TWO ESSAYS ON INCENTIVES

A Dissertation

by

BROOKE W. STANLEY

Submitted to the Office of Graduate Studies of
Texas A&M University
in partial fulfillment of the requirements for the degree of

DOCTOR OF PHILOSOPHY

May 2008

Major Subject: Finance

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Approved by:

Chair of Committee,	Shane A. Johnson
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ABSTRACT

Two Essays on Incentives. (May 2008)

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Chair of Advisory Committee: Dr. Shane A. Johnson

I examine two sets of incentives faced by corporate CEOs to determine how they respond to those incentives. I compare firms that restate financial statements to firms that do not restate to test the hypotheses that bank monitoring should provide incentives to deter misreporting. For relatively less (more) severe misreporting, I find the likelihood of misreporting is positively related (unrelated) to bank borrowing, and that *ex ante* changes in bank debt are positive (unrelated) for misreporting firms versus control firms. These results suggest that bank monitoring is insufficient to deter or detect misreporting, rather that it may provide incentives for managers to engage in relatively less severe misreporting, consistent with the "debt covenant hypothesis".

I next examine the incentives that CEOs have to increase firm value that result from their compensation packages and opportunities for advancement in the managerial labor market. Traditional methods for estimating pay-performance sensitivity exclude incentives that derive from opportunities for advancement in the managerial labor market and assume a linear relation between changes in pay and changes in performance. But results in recent literature imply that advancement opportunities may be a significant source of incentives and that the relation between changes in pay and

changes in performance may depend upon the level of performance. I estimate pay-performance sensitivities that incorporate these results. I find that although performance may be positively related to opportunities for advancement, the contribution to a CEO's total pay-performance sensitivity is too small to be economically significant. I also find that pay-performance sensitivities vary depending on the level of performance and may be higher or lower than estimates from linear models suggest. In sum, observed CEO pay packages may not be as suboptimal as some prior studies suggest.

DEDICATION

To the love of my life, I miss you every single day.

ACKNOWLEDGEMENTS

This work would not be possible without the guidance, advice and support of the faculty and staff of Louisiana State University and Texas A&M University, especially Drs. Anwer Ahmed, David Blackwell, L. Paige Fields, Michael Gallmeyer, Harley Ryan and Myron Slovin. I thank the Rutherford and Waig families for opening their homes and hearts to me. I am most grateful to Shane A. Johnson, for a full decade of service as my advisor and mentor. I will be forever in his debt.

NOMENCLATURE

AGE OF FIRM	The first year the firm is publicly traded (from CRSP) subtracted from the current year.
ALTMAN'S Z SCORE	Altman's (1968) proxy for financial distress risk.
BOARD MEETINGS	The number of board meetings held during the fiscal year (Execucomp variable Board meetings).
CEO TENURE	The current year less Execucomp variable BECAMECEO.
FIXED ASSET RATIO	Net property, plant and equipment (Compustat data #8) divided by total assets (Compustat data #6) (averaged over the five preceding years).
LEVERAGE	Is the residual from regressing long-term debt divided by total assets on age, total assets, market-to-book ratio, and fixed asset ratio (averaged over the five preceding years).
MARKET-TO-BOOK	The ratio of (book value of assets minus book value of equity plus market value of equity) to book value of assets; Compustat data items [#6 - #216 + (#199 * #25)] / #6 (averaged over the five preceding years).
MERGER	An indicator variable that takes the value of one in the event of an acquisition (if Compustat data #249 > 0) and zero otherwise.
OPTION RATIO	$ONEPCT_{options}$ / ($ONEPCT_{stock}$ + $ONEPCT_{options}$ + salary + bonus), where $ONCEPCT_{options}$ is the dollar change in the value of CEO options holdings coming from a one percent increase in firm's stock price, as defined in Bergstresser and Philippon (2006), with option values calculated using the Core and Guay (2002) one-year approximation method.
SALES GROWTH	The percent change in sales (Compustat data #12) versus the prior year, measured as of the year preceding the alleged incident for misreporting firms.

STOCK RATIO	$ONEPCT_{stock}$ / ($ONEPCT_{stock}$ + $ONEPCT_{options}$ + salary + bonus), where $ONCEPCT_{stock}$ is the dollar change in the value of CEO stock holdings coming from a one percent increase in firm's stock price, as defined in Bergstresser and Philippon (2006), with option values calculated using the Core and Guay (2002) one-year approximation method.
TOTAL ASSETS	The firm's total assets (Compustat data #6), measured as of the year preceding the alleged incident for misreporting firms.
UNRATED	Indicator variable that takes a value of one if the firm has no existing bond rating, zero otherwise.
VOLATILITY	The standard deviation of returns over the previous sixty months, the variable BS_Volatility in Execucomp. If this variable is missing, I compute it using returns data from CRSP.
3-yr OIBD/TA	The three-year change in operating income before depreciation (Compustat data #13) divided by total assets (Compustat data #6), less the industry median of this ratio

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1. INTRODUCTION

At the heart of finance and economics are questions regarding how people respond to incentives. Questions of this nature are prevalent throughout the literature on agency problems (Jensen and Meckling (1976)), banking (Diamond (1991a)), capital structure (Myers (1977)), and even microstructure (Christie and Schulz (1993)), to name just a few. The purpose of this paper is to consider two sets of incentives that firm managers face, and examine how they respond to those incentives.

The second section considers the incentives to accurately report financial performance that derive from a firm's use of bank debt in its capital structure. Because banks monitor their borrowers, the use of bank debt should increase the probability that financial statement misreporting is detected, which should therefore decrease the probability that managers choose to misreport. I find instead that managers are sometimes more likely to misreport if their firms use bank debt financing, because bank loan agreements require firms to uphold certain covenants that are written on financial statement numbers. These covenants provide incentives to misreport if managers cannot uphold them legitimately. Further, if misreporting can give the appearance of financial health, then it can help the firm to reduce its borrowing costs. I find two key sets of results because the costs of misreporting, once it is detected and exposed, depend upon its severity. My results suggest that the benefits of misreporting can exceed the expected costs but only when those expected costs are small, as they are for relatively less severe types of misreporting. I find the likelihood of misreporting is positively related to the use

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This dissertation follows the style of *Journal of Finance*.

of bank debt, and that *ex ante* changes in bank debt are positive for misreporting firms relative to control firms when misreporting is relatively less severe. For relatively more severe misreporting, such as accounting malfeasance that triggers an SEC investigation or filing of an AAER, I find no relation between the use of bank debt and the likelihood of misreporting. The most important result is that I do not find any negative relations between the use of bank debt and the likelihood of misreporting, inconsistent with the extant literature on the value of banks as delegated monitors.

The third section addresses the questions of whether the market for managerial talent provides incentives for CEOs to improve firm performance, and whether these and other incentives are the same for all CEOs. That is, a manager whose performance is very good may face a different set of incentives than one whose performance is poor. I find that although boards of directors may consider past performance when hiring an outside CEO, the magnitude of this incentive is relatively small when compared to the other incentives that CEOs face. Further, I find that the incentives deriving from changes in own-firm pay and the threat of dismissal for poor performance are non-linear across the performance range. In short, a manager whose performance is good faces a different set of incentives than one whose performance is poor.

In sum, these results contribute to the literature by demonstrating that although managers respond rationally to the incentives that they face, those incentives are many and their effects sometimes ambiguous. Shareholders, and the boards of directors that they have elected to serve their interests, must consider all of the incentives that CEOs face when designing executive compensation packages to align the interests of the CEO with their own.

2. AN EMPIRICAL ANALYSIS OF THE RELATIONS BETWEEN FINANCIAL STATEMENT MISREPORTING AND FIRMS' USE OF BANK DEBT

"You know, in a collapse of this magnitude the first question becomes the same question we asked about Enron, which is where were the accountants, and where were the banks where this money was supposed to be? Didn't someone see something irregular going on?" - Gwen Ifill, Senior Correspondent, The NewsHour, January 7, 2004

2.1 Introduction

The act of financial statement misreporting[1] represents the outcome of an optimization problem faced by a manager who has weighed the expected costs and benefits of misreporting, and has concluded that the benefits are greater. Ceteris paribus, the use of debt in a firm's capital structure should increase both the expected costs and benefits of misreporting, due to monitoring by the lender and restrictive covenants in the loan contract, respectively. Because bank debt is subject to greater monitoring and tighter covenants than other types of debt, I argue that the manager of a firm financed with bank debt should face even greater expected costs and benefits of misreporting than a manager of an otherwise identical firm. The purpose of this paper is to test empirically whether the likelihood of misreporting differs at firms that borrow from banks.

To the extent that outside claimants assess a firm's value based on its financial statements, misreporting can benefit the firm's managers (and shareholders) by altering this assessment in managers' favor (Dye (1988); Trueman and Titman (1988)). Thus the benefits of misreporting, if it escapes detection, can include a higher stock price if outsiders overestimate the firm's true value. If the manager owns stock or options in the firm, this can increase the manager's personal wealth. Misreporting can also help to

[1] I use the terms "misreporting" and "earnings management" interchangeably, and define them as in Healy and Wahlen (1999), "when managers use judgment in financial reporting and in structuring transactions to alter financial reports to either mislead some stakeholders about the underlying economic performance of the company or to influence contractual outcomes that depend on reported accounting numbers."

lower outsiders' perceptions of the firm's degree of risk, and thereby improve contractual terms with employees, suppliers, lenders and others, resulting in lower operating costs. These benefits should be greater at a firm financed with bank debt because most restrictive covenants in bank loan agreements are written on information gleaned from the borrowers' financial statements. So managers who find that they are close to a covenant violation may be able to avoid a technical default on an outstanding loan by misreporting. This is referred to in the accounting literature as the "debt covenant hypothesis" (Watts and Zimmerman (1986)). Empirical evidence in support of this hypothesis includes the work of Defond and Jiambalvo (1994), Sweeney (1994) and Dichev and Skinner (2002). In addition, managers may be able to obtain bank debt financing on more favorable terms, such as a lower interest rate, if they can give the appearance of higher credit quality through earnings management.

The expected costs of misreporting are the product of the cost in the event of detection, and the probability of detection. Importantly, the cost in the event of detection can vary depending on the severity of the misreporting. If it is relatively less severe, such as income smoothing, the cost may be negative media attention and a decline in stock price (Palmrose, Richardson and Scholz (2004)). This can reduce the manager's personal wealth if he has an equity stake in the firm. If the misreporting is relatively more severe, upon detection a manager could face termination, fines, or perhaps even criminal charges and incarceration in addition to his personal wealth loss.

At firms financed with bank debt, these expected costs should be even higher because monitoring by the lending bank should increase the probability that misreporting is detected. A substantial literature demonstrates theoretically and empirically that banks

are effective monitors who add value to borrowing firms (Diamond (1984); Sharpe (1990); James (1987); Lummer and McConnell (1989); Slovin, Johnson and Glascock (1992)). Banks have clear incentives to monitor their borrowers because of their financial stakes in these firms. It is in the best interests of banks to ensure that their borrowers do not take any actions that would reduce the value of their stakes, such as asset substitution or financial statement misreporting. Banks' incentives to monitor their borrowers' financial statements in particular derive from covenants in the bank loan agreements, because they are written on information obtained from the borrowers' financial statements. A covenant violation helps to protect the bank from losses, and allows the bank to force a renegotiation of the loan, thereby shifting bargaining power from the borrower to the lender (Rajan and Winton (1995)). If a borrower's financial statements are inaccurate, then the bank is deprived of this increased bargaining power and opportunity to protect the value of its assets.

In addition, banks have a unique ability to monitor their borrowers. Recent empirical evidence in Mester, Nakamura and Renault (2007) suggests that the transaction accounts that borrowers hold give banks a comparative advantage over other external monitors. Mester *et al.* (2007) demonstrate that transaction accounts allow banks to observe the actual cash transactions that generate the figures reported in the borrowing firms' financial statements, making it more difficult for these firms to misreport.[2] "The loan contract… requires the borrower to report shipments to customers that constitute new accounts receivable as well as customer payments on accounts receivable… the loan

[2] Virtually all publicly traded firms use accrual accounting methods. Mester *et al.*. (2007) note, however, that bank loan agreements typically require "the borrower to report shipments to customers that constitute new accounts receivable, as well as customer payments on accounts receivable." The same is true for inventory, which suggests that even transactions that occur on an accrual basis should be detectable.

officer can do an item-by-item reconciliation of the accounts receivable… The checking account provides a check… on the veracity of the borrower..." Thus, relative to other external monitors, banks have greater incentives and greater abilities to monitor their borrowers, which should increase the probability of the detection of misreporting and thus the expected costs. This should therefore decrease the likelihood that a manager of a firm financed with bank debt chooses to misreport, compared to a manager of an identical firm that does not borrow from banks.

To investigate how managers respond to these potentially higher expected costs and benefits, I use data on misreporting from two sources. I discriminate between misreporting that is relatively more or less severe, because of the different costs that may be imposed upon detection. I use both the GAO restatement database and a sample of AAERs filed by the SEC to identify misreporting firms. In addition, I differentiate the restatements in the GAO sample based on the severity of the accounting irregularities that are restated. I compare firms that restate their financial statements to a set of control firms that do not restate, and importantly, I find that the results of each of these tests differ depending on the severity of the misreporting.

My results suggest that, for firms financed with bank debt, the benefits of misreporting can exceed the expected costs but only when those expected costs are small, as they are for relatively less severe types of misreporting. I find the likelihood of relatively less severe misreporting increases with firms' use of bank debt. This result is consistent with the debt covenant hypothesis and suggests managers may use aggressive accounting techniques to avoid covenant violations or to lower borrowing costs. I find *ex ante* changes in bank debt are positively related to misreporting. These results are

consistent with the hypothesis that an increased reliance on bank debt increases the benefits of misreporting more than the expected costs. I also find, however, that *ex post* changes in bank borrowing are not related to misreporting, suggesting that managers who have misreported do not attempt to avoid monitoring by subsequently reducing their reliance on bank debt.

When the misreporting is relatively more severe, as it was in the spectacular cases of Enron and WorldCom, I find neither a relation between the likelihood of misreporting and the use of bank debt, nor a relation to changes in bank debt. These results suggest that there must be some other benefit, such as the manager's desire to increase his personal wealth, which provides an incentive to misreport when the expected costs of doing so are large (e.g. Johnson, Ryan and Tian (2007)).

The most important result is that I do not find a negative relation between the use of bank debt and the likelihood of financial statement misreporting, as suggested by the literature on bank monitoring. Together my results suggests that, despite the extant literature on banks as external monitors who increase the value of their borrowers, any monitoring they provide is insufficient to serve as a deterrent to financial statement misreporting or to detect misreporting once it has occurred.

But because I can observe only misreporting that has been detected and restated, these results are open to an alternative interpretation. The positive relations that I find may result because banks have monitored their borrowers and exposed their accounting irregularities. That is, a positive relation could exist because firms with bank debt are more likely to *misreport,* or because they are more likely to *restate*. I attempt to address this imperfection of the data in two ways. First, I compare firms whose misreporting is

detected immediately to firms whose misreporting is initially undetected. I find that these firms do not differ in their use of bank debt. When I compare firms whose misreporting is initially undetected to control firms, I once again find a positive relation between misreporting and bank debt. These results suggest that it is misreporting *per se*, and not its detection and restating, that is related to the use of bank debt. Second, I use accounting accruals as an alternative proxy for misreporting. This proxy allows for some degree of managerial discretion in reporting to occur at all firms, which need not be detected in order to be measured. Consistent with my other results, I once again find a positive relation between the likelihood of misreporting and firms' use of bank debt.

In sum, my results lend support to theoretical models developed by Dye (1988) and Trueman and Titman (1988), who predict that managers will manage earnings in order to alter outside claimants' perceptions regarding firm value. My results are consistent with a broad body of empirical literature that finds evidence suggesting managers use earnings management in order to meet the thresholds set by outside agents such as lenders, analysts and auditors (Degeorge *et al.* (1999); Kasznik (1999); Burgstahler and Eames (2006); Abarbanell and Lehavy (2003)). More specifically, my results accord with those of Defond and Jiambalvo (1994), Sweeney (1994) and Dichev and Skinner (2002) who find evidence consistent with earnings management to avoid debt covenant violations. This paper complements the work of Efendi, Srivastava and Swanson (2006), who examine long-term borrowing and find misreporting is more likely when firms are constrained by debt covenants or need to raise new capital.

My paper differs from these by examining bank debt. Although the hypotheses I test should hold for debt in general, the predicted relations are all stronger for bank debt

in particular due to the tighter covenants and greater monitoring imposed by banks. Because bank debt is not reported in firms' financial statements in a consistent way, I use a measure of short-term debt as a proxy for bank debt. Short-term debt is subject to greater monitoring than long-term debt because it requires periodic re-evaluation of borrowers' creditworthiness (Fama (1985)), because failure to renew a loan could lead to an inefficient liquidation of the firm (Diamond (1991a)) and because it is less likely to be collateralized (Qian and Strahan (2005)). Thus my proxy for bank debt should be an effective measure of the extent to which the firm incurs external monitoring by banks and other lenders, and much of my interpretation of the results is unchanged if this measure is viewed as simply short-term debt. Nonetheless, for robustness I repeat my key test using hand-collected data on bank debt for a sub-sample of firms, and find consistent results.

This paper also differs by distinguishing between relatively more and less severe types of misreporting, which much of the prior literature has not done. In this way, the paper complements the work of Degeorge, Ding, Jeanjean and Stolowy (2005), who examine analyst following and similarly find two different effects on earnings management. Their international evidence suggests that analyst following encourages managers to use subtle, short-term earnings management to meet analysts' forecasts, but discourages more visible earnings management by firms operating in "transparent environments", those with high disclosure requirements.

Taken together the results of this paper have implications for the design of optimal capital structures and governance mechanisms, as they suggest that bank borrowing may increase certain agency costs for borrowing firms. Firms that rely more

heavily on bank financing may require greater internal monitoring mechanisms in light of the incentives to misreport that derive from bank debt.

The paper proceeds as follows. Section II discusses the data and methodology, and in Section III I present univariate results. The results of the relations between bank debt and the likelihood of misreporting are presented in Section IV. Section V discusses an alternative interpretation of those results. The results for changes in bank debt before and after misreporting are in Section VI. In Section VII I present robustness checks using different methods and data. Section VIII concludes.

2.2 Data and methodology

This paper, like the work of Johnson *et al.* (2007), Erickson, Hanlon and Maydew (2006), Beasley (1996) and others, is limited to the examination of only misreporting that has been exposed. In the analysis that follows I use data from two different sources, each of which is a noisy measure of misreporting, and might best be considered as simply a proxy for whether misreporting has actually occurred. Further, the control sample to which they are compared is likely to contain some observations on firms whose misreporting is undetected, but the presence of these firms in the control sample creates a bias against finding any results, and so should bolster confidence in any significant results that are found. Although numerous empirical studies have examined the likelihood of misreporting using one of these two data sources, to my knowledge this is the first study to perform the same analysis using both sources and to compare the results.

The first source of data is the General Accounting Office. Their sample of restatements includes bona fide errors, earnings management that falls outside of GAAP

(Generally Accepted Accounting Principles), or even within GAAP but is considered to be too aggressive. The GAO examined financial statement restatements from 1997 – 2002.[3] Restatements are not uncommon and most are relatively benign. Firms routinely restate financials in response to changes in GAAP, to mergers and acquisitions, or to stock splits. Such restatements are not included in the GAO sample unless they represent some irregularity. Rather, the sample is meant to represent cases of "'aggressive' accounting practices, intentional and unintentional misuse of facts applied to financial statements, oversight or misinterpretation of accounting rules, and fraud".[4] To generate this sample the GAO searched Lexis-Nexis for press releases including some form of the word "restate", "adjust", "amend" or "revise" within fifty words of "financial statement" or "earnings". As a result, they created a database of firms that announced 919 restatements. Since this database includes the date the restatement is announced, but not of the misreporting itself, I search Lexis-Nexis for press releases to identify the periods that are being restated. I also use these press releases to identify restatements resulting from accounting malfeasance. I follow Efendi *et al.* (2006) and classify a restatement as resulting from malfeasance if the GAO database records the SEC or auditor as the prompter of the restatement, or if the press release announcing the restatement includes an allegation of fraud or SEC investigation.

The second source I use to identify firms accused of misreporting is the Securities and Exchange Commission. The SEC investigates alleged violations of federal securities

[3] This sample was augmented to include restatements beginning in 1994. I thank Sudheer Chava and Shane Johnson for providing this additional data.
[4] U.S. General Accounting Office, 2002, Financial statement restatements: Trends, market impacts, regulatory responses, and remaining challenges, Report to Chairman, Committee on Banking, Housing, and Urban Affairs, page 76.

laws, and when it finds sufficient evidence to bring civil charges against a firm for accounting irregularities, it files an Accounting and Auditing Enforcement Release (AAER). Whereas restatements are somewhat common, Palmrose and Scholz (2004) find that only 13% of restatements result in an AAER. The SEC has limited resources, and it selects cases to investigate and prosecute based upon the likelihood of success (Feroz, Park and Pastena (1991)), thus there is clearly a selection bias inherent in this sample. By and large, however, firms that are the subjects of AAERs have committed relatively more egregious types of misreporting (Farber (2005)), and this sample includes the spectacular cases of Enron and WorldCom.

I use the sample of AAERs identified by Johnson *et al.* (2007). To generate their sample, these authors begin with the Execucomp database and search AAERs for these firms. They require that the misreporting occur between 1992 and 2001. They omit charges of embezzlement by non-executive employees, bribery by such employees of foreign officials or foreign customers, and other cases that do not lend themselves to the study of relations between executive compensation and misreporting. The sample they generate includes 102 firm-year observations on 43 unique misreporting events.

I then collect accounting and governance data for all firms in the Execucomp database for 1992-2004, excluding banks (SIC codes 6000-6199). I omit a firm-year observation if it seems to be erroneous in some way, such as a ratio of bank debt to total debt that is greater than one. All dollar values are adjusted for inflation to 1983 dollars, and most variables are winsorized at the 1% and 99% level to mitigate the influence of outliers. The final sample includes 11,076 firm-year observations on 2,090 firms. Of

these firms, 159 are included in the GAO database and 29 are the subjects of AAERs.[5] The remaining firms represent my control sample. When I examine changes in bank debt, I expand the period by four years, resulting in a total sample of 13,828 firm-year observations on 2,164 firms.

Because bank debt is not reported in a uniform way, I use a proxy for bank debt defined by Slovin, Sushka and Hudson (1990). This proxy is calculated as debt in current liabilities (Compustat data # 34) less the current portion of long-term debt (Compustat data # 44). The first term includes most commercial bank debt, but it may also include other forms of short-term debt due to non-bank entities, and should be an effective proxy for the degree to which banks and other creditors monitor the firm. Short-term debt is subject to greater monitoring by the lender for a number of reasons. First, the process of renewing a loan requires the lender to periodically re-evaluate the borrower's credit worthiness and choose whether or not to provide capital (Fama (1985)). Second, if the lender decides not to renew the loan, the result could be bankruptcy or liquidation of the firm (Diamond (1991a)). Because lenders prefer to lend to creditworthy borrowers and to avoid inefficient liquidation, those who provide short-term financing have greater incentives to monitor than those who provide long-term debt. Finally, Manove, Padilla and Pagano (2001) argue that collateral and monitoring are substitutes. Berger and Udell (1995) and Voordeckers and Steijvers (2006) find that firms with longer relationships with their lending banks are less likely to pledge collateral. To the extent that reputation and monitoring are substitutable (Diamond (1991b)), this result is consistent with the

[5] The GAO specified no restrictions regarding which firms would be included in their database. The sample size declines primarily because I require firms to be in Execucomp. For the AAER sample of Johnson *et al.* (2007), the sample size declines primarily because I exclude misreporting events in 1992-1993, and because I omit banks and insurance companies.

Manove *et al.* (2001) model. Because short-term debt is less likely to be collateralized than long-term debt (Qian and Strahan (2005)), it should be subject to a higher degree of monitoring since the lender has no recourse should the borrower become unable to pay. Because of this increased monitoring, much of my interpretation of the results is unchanged if this measure is viewed as simply short-term debt.

Slovin *et al.* (1990) then scale this measure by the market value of the firm's equity. Since in this study I investigate accounting manipulations, such scaling is problematic. If the purpose or consequence of the misreporting is to inflate the firm's stock price, such an increase will cause a decrease in the ratio of bank debt to market value of equity. This would give the appearance of reducing the firm's use of bank debt, which would obscure the hypothesized relations between bank debt and misreporting. I therefore scale instead by the book value of the firm's assets. Although book value of assets can be manipulated through misreporting, the effect should be smaller than it would be for market value of equity. Regardless of the scaling, the resulting ratio provides the same underlying intuition in that a higher value should indicate greater monitoring on the part of banks and other short-term creditors.

To estimate whether the likelihood of misreporting depends on firms' use of bank debt I estimate logit regressions and control for a number of variables that have been found to influence the likelihood of misreporting. Richardson, Tuna and Wu (2002) find restating firms have higher levels of debt than those that do not restate, therefore Erickson *et al.* (2006) include leverage as a control variable in their analysis. Their measure of leverage, total debt (Compustat data # 34 + #9) scaled by total assets, includes the proxy for bank debt described above that is the key variable of interest in this paper.

So to control for the total leverage effect I partition total debt into two parts, the bank debt variable already described plus other debt (long-term debt including the current portion, Compustat data # 44 + #9), which is also scaled by total assets. In addition, I use the following control variables, most of which are described by Erickson *et al.* (2006).

I include two variables to control for internal monitoring by the board of directors, because this monitoring should decrease the likelihood of misreporting (Beasley (1996)). The first is the number of meetings of the board, as a proxy for direct internal monitoring (Adams (2003) and Vafeas (1999)). Second, Hermalin and Weisbach (2001) argue that a CEO's power increases over time, and that the board's effectiveness in monitoring is most influenced by its independence from the CEO. This suggests board monitoring is decreasing in the tenure of the CEO, so I include this as a control variable.

Numerous studies find that the motive for misreporting is to conceal financial distress or declining performance (Johnson *et al.* (2007)), so I include three performance metrics. I use Altman's Z to control for financial distress (Altman (1968)), and I control for financial performance using sales growth (measured in the year prior the alleged incident for misreporting firms) and the three-year change in the ratio of operating income before depreciation to total assets (industry-adjusted).

Equity markets have requirements with which firms must comply, and the longer a firm has been publicly traded the more likely it meets these requirements (Beasley (1996)). Alternatively, firms may manage earnings prior to an initial public offering to increase their offer price, and reverse the effects with a restatement following the IPO (Teoh, Wong and Rao (1998)). Diamond (1991b) argues that firms develop reputations by repaying bank loans over time, suggesting that firm age also proxies for reputation.

Young firms, with less reputation, have less to lose so the incentives to report accurately provided by bank monitoring are smaller. These papers all suggest a negative relation between the likelihood of misreporting and firm age, so I include age as a control variable.

Erickson and Wang (1999) suggest that firms manipulate earnings in order to raise stock prices prior to acquisitions, so I include an indicator variable to control for an acquisition having occurred. Larger firms are subject to more attention from outsiders such as analysts and the media. This attention may impact the market reaction to restatement (Palmrose, Richardson and Scholz (2004)), and therefore the likelihood of misreporting. To control for this, I use total assets to proxy for firm size.

Unpredictable environments make monitoring more difficult and expensive, yet more necessary (Demsetz and Lehn (1985)), as such environments can facilitate misreporting. To control for this lack of predictability, I include the volatility of stock returns for the prior sixty months as a proxy.

Bergstresser and Philippon (2006) find that accounting manipulations in the form of accruals are more common at firms whose CEOs earn more compensation in the form of stock and options, which they measure with an incentive ratio. Because Johnson *et al.* (2007) find that the relation between earnings management and compensation differs for stock versus option compensation, I partition the incentive ratio described in Bergstresser and Philippon (2006) into an option ratio and a stock ratio. I use these two ratios to control for the possibility that CEOs are more likely to manage earnings when they have personal financial incentives to increase stock prices.

Diamond (1991b) suggests that reputation can substitute for monitoring by external agents such as banks, both of which should reduce the likelihood of misreporting. Firms with greater reputation have access to public debt markets, so as an additional proxy for reputation I use an indicator variable defined by Denis and Mihov (2003) that identifies firms that have bond ratings.

To estimate whether changes in bank debt are related to misreporting I estimate tobit regressions and use five control variables that influence firms' bank borrowing, based on Johnson (1997), that are described below. I also use the proxy for credit quality from Denis and Mihov (2003), because they find that credit quality influences the choice of lender.

Johnson (1997) uses firm age as a proxy for reputation, which influences firms' access to various debt markets. He uses market-to-book value of assets to proxy for investment opportunities, project quality and project liquidation values. Book value of assets proxies for firm size because lender informedness increases, and monitoring costs decrease, with firm size (Fama (1985)). All of these variables are negatively related to firms' use of bank debt. The fixed asset ratio is a proxy for asset collateral value and asset substitution problems and is positively related to firms' use of bank debt. Because there is a relation between total leverage and debt ownership structure, Johnson (1997) creates an instrumental variable for leverage to avoid endogeneity problems, and finds a positive relation between this variable and bank borrowing. Like Johnson (1997), I average each of these variables over the five preceding years to mitigate the influence of extreme values.

If a firm-year observation is missing one of these control variables, I set the variable equal to zero and create a dummy variable that equals one if the variable is missing and zero otherwise. This allows me to retain these observations for the purpose of estimating the effects of the other variables. These dummy variables rarely generate significant coefficients, and so are excluded from the tables in the interest of brevity. Their significance is discussed in the text.

2.3 Univariate results

Summary statistics and the results of difference in means tests of firms in the GAO restatement database versus control firms are reported in Table 1. Restating firms differ from control firms for most of the variables I use. There is more bank debt (p-value <0.01) and more non-bank debt (p-value <0.01) outstanding at misreporting firms, and these firms increase their reliance on bank debt (p-value <0.01) relative to control firms in the years preceding the misreporting. In addition, firms in the GAO sample hold more meetings of the board (p-value <0.01). If boards and lenders serve to monitor activities within the firm, then these results are inconsistent with Beasley (1996) and Dechow, Sloan and Sweeney (1996) who suggest that monitoring and governance decrease the likelihood of misreporting. Alternatively, the latter finding may result from audit committee meetings to investigate or correct accounting irregularities. Misreporting firms have, on average, lower market-to-book values of assets (p-value <0.01) averaged over the five years preceding the misreporting, but smaller changes in operating income before depreciation to total assets (p-value <0.01) at the time the misreporting occurs. These results suggest that managers use accounting manipulations to inflate equity prices, and are consistent with the results of Johnson *et al.* (2007), who

find that were it not for accounting fraud, the equity of the firms in their sample would have underperformed.

The average misreporting firm has higher sales growth (p-value <0.01), consistent with the use of aggressive accounting to give the appearance of consistent growth in sales. Firms that misreport are on average older (p-value <0.01) and larger (p-value <0.01) than control firms. If age and size proxy for reputation, then these results are inconsistent with Diamond (1991b) who suggests that reputation reduces the need for external monitoring. Consistent with Diamond (1991b) however is that misreporting firms are less likely to have a bond rating (p-value = 0.03). The CEOs of misreporting firms earn a larger fraction of their total compensation in the form of options (p-value <0.01) than do CEOs of control firms, consistent with Bergstresser and Philippon (2006). Finally, misreporting firms also have more volatile stock returns (p-value = 0.02), and lower fixed asset ratios (p-value <0.01) than control firms on average, both of which are consistent with misreporting being more prevalent when monitoring is difficult either due to an unpredictable environment (Demsetz and Lehn (1985)), or because market value is derived from investment opportunities rather than assets in place (Smith and Watts (1992)).

2.4 Relations between bank debt and the likelihood of misreporting

To estimate whether the likelihood of misreporting is related to firms' use of bank debt, I use a logit regression analysis. The dependent variable takes a value of one if the firm misreports, and zero otherwise. I report estimated coefficients as well as marginal effects, for which I set all explanatory variables to their sample means except dichotomous variables that are set to zero.

Table 2 gives the results of estimating the logit model comparing control firms to those in the GAO database. The coefficient on bank debt is positive (p-value <0.01). For a standard deviation increase in bank debt, the predicted probability of misreporting increases by 0.84%, holding all other variables constant. The coefficient on other debt is also positive (p-value <0.01). Holding all other variables constant, a standard deviation increase in other debt increases the predicted probability of misreporting by 0.70%. Each of these represents a substantial increase over the unconditional probability of misreporting of 2.09%.[6] These results are inconsistent with the bank monitoring literature.

The positive coefficient on number of board meetings (p-value = 0.01) may suggest that boards or audit committees hold meetings in response to misreporting. The positive coefficient on the 3-year change in OIBD to total assets (p-value = 0.02) is inconsistent with the use of misreporting to conceal declining performance. The positive coefficient on sales growth (p-value <0.01) is consistent with firms misreporting in order to give the appearance of revenue growth. That age (p-value <0.01) generates a positive coefficient is inconsistent with Diamond (1991b), who suggests reputation can substitute for monitoring. The negative coefficient on the merger indicator variable (p-value <0.01) is inconsistent with the results of Erickson and Wang (1999), who find firms manipulate earnings in order to raise stock prices prior to acquisitions. Consistent with misreporting being more common in unpredictable environments (Demsetz and Lehn (1985)), I find a positive coefficient on the volatility of stock returns (p-value <0.01). Option ratio also

[6] This calculation is based on firm-year observations. Based on the number of firms in the sample, the unconditional probability of misreporting is 7.61%.

generates a positive coefficient (p-value < 0.01), consistent with Bergstresser and Philippon (2006). Stock ratio also generates a positive coefficient (p-value =0.08), consistent with Johnson *et al.* (2007). The dummy variable that indicates volatility is missing generates a positive coefficient (p-value =0.06).

Next I follow Efendi *et al.* (2006) and identify whether each restatement results from malfeasance. When misreporting is detected and exposed, the costs to the manager depend on the severity of the accounting manipulations. In the case of more severe accounting irregularities, the consequences can include fines or even criminal charges in the most extreme circumstances. Although some managers might be willing to risk the negative press associated with a restatement in order to avoid covenant violations or reduce borrowing costs, fewer may be willing to risk prison terms. That is, when the expected costs of detection are high, avoidance of loan default may not offer a large enough benefit to induce misreporting. Managers who choose to engage in more egregious accounting manipulations may do so for other reasons, such as to increase their personal wealth (Johnson *et al.* (2007)). This suggests the importance of distinguishing between relatively less and more severe types of misreporting.

I test this distinction with a multinomial logit model, the results of which I report in Table 3. The dependent variable takes a value of two if malfeasance is indicated, a value of one for other restatements, and a value of zero for control observations. For other (non-malfeasance) restatements, I again find a positive relation between bank debt and misreporting, as well as other debt and misreporting (p-values <0.01). These results are reported in Panel A. Among the control variables, number of meetings (p-value =

0.02), sales growth (p-value <0.01), merger indicator (p-value = 0.03) and option ratio (p-value = 0.02) are again significant, and their signs are consistent with those in Table 2.

As reported in Panel B, bank debt generates an insignificant coefficient when malfeasance is indicated (p-value = 0.64), but the coefficient on other debt is still positive (p-value <0.01). Among the control variables, number of meetings (p-value = 0.09), 3-year change in OIBD/TA (p-value = 0.02), sales growth (p-value <0.01), age (p-value <0.01), merger indicator (p-value = 0.10), volatility (p-value <0.01) and stock ratio (p-value = 0.06) are again significant, and their signs are consistent with those in Table 2.

These results are inconsistent with the bank monitoring literature, and suggest that bank monitoring is insufficient to deter misreporting. Instead, bank borrowing may actually provide incentives for managers to misreport. Importantly, however, the benefits that derive from misreporting (avoiding loan default, lowering borrowing costs) are large enough that they outweigh the costs only when the costs are small, as they are for relatively less severe accounting manipulations.

I next investigate the relation between misreporting and bank debt using the sample of AAERs. Because AAERs frequently represent more egregious accounting manipulations[7], the relation between misreporting and bank debt should be stronger for this sample than for the GAO sample. The results of this analysis are reported in Table 4. Using this proxy for misreporting, bank debt has lost its explanatory power (p-value = 0.57). The coefficient on other debt is positive (p-value = 0.04). The coefficient on the merger indicator variable is positive (p-value = 0.05), consistent with Erickson and Wang (1999). All of the other control variables generate insignificant coefficients. These

[7] Both Enron and WorldCom, for example, were the subjects of AAERs.

results suggest that bank monitoring is insufficient to prevent the most egregious types of misreporting. Importantly, these results also suggest that the benefits of avoiding default and reducing borrowing costs are insufficient to motivate the most serious types of accounting manipulations. There must be some other benefit to misreporting that provides an incentive when the expected costs of misreporting are large.

Overall the results thus far suggest that not only is bank monitoring insufficient to deter financial statement misreporting by bank borrowers, but that bank debt may even provide incentives for borrowers to misreport. These incentives arise if managers cannot uphold the covenants in their bank loan agreements legitimately, or if they need to reduce the cost of borrowed funds. Finally, the results suggest that the incentives are great enough to induce only relatively less severe types of misreporting.

2.5 Alternative interpretation

Because I use restating as a proxy for misreporting, an alternative interpretation of these results is also possible. That is, data are available only for those firms whose misreporting is detected and exposed. So a positive relation between bank debt and restatements could result because bank monitoring effectively detects misreporting. To unequivocally distinguish between these alternative interpretations requires a sample of firms whose misreporting is undetected, but such a sample does not exist. There are however firms whose misreporting is *initially* undetected. I use data from the GAO restatement database and Lexis-Nexis newswire searches to identify these firms. Many firms restate in the same year (for misreporting of quarterly financial statements) or the following year, and I use these firms as a sample of detected misreporting. Those firms that restate more than one year after misreporting I use as a sample of initially undetected

misreporting. I compare this sample of initially undetected misreporting to both the sample of detected misreporting, and to the control group (those firms that do not restate), to distinguish between the following:

Alternate Hypothesis 1A: The likelihood of misreporting is increasing in bank debt, because borrowing firms' managers have incentives to misreport to uphold covenants or to lower borrowing costs.

Alternate Hypothesis 1B: The likelihood of restating is increasing in bank debt, because effective bank monitoring helps to detect and expose misreporting.

If the likelihood of *misreporting* is positively related to bank debt, consistent with the debt covenant hypothesis, then I should observe no differences in the use of bank debt between the sample of initially undetected misreporting versus the sample of detected misreporting[8]. I should observe a greater use of bank debt among the sample of initially undetected misreporting compared to the control group.

If the likelihood of *restating* is positively related to bank debt, consistent with the bank monitoring literature, then I should observe a greater use of bank debt in the sample of detected misreporting compared to the sample of initially undetected misreporting. I should observe no difference in the use of bank debt between the sample of initially undetected misreporting compared to the control group.

I test these relations using a logit regression analysis with the same control variables discussed in the last section. In Table 5 Panel A I compare firms whose misreporting is detected versus firms whose misreporting is initially undetected. The dependent variable takes a value of one if misreporting is detected immediately and a

[8] A third scenario is that managers of firms financed with bank debt, knowing they will incur the scrutiny of bank monitoring, must engage in relatively more complex accounting manipulations in order to avoid detection by the bank. If this were the case, I should observe a greater use of bank debt among the group of firms whose misreporting is initially undetected both versus those whose misreporting is detected, and versus the control group.

value of zero otherwise. I find no significant difference in the use of bank debt (p-value = 0.10)[9] or other debt (p-value = 0.84) among these firms. The number of board meetings is positively related to the likelihood of detection (p-value = 0.03), consistent with effective monitoring by the board to detect and expose misreporting, or with meetings of the board in response to misreporting. None of the other control variables generates a significant coefficient.

I next compare the sample of initially undetected misreporting to the control group and report the results in Table 5 Panel B. The dependent variable takes a value of one if misreporting is initially undetected and a value of zero for firms in the control group (those that do not restate). I find a significant difference in the use of bank debt among these firms. Firms whose misreporting is initially undetected use more bank debt and more other debt (p-values <0.01) than control firms. Sales growth (p-value <0.01), firm age (p-value = 0.02), merger indicator (p-value = 0.02), volatility (p-value = 0.09) and stock ratio (p-value = 0.01), generate significant coefficients, the signs of which are consistent with those reported in Table 2. The 3-year change in OIBD/TA generates a negative coefficient (p-value = 0.05). The dummy variable that indicates volatility is missing generates a positive coefficient (p-value < 0.01).

Overall these results confirm the interpretation of the other results presented thus far. Firms that make greater use of bank debt are more likely to misreport, whether that misreporting is detected or not, than firms with less bank debt. And firms whose misreporting is detected do not have more bank debt outstanding than firms whose misreporting is initially undetected, as would be expected if misreporting was being

[9] This p-value = 0.102, thus I interpret the coefficient as being insignificant.

detected through bank monitoring. Together these results suggest that it is misreporting *per se*, and not restating, that is positively related to bank debt, which is inconsistent with the bank monitoring literature.

2.6 Relations between misreporting and changes in bank debt

If bank monitoring is an effective deterrent to accounting manipulation, then managers who have increased their reliance on bank debt and are suddenly subjected to more monitoring by banks have increased their incentives not to misreport. In addition, the Bar-Gill and Bebchuk (2002) model predicts that firms make investments *ex ante* to facilitate fraud, and one such investment may be to reduce the amount of bank monitoring that the firm must incur. This suggests a negative relation between *ex ante* changes in bank debt and misreporting.

On the other hand, if covenants in bank loan agreements provide incentives for managers to misreport, then the managers of firms that have recently increased their reliance on bank debt have also increased their incentives to misreport. This suggests a positive relation between *ex ante* changes in bank debt and misreporting.

To test these predictions, I use a tobit model and report coefficient estimates in Table 6. I compare annual changes in bank debt for misreporting firms two years before misreporting[10] versus annual changes in bank debt for firms in the control group. The results of the analysis using the full GAO sample are given in Panel A. The coefficient on the misreporting dummy variable is positive (p-value <0.01), indicating that *ex ante* changes in bank debt are positive for misreporting firms when compared to control firms. This result is inconsistent with the hypothesis that bank monitoring is an effective

[10] This includes the first year that is misreported.

deterrent to misreporting. Age generates a positive coefficient (p-value <0.01), and changes in bank debt are negatively related to the lack of a bond rating (p-value = 0.07).

I next distinguish between restatements that result from malfeasance versus those that do not. These results are reported in Panels B and C of Table 6. When malfeasance is indicated, I find no relations between *ex ante* changes in bank debt and misreporting (p-value = 0.82). When the misreporting is less severe, however, I find a positive relation between misreporting and *ex ante* changes in bank debt (p-value <0.01). Age generates positive coefficients, (p-values < 0.01), and the absence of a bond rating generates negative coefficients (p-value = 0.05 and 0.08 respectively) in both sub-samples.

Taken together, these results are inconsistent with the bank monitoring literature. The most important result is that I do not find a negative relation between *ex ante* changes in bank debt and misreporting for any sub-sample. That misreporting occurs following increases in the use of bank debt suggests that misreporting may be a last resort for managers with new bank debt, or too much bank debt, facing increased pressure to uphold the covenants in their bank loan agreements, consistent with the debt covenant hypothesis.

My final set of tests examines changes in bank debt following misreporting. Once managers have filed an inaccurate financial statement, if they fear bank monitoring may detect and expose any irregularities, then they may choose to reduce their reliance on bank debt. Or once the irregularities have been exposed, banks may be unwilling to lend additional funds or may demand payment of outstanding balances. This suggests a negative relation between *ex post* changes in bank debt and misreporting. Alternatively, the motive for misreporting may be to conceal the borrower's true financial health in

order to obtain financing on more favorable terms. This suggests a positive relation between *ex post* changes in bank debt and misreporting. I use a tobit model to test these predictions on the same sub-samples of data and report the results in Table 7.

The misreporting indicator variable is not significant in any of these tests. These results suggest that once managers have made the decision to misreport, they do not take steps to avoid being monitored by banks. This could be because managers do not view detection as costly, or do not view banks as being adept at detecting misreporting. Firm age generates positive coefficients in all sub-samples (p-values < 0.01). In the sub-sample of firms accused of malfeasance, the dummy variable indicating the firm does not have a bond rating generates a negative coefficient (p-value = 0.06).

2.7 Robustness tests

2.7.1 Alternative measure of bank debt

A potential criticism of the analysis thus far is that it examines a proxy for bank debt rather than actual bank debt. To address this concern I hand collect data on bank loans from Moody's Industrial Manuals and 10-Ks for 104 firms in the GAO restatement database [11], as well as a sample of size- and industry-matched firms that do not restate. I match first on size, and require that the matched firm be within 30% of the market value of equity as the sample firm. For industry, I look first for matches within the same three-digit SIC code, and if a match cannot be found then I look within the same two-digit SIC code. I use the best match for ninety-seven of my restating firms, and the second-best match for the remaining seven firms due to missing data.

[11] I use the first year in which the firm misreports and search for data on every firm included in my sample. The sample size decreases because the sample firm's 10-K cannot be found, an appropriate matched control firm cannot be found, or the control firm's 10-K cannot be found.

Using this matched pair sample, I repeat the logit analysis from Table 2, and report the results in Table 8. The coefficient on bank debt is positive (p-value <0.01), consistent with and confirming the analysis presented thus far. The coefficient on other debt, however, is insignificant (p-value = 0.14), highlighting the importance of distinguishing between funds borrowed from banks versus other lenders. Consistent with the results presented in Table 2, the 3-year change in OIBD/TA generates a positive coefficient (p-value = 0.08) The positive coefficient on the merger indicator variable (p-value = 0.04) is consistent with Erickson and Wang (1999), but is of the opposite sign as the results in Table 2. CEO tenure generates a negative coefficient (p-value = 0.01), suggesting that relatively new CEOs may misreport because they are under pressure to perform. The positive coefficient on the dummy variable indicating the lack of a bond rating (p-value = 0.02) is consistent with Diamond (1991b) who argues that reputation can substitute for monitoring. Neither of these variables generates significant coefficients when the proxy for bank debt is used as the dependent variable as in Table 2. The coefficients on the remaining control variables are not significant.

2.7.2 Alternative measure of misreporting

I repeat the analysis from Section IV using accruals as an alternative measure of misreporting. Because the independent variable of interest, the proxy for bank debt, is calculated from balance sheet items, I follow Hribar and Collins (2002) and estimate accruals using items from the statement of cash flows to avoid mechanical relations. This estimate of accruals is calculated as earnings before extraordinary items and discontinued operations, less operating cash flows from continued operations. Unlike the misreporting indicator variables use in the rest of the paper, this variable does not require misreporting

to have been detected, and instead allows for some degree of misreporting (or at least discretion) to take place at every firm in the sample. I estimate OLS regressions with accruals as the dependent variable. Table 9 reports the results. Consistent with the logit results in Table 2, I find a positive relation between accruals and bank debt (*p*-value <0.01). I find no relation, however, between accruals and other (long-term) debt (*p*-value = 0.19). Like those of my tests using hand collected bank debt, these results also suggest the importance of distinguishing between loans from banks and other types of debt. The merger indicator variable (*p*-value = 0.02), 3-year change in OIBD/TA (*p*-value <0.01), age (*p*-value = 0.05) and number of meetings (*p*-value <0.01) generate significant coefficients, whose signs are consistent with those in Table 2. Four other control variables generate significant coefficients whose signs are not consistent with those in Table 2. Sales growth (*p*-value = 0.06) generates a negative coefficient, as does volatility (*p*-value <0.01). These results are not consistent with the extant literature. The coefficients on option ratio and stock ratio are also negative (*p*-values = 0.02 and <0.01, respectively), inconsistent with Bergrestresser and Philippon (2006). In addition, three control variables are significant in this model but are inconsistent with the results in Table 2. The tenure of the CEO and total assets both generate positive coefficients (*p*-values < 0.01 and = 0.02, respectively). These results are inconsistent with arguments such as those in Diamond (1991b) regarding the value of reputation. The coefficient on Altman's Z is also positive (*p*-value = 0.06), consistent with Johnson *et al.* (2007) who find evidence consistent with the use of misreporting to conceal financial distress. Finally, the dummy variable that indicates tenure is missing generates a positive coefficient (*p*-value <0.01).

2.8 Conclusion

Managers weigh expected costs and benefits when choosing to misreport their firms' financial statements, and I argue that both should be higher at firms that borrow from banks. Misreporting, if it escapes detection, can help managers to avoid loan covenant violations or to reduce borrowing costs (Dye (1988); Trueman and Titman (1988); Defond and Jiambalvo (1994); Sweeney (1994); Dichev and Skinner (2002)). But monitoring by the lender should increase the probability of detection and thereby the expected cost (Diamond (1984); Sharpe (1990); Mester *et al.* (2007)). Because bank debt is subject to greater monitoring and tighter covenants than other types of debt, managers of firms financed with bank debt should face even greater expected costs and benefits of misreporting than managers of otherwise identical firms. The purpose of this paper is to test empirically whether financial statement misreporting is more likely, due to the greater benefits, or less likely, due to the greater expected costs, that should accompany bank borrowing.

I find two key sets of results because the costs of misreporting, once it is detected and exposed, depend upon its severity. My results suggest that the benefits can exceed the expected costs of misreporting but only when those expected costs are small, as they are for relatively less severe types of misreporting. I find the likelihood of misreporting is positively related to the use of bank debt, and that *ex ante* changes in bank debt are positive for misreporting firms relative to control firms. These results are inconsistent with the bank monitoring literature, and instead suggest that bank debt can provide incentives for managers to misreport, either to avoid covenant violations or to lower borrowing costs. These results are consistent with a body of literature that includes the

work of Sweeney (1994), Defond and Jiambalvo (1994), Dichev and Skinner (2002) and others.

For relatively more severe misreporting, such as accounting malfeasance that triggers an SEC investigation or filing of an AAER, I find no relation between the use of bank debt and the likelihood of misreporting. I find that changes in bank debt are unrelated to more severe types of misreporting. These results also fail to support the bank monitoring literature, but suggest that there must be some other benefit, such as the manager's desire to increase his personal wealth (Johnson *et al.* (2007)), that provides an incentive to misreport when the expected costs of doing so are large.

But because I can observe only misreporting that has been detected and restated, these results are open to an alternative interpretation. If banks are effective monitors who detect and expose misreporting by their borrowers, then the positive relations that I find may result because firms with bank debt are more likely to *restate,* rather than because they are more likely to *misreport*. To make this distinction, I compare firms whose misreporting is initially undetected versus firms whose misreporting is detected immediately, and versus control firms. The results of these tests confirm my initial interpretation and suggest that it is misreporting *per se*, and not its detection and restating, that is related to bank borrowing.

My results cannot distinguish between a lack of incentives versus a lack of ability on the part of banks to detect misreporting. A lack of incentives may result because commercial loan officers, who are typically compensated based on loan volume, suffer personal financial losses when they deny loans. This cost can be particularly high in the case of a borrowing firm with an ongoing relationship with the bank. A loan officer, who has come to depend on the income resulting from this relationship, has a personal incentive to protect it even if doing so is not in the best interest of the bank's or the borrowing firm's shareholders. I therefore find it more likely that this lack of incentives prevents banks from detecting misreporting rather than a lack of ability, especially since banks can observe their borrowers cash flows (Mester *et al.* (2007)). For whatever reason, my results suggests that, despite the large body of literature on the value of banks as external monitors, any monitoring they provide is insufficient to deter or detect financial statement misreporting.

3. CEO PAY: PERFORMANCE INCENTIVES OR TOURNAMENT PRIZE?

3.1 Introduction

I examine how opportunities for advancement in the managerial labor market and asymmetric pay for performance affect measures of CEO pay-performance sensitivity. Pay-performance sensitivity measures typically comprise two parts. The first part is the change in own-firm pay, resulting from both direct payments that the CEO receives each year that he retains his position and changes in the value of his stock and option holdings. The second component is the threat of termination if performance is poor. Recent research by Fee and Hadlock (2003) suggests that there may be a third source of incentives that is not captured by this traditional methodology. They find a positive relation between a firm's stock price performance and its CEO's outside opportunities in the managerial labor market. This result is consistent with the view that even absent explicit pay-for-performance in their compensation contracts, managers have incentives to perform because high performance can be rewarded via outside promotions (see e.g., Fama (1980); Holmstrom (1999); Chevalier and Ellison (1999); Graham (1999); Hong, Kubik and Solomon (2000)). I quantify these incentives in the form of a pay-performance sensitivity in order to compare their magnitude to the two other sources of incentives that CEOs face.

With respect to changes in own-firm pay, the existing literature on pay-performance sensitivities typically assumes that the compensation contract is a linear sharing rule, in which the CEO keeps some fraction of the wealth he generates for shareholders. Consistent with this assumption, pay-performance sensitivities are typically estimated with a linear regression model. Garvey and Milbourn (2006), however, find that pay is

asymmetrically benchmarked. Their results show that compensation is benchmarked to insulate managers from exogenous forces when those forces are unfavorable (i.e., when luck is bad), but there is less benchmarking when luck is good. This implies a negative relation between changes in pay and changes in performance when performance is negative, but a positive relation when performance is positive. This suggests that pay-performance sensitivities may vary in ways that are not captured with the traditional methodology that imposes a linear restriction on this relation. Thus, I estimate the pay-performance sensitivity deriving from changes in own-firm compensation using a nonlinear model.

I then join these effects, and consider whether the incentives that derive from the threat of dismissal or from opportunities in the managerial labor market might exhibit this asymmetry as well. Consider two managers, one whose performance is very high and another whose performance is very low. If each manager affects an identical increase (decrease) in performance, the change in the probability of being promoted (terminated) is likely higher for the manager whose performance is initially very high (low). Jensen and Murphy (1990) and Fee and Hadlock (2003) estimate these relations using logit models that would not capture this asymmetry. In this paper I use nonlinear probability models that allow for changes in probabilities to be asymmetric for a given change in the independent variable at different points along the distribution to estimate whether the likelihood of being fired or promoted is related to stock price performance in an asymmetric way.

Based on these analyses, my study makes two main contributions. First, quantifying the magnitude of the incentives provided by opportunities for advancement

has important implications for the current debate in the executive compensation literature. Tournament theory suggests that this debate, regarding whether observed compensation packages represent efficient contracts or rent extraction, cannot be settled until it is determined whether the title of CEO represents a final tournament prize, because the nature of an efficient contract depends upon whether there are further tournament rounds to be played (Rosen (1986)). Prizes in a tournament take two forms, a payment for winning the current round and an opportunity to compete in future rounds. In the final round of the tournament, the payment for winning must fully replace the incentives that previously came from opportunities to compete in future rounds. If acquiring a CEO position represents the final prize in the tournament, i.e., if current CEOs rarely compete against one another and advance based on stock price performance to secure higher paying positions, then CEOs have no incentives deriving from further opportunities to advance. Then according to tournament theory, CEO pay packages should be disproportionately large and exhibit low sensitivity to performance. My results suggest that opportunities for advancement provide no economically meaningful incentives to CEOs. This implies that acquiring a CEO position is effectively the final tournament prize, which then implies that observed large CEO compensation packages could be optimal.

The second main contribution is to show that own-firm pay is often more sensitive to performance than the traditional methodology indicates. Consistent with Garvey and Milbourn (2006), I find asymmetry in changes in pay for changes in performance by estimating piecewise regressions. Pay is the most sensitive to performance in the middle performance range, and the least sensitive to performance in the bottom quintile of

performance. I find pay-performance sensitivity deriving from changes in all forms of own-firm compensation ranges from $8.33 to $10.19 per thousand dollar change in shareholder wealth. For incentives stemming from the threat of dismissal, I also find that the pay-performance sensitivity varies depending on the level of performance. Depending on the definition of turnover and the proxy for market performance, I find pay-performance sensitivities that are two to five times larger for CEOs in the bottom versus the top quintile of performance. In sum, I find that CEOs face different incentives to increase stock price depending upon their current level of performance. These differing relations cannot be captured with the traditional methodology.

These results contribute to a large body of literature that examines the factors that influence the optimal level of pay-performance sensitivity. Garen's (1994) model suggests that the optimal level of sensitivity should differ according to firm characteristics that influence the variance of its profits. Empirical research in this area suggests relations between pay-performance sensitivity and capital structure (John and John (1993)), firms' investment opportunity sets (Baber, Janakiraman, and Kang (1996); Mayers and Smith (1992)), firm size (Baker and Hall (2004)) the level of managers' position within the firm (Aggarwal and Samwick (2003)) as well as managers' reputations (Milbourn (2003)). Influences outside the firm that affect pay-performance sensitivity include government regulation (Perry and Zenner (2001); Crawford, Ezzell and Miles (1995)) and product market competition (Aggarwal and Samwick (1999b)). Murphy (1999) reviews this literature and concludes that although these characteristics can explain some of the variation in observed pay-performance sensitivities, such contracts may still be insufficient to motivate managers to take actions in the interests of

shareholders. The results of this paper suggest that observed compensation contracts may not be as sub-optimal as some studies in literature would suggest.

The remainder of the paper proceeds as follows. Section 2 describes the sample selection procedure and variable construction. Section 3 presents results for individual components of the pay-performance sensitivity using traditional methods, whereas Section 4 presents the results for each component using methods that allow for nonlinearity. Section 5 aggregates these individual components, and Section 6 concludes.

3.2 Data and methods

3.2.1 Measuring pay-performance sensitivity from changes in own-firm compensation

I begin by estimating pay-performance sensitivity in the traditional way, as described in Jensen and Murphy (1990), using data for all CEOs in ExecuComp from 1992 – 2004. All dollar values are adjusted for inflation to 2000 dollars, and most variables are winsorized at the 1% and 99% level to mitigate the influence of outliers. I first construct the variables that Jensen and Murphy (1990) describe, including:

- Change in total pay: the change in salary plus bonus
- Change in CEO wealth: all pay (except stock options) plus the present value of the change in salary plus bonus
- Change in CEO pay-related wealth: the change in CEO wealth plus the change in the value of stock options
- Change in all forms of compensation: the change in CEO pay-related wealth plus the change in the value of inside stock holdings
- Change in shareholder wealth: the inflation-adjusted dollar change in firm value

- CEO's fractional ownership times the change in shareholder wealth

The change in total pay is the current year's minus the prior year's values of salary plus bonus. All pay is the ExecuComp variable TDC1 (total pay including options) less BLK_VALU (the Black-Scholes value of new option grants). The variable TDC1 is missing for many observations, so when necessary I calculate it as the sum of ExecuComp variables (SALARY, BONUS, OTHANN, RSTKGRNT, BLK_VALU, LTIP, ALLOTHTO).

To calculate the present value of the change in salary plus bonus, Jensen and Murphy (1990) assume that these changes are permanent, and that the CEO retires at age 70. For CEOs already over the age of 70, they assume that the CEO is serving his last year. They use a real interest rate of 3%. I make the same assumptions. I use the ExecuComp variable P_AGE_2[12] to calculate the CEO's age for each observation. I set missing values of age equal to the sample mean.

Because my data sources differ, the next two variables may differ slightly from those in Jensen and Murphy (1990). First, to estimate the change in the value of stock options, Jensen and Murphy (1990) calculate the value of options awarded, plus the change in value of all outstanding options, plus profits from options exercised during the year. These calculations are made using the Black-Scholes model allowing for continuously paid dividends. ExecuComp does not include the Black-Scholes value of outstanding options. Rather, the variable INMONEX (INMONUN) represents the value that would have been realized at year-end if the executive had exercised all vested

[12] This is an executive-specific field in ExecuComp, and represents the CEO's most recently reported age, not the age in the year of the observation.

(unvested) in-the-money options. Using these two variables, I estimate the change in the value of outstanding options using the one-year approximation method of Core and Guay (2002). I add this measure to the ExecuComp variables SOPTEXER and BLK_VALU to estimate the change in the value of all stock options.

Second, Jensen and Murphy's measure of the value of inside stock holdings includes shares held by family members and those for which the CEO is a trustee. I use the ExecuComp variable SHROWNPC, which does not include these items. Furthermore, this variable is missing for nearly half of the ExecuComp population. Whenever possible, I calculate this percentage as number of shares owned divided by number of shares outstanding.[13] Finally, I read proxy statements for a random sample of these missing observations, and find that typically the CEO holds less than 1% of the firm's stock, so for the remaining missing observations I set this variable equal to 0.5%.

The change in shareholder wealth is calculated as firm value at the end of the prior year (ExecuComp variable MKTVAL) times the rate of return on the firm's common stock. Whenever possible, this variable is taken from ExecuComp (TRS1YR) but if it is missing, I use returns from CRSP.

Pay-performance relations could be estimated for each executive, but this would require a time-series of data that is not available. I take the approach that is standard in the literature and instead estimate that relation using first differences in wealth. I estimate sensitivities rather than elasticities because the former has a more

[13] Due to differences in timing of when these variables are reported, the calculation SHROWN / SHRSOUT does not equal SHROWNPC, but is the best available approximation.

straightforward economic interpretation, although the latter provides a better fit to the data.

3.2.2 Measuring pay-performance sensitivity deriving from threat of dismissal

I begin with the entire ExecuComp database 1992 – 2004, and identify each case in which the CEO in a given year is not the same as in the prior year. I search news wire reports for announcements of the resignations in order to determine whether they are forced or voluntary. The majority of resignations are reported as retirements. I assume a resignation is not forced if it is announced well in advance, and/or if the CEO remains on the firms' board of directors. I classify resignations as being forced if (1) they are reported as such; (2) the resignation is sudden or unexpected and includes resignation from the board; or (3) there are other news announcements surrounding the resignation that suggest the CEO is departing on unfavorable terms.[14] This method is stricter than that of Parrino (1997), who identifies turnovers as forced if (1) they are reported as such; (2) the resigning CEO is under age 60 and not resigning for poor health or to take another position elsewhere; or (3) if the resignation is not announced at least six months in advance. The resulting data set has three variables. TURNOVER takes the value of one if the CEO resigns for any reason. PARRINO'S FORCED takes the value of one if Parrino's (1997) taxonomy suggests that the CEO was forced to resigned. Finally, FIRED takes the value of one if my taxonomy suggests that the manager was terminated.

[14] For example, in the months surrounding the resignation of NUI Corp.'s Kean Jr., the firm's bonds are downgraded and a criminal investigation of the firm's accounting practices is announced.

I use each of these events to estimate the pay-performance sensitivity deriving from the threat of dismissal.

The independent variable in these regressions is net-of-market returns as in Jensen and Murphy (1990). I test these relations using returns net of the CRSP value-weighted average, equal-weighted average and a beta-matched portfolio. If a manager is fired for poor performance, the announcement of his termination is likely to generate a positive stock price reaction (Huson, Malatesta and Parrino (2004)). Thus an adjustment to the data is necessary to prevent this increase in stock price from being attributed to the resigning manager if the turnover occurs mid-year. I measure stock returns from January 1 to the day before the announcement of the resignation, and annualize this return.

3.2.3 Measuring pay-performance sensitivity deriving from opportunities for advancement in the CEO labor market

To identify outside CEO hires, I similarly begin by using ExecuComp data to identify each case in which the CEO in a given year is not the same as in the prior year. For each of the new CEOs, I search newswires and proxy statements to collect as much information as possible about their career paths. The majority of CEOs in the sample attain their positions through internal promotions. When CEOs are hired from outside the firm, many hold lower level positions at larger firms, or work for privately held companies, hedge funds, or other entities not subject to disclosure requirements. Since my research design requires measuring the change in pay, I include only those outside CEO hires that come from other publicly traded firms for whom compensation is observable. Since I also require measures of performance and other firm-specific data, I include only those outside CEO hires that come from Compustat firms. If these firms are

not covered by ExecuComp, I hand collect the compensation data from proxy statements. This process generates a sample of 202 outside CEO hires who come from publicly traded companies that report compensation. My tests examine only those executives who hold the title of CEO at one firm, then obtain the title of CEO at another firm (within one year of hire), which reduces the sample to 51. Fee and Hadlock (2003) distinguish between CEOs that are "raided" and those that are not. A raided CEO is one who moves immediately from one job to the next. I therefore create two new variables, PROMOTE takes the value of one if a manager moves from one CEO position to another, and the variable RAID takes a value of one if he does so without a gap in employment.

To measure the relations between performance and outside opportunities in the managerial labor market, I follow Fee and Hadlock (2003) and use buy-and-hold stock returns. Fee and Hadlock (2003) measure these returns for sixty months prior to the CEO's departure from the old firm. Due to the small sample size, I shorten this period to thirty-three months. As in Fee and Hadlock (2003), the end of the performance measurement period is the last month preceding the announcement of the CEO's departure by at least one full month. I present results using six different measures of performance. I use raw stock price performance, performance net of the CRSP equal-weighted and value-weighted average, and three measures described in Fee and Hadlock (2003): performance net of a value-weighted industry matched portfolio, performance net of a size- and book-to-market-matched portfolio, and performance net of an industry-, size- and book-to-market-matched portfolio.

To calculate the change in pay related to these job changes, compensation must be estimated for several reasons. First, most changes occur mid-year. Second, pay at the

new firm is likely to include signing bonuses, relocation expenses, attorney's fees, and other one-time payments. Finally, pay at the old firm frequently includes severance-related pay. I read proxy statements of both the hiring firm and the firm supplying the CEO for information regarding the full year's salary (usually detailed in the employment agreement). When calculating the change in pay related to the promotion, I use the percent change in annual salaries to estimate the total change in compensation. I maintain the Jensen and Murphy (1990) assumptions described above regarding retirement age and interest rate.

3.3 Results using traditional methods

3.3.1 Estimating pay-performance sensitivity deriving from changes in own-firm compensation

I begin by estimating pay-performance sensitivities deriving from changes in pay granted at the CEO's own firm using the traditional approach, ordinary least squares (OLS) regressions. For this set of tests, I include all CEOs in ExecuComp 1992 -2004 who serve at least two full years.[15] I eliminate observations if they are missing one of the dependent variables described below. Column 1 of Table 11 reports the results of an OLS regression of the change in total pay (salary plus bonus) on the change in shareholder wealth (firm value at the end of the prior year times the rate of return on common stock, adjusted for inflation). The pay-performance sensitivity estimated is statistically significant, but economically small, at $0.10.

[15] Because I examine changes in pay, I require at least two full years of compensation. Garvey and Milbourn (2006) provide a thorough discussion of this selection bias, and conclude that its impact is minimal.

I next estimate an OLS regression of the change in CEO wealth (total pay plus the present value of the change in salary and bonus) on the change in shareholder wealth. I find that CEO wealth increases $0.80 for a thousand dollar increase in shareholder wealth. Column 3 of Table 11 reports the results for the change in CEO pay-related wealth (total pay, plus the present value of the change in salary and bonus, plus the change in value of stock options) on the change in shareholder wealth. The pay-performance sensitivity I estimate is $4.70.

I finally regress the change in shareholder wealth on the change in all forms of compensation (total pay, plus the present value of the change in salary and bonus, plus the change in value of stock options, plus the change in value of inside stock holdings). For the median CEO, whose fractional ownership is 0.442%, I find the pay-performance sensitivity generated by the change in all forms of compensation totals $9.19.

Jensen and Murphy (1990) find that inside stock holdings account for the majority (about 85%) of the pay-performance sensitivity they estimate from changes in all forms of compensation. My results show that inside stock is the largest single component of the total, about 49%, but also that a large fraction comes from stock options, about 42%. This result is consistent with the increased use of stock options, which were the largest component of pay in the 1990s (Murphy (1999)). My estimate of $9.19 is somewhat larger than Murphy's (1999) result of $5.90 and Jensen and Murphy's (1990) estimate of $3.25. Thus my results show that pay has become much more sensitive to performance over time, consistent with Murphy (1999) and others.

3.3.2 Estimating pay-performance sensitivity deriving from threat of dismissal

To estimate the incentives generated by the threat of dismissal, I estimate logit regressions of a dummy variable that takes the value of one if turnover occurred on contemporaneous and lagged net-of-market returns. The results are reported in Table 12. I first follow Jensen and Murphy and include all turnover events. By assuming that all turnovers are forced, this estimate represents an upper bound on the incentives provided by the threat of dismissal. I find consistently negative relations between turnover and lagged performance. Current performance generates a negative coefficient when performance is measured net of the CRSP equal-weighted average, but an insignificant coefficient when performance is measured net of the CRSP value-weighted average or the beta-matched portfolio.

Jensen and Murphy use these results to calculate the pay-performance sensitivity resulting from the threat of turnover. First, the expected wealth loss is calculated as the present value of one million dollars received annually beginning the next year and through age 66, multiplied by the implied probability of turnover from the logit regressions. The pay-performance sensitivity is then the difference between the expected wealth losses for net-of-market performance of 0% and –50%, divided by the associated loss in firm value for the median firm. I calculate the pay-performance sensitivity deriving from turnover in the same manner, but because total pay has grown so substantially since 1990, I do not use $1 million as an estimate of annual wealth loss; rather I use the sample mean of $0.654 million for salary only, and $1.930 million for total pay. For the average-aged CEO in my sample (55), I find a pay-performance sensitivity of $0.0128 to $0.0166 for salary alone, and $0.0378 to $0.0489 for total pay

(depending on the performance measure), resulting from the threat of turnover. This represents a substantial decrease from the Jensen and Murphy estimate of $0.30.

I repeat this analysis using two different dummy variables to indicate that a turnover event may have been forced. The first uses Parrino's (1997) taxonomy to identify forced turnover. I find consistently negative relations between both current and lagged performance and forced turnover. The resulting pay-performance sensitivity ranges from $0.0087 to $0.0124 for lost salary, and $0.0258 to $0.0366 for total pay.

When I use my own taxonomy to identify CEOs who are fired, I again find consistently negative coefficients on both current and lagged performance. Lost salary translates into a pay-performance sensitivity ranging from $0.0056 to $0.0077, and total compensation lost implies a pay-performance sensitivity of $0.0164 to $0.0227. These pay-performance sensitivities are smaller than those estimated using Parrino's (1997) taxonomy. This may be because my taxonomy for identifying forced dismissals is too stringent, and some of the CEOs who were forced out are not identified as such by my method, in which case I would have understated the magnitude of these incentives. Alternatively, it may be that I have correctly identified forced dismissals, but they occur too rarely to provide meaningful incentives because boards of directors find the costs to replace a CEO to be too high.

All of these pay-performance sensitivities are smaller than Jensen and Murphy's (1990) estimates, which may suggest that boards today work more quickly to remove CEOs before performance declines too much. Rather this may be a consequence of the increases in pay that have occurred since Jensen and Murphy's (1990) sample period of 1974 - 1986. Replacing a CEO adds value for shareholders only if the cash flows

generated by the new CEO will exceed the cost of replacement. These replacement costs may include severance payments to the departing CEO, plus signing bonuses to compensate the new CEO for forfeited payments from his prior employer (Fee and Hadlock (2003)), plus transactions costs such as relocation and legal fees, all of which have grown substantially over the last two decades. As a result, some boards of directors may find that even if a CEO is performing poorly, the costs to replace him exceed the benefits, and it is cheaper to simply wait for him to reach normal retirement age.

These small relations between threat of dismissal and stock price performance may also occur because using stock returns as the measure of performance is problematic for two reasons. First, as Milbourn (2003) suggests, the stock price at any given time is a weighted average of the firm's value under the current as well as future CEOs. If a firm has been performing poorly, and the market expects the current CEO will be fired, then the stock price will likely increase as a greater weight is placed on the value under the future CEO. This may occur even if the CEO is never actually fired. Second, several of the terminations in this sample are followed by investigations into the firms' accounting practices. To the extent that the market's assessment of firm value is based on accounting data, this introduces substantial noise into the analysis. Either of these issues could obscure the relations between turnover and performance.

3.3.3 Estimating pay-performance sensitivity deriving from opportunities in the managerial labor market

To estimate the incentives generated by the opportunities for advancement implied by results of Fee and Hadlock (2003), I estimate a logit regression of a dummy variable that takes the value of one if an outside CEO is hired on buy-and-hold returns of

the supplying firm. I first include all outside CEO hires ("jumps"), and then include only those CEOs who move directly from one firm to the next ("raids"). The results are reported in Table 13. I find no significant relations between performance and advancement in the managerial labor market, with two exceptions. I find a positive relation for jumps when market performance is measured by the CRSP value-weighted average, and I find a positive relation for raids when market performance is proxied by the CRSP equal-weighted average.

My results differ from the logit results of Fee and Hadlock (2003) (reported in their Table 15) because they include all executives who jump to CEO positions, whereas I examine only CEO to CEO jumps. In their analysis they include a dummy variable to indicate that the executive is jumping from a CEO position, which generates a negative coefficient. Thus, my results do not necessarily contradict theirs. I also have a larger sample, as Fee and Hadlock (2003) examine only S&P 500 firms.

The unconditional probability of moving from one CEO position to another is small, as only one or two CEOs make such a move each year. But the opportunity to do so could still be an important source of incentives if the probability of a jump is strongly related to stock price performance, and if the change in pay is sufficiently large. To quantify these incentives, I estimate the pay-performance sensitivity using the same method as described for the sensitivity resulting from the threat of dismissal. I calculate the present value of the change in pay received annually beginning the next year and through age 66, multiplied by the implied probability of advancement from the logit regressions. The pay-performance sensitivity is then the difference between the expected

wealth gain for improving buy-and-hold returns from the middle to the top quintiles of performance, divided by the resulting change in firm value for the average firm.

To estimate the expected change in wealth associated with obtaining a new CEO position, I use the average percent change in salary awarded to the CEOs who jump, times the average salary of all CEOs in ExecuComp. For all outside CEO hires in my sample, the average salary increases from $573.9 to $720.9 at the new firm. I multiply this 25.6% increase by the average salary of $646.24. Using this estimate, I find pay-performance sensitivities ranging from $(0.0014) to $0.0407[16], depending upon the performance measure. I also estimate the pay-performance sensitivity deriving from changes in total compensation. For the ExecuComp population at large, salary represents 33.9% of total compensation (ExecuComp variable TDC1). I use this figure to estimate the pay-performance sensitivity in the same manner described above. These sensitivities range from $(0.0041) to $0.1202.

Raided CEOs enjoy larger increases in pay when they realize a new position, and on average their salaries increase from $579.0 to $760.0. For raided CEOs if performance improves from the middle to the top quintile, the pay-performance sensitivity of the change in salary alone ranges from $0.0015 to $0.0603; the corresponding figure for the change in total compensation ranges from $0.0047 to $0.1867. When compared to the incentives provided by changes in own-firm compensation, these incentives arguably are not economically significant.

[16] Some of these outside CEOs obtain new their new positions after a bankruptcy or acquisition, which is why I sometimes find a negative pay-performance sensitivity.

There are three issues, however, on which my data are silent, that may impact the incentive effects of the opportunities provided by the managerial labor market. First, a CEO jumps only when both the supply and demand for his labor come together, so what I cannot observe is unsatisfied demand. That is, some of the CEOs in the sample may have received offers to jump that they declined. (In fact, they may even have used those offers to negotiate increases in own-firm compensation.) As a result, the actual incentives may be larger than those that I have estimated. Second, I cannot measure an individual CEO's beliefs about his outside opportunities. A highly skilled manager may perceive that the probability he will be able to jump to another job is much higher than the probabilities that are borne out by the data, and so my results would understate his perceived incentives. Finally, I cannot measure an individual CEO's desire to participate in the labor market. As CEO of Microsoft, Bill Gates generated very high stock price performance, yet likely had little interest in moving to another firm even if such opportunities had been abundant, and so my results would overstate his incentives. To the extent that CEOs are heterogeneous on these two dimensions, some of them may perceive the incentives provided by the market for managerial labor to be greater than others do.

3.4 Results using nonlinear models

3.4.1 Estimating pay-performance sensitivity deriving from changes in own-firm compensation allowing for nonlinear relations

I next consider whether pay-performance sensitivities vary with the level of performance, as suggested by the recent work of Garvey and Milbourn (2006). These authors provide evidence that new pay is granted to executives asymmetrically with

respect to performance. My tests differ slightly because Garvey and Milbourn (2006) examine only newly granted pay, and therefore exclude changes in the value of existing stock and options. But if new pay is granted in an asymmetric way, then it must be the case that changes in pay that include changes in the value of existing stock and options are also asymmetric, as these latter values will move in lockstep with changes in shareholder wealth. To capture this asymmetry, I estimate the relation between changes in own-firm pay and changes in performance using a piecewise linear model in which observations are sorted into quintiles based upon the level of performance.

I otherwise follow the methodology of Aggarwal and Samwick (1999a) and include the cumulative distribution function of the variance of stock returns as a control variable, as well as an interaction of this variable with performance. Aggarwal and Samwick (1999a) find that excluding these variables will bias pay-performance sensitivities towards zero. I also include year dummies, and executive fixed effects to control for any firm- or executive-specific characteristics that might influence changes in pay. These results are presented in Tables 14 and 15. I restrict discussion to results for the bottom, middle, and top quintiles of performance. All pay-performance sensitivities are calculated using the median variance of stock returns (i.e. cdf = 0.5) and median shareholdings by the CEO of 0.442%.

My results are consistent with a nonlinear relation between changes in pay and changes in performance. The change in contemporaneous performance generates positive and significant coefficients for all measures of changes in pay and for all quintiles of performance. For firms in the lowest quintile of performance (median performance = -27.99%), I find a pay-performance sensitivity of $0.14. For the change in

CEO wealth for firms in this quintile, pay-performance sensitivity is just $1.14. CEO pay-related wealth increases by $3.95 for a thousand dollar increase in shareholder wealth. Finally, the change in all forms of compensation is $8.33 for a thousand dollar change in shareholder wealth for managers in this performance quintile. For each of these four measures, the estimated pay-performance sensitivities are lower for CEOs in this quintile of performance than they are for most of the other quintiles. This result is consistent with Garvey and Milbourn (2006) who find that managers at the worst performing firms receive pay that is the least sensitive to performance.[17] Notably, these sensitivities are also lower than those estimated in Table 11 using the linear model.

One explanation for these results may be that the lower bound of compensation is zero, so there is a lower bound on changes in compensation. For example, if options have fallen so far out-of-the money that their value is close to zero, for subsequent performance declines the change in the value of options would also be close to zero. This would lead to a lower pay-performance sensitivity than if in-the-money options move out-of-the-money.

Pay exhibits the highest sensitivity to performance for firms in the middle performance quintile (mean performance = 17.50%). The change in total pay for the CEOs of these firms is $0.19 for a thousand dollar increase in shareholder wealth. The changes in CEO wealth and in CEO pay-related wealth are $1.31 and $5.59, respectively. For all forms of compensation, CEOs in this quintile earn $10.19 for every thousand

[17] Of course it is impossible to isolate causality. Perhaps performance would be better at these firms if their managers were granted more performance-sensitive pay.

dollar increase in shareholder wealth. Most notably, the pay-performance sensitivities estimated here are larger than those estimated using the linear model.

These results may obtain because the lower and upper bounds on compensation that managers in the lowest and highest quintiles of performance face may be absent for those managers in the middle quartile. In this quartile of performance, compensation can swing freely in either direction before hitting some bound, and thus changes in pay are more sensitive to changes in performance when performance is nearer to the median. An alternative explanation is that boards of directors recognize that managers in this range are less likely to face incentives from other sources. These managers probably have little fear of being fired, and yet little hope of being promoted (assuming these two events are related to stock price performance), thus changes in own-firm pay provide their only incentives.

Beyond the middle quintile of performance, sensitivities begin to decline as performance improves. For firms in the top quintile of performance (mean performance = 67.05%), sensitivities are much smaller than they are for the middle quintile, and are not much different than they are for the bottom quintile. The change in total pay is $0.12 for a thousand dollar change in shareholder wealth, and the change in CEO wealth is $0.85. The change in CEO pay-related wealth for CEOs in this quintile of performance is $4.59 for a thousand dollar change in shareholder wealth, and the change in all forms of compensation is $9.20 for a thousand dollar change in shareholder wealth.

Pay-performance sensitivity may be lower for CEOs in this quintile because shareholder outrage effectively places an upper bound on pay. CEOs in this quintile earn an average of almost $8 million per year, and many earn much more than this. Boards

may be reluctant to grant larger compensation packages to these top performing managers, despite their high performance, because of negative media attention and shareholder objections. In addition, undiversified risk-averse managers may choose to reduce their equity in the firm as they become wealthier (Ofek and Yermack (2000)). Since the majority of the pay-performance sensitivities I estimate comes from the changes in the value of stock and options, this may effectively place an upper bound on pay-performance sensitivities.

Overall I find that the relations between changes in pay and changes in performance depend upon the level of performance, consistent with Garvey and Milbourn (2006). The general trend amongst these estimates is that the relation between changes in pay and changes in performance is higher when performance is in the middle range, and is lower when performance is very high or very low. Most importantly, I find that pay can be more, or less, sensitive to performance than the estimates of a linear model would suggest.

Pay-performance sensitivities are known to vary based on firms' investment opportunity sets (Baber, Janakiraman, and Kang (1996); Mayers and Smith (1992)) and capital structures (John and John (1993)). I briefly consider whether the asymmetry I document varies on these dimensions as well. I identify "Jensen-type" firms using the method described in Rajan and Wulf (2006). I first compute the average percent change in each firm's rate of investment in three future periods, then average this growth rate across firms in each three-digit SIC code to estimate each industry's investment opportunity set. I estimate free cash flow as the lag of (operating income before depreciation, less interest, taxes paid and capital expenditures) scaled by firm assets. I

then identify firms in the top quintile of free cash flow and bottom quintile of investment opportunities (Jensen-type firms), and firms in the bottom quintile of free cash flow and top quintile of investment opportunities. Results (not reported) show that pay is much less sensitive to performance at Jensen-type firms than at high growth/low free cash flow firms. Both sets of firms exhibit the same non-linearity that I document for the full sample, but the effect is much more pronounced at high growth/low free cash firms. Sensitivities are often negative for firms in this group, suggesting that these CEOs continue to receive increases in pay even when performance declines. Given the high degree of discretion required by managers at these firms, this negative relation between changes in pay and performance may be efficient, especially if executive retention is important.

Many of the firms in my sample have no long-term debt, so I simply differentiate between firms with and without long-term debt to examine the effect of capital structure. I once again find that changes in pay are related in a nonlinear way to changes in performance for both groups. I find pay-performance sensitivity is higher, and the degree of asymmetry is greater, at firms with debt than at firms without debt. At firms with debt, changes in all forms of compensation are twice as high for firms in the middle quintile of performance than for firms in the bottom quintile. For firms in the top quintile of performance, changes in all forms of compensation are about 74% as large as for those firms in the middle quintile. At firms without debt, changes in all forms of compensation are only one and one-half times as high for firms in the middle quintile of performance as for firms in the bottom quintile. And for firms in the middle and top quintiles, the pay-performance sensitivities are nearly equal. This greater asymmetry at firms with debt

may result because in the presence of a bondholder-shareholder conflict, the CEO's interests may become more aligned with those of shareholders.

3.4.2 Estimating pay-performance sensitivity deriving from threat of dismissal and allowing for nonlinear relations

I next consider whether the threat of dismissal is related to performance in a nonlinear way. That is, I want to examine whether the change in the probability of turnover differs for changes in performance when performance is low than when performance is high. Because the logistic distribution is symmetric about the mean, the logit model will not capture this relation. Instead I estimate the likelihood of turnover using a log-log model, which is asymmetric about the mean. As x increases in this model, the change in the probability that y = 1 increases rapidly from 0 to 0.2, then slowly from 0.8 to 1. This model will better capture the relation between performance and the threat of dismissal if a change in performance has a different influence on the change in the likelihood of dismissal depending on the level of performance. As before, I use three different dependent variables. The first takes the value of one if turnover occurred, the second takes a value of one if Parrino's (1997) taxonomy indicates the CEO was forced to resign and the last takes a value of one if my own taxonomy indicates that the CEO was fired. I also use the same three performance measures as in Table 12: stock returns net of the CRSP equal-weighted average, net of the CRSP value-weighted average and net of a beta-matched portfolio. I also include in these tests the cdf of the variance of these net-of-market returns, because if boards of directors use these returns as a signal of CEO ability (Milbourn (2003)), the quality of that signal is higher if its variance is lower. I use the predicted probabilities from these regressions to calculate

pay-performance sensitivities deriving from changes in performance from one quintile to the next in the same manner described in Section 3.2. I estimate all of these relations for firms with variances at the median (i.e. cdf = 0.5). The results are reported in Tables 16 and 17.

When the dependent variable takes a value of one for all turnover events, I find negative relations between turnover and performance, both contemporaneous and lagged. I find a positive relation between turnover and the variance of net-of-market returns in all specifications, suggesting that boards of directors are more likely to remove CEOs whose performance is more inconsistent. For CEOs in the bottom quintile of performance, the predicted probability of turnover is 18% or higher, depending on the performance measure. The pay-performance sensitivity (based on the change in probability from being in the bottom versus the fourth quartile) ranges from \$0.1014 to \$0.1872 for lost salary alone, and for total pay lost ranges from \$0.3101 to \$0.5726. In the middle quintile, the predicted probability of turnover falls below 10%. The pay-performance sensitivity of lost salary alone ranges from \$0.0852 to \$0.1378, and based on total pay lost ranges from \$0.2608 to \$0.4216. Finally, those CEOs in the top quintile of performance face predicted probabilities of turnover of less than 5%. The pay-performance sensitivity based on lost salary alone ranges from \$0.0441 to \$0.0582, and based on total pay lost ranges from \$0.1350 to \$0.1780.

When the dependent variable takes a value of one for forced turnovers using Parrino's (1997) taxonomy, I again find consistently negative relations between forced turnover and both contemporaneous and lagged performance. I also find consistently positive relations between forced turnover and both the cdf of the variance of returns and

the interaction of this variable with performance. I again find that the predicted probability of turnover decreases monotonically as performance improves. In the bottom quintile of performance, the predicted probability of turnover is between 5% and 6%, depending on the performance measure. The pay-performance sensitivity of lost salary alone ranges from $0.0354 to $0.0542, and based on total pay lost ranges from $0.1082 to $0.1658. The predicted probabilities of turnover for those CEOs in the middle quintile of performance are just over 2%. The pay-performance sensitivity based on lost salary alone ranges from $0.0279 to $0.0363 for these executives, and based on total pay lost ranges from $0.0854 to $0.1112. Finally, for CEOs in the top quintile of performance, the predicted probability of turnover is less than 1%, depending on the performance measure. The pay-performance sensitivity ranges from $0.0117 to $0.0128 for lost salary alone, and for total pay lost ranges from $0.0359 to $0.0391.

Lastly, when the dependent variable takes a value of one if the CEO has been fired according to my taxonomy, I once again find consistently negative relations between firings and performance but consistently positive relations between firings and the variance of performance. Those CEOs in the bottom quintile of performance face predicted probabilities of turnover of just over 3%. The pay-performance sensitivity based on lost salary alone ranges from $0.0261 to $0.0337 for these CEOs, and based on total pay lost ranges from $0.0797 to $0.1030. For CEOs in the middle quintile of performance, the predicted probability of turnover is just below 1%, depending on the performance measure. The pay-performance sensitivity ranges from $0.0171 to $0.0203 for lost salary alone, and for total pay lost ranges from $0.0523 to $0.0620. The predicted probability of turnover is just 0.21% for CEOs in thee top quintile of

performance. The pay-performance sensitivity of lost salary alone ranges from $0.0051 to $0.0058, and based on total pay lost ranges from $0.0155 to $0.0177. Although the pay-performance sensitivities presented in Table 17 are much greater than those in Table 12, they are still much smaller than those deriving from changes in own-firm compensation given in Table 15, suggesting that the threat of dismissal provides managers with incentives that are economically small.

I use Akaike's information criterion to compare the fit of the logit versus the log-log model. I find smaller AIC for the log-log model for all specifications, suggesting the nonlinear model offers a better fit to the data. Importantly, the coefficients estimated with the log-log model generate different predicted probabilities and pay-performance sensitivities, which reveal that the incentives CEOs face to improve firm performance differ depending on the level of performance. For any of my turnover measures or performance measures, pay-performance sensitivities are substantially lower for CEOs in the middle range of performance than for the worst performing CEOs, and are substantially lower for the best performing CEOs than for those CEOs in the middle range of performance.

3.4.3 Estimating pay-performance sensitivity deriving from opportunities for advancement in the managerial labor market and allowing for nonlinear relations

Finally, I estimate the pay-performance sensitivity that derives from the incentives generated by the managerial labor market that are implied by the results of Fee and Hadlock (2003), and consider whether the change in the probability of advancement differs for changes in performance when performance is low than when performance is high. Again, since the logistic distribution is symmetric about the mean, the logit model

will not capture this relation. I estimate the likelihood of advancement using a complementary log-log model, which is asymmetric about the mean. As x increases in this model, the change in the probability that y = 1 increases slowly from 0 to 0.2, then rapidly from 0.8 to 1. This model will better capture the relation between performance and the likelihood of obtaining an outside CEO position if increased performance has a greater influence on the likelihood of advancement for managers whose performance is already high than for managers whose performance is low. I use the same two measures of advancement opportunities (jumps and raids) and the same six performance measures as in Table 13. I again add to these models the cdf of the variance of these performance measures. If boards of directors use stock price as a signal of a CEO's ability as suggested by Fee and Hadlock (2003), the higher the quality of that signal if its variance is lower. As such, boards may consider this variance when making their hiring decisions. I use the predicted probabilities from these regressions to calculate pay-performance sensitivities deriving from changes in performance from one quintile to the next as previously described. I estimate all of these relations for firms with variances at the median (i.e. cdf = 0.5). These results are reported in Tables 18 and 19.

For both jumps and raids, for all size measures of performance, I find insignificant coefficients on both the performance measure and its interaction with the cdf of the variance. Interestingly, the only variable that is significant in these tests is the cdf of the variance in isolation, which generates negative coefficients in ten out of twelve cases. This implies that higher variance in stock returns is punished by the managerial labor market, in the form of lower opportunities for advancement. Together with the results from the last section, that managers whose performance is more volatile face a

higher likelihood of being fired, this suggests that boards of directors have a strong preference for more consistent stock returns.

Because the coefficient estimates are insignificant, the predicted probabilities and pay-performance sensitivities I estimate are small. I restrict discussion to the results on raids, given in Panel B. In no case does the predicted probability of being raided exceed 0.04%. When the independent variable is performance, the pay-performance sensitivity based on total pay lost increases from $0.0002 to $0.0003 for CEOs in the top versus the middle performance range. When the independent variable is performance net of the CRSP value-weighted average, the pay-performance sensitivity based on total pay lost increases from $0.0001 to $0.0002 for those CEOs in the middle quintile of performance rather than the bottom. With these two exceptions, the pay-performance sensitivities I estimate do not differ across performance quintiles.

I again use Akaike's information criterion to compare the fit of the logit versus the complementary log-log model. I find smaller AIC for the complementary log-log model for all specifications except when the independent variable is performance net of an industry-matched portfolio. The most important result is that I find, with all of my models, that opportunities for advancement in the managerial labor market provide managers with incentives that are economically trivial. Although Fee and Hadlock (2003) find that boards of directors consider past stock returns when deciding whether to hire an outside CEO, that they do so is sufficient to provide incentives for CEOs at large to improve firm performance. In other words, the position of CEO represents the grand prize in a tournament, and its accompanying payment must be disproportionately large in

order to fully replace the incentives that were once provided by opportunities for advancement.

I consider briefly whether high performance as a CEO offers access to other potentially lucrative employment, such as a position as a hedge fund manager. If this is the case, then these other employment opportunities would provide incentives not captured in my analysis. Table 21 reports the future employment of all CEOs who depart the Execucomp universe before they reach age 62 and are not forced out according to Parrino's (1997) taxonomy. Only eleven of these CEOs secure positions that have the potential to pay very well, such as entrepreneurship and venture capital investing. The majority of these CEOs appear to have retired, or at least do not obtain further employment that is announced publicly. I conclude that for most CEOs, opportunities for employment in roles other than that of CEO, are not likely to be a meaningful source of incentives.

3.5 CEOs' total incentives

The last remaining task is to aggregate the three forms of incentives that have been measured and to compare their magnitudes. I restrict discussion here to a sub-set of the paper's results. For incentives deriving from the threat of dismissal, I use Parrino's (1997) taxonomy and performance net of the CRSP equal-weighted average. I use Jensen and Murphy's (1990) variable "changes in all forms of compensation" (which includes all direct pay, plus the present value of the change in salary and bonus through retirement, plus the change in the value of stock and options) to measure incentives provided by changes in own-firm pay. For incentives deriving from opportunities to advance in the managerial labor market, I use Fee and Hadlock's (2003) definition of raided CEOs and performance net of the CRSP equal-weighted average.

Using the traditional linear methods (OLS and logit regressions) to estimate pay-performance sensitivities, I find CEOs face total incentives of $9.4046 per thousand dollar change in shareholder wealth. Nearly 98% of this comes from changes in own-firm pay. About 2% comes from the opportunity to be raided, and only about 0.3% comes from the threat of dismissal.

Using non-linear methods (piecewise regressions, log-log and complementary log-log models), I find pay-performance sensitivity is highest when performance is in the middle range, and lower when performance is high or low. For CEOs in the middle range of stock price performance, I find a total pay-performance sensitivity of $10.2784, over 99% of which comes from changes in own-firm pay.

For CEOs in the bottom quintile of performance, I find a total pay-performance sensitivity of $8.4347, over 98% of which comes from changes in own-firm pay. The

threat of dismissal is the second largest component of the total pay-performance sensitivity, contributing about 1.28%. Finally the chance to be raided is less than 1% of the total.

For CEOs in the top quintile of stock price performance, I find a total pay-performance sensitivity of $9.2402, over 99% of which comes from changes in own-firm pay. The threat of dismissal for these managers represents only about 0.41% of their total pay-performance sensitivity. The chance to be raided, even for these highly performing managers, provides incentives whose magnitude is inconsequential.

3.6 Conclusion

The current debate in the literature on executive compensation surrounds whether observed CEO compensation contracts represent efficient contracting or managerial rent extraction. This paper contributes to the debate in two primary ways. First, I consider whether there exists a labor market for CEOs that functions like a tournament in which CEOs advance based on stock price performance. Tournament theory predicts that if there is such a tournament providing incentives for CEOs to improve performance, then the compensation contracts offered to CEOs can be smaller and have lower pay-performance sensitivity than if there is not. I find that the pay-performance sensitivities that derive from the opportunity to advance in the managerial labor market are at best $0.20, and often less than $0.01 (depending on the performance measure and model used). These incentives to perform are trivial compared to the minimum of about $8 deriving from changes in own-firm compensation. These results suggest that the opportunity to move from one CEO position to another does not provide incentives that are economically large enough to motivate managers to improve firm performance.

Therefore, any CEO position represents the final prize in a tournament, the payment for which must be large enough to replace the incentives that had previously been provided by the opportunity to advance in the tournament (Rosen (1986)). Thus observed compensation contracts that are often described as "excessive" and appear to be managerial rent extraction, may actually represent efficient contracts when viewed from the perspective of tournament theory.

Second, based on recent work by Garvey and Milbourn (2006), I estimate pay-performance sensitivities deriving from changes in pay at the CEO's current firm using piecewise regressions that allow for the relations between changes in pay and changes in performance to vary across performance quintiles. I find that pay-performance sensitivities are the smallest when performance is very high or very low, and are the largest when performance is in the middle. My estimates for the pay-performance sensitivity deriving from changes in all forms of compensation range from $8.33 to $10.19 per thousand dollar change in shareholder wealth. These results are consistent with the asymmetry in pay for performance documented by Garvey and Milbourn (2006), and demonstrate that pay-performance sensitivities can vary in ways that are not captured

with a simple linear model.

Further, I also estimate the incentives deriving from the threat of dismissal and opportunities for advancement using nonlinear probability models that allow for changes in probabilities to be asymmetric for a given change in the independent variables at different points along the distribution. I find that incentives from the threat of dismissal vary depending on the level of performance. For a manager whose performance is in the top quintile, this pay-performance sensitivity is not greater than $0.1780, whereas for a manager in the bottom quintile of performance, this pay-performance sensitivity is as high as $0.5726. Overall the results of this paper demonstrate that not all CEOs are remunerated equally. When managers' total incentives to improve firm performance are measured in light of their level of performance, and are considered in light of the literature on tournament theory, observed compensation packages may not be as suboptimal as some of the extant literature would suggest.

4. CONCLUSION

A large body of literature in economics and finance considers how people respond to incentives. These questions are prevalent throughout the literature, and have been addressed in papers on agency problems, banking, capital structure and even microstructure. The purpose of this research is to consider two distinct sets of incentives faced by firm managers, and to examine how managers respond to those incentives.

The second section examines how managers make the choice of whether or not to accurately report financial performance, and how that choice can depend upon the firm's use of bank debt in its capital structure. There are both expected costs and benefits to misreporting. If detected and exposed, misreporting can result in stock price declines and even fines or jail time for the culpable executives. If undetected, misreporting can help to lower borrowing costs or improve contractual terms with stakeholders such as lenders, suppliers and employees, thereby lowering operating costs. My results suggest that, for firms financed with bank debt, the benefits of misreporting can exceed the expected costs but only when those expected costs are small, as they are for relatively less severe types of misreporting. I find the likelihood of relatively less severe misreporting increases with firms' use of bank debt. This result is consistent with the debt covenant hypothesis and suggests managers may use aggressive accounting techniques to avoid covenant violations or to lower borrowing costs. I find *ex ante* changes in bank debt are positively related to misreporting. These results are consistent with the hypothesis that an increased reliance on bank debt increases the benefits of misreporting more than the expected costs. I also find, however, that *ex post* changes in bank borrowing are not related to misreporting, suggesting that managers who have misreported do not attempt to avoid

monitoring by subsequently reducing their reliance on bank debt. But when the misreporting is relatively more severe, as it was in the spectacular cases of Enron and WorldCom, I find neither a relation between the likelihood of misreporting and the use of bank debt, nor a relation to changes in bank debt before or after the misreporting has occurred.

The most important result is that I do not find any negative relations between the use of bank debt and financial statement misreporting, inconsistent with the literature on the value of bank monitoring. My results suggests that, despite the extant literature on banks as external monitors who increase the value of their borrowers, any monitoring they provide is insufficient to serve as a deterrent to financial statement misreporting or to detect misreporting once it has occurred. The results of this paper have implications for the design of optimal capital structures and governance mechanisms, as they suggest that bank borrowing may increase certain agency costs for borrowing firms. Firms that rely more heavily on bank financing may require greater internal monitoring mechanisms in light of the incentives to misreport that derive from bank debt.

The third section addresses the questions of whether the market for managerial talent provides incentives for CEOs to improve firm performance, and whether these and other incentives differ for some CEOs. That is, managers who perform well may face different incentives than those who perform poorly. My study makes two main contributions. First, quantifying the magnitude of the incentives provided by opportunities for advancement has important implications for the current debate in the executive compensation literature. Tournament theory suggests that this debate, regarding whether observed compensation packages represent efficient contracts or rent extraction,

cannot be settled until it is determined whether the title of CEO represents a final tournament prize, because the nature of an efficient contract depends upon whether there are further tournament rounds to be played (Rosen, 1986). Prizes in a tournament take two forms, a payment for winning the current round and an opportunity to compete in future rounds. In the final round of the tournament, the payment for winning must fully replace the incentives that previously came from opportunities to compete in future rounds. If acquiring a CEO position represents the final prize in the tournament, i.e. if current CEOs rarely compete against one another and advance based on stock price performance to secure higher paying positions, then CEOs have no incentives deriving from further opportunities to advance. Then according to tournament theory, CEO pay packages should be disproportionately large and exhibit low sensitivity to performance. My results suggest that opportunities for advancement provide no economically meaningful incentives to CEOs, which implies that acquiring a CEO position is effectively a final tournament prize, which then implies that observed CEO compensation packages may not represent rent extraction.

The second main contribution is to show that own-firm pay is often more sensitive to performance than the traditional methodology indicates. Consistent with Garvey and Milbourn (2006), I find asymmetry in changes in pay for changes in performance by estimating piecewise regressions. Pay is the most sensitive to performance in the middle performance range, and the least sensitive to performance in the bottom quintile of performance. I find pay-performance sensitivity deriving from changes in all forms of own-firm compensation ranges from $8.33 to $10.19 per thousand dollar change in shareholder wealth. For incentives stemming from the threat of

dismissal, I also find that the pay-performance sensitivity varies depending on the level of performance. Depending on the definition of turnover and the proxy for market performance, I find pay-performance sensitivities that are two to five times larger for CEOs in the bottom versus the top quintile of performance. In sum, I find that CEOs face different incentives to increase stock price depending upon their current level of performance, results which would not be captured by the traditional methodology. Murphy (1999) reviews the literature on pay-performance sensitivity, and concludes that observed compensation contracts may be insufficient to motivate managers to take actions in the interests of shareholders. The results of this paper suggest that observed compensation contracts may not be as sub-optimal as some extant studies would suggest.

In sum, these results contribute to the literature by demonstrating that although managers respond rationally to the incentives that they face, those incentives are many and their effects sometimes ambiguous. Shareholders, and the boards of directors that they have elected to serve their interests, must consider all of the incentives that CEOs face when designing executive compensation packages to align the interest of the CEO with their own.

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APPENDIX

Table 1

Descriptive Statistics and Difference in Means Tests for Control and Test Variables

Variable	Mean	Standard Deviation	Upper Quartile	Median	Lower Quartile	Difference in Means, *p*-value
Bank debt proxy						
GAO sample	0.03	0.06	0.04	0.00	0.00	<0.01
Control sample	0.02	0.04	0.02	0.00	0.00	
Other debt						
GAO sample	0.26	0.19	0.38	0.25	0.12	<0.01
Control sample	0.22	0.17	0.33	0.21	0.06	
Change in bank debt						
GAO (before)	0.31	1.55	0.00	0.00	0.00	<0.01
Control sample	0.10	1.05	0.00	0.00	0.00	
GAO (after)	0.10	1.23	0.00	0.00	-0.44	0.96
Board meetings						
GAO sample	7.73	3.46	9.00	7.00	6.00	<0.01
Control sample	6.80	3.21	8.00	6.00	5.00	
CEO tenure						
GAO sample	6.81	7.15	9.00	5.00	2.00	0.80
Control sample	6.69	7.46	9.00	4.00	1.00	
Altman's Z						
GAO sample	24.77	47.66	9.12	4.96	3.00	0.16
Control sample	29.54	50.91	16.11	5.14	2.75	
3-yr OIBD/TA						
GAO sample	0.00	0.32	0.05	-0.00	-0.05	<0.01
Control sample	0.02	0.56	0.03	-0.00	-0.05	
Sales growth						
GAO sample	0.22	0.37	0.28	0.12	0.02	<0.01
Control sample	0.12	0.31	0.20	0.06	-0.02	
Age of firm						
GAO sample	25.83	19.46	36.00	23.50	9.00	<0.01
Control sample	22.10	18.37	31.00	17.00	7.00	
Merger						
GAO sample	0.12	0.32	0.00	0.00	0.00	0.14
Control sample	0.15	0.36	0.00	0.00	0.00	
Total Assets						
GAO sample	3004.97	4777.58	3426.40	805.71	287.05	<0.01
Control sample	1997.17	3959.27	1630.37	552.77	222.91	
Volatility						
GAO sample	0.43	0.20	0.56	0.40	0.29	0.02
Control sample	0.40	0.20	0.50	0.36	0.26	

Table 1 Continued

Variable	Mean	Standard Deviation	Upper Quartile	Median	Lower Quartile	Difference in Means, *p*-value
Stock ratio						
GAO sample	0.15	0.24	0.15	0.05	0.02	0.66
Control sample	0.15	0.22	0.17	0.04	0.01	
Option ratio						
GAO sample	0.15	0.15	0.22	0.11	0.05	<0.01
Control sample	0.12	0.13	0.16	0.08	0.03	
Market-to-Book						
GAO sample	1.90	1.03	2.22	1.59	1.29	<0.01
Control sample	2.25	1.59	2.53	1.71	1.29	
Fixed asset ratio						
GAO sample	0.29	0.21	0.41	0.24	0.15	<0.01
Control sample	0.36	0.23	0.52	0.31	0.18	
Leverage						
GAO sample	-0.05	0.12	0.02	-0.05	-0.14	0.87
Control sample	-0.05	0.14	0.02	-0.07	-0.15	
Unrated						
GAO sample	0.47	0.50	1.00	0.00	0.00	0.03
Control sample	0.54	0.50	1.00	1.00	0.00	

"GAO sample" is the set of firms that restated their financial statements, identified by the General Accounting Office (n = 230 firm-years). Firms in Execucomp that are not accused of misreporting by the GAO or the SEC are included as control firms (n = 10,801 firm-years). Bank debt is by debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Change in bank debt is the percentage change in the bank debt proxy in the two years before or after misreporting. Board meetings is the number of board meetings held during the fiscal year. CEO tenure is the observation year, less Execucomp variable BECAMECEO. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Share ratio and option ratio are similar to the incentive ratio defined in Bergstresser and Philippon (2006), with option values calculated using the Core and Guay (2002) one-year approximation method. Market-to-Book is the ratio of (book value of assets minus book value of equity plus market value of equity) to book value of assets, averaged over the preceding five years. Fixed asset ratio is net property, plant and equipment divided by total assets, averaged over the preceding five years. Leverage is an instrumental variable described in Johnson (1997). Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise.

Table 2
Results of Logit Regressions Comparing GAO Sample to Control Firms

	Coefficient Estimate	Marginal Effect
Bank debt proxy	9.31	0.15
	(<0.01)	(<0.01)
Other debt	2.27	0.04
	(<0.01)	(<0.01)
Board meetings	0.06	0.00
	(<0.01)	(<0.01)
Tenure	0.00	0.00
	(0.66)	(0.66)
Altman's Z	0.00	0.00
	(0.15)	(0.15)
3-yr OIBD/TA	0.59	0.01
	(0.02)	(0.02)
Sales growth	0.86	0.01
	(<0.01)	(<0.01)
Age	0.01	0.00
	(<0.01)	(<0.01)
Merger indicator	-0.58	-0.01
	(<0.01)	(<0.01)
Total assets	0.00	0.00
	(0.67)	(0.67)
Volatility	1.29	0.02
	(<0.01)	(<0.01)
Unrated	-0.02	0.00
	(0.89)	(0.89)
Stock ratio	0.60	0.01
	(0.08)	(0.08)
Option ratio	1.59	0.03
	(<0.01)	(<0.01)
Intercept	-6.32	
	(<0.01)	

"GAO sample" is the set of firms that restated their financial statements, identified by the General Accounting Office (n = 230 firm-years). Firms in Execucomp that are not accused of misreporting by the GAO or the SEC are included as control firms (n = 10,801 firm-years). Dependent variable takes a value of one if firm is in the GAO restatement database. Bank debt is by debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Board meetings is the number of board meetings held during the fiscal year. CEO tenure is the observation year, less Execucomp variable BECAMECEO. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Share ratio and option ratio are similar to the incentive ratio defined in Bergstresser and Philippon (2006), with option values calculated using the Core and Guay (2002) one-year approximation method. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. Significance at 10%, 5% and 1% denoted by ***, **, * respectively. LR Π^2 (17)= 139.05, p-value <0.01; pseudo R-squared = 0. 06. Table reports coefficient estimates and marginal effects, with p-values in parentheses.

Table 3
Results of Multinomial Logit Regressions Comparing GAO Sample, Distinguishing Between Malfeasance and Other Restatements, to Control Firms

	Panel A: Other restatements		Panel B: Restatements resulting from malfeasance	
	Coefficient estimate	**Marginal Effect**	**Coefficient estimate**	**Marginal Effect**
Bank debt proxy	12.37	0.11	1.49	0.01
	(<0.01)	(<0.01)	(0.64)	(0.64)
Other debt	2.26	0.02	2.29	0.01
	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Board meetings	0.06	0.00	0.05	0.00
	(0.02)	(0.02)	(0.09)	(0.09)
Tenure	0.01	0.00	-0.01	0.00
	(0.26)	(0.26)	(0.55)	(0.55)
Altman's Z	0.00	0.00	0.00	0.00
	(0.36)	(0.36)	(0.27)	(0.27)
3-yr OIBD/TA	-0.35	0.00	0.78	0.00
	(0.67)	(0.67)	(0.02)	(0.02)
Sales growth	0.77	0.01	1.00	0.00
	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Age	0.01	0.00	0.02	0.00
	(0.37)	(0.37)	(<0.01)	(<0.01)
Merger indicator	-0.61	0.00	-0.56	0.00
	(0.03)	(<0.01)	(0.10)	(0.02)
Total assets	0.00	0.00	0.00	0.00
	(0.74)	(0.74)	(0.23)	(0.23)
Volatility	0.75	0.01	1.93	0.01
	(0.13)	(0.13)	(<0.01)	(<0.01)
Unrated	0.01	0.00	-0.07	0.00
	(0.97)	(0.87)	(0.78)	(0.58)
Stock ratio	0.31	0.00	0.96	0.01
	(0.49)	(0.49)	(0.06)	(0.06)
Option ratio	1.50	0.01	1.69	0.01
	(0.02)	(0.02)	(0.16)	(0.16)
Intercept	-6.49		-7.69	
	(<0.01)		(<0.01)	

"GAO sample" is the set of firms that restated their financial statements, identified by the General Accounting Office (n = 90 malfeasance; 140 other firm-years). Firms in Execucomp that are not accused of misreporting by the GAO or the SEC are included as control firms (n = 10,801 firm-years). A restatement is classified as resulting from malfeasance if the SEC or auditor prompted the restatement, or if the press release announcing the restatement includes an allegation of fraud or SEC investigation. Dependent variable takes a value of two if malfeasance is indicated or one if firm is otherwise included in the GAO restatement database. Bank debt is by debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Board meetings is the number of board meetings held during the fiscal year. CEO tenure is the observation year, less Execucomp variable BECAMECEO. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Share ratio and option ratio are similar to the incentive ratio defined in Bergstresser and Philippon (2006), with option values calculated using the Core and Guay (2002) one-year approximation method. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. LR Π^2 (38)= 168.25, p-value <0.01; pseudo R-squared = 0.07. Table reports coefficient estimates and marginal effects, with p-values in parentheses.

Table 4
Results of Logit Regressions Comparing AAER Sample to Control Firms

	Coefficient estimate	Marginal Effect
Bank debt proxy	3.20	0.01
	(0.57)	(0.57)
Other debt	2.58	0.00
	(0.04)	(0.05)
Board meetings	0.07	0.00
	(0.15)	(0.17)
Tenure	0.04	0.00
	(0.13)	(0.13)
Altman's Z	0.00	0.00
	(0.96)	(0.96)
3-yr OIBD/TA	1.71	0.00
	(0.33)	(0.33)
Sales growth	0.64	0.00
	(0.19)	(0.20)
Age	0.00	0.00
	(0.75)	(0.75)
Merger indicator	0.84	0.00
	(0.05)	(0.13)
Total assets	0.00	0.00
	(0.12)	(0.13)
Volatility	1.24	0.00
	(0.24)	(0.24)
Unrated	0.39	0.00
	(0.39)	(0.39)
Stock ratio	1.19	0.00
	(0.14)	(0.15)
Option ratio	1.95	0.00
	(0.12)	(0.12)
Intercept	-9.21	
	(<0.01)	

"AAER sample" is the set of firms that were the subject of an Accounting and Auditing Enforcement Release, identified by Johnson, Ryan and Tian (2005) (n = 45 firm-years). Firms in Execucomp that are not accused of misreporting by the GAO or the SEC are included as control firms (n = 10,801 firm-years). Bank debt is by debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Board meetings is the number of board meetings held during the fiscal year. CEO tenure is the observation year, less Execucomp variable BECAMECEO. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Share ratio and option ratio are similar to the incentive ratio defined in Bergstresser and Philippon (2006), with option values calculated using the Core and Guay (2002) one-year approximation method. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. LR Π^2 (15) = 31.29, p-value = 0.01, pseudo R-squared = 0.1045. Table reports coefficient estimates and marginal effects, with p-values in parentheses.

Table 5
Results of Logit Regressions Comparing GAO Sample, Detected Versus not Detected Misreporting

	Panel A: Detected vs. initially undetected misreporting		Panel B: Initially undetected misreporting vs. control firms	
	Coefficient estimate	**Marginal effect**	**Coefficient estimate**	**Marginal effect**
Bank debt proxy	-4.66	-1.16	11.35	0.08
	(0.10)	(0.10)	(<0.01)	(<0.01)
Other debt	-0.20	-0.05	2.56	0.02
	(0.84)	(0.84)	(<0.01)	(<0.01)
Board meetings	0.11	0.03	0.02	0.00
	(0.03)	(0.03)	(0.52)	(0.52)
Tenure	0.00	0.00	0.00	0.00
	(0.96)	(0.96)	(0.76)	(0.76)
Altman's Z	0.00	0.00	0.00	0.00
	(0.88)	(0.88)	(0.34)	(0.34)
3-yr OIBD/TA	-1.33	-0.33	-0.89	0.01
	(0.40)	(0.40)	(0.05)	(0.05)
Sales growth	-0.07	-0.02	0.89	0.01
	(0.88)	(0.88)	(<0.01)	(<0.01)
Age	-0.02	0.00	0.01	0.00
	(0.18)	(0.18)	(0.02)	(0.02)
Merger indicator	0.48	0.12	-0.81	0.00
	(0.31)	(0.29)	(0.02)	(<0.01)
Total assets	0.00	0.00	0.00	0.00
	(0.12)	(0.12)	(0.28)	(0.28)
Volatility	0.37	0.09	0.98	0.01
	(0.70)	(0.70)	(0.09)	(0.09)
Unrated	-0.10	-0.02	0.02	0.00
	(0.77)	(0.77)	(0.94)	(0.94)
Stock ratio	-0.96	-0.24	1.21	0.01
	(0.18)	(0.18)	(0.01)	(0.01)
Option ratio	2.06	0.51	1.12	0.01
	(0.11)	(0.11)	(0.15)	(0.14)
Intercept	-0.30		-6.99	
	(0.73)		(<0.01)	

"GAO sample" is the set of firms that restated their financial statements, identified by the General Accounting Office (n = 123 detected; 107 not detected firm-years). Firms in Execucomp that are not accused of misreporting by the GAO or the SEC are included as control firms (n = 10,801 firm-years). In Panel A, dependent variable takes a value of one if firm is in the GAO restatement database, and the restatement occurs within one year of misreporting. In Panel B, dependent variable takes a value of one if firm is in the GAO restatement database, and if the restatement occurs more than one year after misreporting. Bank debt is by debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Board meetings is the number of board meetings held during the fiscal year. CEO tenure is the observation year, less Execucomp variable BECAMECEO. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Share ratio and option ratio are similar to the incentive ratio defined in Bergstresser and Philippon (2006), with option values calculated using the Core and Guay (2002) one-year approximation method. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. Table reports coefficient estimates followed by marginal effects. Panel A: LR Π^2 (16)= 25.40, p-value = 0.06; pseudo R-squared = 0.08. Panel B: LR Π^2 (17)= 85.52, p-value <0.01; pseudo R-squared = 0.07. Table reports coefficient estimates and marginal effects, with p-values in parentheses.

Table 6
Results of Tobit Regressions Comparing Changes in Level of Bank Debt for Two Years Before Misreporting

	Panel A: GAO sample (*n*= 407) Coefficient Estimate	**Panel B: GAO sub-sample, malfeasance not indicated (*n*= 274) Coefficient Estimate**	**Panel C: GAO sub-sample, malfeasance indicated (*n*= 133) Coefficient Estimate**
Misreporting indicator variable	0.19	0.28	0.02
	(<0.01)	(<0.01)	(0.82)
Age of firm	0.00	0.00	0.00
	(<0.01)	(<0.01)	(<0.01)
Total Assets	0.00	0.00	0.00
	(0.12)	(0.13)	(0.26)
Market-to-Book	0.00	0.00	0.00
	(0.68)	(0.74)	(0.76)
Fixed asset ratio	0.02	0.02	0.02
	(0.69)	(0.73)	(0.68)
Leverage	-0.05	-0.06	-0.05
	(0.48)	(0.43)	(0.52)
Unrated	-0.04	-0.04	-0.04
	(0.07)	(0.05)	(0.08)
Intercept	0.00	0.00	0.00
	(0.99)	(0.99)	(0.98)
LR Π^2 (8)	85.70	92.86	70.27
	(0.00)	(<0.01)	(<0.01)

"GAO sample" is the set of firms that restated their financial statements, identified by the General Accounting Office. Firms in Execucomp that are not accused of misreporting by the GAO or the SEC are included as control firms (n = 13,553 firm-years). A restatement is classified as resulting from malfeasance if the SEC or auditor prompted the restatement, or if the press release announcing the restatement includes an allegation of fraud or SEC investigation. The dependent variable is Change in bank debt, which is the percentage change in the bank debt proxy in the two years before or after misreporting, where bank debt is proxied as (debt in current liabilities less the current portion of long-term debt, scaled by total assets). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Market-to-Book is the ratio of (book value of assets minus book value of equity plus market value of equity) to book value of assets, averaged over the preceding five years. Fixed asset ratio is net property, plant and equipment divided by total assets, averaged over the preceding five years. Leverage is an instrumental variable described in Johnson (1997). Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. Table reports coefficient estimates and marginal effects, with *p*-values in parentheses.

Table 7
Results of Tobit Regressions Comparing Changes in Level of Bank Debt for Two Years After Misreporting

	Panel A: GAO sample (*n*= 253) Coefficient Estimate	**Panel B: GAO sub-sample, malfeasance not indicated (*n*= 171) Coefficient Estimate**	**Panel C: GAO sub-sample, malfeasance indicated (*n*= 82) Coefficient Estimate**
Misreporting indicator variable	-0.04	-0.01	-0.09
	(0.62)	(0.89)	(0.49)
Age of firm	0.00	0.00	0.00
	(<0.01)	(<0.01)	(<0.01)
Total Assets	0.00	0.00	0.00
	(0.26)	(0.27)	(0.26)
Market-to-Book	0.00	0.00	0.00
	(0.74)	(0.76)	(0.80)
Fixed asset ratio	0.02	0.03	0.01
	(0.62)	(0.58)	(0.75)
Leverage	-0.03	-0.03	-0.05
	(0.68)	(0.64)	(0.52)
Unrated	-0.03	-0.03	-0.04
	(0.12)	(0.12)	(0.06)
Intercept	0.00	-0.01	0.00
	(0.91)	(0.89)	(0.98)
LR Π^2 (8)	69.50	67.98	74.66
	(<0.01)	(<0.01)	(<0.01)

"GAO sample" is the set of firms that restated their financial statements, identified by the General Accounting Office. Firms in Execucomp that are not accused of misreporting by the GAO or the SEC are included as control firms (n = 13,553 firm-years) A restatement is classified as resulting from malfeasance if the SEC or auditor prompted the restatement, or if the press release announcing the restatement includes an allegation of fraud or SEC investigation. The dependent variable is Change in bank debt, which is the percentage change in the bank debt proxy in the two years before or after misreporting, where bank debt is proxied as (debt in current liabilities less the current portion of long-term debt, scaled by total assets). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Market-to-Book is the ratio of (book value of assets minus book value of equity plus market value of equity) to book value of assets, averaged over the preceding five years. Fixed asset ratio is net property, plant and equipment divided by total assets, averaged over the preceding five years. Leverage is an instrumental variable described in Johnson (1997). Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. Table reports coefficient estimates, with *p*-values in parentheses.

Table 8
Robustness - Results of Conditional Logit Regressions Comparing GAO Sample to Control Firms Using Hand-Collected Bank Debt Data

	Coefficient estimate
Bank debt	6.51
	(<0.01)
Other debt	2.38
	(0.14)
Board meetings	0.07
	(0.29)
Tenure	-0.08
	(0.01)
Altman's Z	0.01
	(0.19)
3-yr OIBD/TA	4.12
	(0.08)
Sales growth	-0.08
	(0.90)
Age	0.02
	(0.14)
Merger indicator	1.04
	(0.04)
Total assets	0.00
	(0.23)
Volatility	0.50
	(0.67)
Unrated	1.31
	(0.02)
Stock ratio	1.13
	(0.23)
Option ratio	-0.29
	(0.86)
Percent shareholdings, outside directors	6.57
	0.38
Percent inside directors	-1.87
	0.23

"GAO sample" is the set of firms that restated their financial statements, identified by the General Accounting Office. Sample includes 104 firms from GAO sample and 104 control firms matched on industry and size. Matched control firms are selected from those firms in Execucomp that are not accused of misreporting by the GAO or the SEC. Dependent variable takes a value of one if firm is in the GAO restatement database. Bank debt proxy is hand collected from Moody's Industrial Manuals and 10-Ks, scaled by total assets. Other debt is total debt from Compustat less bank debt, scaled by total assets. Board meetings is the number of board meetings held during the fiscal year. CEO tenure is the observation year, less Execucomp variable BECAMECEO. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Share ratio and option ratio are similar to the incentive ratio defined in Bergstresser and Philippon (2006), with option values calculated using the Core and Guay (2002) one-year approximation method. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. LR Π^2 (14)= 31.34, *p*-value = 0.01; pseudo R-squared = 0. 22. Table reports coefficient estimates, with *p*-values in parentheses.

Table 9
Robustness - Results of OLS Regressions Examining Relations Between Accruals and Bank Borrowing

	Coefficient estimate
Bank debt proxy	0.20
	(<0.01)
Other debt	-0.01
	(0.19)
Board meetings	0.00
	(<0.01)
Tenure	0.00
	(<0.01)
Altman's Z	0.00
	(0.06)
3-yr OIBD/TA	0.03
	(<0.01)
Sales growth	-0.01
	(0.06)
Age	0.00
	(0.05)
Merger indicator	-0.01
	(0.02)
Total assets	0.00
	(0.02)
Volatility	-0.05
	(<0.01)
Unrated	0.00
	(0.85)
Stock ratio	-0.02
	(<0.01)
Option ratio	-0.03
	(0.02)
Intercept	-0.03
	(<0.01)

Dependent variable is a measure of accruals as defined in Hribar and Collins (2002). Sample in includes all Execucomp firms except financials (n = 14,488). Bank debt is by debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Board meetings is the number of board meetings held during the fiscal year. CEO tenure is the observation year, less Execucomp variable BECAMECEO. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Share ratio and option ratio are similar to the incentive ratio defined in Bergstresser and Philippon (2006), with option values calculated using the Core and Guay (2002) one-year approximation method. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. F-value 15.27, p-value <0.01. Table reports coefficient estimates, with p-values in parentheses.

Table 10
Summary Statistics

	Mean	**Standard deviation**	**Minimum**	*Median*	*Maximum*
Panel A: ExecuComp sample					
Change in SH wealth	$595,051.03	$1,469,953.39	-$1,525,408.72	$161,627.83	$5,145,502.00
Change in Total Pay	$248.47	$884.93	-$1,454.69	$85.00	$2,669.09
Change in CEO wealth	$2,437.26	$1,998.95	$311.67	$1,792.04	$10,251.58
Change in CEO pay-related wealth	$6,246.71	$10,850.79	-$14,736.59	$2,873.96	$47,751.46
Change in all forms of compensation	$16,852.73	$70,986.47	-$267,946.77	$4,847.55	$900,198.58
Ownership %	2.435%	4.440%	0.016%	0.442 %	16.600%
Performance net of VW market return	7.33%	234.28%	-146.35%	4.38%	149.21%
Performance net of EW market return	-9.89%	195.19%	-187.35%	-10.64%	127.21%
Performance net of beta-matched portfolio	-0.72%	249.62%	-147.61%	-2.64%	108.83%
Panel B: Compustat sample					
Performance	0.50%	0.99%	-4.99%	0.44%	23.54%
Performance net of EW market return	0.40%	0.21%	-0.19%	0.45%	0.97%
Performance net of VW market return	0.32%	0.32%	-0.46%	0.35%	0.79%
Performance net of value-weighted industry-matched portfolio	-0.13%	0.96%	-5.91%	-0.09%	22.29%
Performance net of size- and book-to-market-matched portfolio	0.00%	0.93%	-5.61%	-0.02%	20.81%
Performance net of size-, book-to-market, and industry-matched portfolio	0.00%	0.86%	-4.64%	-0.01%	21.75%

All dollar amounts are in thousands (except performance measures, which are in millions) and are adjusted for inflation to 2000 constant dollars.

Table 11
Estimates of Pay-Performance Sensitivity: Coefficients of Ordinary Least Squares Regressions of Changes in Compensation on Current and Lagged Changes in Shareholder Wealth

	Change in Total Pay	Change in CEO wealth	Change in CEO pay-related wealth	Change in all forms of compensation
Intercept	165.84***	1985.70***	3511.99***	3514.37***
Change in SH wealth	0.0001***	0.0004***	0.0038***	0.0039***
Change in SH wealth, year t-1	0.0000	0.0004***	0.0009***	0.0009***
CEO's fractional ownership * change in shareholder wealth				0.9932***
Estimated PPS	$0.1000	$0.8000	$4.7000	$9.1899
Adjusted R-squared	0.0512	0.2018	0.3079	0.9839

All dollar amounts are in thousands, and are adjusted for inflation to 2000 constant dollars. The sample is all CEOs in ExecuComp 1993-2004 (n = 10,926). The change in shareholder wealth is defined as firm value at the end of the prior year times the rate of return on common stock. The change in total pay is the change in salary and bonus. The change in CEO wealth is salary plus bonus, plus the present value of the change in salary plus bonus (assuming a 3% real interest rate) through age 70. The change in CEO pay-related wealth adds the change in the value of stock options to the change in CEO wealth. The change in all forms of compensation adds the change in the value of inside stock holdings to the change in CEO pay-related wealth. Pay-performance sensitivity for changes in all forms of compensation is calculated using median stock holdings of 0.442%. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 12
Relation between CEO Turnover and Firm Performance: Estimated Logistic Models Predicting CEO Turnover Using Current and Lagged Net-of-Market Shareholder Return

	Intercept	Coefficient on current performance	Coefficient on performance in prior year	PPS	Likelihood Ratio
Panel A: All CEO turnovers (n=871 for beta-matched, 1,261 otherwise)					
Performance net of VW market return	-2.1318***	0.0068	-0.6614***	$0.0128 $0.0378	92.94***
Performance net of EW market return	-2.2710***	-0.2955***	-0.6017***	$0.0166 $0.0489	198.70***
Performance net of beta-matched portfolio	-2.1986***	0.0033	-0.8105***	$0.0155 $0.0457	65.20***
Panel B: Parrino's forced (n=226 for beta-matched, 353 otherwise)					
Performance net of VW market return	-3.5126***	-0.2940***	-0.9640***	$0.0087 $0.0258	76.27***
Performance net of EW market return	-3.7550***	-0.5290***	-1.0492***	$0.0095 $0.0280	133.36***
Performance net of beta-matched portfolio	-3.7840***	-0.5851***	-1.3459***	$0.0124 $0.0366	98.71***
Panel C: Fired (n=112 for beta-matched, 154 otherwise)					
Performance net of VW market return	-4.5146***	-0.4801***	-1.6952***	$0.0075 $0.0221	123.32***
Performance net of EW market return	-4.9039***	-0.5140***	-1.7668***	$0.0056 $0.0164	145.58***
Performance net of beta-matched portfolio	-4.6059***	-0.4506***	-1.8789***	$0.0077 $0.0227	79.42***

The dependent variable takes the value of one if turnover occurred, and zero otherwise. Performance is firm value at the end of the prior year times the rate of return on the firm's common stock, less some measure of market performance. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Pay-performance sensitivities are for change in salary only, and change in all forms of compensation. The sample is all CEOs in ExecuComp. Sample size = 8,415 for beta-matched, and 12,071 otherwise.

Table 13
Relation between Advancement in Managerial Labor Market and Firm Performance: Estimated Logit Models Predicting CEO Promotions Using Buy-and-Hold Stock Returns and Pay-Performance Sensitivity from CEO Promotions

	Coefficient on current performance	PPS	Likelihood Ratio
Panel A: all jumps (n = 51)			
Performance	-0.0338	$(0.0014) $(0.0041)	0.05
Performance net of EW market return	0.8635	$0.0407 $0.1202	1.51
Performance net of VW market return	0.8298*	$0.0405 $0.1195	3.02*
Performance net of VW industry-matched portfolio	-0.0044	$(0.0002) $(0.0006)	0.00
Performance net of size- and book-to-market-matched portfolio	0.0278	$0.0012 $0.0036	0.03
Performance net of size, book-to-market-, and industry-matched portfolio	0.0133	$0.0006 $0.0017	0.01
Panel B: raids only (n = 27)			
Performance	0.0906	$0.0027 $0.0085	0.24
Performance net of EW market return	1.8113*	$0.0603 $0.1867	3.14*
Performance net of VW market return	0.4138	$0.0124 $0.0384	0.43
Performance net of VW industry-matched portfolio	0.0525	$0.0015 $0.0047	0.07
Performance net of size- and book-to-market-matched portfolio	0.0635	$0.0019 $0.0058	0.10
Performance net of size, book-to-market-, and industry-matched portfolio	0.0730	$0.0021 $0.0067	0.11

The dependent variable takes the value of one if the CEO is promoted, and zero otherwise. Performance is the buy-and-hold stock returns measured 33 months before the event, less some measure of market performance. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Pay-performance sensitivities are for change in salary only, and change in all forms of compensation. The sample is all CEOs in Compustat. Sample size = 69,670.

Table 14
Coefficients of Piecewise Regressions of Changes in Compensation on Current and Lagged Changes in Shareholder Wealth; Quintiles by Current Performance

	Change in Total Pay	Change in CEO Wealth	Change in CEO Pay Related Wealth	Change in all Forms of Compensation
Intercept	462.60***	5,888.47***	10,256.31***	10,283.75***
Δ SH wealth, bottom quintile	0.0001**	0.0012***	0.0015***	0.0016***
Δ SH wealth, 2nd quintile	0.0001***	0.0010***	0.0026***	0.0026***
Δ SH wealth, 3rd quintile	0.0001***	0.0009***	0.0028***	0.0029***
Δ SH wealth, 4th quintile	0.0001***	0.0009***	0.0037***	0.0037***
Δ SH wealth, top quintile	0.0001***	0.0006***	0.0027***	0.0028***
CDF of variance, bottom quintile	-108.1222	-1,327.79	-855.07	-813.75
CDF of variance, 2nd quintile	-2.1433	-1,315.40	-1,474.32	-1,576.50
CDF of variance, 3rd quintile	-25.0906	-976.50	-959.88	-1,018.33
CDF of variance, 4th quintile	-97.5780	-1,669.48**	-4,160.64**	-4,162.49**
CDF of variance, top quintile	-129.34	-2,501.36***	-6,141.16***	-6,159.28***
Δ SH wealth * CDF, bottom quintile	0.0001	-0.0000	0.0048***	0.0046***
Δ SH wealth * CDF, 2nd quintile	-0.0000	0.0002	0.0037***	0.0042***
Δ SH wealth * CDF, 3rd quintile	0.0001**	0.0007*	0.0049***	0.0053***
Δ SH wealth * CDF, 4th quintile	-0.0000	0.0002	0.0032***	0.0033***
Δ SH wealth * CDF, top quintile	-0.0000	0.0004	0.0035***	0.0038***
Δ SH wealth, year t-1 bottom quintile	0.0000	-0.0000	0.0000	0.0000
Δ SH wealth, year t-1 2nd quintile	-0.0000	-0.0001	-0.001	-0.001
Δ SH wealth, year t-1 3rd quintile	0.0000*	0.0001	0.0003***	0.0003***
Δ SH wealth, year t-1 4th quintile	0.0000	0.0000	0.0001	0.0001
Δ SH wealth, year t-1 top quintile	0.0000	0.0000***	0.0001***	0.0001***
CEO's fractional ownership * Δ shareholder wealth, bottom quintile				1.0082***
CEO's fractional ownership * Δ shareholder wealth, 2nd quintile				0.9849***
CEO's fractional ownership * Δ shareholder wealth, 3rd quintile				0.9894***
CEO's fractional ownership * Δ shareholder wealth, 4th quintile				0.9958***
CEO's fractional ownership * Δ shareholder wealth, 5th quintile				0.9898***
R-squared	0.0656	0.1622	0.3566	0.9786

All dollar amounts are in thousands, and are adjusted for inflation to 2000 constant dollars. The sample is all CEOs in ExecuComp 1993-2004 (n = 10,926). The change in shareholder wealth is defined as firm value at the end of the prior year times the rate of return on common stock. The change in total pay is the change in salary and bonus. The change in CEO wealth is salary plus bonus, plus the present value of the change in salary plus bonus (assuming a 3% real interest rate) through age 70. The change in CEO pay-related wealth adds the change in the value of stock options to the change in CEO wealth. The change in all forms of compensation adds the change in the value of inside stock holdings to the change in CEO pay-related wealth. Pay-performance sensitivity for changes in all forms of compensation is calculated using median stock holdings of 0.442%. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 15
Estimates of Pay-Performance Sensitivity Based on Coefficients of Piecewise Regressions of Changes in Compensation on Current and Lagged Changes in Shareholder Wealth; Quintiles by Current Performance

	Change in Total Pay	Change in CEO wealth	Change in CEO pay-related wealth	Change in all forms of compensation
Δ SH wealth, bottom quintile	$ 0.14	$ 1.14	$ 3.95	$ 8.33
Δ SH wealth, 2nd quintile	$ 0.12	$ 0.97	$ 4.30	$ 8.93
Δ SH wealth, 3rd quintile	$ 0.19	$ 1.31	$ 5.59	$ 10.19
Δ SH wealth, 4th quintile	$ 0.13	$ 0.99	$ 5.36	$ 9.81
Δ SH wealth, top quintile	$ 0.12	$ 0.85	$ 4.59	$ 9.20

All dollar amounts are in thousands, and are adjusted for inflation to 2000 constant dollars. The sample is all CEOs in ExecuComp 1993-2004 (n = 10,926). The change in shareholder wealth is defined as firm value at the end of the prior year times the rate of return on common stock. The change in total pay is the change in salary and bonus. The change in CEO wealth is salary plus bonus, plus the present value of the change in salary plus bonus (assuming a 3% real interest rate) through age 70. The change in CEO pay-related wealth adds the change in the value of stock options to the change in CEO wealth. The change in all forms of compensation adds the change in the value of inside stock holdings to the change in CEO pay-related wealth. Pay-performance sensitivity for changes in all forms of compensation is calculated using median stock holdings of 0.442%. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Table 16
Relation between CEO Turnover and Firm Performance: Estimated Log-Log Models Predicting CEO Turnover Using Current and Lagged Net-of-Market Shareholder Return

	Intercept	Coefficient on Current Performance	Coefficient on CDF of Variance	Coefficient on CDF * Performance	Coefficient on Performance in Prior Year	Likelihood Ratio
Panel A: All CEO departures (n= 871 for beta-matched, 1,261 otherwise)						
Performance net of value-weighted market return	2.4530***	-1.1839***	0.4679***	1.3974***	-0.6107***	186.56***
Performance net of equal-weighted market return	2.8910***	2.0700***	0.8329***	2.4375***	-0.5818***	390.77***
Performance net of beta-matched portfolio	2.5594***	-2.1901***	0.3985***	2.3415***	-0.7563***	238.49***
Panel B: Parrino's forced(n= 226 for beta-matched, 353 otherwise)						
Performance net of value-weighted market return	4.7487***	-1.4290***	2.0788***	1.6003***	-0.8225***	222.17***
Performance net of equal-weighted market return	4.9804***	-1.1333***	2.1175***	1.1780***	-0.8813***	235.18***
Performance net of beta-matched portfolio	4.8576***	-2.0675***	1.8233***	2.1997***	-1.0818***	187.15***
Panel C: Fired(n= 112 for beta-matched, 154 otherwise)						
Performance net of value-weighted market return	5.4434***	-1.3297***	1.6671***	1.3173***	-1.4290***	166.89***
Performance net of equal-weighted market return	5.7354***	-1.1197***	1.5727***	1.0703***	-1.5435***	177.65***
Performance net of beta-matched portfolio	5.7342***	-2.1698***	1.8920***	2.3108***	-1.5010***	136.16***

The dependent variable takes the value of one if turnover occurred, and zero otherwise. Performance is firm value at the end of the prior year times the rate of return on the firm's common stock, less some measure of market performance. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. The sample is all CEOs in ExecuComp. Sample size = 8,415 for beta-matched, and 12,071 otherwise.

Table 17
Relation between CEO Turnover and Firm Performance: Predicted Probabilities and Pay-Performance Sensitivities of Estimated Log-Log Models Predicting CEO Turnover Using Current and Lagged Net-of-Market Shareholder Return

	Bottom Quintile	Middle Quintile	Top Quintile
Panel A: All CEO departures (n= 871 for beta-matched, 1,261 otherwise)			
Performance net of value-weighted market return	18.00%	9.78%	4.61%
	$0.1014	$0.0852	$0.0441
	$0.3101	$0.2608	$0.1350
Performance net of equal-weighted market return	21.60%	9.34%	3.52%
	$0.1465	$0.1108	$0.0523
	$0.4483	$0.3388	$0.1602
Performance net of beta-matched portfolio	22.30%	9.34%	3.30%
	$0.1872	$0.1378	$0.0582
	$0.5726	$0.4216	$0.1780
Panel B: Parrino's forced(n= 226 for beta-matched, 353 otherwise)			
Performance net of value-weighted market return	5.28%	2.25%	0.81%
	$0.0385	$0.0281	$0.0117
	$0.1178	$0.0861	$0.0359
Performance net of equal-weighted market return	5.26%	2.28%	0.87%
	$0.0354	$0.0279	$0.0124
	$0.1082	$0.0854	$0.0380
Performance net of beta-matched portfolio	5.68%	2.00%	0.61%
	$0.0542	$0.0363	$0.0128
	$0.1658	$0.1112	$0.0391
Panel C: Fired(n= 112 for beta-matched, 154 otherwise)			
Performance net of value-weighted market return	3.00%	0.89%	0.21%
	$0.0278	$0.0171	$0.0051
	$0.0850	$0.0523	$0.0155
Performance net of equal-weighted market return	3.01%	0.89%	0.21%
	$0.0261	$0.0173	$0.0054
	$0.0797	$0.0530	$0.0165
Performance net of beta-matched portfolio	3.11%	0.87%	0.21%
	$0.0337	$0.0203	$0.0058
	$0.1030	$0.0620	$0.0177

The dependent variable takes the value of one if turnover occurred, and zero otherwise. Performance is firm value at the end of the prior year times the rate of return on the firm's common stock, less some measure of market performance. Table reports predicted probability of turnover, followed by pay-performance sensitivities for change in salary only, and change in all forms of compensation. The sample is all CEOs in ExecuComp. Sample size = 8,415 for beta-matched, and 12,071 otherwise.

Table 18
Relation between Advancement in Managerial Labor Market and Firm Performance: Estimated Complementary Log-Log Models Predicting CEO Promotions Using Buy-and-Hold Stock Returns

	Intercept	Coefficient on current performance	Coefficient on CDF of variance	Coefficient on CDF * performance	Likelihood Ratio
Panel A: all jumps (n = 51)					
Performance	-6.3651***	0.3123	-2.3095***	-0.4807	23.52***
Performance net of EW market return	-6.2002***	0.0317	-2.6145***	-0.0800	24.51***
Performance net of VW market return	-6.1687***	0.0338	-2.7119***	-0.1328	26.35***
Performance net of VW industry-matched portfolio	-7.1203***	0.1869	-0.2561	-0.3025	0.46
Performance net of size- and book-to-market-matched portfolio	-6.1213***	0.1337	-2.8915***	-0.1788	29.02***
Performance net of size-, book-to-market-, and industry-matched portfolio	-6.3586***	0.0904	-2.1242***	-0.1155	16.94***
Panel B: raids only (n = 27)					
Performance	-7.0043***	0.5029	-2.6197***	-0.3871	15.45***
Performance net of EW market return	-6.7287***	0.0175	-2.9591***	0.2271	15.83***
Performance net of VW market return	-6.7048***	0.3554	-3.1453***	-0.1739	17.79***
Performance net of VW industry-matched portfolio	-7.5897***	0.3931	-0.5780	-0.5555	1.17
Performance net of size- and book-to-market-matched portfolio	-6.5961***	0.3054	-3.4306***	-0.4149	20.96***
Performance net of size-, book-to-market-, and industry-matched portfolio	-6.6723***	0.5007	-3.1566***	-0.6591	18.60***

The dependent variable takes the value of one if the CEO is promoted, and zero otherwise. Performance is the buy-and-hold stock returns measured 33 months before the event, less some measure of market performance. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. The sample is all CEOs in Compustat. Sample size = 69,670.

Table 19
Relation between CEO Jumps and Firm Performance: Predicted Probabilities and Pay-Performance Sensitivities by Quintile Based on Complementary Log-log Results

	Bottom Quintile	Middle Quintile	Top Quintile
Panel A: all jumps (n = 51)			
Performance	0.05%	0.06%	0.06%
	$0.0000	$0.0000	$0.0000
	$0.0001	$0.0001	$0.0001
Performance net of EW market return	0.06%	0.05%	0.05%
	$0.0000	$0.0000	$0.0000
	$0.0000	$0.0000	$0.0000
Performance net of VW market return	0.06%	0.05%	0.05%
	$0.0000	$0.0000	$0.0000
	$0.0000	$0.0000	$0.0000
Performance net of VW industry-matched portfolio	0.07%	0.07%	0.07%
	$0.0000	$0.0000	$0.0000
	$0.0000	$0.0000	$0.0000
Performance net of size- and book-to-market-matched portfolio	0.05%	0.05%	0.05%
	$0.0000	$0.0000	$0.0000
	$0.0000	$0.0000	$0.0000
Performance net of size-, book-to-market-, and industry-matched portfolio	0.06%	0.06%	0.06%
	$0.0000	$0.0000	$0.0000
	$0.0000	$0.0000	$0.0000
Panel B: raids only (n = 27)			
Performance	0.02%	0.03%	0.04%
	$0.0001	$0.0001	$0.0001
	$0.0002	$0.0002	$0.0003
Performance net of EW market return	0.02%	0.03%	0.03%
	$0.0000	$0.0000	$0.0000
	$0.0001	$0.0001	$0.0001
Performance net of VW market return	0.02%	0.03%	0.04%
	$0.0000	$0.0000	$0.0001
	$0.0001	$0.0002	$0.0002
Performance net of VW industry-matched portfolio	0.03%	0.04%	0.04%
	$0.0000	$0.0000	$0.0000
	$0.0001	$0.0001	$0.0001
Performance net of size- and book-to-market-matched portfolio	0.02%	0.02%	0.03%
	$0.0000	$0.0000	$0.0000
	$0.0001	$0.0001	$0.0001
Performance net of size-, book-to-market-, and industry-matched portfolio	0.02%	0.03%	0.03%
	$0.0000	$0.0000	$0.0000
	$0.0001	$0.0001	$0.0001

The dependent variable takes the value of one if the CEO is promoted, and zero otherwise. Performance is the buy-and-hold stock returns measured 33 months before the event, less some measure of market performance. Table reports predicted probability of promotion, followed by pay-performance sensitivities for change in salary only, and change in all forms of compensation. The sample is all CEOs in Compustat. Sample size = 69,670.

Table 20
Total Pay-Performance Sensitivities, Including all Three Sources of Incentives, Based on Estimates from Linear Versus Nonlinear Models

	Linear Models	**Non-linear models**		
		Bottom quintile	**Middle quintile**	**Top quintile**
Threat of dismissal	\$0.0280	\$0.1082	\$0.0854	\$0.0380
	0.30%	1.28%	0.83%	0.41%
Changes in own-firm pay	\$9.1899	\$8.3264	\$10.1929	\$9.2021
	97.72%	98.72%	99.17%	99.59%
Opportunity to advance	\$0.1867	\$0.0001	\$0.0001	\$0.0001
	1.99%	0.00%	0.00%	0.00%
Total	\$9.4046	\$8.4347	\$10.2784	\$9.2402

In each case the threat of dismissal is as defined in Parrino (1997) and the opportunity to advance is to be raided as in Fee and Hadlock (2003). Table reports pay-performance sensitivities and the percent that each component contributes to the total. When results are for net-of-market performance, market performance is the CRSP equal-weighted average.

Table 21
Information on Future Employment of Departing CEOs

Die or have health problems	48
Return to CEO post later	15
Take a different job at the same firm	25
Take a non-corporate job (university, government, non-profit etc.)	17
Start own company, consultant, VC, major league sports	11
Take a job with another corporation	127
No news concerning future employment	294

Table reports information from news announcements regard future employment of CEOs in the two years after they resign. Included are only those CEOs that are not forced according to Parrino (1997) taxonomy, and who are under age 62.

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