low tax indicator* is the lowest of the covariates. We find that a one standard deviation increase in *High tax indicator* results in about a 0.017 standard deviation increase in *Forced CEO turnover*. This effect is about 49 percent of the *Size* effect, 34 percent of the *Abnormal stock returns* effect, and 45 percent of the *ΔReturn on assets effect*. Of the statistically significant coefficients, only *CEO ownership* and *Low tax indicator* exhibit smaller standardized effects on *Forced CEO turnover*. The results suggest that while not a first-order effect, corporate tax outcomes are still an economically significant determinant of forced CEO turnover.

univariate comparisons. In other words, controlling for determinants of turnover and organizational crisis has little effect on the economic strength of our results, mitigating concerns over endogeneity.[26]

Results from Table 4 also show that *Forced CEO turnover* is positively associated with *Size*, *σAbnormal stock returns*, *σReturn on assets*, and *ΔNOL*. *Forced CEO turnover* is negatively associated with *Abnormal stock returns*, *ΔReturn on assets*, *CEO ownership*, and *Meet/Beat indicator*. These results are generally consistent with expectations and prior research in this area.

Falsification Tests

To gain additional comfort that our primary results are not spurious or the result of correlated omitted factors generally inherent to CEO turnover, we conduct a falsification test. Specifically, we re-estimate our main tests after substituting *Unforced CEO turnover* for *Forced CEO turnover*. Fee et al. (2013) attempt to circumvent the endogeneity associated with most studies of unconditional CEO departure by following the approaches of Johnson, Magee, Nagarajan, and Newman. (1985), Denis and Denis (1995), and Weisbach (1995). Specifically, Fee et al. (2013) use Compustat Research Insight CDs and Factiva searches to identify 824 firms experiencing CEO turnover events related to health, death, and natural retirements from 1990-2007; which they classify as unforced CEO turnover.[27] Unforced CEO departures are unlikely to be the result of organizational stress or crisis that drives board action to deliberately change its

[26] While the economic significance of our variables of interest are meaningful, low R-squared realizations suggest that our model has somewhat limited explanatory power. However, as noted by Brickley (2003), low explanatory power is common in the CEO turnover literature.

[27] Natural retirements are coded as taking place when the CEO is between 63 and 71 at the start of the year. Because some older managers may in fact be forced to depart, Fee et al. (2013) also require that the firm's most recent level of accounting performance exceed the sample annual median. The authors also exclude from this group any departures that are later discovered to be overtly forced.

leader or firm strategy (Fee et al., 2013). Therefore if there is a positive relation between low and high tax rates and CEO turnover due to unobserved reasons inherent to the general turnover process, then we should find a positive relation between our tax indicators and unforced CEO turnover.

[Insert Table 5 Here]

The results of our falsification test (reported in Table 5) are supportive of our hypotheses, as our variables of interest are not positively related to unforced turnover. Specifically, the coefficients on *Low tax indicator* and *High tax indicator* are consistently negative. This finding provides additional comfort that the results documented in Table 4 are not spurious.

Robustness

In this sub-section we perform a series of additional tests to ensure the robustness of our results. Unless stated otherwise, estimation results for each of our tests is presented in Table 6. Table 6 only reports coefficient estimates for *Low tax indicator* and *High tax indicator* for ease of presentation although all tests also include at a minimum the control variables summarized in equation (1).[28] Full estimation results of alternative specifications of equation (1) are available from the authors by request.[29]

[Insert Table 6 Here]

Pretax Performance

Differences in pretax performance offer a plausible alternative explanation of our results. Under this alternative explanation, our results are driven by the denominator of effective tax

[28] Table 6 begins with our baseline results from Table 4 reported under line (1).

[29] Table 6 includes abbreviated results for 22 different models, each making step-wise changes to our primary models. While we only report the coefficient of interest for these models we note that loadings on control variables and model fit are largely the same across all LPM models. Control variables load consistently across our logit models as well, but goodness-of-fit statistics for logit are not directly comparable to those estimated using the LPM.

rates (i.e., pretax performance) as opposed to the numerator. Therefore, we perform three additional tests to ensure that our results are not driven by a denominator effect.

First, we adjust *ETR* and *Cash ETR* using the adjustments outlined in Henry and Sansing (2015), who create a variant of cash ETR by creating a tax preference variable scaled by market value of assets. When basing our tax indicators on these modified measures we continue to find support for H2, but not H1. Second, we then estimate equation (1) only for firms in the top 50th percentile of Δ*Return on assets*. CEOs of these firms should be less likely to be fired for low pretax earnings. After doing so, we find support for both H1 and H2. Finally, we estimate equation (1) only for firms in the top 50th percentile of the level, rather than the change, of *Return on assets*; as CEOs of these firms should also be unlikely to be fired for low pretax earnings. After doing so, we again find support for both H1 and H2.

Governance

Prior research has documented links between firms' governance and tax outcomes including Armstrong et al. (2015), Desai and Dharmapala (2009), Wilson (2009) among others. We examine whether our results can be explained by differences in governance of firms with low or high tax rates. To do so we add several proxies for governance; including *G_index*, *Institutional ownership*, *Board independence*, and *CEO duality*.[30] After doing so, we continue to find support for both H1 and H2; suggesting our results are not driven by variation in corporate governance.

Competition

DeFond and Park (1999) find that the link between relative performance based evaluations and turnover, and the frequency of CEO turnover, is greater in highly competitive

[30] Because of low coverage in some of our governance variables, we follow Hanlon and Slemrod (2009) and set missing values of governance variables to 0, while adding a separate variable computed as 1 for missing governance observations and 0 otherwise.

industries. To ensure our results are not driven by competitive forces within different industries we also add the Herfindahl index (DeFond and Park 1999). After doing so we find that the coefficient on the Herfindahl index is not significant, a finding that is not surprising given that our primary analysis employs industry fixed effects. More importantly, we find that our results are nearly identical after directly controlling for firms' competitive environments.

Determinants of Tax Avoidance

Our primary model attempts to control for known determinants of turnover. We avoid controlling for determinants of tax avoidance because including these variables could control for the effects we seek to capture (Dyreng et al. 2010). However, we find consistent results after including controls from Chyz et al. (2013).

Full Model

We employ a full specification model, including our primary controls, all governance variables included above, the Herfindahl index, as well as the determinants of tax avoidance from Chyz et al. (2013). After using this specification we continue to find support for H1 and H2.

Regression Distributional Assumptions

Our main tests rely on the Linear Probability Model (i.e., OLS estimates of a binary dependent variable), simplifying the economic interpretation of our coefficients of interest. Prior research has shown that these models are comparable to regression models specifically designed for binary data (e.g., probit and logit) when using categorical variables (Shi 2003). In sensitivity tests we estimate our models using logit and again find support for H1 and H2 across specifications using both tax variables.

Multiple Instances of Turnover

It is possible that firms with more than one instance of turnover in our panel of firm-years are unique and not comparable to the rest of our sample. Accordingly, we delete firms with more than one instance of turnover, and continue to find results consistent with H1 and H2 suggesting the inclusion of these firms is not problematic.

Extreme Tax Measures

As noted above, we winsorize our effective tax rate measures at 0 and 1. Doing so could increase the chances that firms with extreme effective tax rate outcomes are driving our results. To address this concern we delete firm with potentially extreme *ETRs* or *Cash ETRs* (i.e., 0 and 1) and continue to find similar results. This suggests that extreme realizations of our effect tax rate measures do not impede our ability to generate inferences.

CEO Locality

Yonker (2016) examines the association between CEO locality (i.e., local vs. non-local CEO) and CEO turnover and finds that unforced turnover is approximately 20 percent less likely for local than similar non-local CEOs. Most importantly for our study, Yonker (2016) finds that the incidence of forced turnover and the sensitivity of performance to turnover are unrelated to CEO locality. Because our paper examines the relation between measures of tax avoidance and forced turnover, the findings in Yonker (2016) suggests that CEO locality is unlikely to be relevant in our study and can be excluded from our model. Consistent with our expectations, when we control for CEO for locality consistent with Yonker (2016) we find very similar results (see Table 6).[31]

Propensity Score Weighting

[31] We thank Scott Yonker for sharing with us his data on CEO locality. Data on CEO locality is only available for 28 percent of our sample. To maintain sample size we follow the approach used in Hanlon and Slemrod (2009) and set missing values of *CEO locality* to 0, while adding a separate variable computed as 1 for missing *CEO locality* observations and 0 otherwise.

[Insert Table 7 Here]

In Table 7 we present results of equation (1) using propensity score weighted samples. Propensity score matching increases covariate balance between treated and nontreated samples, acting as an alternate approach to multivariate regression models (Tucker 2010). Morgan and Winship (2007) suggest weighting by the propensity score approximates propensity score matching. Weighting not only approximates propensity score matching, but also improves the efficiency of second-stage estimates and does not require eliminating data. We create weights using propensity scores, using separate models to compute propensity scores for *Low tax indicator* and *High tax indicator*, as each indicator represents a separate treatment and propensity score matching/weighting does not accommodate multiple treatments at the same time.[32] We then use these weights in our primary model, using a modified sample of our full treatment sample as well as all observations in the control-only group (i.e., observations with tax indicators in the middle three quintiles). Propensity scores are obtained by estimating each treatment indicator on the tax avoidance determinants in Chyz et al. (2013) using a logistic regression. The list of determinants includes, return on assets (*ROA*), leverage (*LEV*), annual change in NOL (*ΔNOL*), foreign income (*FI*), property, plant, and equipment (*PPE*), intangible assets (*INTANG*), equity income (*EQINC*), size (*SIZE*), and market-to-book ratio (*MB*).[33]

In Table 7 Panel C we examine differences in means to assess covariate balance, and find that propensity score weighting yields close to full covariate balance. Only 2 of the possible 36 differences in means are significantly different after our weighting procedure, and both are only

[32] That is, we estimate four first-stage regressions. One for each indicator, performed for tax indicators based on both *ETR* and *Cash ETR*. Weights, following Morgan and Winship (2007), are 1 / propensity score for treatment variables and 1 / (1 – propensity score) for control observations.

[33] Our four first-stage models yield Pseudo-R2s between 10.36 and 13.88 percent.

marginally different; improving the validity of our second state model. As evidenced from our results in panels A and B of Table 7, we continue to find support for both H1 and H2.

Tax Shelter Score

Our primary results use effective tax rates which have been characterized in the past research as being broad and relatively easy to understand summary measures of tax policy outcomes (Dyreng et al. 2010; Armstrong et al. 2015). In the context of the tax avoidance continuum summarized in Lisowsky, Robinson, and Schmidt (2013) effective tax rates potentially capture less aggressive tax positions than tax sheltering. To explore whether our findings are consistent with a measure that potentially captures a more aggressive set of tax positions, in untabulated analyses we use the *Tax shelter score* from Wilson (2009). The *Tax shelter score* has some strengths and weaknesses. It is designed to capture more aggressive tax avoidance while also yielding the greatest sample size relative to similar measures of aggressive tax planning, thus maintaining the generalizability of our tests. However, the *Tax shelter score* is an inferred probability that does not capture direct evidence of tax sheltering.[34]

For our tests that incorporate the *Tax shelter score* we substitute effective tax rate measures with indicators based on *Tax shelter score*, creating two variables of interest: *Low shelter indicator* and *High shelter indicator*. *Low shelter indicator* (*High shelter indicator*) is 1 for firms in the bottom (top) quintile of the *Tax shelter score*, and 0 otherwise. Consistent with our primary analyses our quintile assignment is assessed after benchmarking by industry, year, and size. Inferences from our *Tax shelter score* results map well into our effective tax rate tests. Specifically, we find a positive and significant coefficient on *Low shelter indicator* (coefficient = 0.009, p-value < 0.01) and a positive and significant coefficient on *High shelter indicator*

[34] With respect to tax sheltering in general, Hanlon and Heitzman (2010) note that tax shelters are often single transactions that may not capture the firm's overall tax avoidance behavior.

(coefficient = 0.0075, p-value < 0.05). This latter result is somewhat inconsistent with Gallemore et al. (2014) who are unable to document an increased probability of CEO turnover for firms accused of tax sheltering. One reason we could be able to document results is that the *Tax shelter score* allows for larger sample size and increased test power. Gallemore et al. (2014) cite their small sample and a resulting low test power as a possible reason they do not document statistically significant results. Another possibility is that our sample does not rely on revealed tax shelter participation. Our hypotheses and research design are based on the premise that boards observe contemporaneous tax related performance as an input into their decision to retain or fire the CEO. A potential issue with revealed tax shelter participation is that the revelation of shelter participation often occurs many years after tax shelters have been implemented. Finally, it could be the case that by benchmarking tax sheltering as we do in our study, we are able to better capture the relative-to-peer firm evaluations made by boards.

Regulatory Changes

This section examines our results before and after Sarbanes-Oxley. Our sample spans from 1993 to 2006. During much of this time period a large number of firms engaged in aggressive tax sheltering. To address the increasing use of tax shelters, the U.S. Department of the Treasury issued tax shelter regulations in the early 2000's. Meanwhile the Sarbanes-Oxley Act (SOX) substantially increased regulatory scrutiny on the corporate tax function. As a result, CEOs were under intense pressure to be less aggressive in the years following SOX. Therefore, we expect the coefficient on *Low tax indicator* to be higher in the post-SOX period. It seems unlikely that the effect of paying too much tax on forced turnover would change in periods where

regulatory scrutiny is focused on firms paying too little tax. Therefore, we do not expect the coefficient on *High tax indicator* to change in the post-SOX period.

[Insert Table 8 Here]

To empirically examine the effect of regulatory changes on our results we include an indicator variable for the post-SOX period (*SOX*) and interact it with *Low tax indicator* as well as with *High tax indicator*. The results, reported in Table 8, show that the coefficient on *Low tax indicator* is not significant in the pre-SOX period. However, the effect is positive and significant in the post-SOX period, consistent with the predicted effect of regulatory changes and an increased scrutiny on the corporate tax function. We also find that the effect of *High tax indicator* on turnover is positive and significant in the pre-SOX period, and that there is not a significant change in the coefficient from the pre- to post-SOX period.

Changes in Tax Following Forced Turnover

Our final analysis examines changes in tax avoidance for firms that have relatively high or low tax rates just prior to a forced CEO turnover. If low (high) taxes played a role in the CEOs firing, we expect these trends to be reversed under new management. Given recent research that finds tax avoidance also reduces the standard deviation of effective tax rates (Guenther, Matsunaga, and Williams 2015) we also expect firms undergoing a decrease in ETRs to have decreases in the standard deviation of ETRs and vice versa.[35,36,37] The unit of observation for

[35] Guenther et al. (2015) find a positive correlation between Cash ETR and tax risk, suggesting firms taking aggressive positions are able to reduce their tax payments while maintaining stable rates.

[36] These tests can be particularly insightful in the forced turnover setting because effective tax rates capture an important aspect of firms' financial performance. In addition, recent research has used the volatility of effective tax rates to capture tax risk (McGuire, Neuman, and Omer 2013; Bauer and Klassen 2014; Guenther et al. 2015; Drake, Lusch, and Stekelberg 2015) suggesting these tests speak to an element of firm risk management around forced CEO turnover.

these tests is the firm and not firm-year. In addition, these tests capture the subset of the firms from our primary sample that experience forced turnover. Accordingly, sample sizes will not be directly comparable to our primary analysis.

[Insert Table 9 Here]

Table 9 reports tax rates as well as their standard deviations before and after turnover, after conditioning on firms' tax status just prior to turnover.[38] Each firm enters the sample exactly twice, once prior to turnover and once after, so that the differences we observe are not driven by changes in sample composition. Low-tax firms face an increase in taxes and an increase in standard deviations of tax measures following forced turnover. Increased standard deviations of tax measures could reflect the unwinding of previously employed aggressive tax positions by the incoming CEO. We find the opposite for high-tax firms. In other words, these firms appear to not only lower their tax rates, but manage to do so while also achieving more stable rates. We find little difference in tax rates or their respective standard deviations for firms in the middle three tax quintiles, suggesting the differences in tax outcomes we observe are not driven by non-tax determinants of forced turnover.

V. SUMMARY AND CONCLUSIONS

Our study contributes to the tax literature by examining the role of corporate taxes in forced CEO turnover. We model the probability of a turnover event as a function of peer-adjusted effective tax rates and a set of firm covariates to predict forced CEO turnover using a

[37] We also examine changes in stock returns before and after turnover to capture changes in overall firm performance. We find a general increase in stock returns following turnover, however, the relation between stock returns and turnover is not significantly different for low-tax or high-tax firms.

[38] We report one-year tax rates to show the immediate effect of turnover, and do not benchmark them to allow for greater economic interpretation of outcomes. However, trends in three-year benchmarked *ETR* and *Cash ETR* are similar to those reported in Table 9.

sample of forced CEO changes from Fee et al. (2013). We perform our regression analysis on a large panel of publicly traded firms with and without forced turnover.

We find support for the commonly held view in much of the tax literature that CEOs bear reputational penalties for avoiding too much tax. Though this view has been widely held, prior research that relied on relatively small samples with potentially low test power had been unable to document a significant relation. Our study's broader measures of benchmarked tax avoidance and forced CEO turnover allows us to document evidence in support of the commonly held view of tax avoidance related to reputational costs. Additional analysis reveals that the reputational costs of too much tax avoidance only seem to appear after the enactment of Sarbanes-Oxley. This is consistent with boards responding to regulators' and taxing authorities increased scrutiny of aggressive tax practices that served to increase political and reputational costs surrounding tax avoidance.

We also find support for the opposite of the commonly held view. Specifically, CEOs who do not avoid enough tax are more likely to be forced out. Unlike the effect of avoiding too much tax, the latter result of avoiding too little tax holds throughout our sample period. This is not surprising because the cost of paying too much tax should not change in periods where regulators and taxing authorities have started to focus on firms paying too little tax. By documenting increased forced turnover probabilities for firms paying too much tax, our study is the first to document evidence consistent with significant labor market consequences from what could be interpreted as CEOs exercising poor tax policy management. We note that it is unclear whether the CEO reputational effect from paying too much tax that we document arises from an under-investment in tax avoidance, or a tendency for investments in tax avoidance to fail or be over-turned by the IRS. Also unclear from our study is whether our univariate evidence of a

consistently higher likelihood of turnover for CEOs paying too much tax relative to CEOs paying too little tax is suggestive of a reputational cost differential between these groups. Although paying too little tax potentially increases reputational costs and scrutiny from regulators and taxing authorities, after-tax cash flows for these firms are likely to be higher relative to firms paying too much tax. If the observed level of IRS detection and the resulting penalties, even for the most aggressive tax positions, are quite low as Weisbach (2002) asserts then all else equal, paying too much tax could be more costly than paying too little. These are questions and areas of inquiry for future research.

Because evaluating, hiring, and retaining CEOs is an important board function, our study provides evidence that boards appear to hold CEOs accountable for the tax outcomes of the firm. Furthermore, our additional tests the examine firms following forced turnover suggest two things. First, firms' replacement CEOs appear to move firms' effective tax rates closer to their peers. Second, performance following forced turnover improves for firms in the lowest and highest tax quintiles. Thus it appears that on average, it is rational for boards to engage in tax motivated evaluation, hiring and retention decisions.

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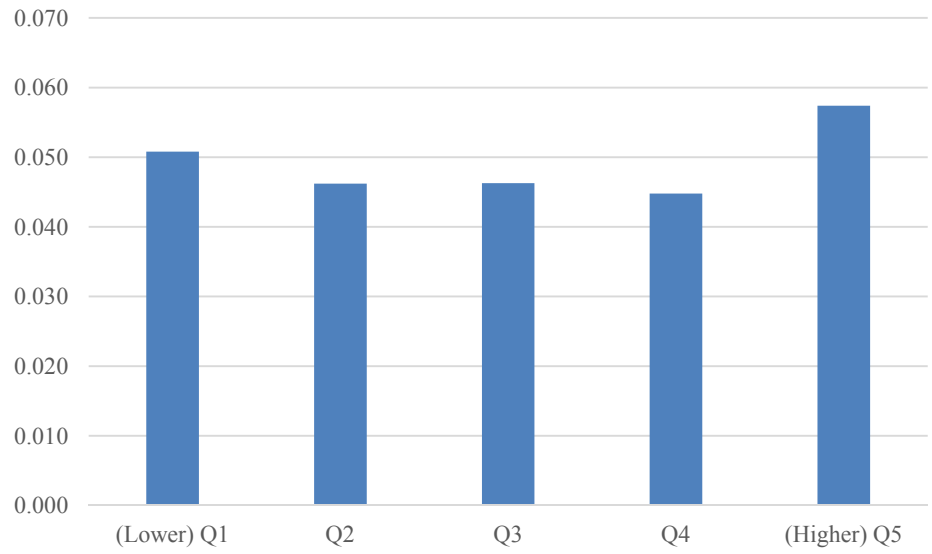
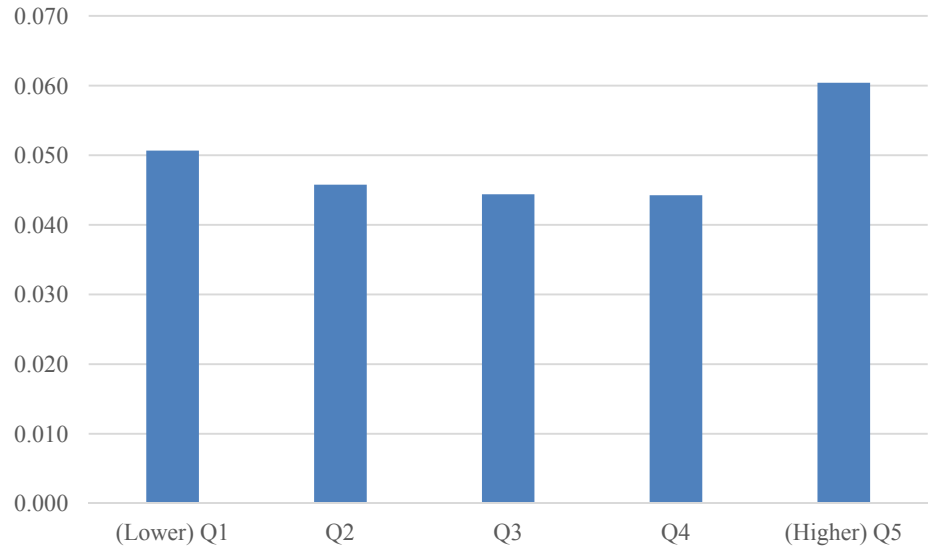
Figure 1
Forced CEO turnover by tax quintiles

Panel A: *Forced CEO turnover* by *ETR* quintiles



ETR	Q1	Q2	Q3	Q4	Q5	Q5 - Q1	p-value
Mean *Forced CEO turnover*	0.051	0.046	0.046	0.045	0.057	0.007	0.11

Panel B: *Forced CEO turnover* by *Cash ETR* quintiles



Cash ETR	Q1	Q2	Q3	Q4	Q5	Q5 - Q1	p-value
Mean *Forced CEO turnover*	0.051	0.046	0.044	0.044	0.060	0.010	0.02

This figure reports the likelihood of CEO turnover across quintiles of *ETR* and *Cash ETR*. p-value relates to a test for differences in Forced CEO turnover means between the first and fifth quintiles.

Table 1
Sample selection

Panel A: Sample selection

All Compustat firm-year observations from 1993-2006			148,398
Less:	Foreign	13,025	
	Financials and utilities	19,738	
	Less than $10 in total assets	34,957	
	Non-positive pre-tax income	36,042	
	Negative equity	997	
	Firms missing *ETR* or *Cash ETR*	7,208	
	Firms missing *Size*	2,173	
	Firms missing *Abnormal stock returns*	1,819	
	Firms missing *Leverage*	77	
	Firms missing σ*Abnormal stock returns*	2,033	
	Firms missing *Managerial ability*	626	118,695
Total firm-year observations			29,703

Panel B: Number of Forced CEO turnover events by year

Year	*Forced CEO turnover*
1993	77
1994	93
1995	117
1996	114
1997	112
1998	142
1999	158
2000	110
2001	88
2002	97
2003	88
2004	90
2005	93
2006	80

This table reports sample selection criteria for the study. *Forced CEO turnover* is 1 in cases where Fee et al. (2013) code the turnover event as an overt firing or a suspected force out, and 0 otherwise. *ETR* (*Cash ETR*) is the three-year sum of tax expense (cash paid for taxes) divided by the three-year sum of pretax income. *ETR* and *Cash ETR* are winsorized to 0 and 1, are set to missing when their respective denominators are non-positive, and are normalized by size, industry, and year. *Size* is the natural log of market value of equity. *Abnormal stock returns* is the annual stock return minus the value weighted return for the S&P 500 for fiscal year *t*. *Return on assets* is pretax income divided by total assets. *Leverage* is long-term debt (including the current portion) divided by total assets. σ*Abnormal stock returns* is the three-year standard deviation of abnormal stock returns. *Managerial ability* follows Demerjian et al. (2012). *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 2
Descriptive Statistics

Panel A: Descriptive statistics by *ETR* Quintiles

	Quintile 1 n = 5,940		Quintiles 2, 3, 4 n = 17,823		Quintile 5 n = 5,940	
Variable	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Forced CEO turnover	0.051	0.220	0.046	0.209	0.057	0.233
Raw *ETR*	0.157	0.118	0.365	0.040	0.537	0.202
ETR	-0.198	0.104	0.001	0.032	0.194	0.183
Size ($ millions)	3,622	20,011	3,792	17,117	2,082	10,134
Abnormal stock returns	0.099	0.642	0.067	0.562	0.046	0.616
Δ Return on assets	-0.005	0.080	-0.006	0.065	-0.007	0.092
Leverage	0.195	0.178	0.199	0.169	0.235	0.178
CEO ownership	0.077	0.530	0.122	0.669	0.075	0.500
Δ Sales	-0.029	0.220	-0.024	0.212	-0.005	0.240
σ Abnormal stock returns	0.561	0.566	0.458	0.454	0.486	0.479
σ Return on assets	0.050	0.048	0.038	0.037	0.056	0.052
Δ NOL	-0.014	0.072	-0.001	0.029	-0.001	0.046
Prior losses indicator	0.015	0.121	0.003	0.056	0.026	0.159
Managerial ability	-0.011	0.134	0.021	0.127	0.005	0.132
Meet/Beat indicator	0.385	0.487	0.450	0.497	0.338	0.473

	Q1 - Q2, 3, 4 differences in means			Q1 - Q5 differences in means			Q5 - Q2, 3, 4 differences in means		
Variable	diff	std. error		diff	std. error		diff	std. error	
Forced CEO turnover	0.005	0.003	*	-0.006	0.004		0.011	0.003	***
Raw *ETR*	-0.208	0.001	***	-0.380	0.003	***	0.172	0.002	***
ETR	-0.199	0.001	***	-0.392	0.003	***	0.193	0.001	***
Size ($ millions)	-170	268		1,540	291	***	-1,710	235	***
Abnormal stock returns	0.032	0.009	***	0.053	0.011	***	-0.021	0.009	**
Δ Return on assets	0.001	0.001		0.002	0.002		-0.001	0.001	
Leverage	-0.004	0.003		-0.040	0.003	***	0.036	0.003	***
CEO ownership	-0.045	0.010	***	0.002	0.009		-0.047	0.009	***
Δ Sales	-0.005	0.003		-0.024	0.004	***	0.019	0.003	***
σ Abnormal stock returns	0.103	0.007	***	0.075	0.010	***	0.028	0.007	***
σ Return on assets	0.012	0.001	***	-0.006	0.001	***	0.018	0.001	***
Δ NOL	-0.013	0.001	***	-0.013	0.001	***	0.000	0.001	
Prior losses indicator	0.012	0.001	***	-0.011	0.003	***	0.023	0.001	***
Managerial ability	-0.032	0.002	***	-0.016	0.002	***	-0.016	0.002	***
Meet/Beat indicator	-0.065	0.007	***	0.047	0.009	***	-0.112	0.007	***

(continued)

Table 2, continued
Descriptive Statistics

Panel B: Descriptive statistics by *Cash ETR* Quintiles

	Quintile 1 n = 5,940		Quintiles 2, 3, 4 n = 17,823		Quintile 5 n = 5,940	
Variable	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Forced CEO turnover	0.051	0.219	0.045	0.207	0.060	0.238
Raw *Cash ETR*	0.083	0.073	0.295	0.092	0.632	0.231
Cash ETR	-0.245	0.066	-0.022	0.066	0.310	0.206
Size ($ millions)	2,207	16,677	4,152	17,496	2,417	13,557
Abnormal stock returns	0.144	0.710	0.067	0.552	0.000	0.556
Δ Return on assets	0.004	0.076	-0.005	0.063	-0.019	0.097
Leverage	0.221	0.193	0.197	0.167	0.215	0.172
CEO ownership	0.072	0.501	0.122	0.671	0.080	0.521
Δ Sales	-0.020	0.223	-0.025	0.211	-0.011	0.239
σ Abnormal stock returns	0.613	0.605	0.453	0.448	0.451	0.435
σ Return on assets	0.048	0.048	0.037	0.036	0.060	0.052
Δ NOL	-0.017	0.075	-0.001	0.029	0.002	0.038
Prior losses indicator	0.009	0.097	0.002	0.049	0.034	0.181
Managerial ability	-0.005	0.134	0.021	0.128	-0.001	0.129
Meet/Beat indicator	0.380	0.485	0.450	0.497	0.342	0.475

	Q1 - Q2, 3, 4 differences in means			Q1 - Q5 differences in means			Q5 - Q2, 3, 4 differences in means		
Variable	diff	std. error		diff	std. error		diff	std. error	
Forced CEO turnover	0.006	0.003	**	-0.009	0.004	**	0.015	0.003	***
Raw *Cash ETR*	-0.212	0.001	***	-0.549	0.003	***	0.337	0.002	***
Cash ETR	-0.223	0.001	***	-0.555	0.003	***	0.332	0.002	***
Size ($ millions)	-1,945	259	***	-210	279		-1,735	249	***
Abnormal stock returns	0.077	0.009	***	0.144	0.011	***	-0.067	0.008	***
Δ Return on assets	0.009	0.001	***	0.023	0.002	***	-0.014	0.001	***
Leverage	0.024	0.003	***	0.006	0.003	*	0.018	0.002	***
CEO ownership	-0.050	0.009	***	-0.008	0.009		-0.042	0.010	***
Δ Sales	0.005	0.003	*	-0.009	0.004	**	0.014	0.003	***
σ Abnormal stock returns	0.160	0.007	***	0.162	0.010	***	-0.002	0.007	
σ Return on assets	0.011	0.001	***	-0.012	0.001	***	0.023	0.001	***
Δ NOL	-0.016	0.001	***	-0.019	0.001	***	0.003	0.000	***
Prior losses indicator	0.007	0.001	***	-0.025	0.003	***	0.032	0.001	***
Managerial ability	-0.026	0.002	***	-0.004	0.002	*	-0.022	0.002	***
Meet/Beat indicator	-0.070	0.007	***	0.038	0.009	***	-0.108	0.007	***

(continued)

Table 2, continued
Descriptive Statistics

This table reports means and standard deviations for the full sample conditioning on *ETR* quintiles (Panel A) and on *Cash ETR* quintiles (Panel B). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively for one-tailed tests examining forced turnover quintiles and two-tailed otherwise.. *Forced CEO turnover* is 1 in cases where Fee et al. (2013) code the turnover event as an overt firing or a suspected force out, and 0 otherwise. Raw *ETR* (Raw *Cash ETR*) is the three-year sum of tax expense (cash paid for taxes) divided by the three-year sum of pretax income. Raw *ETR* and Raw *Cash ETR* are winsorized to 0 and 1 and are set to missing when their respective denominators are non-positive. *ETR* (*Cash ETR*) is obtained by normalizing Raw *ETR* (Raw *Cash ETR*) by size, industry, and year. *Size* is the natural log of market value of equity. *Abnormal stock returns* is the annual stock return minus the value weighted return for the S&P 500 for fiscal year *t*. Δ*Return on assets* is the change in pretax income scaled by total assets. *Leverage* is long-term debt (including the current portion) divided by total assets. *CEO ownership* is the percentage of shares outstanding owned by the CEO. *ΔSales* is the change in sales scaled by total assets. *σAbnormal stock returns* is the three-year standard deviation of abnormal stock returns. *σReturn on assets* is the three-year standard deviation of pretax income scaled by total assets. *ΔNOL* is the change in tax loss carryforwards scaled by total assets. *Prior loss indicator* is a binary variable indicating the presence of losses in the current or prior year. *Managerial ability* follows Demerjian et al. (2012). *Meet/Beat indicator* is a binary variable indicating the meeting or beating of the consensus analyst forecast for the current period. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 3
Correlations

		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
(1)	*Forced CEO turnover*	0.00	**0.02**	0.00	**0.03**	0.01	**-0.06**	**-0.06**	0.00	**-0.02**	0.00	0.01	**0.04**	**0.03**	**0.01**	0.00	**-0.02**
(2)	*Low tax indicator (ETR)*		**-0.25**	**0.37**	**-0.10**	**-0.08**	**0.03**	0.01	**-0.03**	**-0.02**	**-0.02**	**0.08**	**0.07**	**-0.12**	**0.02**	**-0.08**	**-0.03**
(3)	*High tax indicator (ETR)*			**-0.10**	**0.33**	**-0.10**	**-0.02**	-0.01	**0.09**	**-0.02**	**0.04**	0.00	**0.14**	**0.02**	**0.08**	**-0.02**	**-0.08**
(4)	*Low tax indicator (Cash ETR)*				**-0.25**	**-0.13**	**0.06**	**0.07**	**0.04**	**-0.03**	0.00	**0.13**	**0.05**	**-0.15**	0.00	**-0.06**	**-0.04**
(5)	*High tax indicator (Cash ETR)*					**-0.09**	**-0.06**	**-0.09**	**0.03**	**-0.02**	**0.02**	**-0.03**	**0.19**	**0.06**	**0.12**	**-0.05**	**-0.07**
(6)	*Size*						**0.11**	**0.08**	-0.01	**0.11**	0.01	**-0.13**	**-0.14**	**0.05**	**-0.05**	**0.12**	**0.31**
(7)	*Abnormal stock returns*							**0.33**	**-0.08**	**0.06**	-0.01	**0.33**	**0.04**	**-0.07**	**-0.01**	**0.04**	**0.10**
(8)	*Δ Return on assets*								**-0.04**	**0.01**	**0.27**	**0.02**	**-0.15**	**-0.10**	0.00	**0.04**	**0.10**
(9)	*Leverage*									**-0.04**	**-0.02**	**-0.09**	**-0.16**	**0.04**	0.01	**-0.09**	**-0.08**
(10)	*CEO ownership*										0.00	**0.03**	**-0.03**	**0.01**	-0.01	**0.04**	**0.07**
(11)	*Δ Sales*											**-0.06**	**-0.08**	**0.04**	**0.02**	**-0.02**	**0.02**
(12)	*σ Abnormal stock returns*												**0.26**	**-0.07**	0.00	**0.04**	0.00
(13)	*σ Return on assets*													**-0.04**	**0.14**	0.00	**-0.05**
(14)	*Δ NOL*														**0.04**	**-0.04**	0.01
(15)	*Prior losses indicator*															**-0.06**	**-0.03**
(16)	*Managerial ability*																**0.07**
(17)	*Meet/Beat indicator*																

This table reports Pearson correlation coefficients for the full sample (bold coefficients are significant at the 5% level). *Forced CEO turnover* is 1 in cases where Fee et al. (2013) code the turnover event as an overt firing or a suspected force out, and 0 otherwise. *ETR* (*Cash ETR*) is the three-year sum of tax expense (cash paid for taxes) divided by the three-year sum of pretax income. *ETR* and *Cash ETR* are winsorized to 0 and 1, are set to missing when their respective denominators are non-positive, and are normalized by size, industry, and year. *Low tax indicator (ETR)* and *Low tax indicator (Cash ETR)* are set to 1 for observations in the lowest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *High tax indicator (ETR)* and *High tax indicator (Cash ETR)* are set to 1 for observations in the highest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *Size* is the natural log of market value of equity. *Abnormal stock returns* is the annual stock return minus the value weighted return for the S&P 500 for fiscal year *t*. Δ*Return on assets* is the change in pretax income scaled by total assets. *Leverage* is long-term debt (including the current portion) divided by total assets. *CEO ownership* is the percentage of shares outstanding owned by the CEO. *ΔSales* is the change in sales scaled by total assets. *σAbnormal stock returns* is the three-year standard deviation of abnormal stock returns. *σReturn on assets* is the three-year standard deviation of pretax income scaled by total assets. *ΔNOL* is the change in tax loss carryforwards scaled by total assets. *Prior loss indicator* is a binary variable indicating the presence of losses in the current or prior year. *Managerial ability* follows Demerjian et al. (2012). *Meet/Beat indicator* is a binary variable indicating the meeting or beating of the consensus analyst forecast for the current period. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 4
Taxes and Forced CEO turnover

$$Forced\ CEO\ turnover_{i,t+1} = \alpha + \beta_1 Low\ tax\ indicator_{i,t} + \beta_2 High\ tax\ indicator_{i,t} + \beta_k Controls_{i,t} + I_i + T_t + \varepsilon_{i,t} \quad (1)$$

	Forced CEO turnover					
	Tax indicators based on *ETR*			Tax indicators based on *Cash ETR*		
	Coefficient	Std. Error		Coefficient	Std. Error	
Intercept	0.0176	0.0076	**	0.0183	0.0075	**
Low tax indicator	0.0064	0.0033	**	0.0079	0.0034	***
High tax indicator	0.0107	0.0034	***	0.0094	0.0034	***
Size	0.0038	0.0007	***	0.0038	0.0007	***
Abnormal stock returns	-0.0186	0.0025	***	-0.0185	0.0025	***
Δ Return on assets	-0.1106	0.0193	***	-0.1094	0.0194	***
Leverage	-0.0003	0.0076		0.0000	0.0076	
CEO ownership	-0.0051	0.0021	**	-0.0051	0.0021	**
Δ Sales	0.0091	0.0060		0.0090	0.0060	
σ Abnormal stock returns	0.0099	0.0029	***	0.0098	0.0029	***
σ Return on assets	0.1762	0.0323	***	0.1753	0.0324	***
Δ NOL	0.0936	0.0286	***	0.0955	0.0287	***
Prior losses indicator	0.0153	0.0127		0.0151	0.0127	
Managerial ability	-0.0015	0.0099		-0.0010	0.0099	
Meet/Beat indicator	-0.0081	0.0027	***	-0.0082	0.0027	***
Industry fixed effects	Yes			Yes		
Year fixed effects	Yes			Yes		
R-square	0.012			0.012		
N	29,703			29,703		

(continued)

Table 4, continued
Taxes and Forced CEO turnover

This table reports results for our main tests examining the effect of taxes on forced CEO turnover using a Linear Probability Model. Regression coefficients and standard errors are reported side-by-side. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively for one-tailed tests examining H1 and H2, two-tailed otherwise. *Forced CEO turnover* is 1 in cases where Fee et al. (2013) code the turnover event as an overt firing or a suspected force out, and 0 otherwise. *ETR* (*Cash ETR*) is the three-year sum of tax expense (cash paid for taxes) divided by the three-year sum of pretax income. *ETR* and *Cash ETR* are winsorized to 0 and 1, are set to missing when their respective denominators are non-positive, and are normalized by size, industry, and year. *Low tax indicator (ETR)* and *Low tax indicator (Cash ETR)* are set to 1 for observations in the lowest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *High tax indicator (ETR)* and *High tax indicator (Cash ETR)* are set to 1 for observations in the highest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *Size* is the natural log of market value of equity. *Abnormal stock returns* is the annual stock return minus the value weighted return for the S&P 500 for fiscal year *t*. Δ*Return on assets* is the change in pretax income scaled by total assets. *Leverage* is long-term debt (including the current portion) divided by total assets. *CEO ownership* is the percentage of shares outstanding owned by the CEO. *ΔSales* is the change in sales scaled by total assets. *σAbnormal stock returns* is the three-year standard deviation of abnormal stock returns. *σReturn on assets* is the three-year standard deviation of pretax income scaled by total assets. *ΔNOL* is the change in tax loss carryforwards scaled by total assets. *Prior loss indicator* is a binary variable indicating the presence of losses in the current or prior year. *Managerial ability* follows Demerjian et al. (2012). *Meet/Beat indicator* is a binary variable indicating the meeting or beating of the consensus analyst forecast for the current period. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 5
Taxes and Unforced CEO turnover

$$Unforced\ CEO\ turnover_{i,t+1} = \alpha + \beta_1 Low\ tax\ indicator_{i,t} + \beta_2 High\ tax\ indicator_{i,t} + \beta_k Controls_{i,t} + I_j + T_t + \varepsilon_{i,t}$$

	Unforced CEO turnover					
	Tax indicators based on *ETR*			Tax indicators based on *Cash ETR*		
	Coefficient	Std. Error		Coefficient	Std. Error	
Intercept	-0.0043	0.0046		-0.0041	0.0046	
Low tax indicator	-0.0023	0.0020		-0.0037	0.0020	
High tax indicator	-0.0042	0.0020		-0.0045	0.0020	
Size	0.0033	0.0004	***	0.0033	0.0004	***
Abnormal stock returns	-0.0005	0.0015		-0.0005	0.0015	
Δ Return on assets	0.0240	0.0118	**	0.0235	0.0118	**
Leverage	0.0002	0.0046		0.0003	0.0046	
CEO ownership	0.0014	0.0013		0.0014	0.0013	
Δ Sales	0.0036	0.0036		0.0037	0.0036	
σ Abnormal stock returns	-0.0031	0.0018	*	-0.0030	0.0018	*
σ Return on assets	-0.0170	0.0196		-0.0150	0.0197	
Δ NOL	0.0201	0.0174		0.0188	0.0175	
Prior losses indicator	-0.0051	0.0077		-0.0047	0.0077	
Managerial ability	0.0080	0.0060		0.0076	0.0060	
Meet/Beat indicator	-0.0010	0.0016		-0.0010	0.0016	
Industry fixed effects	Yes			Yes		
Year fixed effects	Yes			Yes		
R-square	0.005			0.005		
N	29,703			29,703		

(continued)

Table 5, continued
Taxes and Unforced CEO turnover

This table reports results for our falsification tests examining the effect of taxes on unforced CEO turnover using a Linear Probability Model. Regression coefficients are reported above, while standard errors are reported below. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively for one-tailed tests examining H1 and H2, two-tailed otherwise. *Unforced CEO turnover* is 1 in cases where Fee et al. (2013) code the turnover event as being due to death, health issues, or natural retirement, and 0 otherwise. *ETR* (*Cash ETR*) is the three-year sum of tax expense (cash paid for taxes) divided by the three-year sum of pretax income. *ETR* and *Cash ETR* are winsorized to 0 and 1, are set to missing when their respective denominators are non-positive, and are normalized by size, industry, and year. *Low tax indicator (ETR)* and *Low tax indicator (Cash ETR)* are set to 1 for observations in the lowest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *High tax indicator (ETR)* and *High tax indicator (Cash ETR)* are set to 1 for observations in the highest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *Size* is the natural log of market value of equity. *Abnormal stock returns* is the annual stock return minus the value weighted return for the S&P 500 for fiscal year *t*. Δ*Return on assets* is the change in pretax income scaled by total assets. *Leverage* is long-term debt (including the current portion) divided by total assets. *CEO ownership* is the percentage of shares outstanding owned by the CEO. *ΔSales* is the change in sales scaled by total assets. *σAbnormal stock returns* is the three-year standard deviation of abnormal stock returns. *σReturn on assets* is the three-year standard deviation of pretax income scaled by total assets. *ΔNOL* is the change in tax loss carryforwards scaled by total assets. *Prior loss indicator* is a binary variable indicating the presence of losses in the current or prior year. *Managerial ability* follows Demerjian et al. (2012). *Meet/Beat indicator* is a binary variable indicating the meeting or beating of the consensus analyst forecast for the current period. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 6
Robustness Tests

	Tax Indicators based on *ETR*				Tax Indicators based on *Cash ETR*			
	β_1 (*Low tax ind.*)		β_2 (*High tax ind.*)		β_1 (*Low tax ind.*)		β_2 (*High tax ind.*)	
	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error
(1) Baseline, see Table 4	0.006	0.003 **	0.011	0.003 ***	0.008	0.003 **	0.010	0.003 ***
Adjusting *ETR* and *Cash ETR* in line with Henry and Sansing (2015)	-0.003	0.003	0.007	0.004 **	-0.003	0.003	0.009	0.004 ***
(2) Deleting firms in the bottom half of *ΔROA*	0.008	0.004 **	0.007	0.004 **	0.008	0.004 **	0.007	0.004 **
Deleting firms in the bottom half of *ROA*	0.007	0.005 *	0.011	0.006 **	0.010	0.006 **	0.010	0.006 **
(3) Controlling for governance	0.006	0.003 **	0.010	0.003 ***	0.008	0.003 ***	0.009	0.003 ***
(4) Controlling for competition	0.006	0.003 **	0.011	0.003 ***	0.008	0.003 ***	0.009	0.003 ***
(5) Controlling for tax avoidance determinants from Chyz et al. (2013)	0.007	0.003 **	0.010	0.003 ***	0.008	0.003 ***	0.008	0.003 ***
(6) Full model	0.006	0.003 **	0.008	0.003 ***	0.006	0.003 **	0.007	0.003 **
(7) Estimating equation (1) using LOGIT, instead of OLS	0.140	0.072 **	0.214	0.070 ***	0.170	0.072 ***	0.189	0.070 ***
(8) Deleting firms with more than one instance of *Turnover*	0.011	0.003 ***	0.008	0.003 ***	0.008	0.003 ***	0.009	0.003 ***
(9) Eliminate extreme *ETR* / *Cash ETR* (i.e., 0 or 1)	0.005	0.004 *	0.010	0.003 ***	0.009	0.004 ***	0.008	0.004 ***
(10) Controlling for *CEO locality*	0.006	0.003 **	0.010	0.003 ***	0.007	0.003 ***	0.010	0.003 ***

This table reports coefficients and standard errors for our variables of interest (i.e., *Low tax indicator* and *High tax indicator* based on either *ETR* or *Cash ETR*) for a series of robustness tests. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively for one-tailed tests examining H1 and H2, two-tailed otherwise. In this table we estimate several variations of our main results, but for brevity only report the coefficients on *Low tax indicator* and *High tax indicator*. Full estimation results are available from the authors upon request. The first two columns report estimates for tax indicators based on *ETR*, while the third and fourth columns report estimates for tax indicators based on *Cash ETR*. Robustness tests are estimated in ten groupings, (1) to (10). (1) reports our base estimation of equation (1) (see Table 4). (2) explores the possibility that a denominator effect explains our results. The first test in this series computes tax indicators for *ETR* and *Cash ETR* using the tax preference adjustment from Henry and Sansing (2015). The second and third tests restrict the sample to firms with high pretax returns (i.e., top half of *ΔROA* and *ROA*). (3) examines governance as an alternative explanation by adding the following proxies for governance: *G_index*, *Institutional ownership*, *Board independence*, and *CEO duality*. (4) controls for competition by adding the Helfindahl index as a control consistent with DeFond and Park (1999). (5) uses traditional tax avoidance determinants as control variables, following Chyz et al. (2013). (6) takes the base model reported in (1) and adds all variables used in (4), (5), and (6). (7) uses LOGIT to estimate equation (1) instead of the Linear Probability Model. (8) deletes firms with more than one instance of *Forced CEO turnover*. (9) eliminates observations with extreme values of *ETR* and *Cash ETR* (i.e., 0 or 1). (10) controls for CEO locality (see Yonker 2016 for variable computation).

Table 7
Taxes and Forced CEO turnover (PSM)

Panel A: Tax indicators based on *ETR*

	Forced CEO turnover					
	Tax indicators based on *ETR*					
	Coefficient	Std. Error		Coefficient	Std. Error	
Intercept	0.0166	0.0083	**	0.0093	0.0083	
Low tax indicator	0.0049	0.0028	**			
High tax indicator				0.0081	0.0029	***
Size	0.0040	0.0008	***	0.0031	0.0008	***
Abnormal stock returns	-0.0159	0.0027	***	-0.0177	0.0028	***
Δ Return on assets	-0.1064	0.0225	***	-0.0902	0.0212	***
Leverage	-0.0009	0.0084		0.0172	0.0086	**
CEO ownership	-0.0071	0.0023	***	-0.0043	0.0022	*
Δ Sales	0.0077	0.0068		0.0102	0.0065	
σ Abnormal stock returns	0.0087	0.0032	***	0.0089	0.0034	***
σ Return on assets	0.1355	0.0369	***	0.2239	0.0363	***
Δ NOL	0.1063	0.0281	***	0.0978	0.0396	**
Prior losses indicator	0.0045	0.0179		0.0014	0.0153	
Managerial ability	-0.0173	0.0107		-0.0156	0.0109	
Meet/Beat indicator	-0.0121	0.0030	***	-0.0063	0.0030	**
Industry fixed effects	Yes			Yes		
Year fixed effects	Yes			Yes		
R-square	0.011			0.013		
N	23,763			23,763		

(continued)

Table 7, continued
Taxes and Forced CEO turnover (PSW)

Panel B: Tax indicators based on *Cash ETR*

	Forced CEO turnover					
	Tax indicators based on *Cash ETR*					
	Coefficient	Std. Error		Coefficient	Std. Error	
Intercept	0.0252	0.0084	***	0.0178	0.0083	**
Low tax indicator	0.0108	0.0029	***			
High tax indicator				0.0044	0.0028	*
Size	0.0034	0.0008	***	0.0029	0.0008	***
Abnormal stock returns	-0.0114	0.0027	***	-0.0261	0.0029	***
Δ *Return on assets*	-0.1399	0.0234	***	-0.0682	0.0209	***
Leverage	-0.0075	0.0084		-0.0056	0.0087	
CEO ownership	-0.0092	0.0024	***	-0.0030	0.0022	
Δ *Sales*	0.0029	0.0070		0.0111	0.0065	*
σ *Abnormal stock returns*	0.0067	0.0032	**	0.0144	0.0034	***
σ *Return on assets*	0.1461	0.0378	***	0.1696	0.0359	***
Δ *NOL*	0.1451	0.0272	***	0.1082	0.0437	**
Prior losses indicator	-0.0015	0.0215		0.0322	0.0145	**
Managerial ability	0.0050	0.0109		-0.0079	0.0107	
Meet/Beat indicator	-0.0048	0.0030		-0.0108	0.0030	***
Industry fixed effects	Yes			Yes		
Year fixed effects	Yes			Yes		
R-square	0.013			0.014		
N	23,763			23,763		

(continued)

Table 7, continued
Taxes and Forced CEO turnover (PSW)

Panel C: Pre and post differences in *Tax indicator* determinants

	Tax indicators based on *ETR*				**Tax indicators based on *Cash ETR***			
	Treatment = *Low tax indicator*				Treatment = *Low tax indicator*			
	Differences in means between Control and Treatment samples (full sample)	std. error for test of difference in means	Differences in means between Control and Treatment samples (matched sample)	std. error for test of difference in means	Differences in means between Control and Treatment samples (full sample)	std. error for test of difference in means	Differences in means between Control and Treatment samples (matched sample)	std. error for test of difference in means
ROA	-0.036	0.002 ***	-0.001	0.004	-0.036	0.002 ***	-0.002	0.003
LEV	-0.007	0.003 *	-0.003	0.006	0.030	0.004 ***	0.004	0.006
Δ NOL	-0.013	0.001 ***	0.000	0.001	-0.016	0.001 ***	0.007	0.001
FI	0.004	0.000 ***	0.001	0.001	-0.004	0.000 ***	0.001	0.001
PPE	-0.022	0.004 ***	0.004	0.010	0.003	0.004	-0.004	0.008
INTANG	-0.023	0.003 ***	-0.002	0.007	-0.014	0.003 ***	0.003	0.006
EQINC	0.000	0.000 ***	0.000	0.000	0.000	0.000	0.000	0.000
SIZE	-0.600	0.029 ***	-0.041	0.065	-0.853	0.029 ***	-0.070	0.061
MB	0.024	0.040	0.097	0.077	-0.057	0.040	0.160	0.084 *
	Treatment = *High tax indicator*				Treatment = *High tax indicator*			
	Differences in means between Control and Treatment samples (full sample)	std. error for test of difference in means	Differences in means between Control and Treatment samples (matched sample)	std. error for test of difference in means	Differences in means between Control and Treatment samples (full sample)	std. error for test of difference in means	Differences in means between Control and Treatment samples (matched sample)	std. error for test of difference in means
ROA	-0.048	0.100 ***	0.001	0.003	-0.065	0.002 ***	0.006	0.004 *
LEV	0.038	0.003 ***	0.004	0.005	0.007	0.003 **	-0.003	0.006
Δ NOL	-0.001	0.001	0.000	0.001	0.003	0.000 ***	0.001	0.001
FI	-0.003	0.000 ***	0.000	0.001	-0.005	0.000 ***	-0.001	0.001
PPE	-0.041	0.004 ***	-0.001	0.008	-0.056	0.004 ***	-0.007	0.007
INTANG	0.038	0.003 ***	0.005	0.005	0.004	0.003	0.002	0.005
EQINC	0.000	0.000 ***	0.000	0.000	0.000	0.000 ***	0.000	0.000
SIZE	-0.633	0.029 ***	-0.062	0.057	-0.539	0.029 ***	-0.025	0.057
MB	-0.514	0.038 ***	0.005	0.067	-0.657	0.038 ***	0.071	0.074

(continued)

Table 7, continued
Taxes and Forced CEO turnover (PSW)

This table reports results for our main tests examining the effect of taxes on forced CEO turnover after employing propensity score weighting. Panel A reports estimates of equation (1) using tax indicators based on *ETR*, while Panel B reports estimates of equation (2) using tax indicators based on *Cash ETR*. In both cases we first calculate propensity scores by estimating a logistic model examining the likelihood of being in the bottom and top quintiles of *ETR* and *Cash ETR*, using tax avoidance determinants from Chyz et al. (2013). We then use weights created from the propensity scores and weight our second-stage model by these weights. Panel C reports tests of differences in means to examine covariate balance of determinants of tax rates. Regression coefficients and standard errors are reported side-by-side. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively for one-sided tests examining H1 and H2, two-tailed otherwise. *Forced CEO turnover* is 1 in cases where Fee et al. (2013) code the turnover event as an overt firing or a suspected force out, and 0 otherwise. *ETR* (*Cash ETR*) is the three-year sum of tax expense (cash paid for taxes) divided by the three-year sum of pretax income. *ETR* and *Cash ETR* are winsorized to 0 and 1, are set to missing when their respective denominators are non-positive, and are normalized by size, industry, and year. *Low tax indicator (ETR)* and *Low tax indicator (Cash ETR)* are set to 1 for observations in the lowest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *High tax indicator (ETR)* and *High tax indicator (Cash ETR)* are set to 1 for observations in the highest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *Size* is the natural log of market value of equity. *Abnormal stock returns* is the annual stock return minus the value weighted return for the S&P 500 for fiscal year *t*. Δ*Return on assets* is the change in pretax income scaled by total assets. *Leverage* is long-term debt (including the current portion) divided by total assets. *CEO ownership* is the percentage of shares outstanding owned by the CEO. Δ*Sales* is the change in sales scaled by total assets. σ*Abnormal stock returns* is the three-year standard deviation of abnormal stock returns. σ*Return on assets* is the three-year standard deviation of pretax income scaled by total assets. Δ*NOL* is the change in tax loss carryforwards scaled by total assets. *Prior loss indicator* is a binary variable indicating the presence of losses in the current or prior year. *Managerial ability* follows Demerjian et al. (2012). *Meet/Beat indicator* is a binary variable indicating the meeting or beating of the consensus analyst forecast for the current period. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 8
Taxes and Forced CEO turnover before and after SOX

$$Forced\ CEO\ turnover_{i,t+1} = \alpha + \beta_1 Low\ tax\ indicator_{i,t} + \beta_2 High\ tax\ indicator_{i,t} + \beta_3 SOX_{i,t} + \beta_4 Low\ tax\ indicator_{i,t} * SOX_{i,t} + \beta_5 High\ tax\ indicator_{i,t} * SOX_{i,t} + \beta_k Controls_{i,t} + I_i + T_t + \varepsilon_{i,t}$$

	Forced CEO turnover					
	Tax indicators based on *ETR*			Tax indicators based on *Cash ETR*		
	Coefficient	Std. Error		Coefficient	Std. Error	
Intercept	0.0251	0.0055	***	0.0245	0.0055	***
Low tax indicator	0.0027	0.0038		0.0041	0.0038	
High tax indicator	0.0111	0.0042	***	0.0117	0.0038	***
SOX	-0.0028	0.0037		-0.0014	0.0038	
*Low tax indicator*SOX*	0.0125	0.0075	**	0.0150	0.0074	**
*High tax indicator*SOX*	-0.0029	0.0074		-0.0095	0.0075	
Size	0.0038	0.0007	***	0.0038	0.0007	***
Abnormal stock returns	-0.0192	0.0024	***	-0.0193	0.0024	***
Δ Return on assets	-0.1066	0.0216	***	-0.1035	0.0191	***
Leverage	0.0013	0.0078		0.0015	0.0076	
CEO ownership	-0.0049	0.0016	***	-0.0048	0.0021	**
Δ Sales	0.0088	0.0063		0.0085	0.0060	
σ Abnormal stock returns	0.0103	0.0032	***	0.0103	0.0029	***
σ Return on assets	0.1816	0.0349	***	0.1786	0.0324	***
Δ NOL	0.0989	0.0288	***	0.1020	0.0287	***
Prior losses indicator	0.0161	0.0157		0.0151	0.0127	
Managerial ability	-0.0018	0.0097		-0.0013	0.0098	
Meet/Beat indicator	-0.0076	0.0027	***	-0.0078	0.0027	***
Industry fixed effects	Yes			Yes		
Year fixed effects	No			No		
R-square	0.011			0.011		
N	29,703			29,703		

(continued)

Table 8, continued
Taxes and Forced CEO turnover before and after SOX

This table reports results for our main tests examining the effect of taxes on forced CEO turnover, after conditioning our tests on SOX. Regression coefficients and standard errors are reported side-by-side. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively for one-sided tests examining H1 and H2, two-tailed otherwise. *Forced CEO turnover* is 1 in cases where Fee et al. (2013) code the turnover event as an overt firing or a suspected force out, and 0 otherwise. *ETR* (*Cash ETR*) is the three-year sum of tax expense (cash paid for taxes) divided by the three-year sum of pretax income. *ETR* and *Cash ETR* are winsorized to 0 and 1, are set to missing when their respective denominators are non-positive, and are normalized by size, industry, and year. *Low tax indicator (ETR)* and *Low tax indicator (Cash ETR)* are set to 1 for observations in the lowest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *High tax indicator (ETR)* and *High tax indicator (Cash ETR)* are set to 1 for observations in the highest quintiles of *ETR* and *Cash ETR*, respectively; and 0 otherwise. *SOX* is 1 for the time period following the enactment of Sarbanes Oxley, and 0 otherwise. *Size* is the natural log of market value of equity. *Abnormal stock returns* is the annual stock return minus the value weighted return for the S&P 500 for fiscal year *t*. Δ*Return on assets* is the change in pretax income scaled by total assets. *Leverage* is long-term debt (including the current portion) divided by total assets. *CEO ownership* is the percentage of shares outstanding owned by the CEO. *ΔSales* is the change in sales scaled by total assets. *σAbnormal stock returns* is the three-year standard deviation of abnormal stock returns. *σReturn on assets* is the three-year standard deviation of pretax income scaled by total assets. *ΔNOL* is the change in tax loss carryforwards scaled by total assets. *Prior loss indicator* is a binary variable indicating the presence of losses in the current or prior year. *Managerial ability* follows Demerjian et al. (2012). *Meet/Beat indicator* is a binary variable indicating the meeting or beating of the consensus analyst forecast for the current period. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t.*

Table 9
Tax outcomes before and after forced turnover

Panel A: Tax indicators based on *ETR*

Low tax indicator = 1

	# of firms	before turnover	after turnover	s.e. (diff. in means)	
ETR	71	0.211	0.262	0.028	**
σETR	64	0.088	0.108	0.019	

Low tax indicator = 0 and *High tax indicator* = 0

	# of firms	before turnover	after turnover	s.e. (diff. in means)	
ETR	232	0.355	0.347	0.008	
σETR	228	0.042	0.047	0.007	

High tax indicator = 1

	# of firms	before turnover	after turnover	s.e. (diff. in means)	
ETR	55	0.485	0.396	0.037	**
σETR	53	0.139	0.070	0.024	***

Panel B: Tax indicators based on *Cash ETR*

Low tax indicator = 1

	# of firms	before turnover	after turnover	s.e. (diff. in means)	
Cash ETR	65	0.117	0.219	0.035	***
σ*Cash ETR*	50	0.080	0.118	0.023	*

Low tax indicator = 0 and *High tax indicator* = 0

	# of firms	before turnover	after turnover	s.e. (diff. in means)	
Cash ETR	245	0.311	0.322	0.018	
σ*Cash ETR*	213	0.121	0.140	0.012	

High tax indicator = 1

	# of firms	before turnover	after turnover	s.e. (diff. in means)	
Cash ETR	48	0.506	0.359	0.055	***
σ*Cash ETR*	42	0.215	0.143	0.033	**

This table reports means and standard deviations for *ETR* and *Cash ETR* for firms experiencing forced turnover, conditioning on tax indicators. Standard errors for their respective differences are also presented. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively for tests of difference in means. Here, *ETR* (*Cash ETR*) is the one-year tax expense (cash paid for taxes) divided by pretax income. *ETR* and *Cash ETR* are winsorized to 0 and 1 and are set to missing when their respective denominators are non-positive. Both variables are not normalized for this analysis. Reported *ETR* and *Cash ETR* are one-year rates for t–1 and t+1. Standard deviation of *ETR* and *Cash ETR* are computed over a three-year period and are reported for t–3 and t+3.