CEO AFTER-TAX COMPENSATION INCENTIVES AND CORPORATE TAX AVOIDANCE

by

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ABSTRACT

I examine the association between CEOs' after-tax incentives and their firms' levels of tax avoidance. Economic theory holds that firms should compensate CEOs on an after-tax basis when the expected tax savings generated from additional incentive alignment outweigh the incremental compensation demanded by CEOs for bearing additional tax-related compensation risk. Using publicly available data, I estimate CEOs' after-tax incentives and find a negative relation between the use of after-tax incentives and effective tax rates. While the results suggest that greater use of after-tax measures in CEO compensation leads to higher tax savings, it is possible that these savings will lead to lower pre-tax returns, or implicit taxes. Therefore, I also examine the association between the use of after-tax incentives and implicit taxes and find a positive association between the two. Finally, I find a significant positive relation between after-tax incentives and total CEO compensation, suggesting that CEOs who are compensated after-tax demand a premium for the additional risk they bear.

I. INTRODUCTION

In this paper I examine whether the use of after-tax accounting earnings in chief executive officer (CEO) compensation leads to greater corporate tax avoidance.[1] Numerous prior studies on compensation examine the determinants and consequences of conditioning executive compensation on different elements of performance (e.g., Holmstrom 1979, Demski and Feltham 1979, Antle and Demski 1988). Extending their arguments, Newman (1989) examines the determinants of using before- versus after-tax earnings as performance measures in setting executive compensation. According to Newman, firms decide whether to use after-tax incentives after weighing the benefits (i.e., additional tax savings) against the costs (i.e., additional compensation paid to persuade managers to accept riskier contracts). Hence, while economic theory predicts that using after-tax earnings in setting CEO compensation should lead to lower taxes for the firm and to higher total CEO compensation, empirical studies to date have found neither. Most related studies examine the determinants of using after-tax incentives, but do not analyze the consequences of utilizing such incentives.

One notable exception is Phillips (2003), who analyzes proprietary survey data and finds that compensating business-unit managers on an after-tax basis leads to lower effective tax rates (ETRs) at the firm level, while compensating CEOs after-tax does not. Phillips finds a positive and statistically insignificant relation between ETRs and the use of after-tax CEO incentives. This result, however, is puzzling and inconsistent with

[1] Tax avoidance is broadly defined as the reduction of explicit taxes per dollar of pre-tax accounting earnings or cash flows, consistent with Hanlon and Heitzman (2009). Throughout the paper "pre-tax incentives" relate to the use of pre-tax accounting measures in determining CEO compensation (e.g., pre-tax earnings, EBITDA), while "after-tax incentives" relate to the use of after-tax accounting measures (e.g., after-tax earnings).

Newman (1989) and economic theory. As Phillips points out, it could be that CEOs already have adequate incentives to reduce tax payments through means other than compensation (e.g., job retention). However, the study's inability to document a negative association between firms' ETRs and their use of after-tax CEO incentives could also be due to low statistical power, a limitation inherent to small-sample studies. Because only 209 CEO observations are available to Phillips (2003), I examine this second possibility.

Using descriptive statistics from Phillips (2003), I perform a statistical power test and find evidence of low statistical power (0.639) in his test for mean differences in ETRs between CEOs compensated before and after taxes. This indicates that the statistical test has only a 64% chance to find an existing association, suggesting that the lack of a significant relation between ETRs and after-tax CEO incentives may be due to low statistical power.

I begin my study on the consequences of using after-tax incentives in CEO compensation by re-addressing the issue of whether these incentives lead to greater tax avoidance at the firm level. To address the issue of low statistical power in prior literature, I use publicly available data to analyze a larger sample of US firms (1,298 firms). The analysis is conducted in two steps. First, I determine whether firms use after-tax accounting measures in CEO compensation. To do so, I estimate the firm-specific sensitivity of CEO cash compensation to total income tax expense, controlling for pre-tax accounting income.[2] Firms whose sensitivities are negative (i.e., lower taxes translate into

[2] Including CEO salary in addition to bonus pay allows for an implicit relation between pay and accounting performance. However, I also conduct all my empirical tests using the firm-specific sensitivity of CEO bonus compensation to income tax expense, again controlling for pre-tax accounting income. This procedure yields very similar results and is further discussed in Section IV.

higher cash compensation) and statistically significant at the 0.10 level are coded as using after-tax measures in determining CEO cash compensation. An examination of individual corporate proxy statements indicates that this procedure codes the use of after-tax CEO incentives with reasonable accuracy.[3] Second, I use regression analysis to estimate the statistical association between ETRs and my estimate of after-tax CEO incentives, controlling for several determinants of using after-tax incentives. Using this procedure, I find a negative and statistically significant relation between CEOs' after-tax incentives and firms' ETRs. The use of after-tax CEO incentives translates into a reduction of 260 basis points in the effective tax rate, resulting in annual tax savings of $20.7 million for the average sample firm.

Without further reflection, this finding may be misleading. It implies that the tax savings generated by paying after-tax directly map into higher after-tax earnings. However, it is likely that at least part of these tax savings will be reversed in the economic market through higher implicit taxes (Scholes et al. 2005). Implicit tax theory argues that tax-favored investments will generally yield lower pre-tax returns relative to equally-risky tax-disfavored investments. Consequently, any tax savings accrued to the firm as a result of compensating CEOs after-tax may be diminished, if not eliminated, by implicit taxes in the form of lower pre-tax accounting returns earned by the firm. To examine this possibility, I also investigate whether after-tax CEO incentives are associated with higher implicit taxes. I do so by regressing a firm-specific estimate of the implicit tax rate (Jennings et al. 2009) on my proxy for after-tax CEO incentives,

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[3] See Section IV for more details.

controlling for other determinants of paying after-tax. Using this procedure I find a positive and significant relation between CEOs' after-tax incentives and firms' implicit taxes. In-sample, $1.9 million of the estimated $20.7 million in tax savings are reversed due to implicit taxes arising as a result of paying CEOs after-tax. The combined results suggest that while CEO's after-tax incentives lead to greater tax savings, a relatively small portion of these savings (9.4%) is offset by implicit taxes.

Because Newman (1989) predicts that the use of after-tax incentives should lead not only to lower effective tax rates, but also to higher CEO compensation, I complete my analysis by examining the effect of after-tax CEO incentives on total CEO compensation. Executives should be unwilling to take on additional compensation risk unless they are compensated for doing so. As such, I expect a positive relation between the use of after-tax incentives and total CEO compensation. After controlling for known, observable determinants of paying after-tax I find a positive and significant relation between after-tax CEO incentives and total CEO compensation. The additional compensation associated with paying the CEO after-tax is $789,528 for the average firm in my sample, which represents a significant cost.

This study's contribution is threefold. First, I show that the inability of prior literature to document a significant negative association between after-tax CEO incentives and firms' effective tax rates appears to be a result of low statistical power. I address this limitation by empirically estimating whether firms use such incentives, thus increasing the sample size. As a result, my sample yields a statistical power coefficient of 0.89; well above 0.80, the generally accepted rule of thumb in the social sciences (Cohen

1988). By increasing the statistical power I am able to document a negative and statistically significant relation between the use of after-tax CEO incentives and ETRs, consistent with the predictions in Newman (1989) and Phillips (2003). To my knowledge, this is the first study to document and quantify this effect.

Second, the paper provides support for the implicit tax theory. I find that tax savings associated with after-tax CEO incentives are partially obtained at the expense of pre-tax accounting earnings. The main contribution from this analysis, however, is that this effect appears to be quite small. Several prior studies use measures of explicit taxes as proxies for tax avoidance and examine different mechanisms which are believed to significantly reduce explicit taxes (e.g., Chen et al. 2010, Dyreng et al. 2010, Armstrong et al. 2009, Dyreng et al. 2008, Robinson et al. 2010, Phillips 2003, Rego 2003, and Mills et al. 1998). However, because each of these studies ignores implicit taxes it is unclear whether these mechanisms truly lead to tax savings or whether they simply convert explicit into implicit taxes. By incorporating implicit taxes in the analysis, I am able to quantify the level of total tax savings accrued to the firm as a result of incentivized tax planning. My results indicate that firms retain 90.6% of their tax savings, suggesting that prior studies do not simply document a substitution effect between explicit and implicit taxes.

Third, I find a positive relation between after-tax incentives and total CEO compensation, consistent with managers demanding to be compensated for additional tax-related compensation risk. The result is consistent with Newman (1989) and enriches our understanding of the costs associated with using after-tax incentives.

The remainder of the paper is organized as follows. In Section II I review the related literature and develop the hypotheses. In Section III I outline the data and sample selection criteria. In Section IV I outline the empirical strategy, and in Section V I describe the empirical models. I present the results in Section VI and robustness tests in Section VII. Finally, I conclude in Section VIII.

II. RELATED LITERATURE AND HYPOTHESES DEVELOPMENT

Determinants of Using Pre-tax versus After-tax Earnings in Compensation

Prior work in compensation suggests that both firms and managers are often better off by using accounting-based performance measures in management compensation (e.g., Antle and Demski 1988, Lambert and Larcker 1987, Banker and Datar 1989, Sloan 1993). Given that firms choose to use accounting-based performance measures in management compensation, a subset of the compensation literature examines the determinants of using before- versus after-tax accounting earnings in top executive compensation contracts. In the first paper to study this choice, Newman (1989) hypothesizes that the choice of pre-tax versus after-tax accounting measures is a function of the benefits and costs associated with the use of each measure. Specifically, he argues that firms will use after-tax accounting measures when the additional tax savings from incentive alignment exceed the additional compensation that must be paid to compensate managers for the additional risk imposed on them. Following this reasoning, Newman (1989) predicts and finds that firms are more likely to use after-tax incentives when they have greater tax planning opportunities. He shows that multinational and capital intensive firms, who likely have greater tax planning opportunities, are more likely to use after-tax compensation incentives. He also finds that firm size and the number of operating segments (which reflect economies of scale and greater opportunities for tax planning, respectively) are positively associated with after-tax executive compensation.

Atwood et al. (1998) extend Newman's work and document a negative association between leverage and the choice to contract after-tax. Their finding is consistent with the notion that highly leveraged firms are near tax exhaustion, and are therefore less likely to benefit from the use of after-tax incentives. Carnes and Guffey (2000) conduct a study similar to Newman (1989), using a more recent sample, and confirm Newman's findings. Finally, Dhaliwal et al. (2000) use refined estimates of tax planning opportunities to examine the determinants of the choice to use after-tax earnings measures in CEO compensation. Consistent with prior studies, they find that firms with greater realized tax credits and absolute values of permanent book-tax differences are more likely to use after-tax accounting earnings in CEO compensation.

Effective Tax Rates and After-tax Compensation Incentives

Prior executive compensation research suggests that managers make choices that are consistent with maximizing the value of their compensation (e.g., Wallace 1997). Based on this logic, Phillips (2003) is the first study to investigate whether after-tax compensation incentives are effective in lowering a firm's tax expense. He finds that compensating business-unit managers on an after-tax basis leads to lower ETRs, while compensating CEOs on an after-tax basis does not. The author attributes the insignificant relationship between CEO after-tax incentives and ETRs, which runs contrary to his prediction, to CEOs having other incentives that are sufficient to motivate them to focus on after-tax results.[4] Phillips further suggests that CEO incentives could still have an indirect effect on firms' ETRs if CEOs who are compensated on after-tax earnings are more likely to use them in evaluating business-unit managers, who are in turn successful

[4] For example, if CEOs believe that investors value lower tax payments, then job retention may be sufficient to motivate CEOs to lower ETRs, rendering their after-tax compensation incentives irrelevant.

in lowering firms' ETRs. However, given that firms using after-tax incentives must compensate CEOs for bearing additional risk (Holmstrom 1979, Newman 1989), it is still unclear why firms pay a risk premium if after-tax incentives are ineffective.[5]

On one hand, it is possible that CEOs already have adequate incentives to reduce tax payments through means other than compensation. On the other hand, the study's inability to find a significant relationship between after-tax CEO incentives and ETRs could also be explained by low statistical power attributed to a small sample size. Data limitations restrict Phillips' sample to 209 firms. To investigate this possibility, I use a power test to assess the statistical power in Phillips (2003)'s t-test for differences in ETR means between the two different CEO incentive subsamples. A two-sided t-test like the one used in Phillips (2003) with a mean ETR of 0.368 (0.348) for firms without (with) after-tax CEO incentives, and an ETR standard error of 0.062 over 209 observations, has a statistical power of 0.639.[6] As such, this test only has about 64 chances out of 100 of finding a difference given that one exists. As a rule of thumb, social scientists are advised to conduct tests with at least an 80% chance to find an existing relationship (Cohen 1988). Not doing so will increase the probability of not finding an association given that one exists. To address this limitation I employ a larger sample using publicly available data, and test whether CEOs' after-tax incentives are associated with corporate tax avoidance.

[5] If there are significant benefits to be gained from compensating business-unit managers on an after-tax basis, then CEOs should compensate business-unit managers using after-tax measures regardless of their own compensation incentives. Alternatively, firms could also compensate CEOs based on pre-tax earnings and mandate that CEOs compensate business-unit managers after-tax, thus avoiding the additional compensation demanded by the CEO in order for him or her to accept this riskier contract.

[6] These figures are obtained from Phillips (2003), Table 2 (p. 860). The statistical power calculation assumes an alpha of 0.05.

The prediction above is dictated by economic theory. Seeking to reduce their exposure to operating risk, firms adopt compensation arrangements that condition CEO pay on accounting financial performance (Fama and Jensen 1983). This reduction comes by mitigating agency costs associated with both moral hazard and adverse selection. Profit-based arrangements reduce the agency costs of moral hazard by shifting a portion of the firm's operating risk onto the CEO, effectively aligning the CEO's interests with those of the firm (Jensen and Meckling 1976). In addition, arrangements that condition CEO pay on performance reduce the agency costs of adverse selection by creating a selection effect (Demski and Feltham 1979). Since compensation arrangements that condition pay on performance are too risky for less-able workers to accept, these arrangements induce more-talented workers to self select. As a result, pay-for-performance arrangements act as a CEO screening device.

Both of these effects should lead to lower ETRs for firms adopting after-tax incentives. Such incentives should not only motivate CEOs to reduce their firms' exposure to taxation, but also attract CEOs that are better suited to deal with this exposure. Combined, these two arguments lead to the following hypothesis (stated in alternative form):

H1: The use of after-tax performance measures in CEO compensation is negatively associated with firms' ETRs.

Implicit Taxes and After-tax Compensation Incentives

Miller (1977) implies that in the absence of market frictions, government restrictions, and risk differences; after-tax rates of return should be the same across investments. By construction, it follows that tax-favored investments obtain lower pre-tax rates of return. In a similar vein, Scholes et al. (2005) argue that firms face two different taxes: explicit and implicit. The authors define explicit taxes as tax dollars paid directly to taxing authorities. Implicit taxes are defined as lower pre-tax rates of return obtained on tax-favored investments.[7] These lower levels of pre-tax returns are caused by either higher input prices or lower output prices in the economic market. Higher input prices occur when tax-favored status increases demand for an asset with inelastic supply, leading firms to bid up the price of the tax-favored asset. Lower output prices, on the other hand, occur when firms seeking tax-favored status enter the product market, reducing product prices in equilibrium.

Berger (1993) offers a concrete example of implicit taxes. Berger studies the enactment of the R&D credit in 1981, which created a tax-favored activity. As such, firms increased their levels of R&D expenditures, and were thus able to lower their tax liabilities. However, because labor supply, the major input for R&D activities, is relatively inelastic, the increased demand for R&D workers drove up the wages of research personnel, resulting in an implicit tax. Implicit taxes also occur when outside firms enter the product market, thus lowering equilibrium output prices; although Berger suggests this was likely not the case with the enactment of R&D.

[7] The empirical regularity that municipal bonds earn lower pre-tax returns than other bonds of similar risk offers the classic example of implicit taxes. Because the interest earned on municipal bonds is tax-exempt at the federal level, individuals in higher tax brackets are willing to pay more for them. As a result, they will bid up the price, which lowers the pre-tax returns on these bonds until their after-tax return is the same as that of equally risky taxable bonds. At equilibrium, the tax savings should equal the price increase and the higher input prices paid by high-tax investors flow through to local municipalities (effectively as a federal subsidy).

Under strict assumptions of perfectly competitive markets and no risk differences, Scholes et al. (2005) show that total taxes, or the sum of explicit and implicit taxes, are the same for all firms. In other words, under these assumptions firms may choose to pay either explicit or implicit taxes without consequence to total taxes paid. Nevertheless, implicit taxes may have little to no impact on the firm's financial outcomes under relaxed assumptions.

According to H1, after-tax CEO incentives should induce the CEO to lower the firm's explicit taxes, holding pre-tax returns constant. To do so, managers must invest in tax-favored activities, which theoretically carry some form of implicit tax.[8] Hence, if after-tax incentives are effective in leading the CEO to take actions that lower the firm's cash payments to tax authorities, it is likely that such actions will also lead to higher implicit taxes. This conjecture is tested in Hypothesis 2 (stated in alternative form):

H2: The use of after-tax performance measures in CEO compensation is positively associated with firms' implicit tax rates.

Total CEO Compensation and After-tax Compensation Incentives

In traditional agency models, risk-averse agents are reluctant to take incentive compensation arrangements as these arrangements lead to greater compensation risk. As a result, the principal must pay the agent a risk premium to get him or her to accept the incentive contract.

CEOs who are compensated after-tax bear significant additional risk associated with the tax accounts. Not only are tax accounts large (reaching up to forty percent of

[8] Firms can also reduce their tax liabilities by not incurring profits. Since this is an unlikely strategy for profit-maximizing firms, this facet of tax planning is ignored throughout the paper.

pre-tax earnings), but they are complex as well. Prior literature shows that analysts do not fully comprehend firms' tax implications (Weber 2009). Auditors seem to have problems comprehending tax implications as well (Badertscher et al. 2009). In addition, most CEOs are not tax experts and must rely on their tax departments (Dyreng et al. 2010). While economic theory argues that firms wishing to evaluate managers on an after-tax basis must pay an additional risk premium, this prediction has gone untested. Therefore, I test the following hypothesis (stated in alternative form):

H3: The use of after-tax performance measures in CEO compensation is positively associated with total CEO compensation.

III. DATA

Panel A of TABLE 1 outlines the sample selection criteria. I begin with all firm observations in the Compustat Industrial Annual files for fiscal year 2005 (9,201 firms).[9] Requiring ExecuComp data reduces the sample by 7,448 firms. Because I use firm-specific regressions to estimate whether firms use after-tax incentives in CEO compensation, I require each firm observation in 2005 (the treatment year) to have at least five years of lagged ExecuComp data on CEO cash compensation (the sum of ExecuComp items 'salary', 'bonus', and 'noneq_incent'), pre-tax income (data170), and income tax expense (data16).[10] This requirement reduces the sample by 125 firms. Prior research outlines problems with ETR estimates in the presence of negative pre-tax income. To mitigate estimation problems associated with poor ETR estimates I set ETRs for firms with negative pre-tax income to missing (230 firm observations), and winsorize the remaining ETRs so that the largest observation is one and the smallest is zero (Robinson et al. 2010, Dyreng et al. 2010).[11] To ensure that all firms in my sample face a similar legal environment, I also eliminate firms that are not based in the United States (20 observations).[12] The sample is further reduced by 72 observations with missing data required to compute control variables. Finally, due to extreme observations of return on

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[9] Unless otherwise indicated, all data items refer to Compustat Industrial Annual variables.
[10] I begin with the year 1992 because ExecuComp begins its coverage in 1992 and end in 2005 because it is the last year for which I have complete data files. In additional tests I find that my results also generalize to earlier sample periods (see Section VII).
[11] The results in Section VI are robust to truncating all ETR estimates outside the [0,1] interval.
[12] The results are not sensitive to this requirement.

equity, I truncate *ROE* at the 1st and 99th percentiles, reducing the sample by 8 observations.[13] The result is a final sample of 1,298 firm observations for the year 2005.

Panel B of TABLE 1 presents industry composition statistics for my sample, ExecuComp, and Compustat. My sample is significantly reduced relative to Compustat because ExecuComp only covers a small subset of Compustat firms (i.e., firms currently and formerly listed in the S&P 1500). Even so, the final sample is still relatively large, covering 74% of all ExecuComp firms. As expected, the industry composition of my sample closely follows that of ExecuComp.

[13] This procedure has little effect on the results in Section VI. Prior to truncation, the sample contained *ROE* values that were more than 10 standard deviations away from the sample mean.

IV. EMPIRICAL STRATEGY

I conduct the empirical analysis in two steps. First, I estimate whether CEOs have after-tax incentives.[14] Second, I examine the associations between my estimates of after-tax incentives and three outcome variables (effective tax rates, implicit taxes, and total CEO compensation).

After-tax CEO Incentives

To estimate whether firms employ after-tax incentives in CEO compensation, I begin by considering the sensitivity of CEO cash compensation to accounting earnings, consistent with prior studies documenting a positive association between cash compensation and accounting income (e.g., Sloan 1993, Cadman et al. 2010). Because prior literature on compensation tends to view accounting-based incentives as a way of rewarding short-term performance, I exclude the effects of equity compensation. Since equity compensation is used to set long-term incentives rather than to reward short-term performance (Core and Guay 1999), the inclusion of equity grants would reduce the power of my statistical tests (Cadman et al. 2010).

I then disaggregate accounting earnings into pre-tax earnings and income tax expense, and estimate the sensitivity of CEO cash compensation to income tax expense,

[14] Auxiliary equations have been used in prior studies to estimate executive incentives (e.g., Garen 1994, Krishnan et al. 2006, Eldenburg and Krishnan 2009, and Armstrong et al. 2009).

controlling for pre-tax earnings.[15] The sensitivity of CEO cash compensation to income tax expense is obtained by estimating the following regression:[16]

$$\textit{CEO Cash Compensation}_t = \square + \square_1 \textit{Pre-tax Income}_t + \square_2 \textit{Income Tax Expense}_t + \square_t \quad (1)$$

Equation (1) is estimated within each firm by ordinary least squares (OLS) using observations from 1992-2004 to create an ex-ante measure for 2005. The coefficient λ_2 is expected to be negative. Hence, a negative λ_2 coefficient implies that reductions in the firm's tax liabilities lead to higher cash compensation. Presumably, a statistically significant negative sensitivity of CEO compensation to income tax expense indicates that firms indeed use after-tax accounting measures in assessing CEO cash compensation. Because the construct of the analysis lies on whether firms use after-tax incentives (rather than the extent to which they use them), I create the variable *CEOATAX*; which takes the value of 1 if λ_2 is negative and statistically significant at the 0.10 level, and 0 for remaining observations.

To verify the accuracy of my estimates, I perform a manual verification using random samples of 25 estimates coded as *CEOATAX* = 1 and 25 estimates where *CEOATAX* = 0. In many cases, notes to the proxy statements reveal the nature of the performance measures used in setting bonus compensation (e.g., pre-tax earnings, after-

[15] As noted by Jensen and Murphy (1990), CEO performance should be evaluated before compensation expense. Therefore, in estimating equation (1) I add back CEO cash compensation to pre-tax earnings to simulate this effect. Not doing so has little effect on the results (untabled).

[16] As prior studies also examine the role of accounting earnings within a bonus setting, I also run my analyses after estimating equation (1) using CEO bonus pay rather than total cash compensation. The procedure yields very similar results (untabled).

tax earnings, EBITDA). Because the stipulations of salary compensation are not available, I create *CEOATAX* by estimating equation (1) after replacing cash compensation with bonus pay, and verify how accurately my estimate match firms' bonus disclosures. The verification reveals that my procedure accurately identifies 24 out of the 25 firms (96%) that are coded as using after-tax earnings in CEO compensation (*CEOATAX* = 1). Meanwhile, 23 out of 25 firms (92%) coded as not using after-tax incentives (*CEOATAX* = 0) are correctly identified. Because I cannot verify the computation of salary due to lack of disclosures, I also use the modified version of *CEOATAX* (i.e., using CEO bonus in the first stage rather than cash compensation) in the second stage tests. The results (untabled) are both quantitatively and qualitatively similar.

In estimating equation (1) it is assumed that the use of after-tax incentives is stationary through time. This assumption seems plausible assuming that compensation incentives are endogenous to firm characteristics (Smith and Watts 1992), which are unlikely to change significantly over a relatively short time-series. However, it is possible that firms begin to use after-tax incentives early in the time-series, but later drop their use. In that case, the firm is incorrectly coded as using after-tax incentives when in reality it does not; which should bias the statistical tests against finding a difference in outcomes in the second stage. The same reasoning can be extended to the case where firms are wrongly coded as not using after-tax incentives when in reality they do.

Identification Strategy

My objective in this paper is to study the consequences of using after-tax incentives in CEO compensation. The task would be relatively simple were it possible to randomly assign CEOs to one of two incentive groups and then simply compare the outcomes between CEOs with after-tax incentives and those without. However, the use of after-tax incentives is not random. Specifically, Newman (1989) posits that firms that are more likely to benefit from such incentives are also more likely to use them. Hence, it is likely that the use of after-tax incentives in CEO compensation is correlated with other characteristics of the firm. In such a case, simple comparisons of outcomes between CEOs with and without after-tax incentives are likely to be biased, and will reflect not only the causal impact of the compensation contract but also omitted effects associated with the probability of paying after-tax. I attempt to deal with this non-random assignment of after-tax incentives in two ways.

First, I estimate the effects of after-tax CEO incentives while controlling for several factors found in the literature to affect the probability of compensating after-tax. To do so I use the following model:

$$Y_i = \beta CEOATAX_i + \Gamma X_i + \Phi Z_i + \varepsilon_i \quad (2)$$

where *Y* is one of three outcomes (effective tax rate, implicit taxes, and total CEO compensation), *CEOATAX* is a binary variable that takes on a value of one if the firm uses after-tax incentives in CEO compensation, *X* is a vector of variables that are known to affect the decision to use after-tax incentives, *Z* is a vector of industry fixed-effects (calculated using the Barth et al. 1998 industry groupings), and ε is a disturbance term with mean zero. By controlling for these firm- and industry-specific characteristics (vectors X and Z) associated with the probability of using after-tax incentives I should be able to closely estimate the causal effects of using after-tax incentives in CEO compensation. While this approach seems reasonable, in the presence of unobserved characteristics it is still possible that OLS estimation of equation (1) will lead to inconsistent estimates of β.

Second, I attempt to mitigate and quantify the effects of potential unobservable effects by using a propensity score matched pair research design. This procedure yields similar results and is further discussed in Section 7.[17]

Determinants of Using After-tax CEO Incentives

Prior studies have identified factors that are associated with the choice of using after-tax incentives (Phillips 2003, Dhaliwal et al. 2000, Atwood et al. 1998, Newman 1989). These variables are identified in equation (2) as vector X, and are likely correlated with the benefits and costs of using after-tax incentives. By implication, they are also correlated with the probability of adopting after-tax incentives. Variables in vector X are listed below:

FOR_D: 1 if income from foreign operations (data273) is not zero or missing, and zero otherwise.

CAPINT: Net property, plant, and equipment (data8) at time t, divided by total assets (data6) at time t.

LEV: Total long-term debt (data9) at time t, divided by total assets (data6) at time t.

SIZE: The natural log of the market value of equity (data25*data199) at time t.

[17] Another way of dealing with potential unobservable effects would be to find an exogenous variable that is correlated with the endogenous regressor, but uncorrelated with the structural disturbance term. However, in the words of Maddala (1977, p. 154) "Where do you get such a variable?" Along similar lines, Francis and Lennox (2008) examine selection models in accounting research and suggest the use of propensity score matching over the traditional Heckman (1979) procedure.

ROE:	Pre-tax income (data170) minus special items (data17) both at time t, divided by total book value of equity (data60) at time t. *ROE* is truncated at the 1^{st} and 99^{th} percentiles.
Std(ROE):	Five-year standard deviation of *ROE*, calculated from t-4 to t.
ΔMVA:	The change in the market value of assets (MVA) from t-1 to t calculated as (MVA_t – MVA_{t-1}) / MVA_{t-1}. MVA is [abs$(data199)_t$*$(data25)_t$] + $(data9)_t$].
CEOESO:	The Black-Scholes value of annual CEO equity grants at time t, divided by total CEO compensation at time t, both from ExecuComp.
RD:	Research and development expense (data46) at time t, divided by sales (data12) at time t. When missing, *RD* is set to zero.

A proxy for multinational status, *FOR_D* is used because firms across multinational jurisdictions are expected to have greater tax planning flexibility (Rego 2003), which decreases the cost of tax planning whilst increasing its net benefits. Multinational CEOs also face fewer risks associated with future tax rate changes, as they are relatively "tax diversified." This diversification lowers the executives' compensation risk associated with the tax accounts (Newman 1989). Capital intensive firms (*CAPINT*) have greater tax planning opportunities related to investments in fixed assets (Gupta and Newberry 1997, Stickney and McGee 1982). As such, these firms stand to profit more from using after-tax incentives. Leverage (*LEV*) is included to control for differences in tax planning opportunities related to capital structure decisions (Gupta and Newberry 1997).

SIZE is designed to control for possible economies of scale related to tax planning as well as for variation in the political costs of tax planning (Gupta and Newberry 1997). The pre-tax return on equity (*ROE*) is included as a control for changes in book income.

Std(ROE) controls for earnings variability, and is added because Dhaliwal et al. (2000) suggest that firms with variable earnings have greater risks associated with being in the wrong tax clientele, and thus have greater incentives for tax planning. The change in the market value of assets (*ΔMVA*) is included because higher growth firms are expected to focus less on tax planning (Bankman 1994). I control for the percentage of the CEO's compensation attributable to equity grants (*CEOESO*) because firms with large employee stock option deductions pay fewer taxes, and may have fewer incentives to engage in tax planning (Phillips 2003).[18] Finally, I include research and development (*RD*) because R&D activities are tax-favored (Berger 1993).

[18] Hanlon and Shevlin (2002) describe possible errors in using ETRs in accounting research. Such problems arise because the excess stock option tax benefit does not flow through to tax expense. As a robustness check, I also use the annual cash ETR (Dyreng et al. 2008), which is immune to such errors, and find very similar results.

V. EMPIRICAL MODELS

Effective Tax Rates and CEOs' After-tax Incentives

I study the effect of after-tax CEO incentives on effective tax rates by estimating the following regression, consistent with equation (2):

$$ETR_i = \beta_0 + \beta_1 CEOATAX_i + \beta_2 FOR\_D_i + \beta_3 CAPINT_i + \beta_4 LEV_i + \beta_5 SIZE_i + \beta_6 ROE_i + \beta_7 Std(ROE)_i + \beta_8 \Delta MVA_i + \beta_9 CEOESO_i + \beta_{10} RD_i + Industry\ Effects_i + \varepsilon_i \quad (3)$$

where *ETR* is total income tax expense (data16) at time t, divided by pre-tax income (data170) at time t. All other variables are defined in Section IV. Equation (3) represents the ETR treatment model. The model's parameters are estimated via OLS and its standard errors are calculated using heteroscedasticity-robust standard errors.

In H1 I predict that the use of after-tax CEO incentives should lead to lower ETRs. Thus, β_1 is expected to be negative and should be interpreted as the change in *ETR* associated with the use of after-tax incentives in CEO compensation, maintaining the other variables constant.

Implicit Tax Rates and CEOs' After-tax Incentives

To infer the effect of after-tax CEO incentives on firms' implicit taxes I estimate the following regression:

$$IMPLICIT_i = \beta_0 + \beta_1 CEOATAX_i + \beta_2 FOR\_D_i + \beta_3 CAPINT_i + \beta_4 LEV_i + \beta_5 SIZE_i + \beta_6 ROE_i + \beta_7 Std(ROE)_i + \beta_8 \Delta MVA_i + \beta_9 CEOESO_i + \beta_{10} RD_i + Industry\ Effects_i + \varepsilon_i \quad (4)$$

where *IMPLICIT* is a firm-specific estimate of implicit taxes derived in Jennings et al. (2009). The remaining variables are defined in Section IV.

In theory, the implicit tax rate represents a reversion of tax savings in the economic market, resulting in lower pre-tax returns (accounting or market). Jennings et al. (2009) model the implicit tax rate as a parameter that reverses firms' tax savings when effective tax rates lie below the equilibrium effective tax rate of all firms in the market. As such, their measure is effectively a tax on tax savings. In their model, these "extra" tax savings are reversed through reductions in pre-tax accounting returns. Using their model, I obtain firm-specific estimates of implicit taxes and label the variable *IMPLICIT*. The model from Jennings et al. (2009) and its estimation are fully outlined in Appendix A. To assess the robustness of my results I also use pre-tax accounting returns (*ROE*) as an alternative proxy for implicit taxes and find consistent results. Results for this sensitivity check are presented in Section VII.

Consistent with Jennings et al. (2009), *IMPLICIT* represents the extent to which "abnormal" tax savings are reversed in the economic market. For example, if a firm's *ETR* is 25%, while the average *ETR* for the industry is 35%; an implicit tax rate (*IMPLICIT*) of 50% for that same firm implies that 50% of the 10% of abnormal tax savings (25% – 35%) is lost or reversed in the economic market in the form of lower pre-tax accounting returns. For each dollar of pre-tax income the same firm pays 25 cents in explicit taxes and 5 cents [$1*(10%*50%)] in implicit taxes.

Equation (4) represents the implicit tax treatment model. To test H2 I estimate equation (4) using the same procedure used for equation (3). Because H2 predicts that after-tax CEO incentives are positively related to implicit taxes, I expect β_1 to be positive. The coefficient of interest is interpreted as the change in *IMPLICIT* associated with the use of after-tax incentives in CEO compensation, maintaining the other control variables constant.

Total CEO Compensation and CEOs' After-tax Incentives

The effect of after-tax CEO incentives on total CEO compensation is estimated using the following regression model:

$$TOT\_COMP_i = \beta_0 + \beta_1 CEOATAX_i + \beta_2 FOR\_D_i + \beta_3 CAPINT_i + \beta_4 LEV_i + \beta_5 SIZE_i + \beta_6 ROE_i + \beta_7 Std(ROE)_i + \beta_8 \Delta MVA_i + \beta_9 CEOESO_i + \beta_{10} RD_i + Industry\ Effects_i + \varepsilon_i \quad (5)$$

where *TOT_COMP* is the natural log of the CEO's total compensation. All other variables are defined in Sections IV. I use the same controls used in the prior two models to capture variation in the determinants of *CEOATAX*, which may be related to *TOT_COMP*. I omit personal CEO characteristics (e.g., age and tenure) as these are not likely associated with the decision to use after-tax incentives. In untabled results I also include the effects of CEO age, tenure, gender, and a dummy identifying participation in the compensation committee and find that my inferences remain the same. Equation (5) is estimated via OLS and employs heteroscedasticity-robust standard errors.

In H3 I predict that the use of after-tax CEO incentives should lead to higher total CEO compensation. As such, β_1 should be positive. The coefficient on *CEOATAX* in this model is interpreted as the percentage change in total CEO compensation associated with the use of after-tax incentives in CEO compensation, while holding the other variables in the model constant.

VI. EMPIRICAL RESULTS

Descriptive Statistics

TABLE 2 Panel A displays descriptive statistics for the full sample (1,298 firm observations). The mean for *ETR* is 0.319, which is comparable to that of prior studies. The mean for *IMPLICIT* is 0.396, and is comparable to that reported in Jennings et al. (2009) for their 2001-2005 regime (0.336). The mean *IMPLICIT* is less than one, suggesting that the after-tax benefits of tax avoidance are not completely offset by implicit taxes in the sample period. The average total CEO compensation (*TOT_COMP*) is $5.89 million.

Of the 1,298 firms, 24% are coded as using after-tax incentives (*CEOATAX* = 1). This frequency is somewhat lower than that reported in previous studies, where the percentage of firms using after-tax incentives ranges from 30.1 to 61.2 percent.[19] Two different mechanisms may be driving this difference; neither of which is expected to drive the main results.

First, virtually all prior studies of after-tax incentives employ relatively small samples obtained from either hand collection or survey procedures. Because my sample is larger, comprising 74% of ExecuComp firms, it may not be representative of these smaller hand-collected samples. However, this should increase the external validity of my study.

[19] For example, the use of after-tax earnings in compensation contracts is observed by Healy (1985) for 47.3% of sample firms, by Newman (1989) for 33.9% of sample firms, by Gaver et al. (1995) for 41.9% of sample firms, by Carnes and Guffey (2000) for 30.1% of sample firms, and by Phillips (2003) for 61.2% of sample firms.

Second, it could be that my estimates of *CEOATAX* contain measurement error, resulting in a lower frequency of after-tax incentives for the sample than that of after-tax incentives for the entire population. Because I require statistical significance in computing *CEOATAX* while having a relatively short time-series of observations, the estimation process may induce a Type II error. Thus, I would fail to reject the null hypothesis of no relation between CEO pay and income tax expense when the null hypothesis is indeed false; resulting in a lower percentage of firms being coded as using after-tax incentives (*CEOATAX* = 1) relative to the population. It would be unlikely for Type II errors to drive the estimation results in the second stage. For that to happen, the Type II errors would have to be correlated with the actual likelihood of using after-tax incentives *and* with each of the three outcomes of interest (Wooldridge 2002). In actuality, Type II errors should mitigate differences in outcomes, biasing the analyses against finding differences that are statistically significant. The most likely consequence of measurement error in *CEOATAX* relates to the classical errors-in-variables problem; where the estimated OLS coefficient is attenuated because measurement error increases the variance of the variable of interest, biasing the statistical tests in the second stage against finding results consistent with the hypotheses (Wooldridge 2002).

The remainder of Panel A of TABLE 2 presents descriptive statistics for the control variables, designed to capture firm characteristics that are likely associated with the choice of using after-tax incentives. Within the sample, about 52% of firms have operations outside the US. The average sample firm has 25% of its total assets in PP&E, funds 15% of its assets in long-term debt, and has $17.3 billion in total assets. The average pre-tax ROE is 23.7%, while the average change in market value of assets is 14.7%. About 28% of annual CEO compensation comes from equity grants (e.g., stock options, restricted stock). Finally, research and development as a percentage of sales (*RD*) is 3% on average.

Panel B of TABLE 2 splits the sample into observations coded as using after-tax incentives in CEO compensation and into those coded as not; and reports mean values within the two subsamples. I find univariate results consistent with the first two hypotheses, as *ETR* is significantly lower for firms with after-tax CEO incentives, while *IMPLICIT* is significantly higher for that same group of firms. While the mean *TOT_COMP* is higher for firms that use after-tax incentives, the difference in means is not statistically significant. With the exception of *CEOESO* and *RD*, which are marginally statistically significant, the control variables are not statistically different. The difference in means is economically insignificant for each of the control variables.

Finally, in Section II I argue that the inability of Phillips (2003) to find a significant relation between CEO after-tax incentives and ETRs may have been due to low statistical power, likely because of small sample size. Using similar assumptions and the *ETR* differences from TABLE 2, my sample has an estimated statistical power coefficient of 0.89. Consequently, my study has an 89% probability of finding an existing association between *ETR* and *CEOATAX*.

Simple Correlations

TABLE 3 reports key correlation coefficients. There are four main takeaways from examining these correlations. First, the negative and significant correlation between

ETR and *CEOATAX* (Pearson = -0.076, Spearman = -0.101) supports H1; in which I predict that CEOs' after-tax incentives are negatively associated with ETRs. The correlation between *Cash_ETR* and *CEOATAX* is also negative (Pearson = -0.023, Spearman = -0.025), although not statistically significant in univariate tests. Second, the negative correlations between *ETR* and *IMPLICIT* (Pearson = -0.271, Spearman = -0.383) and *Cash_ETR* and *IMPLICIT* (Pearson = -0.127, Spearman = -0.161) are consistent with implicit tax theory. In other words, firms that receive greater tax benefits appear to do so while incurring some implicit taxes in the form of lower pre-tax returns. Third, the positive correlation between *IMPLICIT* and *CEOATAX* (Pearson = 0.084, Spearman = 0.079) is consistent with the tax savings associated with the use of after-tax CEO incentives being partially offset by higher implicit taxes. Finally, the positive correlation between *TOT_COMP* and *CEOATAX* (Pearson = 0.058, Spearman = 0.059) supports H3.

Effective Tax Rates and CEOs' After-tax Compensation Incentives

TABLE 4 presents the main results testing H1; displaying the estimated coefficients from equation (3). The first column estimates equation (3) using only the control variables. The Adjusted centered R^2 in this specification is 0.041, which indicates that the control variables capture a significant amount of variation in *ETR*, which if omitted could lead to inconsistency in the *CEOATAX* coefficient.

The second column presents estimates of equation (3) after including the variable of interest. In H1 I predict that CEO after-tax incentives are associated with lower ETRs. Consistent with H1, the association between *ETR* and *CEOATAX* is negative (coefficient

= -0.026, t-stat = -3.16) and is statistically significant at the 0.01 level. The result is also economically significant. The coefficient can be interpreted as a reduction of 260 basis points in *ETR* associated with the use of after-tax incentives in CEO compensation, holding the other variables constant. In-sample, the use of after-tax CEO incentives translates into annual tax savings of $20.7 million for the average firm.[20] The use of after-tax incentives represents a decrease of 8.1% from the mean *ETR* (-0.026 / 0.319). Consistent with Newman (1989), it appears that the use of after-tax incentives in CEO compensation is effective in generating tax savings for the firm.

Implicit Tax Rates and CEOs' After-tax Compensation Incentives

TABLE 5 reports the estimation results for equation (4), which models the relation between implicit taxes and after-tax CEO incentives. I predict in H2 that CEOs with after-tax incentives will create explicit tax benefits for the firm partially at the cost of higher implicit taxes. Thus, I expect a positive relation between *IMPLICIT* and *CEOATAX*. As shown in the second column, the coefficient on *CEOATAX* is positive and significant (coefficient = 0.094, t-stat = 3.04). The use of after-tax incentives in CEO compensation is associated with a 940 base point increase in implicit taxes, controlling for other determinants of paying after-tax. That is, of the $20.7 million in additional tax savings that firms achieve from paying after-tax, $1.94 million get reversed in the economic market ($20.7 million * 0.094). While economically significant, the increase in implicit taxes as a result of paying CEOs after-tax is small relative to the tax savings

[20] The figure is calculated as mean pre-tax income for the sample ($796 million) multiplied by the coefficient on *ETR* (-0.026).

associated with after-tax incentives. This is consistent with Jennings et al. (2009), who present market-wide evidence of low implicit taxes during from 2001 to 2005.[21] The finding that firms seem to achieve significant explicit tax benefits without triggering offsetting implicit taxes should be of interest to academics. It suggests that tax planning generates significant tax savings which are not completely offset by higher implicit taxes.

Multinational firms have higher implicit tax rates (coefficient = 0.096, t-stat = 3.00), suggesting that some tax savings from international tax planning are reversed in the economic market. Firms with high capital intensity also have higher implicit tax rates (coefficient = 0.213, t-stat = 2.58), implying that some of the tax savings from depreciation allowances are lost to suppliers or customers. The positive coefficient on *SIZE* (coefficient = 0.031, t-stat = 3.12) indicates that even firms with economies-to-scale in tax planning are not able to generate tax savings that are not subject to implicit taxes. The coefficient on *ROE* is negative and statistically significant (coefficient = -0.220, t-stat = -2.71). Consistent with Berger (1993), the coefficient on *RD* is positive and statistically significant (coefficient = 0.672, t-stat = 2.40); hence, firms that achieve tax savings due to R&D tax credits seem to face lower pre-tax returns and higher implicit taxes. This is likely due to the bidding up of R&D inputs by high marginal tax firms (Berger 1993).

Total CEO Compensation and CEOs' After-tax Compensation Incentives

[21] Jennings et al. (2009) suggest that tax shelter activity is responsible for the low implicit tax rates during 2001-2005. This, they argue, is because outside parties cannot see through these complex instruments, effectively shielding the tax savings of the firm.

Estimation results for equation (5) are found in TABLE 6. Equation (5) models the relation between the log of total CEO compensation and the use of after-tax incentives in CEO compensation. H3 predicts a positive association between *TOT_COMP* and *CEOATAX*, as CEOs who are paid after-tax have more risk imposed on them. The table shows that the coefficient on *CEOATAX* is positive and significant (coefficient = 0.134, t-stat = 2.40) as predicted in H3. Hence, the use of after-tax incentives in CEO compensation is associated with a 13.4% increase in total compensation, controlling for other determinants of the decision to pay after tax. This translates into $789,528 in additional CEO compensation. The result is economically significant, and is consistent with CEOs demanding additional compensation to bear the additional risk associated with being evaluated on the tax performance of the firm.

VII. ADDITIONAL ANALYSIS

General Robustness

To examine whether the results are driven by influential observations, I re-estimate the three outcome equations after excluding influential observations.[22] The results from this procedure (untabled) are very similar to those presented in Section VI. Truncating or winsorizing all continuous variables at the 1st and 99th percentiles also lead to similar results.

Because OLS estimates tend to be less precise when the explanatory variables are highly correlated with each other, I check for the presence of multicollinearity. Multicollinearity could present a problem if the determinants of *CEOATAX* and *CEOATAX* itself are highly correlated. After examining standard multicollinearity diagnostics (untabled), I find that none of the variance inflation factors (VIFs) are greater than 3, while none of the condition indices are greater than 22. While there are no set values of these two diagnostics that serve as a bright line test for the presence of multicollinearity, VIFs smaller than 10 and condition indices smaller than 30 generally imply that multicollinearity is not a problem (Netter et al. 1983, Belsley et al. 1980).

Because the analysis is conducted using 2005 as the treatment year, it is possible that the results might not generalize to other years. Only one year is used because I require observations from prior years to create a proxy for the use of after-tax incentives, excluding the treatment year. To check whether the results hold in additional sample

[22] Influential observations are identified using the DFFITS diagnostic procedure outlined in Belsley et al. (1980).

years, I assume that the use of after-tax incentives is stable throughout the years 1998-2005 and test all three hypotheses using this expanded sample. Results from this approach are qualitatively and quantitatively similar.

Cash ETR

Because the traditional ETR measure includes tax accruals, observing a lower ETR is not sufficient to conclude that tying CEO compensation to after-tax earnings measures also leads to cash tax savings. For instance, Armstrong et al. (2009) find that tax directors are effective at lowering firms' book ETR, but not their cash ETR. To test whether paying CEOs' after-tax results in cash tax savings I estimate equation (3) after substituting *ETR* with *Cash_ETR*, calculated as cash paid for taxes during the year (data370) divided by pre-tax income (data170).

The second column of TABLE 7 displays the estimation results from this procedure. The coefficient on *CEOATAX* is negative and marginally significant (coefficient = -0.021, t-stat = -1.75). The results imply that the use of after-tax CEO incentives is associated with a 210 base point reduction in firms' cash effective tax rates, keeping constant other determinants of the decision to pay after-tax. For added robustness I follow the advice of Hanlon and Heitzman (2009), and scale cash taxes by pre-tax operating cash flows rather than pre-tax income. This procedure yields a coefficient on *CEOATAX* of -0.024 that is statistically significant at the 0.01 level, reported in the third column of TABLE 7. Overall, the evidence suggests that after-tax CEO incentives are effective in lowering both income tax expense as well as cash paid to taxing authorities.

Alternative Measure of Implicit Taxes

While *IMPLICIT* is an appropriate proxy for implicit taxes from a theoretical standpoint, its empirical implementation is likely to be noisy, as it is estimated over a short time-series and often winsorized to its theoretical bounds. The noise introduced, however, is not expected to bias the analysis in the direction of the hypothesis, as it is not likely to be systematic.[23] Nonetheless, to be confident that the documented positive relation between *IMPLICIT* and *CEOATAX* is not spurious, I use the pre-tax accounting return (*ROE*) as an alternative proxy for implicit taxes.

According to implicit tax theory, explicit tax savings are completely offset by lower accounting pre-tax rates of return, such that the after-tax return is the same for all firms in the market.[24] Thus, the theory implies that we should observe a negative relation between pre-tax ROE and *CEOATAX* if the use of after-tax CEO incentives leads to explicit tax savings that are offset by implicit taxes. To examine this possibility, I regress pre-tax accounting returns (*ROE*) on *CEOATAX*, again controlling for the determinants of paying CEOs after-tax (outlined in Section IV).[25]

Empirically, a negative relation between pre-tax ROE and *CEOATAX* may be difficult to observe. While implicit tax theory assumes that firms do not vary in their risk profiles, this is not actually the case. As such, risk differences can mask differences in the tax treatment of assets (Scholes et al. 2005). If firms' risk profiles vary systematically with *CEOATAX*, then the coefficient on *CEOATAX* will reflect not only the tax effect, but

[23] If the measurement error in the dependent variable is not systematically related to *CEOATAX*, then the most likely bias occurs in the opposite direction of the prediction (Wooldridge 2002).
[24] This argument assumes a perfect market and no risk differences between firms (Scholes et al. 2005).
[25] To avoid perfect multicollinearity, *ROE* is dropped from the set of control variables.

also differences in risk. This is unlikely to be a problem in this setting. As TABLE 2 shows, firms do not seem to vary systematically in risk across the different *CEOATAX* subgroups; as the means for common measures of risk (i.e., leverage, size, return on equity, standard deviation of earnings) are not statistically different between the two subgroups.

Estimates of the empirical association between *ROE* and *CEOATAX* are presented in TABLE 8. As predicted by implicit tax theory, the coefficient on *CEOATAX* is negative and statistically significant at the 5% level (coefficient = -0.021, t-stat = -2.37), confirming the results obtained by using *IMPLICIT* reported in Section VI.

Endogeneity

In estimating the effect of *CEOATAX* (the independent variable of interest) on *ETR*, *IMPLICIT*, and *TOT_COMP* I have effectively ignored the effects of the three outcome variables on each other. While this assumption is helpful in conducting the empirical analysis, it may not be realistic. To examine whether my results are driven by endogeneity amongst the outcome variables I re-examine the main results after employing a reverse regression procedure, similar to Scholes et al. (1990). The authors examine whether banks' investing and financing policies can be explained by the choice of tax status. However, because these policies are likely not independent from each other, the authors reverse the order of the regression (i.e., estimate a regression of tax status on investing and financing outcomes) to determine the partial correlations between tax status and various balance sheet items, while controlling for the presence of the other balance sheet items.

Following Scholes et al. (1990), I regress *CEOATAX* on *ETR*, *IMPLICIT*, *TOT_COMP*, and the usual controls. The results (untabled) show a negative and significant coefficient on *ETR* (coefficient = -0.198, t-stat = -2.43), a positive and significant coefficient on *IMPLICIT* (coefficient = 0.066, t-stat = 2.45), and a positive and significant coefficient on *TOT_COMP* (coefficient = 0.026, t-stat = 3.11). Thus, all three hypotheses are supported. In addition, these coefficients remain relatively unchanged when estimating the reverse regression model using each outcome variable separately; suggesting that endogeneity between the three outcome variables is not driving the results.[26]

Propensity Score Matching

While the OLS tests control for several factors that affect the decision to use after-tax incentives, it is still possible that there are unobservable factors not included in my analysis that also affect the incentive choice. To the extent that these unobservable effects also affect my outcome variables in the same direction as that of after-tax CEO incentives, the coefficient estimates presented in Section VI should be considered as upper bound estimates of the true parameters. To mitigate concerns over unobserved effects, I repeat my analyses using a propensity score matched pair research design. I do so because Rosenbaum (2005) demonstrates that propensity score matching can mitigate

[26] Finding the predicted associations through the use of a reverse regression brings up the possibility of reverse causality. Two observations suggest this is likely not the case. First, *CEOATAX* is an ex ante measure, estimated prior to the treatment year. Thus, it is unlikely for variables measured at time t to determine a variable constructed using data available at t-1. Second, the observed associations between *CEOATAX* and *ETR* and *CEOATAX* and *IMPLICIT* are not consistent with reverse causality. According to economic theory, high *ETR* firms should be more likely to use after-tax incentives, while high *IMPLICIT* firms should be less likely to use them. Rather, the observed associations are consistent with both H1 and H2.

the impact of unobservable effects on estimated treatment effects, as this procedure reduces sample heterogeneity.[27]

Propensity score matching allows me to compare specific economic outcomes between firms that use after-tax CEO incentives and firms that do not use them, while assuring that both samples are very similar in observable characteristics. Because both treatment and control firms are similar in the determinants of the treatment but differ in the treatment itself, this method reduces selection bias. Propensity score matching implicitly assumes that firms which are similar in observable characteristics are also similar in unobservables. However, to the extent to which this is not true, the benefits from reduction in selection bias are diminished.

To obtain propensity scores, I regress the treatment variable (*CEOATAX*) on the observable characteristics (covariates) associated with the probability of using after-tax incentives.[28] The propensity score is the estimated probability that a firm will use after-tax incentives in CEO compensation. This approach translates multiple firm characteristics into a single score. The propensity scores are then used to form pairs of treatment (*CEOATAX* = 1) and control (*CEOATAX* = 0) firms.[29] Following this matching procedure, sample means are computed for the outcome variable (i.e., *ETR*, *IMPLICIT*,

[27] Rosenbaum (2005, p. 6) states that "in observational studies, reducing heterogeneity reduces both sampling variability and sensitivity to unobserved bias – with less heterogeneity, larger biases would need to be present to explain away the same effect."
[28] The set of observational characteristics is comprised of the variables identified in Section III. I also consider the squared value of each continuous variable in the matching procedure.
[29] I use a genetic matching algorithm to perform the matching, which specifically creates matches that optimize covariate balance (Sekhon 2007). Following Sekhon (2007), I conduct one-to-one matching with replacement, although results are very similar when matching is done without replacement.

TOT_COMP) within treatment and control subgroups. The difference in means between the two subgroups represents the average treatment effect.

The goal of propensity score matching is to achieve covariate balance between treatment and control groups. Covariate balance is achieved when the determinants of the treatment (*CEOATAX* = 1) are similar between the two groups (*CEOATAX* = 0 or 1). If there are significant differences in these determinants, then the assumptions underlying the statistical tests no longer hold. The standard procedure to assess covariate balance is to test for differences in means and medians between the treatment and control groups for each of the covariates.[30] After examining covariate differences between the subgroups (untabled), I find that none of the covariate differences are statistically significant at the 10% level.

Panel A of TABLE 9 presents figures describing the effectiveness of the matching procedure. The figure on the left presents histograms of propensity scores for both treatment and control groups before and after matching. As evidenced by the histograms in the second column (post matching), the distribution of propensity scores is very similar between treatment and controls groups; suggesting that the treatment firms are well matched. The figure on the right shows a jitter plot of propensity scores for the entire sample. The figure shows that all treatment observations are successfully matched. In addition, the plot is very similar for both treatment and control units, again suggesting that treatment firms are well matched.

[30] Differences in means are evaluated using parametric t-tests, while differences in medians are evaluated using nonparametric Kolmogorov-Smirnov tests.

Panel B of TABLE 9 presents estimates of the treatment effect (*CEOATAX* = 1) for the three outcomes of interest.[31] Estimates from this section are slightly larger than, but consistent with those obtained from OLS in Section VI. The average treatment effect for *ETR* is -0.036, which is statistically significant at the 1% level. This estimate supports H1, and is in line with the OLS results from Section VI. The average effect of *CEOATAX* on *IMPLICIT* is 0.096, and is statistically significant at the 5% level. This estimate is very close to that obtained in Section VI (coefficient = 0.094), and is consistent with H2. The third column of Panel B presents the estimated effect of after-tax incentives on total compensation (*TOT_COMP*). The average treatment effect, 0.186, is also statistically significant at the 5% level. The positive association between *TOT_COMP* and *CEOATAX* supports H3, and is also consistent with the results obtained in Section VI.

A key advantage of propensity score matching is that it enables the researcher to assess the sensitivity of the estimated average treatment effects to unobservable effects. This is done by using the "Rosenbaum bounds" procedure (Rosenbaum 2002, Chapter 4), which quantifies how strong an unobserved effect would have to be to negate the estimated treatment effects computed using the propensity score matched pairs.[32] The values of Γ shown in Panel B of TABLE 9 represent the strength that an unobserved variable must have to cause the estimated treatment effects to be driven solely through

[31] Average treatment effects are average treatment effects for the treated, or the difference in means between the treatment and control groups.
[32] According to DiPrete and Gangl (2004), the procedure works by determining the bounds for the significance level of the test of the null hypothesis for the case where the unobservable effect is so strong that knowledge of the unobservable effect would almost perfectly predict which pair of matched cases would have the higher response, regardless of which observation received the treatment.

non-random assignment (DiPrete and Gangl 2004).[33] A Γ value of 1 indicates that the odds of entering the treatment sample are 50%/50%, a situation in which no bias is present. The Γ value in the first column (1.44) implies that I would have to question my conclusion of a negative treatment effect on *ETR* if an unobserved covariate increased the odds ratio of treatment assignment to differ between treatment and control cases by a factor of 1.44. That is, the unobserved effect would have to shift the assignment of *CEOATAX* from a 50%/50% probability to a 59%/41% probability assignment. The estimated Γ for the average treatment effect of *IMPLICIT* is 1.24, which implies that the analysis is robust to a shift in random assignment to a 55.5%/44.5% probability assignment. Finally, the treatment effect on *TOT_COMP* is the least robust, with a Γ of only 1.12. My conclusion of a positive effect on total CEO compensation would be called into question by a shift in random assignment to a 53%/47% probability assignment due to an unobserved covariate.

As pointed out by DiPrete and Gangl (2004), these are worst-case scenarios. A Γ value of 1.44 does not mean that there is no true negative effect of *CEOATAX* on *ETR*. It means that the confidence interval for the *ETR* effect would include zero if an unobserved variable caused the odds ratio of treatment assignment to differ between control groups by 1.44 *and* if this variable's effect on *ETR* was so strong as to almost perfectly determine whether *ETR* would be smaller for the treatment or the control case in each pair of matched cases in the data. In the case that the unobservable covariate has a weak effect on *ETR*, the conclusion of a negative effect of *CEOATAX* on *ETR* should stand,

[33] For a more detailed explanation of Γ and the odds ratio, see DiPrete and Gangl (2004).

regardless of how strongly the unobservable covariate changes the assignment. Because these two conditions seem unlikely to be met after controlling for several known determinants of paying after-tax, the results appear to be reasonably robust to the presence of correlated omitted variables.

VIII. CONCLUSION

In this study I investigate the association between CEOs' after-tax incentives and corporate tax avoidance. I first create a proxy for CEOs' after-tax incentives by estimating whether CEO cash compensation is sensitive to income tax expense, controlling for pre-tax income; then estimate the relation between this proxy and effective tax rates. I find results consistent with the use of after-tax CEO incentives being negatively associated with effective tax rates. To determine whether these tax savings are reversed in the economic market, I estimate the relation between the use of after-tax CEO incentives and firms' implicit tax rates. The results suggest that firms tradeoff tax savings and implicit taxes to a small degree. Finally, I find that total CEO compensation is positively associated with the use of after-tax CEO incentives. This result is consistent with CEOs being compensated for taking on additional compensation risk. Overall, the results provide empirical evidence consistent with economic theory regarding the use of after-tax incentives in executive compensation.

Note that the average total tax savings accrued to the firm as a result of using after-tax incentives is \$18.75 million. Meanwhile, the average cost associated with the use of after-tax incentives in CEO compensation is much smaller – only about \$790 thousand. While it may seem at first as if the benefits of using after-tax incentives greatly outweigh their costs, these costs are likely accentuated from the perspective of the firm. That is, CEOs who are paid after-tax are more likely to pay their employees after-tax, magnifying the compensation premium effect. Evidence of this is found in Phillips (2003), who finds that CEOs who are paid after-tax are more likely to evaluate their business-unit managers on the same basis. As a result, the total costs of adopting after-tax incentives can plausibly prevent firms from actually doing so.

APPENDIX A: ESTIMATION OF IMPLICIT

Jennings et al. (2009) model a parameter that captures the extent of implicit taxes in terms of the percent of the tax preference that is offset by variation in pre-tax returns. They begin by specifying t^* as the equilibrium corporate tax rate in the economy (i.e. the tax rate all firms would pay if they were to pay the same rate). Thus, firms will retain $(1 - t^*)$ of their equilibrium pre-tax return (PTR^* = equilibrium pre-tax income/owners' equity). They allow actual effective tax rates (etr) to deviate from the equilibrium tax rate (t^*) by introducing lambda (λ), the percentage change in pre-tax income retained by the company such that:

$$(1 + \lambda)(1 - t^*) = (1 - etr) \quad \text{(A1)}$$

The measure compares the firm's tax rate with that of all other firms in the market. If the firm's tax rate is less than that of the average firm, it is coded as having a preference for explicit taxes – since for each additional dollar of profit it will pay less tax than the average firm.

When λ is positive, the company has a tax "preference" and $etr < t^*$. When λ is negative, the company has an additional tax "burden" and $etr > t^*$. The authors then ignore implicit taxes for a moment to establish how tax preferences affect the after-tax performance of a firm. Thus, multiplying both sides of Equation (1) by the equilibrium pre-tax return (PTR^*) yields:

$$(PTR^*)(1 - etr) = (PTR^*)(1 + \lambda)(1 - t^*) \quad \text{(A2)}$$

Observe that $(PTR^*)(1 - etr) = ROE$, the company's actual return on equity, and that $(PTR^*)(1 - t^*) = ROE^*$, the company's equilibrium return on equity, such that:

$$ROE = (1 + \lambda)ROE^* \tag{A3}$$

In other words, assuming no implicit taxes, the firm's actual return on equity will be a function of its tax preference and of the equilibrium return on equity. The authors finally introduce implicit taxes, in the form of a "tax" on the preference (λ):

$$ROE = (1 + \lambda(1 - \delta))ROE^* \tag{A4}$$

I use maximum likelihood to estimate equation (A4) at the firm level. *ROE* is after-tax income [(data170) – (data16)] divided by total book equity (data60). Tax preference is estimated after solving for Equation (A1), where t^* is the sum of tax expense (data16) divided by the sum of pre-tax income (data170) for all firms in the sample. *ROE** is the sum of pre-tax income (data170) minus the sum of income tax expense (data16), divided by the sum of total book equity (data60); by industry, over the sample period, using the Fama-French 48 industry classification. I winsorize all values to their theoretical bounds [0,1], and label the parameter as *IMPLICIT*.

APPENDIX B: VARIABLE DEFINITIONS

Variable	Definition	Calculation
ETR	Effective tax rate	Total tax expense / pre-tax income: [data16] / [data170].
Cash_ETR	Cash effective tax rate	Total cash paid for taxes / pre-tax income: [data317] / [data170].
IMPLICIT	Implicit tax rate	Coefficient obtained from estimating equation (A5). See Appendix A.
TOT_COMP	Total CEO compensation	Natural log of total CEO compensation (ExecuComp).
CEOATAX	After-tax CEO incentive	1 if the sensitivity of CEO cash compensation is negative and statistically significant at the 10% level, 0 otherwise. See Section 3.1.
FOR_D	Foreign income dummy	1 if foreign income [data273] is not equal to zero, 0 otherwise.
CAPINT	Capital intensity	Net property, plant, and equipment / total assets: [data8] / [data6].
LEV	Leverage	Total long-term debt / total assets: [data9] / [data6].
SIZE	Size	Natural log of total market value of equity [data25*data199].
ROE	Pre-tax return on equity	(Pre-tax income – special items) / total equity:{[data170] – [data17]} / [data60].
Std(ROE)	Standard deviation of *ROE*	Standard deviation of *ROE* from t to t-4.
ΔMVA	Change in market value of assets	The market value of assets (MVA) is calculated as fiscal year-end share price times common shares outstanding plus the book value of assets: [data199]*[data25]+ [data9]; while GROWTH, or the change in total market value of assets, is (MVAt – MVAt-1) / MVAt-1.
CEOESO	CEO option awards	Black-Scholes value of annual CEO option awards divided by total CEO compensation; both found in ExecuComp.
RD	R&D intensity	Research and development expense / net sales: [data46] / [data12].

APPENDIX C: TABLES

TABLE 1

SAMPLE SELECTION AND INDUSTRY CONCENTRATION

Panel A outlines the sample selection procedure, beginning with all firms in Compustat during the 2005 fiscal year. Panel B provides the industry composition of my sample, where industry composition follows Barth et al. (1998). The second column shows the industry breakdown of all ExecuComp observations in 2005, while the final column shows the same decomposition for the full Compustat sample in 2005.

TABLE 1, continued
SAMPLE SELECTION AND INDUSTRY CONCENTRACION

Panel A: Sample Selection

All Compustat firms (Fiscal Year = 2005):	9,201
Less:	
Firms not in ExecuComp:	(7,448)
Firms with missing time-series data on Salary, Bonus, Pretax Income, or Tax Expense:	(125)
Firms with negative Pretax Income:	(230)
Firms not based in the United States:	(20)
Firms with missing control variables:	
Missing *SIC*:	(6)
Missing *PPE*:	(49)
Missing *LEV*:	(2)
Missing *SIZE*:	(1)
Missing *Std(ROE)*:	(2)
Missing *ΔMVA*:	(3)
Missing *CEOESO*:	(9)
Firms with extreme values of *ROE*:	(8)
Firms in final sample	1,298

Panel B: Industry Concentration (Barth et al. 1998)

Industry	**Sample**		**ExecuComp**		**Compustat**	
	Number	Percent	Number	Percent	Number	Percent
Mining and Construction	29	2.2%	39	2.2%	202	2.2%
Food	38	2.9%	43	2.5%	153	1.7%
Textiles, Printing, and Publishing	74	5.7%	95	5.4%	265	2.9%
Chemicals	37	2.9%	50	2.9%	186	2.0%
Pharmaceuticals	39	3.0%	61	3.5%	566	6.2%
Extractive Industries	54	4.2%	60	3.4%	331	3.6%
Durable Manufacturers	273	21.0%	362	20.7%	1,485	16.1%
Computers	162	12.5%	241	13.7%	1,163	12.6%
Transportation	55	4.2%	82	4.7%	541	5.9%
Utilities	84	6.5%	94	5.4%	349	3.8%
Retail	165	12.7%	201	11.5%	623	6.8%
Financial Institutions	171	13.2%	224	12.8%	1,277	13.9%
Insurance and Real Estate	5	0.4%	50	2.9%	1,226	13.3%
Services	107	8.2%	138	7.9%	675	7.3%
Other	5	0.4%	13	0.7%	159	1.7%
	1,298	100%	1,753	100%	9,201	100%

TABLE 2

DESCRIPTIVE STATISTICS

All variables presented are defined in Appendix B. Panel A presents descriptive statistics for the full sample (N = 1,298). Panel B presents descriptive statistics by *CEOATAX* subsamples. *ETR* is tax expense divided by pre-tax income. When *ETR* lies outside its normal range it is reset to either 0 or 1. *IMPLICIT* is a firm-specific estimate of the implicit tax rate derived in Jennings et al. (2009). Its calculation is outlined in Appendix A. *TOT_COMP* is the natural log of total CEO compensation. *CEOATAX* is 1 if the sensitivity of CEO cash compensation is negative and statistically significant at the 10% level, and 0 otherwise. *FOR_D* is 1 for firms with foreign income/loss and 0 otherwise. *CAPINT* is net property, plant, and equipment scaled by total assets. *LEV* is long-term debt scaled by total assets. *SIZE* is the natural log of total market value of equity. *ROE* is pre-tax income before special items scaled by total book equity. *Std(ROE)* is the standard deviation of *ROE* from t-4 to t. *ΔMVA* is the one-year percentage change in the market value of assets. *CEOESO* is the Black-Scholes value of annual CEO option awards divided by total CEO compensation. *RD* is research and development expenses scaled by net sales. *, **, *** indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

TABLE 2, continued
DESCRIPTIVE STATISTICS

Panel A: Descriptive Statistics (n = 1,298)

Variable	Mean	Std. Dev.	10%	Q1	Median	Q3	90%
ETR	0.319	0.140	0.152	0.279	0.339	0.374	0.400
IMPLICIT	0.396	0.469	0.000	0.000	0.000	1.000	1.000
TOT_COMP ($MM)	5.892	7.593	0.979	1.753	3.604	6.723	13.126
CEOATAX	0.240	0.427	0.000	0.000	0.000	0.000	1.000
FOR_D	0.515	0.500	0.000	0.000	1.000	1.000	1.000
CAPINT	0.246	0.223	0.016	0.070	0.172	0.369	0.606
LEV	0.153	0.140	0.000	0.020	0.130	0.251	0.335
SIZE ($MM)	17,317	81,980	365	757	2,184	7,747	28,138
ROE	0.237	0.163	0.084	0.137	0.204	0.297	0.410
Std(ROE)	0.157	0.700	0.019	0.030	0.053	0.095	0.190
ΔMVA	0.147	0.379	-0.192	-0.067	0.061	0.268	0.551
CEOESO	0.282	0.264	0.000	0.000	0.244	0.472	0.675
RD	0.030	0.062	0.000	0.000	0.000	0.027	0.122

Panel B: Subsample Means

Variable	Mean for Subsample in which CEOATAX = 1 (N = 312)	Mean for Subsample in which CEOATAX = 0 (N = 986)	t-test for Differences in Means
ETR	0.300	0.325	2.74 ***
IMPLICIT	0.466	0.374	-3.04 ***
TOT_COMP ($MM)	6.342	5.749	-1.20
FOR_D	0.516	0.515	-0.03
CAPINT	0.256	0.242	-0.95
LEV	0.162	0.151	-1.21
SIZE ($MM)	18,818	16,843	-0.37
ROE	0.228	0.240	1.20
Std(ROE)	0.139	0.163	0.52
ΔMVA	0.146	0.147	0.06
CEOESO	0.257	0.289	1.91 *
RD	0.024	0.032	1.90 *

TABLE 3

SIMPLE CORRELATIONS

	ETR	*IMPLICIT*	*TOT_COMP*	*CEOATAX*	*Cash_ETR*
ETR		**-0.271** *<.0001*	-0.007 *0.7954*	**-0.076** *0.0062*	**0.275** *<.0001*
IMPLICIT	**-0.383** *<.0001*		0.022 *0.4344*	**0.084** *0.0024*	**-0.127** *<.0001*
TOT_COMP	**-0.066** *0.0181*	**0.063** *0.0238*		**0.058** *0.038*	-0.017 *0.5461*
CEOATAX	**-0.101** *0.0003*	**0.079** *0.0042*	**0.059** *0.0336*		-0.023 *0.4075*
Cash_ETR	**0.277** *<.0001*	**-0.161** *<.0001*	-0.027 *0.3325*	-0.025 *0.3778*	

Key correlation coefficients are presented (p-values reported below); Pearson coefficients above the diagonal, Spearman coefficients below. Coefficient values in bold are statistically significant at the 0.05 level. *ETR* is tax expense divided by pre-tax income. When *ETR* lies outside its normal range it is reset to either 0 or 1. *IMPLICIT* is a firm-specific estimate of the implicit tax rate derived in Jennings et al. (2009). Its calculation is outlined in Appendix A. *TOT_COMP* is the natural log of total CEO compensation. *CEOATAX* is 1 if the sensitivity of CEO cash compensation is negative and statistically significant at the 10% level, and 0 otherwise. *Cash_ETR* is cash paid for taxes divided by pre-tax income. When *Cash_ETR* lies outside its normal range it is reset to either 0 or 1.

TABLE 4

EFFECTIVE TAX RATES AND CEOS' AFTER-TAX COMPENSATION INCENTIVES (H1)

This table presents OLS regression estimates for equation (3). t-statistics (in parenthesis) are presented below. Industry fixed-effects are included, but not reported. Standard errors are calculated using heteroscedasticity-robust standard errors. *ETR* is tax expense divided by pre-tax income. When *ETR* lies outside its normal range it is reset to either 0 or 1. *CEOATAX* is 1 if the sensitivity of CEO cash compensation is negative and statistically significant at the 10% level, and 0 otherwise. *FOR_D* is 1 for firms with foreign income/loss and 0 otherwise. *CAPINT* is net property, plant, and equipment scaled by total assets. *LEV* is long-term debt scaled by total assets. *SIZE* is the natural log of total market value of equity. *ROE* is pre-tax income before special items scaled by total book equity. *Std(ROE)* is the standard deviation of *ROE* from t-4 to t. *ΔMVA* is the one-year percentage change in the market value of assets. *CEOESO* is the Black-Scholes value of annual CEO option awards divided by total CEO compensation. *RD* is research and development expenses scaled by net sales. *, **, *** indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

TABLE 4, continued
EFFECTIVE TAX RATES AND CEOS' AFTER-TAX COMPENSATION INCENTIVES (H1)

$$ETR_i = \beta_0 + \beta_1 CEOATAX_i + \beta_2 FOR\_D_i + \beta_3 CAPINT_i + \beta_4 LEV_i + \beta_5 SIZE_i + \beta_6 ROE_i + \beta_7 Std(ROE)_i + \beta_8 \Delta MVA_i + \beta_9 CEOESO_i + \beta_{10} RD_i + Industry\ Effects_i + \varepsilon_i$$

		Dep. Variable = ETR	
Variable	**Pred. Sign**	**(1)**	**(2)**
CEOATAX	–		-0.026 ***
			(-3.16)
FOR_D		-0.008	-0.009
		(-0.88)	(-0.95)
CAPINT		-0.036	-0.037
		(-1.43)	(-1.49)
LEV		-0.027	-0.026
		(-0.77)	(-0.76)
SIZE		-0.006 **	-0.005 *
		(-2.13)	(-1.87)
ROE		0.086 ***	0.082 ***
		(3.07)	(2.90)
Std(ROE)		-0.006	-0.006
		(-0.48)	(-0.49)
ΔMVA		-0.013	-0.013
		(-1.13)	(-1.13)
CEOESO		0.004	0.002
		(0.26)	(0.11)
RD		0.082	0.072
		(0.45)	(0.40)
Industry fixed effects		Yes	Yes
Nobs		1,298	1,298
Adjusted R-squared		0.041	0.047

TABLE 5

IMPLICIT TAXES AND CEOS' AFTER-TAX COMPENSATION INCENTIVES (H2)

This table presents OLS regression estimates for equation (4). t-statistics (in parenthesis) are presented below. Industry fixed-effects are included, but not reported. Standard errors are calculated using heteroscedasticity-robust standard errors. *IMPLICIT* is a firm-specific estimate of the implicit tax rate derived in Jennings et al. (2009). Its calculation is outlined in Appendix A. *CEOATAX* is 1 if the sensitivity of CEO cash compensation is negative and statistically significant at the 10% level, and 0 otherwise. *FOR_D* is 1 for firms with foreign income/loss and 0 otherwise. *CAPINT* is net property, plant, and equipment scaled by total assets. *LEV* is long-term debt scaled by total assets. *SIZE* is the natural log of total market value of equity. *ROE* is pre-tax income before special items scaled by total book equity. *Std(ROE)* is the standard deviation of *ROE* from t-4 to t. *ΔMVA* is the one-year percentage change in the market value of assets. *CEOESO* is the Black-Scholes value of annual CEO option awards divided by total CEO compensation. *RD* is research and development expenses scaled by net sales. *, **, *** indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

TABLE 5, continued

IMPLICIT TAXES AND CEOS' AFTER-TAX COMPENSATION INCENTIVES (H2)

$$IMPLICIT_i = \beta_0 + \beta_1 CEOATAX_i + \beta_2 FOR\_D_i + \beta_3 CAPINT_i + \beta_4 LEV_i + \beta_5 SIZE_i + \beta_6 ROE_i + \beta_7 Std(ROE)_i + \beta_8 \Delta MVA_i + \beta_9 CEOESO_i + \beta_{10} RD_i + Industry\ Effects_i + \varepsilon_i$$

Variable	Pred. Sign	*Dep. Variable = IMPLICIT* (1)	(2)
CEOATAX	+		0.094 ***
			(3.04)
FOR_D		0.093 ***	0.096 ***
		(2.93)	(3.00)
CAPINT		0.208 **	0.213 **
		(2.51)	(2.58)
LEV		0.093	0.092
		(0.97)	(0.96)
SIZE		0.034 ***	0.031 ***
		(3.39)	(3.12)
ROE		-0.235	-0.220 ***
		(-2.93)	(-2.71)
Std(ROE)		0.010	0.011
		(0.54)	(0.53)
ΔMVA		-0.066 **	-0.067 **
		(-1.97)	(-2.02)
CEOESO		-0.115 **	-0.107 **
		(-2.27)	(-2.11)
RD		0.638 **	0.672 **
		(2.27)	(2.40)
Industry fixed effects		Yes	Yes
Nobs		1,298	1,298
Adjusted R-squared		0.053	0.060

TABLE 6

CEO TOTAL COMPENSATION AND CEOS' AFTER-TAX COMPENSATION INCENTIVES (H3)

This table presents OLS regression estimates for equation (5). t-statistics (in parenthesis) are presented below. Industry fixed-effects are included, but not reported. Standard errors are calculated using heteroscedasticity-robust standard errors. *TOT_COMP* is the natural log of total CEO compensation. *CEOATAX* is 1 if the sensitivity of CEO cash compensation is negative and statistically significant at the 10% level, and 0 otherwise. *FOR_D* is 1 for firms with foreign income/loss and 0 otherwise. *CAPINT* is net property, plant, and equipment scaled by total assets. *LEV* is long-term debt scaled by total assets. *SIZE* is the natural log of total market value of equity. *ROE* is pre-tax income before special items scaled by total book equity. *Std(ROE)* is the standard deviation of *ROE* from t-4 to t. *ΔMVA* is the one-year percentage change in the market value of assets. *CEOESO* is the Black-Scholes value of annual CEO option awards divided by total CEO compensation. *RD* is research and development expenses scaled by net sales. *, **, *** indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

TABLE 6, continued
CEO TOTAL COMPENSATION AND CEOS' AFTER-TAX COMPENSATION INCENTIVES (H3)

$$TOT\_COMP_i = \beta_0 + \beta_1 CEOATAX_i + \beta_2 FOR\_D_i + \beta_3 CAPINT_i + \beta_4 LEV_i + \beta_5 SIZE_i + \beta_6 ROE_i + \beta_7 Std(ROE)_i + \beta_8 \Delta MVA_i + \beta_9 CEOESO_i + \beta_{10} RD_i + Industry\ Effects_i + \varepsilon_i$$

		Dep. Variable = TOT_COMP	
Variable	**Pred. Sign**	**(1)**	**(2)**
CEOATAX	+		0.134 **
			(2.40)
FOR_D		0.139 ***	0.143 ***
		(2.60)	(2.65)
CAPINT		-0.363 ***	-0.355 ***
		(-2.69)	(-2.62)
LEV		0.868 ***	0.866 ***
		(4.63)	(4.64)
SIZE		0.397 ***	0.393 ***
		(14.47)	(13.90)
ROE		0.268 *	0.289 **
		(1.89)	(2.02)
Std(ROE)		0.029	0.030
		(1.21)	(1.19)
ΔMVA		-0.026	-0.027
		(-0.16)	(-0.16)
CEOESO		1.150 ***	1.161 ***
		(8.78)	(8.73)
RD		-0.170	-0.122
		(-0.30)	(-0.21)
Industry fixed effects		Yes	Yes
Nobs		1,298	1,298
Adjusted R-squared		0.376	0.389

TABLE 7

CASH EFFECTIVE TAX RATES AND CEOS' AFTER-TAX COMPENSATION INCENTIVES

This table presents OLS regression estimates for a regression of *Cash_ETR* on *CEOATAX* and controls. t-statistics (in parenthesis) are presented below. Industry fixed-effects are included, but not reported. Standard errors are calculated using heteroscedasticity-robust standard errors. Within the first and second columns *Cash_ETR* is cash paid for taxes divided by pre-tax income. The third column presents results where *Cash_ETR* is cash paid for taxes divided by pre-tax operating cash flows. When *Cash_ETR* lies outside its normal range it is reset to either 0 or 1. *CEOATAX* is 1 if the sensitivity of CEO cash compensation is negative and statistically significant at the 10% level, and 0 otherwise. *FOR_D* is 1 for firms with foreign income/loss and 0 otherwise. *CAPINT* is net property, plant, and equipment scaled by total assets. *LEV* is long-term debt scaled by total assets. *SIZE* is the natural log of total market value of equity. *ROE* is pre-tax income before special items scaled by total book equity. *Std(ROE)* is the standard deviation of *ROE* from t-4 to t. *ΔMVA* is the one-year percentage change in the market value of assets. *CEOESO* is the Black-Scholes value of annual CEO option awards divided by total CEO compensation. *RD* is research and development expenses scaled by net sales. *, **, *** indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

TABLE 7, continued
CASH EFFECTIVE TAX RATES AND CEOS' AFTER-TAX COMPENSATION INCENTIVES

$$Cash\_ETR_i = \beta_0 + \beta_1 CEOATAX_i + \beta_2 FOR\_D_i + \beta_3 CAPINT_i + \beta_4 LEV_i + \beta_5 SIZE_i + \beta_6 ROE_i + \beta_7 Std(ROE)_i + \beta_8 \Delta MVA_i + \beta_9 CEOESO_i + \beta_{10} RD_i + Industry\ Effects_i + \varepsilon_i$$

		Dep. Variable = Cash_ETR		
Variable	**Pred. Sign**	**(1)**	**(2)**	**(3)**
CEOATAX	–		-0.021 *	-0.025 ***
			(-1.75)	(-2.80)
FOR_D		0.007	0.006	0.004
		(0.48)	(0.44)	(-0.37)
CAPINT		-0.013	-0.015	-0.127 ***
		(-0.38)	(-0.41)	(-3.88)
LEV		-0.053	-0.053	-0.122 ***
		(-1.18)	(-1.17)	(-3.70)
SIZE		0.001	0.001	0.001
		(0.16)	(0.31)	(-0.31)
ROE		-0.031	-0.034	0.166 ***
		(-0.85)	(-0.94)	(-5.47)
Std(ROE)		-0.006	-0.006	-0.016 ***
		(-0.55)	(-0.56)	(-2.59)
ΔMVA		-0.064 ***	-0.064 ***	-0.027 **
		(-4.49)	(-4.49)	(-2.21)
CEOESO		-0.025	-0.027	-0.018
		(-1.16)	(-1.25)	(-1.05)
RD		-0.189	-0.197	-0.370 ***
		(-0.82)	(-0.86)	(-3.31)
Industry fixed effects		Yes	Yes	Yes
Nobs		1,298	1,298	1,298
Adjusted R-squared		0.053	0.054	0.115

TABLE 8

PRE-TAX INCOME AND CEOS' AFTER-TAX COMPENSATION INCENTIVES

This table presents OLS regression estimates for the regression of pre-tax return on equity (*ROE*) on *CEOATAX* and controls. t-statistics (in parenthesis) are presented below. Industry fixed-effects are included, but not reported. Standard errors are calculated using heteroscedasticity-robust standard errors. *ROE* is pre-tax income before special items scaled by total book equity. *CEOATAX* is 1 if the sensitivity of CEO cash compensation is negative and statistically significant at the 10% level, and 0 otherwise. *FOR_D* is 1 for firms with foreign income/loss and 0 otherwise. *CAPINT* is net property, plant, and equipment scaled by total assets. *LEV* is long-term debt scaled by total assets. *SIZE* is the natural log of total market value of equity. *Std(ROE)* is the standard deviation of *ROE* from t-4 to t. *ΔMVA* is the one-year percentage change in the market value of assets. *CEOESO* is the Black-Scholes value of annual CEO option awards divided by total CEO compensation. *RD* is research and development expenses scaled by net sales. *, **, *** indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

TABLE 8, continued

PRE-TAX INCOME AND CEOS' AFTER-TAX COMPENSATION INCENTIVES

$$ROE_i = \beta_0 + \beta_1 CEOATAX_i + \beta_2 FOR\_D_i + \beta_3 CAPINT_i + \beta_4 LEV_i + \beta_5 SIZE_i + \beta_6 Std(ROE)_i + \beta_7 \Delta MVA_i + \beta_8 CEOESO_i + \beta_9 RD_i + Industry\ Effects_i + \varepsilon_i$$

Variable	Pred. Sign	*Dep. Variable = ROE* (1)	(2)
CEOATAX	–		-0.021 **
			(-2.37)
FOR_D		-0.027 **	-0.027 **
		(-2.45)	(-2.50)
CAPINT		-0.021	-0.022
		(-0.74)	(-0.77)
LEV		0.013	0.013
		(0.32)	(0.33)
SIZE		0.030 ***	0.030 ***
		(9.22)	(9.33)
Std(ROE)		0.037 **	0.037 **
		(2.01)	(2.01)
ΔMVA		0.002	0.003
		(0.17)	(0.18)
CEOESO		0.019	0.017
		(0.92)	(0.83)
RD		-0.325 **	-0.331 **
		(-2.53)	(-2.58)
Industry fixed effects		Yes	Yes
Nobs		1,298	1,298
Adjusted R-squared		0.132	0.134

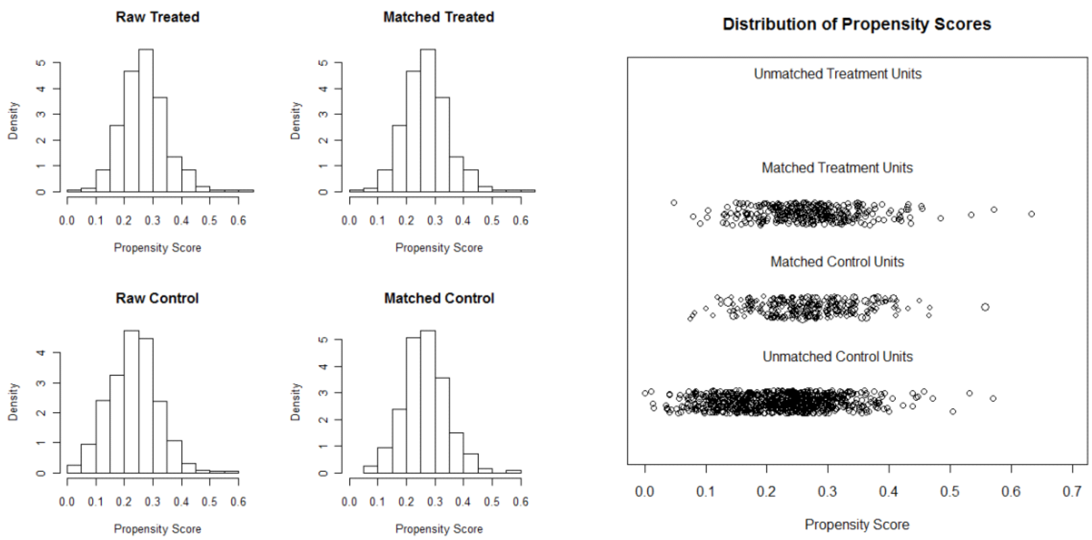
TABLE 9

PROPENSITY SCORE MATCHED PAIR ANALYSIS

This table presents results obtained using the propensity score matched pair approach. Each firm observation where *CEOATAX* = 1 is matched to an observation with a similar propensity score where *CEOATAX* = 0. The figure on the left-hand side of Panel A presents histograms of propensity scores for both treatment and control groups before and after matching. The right-hand side of Panel A presents a jitter plot of propensity scores for the entire sample. Each point represents an observation, its size being proportional to the weight given to that unit. Panel B presents the average treatment effect of *CEOATAX* on *ETR*, *IMPLICIT*, and *TOT_COMP*. Panel B also reports the associated standard errors, t-stats, p-values, and estimates of Γ. The Abadie-Imbens standard error is used because it takes into account the uncertainty of the matching procedure. The reported Γ represents the degree of departure from random assignment that an unobserved covariate would have to induce to render the treatment effect statistically insignificant. *CEOATAX* is 1 if the sensitivity of CEO cash compensation is negative and statistically significant at the 10% level, and 0 otherwise. *ETR* is tax expense divided by pre-tax income. When *ETR* lies outside its normal range it is reset to either 0 or 1. *IMPLICIT* is a firm-specific estimate of the implicit tax rate derived in Jennings et al. (2009). Its calculation is outlined in Appendix A. *TOT_COMP* is the natural log of total CEO compensation. *, **, *** indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

TABLE 9, continued
PROPENSITY SCORE MATCHED PAIR ANALYSIS

Panel A: Distribution of Propensity Scores



Panel B: Average Treatment Effects

	ETR	*IMPLICIT*	*TOT_COMP*
Average Treatment Effect	-0.035 ***	0.096 **	0.186 **
Abadie-Imbens SE	0.012	0.041	0.085
t-stat	-3.00	2.33	2.19
p-value	0.003	0.020	0.029
Γ	1.44	1.24	1.12
Nobs	624	624	624

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