Can paying "too much" tax contribute to forced CEO turnover?

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November 2015

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 mixed evidence of a relation between the payment of low taxes and forced turnover. We do, however, find that forced CEO turnover is more likely when the firm pays a high tax rate. Our results are consistent with the existence of individual reputational costs for not engaging in tax avoidance.

Keywords: CEO turnover, tax avoidance
JEL classification: M40, H25

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We thank Edward Fee, Charles Hadlock, and Joshua Pierce for allowing us to use their data of exogenous and endogenous CEO departures. We are grateful for helpful comments from Qiang Cheng, Alex Edwards, John Gallemore, Michelle Harding, Erin Henry, Christina Lewellen, Brad Lindsay, Dan Lynch, Rick Morton, Terry Shevlin, Tom Linsmeier, Jake Thornock, Erin Towery, and seminar participants at the University of Wisconsin-Madison and Florida State University. We are thankful for the financial support of our respective institutions: the University of Tennessee (Chyz) and University of Wisconsin-Madison (Gaertner).

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; Chen, Huang, Li, and Stanfield 2012; Graham, Hanlon, Shevlin, and Shroff 2013).[1] 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 focus on the opposite of the traditional reputation cost 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 Armstrong, Blouin, Jagolinzer, and Larcker (2015). Our choice of benchmarked effective tax rates stems from 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. We examine forced CEO turnover because it represents a deliberate action by the board to modify the firm's direction, strategy, and

[1] Studies discussing the possibility of reputational costs include: Desai and Dharmapala (2006), Chen, Huang, Li, and Stanfield (2012), Graham, Hanlon, Shevlin, and Shroff (2013), Armstrong, Blouin, Jagolinzer, and Larcker (2015), among others.

leadership (Fee et al., 2013). We focus on CEOs rather than tax directors, who are directly responsible for the tax function of the firm, for three primary reasons.[2] 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, however, is less certain. A growing stream of the literature has investigated the links between CEOs and corporate taxes and taxes including Chyz, Gaertner, Kausar, and Watson (2015), Olsen and Stekelberg (2015), Gaertner (2014), Chyz (2013), Rego and Wilson (2012), and Dyreng, Hanlon, and Maydew (2010). Despite 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. Our main analysis extends our univariate analysis into three sets of multivariate regressions. First, we 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 tax rate quintile, controlling for year effects and turnover determinants from prior literature. Second, we perform our linear probability regression analysis on samples matched by propensity scores.[3] Lastly, to

[2] 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.

[3] Propensity score matching is done separately for *Low tax indicator* and *High tax indicator* tests.

address concerns that unobservable firm characteristics impact our ability to generate meaningful inferences, we perform our linear probability regression analysis while including firm fixed effects. Employing firm fixed effects forces our coefficients to only capture within-firm effects, thus minimizing endogeneity concerns. Our multivariate results confirm univariate findings, indicating that CEOs are indeed more likely to experience forced turnover when benchmarked tax rates are high. We find mixed evidence regarding the relation between low tax rates and forced CEO turnover.

In additional 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 exogenous turnover events. When we examine unforced CEO turnovers we find no evidence of a positive association between turnover and either the high or low effective tax rate treatment firms. These falsification tests provide further support for the inferences documented in our primary tests.

Our primary results are robust to different regression distributional assumptions and alternative sample selection restrictions. Our primary results also continue to hold after controlling for a number of alternative explanations including (1) managerial ability, (2) governance, (3) stock price performance, and (4) pretax accounting performance. Addressing pretax accounting performance as an alternative explanation is important in our setting 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. In the spirit of Henry and Sansing (2015) we offer a number of alternative effective tax rate scalars to address such concerns and continue to document consistent results. We also document consistent results after dropping firms in the bottom half of our sample's return on assets and when allowing for nonlinearities in performance measures. Finally, we examine the sensitivity of our results to several additional items, including distributional assumptions of our empirical models (i.e., logit versus LPM), and continue to find similar results.

In additional analysis we show that taxes have a unique role in CEO turnover. In some settings boards may be less concerned with tax outcomes. We argue firms with analyst coverage is such a setting. Recent remarks by a senior financial analyst speaking on behalf of herself and other analysts at the 2015 Illinois Tax Symposium confirmed that operating income (i.e., earnings before taxes) is seen as the most important driver of earnings. Thus, increases in pretax earnings are given more weight by analysts in their recommendations than reductions in income tax expense. The speaker also indicated that financial analysts largely ignore variation in corporate tax strategies, believing that performing a detailed tax analysis does not present analysts with a meaningful opportunity to generate alpha. Thus, to the extent boards and CEOs prioritize pretax performance to satisfy analysts (at the expense of tax strategy), our results should be weaker for firms with analyst coverage. In cross-sectional analysis this is indeed what we find. Specifically the relation between paying relatively high taxes and CEO turnover is weaker for firms with analyst coverage.

Our findings yield a number of important insights. First, across all regression specifications we document a statistically and economically significant relation between paying higher peer-adjusted taxes and forced CEO turnover. Given the unconditional probability of CEO turnover in our sample of 4.72%, the coefficient magnitudes in our regression analyses suggest that firms paying high taxes relative to peers have turnover rates 17% to 25% 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 (Desai and Dharmapala, 2006; Chen, et al., 2012; Graham, et al., 2014; Armstrong, et al., 2015; among others).

Second, we document mixed evidence of a relation between paying lower peer-adjusted taxes and forced CEO turnover. Our low tax firm indicator is economically and statistically significant in our base sample and propensity scored matched sample linear probability regressions. Given the unconditional probability of CEO turnover in our sample of 4.72%, the coefficient magnitudes in the first two sets of regression analyses suggest that firms paying lower taxes relative to peers have turnover rates 19% to 25% higher than non-treatment firms. Our results are mixed when our models include firm fixed effects. Our results linking low taxes and forced turnover lose statistical significance after including firm fixed effects in our main specification, but regain significance in several models reported under robustness tests. Despite the mixed nature of our results, 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.

Third, we add to the literature that examines the role of CEOs on corporate tax outcomes (Chyz et al., 2015; Olsen and Stekelberg, 2015; Gaertner (2014); Chyz, 2013; Rego and Wilson

2012; Dyreng et al., 2010). 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.

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 discuss our main results. We summarize our findings and conclude in Section 5.

II. RELATED LITERATURE AND HYPOTHESIS DEVELOPMENT

Corporate effective tax rates

Recent studies present evidence of substantial variation in effective tax rates (Dyreng et al., 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. Rego and Wilson (2012) and Gaertner (2014) find links between CEO compensation and corporate tax avoidance. 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, Wolfson, Erickson, Hanlon, Maydew and

Shevlin, 2014) 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.[4] 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; Armstrong, et al., 2015).

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 Graham et al. (2013).[5] 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) survey results suggest that 69% of executives cite "potential harm to firm reputation" as a reason

[4] See Hanlon and Heitzman (2010), Maydew (2001), and Shackelford and Shevlin (2001) for comprehensive summaries on the related literature.
[5] Dyreng, Hoopes, and Wilde (2015) document decreases in tax rates for large U.K. firms following public scrutiny of firm subsidiary locations.

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 other tests aimed at quantifying both firm and executive level reputational effects, the authors examine the likelihood of CEO turnover following 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.[6] In addition, our study extends prior literature by distinguishing between forced and unforced CEO turnover. Fee et al. (2013) suggest that failing to distinguish between forced and unforced turnover could introduce noise or even bias empirical tests. This leads 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.*

[6] In other words, Gallemore et al. (2014)'s approach attempts to link evidence of low taxes to executive turnover, while our approach is to look at a much larger sample of CEO turnover and examine whether there is evidence of low taxes preceding forced CEO turnover.

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. 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 individual CEOs impacting corporate tax avoidance. For example, Dyreng et al. (2010) find an economically significant CEO effect on tax avoidance and Chyz (2013) finds that personally tax aggressive executives tend to engage in more tax sheltering. Chyz et al. (2015) and Olsen and Stekelberg (2015) find that CEO overconfidence and narcissism are related to corporate tax outcomes. Rego and Wilson (2012) and Gaertner (2014) find links between CEO compensation and tax avoidance. Results from these studies suggest a link between CEOs their firms' corporate tax outcomes. 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."[7]

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

Focusing on CEOs as we do and not line-level managers is consistent with Dyreng et al. (2010) and Rego and Wilson (2012), who 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

[7] Abbreviated transcript available at http://www.cbsnews.com/news/a-look-at-the-worlds-new-corporate-tax-havens-25-03-2011/.

introduced by Hambrick and Mason (1984). An alternative view is that CEOs are rarely if ever tax experts and may not be involved in the selection and implementation of tax strategies (Dyreng et al., 2010). 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 tend to 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 a component of the CEO's corporate tax management. Whether boards ultimately hold the CEO responsible for corporate tax management is an empirical question.

III. DATA, MEASURES, AND RESEARCH DESIGN

Data

We begin with a sample of firms experiencing endogenous or 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 the unforced turnover sample 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 "suspect forced departures" which are not identified as unforced and that relate to a 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.

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. This procedure yields a final sample of 33,955 firm-years (6,184 firms) from 1993 to 2006 for our full multivariate model (see Table 5).[8] Our sample covers a period with consistent accounting for income taxes (i.e., post FAS 109/ASC 740

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

implementation) and with an unchanged top corporate statutory tax rate of 35%. Table 1, Panel B summarizes the annual distribution of forced turnover events. The frequency of forced turnover is highest in 1999 with 159 events and lowest in 2001 with 81 events. The mean annual turnover over our sample period is approximately 114.

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 annually, capture the amount of tax firms pay relative to their pretax accounting income. For each firm-year we construct industry 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 (Armstrong et al., 2015).

Cash ETR reflect firms' actual cash tax payments for a given level of pretax income. *ETR* reflects firms' 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 contingency (tax cushion) (Dyreng, Hanlon, and Maydew, 2008). We examine both GAAP and cash effective tax rates because Graham, Hanlon, Shevlin, and Shroff (2014) 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 the GAAP. 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 that tax watch dog groups like Citizens for Tax Justice focus on effective tax rates.[9] To the extent that 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, Hanlon, Shevlin, and Shroff (2015) suggest that 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 that boards are in constant review of firms' effective tax rates.

ETR is estimated as total tax expense (#TXT) divided by pretax income (#PI).[10] We set *ETR* equal to zero in the case of tax refunds, and equal to one when *ETR* is greater than one. Both measures are set to missing when pretax income is non-positive. *Cash ETR* is total cash paid for taxes divided by pretax income. Similar to *ETR* we set *Cash ETR* equal to zero for tax

[9] News and Analysis – Economic Analysis. February 13, 2012.
[10] Unless stated otherwise, all data references correspond to Compustat XpressFeed annual data items.

refunds, and equal to one when *Cash ETR* is greater than one. Lastly, we normalize *ETR* and *Cash ETR* by size, industry, and year; so that our measures of tax performance are benchmarked to other firms of similar size in the same industry consistent with Armstrong et al. (2015).

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:[11]

$$\begin{aligned} \textit{Forced CEO turnover}_{i,t+1} = {} & \beta_1 \textit{Low tax indicator}_{i,t} + \beta_2 \textit{High tax indicator}_{i,t} + \beta_3 \textit{Size}_{i,t} \\ & + \beta_4 \textit{Abnormal stock returns}_{i,t} + \beta_5 \textit{Return on assets}_{i,t} \\ & + \beta_6 \textit{Leverage}_{i,t} + T_t + \varepsilon_{i,t} \end{aligned} \quad (1)$$

Variable definitions for equation (1) are as follows: *Forced CEO turnover* is an indicator variable capturing forced CEO turnover events 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;[12] *Low tax indicator* is an indicator variable equal to 1 for observations in the lowest quintile of *ETR* /

[11] Subscript *i* denotes firm and *t* denotes year. Unless stated otherwise, all input data for control variables are calculated as of time *t*. We use a linear probability model to simplify the interpretation of coefficients. Linear probability models 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). As noted and reported later in our discussion of robustness tests, we find that our inferences are unchanged using a logit specification.

[12] 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.

Cash ETR, and 0 otherwise; *High tax indicator* is an indicator equal 1 for observations in the highest annual quintile of *ETR* / *Cash ETR*, and 0 otherwise, *T* is a vector of year fixed effects, and ε is a disturbance term with mean zero.[13] Our inclusion of year fixed effects removes the effects of macroeconomic conditions that might be associated with forced CEO 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.

We also present results of equation (1) using propensity score matched samples. Propensity score matching is done separately for *Low tax indicator* and *High tax indicator* tests. We match treated observations to observations from the middle three tax quintiles with the closest propensity score match, within the same industry and year as the treated firm. Propensity scores are obtained by estimating each treatment indicator on the turnover determinants examined in Gallemore et al. (2014).

Finally we present results of equation (1) after including firm fixed effects. Adding firm fixed effects forces the analysis to be conducted using only within-firm variation; therefore the firm acts as its own control. As a result our variables of interest compare a firm's benchmarked effective tax rate quintile assignment immediately preceding the turnover event relative to its average benchmarked effective tax rate quintile assignment over the sample period. Our model effectively tests whether the probability a CEO is fired increases if his or her firm starts to pay the lowest amount of tax relative to peers (*Low tax indicator*) or the highest amount of tax

[13]We form quintiles annually with the pooled sample of peer adjusted tax rates. Our inferences are largely unchanged if we form annual quintiles within industry-size matched peer groups.

relative to peers (*High tax indicator*). If a firm is consistently paying low or high taxes relative to peers, then there would not be any variation at the firm level and these firms would not impact our variables of interest.

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 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 draw control variables from Gallemore et al. (2014), as it is the only paper of which we are aware that examines executive turnover in a tax setting.[14] As such, our initial set of control variables includes *Size*, *Abnormal stock returns*, *Return on assets*, and *Leverage*. *Size* is the natural log of total assets (#AT). *Abnormal stock returns* is the annual stock return for the fiscal year minus the value weighted return for the S&P 500. *Return on assets* is pretax income divided by total assets (#PI/#AT). *Leverage* is long term debt (including the current portion) divided by total assets (#DLC+#DLTT) / #AT.

IV. EMPIRICAL RESULTS

Descriptive statistics

[14] In robustness tests we report several modifications to this model.

Table 2 presents descriptive statistics conditioned on *ETR* quintiles. Panel A presents descriptive statistics observations in the first quintile of *ETR*, Panel B for observations in quintiles two through four, while Panel C presents statistics for observations in the fifth quintile. Raw *ETR* reports *ETR* before benchmarking by industry, year, and size; while *ETR* reports the benchmarked measure. On average, firms in the bottom *ETR* quintile have tax rates that are 12.5 percentage points lower than their peers. On average, firms in the top *ETR* quintile have tax rates that are 17.3 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. Firms in the middle quintiles have similar stock returns as those in the fifth quintile. Firms in the middle three quintiles have the highest *Return on assets*. This difference in *Return on assets* between the first and middle quintiles is a relatively modest 3.6 percentage points. The difference between the middle quintiles and highest quintile is a smaller 2.3 percentage points *Leverage* is fairly similar across all three partitions.

Table 3 presents descriptive statistics conditioned on *Cash ETR* quintiles. Panel A presents descriptive statistics observations in the first quintile of ETR, Panel B for observations in quintiles two through four, while Panel C presents statistics for observations in the fifth quintile. 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.

Overall both Table 2 and Table 3 support H1 and H2. However, univariate results should be interpreted with caution as they do not offer within-firm comparisons or control for firm characteristics, which mitigate concerns over endogeneity.

Correlations

Table 4 presents Pearson correlation coefficients for the main variables used in our study. Correlation coefficients are accompanied by p-values below each coefficient estimate. Consistent with univariate results presented earlier, low and high tax indicators (based on both *ETR* and *Cash ETR*) are positively correlated with *Forced CEO turnover*. These correlations are only statistically significant for the *Low tax indicator (ETR)* (p-value = 0.005) and for *High tax indicator (Cash ETR)* (p-value < 0.001). The P-value for the *Low tax indicator* (*Cash ETR*) is 0.118 and 0.103 for the *High tax indicator* (*ETR*).

Our correlation analysis also reveals some other relations. Larger firms appear more likely to experience forced turnover as revealed by the correlation coefficient on *Size* of 0.018 (p-value = 0.001). *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. Leverage is not significantly correlated with *Forced CEO turnover*.

Forced turnover by tax quintiles

In Figure 1 we plot the likelihood of forced CEO turnover by tax 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 relative effective tax rates are lowest). Under H2 we expect means realization of forced CEO turnover to be higher in Q5 (when relative effective tax rates are highest). Consistent with our earlier results, Figure 1 shows that 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 H1 and H2.

Effective tax rates before and after forced turnover

In Figure 2 we plot the time-series of effective tax rates before and after a forced CEO turnover. Panel A presents results using raw *ETR*. Panel B presents results using raw *Cash ETR*. Panel C presents results using peer-adjusted *ETR* and Panel D presents results using peer-adjusted *Cash ETR*. For each panel we present both mean and median realizations. For all graphs, the year of turnover is equal to 0. We show the mean and median realizations 1, 2 and 3 years before and after the turnover year.

In seven of the eight cases, the tax rates are highest in the year when CEOs are forced out. Because these graphs are not conditioned on tax rate quintile, they suggest that the effect of paying high taxes on CEO turnover dominates the effect of paying low taxes. We also find that in most cases there is an upward trend in effective tax rates. In all cases we find a downward trend in effective tax rates after forced CEO turnover. This latter result would be consistent with the assertion that boards fire CEOs, at least in part, to change corporate tax policy.

Multivariate results

Table 5 reports multivariate estimates of *Forced CEO turnover* on *Low tax indicator* and *High tax indicator*. Panel A of Table 5 reports estimates using tax indicators based on *ETR*, while Panel B reports estimates using tax indicators based on *Cash ETR*. Column (1) reports estimates for equation (1) using our full sample but excluding firm fixed effects. Columns (2) and (3) report similar estimates as Column (1) but employs samples matched on propensity scores. Column (4) presents estimates of equation (1), including firm fixed effects.[15] In all cases our multivariate specifications include year fixed effects that control for time-specific effects.

The variable *Low tax indicator* captures the sub-sample of CEOs at firms with low effective tax rates. If CEOs of firms paying little tax relative to peers 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 high effective tax rates. If CEOs of firms paying high effective tax rates experience higher turnover rates then we expect the coefficient on *High tax indicator* to be positive and significant. This would support H2.

Across both panels of Table 5 we find a positive and significant coefficient on *Low tax indicator* for columns (1) and (2), 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. In both panels the coefficient on *Low tax indicator* is no longer significant after including firm fixed effects. The lack of significance on *Low tax indicator* could be consistent with Gallemore et al. (2014) who find no evidence of turnover following news of

[15] For the firms in our sample that do not experience turnover, the firm fixed effect would explain all of the variation in our dependent variable and these firms would not impact any of the tabulated coefficients in our fixed effect model. Dropping firms from our sample that do not experience turnover does not alter our inferences.

reputational costs. However, it could be that employing firm fixed effects might be overly restrictive. Overall, however, there seems to be some multivariate support for H1.

We find a positive and significant coefficient on *High tax indicator* for columns (1) and (3), similar to results on Low tax indicator. However, unlike the results for *Low tax indicator*, we also find a positive and significant coefficient after including firm fixed effects in column (4). The results suggest CEOs are more likely to be fired when firms pay high tax rates; holding the remaining variables constant. The results presented in both panels of Table 5 support H2. While both hypotheses seem descriptive, results supporting H2 appear to be more robust than the results supporting H1.

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* under columns (1) and (2) imply firms paying low tax rates experience incrementally higher CEO turnover rates ranging from 0.9% to 1.1%. Given the unconditional probability of CEO turnover in our sample of 4.72%, these marginal effects suggest that firms paying much lower taxes relative to peers have turnover rates that are anywhere from 19% to 25% higher than the control group of firms. Economic estimates of *High tax indicator* under columns (1), (3), and (4) imply firms paying high tax rates experience incrementally higher CEO turnover rates ranging from 0.8% to 1.2%. These marginal effects suggest that firms paying higher taxes relative to peers have turnover rates that are anywhere from 17% to 25% higher than the control group of firms. Economic magnitudes derived from our multivariate results are generally consistent with those implied in univariate comparisons. In other words, controlling for

firm size, performance, leverage, and forcing all comparisons to be done within firms has little effect on the economic strength of our results; mitigating concerns over endogeneity.

Panels A and B of Table 5 also show that *Forced CEO turnover* is positively associated with *Size*, and that better performance (i.e., *Abnormal stock returns* and *Return on assets*) and higher *Leverage* generally result in lower rates of forced CEO turnover. 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 et al. (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.[16] Unforced CEO departures are unlikely to be the result of organizational stress or crisis that drives board action to deliberately change its 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.

[16] 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.

Panels A and B of Table 6 summarize the results of these tests, structured similarly to those reported in Table 5. After estimating the effect of taxes on unforced CEO turnover we find that the coefficients on *Low tax indicator* and *High tax indicator* are never significant. As such, results from our falsification tests provide additional comfort that the results documented in Table 5 are not spurious.

Robustness

In this sub-section we perform a series of additional tests to ensure the robustness of our results. These tests are organized in five groups: (1) managerial ability as an alternative explanation, (2) pretax performance as an alternative explanation, (3) governance as an alternative explanation, (4) stock price performance as an alternative explanation, and (5) other issues. With all of these tests we begin with our main results (i.e., estimates of equation (1), in Table 5 column 4) and make modifications to this specification to address different issues. Unless stated otherwise, estimation results for each of our tests is presented in Table 7. Table 7 only reports coefficient estimates for *Low tax indicator* and *High tax indicator* for ease of presentation. Full estimation results of alternative specifications of equation (1) are available from the authors by request. Through a battery of robustness tests, our results and inferences generally do not change. As a result, we believe that our results are robust to numerous alternative explanations.

Managerial Ability

Our first test, reported under group (1), examines the possibility that our results are driven by differences in managerial ability. Koester et al. (2015) show that managerial ability is a

determinant of corporate tax avoidance. Therefore, an alternative explanation of our results is that our tax indicators are simply proxies for managerial ability. After adding a measure of managerial ability, consistent with Demerjian et al. (2012) and Koester et al. (2015), as an additional control variable we find similar results to those reported in Table 5; suggesting our effect is incremental to managerial ability.

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 rates (i.e., pretax performance) as opposed to the numerator. While our main specification controls for pretax performance, mitigating this concern, we perform several additional tests under group (2) to ensure that our results are not driven by a denominator effect.

First we form tax indicators based on book-tax differences, as opposed to effective tax rates. Book-tax differences are advantageous in addressing the possibility of a denominator effect in that they are scaled by total assets. We find firms with large negative book-tax differences experience higher forced CEO turnover, consistent with H2. We also adjust *Cash ETR* using the adjustments outlined in Henry and Sansing (2015), who create a variant of cash effective taxes by creating a tax preference variable scaled by market value of assets. When basing our tax indicators on this measure, rather than on *Cash ETR*, we again find support for H2. We then estimate equation (1) only for firms in the top 50th percentile of *Return on assets*. CEOs of these firms should be less likely to be fired for low pretax earnings. After doing so, we again find support for H2 and some support for H1.

We also scale *ETR* and *Cash ETR* by a number of alternative scalars: including market value of assets, market value of equity, book assets, book equity, and sales; rather than pretax income. In all but one instance we find support for H2 across both tax variables. We also find some support for H1 under several of these specifications when our tax indicators are based on *ETR*.

Finally, we allow for non-linear specifications of non-tax performance. We add binary variables indicating low and high *Abnormal stock returns* and *Return on assets* and continue to find consistent results. Overall, our tests overwhelmingly suggest our results are not driven by a denominator effect.

Governance

Our third set of tests examine whether our results can be explained by differences in governance of firms with low or high tax rates. In tests reported under group (3) we add several proxies for governance; including *G_index*, *Institutional ownership*, *Board independence*, and *CEO duality*.[17] In all four of these tests we continue to find strong support for H2. We also find some support for H1.

Stock Market Performance

Our fourth set of tests examines whether our tax indicators are simply proxies for stock market performance. We include abnormal stock returns as a control in our main tests, mitigating this concern. However, we find similar results controlling for *Stock price volatility*, the *# of*

[17] Because of low coverage in some of our governance variables, we follow Hanlon et al. (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.

trading days with large negative stock returns (i.e., daily stock return < 5%), as well as the *Mean return over the 5 worst trading days* of the fiscal year.

Other Tests

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 researchhas 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 H2 across both tax variables. We also find some support for H1 when tax indicators are based on *ETR*.

We also find similar results after deleting firms with multiple turnover events and after deleting firms with extreme *ETRs* / *Cash ETRs* (i.e., 0 and 1). We find consistent results after adjusting the denominator of *ETR* and *Cash ETR* for special items. We also find similar results after controlling for the standard deviation of *ETR* / *Cash ETR*. Finally, we find stronger results after basing our controls on Chyz et al. (2013), which attempts to directly control for determinants of effective tax rates in their models, rather than the turnover model used by Gallemore (2014) in exploring the effects of tax shelter announcements on executive turnover.

Overall, our main results and sensitivity tests overwhelmingly support H2. While we do not find support for H1 under our fully specified estimates of equation (1), we do find support for H1 in tests employing between-firm variation, as well as in several *ETR*-based modified estimates of our fully specified model of equation (1) reported in robustness tests.

Cross-sectional analysis: Taxes, Analysts, and Forced CEO turnover

In our final set of tests we perform a cross-sectional analysis on analyst coverage. These tests provide some evidence that the relation between CEO performance as measured by tax outcomes, is unique to other (non-tax) performance measures.

Recent remarks by a senior financial analyst speaking on behalf of herself and other analysts at the 2015 Illinois Tax Symposium suggested that analysts spend little time on tax forecasting. It was also suggested that analysts tend to weight before tax changes in earnings per share much more heavily than effective tax rate driven changes in earnings per share.[18] Financial analysts also note that all else equal, variation in corporate tax strategies would not represent a meaningful opportunity for analysts to generate alpha. Furthermore, Bradshaw (2011) reminds us that a substantial portion of analyst activity extends beyond earnings forecasting and relates to stock recommendations, growth projections, target prices, and risk ratings. In short, analysts appear to invest very little in understanding the variation in firms' tax outcomes and how these could impact the future profitability of covered firms. Instead, it appears that analysts invest more heavily in understanding and forecasting pretax income. This is important in our setting because managers must be able to understand what information analysts demand and how they process and communicate it (Bradshaw, 2011). To the extent that boards and CEOs prioritize pretax performance to satisfy analysts (at the expense of tax policy management), our results should be weaker for firms with analyst coverage.

In Table 8 we supplement equation (1) with an indicator variable that equals one for firms with analyst following and zero otherwise (*Analyst*). We also interact *Analyst* with our high and low tax indicators. Based on the discussion above, if having analyst following means that tax

[18] Gleason and Mills (2008) show that firms are rewarded less when meeting analyst expectations through tax decreases.

is less likely to be an important incremental performance metric than these interactions should be negative. In all specifications summarized in Panels A and B interactions between both our high and low tax indicators and *Analyst* are negative. However, only the interactions with our high tax indicator are ever statistically significant. These results suggest that relation between paying relatively high taxes and CEO turnover is weaker for firms with analyst coverage. It also suggests that the relation between paying relative low taxes and CEO turnover does not change for firms with analyst coverage.

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 from prior literature thought to predict forced CEO turnover using a sample of endogenous CEO changes from Fee et al. (2013). This allows us to separately identify forced CEO turnover for our main tests and turnover resulting from natural CEO retirement, health reasons and CEO death, which we use for falsification tests. We perform our regressions on a large panel of publicly traded firms with and without forced turnover. We supplement these results with regressions that control for stationary firm characteristics and with propensity score matched samples.

We find some mixed results for the commonnly held view in much of the tax literature that CEOs bear substantial reputational penalties for avoiding too much tax. The stronger result is the opposite of the commonly held view. Specifically, CEOs who do not avoid enough tax are the ones bearing significant reputational costs.

Our study is the first to document evidence consistent with significant reputational consequences from what could be interpreted as CEOs exercising poor tax policy management. We note that it is unclear whether the CEO reputational effect 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. We also note that there are several other possible manifestations for reputational costs (e.g., low sales) that we do not examine. These are questions and areas of inquiry for future research. In any case, our study provides evidence that boards hold CEOs accountable for tax outcomes.

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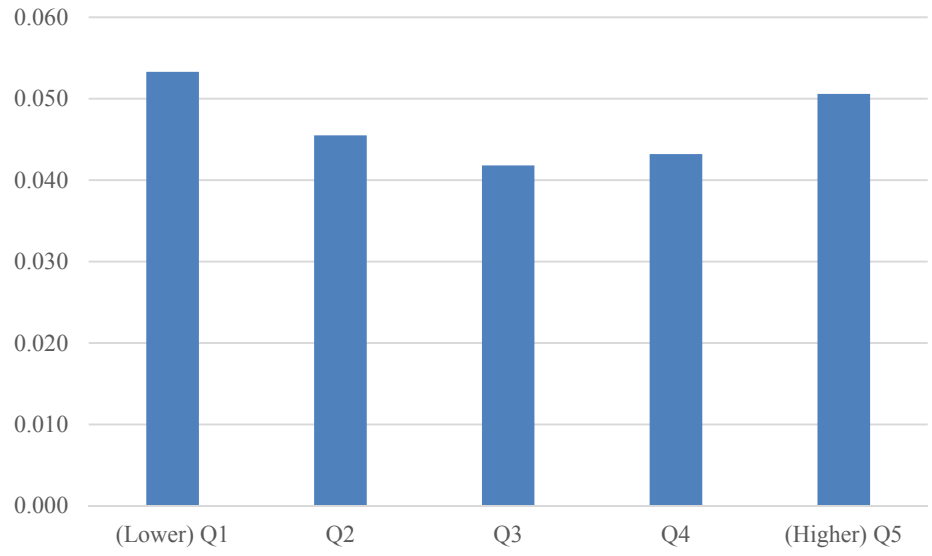
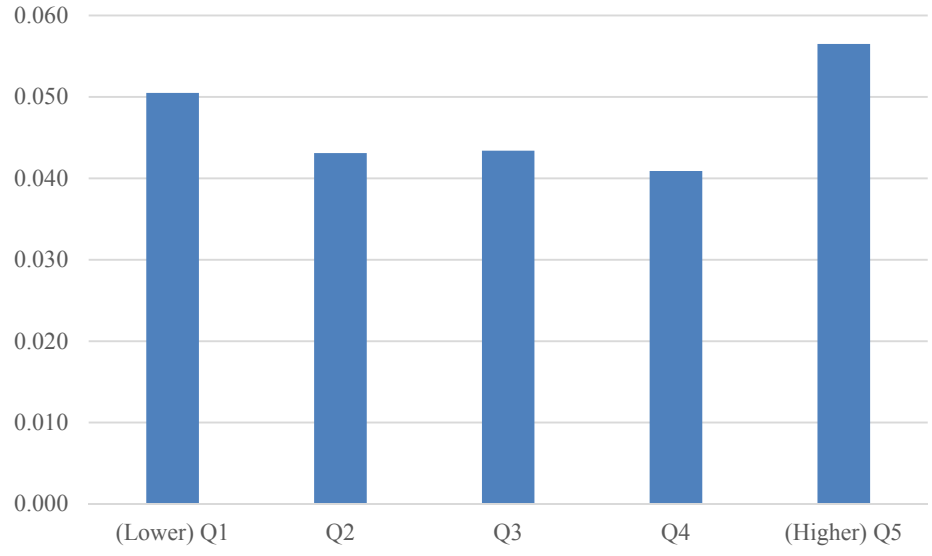
Figure 1
Forced CEO turnover by tax quintiles

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



ETR	Q1	Q2	Q3	Q4	Q5
Mean *Forced CEO turnover*	0.053	0.046	0.042	0.043	0.051

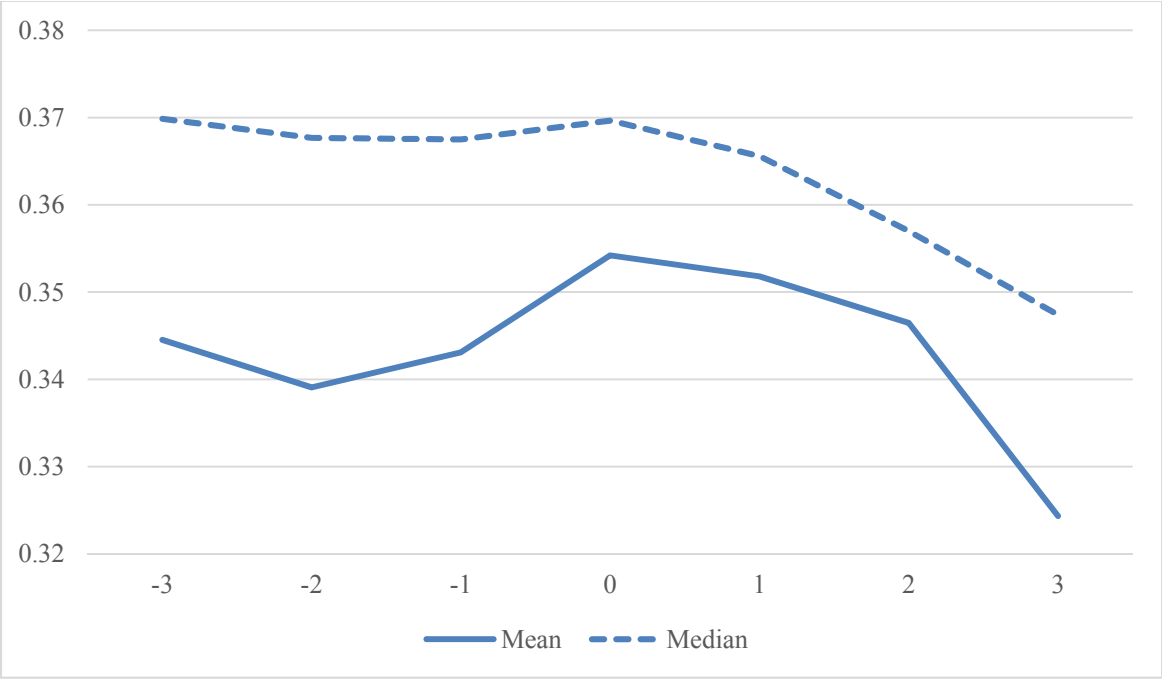
Panel B: *Forced CEO turnover* by *Cash ETR* quintiles



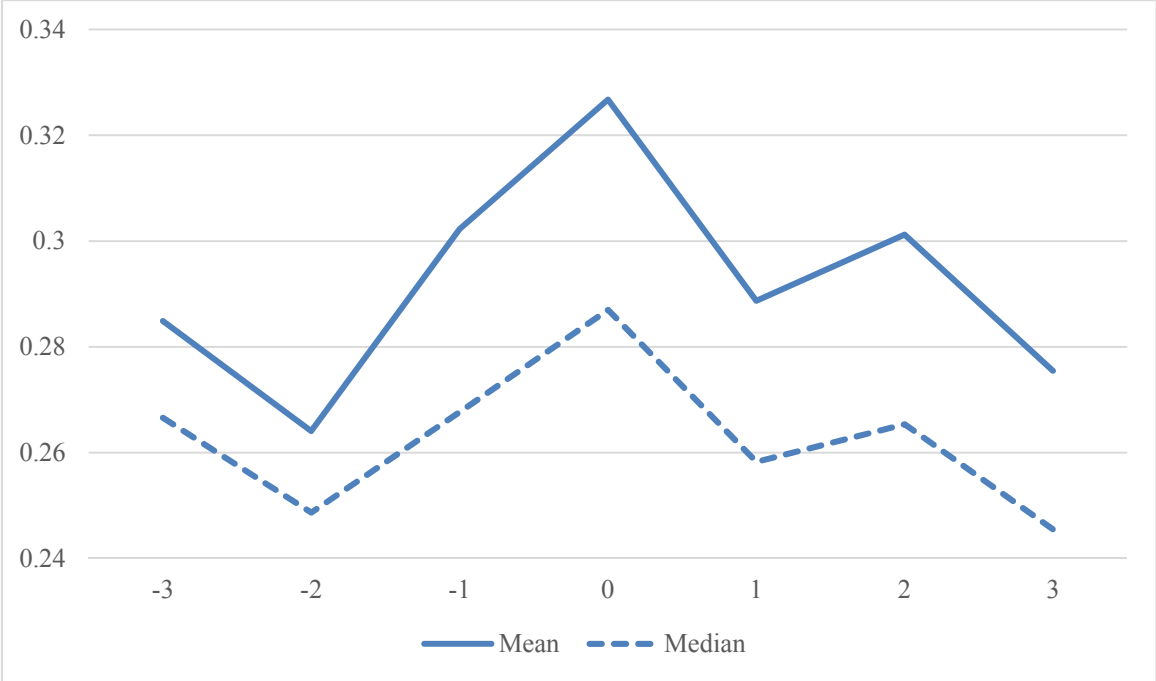
Cash ETR	Q1	Q2	Q3	Q4	Q5
Mean *Forced CEO turnover*	0.051	0.043	0.043	0.041	0.057

Figure 2
Effective tax rates before and after forced CEO turnover

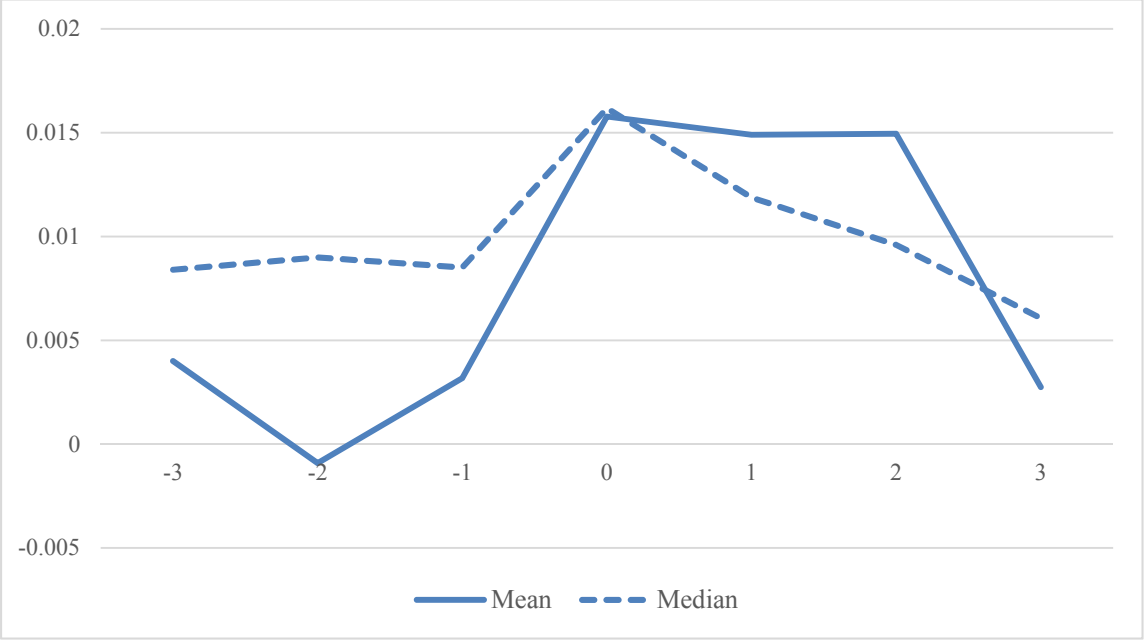
Panel A: *Raw ETR before and after forced CEO turnover*



Panel B: *Raw Cash ETR before and after forced CEO turnover*



Panel C: *Peer-adjusted ETR before and after forced CEO turnover*



Panel D: *Peer-adjusted Cash ETR before and after forced CEO turnover*

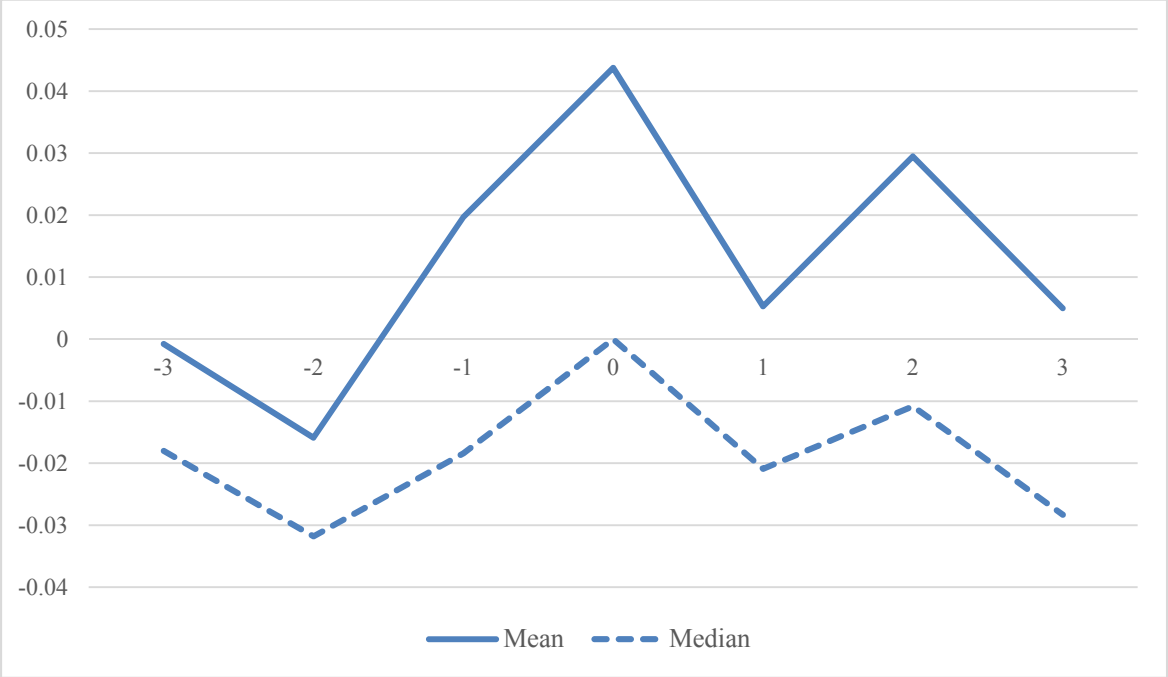


Table 1
Sample selection

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	27,701	
	Negative equity	1,422	
	Firms missing *ETR* or *Cash ETR*	9,124	
	Firms missing *Size*	0	
	Firms missing *Abnormal stock returns*	8,393	
	Firms missing *Return on assets*	0	
	Firms missing *Leverage*	83	114,443
Total firm-year observations			33,955

Panel B: Number of Forced CEO turnover events by year

Year	*Forced CEO turnover*
1993	91
1994	112
1995	130
1996	135
1997	144
1998	153
1999	159
2000	112
2001	81
2002	104
2003	96
2004	94
2005	96
2006	86

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* is tax expense divided by pretax income. *ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *Cash ETR* is the cash paid for taxes divided by pretax income. *Cash ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *ETR* and *Cash ETR* are normalized by size, industry, and year. *Size* is the natural log of total assets. *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. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 2

Descriptive Statistics (*ETR*)

Panel A: Descriptive statistics (Quintile 1 of *ETR*)

Variable	N	Mean	Std Dev	P10	P25	P50	P75	P90
Forced CEO turnover	6,791	0.053	0.225	0.000	0.000	0.000	0.000	0.000
Raw *ETR*	6,791	0.125	0.122	0.000	0.000	0.099	0.229	0.302
ETR	6,791	-0.201	0.092	-0.327	-0.277	-0.195	-0.114	-0.086
Size	6,791	5.462	1.889	3.116	3.992	5.243	6.718	7.984
Abnormal stock returns	6,791	0.210	0.801	-0.473	-0.252	0.022	0.396	1.056
Return on assets	6,791	0.079	0.068	0.013	0.030	0.061	0.108	0.166
Leverage	6,791	0.206	0.187	0.000	0.019	0.178	0.337	0.474

Panel B: Descriptive statistics (Quintiles 2, 3, and 4 of *ETR*)

Variable	N	Mean	Std Dev	P10	P25	P50	P75	P90
Forced CEO turnover	20,373	0.044	0.204	0.000	0.000	0.000	0.000	0.000
Raw *ETR*	20,373	0.359	0.053	0.296	0.336	0.369	0.390	0.409
ETR	20,373	0.009	0.035	-0.041	-0.015	0.011	0.038	0.056
Size	20,373	6.053	1.730	3.930	4.808	5.907	7.163	8.399
Abnormal stock returns	20,373	0.105	0.605	-0.461	-0.240	0.005	0.296	0.708
Return on assets	20,373	0.115	0.074	0.035	0.061	0.099	0.152	0.213
Leverage	20,373	0.205	0.173	0.000	0.038	0.187	0.325	0.446

Panel C: Descriptive statistics (Quintile 5 of *ETR*)

Variable	N	Mean	Std Dev	P10	P25	P50	P75	P90
Forced CEO turnover	6,791	0.051	0.219	0.000	0.000	0.000	0.000	0.000
Raw *ETR*	6,791	0.484	0.175	0.360	0.383	0.415	0.499	0.750
ETR	6,791	0.173	0.144	0.079	0.090	0.116	0.174	0.380
Size	6,791	5.207	1.781	3.134	3.834	4.916	6.350	7.721
Abnormal stock returns	6,791	0.091	0.659	-0.531	-0.310	-0.032	0.308	0.802
Return on assets	6,791	0.092	0.079	0.012	0.031	0.071	0.132	0.200
Leverage	6,791	0.208	0.182	0.000	0.030	0.185	0.340	0.464

This table reports descriptive statistics for the full sample, conditioning on *ETR* quintiles. Panel A reports descriptive statistics for the lowest quintile of *ETR*, Panel B reports descriptive statistics for the middle three quintiles, while Panel C reports descriptive statistics for the highest quintile of *ETR*. *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* is tax expense divided by pretax income, winsorized at 0 and 1, and set to missing when pretax income is non-positive. *ETR* is obtained by normalizing Raw *ETR* by size, industry, and year. *Size* is the natural log of total assets. *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. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 3
Descriptive Statistics (*Cash ETR*)

Panel A: Descriptive statistics (Quintile 1 of *Cash ETR*)

Variable	N	Mean	Std Dev	P10	P25	P50	P75	P90
Forced CEO turnover	6,791	0.051	0.219	0.000	0.000	0.000	0.000	0.000
Raw *Cash ETR*	6,791	0.051	0.062	0.000	0.000	0.029	0.077	0.138
Cash ETR	6,791	-0.247	0.055	-0.322	-0.282	-0.237	-0.202	-0.182
Size	6,791	5.368	1.750	3.133	4.045	5.212	6.531	7.712
Abnormal stock returns	6,791	0.219	0.801	-0.485	-0.264	0.039	0.429	1.085
Return on assets	6,791	0.081	0.067	0.017	0.035	0.064	0.108	0.169
Leverage	6,791	0.231	0.196	0.000	0.032	0.213	0.375	0.512

Panel B: Descriptive statistics (Quintiles 2, 3, and 4 of *Cash ETR*)

Variable	N	Mean	Std Dev	P10	P25	P50	P75	P90
Forced CEO turnover	20,373	0.043	0.202	0.000	0.000	0.000	0.000	0.000
Raw *Cash ETR*	20,373	0.251	0.120	0.074	0.171	0.265	0.338	0.392
Cash ETR	20,373	-0.024	0.079	-0.137	-0.090	-0.020	0.040	0.084
Size	20,373	5.969	1.807	3.696	4.629	5.827	7.163	8.420
Abnormal stock returns	20,373	0.137	0.636	-0.443	-0.220	0.018	0.318	0.769
Return on assets	20,373	0.117	0.075	0.035	0.062	0.102	0.155	0.217
Leverage	20,373	0.199	0.171	0.000	0.033	0.179	0.317	0.438

Panel C: Descriptive statistics (Quintile 5 of *Cash ETR*)

Variable	N	Mean	Std Dev	P10	P25	P50	P75	P90
Forced CEO turnover	6,791	0.057	0.231	0.000	0.000	0.000	0.000	0.000
Raw *Cash ETR*	6,791	0.604	0.229	0.368	0.426	0.522	0.768	1.000
Cash ETR	6,791	0.319	0.198	0.135	0.161	0.235	0.445	0.667
Size	6,791	5.552	1.782	3.357	4.190	5.384	6.664	7.942
Abnormal stock returns	6,791	-0.013	0.547	-0.573	-0.349	-0.090	0.206	0.562
Return on assets	6,791	0.083	0.075	0.009	0.026	0.062	0.118	0.184
Leverage	6,791	0.201	0.174	0.000	0.031	0.179	0.325	0.444

This table reports descriptive statistics for the full sample, conditioning on *Cash ETR* quintiles. Panel A reports descriptive statistics for the lowest quintile of *Cash ETR*, Panel B reports descriptive statistics for the middle three quintiles, while Panel C reports descriptive statistics for the highest quintile of *Cash ETR*. *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 *Cash ETR* is cash paid for taxes by pretax income, winsorized at 0 and 1, and set to missing when pretax income is non-positive. *Cash ETR* is obtained by normalizing Raw *Cash ETR* by size, industry, and year. *Size* is the natural log of total assets. *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. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 4
Correlations

	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1) *Forced CEO turnover*	0.015	0.009	0.009	0.023	0.018	-0.058	-0.032	-0.003
	0.005	0.103	0.118	<.0001	0.001	<.0001	<.0001	0.643
(2) *Low tax indicator (ETR)*		-0.250	0.304	-0.113	-0.090	0.035	-0.186	-0.010
		<.0001	<.0001	<.0001	<.0001	<.0001	<.0001	0.069
(3) *High tax indicator (ETR)*			-0.087	0.241	-0.163	-0.037	-0.105	0.002
			<.0001	<.0001	<.0001	<.0001	<.0001	0.663
(4) *Low tax indicator (Cash ETR)*				-0.250	-0.108	0.042	-0.161	0.056
				<.0001	<.0001	<.0001	<.0001	<.0001
(5) *High tax indicator (Cash ETR)*					-0.061	-0.114	-0.171	-0.011
					<.0001	<.0001	<.0001	0.040
(6) *Size*						0.012	-0.036	0.273
						0.033	<.0001	<.0001
(7) *Abnormal stock returns*							0.157	-0.062
							<.0001	<.0001
(8) *Return on assets*								-0.386
								<.0001
(9) *Leverage*								

This table reports Pearson correlation coefficients for the full sample, with p-values reported below each correlation. *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* is tax expense divided by pretax income. *ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *Cash ETR* is the cash paid for taxes divided by pretax income. *Cash ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *ETR* and *Cash ETR* 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 total assets. *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. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 5
Taxes and Forced CEO turnover

$$\textit{Forced CEO turnover}_{i,t+1} = \beta_1 \textit{Low tax indicator}_{i,t} + \beta_2 \textit{High tax indicator}_{i,t} + \beta_3 \textit{Size}_{i,t} + \beta_4 \textit{Abnormal stock returns}_{i,t} + \beta_5 \textit{Return on assets}_{i,t} + \beta_6 \textit{Leverage}_{i,t} + F_i + T_t + \varepsilon_{i,t} \quad (1)$$

Panel A: Tax indicators based on *ETR*

	Forced CEO turnover			
	OLS	Propensity Score Match	Propensity Score Match	Firm fixed effects
	(1)	(2)	(3)	(4)
Low tax indicator	0.011 ***	0.012 ***		0.006
	(0.003)	(0.004)		(0.004)
High tax indicator	0.009 ***		0.012 ***	0.008 ***
	(0.003)		(0.003)	(0.004)
Size	0.003 ***	0.003 ***	0.005 ***	0.012 ***
	(0.001)	(0.001)	(0.001)	(0.003)
Abnormal stock returns	-0.013 ***	-0.008 ***	-0.017 ***	-0.014 ***
	(0.002)	(0.002)	(0.003)	(0.002)
Return on assets	-0.055 ***	-0.040	-0.087 ***	-0.053 **
	(0.017)	(0.030)	(0.026)	(0.025)
Leverage	-0.023 ***	-0.025 ***	-0.040 ***	0.008
	(0.007)	(0.011)	(0.011)	(0.015)
Firm fixed effects	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R-square	0.004	0.004	0.008	0.222
N	33,955	13,582	13,582	33,955

(continued)

Table 5, continued
Taxes and Forced CEO turnover

Panel B: Tax indicators based on *Cash ETR*

	Forced CEO turnover			
	OLS	Propensity Score Match	Propensity Score Match	Firm fixed effects
	(1)	(2)	(3)	(4)
Low tax indicator	0.009 ***	0.009 ***		-0.005
	(0.003)	(0.004)		(0.004)
High tax indicator	0.011 ***		0.008 ***	0.008 ***
	(0.003)		(0.004)	(0.003)
Size	0.003 ***	0.003 **	0.003 ***	0.012 ***
	(0.001)	(0.001)	(0.001)	(0.003)
Abnormal stock returns	-0.013 ***	-0.011 ***	-0.024 ***	-0.013 ***
	(0.002)	(0.002)	(0.004)	(0.002)
Return on assets	-0.054 ***	-0.020	-0.076 ***	-0.053 **
	(0.017)	(0.030)	(0.030)	(0.025)
Leverage	-0.023 ***	-0.015	-0.034 ***	0.010
	(0.007)	(0.010)	(0.012)	(0.015)
Firm fixed effects	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R-square	0.004	0.004	0.008	0.222
N	33,955	13,582	13,582	33,955

This table reports results for our main tests examining the effect of taxes on forced CEO turnover. 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-sided tests examining H1 and H2, two-tailed otherwise. The first column uses OLS (Linear Probability Model) specification, the second and third match observations in low and high tax quintiles to observations in tax quintiles 2-4 using propensity score matching, and the fourth column employs firm fixed effects (i.e., equation 1). *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* is tax expense divided by pretax income. *ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *Cash ETR* is the cash paid for taxes divided by pretax income. *Cash ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *ETR* and *Cash ETR* 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 total assets. *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. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 6
Taxes and Unforced CEO turnover

$$\textit{Unforced CEO turnover}_{i,t+1} = \beta_1 \textit{Low tax indicator}_{i,t} + \beta_2 \textit{High tax indicator}_{i,t} + \beta_3 \textit{Size}_{i,t} + \beta_4 \textit{Abnormal stock returns}_{i,t} + \beta_5 \textit{Return on assets}_{i,t} + \beta_6 \textit{Leverage}_{i,t} + F_i + T_t + \varepsilon_{i,t}$$

Panel A: Tax indicators based on *ETR*

	Unforced CEO turnover			
	OLS	Propensity Score Match	Propensity Score Match	Firm fixed effects
	(1)	(2)	(3)	(4)
Low tax indicator	-0.002	0.001		-0.001
	(0.002)	(0.002)		(0.002)
High tax indicator	-0.003		-0.002	-0.004
	(0.002)		(0.002)	(0.002)
Size	0.004 ***	0.003 ***	0.003 ***	0.006 ***
	(0.001)	(0.001)	(0.001)	(0.002)
Abnormal stock returns	-0.002	-0.004 ***	0.003 *	0.000
	(0.001)	(0.001)	(0.002)	(0.001)
Return on assets	0.059 ***	0.094 ***	0.033 **	0.056 ***
	(0.010)	(0.016)	(0.014)	(0.016)
Leverage	0.000	0.003	0.001	-0.010
	(0.004)	(0.006)	(0.006)	(0.010)
Firm fixed effects	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R-square	0.004	0.006	0.004	0.136
N	33,955	13,582	13,582	33,955

(continued)

Table 6, continued
Taxes and Unforced CEO turnover

Panel B: Tax indicators based on *Cash ETR*

	Unforced CEO turnover			
	OLS	Propensity Score Match	Propensity Score Match	Firm fixed effects
	(1)	(2)	(3)	(4)
Low tax indicator	-0.002	-0.002		0.002
	(0.002)	(0.002)		(0.002)
High tax indicator	-0.002		-0.003	-0.001
	(0.002)		(0.002)	(0.002)
Size	0.004 ***	0.002 ***	0.004 ***	0.006 ***
	(0.001)	(0.001)	(0.001)	(0.002)
Abnormal stock returns	-0.002	-0.002	-0.004 *	0.000
	(0.001)	(0.001)	(0.002)	(0.001)
Return on assets	0.059 ***	0.052 ***	0.036 **	0.058 ***
	(0.010)	(0.017)	(0.016)	(0.016)
Leverage	0.000	0.003	-0.009	-0.011
	(0.004)	(0.006)	(0.007)	(0.009)
Firm fixed effects	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R-square	0.004	0.004	0.005	0.135
N	33,955	13,582	13,582	33,955

This table reports results for our falsification tests examining the effect of taxes on unforced CEO turnover. 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-sided tests examining H1 and H2, two-tailed otherwise. The first column uses OLS (Linear Probability Model), the second and third match observations in low and high tax quintiles to observations in tax quintiles 2-4 using propensity score matching, and the fourth column employs firm fixed effects. *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* is tax expense divided by pretax income. *ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *Cash ETR* is the cash paid for taxes divided by pretax income. *Cash ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *ETR* and *Cash ETR* 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 total assets. *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. *Unforced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.

Table 7
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 Dev	Coeff	Std Dev	Coeff	Std Dev	Coeff	Std Dev
	Baseline, see Table 5 panels A and B, model (4)	0.006	0.004	0.008	0.004 ***	-0.005	0.004	0.008	0.003 ***
(1)	Controlling for managerial ability	0.005	0.004	0.008	0.004 ***	-0.005	0.004	0.008	0.003 ***
	Substituting *Book-tax Differences* for *ETR*	0.014	0.004 ***	0.002	0.004				
	Adjusting *Cash ETR* in line with Henry and Sansing (2015)					-0.007	0.003	0.008	0.004 **
	Deleting firms in the bottom half of *ROA*	0.009	0.006 *	0.013	0.006 ***	-0.011	0.006	0.007	0.005 *
	Scaling *ETR* and *Cash ETR* by *MVA,* instead of *PI*	0.006	0.004 *	0.005	0.004 *	-0.002	0.004	0.011	0.004 ***
(2)	Scaling *ETR* and *Cash ETR* by *MVE,* instead of *PI*	0.007	0.004 **	0.005	0.004 *	0.000	0.004	0.007	0.004 **
	Scaling *ETR* and *Cash ETR* by *AT*, instead of *PI*	0.002	0.004	-0.001	0.005	0.000	0.004	0.007	0.004 **
	Scaling *ETR* and *Cash ETR* by *CEQ*, instead of *PI*	0.006	0.004 *	0.006	0.004 *	0.002	0.004	0.008	0.004 **
	Scaling *ETR* and *Cash ETR* by *SALE*, instead of *PI*	0.003	0.004	0.010	0.005 ***	-0.002	0.004	0.010	0.004 ***
	Allow for non-linearities in performance measures	0.005	0.004 *	0.007	0.004 **	-0.005	0.004	0.007	0.003 **
	Controlling for *G_index*	0.005	0.004 *	0.008	0.004 **	-0.005	0.004	0.008	0.003 ***
(3)	Controlling for *Institutional ownership*	0.005	0.004 *	0.008	0.004 **	-0.005	0.004	0.008	0.003 **
	Controlling for *Board independence*	0.005	0.004 *	0.007	0.004 **	-0.005	0.004	0.008	0.003 **
	Controlling for *CEO duality*	0.005	0.004 *	0.007	0.004 **	-0.005	0.004	0.008	0.003 **
	Controlling for *Stock price volatility*	0.005	0.004 *	0.008	0.004 **	-0.005	0.004	0.008	0.003 **
(4)	Controlling for the *# of days with large negative returns*	0.005	0.004 *	0.008	0.004 **	-0.005	0.004	0.008	0.003 **
	Controlling for *Mean return over 5 worst trading days*	0.005	0.004 *	0.007	0.004 **	-0.005	0.004	0.008	0.003 **
	Estimating equation (1) using LOGIT, instead of OLS	0.134	0.091 *	0.142	0.086 **	-0.071	0.086	0.166	0.079 **
	Deleting firms with more than one instance of *Turnover*	0.004	0.004	0.006	0.003 **	-0.003	0.003	0.005	0.003 **
(5)	Eliminate extreme *ETR / Cash ETR* (i.e., 0 or 1)	0.006	0.004 *	0.007	0.004 **	-0.006	0.004	0.006	0.004 **
	Adjusting *ETR / Cash ETR* for special items	0.006	0.004 *	0.005	0.004 *	-0.008	0.004	0.008	0.003 **
	Controlling for *Standard deviation of ETR / Cash ETR*	0.005	0.004	0.010	0.004 ***	-0.005	0.004	0.006	0.004 *
	Following Chyz et al. (2013) control specification	0.006	0.004 *	0.010	0.004 ***	-0.007	0.004	0.011	0.004 ***

(continued)

Table 7, continued
Robustness Tests

Table 7 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. Our robustness tests begin with our fully specified main model (i.e., *Forced CEO turnover* on tax indicators, controls following Gallemore et al. 2014, firm and year fixed effects):

$$\textit{Forced CEO turnover}_{i,t+1} = \beta_1 \textit{Low tax ind.}_{i,t} + \beta_2 \textit{High tax ind.}_{i,t} + \beta_3 \textit{Size}_{i,t} + \beta_4 \textit{Abnormal stock returns}_{i,t} + \beta_5 \textit{Return on assets}_{i,t} + \beta_6 \textit{Leverage}_{i,t} + F_i + T_t + \varepsilon_{i,t} \quad (1)$$

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* is tax expense divided by pretax income. *ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *Cash ETR* is the cash paid for taxes divided by pretax income. *Cash ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *ETR* and *Cash ETR* 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 total assets. *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.

We then estimate several variations of our main results, modifying either the sample, the dependent variable, or our set of control variables. Only the coefficients on *Low tax indicator* and *High tax indicator* are reported. 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 five groupings, (1) to (5). Grouping (1) explores managerial ability as an alternative explanation by adding a control variable capturing managerial ability from Demerjian et al. (2012). Grouping (2) explores the possibility that a denominator effect explains our results. The first two tests in this series include computing tax indicators for *Book-tax Differences* instead of *ETR* and computing tax indicators based on the tax preference measure from Henry and Sansing (2015) instead of *Cash ETR*. The third test restricts the sample to firms with high pretax returns (i.e., top half of *Return on assets*). The following five tests in the series employ alternative scalars in the computation of *ETR* and *Cash ETR*; including market value of assets (*MVA*), market value of equity (*MVE*), book value of assets (*AT*), book value of equity (*CEQ*), and sales (*SALE*). The final test in grouping (2) employs non-linear measures of performance, by adding binary variables indicating whether observations are in the top or bottom quintiles of *Return on assets* and *Abnormal stock returns*. Grouping (3) examines governance as an alternative explanation. Tests in this grouping control for various aspects of governance; including *G_index*, *Institutional ownership*, *Board independence*, and *CEO duality*. Grouping (4) includes additional market controls; including annual *Stock price volatility*, *# of trading days with large negative returns* within the fiscal year, and the *Average return over the 5 worst trading days* within the fiscal year. Grouping (5) explores the remaining issues. The first tests in this grouping uses LOGIT to estimate equation (1) instead of the Linear Probability Model. The second test deletes firms with more than one instance of Forced CEO turnover. The third tests eliminates observations with extreme values of *ETR* and *Cash ETR* (i.e., 0 or 1). The fourth tests adjusts *ETR* and *Cash ETR* for special items. The fifth test includes controls for the standard deviations of *ETR* / *Cash ETR*. The final test replaces control variables from Gallemore et al. (2014) with variables from Chyz et al. (2013); including *Operating cash flow*, *Leverage*, *NOL*, *Annual change in NOL*, *Foreign income*, *PP&E*, *Intangible assets*, *Equity income in earnings*, *Size*, *Market-to-book ratio*, and *R&D*.

Table 8
Taxes, Analysts, and Forced CEO turnover

$$\textit{Forced CEO turnover}_{i,t+1} = \beta_1 \textit{Low tax indicator}_{i,t} + \beta_2 \textit{High tax indicator}_{i,t} + \beta_3 \textit{Analyst}_{i,t} + \beta_4 \textit{Low tax indicator}_{i,t} * \textit{Analyst}_{i,t} + \beta_5 \textit{High tax indicator}_{i,t} * \textit{Analyst}_{i,t} + \beta_6 \textit{Size}_{i,t} + \beta_7 \textit{Abnormal stock returns}_{i,t} + \beta_8 \textit{Return on assets}_{i,t} + \beta_9 \textit{Leverage}_{i,t} + F_i + T_t + \varepsilon_{i,t}$$

Panel A: Tax indicators based on *ETR*

	Forced CEO turnover			
	OLS	Propensity Score Match	Propensity Score Match	Firm fixed effects
	(1)	(2)	(3)	(4)
Low tax indicator	0.016 ***	0.018 ***		0.009
	(0.006)	(0.007)		(0.008)
High tax indicator	0.020 ***		0.036 ***	0.024 ***
	(0.006)		(0.007)	(0.007)
Analyst	0.0178 ***	0.0218 ***	0.0331 ***	0.0026
	(0.004)	(0.006)	(0.006)	(0.006)
*Analyst*Low tax indicator*	-0.0049	-0.0078		-0.0042
	(0.007)	(0.008)		(0.009)
*Analyst*High tax indicator*	-0.0142 **		-0.0313 ***	-0.0227 ***
	(0.007)		(0.008)	(0.008)
Size	0.002 **	0.001	0.003 ***	0.013 ***
	(0.001)	(0.001)	(0.001)	(0.003)
Abnormal stock returns	-0.013 ***	-0.008 ***	-0.016 ***	-0.014 ***
	(0.002)	(0.002)	(0.003)	(0.002)
Return on assets	-0.063 ***	-0.047	-0.101 ***	-0.053 **
	(0.017)	(0.030)	(0.027)	(0.025)
Leverage	-0.020 ***	-0.019 *	-0.037 ***	0.007
	(0.007)	(0.011)	(0.011)	(0.015)
Firm fixed effects	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R-square	0.005	0.005	0.010	0.222
N	33,955	13,582	13,582	33,955

(continued)

Table 8, continued
Taxes, Analysts, and Forced CEO turnover

Panel B: Tax indicators based on *Cash ETR*

	Forced CEO turnover			
	OLS	Propensity Score Match	Propensity Score Match	Firm fixed effects
	(1)	(2)	(3)	(4)
Low tax indicator	0.014 ***	0.015 **		0.002
	(0.006)	(0.007)		(0.007)
High tax indicator	0.015 ***		0.018 ***	0.019 ***
	(0.006)		(0.008)	(0.007)
Analyst	0.0152 ***	0.0174 ***	0.0242 ***	0.000
	(0.004)	(0.006)	(0.007)	(0.006)
*Analyst*Low tax indicator*	-0.0067	-0.0084		-0.0086
	(0.007)	(0.008)		(0.008)
*Analyst*High tax indicator*	-0.0046		-0.0133 *	-0.0144 **
	(0.007)		(0.009)	(0.008)
Size	0.002 **	0.001	0.001	0.012 ***
	(0.001)	(0.001)	(0.001)	(0.003)
Abnormal stock returns	-0.013 ***	-0.011 ***	-0.024 ***	-0.013 ***
	(0.002)	(0.002)	(0.004)	(0.002)
Return on assets	-0.062 ***	-0.029	-0.090 ***	-0.052 **
	(0.017)	(0.031)	(0.030)	(0.025)
Leverage	-0.020 ***	-0.011	-0.030 **	0.009
	(0.007)	(0.011)	(0.012)	(0.015)
Firm fixed effects	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
R-square	0.005	0.004	0.009	0.222
N	33,955	13,582	13,582	33,955

This table reports results for our main tests examining the effect of taxes on forced CEO turnover. 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-sided tests examining H1 and H2, two-tailed otherwise. The first column uses OLS (Linear Probability Model) specification, the second and third match observations in low and high tax quintiles to observations in tax quintiles 2-4 using propensity score matching, and the fourth column employs firm fixed effects (i.e., equation 1). *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* is tax expense divided by pretax income. *ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *Cash ETR* is the cash paid for taxes divided by pretax income. *Cash ETR* is winsorized to 0 and 1 and is set to missing when pretax income is non-positive. *ETR* and *Cash ETR* 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. *Analyst* indicates analyst coverage. *Size* is the natural log of total assets. *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. *Forced CEO turnover* is measured as of *t+1*, while all other variables as measured at *t*.