On the Objective of Corporate Boards: Theory and Evidence*

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Abstract

We develop a principal-agent model linking CEO incentive pay to overstatements that allows us to differentiate between boards that prevent and boards that encourage overstatements. Using the Sarbanes-Oxley Act of 2002 as an exogenous increase in the cost of overstatements, we infer from the observed decrease in CEO incentives that boards must benefit from overstatements. As predicted by the model, empirical proxies for board benefits from overstatements are also indicative of higher CEO incentives in the cross-section, and the decrease in CEO incentives around SOX is concentrated in firms whose boards are more likely to benefit from overstatements.

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Abstract

We develop a principal-agent model linking CEO incentive pay to overstatements that allows us to differentiate between boards that prevent and boards that encourage overstatements. Using the Sarbanes-Oxley Act of 2002 as an exogenous increase in the cost of overstatements, we infer from the observed decrease in CEO incentives that boards must benefit from overstatements. As predicted by the model, empirical proxies for board benefits from overstatements are also indicative of higher CEO incentives in the cross-section, and the decrease in CEO incentives around SOX is concentrated in firms whose boards are more likely to benefit from overstatements.

CEO incentives have been linked to income-increasing accrual choices, earnings reports that systematically exceed analysts' forecasts, earnings restatements, consecutive strings of earnings increases, and securities class action law suits for financial misrepresentation.[1] In an effort to improve the quality of financial reporting, recent corporate governance reforms have put great emphasis on board and committee independence. The view behind these reforms is that independent boards act as monitors to constrain managers from enriching themselves at the expense of shareholders (early proponents are Fama (1980) and Fama and Jensen (1983)).

In contrast to the view that boards act as monitors, several arguments have been put forth in the theoretical literature for why boards, even the independent ones, may benefit from inflated share prices.[2] For example, Dye (1988) cites accounting based contracts with suppliers, debt covenants, and rate-of-return regulations as motivations for earnings overstatements. Shleifer (2004) argues that shareholders benefit from attracting external finance at lower cost or selling shares at inflated prices. In this view, boards may benefit from overstatements and encourage managers to inflate market values. If inflating earnings is a valuable skill on the part of corporate managers, then we would expect it to be reflected in CEOs' contracts.

To differentiate between these two opposing views on the boards' underlying preference for earnings overstatements, we develop a novel test based on predictions derived from a principal-agent model linking pay and the cost of overstatements. We show that when the agent's cost of overstatement increases, the change in the optimal pay-for-performance sensitivity (PPS) for a CEO depends on the board's preference. Specifically, if the board maximizes the fundamental value of a company (i.e. boards do not benefit from overstatements),

[1] For examples, see Bergstresser and Philippon (2006), Denis, Hanouna, and Sarin (2006), Efendi, Srivastava, and Swanson (2007), Kadan and Yang (2004), Cheng and Warfield (2005), Burns and Kedia (2006), Ke (2004), and Peng and Röll (2007).

[2] Boards can benefit directly by retaining their position on the board or through stock ownership, or indirectly as representatives of current shareholders.

an increase in the cost of overstatement leads to larger pay-for-performance sensitivity to induce more productive effort. However, when the board maximizes the market value (i.e. boards benefit from overstatements), optimal pay-for-performance sensitivity decreases as it is more costly to induce overstatement.[3]

To infer board objectives from observed changes in pay-for-performance sensitivities, we exploit the increase in CEOs' expected cost of overstatement with the passage of the Sarbanes-Oxley Act of 2002 (SOX). SOX increased the cost to CEOs for overstating earnings by (i) increasing the limits on financial penalties and prison terms for financial misrepresentation; (ii) requiring CEOs to reimburse any incentive based compensation or profit from the sale of stock received within 12 months after the misreporting if there is an accounting restatement as a result of misconduct; (iii) providing an additional $776 million in funding to the Securities and Exchange Commission (SEC) to step up its monitoring and enforcement efforts; and numerous other provisions.

The benefit of using SOX as an exogenous shock to firms' optimal incentive contracts is that it allows us to compare incentives within firms over time. Thus, we do not need to rely on cross-sectional variation to draw inferences about board objectives, as board objectives likely differ across firms in unobservable ways. We find that pay-for-performance sensitivity decreases significantly in the fiscal year of and after SOX, but not in other years. In particular, we estimate that incentives fall by 8% (or about $23,000 per 1% change in firm value at the median and $110,000 at the mean) from before to after SOX.

[3]We do not speak to the efficiency of overstatements. On the one hand, earnings overstatements can distort investment decisions. If firms appear more profitable than they are, managers invest in insufficiently profitable projects to mimic investment and employment of truly profitable firms (as documented in Kedia and Philippon (2007), for example). On the other hand, Shleifer and Vishny (1990) argue that short-term arbitrage being cheaper than long-term arbitrage leads to firms focusing on short-term assets to avoid prolonged underpricing. That is, firms may avoid long-term investments with positive net present values, because of fear of underpricing. Therefore, boards who set contracts that encourage CEOs to avoid underpricing (i.e. by inflating earnings) may in fact alleviate underinvestment in long-term assets.

The drawback to relying on SOX for identification is that contemporaneous events can also affect optimal incentives. For example, the NYSE and NASDAQ revised their listing standards to require independent boards following SOX. However, we find that the results are qualitatively the same if we restrict our tests to companies that were already in compliance with the new standards before SOX. This suggests that the decrease in incentives is not driven by recent governance reforms. Our results are also robust to changes in the sample window, to alternative measures of CEO incentives, and to estimating median effects instead of mean effects.

To substantiate our finding that board objectives are reflected in CEO incentives, our second approach relies on empirical proxies for board benefits from overstatements (BBO). Our model makes two predictions in this regard. First, greater benefits from overstatements should lead to higher CEO incentives. Second, CEO incentives should fall by more around SOX in firms whose boards benefit more from overstatements. To test these predictions, we use three proxies for board benefits from overstatements: (i) the Kaplan-Zingales (1997) measure of capital constraint (overstatements temporarily reduce the cost of capital); (ii) the anti-takeover score of Gompers, Ishii, and Metrick (2003) (overstatements reduce the probability of becoming a takeover target); and (iii) the portfolio turnover rate of firms' institutional blockholders (overstatements increase the return to influential short-term investors).

We find strong support for the model's predictions. Higher capital constraints, fewer anti-takeover provisions, and higher portfolio turnover rates are indicative of higher CEO incentives cross-sectionally. Moving from the 25th to the 75th percentile of each measure corresponds to differences in CEO incentives of 34%, 15%, and 18%. We also find that the decrease in CEO incentives is concentrated in firms whose boards are most likely to benefit from overstatements. This difference-in-difference approach implicitly controls for confounding events or changes in market conditions that affect firms with high and low board benefits from overstatements equally.

To summarize, we provide a novel test of board objectives with implications for corporate governance, the provision of incentives, and public policy. Taken together, our results suggest that both costs and benefits of overstatements are reflected in CEO contracts through pay for performance. Our results are consistent with those found in the literature linking CEO incentives to earnings management, but challenge the majority view that overstatements are an unintended consequence of inducing productive effort.

Our paper makes two additional contributions to the corporate governance literature. To the best of our knowledge, our model is the first to derive empirically testable implications on boards' objectives. The key point in the theoretical model is the tradeoff between inducing overstatement and inducing productive effort. Few models have captured this trade-off because most of them look at either overstatements or productive effort, but not both. For example, Stein (1989) and Fischer and Verrecchia (2004) do not consider the agent's productive effort or the optimal contract. Holmström (1999) and Gibbons and Murphy (1992) do not consider the agent's overstatement. And several models that capture this tradeoff do not consider different objectives of the principal. While Crocker and Slemrod (2005) and Kwon and Yeo (2007) do not model board benefits from overstatements, Dye (1988), Bolton, Scheinkman, and Xiong (2006) and Goldman and Slezak (2006) do not offer empirically testable implications to distinguish between boards that discourage and boards that encourage overstatements.

Second, our revealed-preference-approach to uncovering board objectives circumvents the problem of how to identify earnings overstatements. Researchers disagree whether accruals (or which accruals) are good proxies for earnings management (for examples see Schipper (1989), Beneish (2001), Dechow and Dichev (2002), Kothari, Leone, and Wasley (2005), and Ball and Shivakumar (2006)). Durtschi and Easton (2005) criticize the use of forecast errors as evidence of earnings management. Other measures of overstatements, such as shareholder litigation, earnings restatements, and enforcement actions by the SEC suffer from the drawback that only a fraction of overstatements is detected (for examples, see Dechow, Sloan, and Sweeney (1996), Wang (2004), Burns and Kedia (2006), and Peng and Röll (2007)). It is also unclear where to draw the line between desired and undesired earnings management, because the cost of overstatement increases with its magnitude. Boards should therefore be inclined to constrain management from violating GAAP for relatively minor benefits (such as a typical equity offering), but not if the survival of the company is at stake (e.g. Enron and WorldCom). Our research design has the advantage that, unlike the aforementioned studies, we infer boards' underlying objectives from the observed CEO contracts without relying on a proxy for earnings management.

I. Theoretical Model

A. Set-up

We consider a firm with one principal (e.g. a board) and one agent (e.g. a CEO).[4] The agent exerts productive effort (a) to increase a firm's underlying fundamental value, $y^t = a + \epsilon$, where ϵ follows a normal distribution $N(0, \sigma_\epsilon^2)$. As in Kwon and Yeo (2007), we allow the agent to overstate the fundamental value by m, and report the overstated performance, $y^r = y^t + m + \epsilon_m$, where ϵ_m is a random noise following a normal distribution $N(0, \epsilon_m^2)$.[5]

Both the principal and the market observe only the reported performance y^r. Neither one observes the fundamental value (y^t) or overstatement (m). However, the market can

[4] Throughout the paper, we ignore the possible agency problem between the shareholders and the board. Allowing such agency problem in this model would be an interesting topic for future research.

[5] In this paper, we do not consider the agent's incentive to understate performance to smooth income, for example. If there is such an incentive, we can regard m as the overstatement above and beyond the understated performance.

discount the reported value by its expectation on overstatement (m^e). Then, a firm's *market* performance (e.g. stock price), denoted by y^m, is determined by $y^m = y^r - m^e$.

As in Bolton, Scheinkman, and Xiong (2006), we assume that investors have heterogenous beliefs on the agent's overstatement, and that the firm's market value is determined by the most optimistic investor (or the smallest expected overstatement). In other words, investors who value the firm's shares most highly hold the long positions. More specifically, let us denote an investor $i's$ expectation on the agent's overstatement by m_i^e, where m_i is distributed over $[\underline{m}, \overline{m}]$, and $\overline{m} > \underline{m} > 0$. We assume that $E[m_i^e] = m^*$, where m^* is the agent's equilibrium overstatement level. Therefore, investors' expectations are rational on average. However, Bolton, Scheinkman, and Xiong (2006) show that if short selling is costly, the market price is determined by the most optimistic belief, $m^e = \underline{m}$.[6]

Let us define θ such that $m^e = \underline{m} = \theta m^*$. Note that $0 < \theta < 1$, since $E[m_i^e] = m^* > \underline{m} > 0$, and the market underestimates the extent of overstatement. If market uncertainty increases and investors' beliefs are more dispersed (holding the mean constant), then $\underline{m}$ (θ) becomes smaller and the market will underestimate the extent of overstatement by more. Thus, we can interpret θ as a measure of market uncertainty.

The agent's wage (w) is contingent on the firm's market value.[7] In the spirit of Holmström and Milgrom (1992), we assume a linear contract, where $w = s + \beta y^m$.[8] As long as the market underestimates overstatement (i.e. $\theta < 1$), the agent gains from overstatement.

[6] For example, D'Avolio (2002), Geczy, Musto, and Reed (2002), and Jones and Lamont (2002) provide empirical evidence that it is costly to short sell stocks.

[7] This assumption reflects the usual stock- and option based compensation packages for CEOs. Technically, we assume that the agent's reported performance is not verifiable. For example, the agent may only know the probability distribution of the true performance, and can only report the mean of the distribution. Then, the agent is unlikely to become liable for the report. Technically, this assumption allows us to avoid the revelation mechanism, as discussed in Dye (1988) and Crocker and Slemrod (2005). Under a revelation mechanism, there is no tradeoff between inducing overstatements and productive effort.

[8] For recent attempts to characterize general non-linear contracts, see Crocker and Slemrod (2005) and Hemmer, Kim, and Verrecchia (2000)

The principal is risk-neutral, and the agent is risk-averse. Specifically, the agent's utility function is given by $U(w,a,m) = -\exp^{-r(w-\frac{1}{2}a^2-\frac{k_m}{2}m^2)}$, where $\frac{1}{2}a^2$ is the cost of productive effort and $\frac{k_m}{2}m^2$ captures the cost of overstatement. We assume an increasing marginal cost of overstatement. This can be justified either because overstatement is increasingly difficult to accomplish or because the probability and cost of getting caught are growing increasingly fast with the amount of overstatement. We normalize the agent's reservation utility to -1, and assume that the principal has all the bargaining power.

The timing of the game is as follows. First, the principal and the agent sign a binding wage contract. Then, the agent chooses productive effort (a). After the fundamental value (y^t) is realized, the agent chooses his overstatement level (m) and report (y^r). The market discounts the reported value and determines the market value of the firm (y^m). The agent then gets paid based on the initial contract.

B. Overstatement and Effort

We solve the model by backward induction. Given the contract and the market's expectation, we first characterize the agent's incentive constraints for overstatement (m) and productive effort (a).

Overstatement Given fundamental value ($y^t = a + \epsilon$), the agent solves the following maximization problem to determine the optimal level of overstatement:

$$
\begin{aligned}
& \max_m \; E\left[-\exp(-r(s+\beta y^m - \frac{1}{2}a^2 - \frac{k_m}{2}m^2))\right] \\
= \quad & E\left[-\exp(-r(s+\beta(y^t+m+\epsilon_m-m^e) - \frac{1}{2}a^2 - \frac{k_m}{2}m^2))\right] \\
\Longleftrightarrow \quad & \max_m s + \beta(y^t+m-m^e) - \frac{1}{2}a^2 - \frac{k_m}{2}m^2 - \frac{r}{2}\beta^2\sigma_m^2 \; .
\end{aligned}
$$

From the first order condition, we obtain the optimal level of overstatement

$$m^*(y^t) = \frac{\beta}{k_m} \ . \tag{1}$$

Since the agent's overstatement level does not depend on the reported value, it is rational for the market to discount the reported value by a constant. Therefore, in this simple equilibrium, the agent can take the market expectation (m^e) as given.[9]

Effort Given the agent's optimal overstatement rule in (1), the agent's optimal choice of effort solves the following optimization problem:

$$\begin{aligned} & \max_a \ s + \beta E(a + \epsilon + m^*(y^t) + \epsilon_m - m^e) - \frac{1}{2}a^2 - \frac{k_m}{2} m^*(y^t)^2 - \frac{r}{2}\beta^2(\sigma_m^2 + \sigma_\epsilon^2) \\ = \ & s + \beta(a + (1-\theta)\frac{\beta}{k_m}) - \frac{1}{2}a^2 - \frac{k_m}{2}(\frac{\beta}{k_m})^2 - \frac{r}{2}\beta^2(\sigma_m^2 + \sigma_\epsilon^2) \ . \end{aligned}$$

When the agent decides on his effort level, both ϵ and ϵ_m are still random variables. The first order condition yields,

$$a^* = \beta \ . \tag{2}$$

Not surprisingly, if β increases, the agent exerts more productive effort. But from eq. (1), the agent will also overstate the fundamental value by more, which presents a potential trade-off to the principal.

[9] Kwon and Yeo (2007) show that there is another, more complex equilibrium where market expectation is a strictly increasing function of reported performance. Such an equilibrium becomes quickly untractable in this paper, but the qualitative results of this paper should hold in that equilibrium too.

The agent's participation constraint must also be binding. That is,

$$\begin{aligned} E\left[-\exp^{-r(w-\frac{1}{2}a^2-\frac{k_m}{2}m^2)}\right] &= -1 \\ &\Updownarrow \\ s+\beta(a+(1-\theta)\frac{\beta}{k_m})-\frac{1}{2}a^2-\frac{k_m}{2}(\frac{\beta}{k_m})^2-\frac{r}{2}\beta^2(\sigma_m^2+\sigma_\epsilon^2) &= 0\ . \end{aligned} \tag{3}$$

C. The Optimal Contract

We model two opposing views on board objectives: maximization of either the market value or fundamental value of the firm. To encompass both views, we assume that the board maximizes the weighted average of market performance and fundamental performance of the firm. We introduce λ to capture the weight the board places on its firm's market value instead of its fundamental value. The principal's optimization problem is thus given by

$$\max_{s,\beta}\ E[\lambda y^m+(1-\lambda)y^t-w]=a+\lambda(m-m^e)-(s+\beta(a+m-m^e))\ ,$$

subject to the incentive constraints (1) and (2), and the participation constraint (3).

Substituting (1), (2), and (3) into the principal's objective function yields

$$\max_{\beta}\ \beta+\lambda(\frac{\beta}{k_m}-\theta\frac{\beta}{k_m})-(-(\beta(\beta+(1-\theta)\frac{\beta}{k_m})-\frac{1}{2}\beta^2-\frac{k_m}{2}(\frac{\beta}{k_m})^2-\frac{r}{2}\beta^2(\sigma_m^2+\sigma_\epsilon^2))+\beta(\beta+\frac{\beta}{k_m}-\theta\frac{\beta}{k_m}))\ .$$

The first order condition is

$$1+\lambda\left(\frac{1-\theta}{k_m}\right)-(1+\frac{1}{k_m}+r(\sigma_m^2+\sigma_\epsilon^2))\beta=0\ . \tag{4}$$

This first order condition reveals the trade-off in choosing the optimal pay-for-performance sensitivity (PPS), β. The marginal benefits of raising β include the increased productive effort and the returns from the agent's overstatement, $\lambda\left(\frac{1-\theta}{k_m}\right)$. The marginal costs of raising β include the increased cost of productive effort, overstatement, and risk-premium.

The optimal PPS, β^*, is given by

$$\beta^* = \frac{1+\lambda\left(\frac{1-\theta}{k_m}\right)}{1+\frac{1}{k_m}+r(\sigma_m^2+\sigma_\epsilon^2)} \ . \tag{5}$$

We are interested in how optimal PPS changes in response to an exogenous increase in the agent's cost of overstatement, k_m. The following Proposition states that optimal PPS can either increase or decrease depending on the principal's preference (λ) and market uncertainty (θ).

Proposition 1

(i) If $\theta \geq \frac{r(\sigma_m^2+\sigma_\epsilon^2)}{1+r(\sigma_m^2+\sigma_\epsilon^2)}$, *then* $\frac{\partial\beta^*}{\partial k_m} > 0$ *regardless of* λ .

(ii) If $\theta < \frac{r(\sigma_m^2+\sigma_\epsilon^2)}{1+r(\sigma_m^2+\sigma_\epsilon^2)}$, *then* $\frac{\partial\beta^*}{\partial k_m} < 0$ *if and only if* $\lambda > \frac{1}{(1-\theta)(1+r(\sigma_m^2+\sigma_\epsilon^2))}$.

Proof. See Appendix A.

One might think that an increase in the agent's cost of overstatement will reduce PPS. However, Proposition 1 shows that if market uncertainty is sufficiently small (i.e. θ is sufficiently large), an increase in the agent's cost of overstatement will *increase* optimal PPS. Also, even when the market uncertainty is large, if the principal cares about the fundamental value of the firm (i.e. λ is sufficiently small), optimal PPS will increase too.

Intuitively, suppose that the principal does not want the agent to overstate performance, either because there is no payoff from overstating (high θ) or because the principal does not care about market value (low λ). In order to induce the agent's productive effort, the principal still has to provide positive PPS and induce overstatements. In this case, if the agent's cost of overstatement increases, the agent will reduce overstatements voluntarily, and the principal can raise PPS to induce more productive effort with less overstatement.

However, if the principal wants the agent to overstate, because overstating yields high returns (low θ) and because the principal cares about the market value of the firm(high λ), the principal would provide high PPS to induce large overstatements. In this case, as the agent's cost of overstatement increases, it becomes more costly for the principal to induce the agent to inflate the market value of the firm. Thus, the principal would have to reduce PPS.

These results are significant, as they show that we can potentially distinguish between board objectives of maximizing firms' market values and fundamental values. More specifically, when there is an exogenous increase in the agent's cost of overstatement, if the firm increases PPS, it implies either that the firm cares more about the fundamental value or that the returns from overstatement are negligible. However, if the firm decreases PPS, it would be an indication that the firm focuses relatively more on the market value, and not the fundamental value of the firm.

To the extent that we can find empirical measures of λ, we can test the model's predictions directly (i.e. without inferring board objectives). In particular, the model predicts:

Proposition 2

(i) $\frac{\partial \beta^*}{\partial \lambda} > 0$.

(ii) $\frac{\partial^2 \beta^*}{\partial k_m \partial \lambda} < 0$.

Proof. See Appendix A.

When the principal focuses more on the market value, instead of the fundamental value, the principal wishes to encourage more overstatement by providing larger incentives. Thus, as λ increases, optimal incentives increase too.

However, exactly when the principal cares more about the market value (i.e. λ is large), the effect of the increased cost of overstatement (k_m) becomes even bigger. In other words, when the agent's cost of overstatement increases, optimal incentives in firms that focus relatively more on market value will decrease by more (or increase by less) compared to firms that focus relatively more on fundamental value.

II. Empirical Analysis

A. Overview

Our empirical analysis consists of three parts. First, we utilize Proposition (1) to infer board objectives from observed changes in CEO incentives around the Sarbanes-Oxley Act. We argue that SOX increased the cost to the agent for overstating earnings directly by increasing CEOs' personal exposure to liability (e.g. through higher expected penalties) and indirectly by making financial misrepresentation more difficult (e.g. through more auditor oversight and independence).

Specifically, SOX requires CEOs to reimburse any incentive based compensation or profit from the sale of stock received within 12 months after the misreporting if there is an accounting restatement as a result of misconduct (section 304). SOX also grants the SEC power to permanently bar fraudulent executives from serving as officers or directors in the future (section 1105). Maximum criminal penalties for fraud under the Securities and Exchange Act of 1934 are increased to \$5 million and 20 years of prison (section 1106), and maximum prison terms increase to 25 years for securities fraud and up to 20 years for mail and wire fraud (sections 807 and 903). In addition, SOX requires CEOs to personally certify the correctness and completeness of the financial statement (section 302), as well as to disclose any significant deficiencies and changes in internal controls over financial misrepresentation (section 404). The purpose of these certifications is to prevent CEOs from hiding behind the veil of ignorance (Bainbridge (2007)). SOX also institutes stiff penalties for noncompliance with the certification requirements; they are punishable with up to $5 million in fines and 20 years in prison (section 906).

Furthermore, the SEC is apportioned an additional $776 million of funding for fiscal year 2003, of which $201 million are intended for higher staff compensation and at least 200 new hires (section 601). To better protect investors, SOX mandates the SEC to review each firm's disclosures at least once every three years (section 408). SOX also makes it more difficult to misrepresent a firm's financial situation by creating the Public Company Accounting Oversight Board (title I); requiring auditor independence (title II); improving the quality of audit committees through independence (section 301) and financial expertise (section 407); and providing explicit protection of whistleblowers (sections 806 and 1107).

SOX provides a quasi-experimental increase in the cost of overstatements that allows us to differentiate between board objectives:

Hypothesis 1 *According to Proposition* (1)*, an observed decrease in CEOs' pay-for-performance sensitivity in response to SOX is consistent with boards maximizing market values, but inconsistent with boards maximizing fundamental values (i.e. boards must benefit from overstatements). On the other hand, if CEO incentives increase in response to SOX, overstatements are either ineffective (i.e. there are no gains) and/or boards do not benefit from overstatements (i.e. they do not value those gains).*

The second and third parts of our empirical analysis are tests of Proposition (2). These tests are independent from inferred board objectives based on Proposition (1). The model parameter λ captures the weight boards assign to the market value as opposed to the fundamental value of the firm. While λ is not directly observable, we can proxy for λ using measures of board benefits from overstatements. Our three measures are (i) the Kaplan-Zingales measure of capital constraint (KZ-score); (ii) the number of anti-takeover provisions

(AT-score); and (iii) the turnover rate in the portfolios of large institutional shareholders (IT-score). We discuss these measures in more detail in section C.2..

The model makes the following testable predictions about the relationship between CEO incentives and board benefits:

Hypothesis 2 *According to Proposition 2 (i), higher board benefits from overstatements are reflected in higher CEO incentives.*

Hypothesis 3 *According to Proposition 2 (ii), higher board benefits from overstatement are reflected in a larger decrease in CEO incentives around SOX.*

To test hypothesis (2), we link cross-sectional variation in the proxies for board benefits to CEO incentives. In addition, using a difference-in-difference approach around SOX, we test if incentives fall by more in firms exposed to high pre-SOX board benefits. This approach allows us to rule out alternative explanations of the decrease in CEO incentives that affect boards with high and low benefits from overstatements equally.

B. Sample Description

Our sample covers over 850 large publicly traded firms with fiscal years 1999–2005. We require annual data on CEO incentives (from Execucomp) and firm characteristics (from Compustat). To avoid entry and exit effects, we only keep firms with CEO incentive data for all seven years of the sample. However, our results are qualitatively unchanged if we relax this restriction. Our findings are also robust to excluding firms with missing control variables and to excluding financial firms and utilities. Definitions of all variables are provided in Table I (Appendix B describes the calculation of PPS in more detail). Table II, panel A, displays the means of all variables for each fiscal year. Panel B provides further summary statistics for the pooled cross-section. All nominal values are expressed in December 2006 dollars (using the BLS CPI for all urban consumers – current series).

[Insert Table I here.]

[Insert Table II here.]

C. Results

C.1. The Change in CEO Incentives Around SOX

Timing of SOX and Changes in CEO Incentive Levels

To infer board objectives from hypothesis (1), we need to determine how CEO incentives change with the passage of SOX in 2002. Whether firms had sufficient time to react to SOX in the fiscal year of its passage is a priori uncertain. Therefore, we treat fiscal year 2002 as the transition year (event year $t = 0$). Initially, we consider fiscal years 1999-2001 as the pre-SOX period ($-3 \leq t \leq -1$) and fiscal years 2003-2005 as the post-SOX period ($1 \leq t \leq 3$).[10] To study changes in CEO incentives around SOX, we estimate the regression

$$incentives_{it} = \sum_{t=-2}^{+3} \delta_t D_t + \alpha_0 + \sum_{j=1}^{k} \alpha_j X_{jit} + \upsilon_i + \epsilon_{it} \ , \tag{6}$$

where t denotes the number of years before or after SOX, i denotes firms, and j denotes control variables. δ_{-2}–δ_{+3} are the coefficients of interest. D_t are year dummies, X_{jit} includes standard control variables used in the literature on executive compensation, namely market value of equity, stock price volatility, market-to-book ratio, and leverage as measures of firm characteristics; return on assets, firms' total shareholder returns, and market returns as performance controls; as well as CEO tenure, CEO turnover, and CEO option exercises.

[10] SOX was passed in July 2002 in response to the large corporate scandals in the preceding year (e.g. Enron, Tyco, Worldcom). We assume that fiscal year 2003 falls into the post-SOX period, as its begin date falls between June 2002 and May 2003. To the extent that the expected cost of overstatements increased prior to the adoption of SOX (e.g. through anticipated regulatory changes or higher scrutiny by investors and enforcement agencies), incentive effects may already be visible in earlier years.

υ_i are firm fixed effects. We estimate heteroskedasticity-robust standard errors, clustered at the firm level to address serial correlation concerns.

We set $D_t = 1$ for all fiscal years in or after event year t, and equal to zero otherwise. That is, D_t is not the usual year dummy which captures the *cumulative* change from the base year (in our case 1999). Instead, we define it to capture the *marginal* change from the prior year. This definition allows us to use the t-test for significant difference from zero to determine if incentives fall or rise from their level in the previous year. To the extent that incentives adjust slowly (i.e. over several years), we may have to add the coefficients for $t \geq 0$ to obtain the full impact of SOX on the level of incentives.

Column 1 in Table III shows the results for CEOs' pay-for-performance sensitivity as the dependent variable. Following Core and Guay (2002), we define PPS as the dollar change in executives' stock- and option holdings for a hypothetical one percent change in firm value. In column 2, the dependent variable is the PPS-ratio, an alternative measure of incentives (as used in Bergstresser and Philippon (2006) and Cornett, Marcus, and Tehranian (2008)). It scales PPS by the sum of PPS, salary, and bonus. The PPS-ratio measures the importance of incentive pay that is directly tied to the stock price relative to CEOs' total compensation. It also implicitly controls for changes in the level of CEO pay, because the denominator captures the bulk of annual CEO pay.

[Insert Table III here.]

We make the following three observations. First, we observe that PPS and the PPS-ratio fall in fiscal years 2002 and 2003 by a statistically significant amount, but not in other years: the adjustment begins immediately in the transition year and is completed by the following year. The empirical evidence thus suggests that firms adjust CEO incentives around SOX. Second, the economic magnitude of the adjustment is significant. We find that log(PPS) falls by a combined 0.239 over 2002 and 2003, which translates into an average drop in PPS of about 21.3% (or about $60,000 per 1% change in firm value at the median and $294,000 at the mean).[11] Similarly, we estimate that the PPS-ratio falls by 5.6 percentage points around SOX, or by about 20% from its average level in 2001. Third, the adjustment seems permanent in the sense that it is not reversed in fiscal years 2004 and 2005. While we estimate that log(PPS) increases in 2004 by 0.047 from the previous year, the magnitude of the increase is not sufficient to offset the earlier decrease.

Incentive Levels vs. Flow

One potential drawback to our incentive measure PPS is that it may not only reflect optimal contracting considerations, but also CEOs' timing of option exercises and stock sales. For example, if CEOs choose to unwind their holdings of exercisable options following SOX, then we could mistakenly attribute the decrease in PPS to boards maximizing market values. We provide three arguments against this alternative explanation. First, as is evident from Table II, panel A, the option exercise ratio drops sharply in 2003. Fewer exercised options translate into higher PPS. Second, we include the option exercise ratio as a control variable in our regressions. As expected, its effect on PPS is negative. Third, we use the equity grant ratio as an alternative incentive measure that is arguably less affected by CEOs' choices and market conditions. The equity grant ratio captures the fraction of annual pay in the form of stock and option grants, which are more performance sensitive than salary, bonus, and other pay. Contrary to PPS which measures the stock of incentives, the equity ratio indicates the performance sensitivity of the flow of pay. The results are presented in column (iii) and are consistent with the results for PPS. The fall in the level of incentives is mirrored in the composition of the flow of incentive pay.

[11] We calculate the percentage change as $exp(-0.239) - 1 = 21.3\%$. We calculate the dollar change by multiplying the percentage change with the mean and median values of PPS of the sample firms before SOX.

Pre-SOX vs. Post-SOX Period

While estimating year dummies sheds light on when the changes in CEO incentives take hold, the year dummies are not well-suited for interacting with proxies for board benefits from overstatements, which we do in parts 2 and 3 of our empirical analysis. Thus, for ease of interpretation and comparison of incentive levels between the pre- and post-SOX periods, we re-estimate eq. (6), but replace the year dummies with one post-SOX dummy. When using the post-SOX dummy, we cluster standard errors by firm-periods to address serial correlation concerns and to account for the fact that SOX affected the firms simultaneously. The results are displayed in Table IV.

[Insert Table IV here.]

For the first two columns, we define fiscal years 2002 and later as the post-SOX period, because CEO incentives start falling in fiscal year 2002. As a robustness check, we define all fiscal years beginning on or after August 1, 2002 as post-SOX years, as SOX was signed into law on July 30, 2002. The change in the definition of post-SOX affects a large number of firm-years. 840 observations of fiscal year 2002 and 61 observations of fiscal year 2003 are considered post-SOX in columns 1 and 2, but pre-SOX in colums 3 and 4. The results, however, are very similar across the definitions of post-SOX. We estimate that, on average, log(PPS) falls by about 0.083 and the PPS-ratio by about 0.032 from before to after SOX. The reason that the definition of post-SOX does not significantly affect the results is that CEO incentive levels in 2002 lie in between those of earlier and later years. Shifting fiscal year 2002 observations from the post- to the pre-SOX period lowers the averages in both periods, but leaves the difference largely unaffected.

Our estimates of the decrease in CEO incentives based on the post-SOX dummy are smaller than those based on year dummies. The primary reason is that CEO incentive levels were relatively low in 1999, which reduces the average incentive level in the pre-SOX period.

The economic magnitude is still significant, with an estimated decrease in PPS by $23,000 per 1% change in firm value at the median and $110,000 at the mean, especially in light of average shareholder returns of 16% in our sample (with a standard deviation of 67%).

In further robustness checks we estimate variations of eq. (6) for different event windows (± 1, 2, or 3 years around SOX, including and excluding 2002). While our estimates of the magnitude of the decrease in CEO incentives vary depending on the size of the event window, the results are qualitatively unchanged. Since our theory only makes directional predictions about CEO incentives, and not their magnitude, the choice of the event window is largely inconsequential.

Representativeness of the Mean Effect

In Tables III and IV, we report results from firm-fixed-effects regressions that estimate the mean change in CEO incentives from before to after SOX. To ensure that our results are representative of the typical firm in the sample instead of being driven by large changes in a few firms, we also estimate median regressions. The results are presented in Table V. We purge firm fixed effects by demeaning all variables.[12] The estimated median change in log(PPS) from before to after SOX is equal to the mean effect, and only slightly smaller for the PPS-ratio. We conclude that the change in CEO incentives is pervasive and representative of the typical firm in our sample.

[Insert Table V here.]

Bonus Pay

Our measures of CEO incentives emphasize CEOs' wealth gains from stock and option holdings. In practice, however, other forms of pay, such as bonuses, are also tied to firm performance and can thus provide incentives for overstatements. Our first measure of the

[12] First-differencing instead of demeaning does not materially affect the results.

level of CEO incentives — log(PPS) — completely ignores CEOs' bonus compensation. Although our second measure of CEO incentives — PPS-ratio — includes bonuses, it assumes that bonuses provide CEOs with fewer incentives to overstate performance than stock- and option holdings. To rule out the possibility that CEO incentives shifted from PPS to bonus pay around SOX without affecting the link between total CEO pay and firm performance, we take an alternative approach offered in the prior literature on CEO pay to estimate how the performance-sensitivity of CEO pay has changed around SOX. We regress bonus pay and total CEO pay on two measures of firm performance: return on assets and firm stock returns. We also interact the performance measures with the post-SOX dummy to allow for changes in the performance sensitivity of CEO pay:

$$\begin{aligned} pay_{it} &= \tau_1 \; performance_{it} + \tau_2 \; D(t \geq 0)_t \times performance_{it} + \tau_3 \; D(t \geq 0)_t \\ &+ \alpha_0 + \sum_{j=1}^{k} \alpha_j X_{jit} + \upsilon_i + \epsilon_{it} \; , \end{aligned} \tag{7}$$

where $D(t \geq 0)_t$ is a dummy set to one for fiscal years 2002–2005. The interaction term captures whether the link between pay and performance has strengthened or weakened from before to after SOX. Again, we estimate heteroskedasticity-robust standard errors, clustered at the firm-period level.

The results are displayed in Table VI. In column 1, we use bonus pay as the dependent variable. In column 2, we use total CEO pay as the dependent variable, which includes the flow of compensation (such as salary, bonus, stock and option grants), as well as changes in the value of CEOs' stock and option holdings. We use the dollar value of bonus and total pay (in $ mill.) instead of their logarithmic values, because the dollar amounts are zero or negative in a non-negligible fraction of observations. To alleviate the concern that outliers severely affect the magnitude of our estimates, we winsorize the pay and performance measures at the top and bottom percentile.

[Insert Table VI here.]

The result for bonus pay confirms that incentive pay has in fact shifted from stocks and options toward bonus pay. We estimate that bonus pay has increased by $166,000 around SOX on average. Furthermore, bonus pay does increase with return on assets (accounting performance) and with firm stock returns (market performance). Most interestingly, however, is the finding that the accounting-performance sensitivity decreases around SOX, while the market-performance sensitivity of bonus pay increases. This shift towards bonus pay and its increasing market-performance sensitivity suggest that our earlier results based on log(PPS) overstate the true decrease in incentives.

Turning to total pay, we find that it primarily responds to firms' market performance. The economic magnitude of its performance sensitivity swamps the wealth effects from bonus pay.[13] More importantly, the performance-sensitivity of total pay decreases sharply around SOX by almost half. We conclude that the declining performance sensitivity of stock and option holdings outweighs the increasing weight placed on bonus pay and its increasing market-performance sensitivity.

To summarize, we find CEO incentive levels decrease in response to SOX by an economically large and statistically highly significant amount, which is inconsistent with the hypothesis that boards discourage overstatements, but consistent with the alternative view that boards benefit from overstatements.

C.2. CEO Incentives and Board Benefits from Overstatements

We now take a completely independent approach to identify board objectives. In the previous section, we inferred board objectives from the change in CEO incentives in response to an

[13] As CEO pay is highly skewed, estimated mean effects are not representative of the typical firm. Using median regressions reduces the magnitude of the estimates by factors ranging from 2 to 10, but the qualitative findings do not change.

increase in the cost of overstatements. Here we proxy for unobservable board objectives and test if they are reflected in CEO incentives as predicted by the model. As stated in hypothesis (2), we expect firms with higher benefits from overstatements to provide more incentives. To test this prediction, we build on the following regression equation linking CEO incentives and board benefits:

$$incentives_{it} = \psi_1 \; BBO_{it} + \psi_2 \; D(t \geq 0)_t + \alpha_0 + \sum_{j=1}^{k} \alpha_j X_{jit} + \upsilon_i + \epsilon_{it} \;, \tag{8}$$

where ψ_1 is the coefficient of interest and BBO_{it} is the generic label for our proxies for board benefits from overstatements. As before, $D(t \geq 0)_t$ is the post-SOX dummy and X_{jit} includes control variables: market value of equity, stock price volatility, market-to-book ratio, leverage, firms' total shareholder returns, market returns, CEO tenure, CEO turnover, and CEO option exercises.

Our three measures of board benefits to proxy for board objectives are (i) the Kaplan-Zingales measure of capital constraint (KZ-score); (ii) the number of anti-takeover provisions (AT-score); and (iii) the turnover rate in the portfolios of large institutional shareholders (IT-score). We define all board benefit proxies such that higher values reflect higher benefits from overstatements.

The KZ-score (as used, for example, in Lamont, Polk, and Saá-Réquejo (2001), Malmendier and Tate (2005), and Bergman and Jenter (2007)) captures the likelihood of financial constraint.[14] Financially constrained firms benefit from overstating performance, as

[14] In robustness checks, we also use the WW-index (Whited and Wu 2005) to measure financial constraints. The results are robust to using the WW-index in section C.3., but not in section C.2.. In our research setting, the KZ-index is more desirable for two reasons. First, it is more highly correlated with actual debt and equity issuances in our sample. We posit that it is this access to capital markets that provides boards with benefits from overstatements. Second, the main difference between the KZ- and WW-indexes is that the WW-index includes firm size (as measured by total assets) as an indicator of financial constraint. However, firm size directly affects PPS: small firms are more financially constrained according to WW, but also offer lower PPS (e.g. because the marginal returns of CEO effort are smaller). Therefore, it is not surprising that we find no significant link between the WW-index and PPS when estimating eq. (9). However, when we estimate eq. (10), the direct effect of firm size in the WW-index on PPS is cancelled out through the difference-in-difference approach.

it helps them reduce the cost of external financing. For example, Teoh, Welch, and Wong (1998a, 1998b) and Rangan (1998) provide empirical evidence of earnings overstatements around IPOs and SEOs, and DeFond and Jiambalvo (1994) document overstatements to avoid debt-covenant violations.

Similarly, firms with fewer anti-takeover provisions are exposed to more pressure from the market for corporate control. This is a standard measure in the literature on corporate governance (introduced by Gompers, Ishii, and Metrick (2003) and subsequently used, for example, in Boone, Field, Karpoff, and Raheja (2007)). Overstatements benefit directors in firms with few anti-takeover provisions, because they make those firms less attractive takeover targets and help directors retain their positions.

The IT-score captures internal pressure from firms' investors. Large institutional shareholders with higher portfolio turnover rates are more likely to value short-term performance (see Carhart (1997) and Gaspar, Massa, and Matos (2005) for applications). Also, there is mounting evidence that large shareholders actively influence management. Carleton, Nelson, and Weisbach (1998), Becht, Franks, Mayer, and Rossi (2008), and Brav, Jiang, Partnoy, and Thomas (2008) provide detailed studies of shareholder activism by TIAA-CREF in the US, the Hermes Fund in the UK, and for a sample of US hedge funds. Cronqvist and Fahlenbrach (2007) document that blockholders affect numerous corporate outcomes, such as investment and executive pay. In the same vein, Guthrie and Sokolowsky (2008) show that firms inflate earnings around seasoned equity offerings by more in the presence of outsider blockholders than in their absence.

A difficulty in estimating eq. (8) is that CEO incentives may reflect variation in board benefits either over time and/or across firms. The fixed effects estimator, however, utilizes only within-firm variation and the between estimator uses only cross-sectional variation. Applying the random effects estimator to eq. (8) constrains the within-effect to equal the between-effect. Yet, there is no reason to expect that the difference in CEO incentives between two firms reflecting a one unit difference in BBO is equal to the change in CEO incentives within a firm for a one unit increase in BBO. Furthermore, our BBO measures vary considerably in their between- and within-variances. For example, the KZ-score exhibits as much variation in the cross-section as within firms over time. The AT-score, on the other hand, varies greatly between firms, but is close to constant for most firms over the sample period.

To allow the between-firm effects to differ from the within-firm effects, we decompose every right hand side variable from eq. (8) into a firm-fixed component (the average value for each firm — denoted by ∅) and the firm-change component (the period-to-period fluctuations around the firm average — denoted by Δ), as explained in Gould (2001):

$$\begin{aligned} incentives_{it} &= \psi_1^{\varnothing}\ BBO_i^{\varnothing} + \psi_2^{\varnothing}\ D(t \geq 0)_i^{\varnothing} + \sum_{j=1}^{k} \alpha_j^{\varnothing} X_{ji}^{\varnothing} \\ &+ \psi_1^{\Delta}\ BBO_{it}^{\Delta} + \psi_2^{\Delta}\ D(t \geq 0)_t^{\Delta} + \sum_{j=1}^{k} \alpha_j^{\Delta} X_{jit}^{\Delta} + \upsilon_i + \epsilon_{it}\ . \end{aligned} \tag{9}$$

To account for the increase in the cost of overstatements from SOX, we allow the effect of the board benefit measures to vary from before to after SOX by interacting them with pre- and post-SOX dummies. We estimate the regression using the random-effects estimator. ∅-coefficients equal the coefficients that would be estimated using the between estimator; the Δ-coefficients equal the coefficients that would be estimated using the fixed-effects estimator. $D(t \geq 0)_i^{\varnothing}$ gets dropped from the regression, because it does not vary between firms (due to our requirement of no entry into and exit from the sample). Again, we estimate heteroskedasticity-robust standard errors and account for clustering at the firm-period level.

We run six versions of regression (9): two measures for CEO incentives (log(PPS) and PPS-ratio) times three measures of board benefits. The results are displayed in Table VII. In all six cases, we obtain a positive and statistically significant estimate of the effect of board benefits on CEO incentives in the cross-section before SOX. We also find that the cross-sectional link between board benefits and CEO incentives weakens after SOX. The p-value for Δ_{sox} confirms our conjecture that $\psi_1^{\varnothing}$ is indeed smaller after SOX than before SOX. In contrast to the strong cross-sectional results, we find a substantially weaker relationship between within-firm variation in the KZ-score and CEO incentives, and no link for the AT- and IT-scores. This finding is not unexpected, given the between- and within-variation in the BBO-scores discussed previously.

[Insert Table VII here.]

To compare the economic magnitudes across the different measures of board benefits from overstatements, we evaluate the percentage difference in expected CEO incentives for moving from the 25th to the 75th percentile in the pooled cross-sectional distribution of the BBO measures. The interquartile ranges ($\Delta_{iq}BBO$) are 1.06 for the KZ-score, 4 for the AT-score, and 0.30 for the IT-score. The percentage change in CEO incentives is then given by $exp(\widehat{\psi_1^{\varnothing}} \times \Delta_{iq}BBO)$. We obtain KZ-, AT-, and IT-effects of 34%, 15%, and 18% on PPS, which translate into differences between $\$201,000$–$\$464,000$ per 1% increase in firm value at the mean of pre-SOX PPS, and $\$41,000$–$\$95,000$ at the median of pre-SOX PPS.

Our findings on the cross-sectional relationships between proxies for BBO and PPS are consistent with contemporaneous work on executive compensation: Wang (2008) finds that CEO pay-for-performance sensitivities are higher in financially constrained firms than in unconstrained firms; Fahlenbrach (2008) shows that CEO incentives decrease with the number of anti-takeover provisions; and Shin (2008) documents that short-term institutional ownership is associated with higher option compensation.

To summarize, we show that cross-sectional variation in board benefits from overstatements is reflected in cross-sectional variation in CEO incentives. We also document that the cross-sectional link between board benefits and CEO incentives is stronger before SOX than after SOX. These findings suggest that board objectives vary cross-sectionally with board benefits from overstatements.

C.3. The Change in CEO Incentives Around SOX: The Effect of Board Benefits from Overstatements

In the preceding section we show that our measures of board benefits of overstatements are consistent with the model's prediction about the effect of board preferences λ in the cross-section. In this section we go one step further and test if, within firms, incentives also fall by more around SOX in firms with high board benefits, as stated in hypothesis (3). To that end, we run the regression

$$\begin{aligned} \textit{incentives}_{it} &= \phi_1 \; BBO_{it} + \phi_2 \; D(t \geq 0)_t \times D(BBO|t < 0)_i + \phi_3 \; D(t \geq 0)_t \\ &+ \alpha_0 + \sum_{j=1}^{k} \alpha_j X_{jit} + \upsilon_i + \epsilon_{it} \;, \end{aligned} \tag{10}$$

where $D(t \geq 0)_t$ is a dummy set to one for fiscal years 2002–2005 and $D(BBO|t < 0)_i$ is a dummy that indicates high board benefits from overstatements in the period before SOX. In particular, for the time-varying KZ- and IT-scores, we average the score over the three-year pre-SOX period for each firm. We consider the upper half of the distribution to have high BBO ($D(BBO|t < 0)_i = 1$). The AT-score has negligible variation over time, and we have observations for fiscal years 2000, 2002, and 2004 only. The 2002 observations became available from IRRC in February 2002, so we use those AT-scores for determining $D(BBO|t < 0)_i$. While separating the BBO groups at the median is coarse, it is transparent and easily interpretable.[15] Thus, ϕ_2 is the coefficient of interest. A negative estimate of ϕ_2 would indicate that incentives fall by more in firms with high board benefits from

[15] The results remain qualitatively unchanged if we use continuous pre-SOX averages of the proxies for board benefits instead of their dummy versions.

overstatements before SOX.[16] To control for the possibility that the within-firm change in incentives is driven by the within-firm change in board benefits from overstatements over time, we also include the time-varying continuous measure of board benefits in the regression. X_{jit} contains the same standard determinants of CEO incentives as regression (6). As before, υ_i are firm fixed effects. We estimate heteroskedasticity-robust standard errors, clustered by firm-period.

The results are displayed in Table VIII. The coefficients are directly comparable across BBO measures for the same measure of incentives, because the interaction term uses a dummy for BBO. Our estimates are remarkably similar across the different specifications. Specifically, we find that log(PPS) falls by 0.122–0.145 more in firms with high pre-SOX board benefits than in firms with low pre-SOX board benefits. Translating these estimates into dollar figures yields an additional decrease in PPS for high BBO firms between $\$162,000$ and $\$190,000$ at the mean level of pre-SOX PPS, and between $\$31,000$ and $\$36,000$ at the median level of PPS. The results for the PPS-ratio are similar. All interaction terms, except for one, are significant at the 1% confidence level or better.

[Insert Table VIII here.]

It is worth noting that, when measuring CEO incentives with log(PPS), the coefficient for the post-SOX dummy loses its statistical significance and much of its economic magnitude compared to the specifications in Table IV. This finding suggests that the decrease in stock- and option-based incentives around SOX is fully concentrated in firms with high benefits from overstatements prior to SOX: only boards with high benefits from overstatements value market performance. When measuring CEO incentives with the PPS-ratio, however, the post-SOX dummy remains negative with sizable magnitude in all regressions. This finding

[16] Here we allow the average within-firm response of CEO incentives to SOX to vary cross-sectionally. The fixed effects estimator identifies ϕ_2, because the time-invariant board benefits variable is interacted with the time-varying post-SOX dummy.

suggests that all firms — with and without benefits from overstatements — increase the relative importance of salary and bonus pay around SOX.

The evidence in Table VIII is arguably stronger than the evidence presented in Table IV. While significant changes in PPS coincide with SOX, the estimated SOX effect potentially reflects other events or changes in market conditions. The results in Table VIII implicitly control for such confounding effects, because we compare the change in PPS around SOX between firms with high and low board benefits from overstatements. Through this difference-in-difference approach we are able to rule out alternative explanations that affect the two groups equally. For example, heterogeneity in investors' beliefs about the extent of overstatements may have decreased with the revelation of more and more accounting scandals. Growing media and social pressure on restraining skyrocketing CEO pay, especially in the form of stock options, may have also led to the change in the structure of CEO compensation.

C.4. Contemporaneous Changes in NYSE/NASDAQ Listing Requirements

Contemporaneous to SOX, NYSE and NASDAQ were in the process of revising their listing requirements. The goal of these reforms was to improve the quality of corporate governance by increasing the independence of corporate boards and their committees. In particular, the new listing requirements on the NYSE and NASDAQ require each board to have a majority of independent directors, as well as fully independent compensation and audit committees. The new NYSE and NASDAQ rules became effective with a company's first annual meeting occurring after January 15, 2004, but no later than October 31, 2004. For the majority of firms, the new requirements became binding for fiscal year 2003 reports.

We use board data provided by Riskmetrics to determine firms' compliance status. We match the Riskmetrics observation to the fiscal year into which the board meeting date falls. We classify boards as compliant or non-compliant based on their board independence in fiscal year 2002, the year prior to the rule change. Following Chhaochharia and Grinstein (2008), we reclassify directors as independent when their employment relationship terminated three or more years ago to reconcile the differences in how Riskmetrics and the NYSE/NASDAQ listing standards define independence. Of our 857 sample firms, we classify 138 as non-compliant, and lack board data for 77.

The new listing requirements had a noticeable impact on board independence. The change in board independence is evident in Table II, panel A. Firms that were failing the new director independence standards in the year prior to those rules going into effect, improved their governance drastically over the following years. In the non-compliant firms, only 42% of directors were independent before the new rules, but independence increased by 10 percentage points within one year and by 20 percentage points by 2005. On the other hand, firms that already met the requirements show an increase of only 3 percentage points from 2002 to 2005. The fraction of compliant boards in our sample jumps from 82% in 2002 to 93% in 2004.

We allow the effect of SOX on CEO incentives to differ between compliant and non-compliant firms by estimating regression (6) separately for compliers and non-compliers. Table IX displays the results. CEO incentives decrease in compliant firms, which suggests that even independent boards emphasize market values over fundamental values. Thus, we should not expect independent boards to be effective monitors of overstatements.

[Insert Table IX here.]

The economic magnitude of the change in CEO incentives, however, is much larger for non-compliers: fourfold for log(PPS) and double for PPS-ratio. The difference between the SOX effects is statistically significant at the 4.1% significance level for log(PPS), and at the 9.4% level for the PPS-ratio.

There are at least three possible explanations for this finding. First, non-compliant boards have larger equity stakes in their firms: the median pre-SOX voting power of compliant boards is only 1.3%, but 12.1% in non-compliant firms. To the extent that overstatements are beneficial to the owners of the firm, larger equity stakes translate into higher incentives for overstatements. Second, the decrease in CEO incentives in non-compliant firms is consistent with the view that oversight and incentive pay are substitutes (Holmström (1979)). The large decrease could thus reflect not just the change in the cost of overstatement, but also the improvement in the quality of corporate governance. Third, as suggested by Bertrand and Mullainathan (2001), non-independent boards may not have been setting or enforcing optimal incentive contracts. Therefore, the large decrease in CEO incentives could also be attributable to a regime shift from managerial skimming to optimal contracting.

Given the variety of possible interpretations of the larger decrease in CEO incentives in non-compliant firms compared to compliant firms, it is difficult to make any predictions about the interactions of non-compliance with the proxies for board benefits in testing hypotheses (2) and (3). Therefore, we replicate those tests for the subsample of firms in compliance with the new board independence requirement in fiscal year 2002. The results remain qualitatively, and in most cases even quantitatively, unchanged. We conclude that our findings are not attributable to the contemporaneous changes in board characteristics.

[Insert Table X here.]

[Insert Table XI here.]

III. Conclusion

Recent corporate governance reforms have put great emphasis on board independence to improve the quality of financial reporting. However, the role boards play in monitoring earnings overstatements is not yet well understood. In one view, managers seek to inflate the stock price for private benefits and corporate boards act as monitors to limit earnings overstatements. In the other view, boards themselves value overstatements and set incentive contracts to reward overstatements.

To distinguish between these two competing views on board objectives, we propose a principal-agent model linking optimal incentives to the cost of earnings overstatements. A board acting as a monitor should raise incentives following an increase in the agent's cost of overstatements to induce more productive effort. However, a board maximizing the market value of a firm should decrease incentives to reduce the cost of induced overstatements. The model enables us to infer board objectives without relying on contentious measures of earnings management.

In addition, the model offers two predictions that allow us to test whether empirical proxies for board benefits from overstatements are reflected in incentive contracts. First, boards with greater benefits from overstatements should offer higher incentives to their CEOs. Second, the more boards value overstatements, the greater should be the effect of SOX on PPS.

Our two approaches — inferring board objectives and proxying for board objectives — yield results that are consistent with each other. We find that boards of large public companies in the U.S. respond to the increase in the cost of earnings overstatements imposed by the Sarbanes-Oxley Act of 2002 by reducing CEO incentives. From this we can infer that boards must value overstatements.

Using three proxies for board benefits from overstatements (capital constraints, anti-takeover provisions, and portfolio turnover rates of institutional blockholders), we document a positive relationship between board benefits and CEO incentives. We also find that the decrease in CEO incentives is concentrated in firms whose boards benefit from overstatements. These results indicate that CEO incentives reflect boards' benefits and CEOs' costs from overstatements.

We conclude that corporate boards face a conflict of interest in their role as monitors of overstatements, and thus cannot be expected to effectively prevent overstatements.

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Appendix A

Proof of Proposition 1

(i) From (5),

$$\begin{aligned}\frac{\partial\beta^*}{\partial k_m} &= \frac{1-\lambda(1-\theta)(1+r(\sigma_m^2+\sigma_\epsilon^2))}{(k_m+k_m r(\sigma_m^2+\sigma_\epsilon^2)+1)^2} > 0 \\ &\iff \lambda < \frac{1}{(1-\theta)(1+r(\sigma_m^2+\sigma_\epsilon^2))}.\end{aligned} \tag{A-1}$$

Since $\lambda < 1$, $\frac{\partial\beta^*}{\partial k_m}$ is always positive if $\frac{1}{(1-\theta)(1+r(\sigma_m^2+\sigma_\epsilon^2))} \geq 1$, that is, if $\theta \geq \frac{r(\sigma_m^2+\sigma_\epsilon^2)}{1+r(\sigma_m^2+\sigma_\epsilon^2)}$.

(ii) If $\theta \geq \frac{r(\sigma_m^2+\sigma_\epsilon^2)}{1+r(\sigma_m^2+\sigma_\epsilon^2)}$, however, $\frac{\partial\beta^*}{\partial k_m} < 0$ if and only if $\lambda > \frac{1}{(1-\theta)(1+r(\sigma_m^2+\sigma_\epsilon^2))}$.

Proof of Proposition 2

(i) From (5), it is straightforward to show that

$$\frac{\partial\beta^*}{\partial\lambda} = \frac{\frac{1-\theta}{k_m}}{1+\frac{1}{k_m}+r(\sigma_m^2+\sigma_\epsilon^2)} > 0\ . \tag{A-2}$$

(ii) From (A-1),

$$\frac{\partial^2\beta^*}{\partial\lambda\partial k_m} = -\frac{(1-\theta)(1+r(\sigma_m^2+\sigma_\epsilon^2))}{(k_m+k_m r(\sigma_m^2+\sigma_\epsilon^2)+1)^2} < 0\ . \tag{A-3}$$

Appendix B

Details on Calculating PPS

We construct the incentive measure following Core and Guay (2002). In particular, we compute the dollar change in executives' stock- and option holdings for a hypothetical one percent change in firm value (we call this variable pay-for-performance sensitivity [PPS]). We separately calculate PPS for newly granted options, previously granted exercisable and unexercisable options, and stock holdings. Measuring PPS requires six inputs: the risk-free rate, stock price volatility, dividend yield, time to maturity, stock price, and number of options granted or held. All variables except for the risk-free rate can be obtained from Execucomp, either directly (e.g. dividend yield and volatility, stock price) or indirectly (time-to-maturity, number of options held).

Following the Execucomp convention in calculating option grant values, we winsorize volatility and dividend yields within each fiscal year. The largest and smallest values are least likely to be good representations of expectations about their future values. We replace missing values of the 3-year average dividend yield (*bs_yield*) with current dividend yields, missing values for volatility (*bs_volat*) with the Execucomp sample mean, and missing values for exercise price (*expric*) with either the market price (*mktpric*) or the average of the fiscal-year-end closing price (*prccf*) and the closing price discounted by total shareholder returns that year (*trs1yr*). We also observe that firms who make only one grant to an executive within a fiscal year often only report the total number of options granted (*soptgrnt*), but not the number of options in that grant (*numsecur*). We estimate maturity to be the difference between exercise date and grant date. Missing values are assumed to be 10 years. Some maturities are computed to be 0 years, so we replace those with 1 year. We also value the options at the end of the fiscal year, not at the time of the grant to make all values comparable and current at fiscal year end. Finally, we weight the individual grants' deltas by the grant values to each executive within each year to compute PPS from new option grants for each executive-firm-year.

Estimating the inputs for previous grants is harder. Information on the characteristics of past option grants is not available. For example, the number and value of unexercisable options are available, but we do not know the composition of the unexercisable options from previous grants. Similarly, for exercisable options, we do not know which previously granted options were exercised by the executives and which ones were kept in the portfolio. However, Core and Guay's main contribution lies in showing that imputing the missing characteristics yields a very close approximation to hand-collected, full-information option portfolios. Unfortunately, the documentation in Core and Guay does not allow us to replicate their imputation strategy directly. We encounter a number of problems. For example, the reported value of (un)exercisable options pertains only to in-the-money options, but the number of (un)exercisable options also includes out-of-the-money options. Furthermore, adjusting the value and number of unexercisable options for current year option grants imply that about half of our observations would end up with negative values. We assume that the reported number of unexercisable options held includes newly granted options, unless the number of options granted exceeds the holdings. Similar to our approach for newly granted options, we estimate the exercise price for previously granted options by appropriately discounting the adjusted fiscal-year end stock price by total shareholder returns (*trs3yr*). The maturity of unexercisable options is assumed to be one year less than the maturity of any option grant in the previous year, or 9 years if no options were granted in the previous year. The maturity of exercisable options is assumed to be 3 years less than that of unexercisable options.

Table I: **Definition of Variables**

variable	unit	transformation	definition
PPS	thsd. $	log	Δ in $ value of CEOs' equity and option holdings from a 1% increase in share price; inflation adjusted; see Core and Guay (2002) for details
PPS-ratio	fraction		PPS scaled by sum of PPS, salary, and bonus
equity-ratio	fraction		value of option and stock grants scaled by annual pay; ([blk_valu]+[rstkgrnt])/[tdc1] from Execucomp
market value	mill. $	log	$[199] * [25]$ from Compustat; inflation adjusted
stock price volatility		log	[bs_volat] from Execucomp
market-to-book ratio	multiple	log	$([6] - [60] - [74] + (199 * 25))/[6]$ from Compustat
leverage	fraction		$([9] + [34])/[6]$ from Compustat
return on assets	fraction	log(1+roa)	[18]/[6] from Compustat
shareholder return	fraction	log(1+ret)	calculated from monthly [ret] from CRSP; inflation adjusted
market return	fraction	log(1+ret)	calculated from monthly value-weighted market return incl. dividends [vwretd] from CRSP; inflation adjusted
CEO tenure	years	log	based on [becameceo] from Execucomp
option exercise ratio	fraction		fraction of exercisable options exercised; [opt_exer_val]/([opt_exer_val]+[opt_unex_exer_est_val]) from Execucomp
CEO turnover	dummy		= 1 if Δ_t[co_per_rol]$\neq 0$ from Execucomp
KZ-score			based on Kaplan and Zingales (1997); inputs are scaled by [6] (lagged) $= -1.002 * [308] - 39.368 * [127] - 1.315 * [1] + 3.139*[lev]+0.283*[mb]$ high likelihood of cap. constraints $\Rightarrow$ high benefits from overstatements
AT-score			based on Gompers, Ishii, and Metrick (2003); AT-score=18-GINDEX few anti-takeover provisions $\Rightarrow$ high benefits from overstatements
IT-score			institutional blockholders' annual portfolio turnover rate high portfolio turnover $\Rightarrow$ high benefits from overstatements

Table II: **Summary Statistics on CEO and Firm Characteristics**

Our sample covers large publicly traded firms with fiscal years 1999–2005. We require annual data on CEO incentives (from Execucomp) and firm characteristics (from Compustat). To avoid entry and exit effects, we only keep firms with CEO incentive data for all seven years of the sample. However, our results are qualitatively unchanged if we relax this restriction. Table II, panel A, displays the means of all variables for each fiscal year. We consider fiscal years 1999-2001 as pre-SOX and fiscal years 2003-2005 as post-SOX. It is a priori unclear how fiscal year 2002 is affected by SOX, so we treat it as a transition year. Panel B provides further summary statistics for the pooled cross-section.

Panel A: Means by Fiscal Year

	1999	2000	2001	2002	2003	2004	2005
PPS ($ thsd.)	2,690	2,013	1,659	1,333	1,569	1,687	1,635
PPS-ratio	0.29	0.29	0.28	0.23	0.25	0.25	0.24
equity-ratio	0.43	0.44	0.48	0.45	0.41	0.42	0.42
market value ($ mill.)	9,765	10,708	9,099	7,089	8,736	9,356	9,439
return volatility	0.40	0.46	0.48	0.50	0.48	0.45	0.39
market-to-book ratio	2.86	2.33	1.96	1.63	1.88	1.90	1.87
leverage	0.25	0.25	0.25	0.25	0.24	0.23	0.22
return on assets	0.05	0.05	-0.01	0.01	0.02	0.04	0.04
shareholder return	0.30	0.21	0.07	-0.12	0.44	0.17	0.07
market return	0.22	-0.09	-0.15	-0.22	0.27	0.10	0.05
CEO tenure	8.53	8.39	8.05	8.17	8.13	8.52	8.20
option exercise ratio	0.13	0.15	0.15	0.16	0.11	0.16	0.17
CEO turnover	0.11	0.13	0.13	0.10	0.11	0.10	0.13
KZ-score	1.12	1.06	0.80	0.60	0.66	0.59	0.54
AT-score	.	8.82	.	8.62	.	8.58	.
IT-score	0.82	0.88	0.76	0.71	0.63	0.64	0.71
compliant boards	0.77	0.79	0.80	0.82	0.88	0.93	0.95
board independence - compliers	0.70	0.72	0.73	0.75	0.76	0.77	0.78
board independence - noncompliers	0.43	0.43	0.44	0.42	0.52	0.60	0.62

Panel B: Summary Statistics

	25th %ile	50th %ile	75th %ile	mean	st dev	# obs
PPS ($ thsd.)	102	294	790	1,798	17,001	6153
PPS-ratio	0.09	0.18	0.34	0.26	0.24	6153
equity-ratio	0.19	0.46	0.67	0.43	0.29	6106
market value ($ mill.)	650	1,786	6,209	9,170	27,557	6152
return volatility	0.30	0.39	0.55	0.45	0.22	5913
market-to-book ratio	1.12	1.45	2.19	2.06	2.57	6150
leverage	0.08	0.23	0.35	0.24	0.19	6137
return on assets	0.01	0.04	0.08	0.03	0.18	6152
shareholder return	-0.16	0.07	0.32	0.16	0.67	6131
market return	-0.14	0.04	0.22	0.03	0.18	6146
CEO tenure	3.00	6.00	11.00	8.28	7.58	5891
option exercise ratio	0.00	0.00	0.19	0.15	0.26	6152
CEO turnover	0.00	0.00	0.00	0.12	0.32	6054
KZ-score	0.16	0.65	1.22	0.77	1.62	5721
AT-score	7.00	9.00	11.00	8.67	2.59	2408
IT-score	0.56	0.69	0.86	0.73	0.31	4702

Table III: **The Change in Incentives Around SOX — Year Dummies**

In this table, we document that CEO incentives decrease around the Sarbanes-Oxley Act of 2002, which was signed into law on 7/25/2002. We define 2002 as the transition year, as SOX falls into fiscal year 2002 for most companies. Fiscal years 1999–2001 are considered pre-SOX and fiscal years 2003–2005 are considered post-SOX. The year dummies are defined to capture the *marginal* effect of each year on the level of incentives and incentive pay (i.e. each year dummy captures the change from the previous year). Our measures of the level of CEO incentives are the dollar change in CEOs' stock and option holdings from a hypothetical 1% increase in firm value (PPS) in column (i); and the fraction of income derived from PPS relative to the sum of PPS, salary, and bonus (PPS-ratio) in column (ii). As a robustness check, we also look at the fraction of stock and option grants of total pay (equity-ratio) in column (iii), which captures the flow of incentives. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm level — are in parentheses. $*{*}*$, $**$, and $*$ denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	log(PPS)	PPS-ratio	equity-ratio
2000 (pre-SOX)	0.123***	0.033***	0.033*
	(0.007)	(0.001)	(0.089)
2001 (pre-SOX)	0.026	0.003	0.046***
	(0.314)	(0.533)	(0.000)
2002 (transition year)	-0.042*	-0.018***	-0.046***
	(0.083)	(0.000)	(0.000)
2003 (post-SOX)	-0.197***	-0.038***	-0.055**
	(0.001)	(0.005)	(0.041)
2004 (post-SOX)	0.047*	-0.005	0.000
	(0.078)	(0.367)	(0.988)
2005 (post-SOX)	0.016	0.000	-0.007
	(0.565)	(0.989)	(0.489)
market value (log)	0.920***	0.067***	0.064***
	(0.000)	(0.000)	(0.000)
return volatility (log)	0.168	0.001	-0.015
	(0.114)	(0.957)	(0.572)
market-to-book ratio (log)	0.210*	0.104***	0.019
	(0.053)	(0.000)	(0.386)
leverage	-0.129	-0.002	-0.009
	(0.262)	(0.936)	(0.841)
return on assets (log)	0.019	-0.038*	-0.062*
	(0.862)	(0.054)	(0.062)
shareholder return (log)	0.184***	-0.009**	-0.043***
	(0.000)	(0.042)	(0.000)
market return (log)	0.296***	0.070***	0.049
	(0.010)	(0.007)	(0.341)
CEO tenure (log)	0.442***	0.068***	-0.054***
	(0.000)	(0.000)	(0.000)
option exercise ratio	-0.185***	-0.034***	0.029**
	(0.000)	(0.000)	(0.040)
CEO turnover (dummy)	0.061	0.045***	0.017
	(0.167)	(0.000)	(0.270)
# of observations	5,549	5,549	5,511
# of firms	857	857	856
within-R^2	0.538	0.285	0.069

Table IV: **The Change in Incentives Around SOX — Post-SOX Dummy**

In this table, we simplify our regressions from Table III by replacing the year dummies with a single dummy variable to differentiate between pre- and post-SOX years. We use this specification for ease of interpretation of our subsequent results. In the first two columns, we define fiscal years 2002 and later to be post-SOX. We choose to count fiscal year 2002 toward post-SOX, because the downward adjustment in CEO incentives becomes evident in fiscal year 2002, as shown in Table III. In columns 3 and 4, we document that our finding is robust to an alternative definition of the post-SOX period. There, the post-SOX period includes all fiscal years that begin on or after 8/1/2002 (i.e. the first month after the Sarbanes-Oxley Act was signed into law on 7/25/2002). Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. $***$, $**$, and $*$ denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	fiscal year ≥ 2002		fiscal year begins ≥ 8/1/2002	
	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio
post-SOX (dummy)	-0.083***	-0.032***	-0.082***	-0.036***
	(0.000)	(0.000)	(0.000)	(0.000)
market value (log)	0.919***	0.065***	0.923***	0.068***
	(0.000)	(0.000)	(0.000)	(0.000)
return volatility (log)	0.197***	0.024*	0.174***	0.015
	(0.004)	(0.092)	(0.010)	(0.297)
market-to-book ratio (log)	0.212**	0.104***	0.225***	0.105***
	(0.012)	(0.000)	(0.007)	(0.000)
leverage	-0.130	-0.001	-0.118	0.002
	(0.176)	(0.955)	(0.219)	(0.938)
return on assets (log)	0.022	-0.039**	0.027	-0.038**
	(0.816)	(0.026)	(0.776)	(0.043)
shareholder return (log)	0.184***	-0.008**	0.184***	-0.007*
	(0.000)	(0.046)	(0.000)	(0.073)
market return (log)	-0.068*	-0.009	0.019	0.032***
	(0.061)	(0.217)	(0.710)	(0.001)
CEO tenure (log)	0.443***	0.069***	0.443***	0.069***
	(0.000)	(0.000)	(0.000)	(0.000)
option exercise ratio	-0.186***	-0.035***	-0.187***	-0.035***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO turnover (dummy)	0.061	0.044***	0.066*	0.046***
	(0.137)	(0.000)	(0.099)	(0.000)
# of observations	5,549	5,549	5,549	5,549
# of firms	857	857	857	857
within-R^2	0.537	0.280	0.536	0.278

Table V: **The Change in Incentives Around SOX — Median Regression**

In Tables III and IV, we report results from firm-fixed-effects regressions that estimate the mean change in CEO incentives from before to after SOX. To ensure that our results are representative of the typical firm in the sample (instead of being driven by large changes in a few firms), we also estimate median regressions. We purge firm fixed effects by demeaning all variables. In columns 1 and 2, the post-SOX period includes fiscal years 2002 and later. In columns 3 and 4, the post-SOX period includes all fiscal years that begin on or after 8/1/2002. Two-sided p-values are in parentheses. ∗∗∗, ∗∗, and ∗ denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	fiscal year ≥ 2002		fiscal year begins ≥ 8/1/2002	
	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio
post-SOX (dummy)	-0.080***	-0.020***	-0.084***	-0.022***
	(0.000)	(0.000)	(0.000)	(0.000)
market value (log)	0.884***	0.062***	0.889***	0.063***
	(0.000)	(0.000)	(0.000)	(0.000)
return volatility (log)	0.054*	0.014*	0.026	0.009
	(0.074)	(0.063)	(0.445)	(0.119)
market-to-book ratio (log)	0.302***	0.088***	0.316***	0.089***
	(0.000)	(0.000)	(0.000)	(0.000)
leverage	-0.048	-0.002	-0.029	-0.003
	(0.408)	(0.889)	(0.650)	(0.778)
return on assets (log)	0.041	-0.073***	0.050	-0.066***
	(0.362)	(0.000)	(0.303)	(0.000)
shareholder return (log)	0.121***	-0.003	0.127***	-0.002
	(0.000)	(0.372)	(0.000)	(0.514)
market return (log)	-0.031	0.004	0.053	0.029***
	(0.265)	(0.581)	(0.149)	(0.000)
CEO tenure (log)	0.386***	0.042***	0.383***	0.041***
	(0.000)	(0.000)	(0.000)	(0.000)
option exercise ratio	-0.137***	-0.028***	-0.141***	-0.029***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO turnover (dummy)	0.014	0.017***	0.022	0.017***
	(0.487)	(0.001)	(0.334)	(0.000)
# of observations	5,549	5,549	5,549	5,549
# of firms	857	857	857	857
Pseudo-R^2	0.394	0.162	0.393	0.160

Table VI: **The Changing Link between CEO Pay and Firm Performance**

Our first measure of the level of CEO incentives — log(PPS) — has the potential drawback that it does not include CEOs' bonus compensation, which can also be tied to firm performance. Although our second measure of CEO incentives — PPS-ratio — does include bonuses, it assumes that bonuses provide CEOs with fewer incentives to overstate performance than stock- and option holdings. To rule out the possibility that CEO incentives shifted from PPS to bonus pay around SOX without affecting the link between total CEO pay and firm performance, we take an alternative approach offered in the prior literature on CEO pay. To this end, we regress CEO pay (in $ mill.) on two measures of firm performance: return on assets and firm stock returns. We also interact the performance measures with the post-SOX dummy to allow for changes in the performance sensitivity of CEO pay. In column 1, we only consider bonuses. In column 2, we consider total CEO pay, which includes both the flow of compensation (e.g. stock and option grants, salary, and bonus) as well as changes in the value of CEOs' stock- and option holdings. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. $***$, $**$, and $*$ denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	Bonus Pay	Total Pay
post-SOX (dummy)	0.166***	0.994
	(0.000)	(0.500)
market value (log)	0.326***	-1.470
	(0.000)	(0.625)
return volatility (log)	-0.231***	-15.634***
	(0.002)	(0.001)
market-to-book ratio (log)	-0.257***	32.388***
	(0.000)	(0.000)
leverage	0.008	17.393
	(0.939)	(0.157)
return on assets (log)	0.990***	-31.517
	(0.000)	(0.204)
return on assets × post-SOX	-0.450**	1.351
	(0.032)	(0.952)
shareholder return (log)	0.139***	65.414***
	(0.000)	(0.000)
shareholder return (log) × post-SOX	0.127***	-29.929***
	(0.009)	(0.000)
market return (log)	-0.072	19.384**
	(0.441)	(0.012)
market return (log) × post-SOX	-0.016	-1.614
	(0.889)	(0.860)
CEO tenure (log)	0.031	4.393***
	(0.277)	(0.000)
option exercise ratio	0.058	-1.024
	(0.173)	(0.694)
CEO turnover (dummy)	-0.031	3.710*
	(0.497)	(0.068)
# of observations	5,549	5,361
# of firms	857	857
within-R^2	0.103	0.232

Table VII: **The Link between CEO Incentives and Board Benefits from Overstatements**

Our model predicts that greater board benefits from overstatements lead to higher CEO incentives. This table presents empirical evidence linking board benefits to incentives. We use three continuous proxies of board benefits: (i) the Kaplan-Zingales measure of capital constraint; (ii) the anti-takeover score of Gompers et. al.; and (iii) the portfolio turnover rate of firms' institutional blockholders. We employ generalized random effects regressions to allow the between-firm effect of each right hand side variable to differ from its within-firm effect. The regressions include all the previous control variables, including the post-SOX dummy. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. $***$, $**$, and $*$ denote significant differences from zero at the 1%, 5%, and 10% confidence levels. p-values for Δ_{sox} provide the confidence level for rejecting the null hypothesis that the link between board benefits from overstatements and CEO incentives is stronger after SOX than before SOX.

BBO measure	KZ capital constraints		anti-takeover provisions		institutional investor horizon	
	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio
Between Effects: Utilizing Variation Between Firms						
BBO-score × pre-SOX	0.273***	0.055***	0.034**	0.011***	0.563**	0.109**
	(0.001)	(0.000)	(0.049)	(0.000)	(0.016)	(0.011)
BBO-score × post-SOX	0.223***	0.045***	0.005	0.006**	0.359*	0.069*
	(0.005)	(0.001)	(0.698)	(0.012)	(0.082)	(0.070)
p-value for Δ_{sox}	0.035	0.024	0.010	0.011	0.059	0.044
Within Effects: Utilizing Variation Within Firms Over Time						
BBO-score × pre-SOX	0.062***	0.024***	0.020	0.005	-0.082	0.006
	(0.007)	(0.000)	(0.650)	(0.540)	(0.203)	(0.623)
BBO-score × post-SOX	0.041	0.017**	0.018	0.002	0.013	-0.000
	(0.121)	(0.010)	(0.592)	(0.766)	(0.817)	(0.990)
p-value for Δ_{sox}	0.261	0.140	0.484	0.389	0.858	0.347
# of observations	5,217	5,217	2,249	2,249	4,287	4,287
# of firms	813	813	838	838	792	792
overall-R^2	0.640	0.382	0.646	0.355	0.633	0.368

Table VIII: **The Change in Incentives around SOX: The Effect of Board Benefits from Overstatements**

In this table we test the model's prediction that around SOX incentives will fall by more in firms with higher benefits from overstatements before SOX. *post-SOX* equals one for fiscal years 2002 – 2005, and zero otherwise. *pre-SOX KZ/IT-dummy* equals one if the mean value of the KZ/IT-scores over the pre-SOX period falls in the upper half of the distribution, and zero otherwise. Similarly, *pre-SOX AT-dummy* equals one if the number of anti-takeover provisions in fiscal year 2002 places a firm in the lower half of the distribution. We also control for the variation in the KZ/AT/IT-scores over time, as well as all other previous controls. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. ∗∗∗, ∗∗, and ∗ denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

BBO measure	KZ capital constraints		anti-takeover provisions		institutional investor horizon	
	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio
post-SOX × pre-SOX BBO-dummy	-0.124***	-0.018***	-0.145***	-0.017*	-0.122***	-0.020***
	(0.000)	(0.005)	(0.002)	(0.055)	(0.001)	(0.008)
BBO-score	0.045**	0.020***	0.018	0.003	-0.058	-0.001
	(0.012)	(0.000)	(0.330)	(0.347)	(0.131)	(0.867)
post-SOX (dummy)	-0.017	-0.021***	0.022	-0.019***	-0.039	-0.022***
	(0.482)	(0.000)	(0.450)	(0.002)	(0.148)	(0.000)
# of observations	5,096	5,096	2,217	2,217	4,041	4,041
# of firms	779	779	812	812	700	700
within-R^2	0.544	0.298	0.559	0.235	0.540	0.282

Table IX: **The Change in Incentives Around SOX: The Effect of Non-Compliance with the New NYSE/NASDAQ Listing Requirements**

This table replicates the tests reported in columns 1 and 2 of Table IV, except that we run the regressions separately for firms whose boards of directors were compliant and non-compliant with the new NYSE/NASDAQ listing requirements for board independence. We determine compliance status in fiscal year 2002, which for most firms is the year preceding the announcement of the new governance standards. CEO incentives decreased even in compliant firms, although by a smaller magnitude than in non-compliant firms, indicating that our results are fully attributable to the contemporaneous changes in governance. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. ∗∗∗, ∗∗, and ∗ denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	log(PPS)		PPS-ratio	
	compliant	non-compliant	compliant	non-compliant
post-SOX (dummy)	-0.052***	-0.206***	-0.027***	-0.045***
	(0.006)	(0.006)	(0.000)	(0.000)
market value (log)	0.923***	0.925***	0.079***	0.039**
	(0.000)	(0.000)	(0.000)	(0.028)
return volatility (log)	0.127**	0.710***	0.029**	0.090**
	(0.033)	(0.004)	(0.015)	(0.013)
market-to-book ratio (log)	0.254***	-0.061	0.090***	0.154***
	(0.000)	(0.885)	(0.000)	(0.000)
leverage	-0.110	-0.391	-0.035	-0.068
	(0.322)	(0.222)	(0.164)	(0.175)
return on assets (log)	0.059	0.113	-0.045*	0.006
	(0.536)	(0.696)	(0.081)	(0.872)
shareholder return (log)	0.223***	0.173*	-0.007	-0.013
	(0.000)	(0.072)	(0.111)	(0.244)
market return (log)	-0.081**	-0.021	-0.011	0.008
	(0.030)	(0.824)	(0.115)	(0.737)
CEO tenure (log)	0.400***	0.665***	0.057***	0.112***
	(0.000)	(0.000)	(0.000)	(0.000)
option exercise ratio	-0.180***	-0.299**	-0.041***	-0.027
	(0.000)	(0.034)	(0.000)	(0.168)
CEO turnover (dummy)	0.007	0.244**	0.033***	0.070***
	(0.877)	(0.050)	(0.000)	(0.001)
# of observations	4,218	853	4,218	853
# of firms	642	138	642	138
within-R^2	0.551	0.450	0.268	0.399

Table X: **The Link between CEO Incentives and Board Benefits from Overstatements: Excluding Firms Affected by New NYSE/NASDAQ Listing Requirements**

This table replicates the tests reported in Table VII, except that we restrict the sample to firms whose boards of directors were compliant with the new NYSE/NASDAQ listing requirements for board independence. This restriction ensures that our results are not driven by the contemporaneous changes in governance. The regressions include all the previous control variables, including the post-SOX dummy. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. $***$, $**$, and $*$ denote significant differences from zero at the 1%, 5%, and 10% confidence levels. p-values for Δ_{sox} provide the confidence level for rejecting the null hypothesis that the link between board benefits from overstatements and CEO incentives has strengthened around SOX.

BBO measure	KZ capital constraints		anti-takeover provisions		institutional investor horizon	
	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio
Between Effects: Utilizing Variation Between Firms						
BBO-score × pre-SOX	0.275***	0.048***	0.024	0.008***	0.530**	0.086*
	(0.000)	(0.001)	(0.187)	(0.009)	(0.046)	(0.071)
BBO-score × post-SOX	0.209***	0.036***	-0.019	0.003	0.333	0.049
	(0.002)	(0.009)	(0.172)	(0.274)	(0.145)	(0.234)
p-value for Δ_{sox}	0.009	0.031	0.001	0.014	0.101	0.110
Within Effects: Utilizing Variation Within Firms Over Time						
BBO-score × pre-SOX	0.052*	0.026***	-0.023	0.003	-0.054	0.008
	(0.054)	(0.000)	(0.623)	(0.717)	(0.490)	(0.589)
BBO-score × post-SOX	0.070**	0.020***	0.036	-0.001	-0.051	-0.005
	(0.037)	(0.010)	(0.401)	(0.856)	(0.431)	(0.642)
p-value for Δ_{sox}	0.678	0.217	0.782	0.368	0.511	0.246
# of observations	3,929	3,929	1,746	1,746	3,280	3,280
# of firms	601	601	631	631	597	597
overall-R^2	0.653	0.367	0.668	0.346	0.643	0.343

Table XI: **The Impact of Board Benefits from Overstatements on the Change in Incentives around SOX: Excluding Firms Affected by New NYSE/NASDAQ Listing Requirements**

This table replicates the tests reported in Table VIII, except that we restrict the sample to firms whose boards of directors were compliant with the new NYSE/NASDAQ listing requirements for board independence. This restriction ensures that our results are not driven by the contemporaneous changes in governance. *post-SOX* equals one for fiscal years 2002 – 2005, and zero otherwise. *pre-SOX KZ/IT-dummy* equals one if the mean value of the KZ/IT-scores over the pre-SOX period falls in the upper half of the distribution, and zero otherwise. Similarly, *pre-SOX AT-dummy* equals one if the number of anti-takeover provisions in fiscal year 2002 places a firm in the lower half of the distribution. We also control for the variation in the KZ/AT/IT-scores over time, as well as all the previous control variables. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. $***$, $**$, and $*$ denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

BBO measure	KZ capital constraints		anti-takeover provisions		institutional investor horizon	
	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio	log(PPS)	PPS-ratio
post-SOX × pre-SOX BBO-dummy	-0.106***	-0.010	-0.209***	-0.022**	-0.128***	-0.017**
	(0.002)	(0.135)	(0.000)	(0.025)	(0.001)	(0.042)
BBO-score	0.055**	0.024***	0.008	0.001	-0.092**	-0.003
	(0.015)	(0.000)	(0.718)	(0.829)	(0.043)	(0.740)
post-SOX (dummy)	0.009	-0.020***	0.073**	-0.016**	-0.003	-0.020***
	(0.721)	(0.000)	(0.018)	(0.013)	(0.908)	(0.000)
# of observations	3,858	3,858	1,729	1,729	3,116	3,116
# of firms	582	582	618	618	534	534
within-R^2	0.556	0.282	0.590	0.245	0.559	0.261