TWO ESSAYS IN CORPORATE FINANCE

by

GEOFFREY PETER SMITH

(Under the Direction of James Linck)

ABSTRACT

This dissertation is comprised of two essays. In the first essay, I study how systematic risk and managerial ability affect the relation between chief executive officer compensation and peer firm stock returns (commonly referred to as relative performance evaluation or RPE). Based on a sample of 323 executives from 1996 to 2000, I find that peer firm stock returns have a lesser impact on executive pay when: (i) the firm's stock return sensitivity to systematic risk is low, and (ii) the executive's managerial ability level is high. These findings suggest that firm- and executive- specific characteristics can counteract the incentive effects of peer-return-based performance criteria. In the second essay, I study the response of cross-listed firms to the Sarbanes-Oxley Act of 2002 (SOX). I find negative and significant cumulative abnormal returns around SOX announcement dates for my sample of 424 cross-listed firms; firms from countries with low-quality regulatory systems responded less negatively than firms from countries with high-quality systems. I also find a significant downturn in cross-listing activity and that voluntary de-listings increased, post-SOX. Low profitability and low US market liquidity also increased the likelihood of a post-SOX de-listing.

INDEX WORDS: Executive compensation; Relative performance evaluation; Sarbanes-Oxley Act of 2002; Cross-listing

TWO ESSAYS IN CORPORATE FINANCE

by

GEOFFREY PETER SMITH

BS, Bradley University, 1989

MBA, DePaul University, 1992

A Dissertation Submitted to the Graduate Faculty of The University of Georgia in Partial Fulfillment of the Requirements for the Degree

DOCTOR OF PHILOSOPHY

ATHENS, GEORGIA

2006

TWO ESSAYS IN CORPORATE FINANCE

by

GEOFFREY PETER SMITH

Major Professor: James Linck

Committee: Jeffry Netter
Chris Stivers

Electronic Version Approved:

Maureen Grasso
Dean of the Graduate School
The University of Georgia
August 2006

DEDICATION

To my wife and daughter,

Krisanne and Mallory Smith

ACKNOWLEDGEMENTS

I thank The University of Georgia for the opportunity to continue my education. I also thank the Terry College of Business faculty for teaching me how to do research in finance and my Doctoral Advisory Committee for their helpful comments on this Dissertation.

TABLE OF CONTENTS

CHAPTER 1

INTRODUCTION

This dissertation is comprised of two essays. In the first essay, I study how systematic risk and managerial ability affect relative-performance-based executive compensation. Discord between compensation theory and empirical test results motivate the study. Specifically, the theory suggests that peer-based job performance benchmarking reduces agency costs and improves incentives. Yet empirical tests yield mixed results regarding the predominance of such practice. In response, researchers have proposed and tested solutions to what has come to be known as the Relative Performance Evaluation (RPE) Puzzle. My study contributes to this literature by examining how systematic risk and managerial ability affect the relation between chief executive officer compensation and peer firm stock returns. I find that peer firm stock returns have a lesser impact on executive pay when: (i) the firm's stock return sensitivity to systematic risk is low, and (ii) the executive's managerial ability level is high. These findings suggest that firm and executive characteristics can counteract the incentive effects of peer-based performance criteria and help explain why the use of such benchmarking is not as widespread as the theory suggests it should be.

In the second essay, I study the response of cross-listed firms to the Sarbanes-Oxley Act of 2002 (SOX). The study is motivated by interest in both the differential impact of the Act on various subsets of US-listed firms and interest in the reasons why non-US firms cross-list on US stock exchanges. The study is divided into two parts. In the first part, I study cross-listed firm stock returns around SOX announcement dates. In the second part, I study non-US-firm cross-listing/de-listing activity in the period immediately surrounding the Act's enactment. Using both home-country and US market indices as benchmarks, I find negative and significant cumulative abnormal returns around SOX announcement dates; firms from countries with lower-quality regulatory systems responded less negatively than firms from countries with higher-quality systems. I also find a significant decline in post-SOX cross-listing activity and that, while there was no change in overall de-listing activity, voluntary de-listings increased, and involuntarily de-listings decreased. Low profitability and low US market liquidity also increased the likelihood of a post-SOX de-listing. These results suggest that the costs of complying with US regulations are a disincentive to cross-list — helping explain why some non-US firms choose not to list on the major US stock exchanges.

CHAPTER 2

LITERATURE REVIEW

How do Systematic Risk and Managerial Ability Affect Relative-Performance-Based Compensation?

Several notable theoretical essays solve the problem of executive (agent) incentive maximization when external factors impair the performance criteria [Holmstrom (1979, 1982), Holmstrom and Milgrom (1987)]. According to Holmstrom (1979, p.87), incentives are maximized when compensation agreements use "any informative signal, regardless of how noisy it is" to refine performance criteria. Peer firms stock returns are one such informative signal. In fact, the idea that peer-return-based performance benchmarking improves incentives is so entrenched in the literature that researchers generally refer to it by shorthand as relative performance evaluation (RPE).

What is puzzling to researchers is that firms apparently make little use of peer-return-based performance benchmarks. According to Murphy (1999, p.40) , "the paucity of RPE in options and other components of executive compensation remains a puzzle worth understanding." Abowd and Kaplan (1999, p.157) concur, stating that, "We therefore conclude that, despite the obvious attractive features of relative performance evaluation, it is surprisingly absent from US executive compensation practices."

The surprising results of the Towers Perrin 1997 *Annual Incentive Plan Design Survey* confirm these observations. The survey results indicate that only 21% of industrial firms, 57% of finance/insurance firms, and 42% of utilities make explicit make use RPE. Empirical studies also yield similarly curious results. Barro and Barro (1990) find no evidence of RPE in a sample of 83 bank CEO's from 1982 to 1987. Antle and Smith (1986) find weak evidence of RPE based on accounting data, but do not find evidence relating compensation to peer firm stock returns. On the other hand, Coughlan and Schmidt (1985) and Main (1991) find that changes in executive compensation are positively related to abnormal stock returns (based on a market model), while Gibbons and Murphy (1990) find strong support for the presence of RPE in their sample of 2,214 executives from 1974 to 1986.

In response to these findings, researchers have proposed and tested several alternative solutions to this "RPE puzzle." Aggarwal and Samwick (1999) argue that RPE undermines the price support firms elicit through tacit collusion within oligopolistic product markets. In support, they find that the impact of peer returns on executive pay is increasing in the level of product market competition. Garvey and Milbourn (2003) attribute the RPE puzzle to cross-sectional differences in executives' ability to shield their personal incomes from systematic risk. In support, they find that the RPE relation is stronger for executives who lack the wherewithal to hedge risk within their personal investment accounts. Bizjak, Lemmon, and Naveen (2004) attribute the RPE puzzle to the ubiquitous practice of compensation benchmarking — setting executive pay relative to the pay levels in comparable firms without regard to relative performance.

An alternative line of reasoning suggests that the RPE puzzle is attributable to executives' undue influence over the board of directors. Proponents of the *management power hypothesis* argue that biases in the board member selection process preclude the possibility of arm's length contract negotiations between executives and boards [Bebchuk, Fried, and Walker (2002)]. Such biases arise because about one-third of board nominating committees accept the CEO as a member [Shivdasani and Yermack (1999)]. And even when the CEO does not formally sit on the board nominating committee, the CEO exerts considerable influence on the board member nomination process [Main (1991)]. Moreover, the personal costs to a board member from speaking up can outweigh the personal benefits; board members themselves frequently have little financial stake in the firms that they preside over [Baker, Jensen, and Murphy (1988)]. Board members are also quite complaisant when it comes to negotiating the terms of executive compensation agreements [Blanchard, Lopez-de-Silanes, and Shleifer (1994), Chance, Kumar, and Todd (2000), Core, Holthausen, and Larcker (1999)].

Nevertheless, I take the *optimal contracting* point of view. This point of view suggests that boards develop compensation agreements that minimize agency costs and maximize incentives to create shareholder wealth. I take this point of view for two reasons. First, this approach is consistent with several theoretical models of executive compensation [Grossman and Hart (1983), Holmstrom (1979, 1982), Mirrlees (1976), Mookherjee (1984), Ross (1973), Shavell (1979)]. It also forms the basis of the empirical pay-to-peer-performance analyses in Aggarwal and Samwick (1999) and Garvey and Milbourn (2003). Second, the management power hypothesis does not explain *why* executives disfavor relative-performance-based compensation. In reality, risk-averse executives probably *prefer* relative-performance-based compensation because it reduces the variability of their expected incomes [Holmstrom (1979), Lazear and Rosen (1981)].

Under the optimal contracting assumption, solving the RPE puzzle requires identification of market, firm, or executive characteristics that make relative performance benchmarking more or less compatible with the goals of agency cost minimization and incentive maximization. I identify two such characteristics: (i) the firm's stock price sensitivity to systematic risk, and (ii) the executive's managerial ability level.

I base my selection of the firm's stock price sensitivity to systematic risk as a relevant characteristic on the analyses in Holmstrom (1979) and Lazear and Rosen (1981). Both papers demonstrate how systematic risk factors diminish the incentives of risk-averse executives by increasing the variability of their expected incomes. Eliminating systematic risk factors through peer-return-based performance benchmarking improves incentives by reducing such variability. This suggests that the greater the firm's stock price sensitivity to systematic risk, the more valuable it is to eliminate the effects of such risk on executive pay. I refer to tests of this idea as tests of the *noisy evaluation hypothesis*.

I base my selection of managerial ability level as a relevant characteristic on the analyses in Himmelberg and Hubbard (2000) and Oyer (2004). Both papers suggest that demand for high-ability individuals — those capable of running large complex organizations — rises and falls with conditions in the macro economy. Accordingly, opportunities for high-ability individuals to change firms increase when overall economic conditions are good. Thus, while traditional RPE theory suggests a negative relation between executive pay and peer firm returns, this line of reasoning suggests the opposite — a positive relation for executives whose skills are inelastically supplied in the labor market. I refer to tests of this idea as tests of the *managerial ability hypothesis*.

The Effects of the Sarbanes-Oxley Act of 2002 on Cross-Listed Firms and Cross-Listing Activity on US Stock Markets

A number of studies examine the effects of the Sarbanes-Oxley Act of 2002 (SOX), yet evidence regarding the Act's net benefits remains mixed. On one hand, Akhigbe and Martin (2006) find that SOX-related transparency improvements increased firm values in the financial services industry. Chhaochharia and Grinstein (2004) find that large firms needing to make SOX-related governance changes responded more favorably than other large firms did. Li, Pincus, and Rego (2004) find positive returns associated with events that resolved uncertainty about the Act's final provisions or were informative about its enforcement. They also find that investors expect SOX to improve the accuracy and reliability of financial reports by constraining managements' ability to manage earnings and by enhancing corporate governance.

On the other, Engel, Hayes, and Wang (2004) find that the quarterly frequency of going private transactions increased, post-SOX, that small firms with relatively illiquid shares responded less favorably than other firms to SOX announcements, and that small firms with greater inside ownership experienced higher going-private announcement returns, post-SOX, as compared to pre-SOX. Chhaochharia and Grinstein (2004) find that the costs of implementing the new rules exceed the benefits in small firms. Leuz, Triantis, and Wang (2004) find that about 200 US companies went "dark" in response to SOX, choosing to cease filing reports with the SEC by deregistering their securities and moving to the OTC. Cohen, Dey, and Lys (2004) find a significant decrease in the ratio of incentive compensation to gross compensation as well as a significant decline in research and development expenses, post-SOX, suggesting that SOX-related personal liability exposure has affected executives' willingness to take risks. Linck, Netter, and Yang (2005) find that SOX increased board of directors costs, particularly for small firms. They find that boards are larger and more independent, director workload and risk increased, and director and officer insurance premiums more than doubled, post-SOX.

I extend this literature by examining the effects of SOX on cross-listed firms. What makes this sample particularly interesting to study is the suggestion — by Coffee (1999, 2002), Fuerst (1998), and Stulz (1999) — that cross-listed firms create value through adherence to US regulations, which are typically more stringent than other countries'. This study provides new insight into the net benefits of US regulations to these firms.

Previous literature on SOX and the costs and benefits of a US cross-listing motivates the firm characteristics I use in cross-sectional analyses of the SOX announcement returns. Holmstrom and Kaplan (2003) suggest that small firms will have more difficulty accommodating the new SOX-related expenditures than large firms. Since lawmakers intend for SOX to reduce agency problems and because agency problems only arise to the extent that insiders own less than one hundred percent of the firm's equity [Jensen and Meckling (1976)], I study the relation between the announcement returns and the fraction of total shares held by insiders. Sarkissian and Schill (2003) argue that greater market liquidity reduces cross-listed firms' cost of capital and that the level of a firm's exports is highly correlated with cross-listed firm valuations. Coffee (1999, 2002), Fuerst (1998), and Stulz (1999) suggest cross-listed firms trade at a premium relative to their non-cross-listed counterparts because cross-listed firms subject themselves to: (i) the law enforcement powers of the Securities and Exchange Commission (SEC); (ii) an exigent litigation environment; (iii) enhanced financial disclosure rules that require reconciliation to US generally accepted accounting principles (GAAP); and (iv) increased scrutiny from outsiders such as investors, auditors, analysts, and investment bankers. This suggests that there are cross-sectional differences in SOX announcement returns based on each firm's home-country level of accounting standards and shareholder rights. Finally, Doidge, Karolyi, and Stulz (2004) find that growth opportunities are more highly valued for firms that cross-list in the US, particularly those from countries with poorer investor rights. They suggest that this is because a US listing reduces the extent to which controlling shareholders can engage in expropriation, which improves cross-listed firms' ability to finance their growth opportunities.

CHAPTER 3

HOW DO SYSTEMATIC RISK AND MANAGERIAL ABILITY AFFECT RELATIVE–PERFORMANCE–BASED COMPENSATION?

3.1 Introduction

The most costly natural disaster in US history — hitting the US Gulf Coast on August 29, 2005 and submerging over 80% of New Orleans — Hurricane Katrina caused over $100 billion in damage and the deaths of at least 1,300 people.[1] With Gulf Coast residents struggling to repair their lives in the hurricane's wake, good news emanating from the entire region was scarce — unless you owned shares in an oil company.

With oil and gas production shut down due to hurricane-related damage, oil prices reached record highs within days of the hurricane's landfall. Shares of major petroleum refineries, Sunoco, Marathon Oil, Holly Corporation, Tesoro, and Valero Energy, surged by over 10%; shares of Frontier Oil increased over 23%. Within three days, the value of Sunoco CEO John G. Drosdick's stock holdings increased by over $3.5 million; the value of Frontier Oil CEO James R. Gibbs' stock holdings increased over $5.0 million.

To be sure, there are other uncontrollable events — acts of terror, wars, even changes in interest rates or in federal regulations — that can diminish the value of corporate stocks. But the example of how oil company executives profited in the wake of Hurricane Katrina highlights one of the most interesting questions about executive compensation practices in the US today. That is, are CEOs compensated for the shareholder value created by their on-the-job performance (i.e., "effort") or the value created by uncontrollable shocks in the macro economy (i.e., "luck")?

---

[1]Source: *Hurricane Katrina, A Climatological Perspective. Preliminary Report* (US Department of Commerce National Oceanic and Atmospheric Administration, Technical Report 2005-01, October 2005)

Countless elements affect stock prices. Some, internal factors such as operating policies or marketing schemes, fall within a chief executive's circle of influence. Others, external factors such as natural disasters or unanticipated macro economic shocks, do not. External factors confound shareholders' ability to evaluate an executive's job performance because they introduce noise into traditional, stock-return-based performance criteria. How can shareholders be expected to evaluate an executive's job performance when there are elements beyond the executive's control that affect stock prices? What is the optimal incentive compensation plan when external factors impair the performance criteria?

Holmstrom (1979, 1982) and Holmstrom and Milgrom (1987) solve the problem of executive (agent) incentive maximization when external factors impair the performance criteria. According to Holmstrom (1979, p.87), incentive maximizing compensation agreements use "any informative signal, regardless of how noisy it is" to refine the performance criteria.[2] Peer firm stock returns are one such informative signal. In fact, the idea that peer-return-based performance benchmarking improves incentives has become so entrenched in the literature that researchers generally refer to it by shorthand as relative performance evaluation (RPE).

What is puzzling to researchers is that firms apparently make little use of such peer-return-based benchmarks. According to Murphy (1999, p.40) , "the paucity of RPE in options and other components of executive compensation remains a puzzle worth understanding." Abowd and Kaplan (1999, p.157) concur, stating that, "We therefore conclude that, despite the obvious attractive features of relative performance evaluation, it is surprisingly absent from US executive compensation practices."

[2]This is commonly known as Holmstrom's Informativeness Principle. The solution to the contracting problem I describe is referred to as the "second-best" solution because it is inferior only to the Pareto-optimal solution (the "first-best" solution), which itself is precluded by the assumption that shareholders are unable to observe the actions of the CEO directly.

The surprising results of the Towers Perrin 1997 *Annual Incentive Plan Design Survey* confirm these observations.[3] The survey results indicate that only 21% of industrial firms, 57% of finance/insurance firms, and 42% of utilities use RPE (see table 3.1). Empirical studies also yield similarly curious results. Barro and Barro (1990) find no evidence of RPE in a sample of 83 bank CEO's from 1982 to 1987. Antle and Smith (1986) find weak evidence of RPE based on accounting data, but not peer firm stock returns. On the other hand, Coughlan and Schmidt (1985) and Main (1991) find that changes in executive compensation are positively related to abnormal stock returns (based on a market model), while Gibbons and Murphy (1990) find strong support for the presence of RPE in their sample of 2,214 executives from 1974 to 1986.

In response, researchers have proposed and tested various alternative solutions to this "RPE puzzle." Aggarwal and Samwick (1999) argue that RPE undermines the price support firms elicit through tacit collusion within oligopolistic product markets. In support, they find that the impact of peer returns on executive pay is increasing in the level of product market competition. Garvey and Milbourn (2003) attribute the RPE puzzle to cross-sectional differences in executives' ability to shield their personal incomes from systematic risk. In support, they find that the RPE relation is stronger for executives who lack the wherewithal to hedge risk within their personal investment accounts. Bizjak, Lemmon, and Naveen (2004) attribute the RPE puzzle to the ubiquitous practice of compensation benchmarking — setting executive pay relative to the pay levels in comparable firms without regard to relative performance.

An alternative line of reasoning suggests that the RPE puzzle is attributable to executives' ability to exploit their influence over the board of directors. Proponents of the

---

[3]Towers Perrin is a large, international business-consulting firm in the areas of human resources, reinsurance, and actuarial services.

management power hypothesis argue that biases in the board member selection process preclude the possibility of arm's length contract negotiations between executives and boards [Bebchuk, Fried, and Walker (2002)]. These biases arise because about one-third of board nominating committees accept the CEO as a member [Shivdasani and Yermack (1999)]. Even when the CEO does not formally sit on the board nominating committee, the CEO exerts considerable influence on the board member nomination process [Main (1991)]. The personal costs to a board member from speaking up can outweigh the personal benefits; board members themselves frequently have little financial stake in the firms that they preside over [Baker, Jensen, and Murphy (1988)]. Several other papers document that boards of directors are complaisant when it comes to negotiating the terms of executive compensation agreements [Blanchard, Lopez-de-Silanes, and Shleifer (1994), Chance, Kumar, and Todd (2000), Core, Holthausen, and Larcker (1999)].

Nevertheless, in this paper I take the *optimal contracting* point of view. Under this point of view, boards design and enforce compensation agreements that minimize agency costs and maximize the CEO's incentives to create shareholder wealth. I take this point of view for two reasons. First, this approach is consistent with several theoretical models of executive compensation [Grossman and Hart (1983), Holmstrom (1979, 1982), Mirrlees (1976), Mookherjee (1984), Ross (1973), Shavell (1979)]. It also forms the basis of the empirical pay-to-peer-performance analyses in Aggarwal and Samwick (1999) and Garvey and Milbourn (2003). Second, the management power hypothesis does not explain *why* executives would disfavor relative-performance-based compensation. In fact, risk-averse executives may *prefer* relative-performance-based compensation because it reduces the variability of their expected incomes [Holmstrom (1979), Lazear and Rosen (1981)].

Under the optimal contracting assumption, the RPE puzzle is solved through identification of market, firm, or executive characteristics that make relative-performance-based compensation more or less compatible with the goals of agency cost minimization and incentive maximization. In this paper, I identify two such characteristics: (i) the firm's stock price sensitivity to systematic risk, and (ii) the executive's managerial ability level.

I select stock price sensitivity to systematic risk as a relevant characteristics based on the analyses in Holmstrom (1979) and Lazear and Rosen (1981). Both of these papers demonstrate how systematic risk factors diminish the incentives of risk-averse executives by increasing the variability of their expected incomes. Eliminating systematic risk factors through peer-return-based performance benchmarking improves incentives by reducing such variability. This line of reasoning suggests that the greater the firm's stock price sensitivity to systematic risk, the more important it is to eliminate the effects of such risk on the variability of executive pay. I test this idea, under what I refer to as the *noisy evaluation hypothesis*, by studying how the impact of peer-firm stock returns on executive pay varies with the firm's stock return sensitivity to systematic risk.

I select managerial ability level as a relevant characteristic based on the analyses in Himmelberg and Hubbard (2000) and Oyer (2004). Both of these papers suggest that the demand for high-ability executives — those capable of running large, complex organizations — rises and falls with conditions in the macro economy. Accordingly, opportunities for high-ability executives to change firms increase with overall economic conditions. Thus, while traditional RPE theory suggests a negative relation between executive pay and peer firm returns, Himmelberg and Hubbard (2000) and Oyer (2004) suggest the opposite — a positive relation for those executives whose high-level skills are inelastically supplied in the labor market. I test this idea, under what I refer to as the *managerial ability hypothesis*, by studying how the impact of peer-firm stock returns on executive pay varies with the executive's managerial ability level. Because ability is not directly observable, I proxy for it with two variables: (i) the firm's gross total assets, and (ii) the firm's number of employees. Selection of these proxy variables is based on the analysis in Rosen (1982) in which the author argues that higher-ability managers will be employed by larger firms.

The results of the study support both the noisy evaluation and managerial ability hypotheses. As predicted by the noisy evaluation hypothesis, I find statistically significant differences between the impact of peer firm stock returns on executive pay in low and high beta firms and that the impact of peer firm stock returns on executive pay is decreasing in the firm's stock return sensitivity to systematic risk. As predicted by the managerial ability hypothesis, I find statistically significant differences between that the impact of peer firm stock returns on executive pay in small and large firms and that the impact of peer firm stock returns on executive pay is increasing in the executive's managerial ability level. These findings are robust to various measures of compensation (defined in the body of the paper) and performance benchmarks, including related-industry firm returns and overall market returns. Overall, the results are consistent with the broad idea that firm- and executive- specific characteristics can increase or diminish the usefulness of peer-return-based performance criteria — helping explain why relative performance benchmarking is not as pervasive as early compensation theory suggests it should be.

The paper proceeds as follows. In Section 2, I introduce the theory and develop the hypotheses. In Section 3, I describe the testing methods, break down the sampling technique, and provide summary statistics for the data set. I analyze the test results in Section 4. Section 5 concludes.

3.2 Theory and hypotheses development

3.2.1 Noisy evaluation hypothesis

The notion that noisy performance criteria diminish incentives dates back to Holmstrom (1979) and Lazear and Rosen (1981). In each of these papers, the authors develop incentive-maximizing compensation plans in circumstances where external elements impair the performance criteria. Both papers demonstrate how shareholders can improve risk-averse executives' incentives by reducing the noise external factors bring to the performance criteria.

Under normal circumstances, all firms (wherein there is a separation of ownership and control) must design incentive compensation plans when the CEO's true effort is difficult to observe and external factors impair otherwise useful performance criteria (such as the firm's stock return). For example, suppose that shareholders pay risk-averse executives based on some measure of output and that the output, $\tilde{o}_i$, of firm/executive i is stochastic such that:

$$\tilde{o}_i = o_i(e_i) + m + l_i \tag{3.1}$$

where "true output", o_i, is a function of executive i's unobservable effort level, e_i, m is an uncontrollable random external random shock affecting the output of all executives in common, and l_i, is a random "luck" term affecting only the output of executive i. If effort, e_i, was observable, then the optimal incentive payment plan takes the form $w(e_i)$. The executive's compensation is entirely under the executive's control (via effort) and external shocks have zero impact on the executive's pay. Unfortunately, under normal circumstances, e is difficult or costly to observe and so shareholders must use the noisy, but observable, output measure, $\tilde{o}_i$, as a proxy for effort. Under these circumstances, performance-based incentive payments, w, take the form $w(\tilde{o}_i) = w[o_i(e_i) + m + l_i]$, which subjects executive i's income to variability attributable to personal luck, l_i, or the whims of the market, m. The exact amount of risk faced by the executive under this simple compensation plan is:

$$var(\tilde{o}_i) = var(m) + var(l_i) \tag{3.2}$$

because $o_i(e_i)$ is non-random and m and l_i are independently distributed by assumption.

According to Holmstrom (1979) and Lazear and Rosen (1981) both executives and shareholders can derive value by finding a means to reduce this variability. Reducing the variability improves the executive's incentives because it reduces uncertainty over the value of future compensation payments. It also benefits shareholders by reducing compensation payments made for "luck."

To see how relative performance benchmarking reduces risk, consider what happens when shareholders pay executives based on output as it compares to the contemporaneous performance of a competitor (or group of competitors). Let $\tilde{o}_j$, equal the expected output of peer firm j, and $\tilde{o}_j = o_j(e_j) + m + l_j$. Because the random error term, m, has an equal effect on the output of both firms i and j (thereby canceling out), and under the assumption that l_i and l_j are i.i.d., executive i's probability, P, of outperforming executive j is:

$$P[\tilde{o}_i > \tilde{o}_j] = P[o_i(e_i) - o_j(e_j) > (l_j - l_i)] = P[o_i(e_i) - o_j(e_j) > \lambda] = G[o_i(e_i) - o_j(e_j)], \tag{3.3}$$

where $\lambda = [(l_j - l_i)]$, $\lambda \sim g(\lambda)$, $G(\cdot)$ is the cumulative distribution function of λ, $E(\lambda) = 0$, and $E(\lambda^2) = 2 \times var(l_i)$. Under this relative-performance-based incentive plan, the amount of risk faced by executive i is $2 \times var(l_i)$. Comparing this amount of risk to the amount of risk faced by the executive under the simple performance-based plan, $var(m) + var(l_i)$, [see equation (3.2)] it is easy to see that relative performance-based compensation reduces risk to the extent the common error term, m, contributes to the total variance of the performance criterion, $var(\tilde{o}_i)$.

We can now use the above reasoning to see how relative performance evaluation reduces risk when the performance criterion is the executive's own firm's stock return. Consider, that instead of basing the executive's compensation on output, $\tilde{o}_i$; the firm pays the executive based on stock return, R_i. Under the capital asset pricing model, the variance of R_i equals:

$$var(R_i) = \beta^2 \times var(m) + var(\varepsilon) \tag{3.4}$$

where $\beta^2 \times var(m)$ represents the contribution of common macro economic shocks to the total variance of the performance criterion, R_i, and $var(\varepsilon)$ represents the contribution of idiosyncratic risk. Notably, when β equals zero, all variance is idiosyncratic. As the above analysis suggests, when the performance criterion is the return on the firm's common stock, the capacity for relative performance benchmarking to lower risk/improve incentives is increasing in the square of the firm's beta coefficient. This is because, the fraction of stock return variability caused by developments external to the firm, but common to all firms, is larger in high beta firms. Thus, we are likely to observe that peer firm stock returns have a greater (more negative) impact on executive compensation in high beta firms because filtering out the effects macro economic shocks improves incentives and better aligns the interests of executives' with those of the shareholders of these firms.

Based on this analysis, I make my first prediction regarding the relation between executive pay and peer firm stock returns:

H1: *Peer firm stock returns have a greater (more negative) impact on executive compensation in high beta firms.*

3.2.2 Managerial ability hypothesis

Analyses of the impact of peer returns on executive pay implicitly assume that the executive's outside employment opportunities are uncorrelated with overall economic conditions. Under this assumption, positive macro economic shocks have no impact on the executive's opportunities to change firms. In contrast, Himmelberg and Hubbard (2000) and Oyer (2004) argue that the supply of executives qualified to run large, complex organizations is inelastic. When the economy is growing, opportunities for these executives to change firms increase; firms must increase their pay to prevent high-ability executives from pursuing better-paying job opportunities. Because frequently re-negotiating compensation agreements is costly, firms build in automatic adjustments that rise and fall with the macro economy. These automatic adjustments are especially important for firms that employ high-ability executives because high-ability executives are the ones that are likely to receive offers for better-paying jobs when macro economic conditions are good. This suggests that high-ability executives will receive pay *increases* when overall stock prices are rising — contrary to the predictions of the traditional RPE theory.

Himmelberg and Hubbard (2000) test their theory and find that the sensitivity of executive compensation to the systematic component of firm stock returns is higher for higher ability CEOs — those who possess skills that are inelastically supplied in the labor market. The authors proxy for managerial ability using the firm's sales level, price-cost margin, R&D intensity, advertising intensity, and executive job tenure.

Rajgopal, Shevlin, and Zamora (2006) also test the Himmelberg and Hubbard (2000) and Oyer (2004) theory. They similarly find that the sensitivity of CEO compensation to market-wide and industry-wide performance is systematically higher for high-ability CEOs. These authors proxy for managerial ability using the executive's financial press visibility (given by the number of articles in major newspapers containing the CEO's name), and the firm's industry adjusted ROA.

In this paper, I test the hypotheses in Himmelberg and Hubbard (2000) and Oyer (2004) using two alternative variables related to managerial ability, namely: (i) the book value of gross total assets, and (ii) number of employees.[4] Like Himmelberg and Hubbard, I justify my use of these variables on Rosen (1982), who argues that there is a strong correlation between firm size and managerial ability. This is because in hierarchical organizations, decisions at each level above the bottom affect productivity at the next lowest level. The repercussions of decisions made at the uppermost level "spill over" down through the ranks in the same way champagne spills down through a champagne tree. Subordination of less talented individuals to more talented individuals, in combination with spillover effects, effectively allows more talented individuals to "clone" themselves (imperfectly) by transferring part of their talent to their immediate subordinates, who transfer it to their subordinates and so on down the line. Competition over the use of scarce resources ensures that markets allocate managers to their best use (i.e., markets are "allocatively efficient"). So markets sort high-ability managers into the top positions of the largest firms, where their talents are more widely cloned, and the next most talented managers into the top ranks of smaller firms (or to lower level positions in larger firms).

Several empirical studies support Rosen's hypothesis. Baker and Hall (2004) find that CEOs of large firms have a considerable "spill over" effect upon firm value. Hayes and Schaefer (1999) find that the abnormal returns of firms losing key managers to rival firms are more negative when the rival firm is larger in terms of total assets. Brown and Medoff (1989) find that differences in worker quality explain the wage differentials between large and small firms.

[4]I define gross total assets as the sum of net total assets plus accumulated depreciation, depletion, and amortization. I use this variable because it accounts for the likelihood that the book value of assets does not represent the market value of assets and that the market value is the more relevant value measure. Doing so also eliminates the impact of differences between each firm's choices of depreciation method.

Based on this analysis, I make my second prediction regarding the relation between executive pay and peer firm stock returns:

H2: *Peer firm stock returns have a greater (more negative) impact on executive compensation in small firms*.

3.3 Data and method

3.3.1 Data

Considering the magnitude of the financial commitment and the impact that a CEO has on all areas of the firm's operations, it would seem that the first place to look for information regarding performance-based incentive plans would be in the CEO's employment contract. Such information is seemingly readily available because the SEC requires firms to disclose the terms and conditions of such contracts under regulation S–K, Item 402. Unfortunately, it turns out that most firms simply do not have written contracts with their CEO. In fact, Gillan, Hartzell, and Parrino (2005) find that, as of the year 2000, more than half of the firms in the S&P 500 either do not have a comprehensive written employment agreement with their CEO at all or have agreements that cover only limited aspects of their relationship with the CEO, such as change of control agreements. Thus, for the purposes of this study, I use actual compensation and stock return data to examine the relationship between executive pay and peer performance, rather than data gathered from reading employment contracts (which is generally unavailable).

I test the noisy evaluation and managerial ability hypotheses by modeling the level of chief executive officer compensation as a function of own firm stock returns, peer firm stock returns, and the interaction of own and peer firm stock returns with variables representing the firm's stock price sensitivity to systematic risk and the executive's managerial ability level. I choose annual stock returns as the performance measure (rather than accounting-based or other performance measures) because the ultimate goal of the firm's chief executive is the maximization of shareholder wealth. Moreover, various earnings management techniques diminish the usefulness of accounting data as a measure of firm performance. According to Healy and Wahlen (1999, p.368), "Earnings management occurs 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." Opportunities for managers to exercise such judgment occur when managers: (i) estimate expected lives and salvage values of long-term assets or losses from bad debts and asset impairments, (ii) select among the various depreciation and inventory valuation methods, (iii) make or defer discretionary expenditures on items like research and development, advertising, or maintenance, (iv) speed up or slow down the collections and payments of accounts receivable and payable, and (v) structure corporate transactions such as business combinations, lease contracts, and equity investments. Several studies indicate that managers actually do use earnings management to affect their pay under performance-based compensation plans. Guidry, Leone, and Rock (1999) find that business-unit managers defer income both when the earnings target in their bonus plan will not be met and when they have already reached the maximum bonus permitted under the plan. Healy (1985) and Holthausen, Larcker, and Sloan (1995) find that managers manipulate earnings downward when their bonuses have already reached the maximum allowed under their contract. Dechow and Sloan (1991) find that CEOs manage discretionary research and development expenditures to increase earnings during their final years in office. A recently released report regarding the accounting practices at giant mortgage lender Fannie Mae similarly states that, "management's adoption of certain accounting policies and financial reporting procedures was motivated by a desire to show stable earnings growth, achieve forecasted earnings, and avoid income statement volatility."[5] Given this evidence, the use of the comparatively unbiased stock return measure of performance seems justified.

I identify my sample and collect compensation data from the *EXECUCOMP* database. The *EXECUCOMP* database includes detailed compensation data on the top five executives (ranked annually by salary and bonus) for firms in the S&P 500, S&P Midcap 400, and the S&P Smallcap 600, including salary, bonus, stock option, long-term incentive plan payouts, and other compensation information. In this study, I use two basic measures of annual compensation: (i) total compensation, and (ii) cash compensation. I define total compensation as the sum of base salary, bonus, long-term incentive payouts, restricted stock grants, stock option grants (at their full Black-Scholes option value), and all other compensation (e.g., debt forgiveness, signing bonuses, 401K contributions, and other perquisites). I define cash compensation as the sum of salary plus bonus.[6]

In addition, I construct two additional measures of compensation to account for the impact of personal income taxes and risk (hereafter referred to as measures of after-tax, certainty equivalent compensation). I construct these measures based on the analyses in Bettis, Bizjak, and Lemmon (2005) and Hall and Murphy (2002), who point out that there can be a considerable difference between the *cost* of pay to the firm and the *value* of the pay to the executive due to difficulties in the valuation of executive stock options and restricted stock grants. This is because the Black-Scholes model of option pricing assumes liquid stock and option markets, but

[5]Source: *A Report to the Special Review Committee of the Board of Directors of Fannie Mae, Executive Summary*, February 23, 2006. Fannie Mae is a US Federal Government Agency with over $1,000M in assets, as of December 31, 2003.

[6]This measure of cash compensation is identical to the measure used by Lambert and Larcker (1987). It is also similar to the one used by Aggarwal and Samwick (1999) who define short-term compensation as the sum of salary, bonus, and other annual payments (e.g., gross-ups for tax liabilities, perquisites, preferential discounts on stock purchases).

there is no liquid market for executive stock options; executives cannot sell their stock options to a third party. Moreover, firms typically issue executive stock options under a vesting schedule and executives must exercise the options before a defined maturity date, typically ten years from the option grant date. Executives also forfeit previously issued and unexercised stock options grants when they leave the firm. Thus, the Black-Scholes option pricing model provides an *estimate* of the true value of executive stock option grants, but the *actual* value of such grants to the executive is typically less than the full Black-Scholes value. Similarly, restricted stock grants are "restricted" in the sense that they also vest over a specified holding period. As in the case of stock option grants, executives typically forfeit unvested shares of restricted stock upon their departure from the firm. Thus, the *EXECUCOMP* value of restricted stock grants is likely overstated as well.

I account for the effects of personal income taxes and risk as follows. For base salary, bonus, and long-term incentive payouts, I apply the top marginal tax rates, obtained from the Internal Revenue Service, to each year's payment to derive an after-tax value. No adjustments for risk are required because these are cash payments. For restricted stock grants, there are no tax adjustments because executives do not pay taxes on restricted stock grants until the end of the vesting period. However, the possibility that executives might forfeit the grants makes them a risky form of compensation. Hall and Murphy (2002), estimate that the risk premium on restricted stock is approximately 25%. Based on this estimate, I multiply the dollar value of each year's restricted stock grant by 0.75 to adjust the gross value down to its certainty equivalent value estimate. For stock option grants, Hall and Murphy (2002) suggest a 60% discount and Bettis, Bizjak, and Lemmon (2005) suggest a 61% discount from the option grant values on *EXECUCOMP*. Following these analyses, I discount the *EXECUCOMP* values of the annual stock option grants by 60% to adjust the gross value down to its certainty equivalent value estimate. For items such as debt forgiveness, payment for unused vacation, and signing bonuses, I net out the associated income tax liability. Finally, I ignore all other items such as perquisites and deferred compensation in my calculations of after-tax, certainty-equivalent compensation.

I illustrate the effects of these adjustments in charts 3.1 and 3.2. Before adjustment, stock options comprise about 47% of the total executive pay, while salary and bonus comprise approximately 32%. After adjusting for taxes and risk, salary and bonus increase to approximately 37% of total compensation, while the value of stock option grants decreases to about 37%. Notably, the overall trend is for stock option grants to replace salary and bonus as the main form of compensation. Salary and bonus comprise 38% of total compensation in 1996 and only 28% in 2000, while option grants increased from 43% of total compensation in 1996 to 50% in 2000.

Tests of the noisy evaluation hypothesis require identification of a variable that represents the firm's stock return sensitivity to systematic risk. To estimate this sensitivity, I calculate the firm's beta coefficient (*BETA*) from a regression of each firm's daily stock return on the equally weighted index return from *CRSP* in year immediately preceding each year in the sample period 1996–2000. Doing so allows this measure to vary both cross-sectionally and annually throughout my sample period. The information I use to calculate beta would have been available to the board of directors and executives before the beginning of each year in the sample period so that shareholders and managers would have had similar *ex ante* estimates of the firms' stock return sensitivity to systematic risk.

Tests of the managerial ability hypothesis require identification of a variable that represents each executive's managerial ability level. I use two measures of firm size to represent managerial ability level: (i) gross total assets (*GTA*), and (ii) number of employees (*EMPS*). Like Himmelberg and Hubbard (2000), I base my use of firm size variables to represent managerial ability level on Rosen (1982). However, Himmelberg and Hubbard use sales level, while I use gross total assets and number of employees to represent firm size. I select these measures because gross total assets and number of employees better represent the point of the Rosen hypothesis than sales. Recall that Rosen argues that higher-ability executives go to larger firms so that their superior talents can transfer down through the organization to a greater number of lower-level employees. This suggests that firm size, *per se*, is not the essential element underlying the hypothesis, but rather it is the number of employees supervised. Moreover, the firm's book value of gross total assets more perfectly correlates with managerial ability than sales because: (i) assets are under the direct control of the CEO while sales levels are a function of the (externally determined) demand for the firm's products, and (ii) a greater level of assets implies a more complex organization while a greater level of sales does not necessarily imply complexity. The ability to manage a large organization and the ability to generate sales are quite different; the former requires managerial ability, while the latter requires persuasive ability. Data for both measures of firm size, *GTA* and *EMPS*, comes from the *COMPUSTAT* database. Each variable represents beginning of the year values such that shareholders and managers would have had similar *ex ante* estimates of the executive's managerial ability level (and marketability) before the beginning of each year in my sample period.

Sample firms/executives meet the following two requirements: (i) executives were employed as CEO for the entire five-year period 1996–2000, and (ii) the firm's financial statements are dated as of the calendar year end. This means that all of the compensation, own-firm stock return, and peer firm stock return data used in the study correspond to the five calendar years ending 1996–2000, while all of the other data (i.e., *BETA*, *GTA*, and *EMPS*) are dated as of the calendar years ending 1995–1999. The advantages of selecting the sample with these restrictions are twofold. First, it ensures that the sample executives were employed for a sufficiently long enough time that the board could evaluate their strengths, weaknesses, and overall performance and make adjustments to their compensation plans. Second using data compiled over a matching period allows me to control for yearly fixed effects using year dummy variables. Failure to control for these fixed effects creates a bias in regression parameter estimates. The major disadvantage of the sampling restrictions is that I systematically ignore circumstances wherein the board terminated the CEO. Naturally, any sort of sample selection bias takes a little away from the generalizability of the results. Nevertheless, simply viewing this study as an analysis of relative performance evaluation in *long-term* employment relationships alleviates the concern over survivor bias.

The final sample is comprised of 323 firms/executives, from 126 different industries (defined by three-digit SIC code) for a total potential sample size of 1,615 firm-year observations. Missing data ultimately reduces the sample size to from between 1,306 and 1,464 firm-year observations, depending upon the data requirements for each model that I estimate. Missing data otherwise has no serious econometric consequences so long as the reasons for missing data are sufficiently exogenous.[7]

I present means and medians of key variables for both sample and non-sample firms in table 3.2. I define non-sample firms as all of the firms on the *EXECUCOMP* database that did not meet the sampling criteria. Table 3.2 indicates that there are differences between the sample and non-sample firms that warrant mentioning. First, it is not surprising that the sampled

[7]See Wooldridge (2002, p.551) for a detailed exposition regarding the use of unbalanced panel data with ordinary least squares and fixed effects models. I have no reason to believe that there is any systematic reason why data is missing.

executives' tenure as CEO is significantly longer than that of the non-sample executives.[8] This is, of course, attributable to the restriction that the sampled executives are employed as CEO for at least five consecutive years. In each of the five years in the study, the sampled executives' mean tenure is significantly longer than that of the non-sample executives, with the smallest difference being about 1 year in 1996, progressively rising to 6 years difference in 2000. The median tenure as CEO for sample firms in 1996 is six years, rising to 10 years by 2000, while the median tenure for non-sample firms is 5 years in 1996, falling to 3 years by 2000. Over the five years in the study, the median tenure for sample firm executives is 8.0 years, while it is 4.0 years for non-sample firms. This difference is significant at the 5% level.

Despite the difference in tenure, the sample executives are generally not paid more, on average, than the non-sample executives are. In 1996 and 1997, the difference in mean total compensation between sample and non-sample executives is not statistically significant. In 1998 and 2000, the sample executives are paid significantly less than the non-sample executives are, and in 1999, they are paid significantly more. Over the five years in the study, the median annual compensation is $2.28 million for sample firms and $1.92 million for non-sample firms. This difference is significant at the 5% level.

The average annual stock return for sample firms is not statistically different from that of the non-sample firms in the first three years of the study, while in 1999, non-sample average stock returns were significantly greater than the sample firm returns, and in 2000, sample firm average stock returns were significantly greater than the non-sample firm returns. Over the five years in the study, the median annual stock return is 13.3% for sample firms and 13.6% for non-sample firms. This difference is not significant.

[8] I calculate tenure by subtracting the year the individual in question became CEO from the year under consideration in the study.

Sample firms' stock returns are also generally less sensitive to market risk than the non-sample firms' are. In each year of the study, the sample firms' beta coefficients are significantly less than the non-sample firms'. Over the five years in the study, the mean beta coefficient for sample firms is 0.95, while it is 1.09 for non-sample firms. This difference is significant at the 5% level.

In terms of both gross total assets and number of employees, the sample firms are reliably larger than the non-sample firms. In each year of study, sample firms have significantly greater gross total assets and number of employees than non-sample firms. Moreover, the average annual growth rate in gross total assets is 14.2% for sample firms and only 11.8% for non-sample firms. Similarly, the average annual growth rate in the number of employees is 6.3% for sample firms and only 4.9% for non-sample firms. Over the five years in the study, the median gross total assets for sample firms are $1.72 million, while it is $0.81 million for non-sample firms. The average number of employees is 6,700 and 3,700 for sample and non-sample firms respectively. Both differences are significant at the 5% level.

3.3.2 Method

The specific models I use to test the hypotheses are based on the models used in Aggarwal and Samwick (1999), Bizjak, Lemmon, and Naveen (2004), and Garvey and Milbourn (2003). Specifically, I estimate these three equations using the above-described data:

$$w_{it} = \eta_1 \pi^o{}_{it} + \eta_2 \pi^r{}_{it} + \eta_3 \pi^o{}_{it} F(P_{it}) + \eta_4 \pi^r{}_{it} F(P_{it}) + \eta_5 F(P_{it}) + \sum_{t=1}^{5} \varphi_t \, dy_t + \varepsilon_{it} \quad (3.5)$$

$$\Delta w_{it} = \eta_1 \pi^o{}_{it} + \eta_2 \pi^r{}_{it} + \eta_3 \pi^o{}_{it} F(P_{it}) + \eta_4 \pi^r{}_{it} F(P_{it}) + \eta_5 F(P_{it}) + \sum_{t=2}^{5} \varphi_t \, dy_t + \varepsilon_{it} \quad (3.6)$$

$$w_{it} = \eta_1 \pi^o{}_{it} + \eta_2 \pi^r{}_{it} + \eta_3 \pi^o{}_{it} F(P_{it}) + \eta_4 \pi^r{}_{it} F(P_{it}) + \eta_5 F(P_{it}) + \sum_{t=1}^{5} \varphi_t \, dy_t + \sum_{i=1}^{N} \psi_i \, df_i + \varepsilon_{it} \quad (3.7)$$

where w_{it} is executive i's compensation in year t, π^o_{it} is own-firm performance, and π^r_{it} is peer firm performance. So that the parameter estimates, η, represent the dollar change in executive compensation for every $1,000 change in shareholder wealth created, I calculate own-firm performance as

$$\pi^o_{it} = \rho_{it}\, W_{i,t\text{-}1} \tag{3.8}$$

where ρ_{it} is firm i's annual stock return in year t from *CRSP* and $W_{i,t\text{-}1}$ is the beginning of year market value of equity for firm i from *COMPUSTAT*. I calculate peer firm performance as

$$\pi^r_{it} = \rho_{\text{-}it}\, W_{i,t\text{-}1} \tag{3.9}$$

where $\rho_{\text{-}it}$ is the average annual stock return of peer firms and $W_{i,t\text{-}1}$ remains the beginning of period market value of firm i. In other words, π^r_{it} is what firm i's change in overall shareholder wealth would have been had firm i earned the average return of their group of peer firms. In the actual calculations, I use two different measures of $\rho_{\text{-}it}$: (i) the equally-weighted mean return of firms within firm i's three-digit SIC code, and (ii) the equally-weighted market return; both of which are pulled from the *CRSP* database. $F(P_{it})$ represents the empirical cumulative distribution function of the variables used to represent either the firm's stock price sensitivity to systematic risk or the executive's managerial ability level.[9] I convert these variables into their empirical cumulative distribution functions because it makes interpretation of parameter estimates much easier. For example, by setting $F(P_{it})$ equal to 0.0 or 1.0, I can evaluate the pay-performance sensitivity of firms with the smallest and largest values of the proxy variables, respectively. Evaluating equations (3.5) through (3.7) setting $F(P_{it})$ equal to 0.5 allows me to evaluate the pay-performance sensitivity of firms having the median value of the proxy variables.

[9]The empirical cumulative distribution function of a sample, $F_n(y)$, is the proportion of non-missing observations less than or equal to y. That is, $F_n(y) = \frac{1}{n}\sum_{i=1}^{n} I(y_i \leq y)$, where n is the number of non-missing observations, and $I(y_i \leq y)$ is an indicator function with a value of one if $(y_i \leq y)$ and zero, otherwise.

Finally, I include firm, *df*, and time, *dy*, dummy variables for reasons specified below. The inclusion of these dummy variables obviates the need for an intercept in the models.

I use panel data to estimate the models for several reasons. First, using panel data increases the sample size, which leads to estimation that is more precise and for hypotheses tests that have greater power. Second, since panel data includes both cross-sectional and time-series elements, it creates more variability in the data and alleviates the problem of multicolinearity. Finally, and most importantly, panel data allows me to control for unobservable items correlated with both the independent and dependent variables, which, when omitted from models that relate pay and stock performance, result in biased parameter estimates. In fact, estimation of models (3.5) through (3.7) represents my progressively more restrictive attempts to deal with the issue of potential correlation between the error term and the explanatory variables. For example, in equation (3.5), the implicit assumption is that the time dummy variables, *dy*, absorb all of the correlation between performance and the level of compensation attributable to unspecified factors within the model's error term. In equation (3.6), first differencing the dependent variable purges the estimates of bias unless the unobservables are also correlated with both the level and changes in compensation. Finally, in equation (3.7), the two-way fixed effects model, I add firm dummy variables, *df*, in order to purge the estimates of bias related to correlation between executive-specific constants and my performance criteria.

The cost of moving from equation (3.5) to equation (3.7) is that each succeeding model uses progressively less of the variation in the compensation and performance data to estimate the coefficients. In equation (3.6), first differencing guards against, but does not eliminate the omitted variable issue, but it also weakens my ability to make inference regarding the relationship between the level of compensation and performance because it only considers correlation between the *change* in compensation and performance. In equation (3.7), including firm dummy variables as regressors reduces degrees of freedom by the number of firms in the data set and increases the standard errors of the parameter estimates. Nevertheless, the conclusions reached when all three models produce similar parameter estimates are more credible than if I used only one model to test the hypotheses.

The specific variable of interest from estimation of models (3.5) through (3.7) is the partial derivative of the CEO's compensation with respect to peer firm performance, π^r_{it}, after controlling for own firm performance, π^o_{it}, which is

$$H = \eta_2 + \eta_4 F(P_{it}). \tag{3.10}$$

Because the performance estimates, π^o_{it} and π^r_{it}, are stated in terms of millions of dollars while the compensation estimate, w_{it}, is stated in terms of thousands of dollars, H equals the average dollar change in chief executive pay for every \$1,000 change in peer-return-based shareholder wealth — after controlling for the actual change in shareholder wealth.

I test my hypotheses by allowing $F(P_{it})$ to vary from 0.0 to 1.0 and observing: (i) whether movement from $F(P_{it})$ equals 0.0 to $F(P_{it})$ equals 1.0 changes H in the direction predicted by either the noisy evaluation or managerial ability hypotheses, and (ii) whether there are significant differences between the estimates of H when $F(P_{it})$ equals 0.0 and $F(P_{it})$ equals 1.0. Testing for significant differences in the parameter estimates entails rearranging equation (3.10) such that:

$$\eta_2 = H - \eta_4 F(P_{it})^*, \tag{3.11}$$

where $F(P_{it})^*$ equals 0.0, 0.5, or 1.0. I then substitute equation (3.11) into each of the equations (3.5) through (3.7). For example, using equation (3.5) and substituting in equation (3.11) gives,

$$w_{it} = \eta_1 \pi^o_{it} + [H - \eta_4 F(P_{it})^*] \pi^r_{it} + \eta_3 \pi^o_{it} F(P_{it}) + \eta_4 \pi^r_{it} F(P_{it}) + \eta_5 F(P_{it}) + \sum_{t=1}^{5} \varphi_t dy_t + \varepsilon_{it}. \tag{3.12}$$

Gathering like terms gives,

$$w_{it} = \eta_1\, \pi^o{}_{it} + [H]\, \pi^r{}_{it} + \eta_3\, \pi^o{}_{it}\, F(P_{it}) + \eta_4\, \pi^r{}_{it}\, [F(P_{it}) - F(P_{it})^*] + \eta_5\, F(P_{it}) + \sum_{t=1}^{5} \varphi_t\, dy_t + \varepsilon_{it}. \tag{3.13}$$

I estimate H and its standard error by setting $F(P_{it})^*$ equal to 0.0, 0.5, or 1.0 and estimating equations (3.5) through (3.7) as suggested by equation (3.13). I then use the parameter estimates, H, and their respective standard errors to conduct the usual tests of significance using the t-test.

3.4 Results and analysis

3.4.1 Noisy evaluation hypothesis

I present the results from the tests of the noisy evaluation hypothesis in tables 3.3 and 3.4. Each of these tables has six columns. Each column presents the results from estimation of equations (3.5) through (3.7) twice — once, using the SIC-code-based return benchmark, and again using the *CRSP* market index returns as the relative performance measure. In table 3.3, the dependent variables are total compensation (panel A) and cash compensation (panel B). In table 3.4, the dependent variables are after-tax, certainty equivalent total compensation (panel A) and after-tax, certainty equivalent cash compensation (panel B).

The results in table 3.3 support the noisy evaluation hypothesis. As predicted, I find that the impact of peer firm stock returns on executive pay is decreasing in the firm's stock return sensitivity to systematic risk. For example, in panel A, wherein the dependent variable is total compensation, I find that, moving from low to median to high *BETA* firms, sample executives' pay to SIC-code-based peer firm stock performance decreases from a significant $+0.29, to $0.00, to a significant $-0.30 for every $1,000 increase in relative performance, using OLS. Using fixed effects, the pay to peer performance sensitivities decrease from $+0.03, to a significant $-0.04, to a significant $-0.11. In both of these cases, the high *BETA* firm parameter estimates are also significantly less than the low *BETA* firm parameter estimates. The results are inconclusive, however, when I use the *CRSP* market index returns as the performance benchmark. Using OLS I find that, again, moving from low to median to high *BETA* firms, sample executives' pay to market-index-based peer performance decreases from a significant $+0.43, to a significant $+0.28, to $+0.14 for every $1,000 increase in relative performance. However, using fixed effects, the parameter estimates *increase* from a significant $-0.17, to $+0.04, to a significant $+0.26. In panel B, wherein the dependent variable is cash compensation, I find that, moving from low to median to high *BETA* firms, sample executives' pay to SIC-code-based peer performance decreases from a significant $+0.07, to a significant $+0.01, to a significant $-0.06, using OLS. Using fixed effects, the pay to peer performance sensitivities decrease from a significant $+0.03, to $0.00, to a significant $-0.02. Similarly, using the market-index-based benchmark, I find that the parameter estimates decrease from a significant $+0.17, to a significant $+0.06, to a significant $-0.06, using OLS. Using fixed effects, the pay to peer performance sensitivities decrease from a significant $+0.11, to a significant $+0.02, to a significant $-0.07. I each case, the high *BETA* firm parameter estimates are significantly less than the low *BETA* firm parameter estimates.

The results in table 3.4, wherein the dependent variable is after-tax, certainty equivalent total compensation, also support the noisy evaluation hypothesis. For example, in panel A I find that, moving from low to median to high *BETA* firms, the executive pay to SIC-code-based peer performance parameter estimates decrease from a significant $+0.15, to $0.00, to a significant $-0.15, using OLS. Using fixed effects, the pay to peer performance sensitivities decrease from $+0.02, to a significant $-0.02, to a significant $-0.05. Once again, the high *BETA* firm parameter estimates are significantly less than the low *BETA* firm parameter estimates. Similar to the results in table 3.3, the results are inconclusive when I substitute in the *CRSP* market index returns as the performance benchmark. Using OLS I find that, again, moving from low to median to high *BETA* firms, sample executives' pay to market-index-based peer performance decreases from a significant \$+0.21, to a significant \$+0.16, to \$+0.10 for every \$1,000 increase in relative performance. However, using fixed effects, the parameter estimates *increase* from a significant \$-0.08, to a significant \$+0.04, to a significant \$+0.16. In panel B, wherein the dependent variable is after-tax, certainty equivalent cash compensation, I find that, moving from low to median to high *BETA* firms, sample executives' pay to SIC-code-based peer performance decreases from a significant \$+0.04, to \$0.00, to a significant \$-0.04, using OLS. Using fixed effects, the pay to peer performance sensitivities decrease from a significant \$+0.02, to \$0.00, to a significant \$-0.01. Similarly, using the market-index-based benchmark, I find that the parameter estimates decrease from a significant \$+0.10, to a significant \$+0.03, to a significant \$-0.04, using OLS. Using fixed effects, the pay to peer performance sensitivities decrease from a significant \$+0.07, to a significant \$+0.01, to a significant \$-0.04. Once again, the high *BETA* firm parameter estimates are significantly less than the low *BETA* firm parameter estimates no matter the model or the peer return benchmark used.

Overall, these results support the noisy evaluation hypothesis. That is, the data indicate that the impact of peer firm returns on executive pay is decreasing in the firm's stock return sensitivity to systematic risk, as measured by *BETA*. *BETA* conditions the impact of peer-firm stock returns on executive pay. The greater the firms' stock return sensitivity to systematic risk, the more negative the impact of peer returns on executive compensation.

3.4.2 Managerial ability hypothesis

Tables 3.5 through 3.8 present the results from the tests of the managerial ability hypothesis. In tables 3.5 and 3.6, I use the dollar value of firm's gross total assets, *GTA*, as the proxy for managerial ability. In tables 3.7 and 3.8, I use number of employees, *EMPS*.

The results in table 3.5 support the noisy evaluation hypothesis. As predicted, the impact of peer firm returns on executive pay is decreasing in the executive's managerial ability level. For example, in panel A, wherein the dependent variable is total compensation, I find pay to peer performance parameter estimates of $+0.02 for high *GTA* firms and $-0.23 for low *GTA* firms using OLS and the SIC-code-based performance benchmark. While each of these parameter estimates is not significant, they are significantly different from each other. Using fixed effects, the pay to peer performance sensitivities decrease from high *GTA* to low *GTA* from a significant $-0.03 to $-0.13. Using the *CRSP* market index returns as the performance benchmark, I also find statistically significant differences between the high *GTA* and low *GTA* parameter estimates. Using OLS, the high *GTA* estimate is a significant $+0.13, while the low *GTA* estimate is $0.01. Using fixed effects, the respective parameter estimates are a significant $-0.09 and $-0.50. In each case, the high *GTA* firm parameter estimate is significantly greater than the low *GTA* firm parameter estimate. In panel B, in which the dependent variable is cash compensation, I find that, moving from high to median to low *GTA* firms, sample executives' pay to SIC-code-based peer performance decreases from a significant $+0.01, to $-0.03, to $-0.08, using OLS. Using fixed effects, the pay to peer performance sensitivities decrease from a significant $+0.01, to $-0.03, to $-0.06. Similarly, using the market-index-based benchmark, I find that the parameter estimates decrease from a significant $+0.07, to $-0.08, to a significant $-0.23, using OLS. Using fixed effects, the pay to peer performance sensitivities decrease from a significant $+0.04, to a significant $-0.10, to a significant $-0.24. Regardless of the estimation method or peer-return benchmark used, the high *GTA* firm parameter estimates are significantly greater than the low *GTA* firm parameter estimates.

The results from table 3.6, in which the dependent variables are after-tax, certainty equivalent total compensation (panel A) and after-tax, certainty equivalent cash compensation (panel B) also support the managerial hypothesis. For example, in panel A, I find that, moving from high to median to low *GTA* firms, the SIC-code-based parameter estimates decrease from a significant $+0.02, to $-0.09, to $-0.19, using OLS. Using fixed effects, the estimates decrease from $-0.01, to $-0.08, to $-0.15. Using the *CRSP* market index returns as the performance benchmark I find estimates that, moving from high to median to low *GTA* firms, decrease from a significant $+0.07, to $-0.02, to, $-0.10, using OLS. Using fixed effects, the estimates decrease from a significant $-0.03, to $-0.16, to, $-0.29. Once again, the high *GTA* parameter estimates are significantly greater than the low *GTA* parameter estimates. In panel B, I find that moving from high to median to low *GTA* firms, the SIC-code-based parameter estimates decrease from a significant $+0.01, to $-0.02, to $-0.05, using OLS. Using fixed effects, the estimates decrease from $0.00, to $-0.02, to $-0.04. Using the *CRSP* market index returns as the performance benchmark I find estimates of a significant $+0.04, $-0.05, and a significant $-0.14, moving from high to median to low *GTA* firms, using OLS. Using fixed effects, the estimates decrease from a significant $+0.02, to a significant $-0.06, to a significant $-0.15. The high *GTA* parameter estimates are significantly greater than the low *GTA* parameter estimates in each case.

In table 3.7, I present the results from when I use the firm's number of employees, *EMPS*, to proxy for managerial ability. Once again, the results support the managerial ability hypothesis in that I find that the impact of peer firm stock returns on executive pay is decreasing in the executive's managerial ability level. For example, in panel A, in which the dependent variable is total compensation, I find that, moving from high to median to low *EMPS* firms, the SIC-code-based parameter estimates decrease from a significant $-0.04, to $-0.11, to $-0.17, using fixed effects. Similarly, when the *CRSP* market index returns are the performance benchmark, the parameter estimates decrease from $+0.07, to a significant $-0.45, to a significant $-0.97. In panel B, in which the dependent variable is cash compensation, I find that moving from high to median to low *EMPS* firms, the SIC-code-based parameter estimates decrease from a significant $+0.01, to $-0.02, to $-0.05, while the market-index-based estimates decrease from a significant $+0.03, to $-0.03, to $-0.08. Once again, the high *EMPS* parameter estimates are significantly greater than the low *EMPS* parameter estimates.

In table 3.8, I change the dependent variables from total compensation and cash compensation to their after-tax, certainty-equivalent values estimates (representing panels A and B, respectively). Despite the change, the results continue to support the managerial ability hypothesis in that I find the impact of peer firm returns on executive pay is decreasing in the executive's managerial ability level. For example, in panel A, moving from high to median to low *EMPS* firms, the SIC-code-based parameter estimates decrease from a significant $-0.01, to $-0.08, to $-0.15, using fixed effects. The market-index-based estimates decrease from a significant $+0.06, to a significant $-0.30, to a significant $-0.66. In panel B, I find that moving from high to median to low *EMPS* firms, the SIC-code-based parameter estimates decrease from $0.00, to $-0.01, to $-0.03, while the market-index-based estimates decrease from a significant $+0.02, to $-0.02, to $-0.05, with the high *EMPS* parameter estimates significantly greater than the low *EMPS* parameter estimates in each case.

Overall, the results from each table demonstrate that the impact of peer firm returns on executive pay is decreasing in the executive's ability level. These findings are consistent with the idea that larger firms increase their payments to their higher-ability executives when stock prices in general are rising.

3.5 Conclusion

Executive compensation theory predicts a negative relation between executive pay and peer firm stock returns. Nevertheless, empirical tests yield conflicting results. Interest is increasing in research dedicated to solving this unsolved "puzzle" [Aggarwal and Samwick (1999), Bizjak, Lemmon, and Naveen (2004), Garvey and Milbourn (2003), Himmelberg and Hubbard (2000), Oyer (2004), Rajgopal, Shevlin, and Zamora (2006)].

In the spirit of these studies, I test two alternative hypotheses regarding the impact of peer firm stock returns on executive pay: (i) the noisy evaluation hypothesis, and (ii) the managerial ability hypothesis. The noisy evaluation hypothesis predicts that peer firm stock returns have a greater (more negative) impact on executive compensation in high beta firms. The managerial ability hypothesis predicts that peer firm stock returns have a greater (more negative) impact on the compensation of lower-ability executives.

Based on a sample of 323 CEOs from 1996 to 2000, I find evidence supporting both hypotheses. Specifically, I find that peer firm stock returns have a lesser impact on CEO pay when: (i) the firm's stock return sensitivity to systematic risk is low, and (ii) the executive's managerial ability level is high. Consistent with theories of relative performance evaluation developed in Holmstrom (1979), Lazear and Rosen (1981), Himmelberg and Hubbard (2000), and Oyer (2004), these findings suggest that firms practice relative performance evaluation selectively — when it is more or less consistent with the goals of agency problem resolution and incentive maximization. The findings also suggest that firm- and executive- specific characteristics can counteract the incentive effects of peer-return-based performance criteria.

This study leaves several areas for future research. First, a similar study of the relationship between executive pay and peer firm *accounting* performance would be useful since executive performance is typically not only evaluated using stock returns. A finding of a significant relationship would not be surprising since employment agreements frequently tie annual bonuses to accounting-based performance measures, rather than stock performance. Second, a re-examination of the results from earlier studies of the relation between executive pay and peer firm performance seems justified given our updated understanding of the difference between value of stock options and restricted stock grants to executives and their cost to the firm.

Table 3.1 — Relative performance evaluation (RPE) in annual incentive plans in 177 large US corporations.[10]

	Industrials (n=125)	Finance/Insurance (n=21)	Utilities (n=31)
Number of Firms Using RPE	26 (21%)	12 (57%)	13 (42%)
How RPE is Used:			
Determining Threshold Performance[a]	0%	0%	30%
Defining Performance	100%	100%	70%
How RPE is Measured:			
Performance Percentile or Ranking[b]	42%	50%	77%
Performance vs. Peer Group Mean/Median	19%	0%	0%
Subjective Assessment of Perf. Vs. Peers	15%	8%	0%
Unknown/Other	23%	42%	23%
How Peer Groups are Defined:			
Peer Group used in Proxy Statement[c]	20%	25%	0%
Self-Selected Industry Peer Group	58%	75%	85%
Published Industry Index	8%	0%	8%
Broad-Based Peer Group	15%	0%	8%

Source: Data extracted from Towers Perrin's *Annual Incentive Plan Design Survey*, 1997.

[a]Threshold performance must be attained before any bonuses are paid. The threshold performance measure is often different from the performance measure that determines the magnitude of bonuses. For example, bonuses equal to 2% of net income might be paid only if company return on equity exceeds the peer group return on equity: the performance measure of net income but the threshold major is relative return on equity.

[b]A typical formula might pay "75th percentile bonuses for 75th percentile performance."

[c]Proxy statements include a chart showing the company's five-year stock-price performance measured relative to either the market or an "industry peer group" which may be self-selected or a published industry index.

[10]Reproduced from Murphy (1999) table 9. Towers Perrin declined my request for updated information.

Table 3.2 — Key statistics for sample and non-sample firms.
This table presents means, medians, standard deviations, and number of observations for the major variables used in the study. Sample firms include the 323 firms that met the selection criteria described in Section 3.3. Non-sample firms include all of the other firms appearing contemporaneously on the *EXECUCOMP* database. Differences are tested for statistical significance using the Wilcoxon rank sum test.

	------------------Sample firms------------------				---------------Non-sample firms---------------				-----Difference-----	
Variable	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N	Mean	Median
Experience (years)										
1996	7.97	6.00	7.29	323	7.06	5.00	7.32	1,134	0.91*	1.00*
1997	8.97	7.00	7.29	323	6.85	4.00	7.44	1,167	2.11*	3.00*
1998	9.97	8.00	7.29	323	6.48	4.00	7.26	1,238	3.49*	4.00*
1999	10.97	9.00	7.29	323	6.23	4.00	7.01	1,318	4.73*	5.00*
2000	11.97	10.00	7.29	323	5.65	3.00	6.69	1,337	6.32*	7.00*
Total compensation ($000's)										
1996	3,031.97	1,632.50	4,049.58	321	3,173.11	1,567.28	8,064.46	1,321	-141.15	65.22
1997	4,224.64	2,049.87	8,855.40	322	3,733.92	1,891.87	7,219.48	1,342	490.72	158.00
1998	4,118.67	2,370.00	6,494.34	323	4,581.17	1,898.03	19,637.08	1,401	-462.50*	471.97*
1999	5,802.74	2,596.55	9,721.97	322	5,097.85	2,098.52	12,309.95	1,477	704.89*	498.03*
2000	6,300.67	3,139.16	10,263.16	322	6,781.77	2,288.78	22,973.85	1,460	-481.10*	850.38*
Annual stock return										
1996	21.9%	18.6%	36.2%	303	27.1%	20.3%	51.6%	1,618	-5.2%	-1.7%
1997	30.5%	32.2%	34.5%	304	32.2%	28.6%	52.7%	1,635	-1.7%	3.6%
1998	6.8%	1.6%	45.1%	308	17.7%	3.3%	75.6%	1,595	-10.9%	-1.7%
1999	9.3%	-7.3%	59.5%	308	42.7%	2.2%	148.8%	1,511	-33.4%*	-9.5%*
2000	26.6%	19.1%	64.8%	310	15.9%	5.3%	72.5%	1,420	10.7%*	13.8%*

*Significantly different from zero at the 5% level.

Table 3.2 — Key statistics for sample and non-sample firms, continued.

	Sample firms				Non-sample firms				Difference	
Variable	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N	Mean	Median
Beta										
1996	0.97	0.82	0.69	303	1.25	1.02	0.98	1,620	-0.28*	-0.20*
1997	0.93	0.78	0.56	305	1.19	0.97	0.79	1,725	-0.25*	-0.19*
1998	1.12	1.08	0.51	308	1.32	1.19	0.83	1,797	-0.21*	-0.11*
1999	1.05	1.05	0.48	309	1.22	1.16	0.63	1,759	-0.17*	-0.11*
2000	1.01	0.98	0.53	311	1.29	1.09	0.96	1,704	-0.28*	-0.11*
Gross total assets ($000's)										
1996	7,547.58	1,356.17	22,999.72	284	4,146.76	608.76	16,603.62	1,718	3,400.82*	747.41*
1997	8,280.72	1,511.49	25,829.07	289	4,392.63	700.90	17,662.86	1,774	3,888.09*	810.59*
1998	9,097.19	1,702.70	28,304.45	291	4,979.47	807.84	21,016.97	1,720	4,117.72*	894.86*
1999	10,749.18	1,965.86	35,207.84	289	5,507.09	925.11	22,937.69	1,660	5,242.09*	1,040.75*
2000	12,816.70	2,266.22	42,913.98	289	6,454.47	1,103.34	26,926.83	1,548	6,362.23*	1,162.88*
Employees (000's)										
1996	15.11	5.64	28.80	308	13.31	3.28	41.46	1,701	1.80*	2.36*
1997	16.05	6.07	30.43	310	13.44	3.40	41.34	1,786	2.60*	2.67*
1998	17.35	6.56	33.55	312	14.27	3.70	42.57	1,760	3.09*	2.86*
1999	18.27	7.18	35.40	308	15.42	4.06	44.56	1,685	2.85*	3.12*
2000	19.30	7.40	38.25	313	16.10	4.19	47.42	1,617	3.19*	3.21*

Table 3.3 — Tests of the noisy evaluation hypothesis.
Coefficients from ordinary least squares regressions of executive compensation on own- and peer-firm performance. The dependent variable is total compensation in panel A and cash compensation in panel B. Own and peer performance represents the own-stock-return based change in shareholder wealth and the peer-stock-return based change in shareholder wealth, respectively. Peer stock returns are represented by either the average return of firms within the sample firm's three digit SIC code or firms comprising the *CRSP* market index. Each panel presents the results of three sets of regressions, corresponding to estimation of equations (3.5) through (3.7) wherein which the variable, *F*(*P*), is the c.d.f. of the firm's beta coefficient (*BETA*). Standard errors are in parenthesis.

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel A: Dependent variable is total compensation						
Own performance	0.2191*	0.3290*	0.2311*	0.2564*	0.0103	-0.0145
	(0.0593)	(0.0561)	(0.0751)	(0.0710)	(0.0570)	(0.0582)
Peer performance	0.2881*	0.4274*	0.0612	0.0667	0.0283	-0.1725*
	(0.0363)	(0.0710)	(0.0413)	(0.0844)	(0.0367)	(0.0767)
Own performance x *F(BETA)*	0.0560	-0.1707*	-0.3464*	-0.4429*	-0.0330	-0.0733
	(0.0911)	(0.0852)	(0.1131)	(0.1053)	(0.0845)	(0.0807)
Peer performance x *F(BETA)*	-0.5856*	-0.2872**	-0.2270*	0.3672*	-0.1342**	0.4275*
	(0.0724)	(0.1506)	(0.0873)	(0.1840)	(0.0726)	(0.1464)
R^2	0.3768	0.3779	0.0401	0.0509	0.5396	0.5393
Number of observations	1,464	1,464	1,169	1,169	1,464	1,464
Hypotheses tests at the low, median, and high values of *F(BETA)*						
Own performance						
Low, *F(BETA)*=0.0	0.2191*	0.3290*	0.2311*	0.2564*	0.0103	-0.0145
	(0.0593)	(0.0561)	(0.0751)	(0.0710)	(0.0570)	(0.0582)
Median, *F(BETA)*=0.5	0.2471*	0.2436*	0.0579**	0.0349	-0.0062	-0.0512**
	(0.0260)	(0.0251)	(0.0351)	(0.0327)	(0.0300)	(0.0310)
High, *F(BETA)*=1.0	0.2751*	0.1583*[a]	-0.1153*[a]	-0.1865*[a]	-0.0227	-0.0878*[a]
	(0.0446)	(0.0418)	(0.0568)	(0.0514)	(0.0460)	(0.0424)
Peer performance						
Low, *F(BETA)*=0.0	0.2881*	0.4274*	0.0612	0.0667	0.0283	-0.1725*
	(0.0363)	(0.0710)	(0.0413)	(0.0844)	(0.0367)	(0.0767)
Median, *F(BETA)*=0.5	-0.0047	0.2838*	-0.0523*	0.2503*	-0.0388*	0.0413
	(0.0164)	(0.0413)	(0.0214)	(0.0524)	(0.0142)	(0.0383)
High, *F(BETA)*=1.0	-0.2975*[a]	0.1401[a]	-0.1658*[a]	0.4339*[a]	-0.1059*[a]	0.2550*[a]
	(0.0429)	(0.0985)	(0.0549)	(0.1237)	(0.0410)	(0.0882)

Regression coefficients	Dependent variable in levels (Eq. 3.5) 3-digit	market	Dependent variable in differences (Eq. 3.6) 3-digit	market	Two-way fixed effects (Eq. 3.7) 3-digit	market
Panel B: Dependent variable is cash compensation						
Own performance	0.0315*	0.0557*	0.0112	0.0138**	-0.0132	0.0010
	(0.0113)	(0.0105)	(0.0085)	(0.0076)	(0.0082)	(0.0081)
Peer performance	0.0724*	0.1727*	0.0013	0.1110*	0.0263*	0.1112*
	(0.0069)	(0.0132)	(0.0047)	(0.0091)	(0.0053)	(0.0106)
Own performance x *F(BETA)*	0.0528*	0.0072	0.0091	0.0015	0.0427*	0.0218**
	(0.0174)	(0.0159)	(0.0128)	(0.0113)	(0.0121)	(0.0112)
Peer performance x *F(BETA)*	-0.1338*	-0.2320*	-0.0119	-0.1719*	-0.0489*	-0.1828*
	(0.0139)	(0.0281)	(0.0099)	(0.0198)	(0.0104)	(0.0202)
R^2	0.6042	0.6250	0.0485	0.1588	0.7591	0.7753
Number of observations	1,464	1,464	1,169	1,169	1,464	1,464
Hypotheses tests at the low, median, and high values of *F(BETA)*						
Own performance						
Low, *F(BETA)*=0.0	0.0315*	0.0557*	0.0112	0.0138**	-0.0132	0.0010
	(0.0113)	(0.0105)	(0.0085)	(0.0076)	(0.0082)	(0.0081)
Median, *F(BETA)*=0.5	0.0579*	0.0593*	0.0157*	0.0145*	0.0081**	0.0209*
	(0.0050)	(0.0047)	(0.0040)	(0.0035)	(0.0043)	(0.0043)
High, *F(BETA)*=1.0	0.0843*[a]	0.0629*	0.0203*[b]	0.0153*	0.0295*[a]	0.0318*[a]
	(0.0085)	(0.0078)	(0.0064)	(0.0055)	(0.0066)	(0.0059)
Peer performance						
Low, *F(BETA)*=0.0	0.0724*	0.1727*	0.0013	0.1110*	0.0263*	0.1112*
	(0.0069)	(0.0132)	(0.0047)	(0.0091)	(0.0053)	(0.0106)
Median, *F(BETA)*=0.5	0.0055**	0.0568*	-0.0046**	0.0250*	0.0018	0.0198*
	(0.0031)	(0.0077)	(0.0024)	(0.0056)	(0.0020)	(0.0053)
High, *F(BETA)*=1.0	-0.0614*[a]	-0.0592*[a]	-0.0105**[a]	-0.0609*[a]	-0.0227*[a]	-0.0715*[a]
	(0.0082)	(0.0184)	(0.0062)	(0.0133)	(0.0059)	(0.0122)

* Significantly different from zero at the 5% level.
** Significantly different from zero at the 10% level.
[a] Significantly different from the low *F(P)* estimate at the 5% level.
[b] Significantly different from the low *F(P)* estimate at the 10% level.

Table 3.4 — Tests of the noisy evaluation hypothesis with after-tax certainty, equivalent compensation.
Coefficients from ordinary least squares regressions of after-tax, certainty equivalent executive compensation on own- and peer-firm performance. The dependent variable is after-tax, certainty equivalent total compensation in panel A, and after-tax, certainty equivalent cash compensation in panel B. Own and peer performance represents the own-stock-return based change in shareholder wealth and the peer-stock-return based change in shareholder wealth, respectively. Peer stock returns are represented by either the average return of firms within the sample firm's three digit SIC code or firms comprising the *CRSP* market index. Each panel presents the results of three sets of regressions, corresponding to estimation of equations (3.5) through (3.7) wherein which the variable, *F*(*P*), is the c.d.f. of the firm's beta coefficient (*BETA*). Standard errors are in parenthesis.

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel A: Dependent variable is after-tax certainty equivalent total compensation						
Own performance	0.1081*	0.1638*	0.0922*	0.0993*	-0.0058	-0.0135
	(0.0309)	(0.0292)	(0.0401)	(0.0379)	(0.0301)	(0.0307)
Peer performance	0.1464*	0.2142*	0.0227	0.0133	0.0165	-0.0844*
	(0.0189)	(0.0369)	(0.0220)	(0.0450)	(0.0194)	(0.0404)
Own performance x *F(BETA)*	0.0381	-0.0766**	-0.1126**	-0.1545*	0.0075	-0.0144
	(0.0475)	(0.0443)	(0.0603)	(0.0562)	(0.0446)	(0.0425)
Peer performance x *F(BETA)*	-0.2939*	-0.1155	-0.1043*	0.2290*	-0.0640**	0.2425*
	(0.0378)	(0.0783)	(0.0465)	(0.0982)	(0.0384)	(0.0771)
R^2	0.3692	0.3733	0.0284	0.0386	0.5233	0.5256
Number of observations	1,464	1,464	1,169	1,169	1,464	1,464
Hypotheses tests at the low, median, and high values of *F(BETA)*						
Own performance						
Low, *F(BETA)*=0.0	0.1081*	0.1638*	0.0922*	0.0993*	-0.0058	-0.0135
	(0.0309)	(0.0292)	(0.0401)	(0.0379)	(0.0301)	(0.0307)
Median, *F(BETA)*=0.5	0.1271*	0.1255*	0.0360**	0.0221	-0.0020	-0.0207
	(0.0136)	(0.0130)	(0.0187)	(0.0174)	(0.0158)	(0.0164)
High, *F(BETA)*=1.0	0.1462*[b]	0.0872*[a]	-0.0203[a]	-0.0552*[a]	0.0017	-0.0279
	(0.0233)	(0.0217)	(0.0303)	(0.0274)	(0.0243)	(0.0223)
Peer performance						
Low, *F(BETA)*=0.0	0.1464*	0.2142*	0.0227	0.0133	0.0165	-0.0844*
	(0.0189)	(0.0369)	(0.0220)	(0.0450)	(0.0194)	(0.0404)
Median, *F(BETA)*=0.5	-0.0006	0.1565*	-0.0295*	0.1278*	-0.0155*	0.0369**
	(0.0085)	(0.0215)	(0.0114)	(0.0279)	(0.0075)	(0.0202)
High, *F(BETA)*=1.0	-0.1475*[a]	0.0987**[a]	-0.0816*[a]	0.2423*[a]	-0.0475*[a]	0.1581*[a]
	(0.0224)	(0.0512)	(0.0293)	(0.0660)	(0.0217)	(0.0465)

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel B: Dependent variable is after-tax certainty equivalent cash compensation						
Own performance	0.0190*	0.0337*	0.0068	0.0083**	-0.0080	0.0060
	(0.0069)	(0.0063)	(0.0052)	(0.0046)	(0.0049)	(0.0049)
Peer performance	0.0437*	0.1043*	0.0008	0.0670*	0.0159*	0.0672*
	(0.0042)	(0.0080)	(0.0028)	(0.0055)	(0.0032)	(0.0064)
Own performance x *F(BETA)*	0.0319*	0.0043	0.0055	0.0009	0.0258*	0.0132**
	(0.0105)	(0.0096)	(0.0078)	(0.0068)	(0.0073)	(0.0067)
Peer performance x *F(BETA)*	-0.0808*	-0.1401*	-0.0072	-0.1038*	-0.0296*	-0.1104*
	(0.0084)	(0.0170)	(0.0060)	(0.0119)	(0.0063)	(0.0122)
R^2	0.6042	0.6250	0.0485	0.1588	0.7591	0.7753
Number of observations	1,464	1,464	1,169	1,169	1,464	1,464
Hypotheses tests at the low, median, and high values of *F(BETA)*						
Own performance						
Low, *F(BETA)*=0.0	0.0190*	0.0337*	0.0068	0.0083**	-0.0080	0.0060
	(0.0069)	(0.0063)	(0.0052)	(0.0046)	(0.0049)	(0.0049)
Median, *F(BETA)*=0.5	0.0350*	0.0358*	0.0095*	0.0088*	0.0049**	0.01262*
	(0.0030)	(0.0028)	(0.0024)	(0.0021)	(0.0026)	(0.0026)
High, *F(BETA)*=1.0	0.0509*[a]	0.0380*	0.0123*[b]	0.0092*	0.0178*[a]	0.0192*[a]
	(0.0052)	(0.0047)	(0.0039)	(0.0033)	(0.0040)	(0.0035)
Peer performance						
Low, *F(BETA)*=0.0	0.0437*	0.1043*	0.0008	0.0670*	0.0159*	0.0672*
	(0.0042)	(0.0080)	(0.0028)	(0.0055)	(0.0032)	(0.0064)
Median, *F(BETA)*=0.5	0.0033**	0.0343*	-0.0028**	0.0151*	0.00109	0.0120*
	(0.0019)	(0.0047)	(0.0015)	(0.0034)	(0.0012)	(0.0032)
High, *F(BETA)*=1.0	-0.0371*[a]	-0.0358*[a]	-0.0064**[a]	-0.0368*[a]	-0.0137*[a]	-0.0432*[a]
	(0.0050)	(0.0111)	(0.0038)	(0.0080)	(0.0036)	(0.0074)

* Significantly different from zero at the 5% level.
** Significantly different from zero at the 10% level.
[a] Significantly different from the low *F(P)* estimate at the 5% level.
[b] Significantly different from the low *F(P)* estimate at the 10% level.

Table 3.5 — Tests of the managerial ability hypothesis using gross total assets

Coefficients from ordinary least squares regressions of executive compensation on own- and peer-firm performance. The dependent variable is total compensation in panel A, and cash compensation in panel B. Own and peer performance represents the own-stock-return based change in shareholder wealth and the peer-stock-return based change in shareholder wealth, respectively. Peer stock returns are represented by either the average return of firms within the sample firm's three digit SIC code or firms comprising the *CRSP* market index. Each panel presents the results of three sets of regressions, corresponding to estimation of equations (3.5) through (3.7) wherein which the variable, *F*(*P*), is the c.d.f. of the firm's gross total assets (*GTA*). Standard errors are in parenthesis.

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel A: Dependent variable is total compensation						
Own performance	0.3468	0.2862	0.8511*	0.9050*	0.6802*	0.7278*
	(0.2446)	(0.2318)	(0.3011)	(0.2808)	(0.2888)	(0.2648)
Peer performance	-0.2326	0.0184	-0.2900	-1.7238**	-0.1275	-0.5000
	(0.3113)	(0.7435)	(0.3733)	(0.8993)	(0.2827)	(0.6720)
Own performance x *F(GTA)*	-0.1757	-0.1147	-0.9332*	-0.9979*	-0.7345*	-0.8176*
	(0.2571)	(0.2447)	(0.3204)	(0.2987)	(0.3049)	(0.2818)
Peer performance x *F(GTA)*	0.2544	0.1134	0.2790	1.8305*	0.1003	0.4088
	(0.3197)	(0.7638)	(0.3838)	(0.9243)	(0.2905)	(0.6911)
R^2	0.4378	0.4420	0.0369	0.0403	0.5603	0.5614
Number of observations	1,306	1,306	1,044	1,044	1,306	1,306
Hypotheses tests at the low, median, and high values of *F(GTA)*						
Own performance						
Low, *F(GTA)*=0.0	0.3468	0.2862	0.8511*	0.9050*	0.6802*	0.7278*
	(0.2446)	(0.2318)	(0.3011)	(0.2808)	(0.2888)	(0.2648)
Median, *F(GTA)*=0.5	0.2590*	0.2288*	0.3845*	0.4061*	0.3130*	0.3190*
	(0.1174)	(0.1108)	(0.1430)	(0.1334)	(0.1377)	(0.1256)
High, *F(GTA)*=1.0	0.1712*[a]	0.1714*[a]	-0.0822*[a]	-0.0928*[a]	-0.0543**[a]	-0.0898*[a]
	(0.0278)	(0.0273)	(0.0390)	(0.0368)	(0.0325)	(0.0335)
Peer performance						
Low, *F(GTA)*=0.0	-0.2326	0.0184	-0.2900	-1.7238**	-0.1275	-0.5000
	(0.3113)	(0.7435)	(0.3733)	(0.8993)	(0.2827)	(0.6720)
Median, *F(GTA)*=0.5	-0.1054	0.0751	-0.1505	-0.8086**	-0.0774	-0.2955
	(0.1518)	(0.3627)	(0.1819)	(0.4386)	(0.1378)	(0.3275)
High, *F(GTA)*=1.0	0.0218[a]	0.1318*[a]	-0.0110[a]	0.1067**[a]	-0.0273**[a]	-0.0911*[a]
	(0.0174)	(0.0438)	(0.0221)	(0.0552)	(0.0157)	(0.0430)

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel B: Dependent variable is cash compensation						
Own performance	-0.0262	-0.0219	0.0608	0.0939*	0.1292*	0.0967*
	(0.0470)	(0.0437)	(0.0375)	(0.0336)	(0.0449)	(0.0406)
Peer performance	-0.0755	-0.2316**	0.0078	-0.4070*	-0.0640	-0.2425*
	(0.0598)	(0.1400)	(0.0465)	(0.1077)	(0.0439)	(0.1031)
Own performance x *F(GTA)*	0.0728	0.0681	-0.0504	-0.0851*	-0.1322*	-0.0900*
	(0.0494)	(0.0461)	(0.0399)	(0.0358)	(0.0474)	(0.0432)
Peer performance x *F(GTA)*	0.08730	0.3012*	-0.0114	0.4700*	0.0697	0.2807*
	(0.0614)	(0.1438)	(0.0478)	(0.1107)	(0.0451)	(0.1061)
R^2	0.6448	0.6613	0.0466	0.1220	0.7419	0.7487
Number of observations	1,306	1,306	1,044	1,044	1,306	1,306
Hypotheses tests at the low, median, and high values of *F(GTA)*						
Own performance						
Low, *F(GTA)*=0.0	-0.0262	-0.0219	0.0608	0.0939*	0.1292*	0.0967*
	(0.0470)	(0.0437)	(0.0375)	(0.0336)	(0.0449)	(0.0406)
Median, *F(GTA)*=0.5	0.0102	0.0122	0.0357*	0.0514*	0.0631*	0.0517*
	(0.0226)	(0.0209)	(0.0178)	(0.0160)	(0.0214)	(0.0193)
High, *F(GTA)*=1.0	0.0467*[a]	0.0462*[a]	0.0105*[a]	0.0089*[a]	-0.0030[a]	0.0068[a]
	(0.0053)	(0.0051)	(0.0049)	(0.0044)	(0.0051)	(0.0052)
Peer performance						
Low, *F(GTA)*=0.0	-0.0755	-0.2316**	0.0078	-0.4070*	-0.0640	-0.2425*
	(0.0598)	(0.1400)	(0.0465)	(0.1077)	(0.0439)	(0.1031)
Median, *F(GTA)*=0.5	-0.0318	-0.0810	0.0021	-0.1720*	-0.0291	-0.1021*
	(0.0292)	(0.0683)	(0.0227)	(0.0525)	(0.0214)	(0.0503)
High, *F(GTA)*=1.0	0.0118*[a]	0.0696*[a]	-0.0036[a]	0.0630*[a]	0.0057*[a]	0.0383*[a]
	(0.0033)	(0.0082)	(0.0028)	(0.0066)	(0.0024)	(0.0066)

* Significantly different from zero at the 5% level.
** Significantly different from zero at the 10% level.
[a] Significantly different from the low *F(P)* estimate at the 5% level.

Table 3.6 — Tests of the managerial ability hypothesis using gross total assets and after-tax certainty equivalent compensation

Coefficients from ordinary least squares regressions of executive compensation on own- and peer-firm performance. The dependent variable is after-tax, certainty equivalent total compensation in panel A, and after-tax, certainty equivalent cash compensation in panel B. Own and peer performance represents the own-stock-return based change in shareholder wealth and the peer-stock-return based change in shareholder wealth, respectively. Peer stock returns are represented by either the average return of firms within the sample firm's three digit SIC code or firms comprising the *CRSP* market index. Each panel presents the results of three sets of regressions, corresponding to estimation of equations (3.5) through (3.7) wherein which the variable, *F*(*P*), is the c.d.f. of the firm's gross total assets (*GTA*). Standard errors are in parenthesis.

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel A: Dependent variable is after-tax certainty equivalent total compensation						
Own performance	0.1264	0.0812	0.4185*	0.4406*	0.3570*	0.3415*
	(0.1237)	(0.1172)	(0.1552)	(0.1447)	(0.1481)	(0.1359)
Peer performance	-0.1924	-0.1027	-0.2565	-1.1892*	-0.1464	-0.2867
	(0.1574)	(0.3761)	(0.1923)	(0.4635)	(0.1450)	(0.3448)
Own performance x *F(GTA)*	-0.0310	0.0153	-0.4391*	-0.4692*	-0.3751*	-0.3730*
	(0.1300)	(0.1238)	(0.1651)	(0.1540)	(0.1565)	(0.1446)
Peer performance x *F(GTA)*	0.2077	0.1735	0.2496	1.2365*	0.1395	0.2493
	(0.1616)	(0.3863)	(0.1978)	(0.4764)	(0.1490)	(0.3546)
R^2	0.4445	0.4481	0.0324	0.0355	0.5507	0.5518
Number of observations	1,306	1,306	1,044	1,044	1,306	1,306
Hypotheses tests at the low, median, and high values of *F(GTA)*						
Own performance						
Low, *F(GTA)*=0.0	0.1264	0.0812	0.4185*	0.4406*	0.3570*	0.3415*
	(0.1237)	(0.1172)	(0.1552)	(0.1447)	(0.1481)	(0.1359)
Median, *F(GTA)*=0.5	0.1109**	0.0888	0.1990*	0.2060*	0.1695*	0.1550*
	(0.0594)	(0.0560)	(0.0737)	(0.0687)	(0.0707)	(0.0645)
High, *F(GTA)*=1.0	0.0954*[a]	0.0965*	-0.0205[a]	-0.0286[a]	-0.0181[a]	-0.0315**[a]
	(0.0141)	(0.0138)	(0.0201)	(0.0190)	(0.0167)	(0.0172)
Peer performance						
Low, *F(GTA)*=0.0	-0.1924	-0.1027	-0.2565	-1.1892*	-0.1464	-0.2867
	(0.1574)	(0.3761)	(0.1923)	(0.4635)	(0.1450)	(0.3448)
Median, *F(GTA)*=0.5	-0.0886	-0.0159	-0.1317	-0.5709*	-0.0766	-0.1620
	(0.0767)	(0.1834)	(0.0937)	(0.2261)	(0.0707)	(0.1681)
High, *F(GTA)*=1.0	0.0153**[a]	0.0709*[a]	-0.0069[a]	0.0474**[a]	-0.0069[a]	-0.0374**[a]
	(0.0088)	(0.0221)	(0.0114)	(0.0285)	(0.00806)	(0.0221)

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel B: Dependent variable is after-tax certainty equivalent cash compensation						
Own performance	-0.0158	-0.0132	0.0367	0.0567*	0.0780*	0.0584*
	(0.0284)	(0.0264)	(0.0227)	(0.0203)	(0.0271)	(0.0246)
Peer performance	-0.0456	-0.1399**	0.0047	-0.2458*	-0.0386	-0.1465*
	(0.0361)	(0.0846)	(0.0281)	(0.0651)	(0.0265)	(0.0623)
Own performance x *F(GTA)*	0.0440	0.0411	-0.0304	-0.0514*	-0.0798*	-0.0543*
	(0.0298)	(0.0278)	(0.0241)	(0.0216)	(0.0286)	(0.0261)
Peer performance x *F(GTA)*	0.0527	0.1820*	-0.0069	0.2839*	0.0421	0.1696*
	(0.0371)	(0.0869)	(0.0289)	(0.0669)	(0.0273)	(0.0641)
R^2	0.6448	0.6613	0.0466	0.1220	0.7419	0.7487
Number of observations	1,306	1,306	1,044	1,044	1,306	1,306
Hypotheses tests at the low, median, and high values of *F(GTA)*						
Own performance						
Low, *F(GTA)*=0.0	-0.0158	-0.0132	0.0367	0.0567*	0.0780*	0.0584*
	(0.0284)	(0.0264)	(0.0227)	(0.0203)	(0.0271)	(0.0246)
Median, *F(GTA)*=0.5	0.0062	0.0073	0.0215*	0.0310*	0.0381*	0.0313*
	(0.0136)	(0.0126)	(0.0108)	(0.0097)	(0.0129)	(0.0116)
High, *F(GTA)*=1.0	0.0282*[a]	0.0279*[a]	0.0063*[a]	0.0054*[a]	-0.0018[a]	0.0041[a]
	(0.0032)	(0.0031)	(0.0029)	(0.0027)	(0.0031)	(0.0031)
Peer performance						
Low, *F(GTA)*=0.0	-0.0456	-0.1399**	0.0047	-0.2458*	-0.0386	-0.1465*
	(0.0361)	(0.0846)	(0.0281)	(0.0651)	(0.0265)	(0.0623)
Median, *F(GTA)*=0.5	-0.0192	-0.0489	0.0013	-0.1039*	-0.0176	-0.0617*
	(0.0176)	(0.0413)	(0.0137)	(0.0317)	(0.0129)	(0.0304)
High, *F(GTA)*=1.0	0.0071*[a]	0.0421*[a]	-0.0022[a]	0.0381*[a]	0.0035*[a]	0.0231*[a]
	(0.0020)	(0.0050)	(0.0017)	(0.0040)	(0.0015)	(0.0040)

* Significantly different from zero at the 5% level.
** Significantly different from zero at the 10% level.
[a] Significantly different from the low *F(P)* estimate at the 5% level.

Table 3.7 — Tests of the managerial ability hypothesis using number of employees

Coefficients from ordinary least squares regressions of executive compensation on own- and peer-firm performance. The dependent variable is total compensation in panel A, and cash compensation in panel B. Own and peer performance represents the own-stock-return based change in shareholder wealth and the peer-stock-return based change in shareholder wealth, respectively. Peer stock returns are represented by either the average return of firms within the sample firm's three digit SIC code or firms comprising the *CRSP* market index. Each panel presents the results of three sets of regressions, corresponding to estimation of equations (3.5) through (3.7) wherein which the variable, *F*(*P*), is the c.d.f. of the firm's number of employees (*EMPS*). Standard errors are in parenthesis.

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel A: Dependent variable is total compensation						
Own performance	0.4705*	0.6211*	0.6008*	0.6804*	0.71832*	0.6482*
	(0.2046)	(0.1787)	(0.2650)	(0.2294)	(0.2474)	(0.2062)
Peer performance	0.1153	-0.8114	-0.0666	-1.4057**	-0.1744	-0.9673**
	(0.2452)	(0.5868)	(0.3157)	(0.7619)	(0.2317)	(0.5575)
Own performance x *F(EMPS)*	-0.3065	-0.4745*	-0.6734*	-0.7603*	-0.7850*	-0.7113*
	(0.2173)	(0.1914)	(0.2860)	(0.2488)	(0.2629)	(0.2221)
Peer performance x *F(EMPS)*	-0.1155	1.0986**	0.0411	1.6528*	0.1357	1.0406**
	(0.2514)	(0.6072)	(0.3244)	(0.7895)	(0.2374)	(0.5777)
R^2	0.4226	0.4414	0.0284	0.0425	0.5293	0.5272
Number of observations	1,404	1,404	1,117	1,117	1,404	1,404
Hypotheses tests at the low, median, and high values of *F(EMPS)*						
Own performance						
Low, *F(EMPS)*=0.0	0.4705*	0.6211*	0.6008*	0.6804*	0.71832*	0.6482*
	(0.2046)	(0.1787)	(0.2650)	(0.2294)	(0.2474)	(0.2062)
Median, *F(EMPS)*=0.5	0.3173*	0.3838*	0.2641*	0.3003*	0.3258*	0.2925*
	(0.0976)	(0.0847)	(0.1247)	(0.1078)	(0.1179)	(0.0977)
High, *F(EMPS)*=1.0	0.1640*[a]	0.1466*[a]	-0.0726**[a]	-0.0799*[a]	-0.0667*[a]	-0.0632**[a]
	(0.0286)	(0.0276)	(0.0423)	(0.0398)	(0.0335)	(0.0354)
Peer performance						
Low, *F(EMPS)*=0.0	0.1153	-0.8114	-0.0666	-1.4057**	-0.1744	-0.9673**
	(0.2452)	(0.5868)	(0.3157)	(0.7619)	(0.2317)	(0.5575)
Median, *F(EMPS)*=0.5	0.0576	-0.2622	-0.0461	-0.5793	-0.1066	-0.4469**
	(0.1200)	(0.2845)	(0.1542)	(0.3690)	(0.1135)	(0.2703)
High, *F(EMPS)*=1.0	-0.0002[a]	0.2871*[a]	-0.0255[a]	0.2471*[a]	-0.0388*[a]	0.0734[a]
	(0.0170)	(0.0432)	(0.0230)	(0.0582)	(0.0156)	(0.0465)

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel B: Dependent variable is cash compensation						
Own performance	0.0326	0.0481	0.0245	0.0262	0.0991*	0.0628*
	(0.0386)	(0.0334)	(0.0276)	(0.0236)	(0.0360)	(0.0297)
Peer performance	0.0062	-0.0774	0.0027	-0.0766	-0.0500	-0.0832
	(0.0462)	(0.1097)	(0.0329)	(0.0784)	(0.0338)	(0.0804)
Own performance x *F(EMPS)*	0.0105	-0.0053	-0.0106	-0.0091	-0.1026*	-0.0551**
	(0.0410)	(0.0358)	(0.0298)	(0.0256)	(0.0383)	(0.0320)
Peer performance x *F(EMPS)*	0.0031	0.1457	-0.0001	0.1154	0.0555	0.1154
	(0.0474)	(0.1135)	(0.0338)	(0.0812)	(0.0346)	(0.0833)
R^2	0.6527	0.6700	0.0684	0.1047	0.7548	0.7587
Number of observations	1,404	1,404	1,117	1,117	1,404	1,404
Hypotheses tests at the low, median, and high values of *F(EMPS)*						
Own performance						
Low, *F(EMPS)*=0.0	0.0326	0.0481	0.0245	0.0262	0.0991*	0.0628*
	(0.0386)	(0.0334)	(0.0276)	(0.0236)	(0.0360)	(0.0297)
Median, *F(EMPS)*=0.5	0.0378*	0.0454*	0.0192	0.0216**	0.0478*	0.0353*
	(0.0184)	(0.0158)	(0.0130)	(0.0111)	(0.0172)	(0.0141)
High, *F(EMPS)*=1.0	0.0431*[a]	0.0428*	0.0139*[a]	0.0171*[a]	-0.0035[a]	0.0078[a]
	(0.0054)	(0.0052)	(0.0044)	(0.0041)	(0.0049)	(0.0051)
Peer performance						
Low, *F(EMPS)*=0.0	0.0062	-0.0774	0.0027	-0.0766	-0.0500	-0.0832
	(0.0462)	(0.1097)	(0.0329)	(0.0784)	(0.0338)	(0.0804)
Median, *F(EMPS)*=0.5	0.0077	-0.0045	0.0026	-0.0189	-0.0223	-0.0255
	(0.0226)	(0.0532)	(0.0161)	(0.0380)	(0.0165)	(0.0390)
High, *F(EMPS)*=1.0	0.0093*	0.0684*[a]	0.0026	0.0388*[a]	0.0055*[a]	0.0322*[a]
	(0.0032)	(0.0081)	(0.0024)	(0.0060)	(0.0023)	(0.0067)

* Significantly different from zero at the 5% level.
** Significantly different from zero at the 10% level.
[a] Significantly different from the low *F(P)* estimate at the 5% level.

Table 3.8 — Tests of the managerial ability hypothesis using number of employees and after-tax, certainty equivalent compensation.

Coefficients from ordinary least squares regressions of executive compensation on own- and peer-firm performance. The dependent variable is after-tax, certainty equivalent total compensation in panel A, and after-tax, certainty equivalent cash compensation in panel B. Own and peer performance represents the own-stock-return based change in shareholder wealth and the peer-stock-return based change in shareholder wealth, respectively. Peer stock returns are represented by either the average return of firms within the sample firm's three digit SIC code or firms comprising the *CRSP* market index. Each panel presents the results of three sets of regressions, corresponding to estimation of equations (3.5) through (3.7) wherein which the variable, *F*(*P*), is the c.d.f. of the firm's number of employees (*EMPS*). Standard errors are in parenthesis.

	Dependent variable in levels (Eq. 3.5)		Dependent variable in differences (Eq. 3.6)		Two-way fixed effects (Eq. 3.7)	
Regression coefficients	3-digit	market	3-digit	market	3-digit	market
Panel A: Dependent variable is after-tax certainty equivalent total compensation						
Own performance	0.2385*	0.3162*	0.2943*	0.3429*	0.4104*	0.3389*
	(0.1075)	(0.0936)	(0.1409)	(0.1220)	(0.1310)	(0.1089)
Peer performance	0.0340	-0.5322**	-0.0559	-0.8995*	-0.1537	-0.6626*
	(0.1288)	(0.3076)	(0.1679)	(0.4054)	(0.1227)	(0.2944)
Own performance x *F(EMPS)*	-0.1552	-0.2422*	-0.3131*	-0.3678*	-0.4443*	-0.3633*
	(0.1142)	(0.1004)	(0.1521)	(0.1324)	(0.1392)	(0.1173)
Peer performance x *F(EMPS)*	-0.0321	0.6954*	0.0395	1.0290*	0.1395	0.7236*
	(0.1321)	(0.3183)	(0.1725)	(0.4201)	(0.1258)	(0.3051)
R^2	0.4116	0.4336	0.0237	0.0368	0.5166	0.5172
Number of observations	1,404	1,404	1,117	1,117	1,404	1,404
Hypotheses tests at the low, median, and high values of *F(EMPS)*						
Own performance						
Low, *F(EMPS)*=0.0	0.2385*	0.3162*	0.2943*	0.3429*	0.4104*	0.3389*
	(0.1075)	(0.0936)	(0.1409)	(0.1220)	(0.1310)	(0.1089)
Median, *F(EMPS)*=0.5	0.1609*	0.1951*	0.1378*	0.1590*	0.1882*	0.1572*
	(0.0513)	(0.0444)	(0.0663)	(0.0574)	(0.0624)	(0.0516)
High, *F(EMPS)*=1.0	0.0833*[a]	0.0740*[a]	-0.0187[a]	-0.0249[a]	-0.0339**[a]	-0.0244[a]
	(0.0150)	(0.0144)	(0.0225)	(0.0212)	(0.0177)	(0.0187)
Peer performance						
Low, *F(EMPS)*=0.0	0.0340	-0.5322**	-0.0559	-0.8995*	-0.1537	-0.6626*
	(0.1288)	(0.3076)	(0.1679)	(0.4054)	(0.1227)	(0.2944)
Median, *F(EMPS)*=0.5	0.0180	-0.1845	-0.0361	-0.3850**	-0.0839	-0.3008*
	(0.0631)	(0.1491)	(0.0820)	(0.1963)	(0.0601)	(0.1427)
High, *F(EMPS)*=1.0	0.0020[a]	0.1632*[a]	-0.0163[a]	0.1295*[a]	-0.0142**[a]	0.0610*[a]
	(0.0090)	(0.0227)	(0.0122)	(0.0310)	(0.0083)	(0.0246)

Regression coefficients	Dependent variable in levels (Eq. 3.5) 3-digit	market	Dependent variable in differences (Eq. 3.6) 3-digit	market	Two-way fixed effects (Eq. 3.7) 3-digit	market
Panel B: Dependent variable is after-tax certainty equivalent cash compensation						
Own performance	0.0197	0.0290	0.0148	0.0158	0.0599*	0.0380*
	(0.0233)	(0.0202)	(0.0167)	(0.0143)	(0.0218)	(0.0180)
Peer performance	0.0037	-0.0467	0.0016	-0.0462	-0.0302	-0.0503
	(0.0279)	(0.0662)	(0.0199)	(0.0473)	(0.0204)	(0.0485)
Own performance x *F(EMPS)*	0.0063	-0.0032	-0.0064	-0.0055	-0.0620*	-0.0333**
	(0.0248)	(0.0216)	(0.0180)	(0.0155)	(0.0231)	(0.0193)
Peer performance x *F(EMPS)*	0.0019	0.0880	-0.0001	0.0697	0.0335	0.0697
	(0.0286)	(0.0685)	(0.0204)	(0.0491)	(0.0209)	(0.0503)
R^2	0.6527	0.6700	0.0684	0.1047	0.7548	0.7587
Number of observations	1,404	1,404	1,117	1,117	1,404	1,404
Hypotheses tests at the low, median, and high values of *F(EMPS)*						
Own performance						
Low, *F(EMPS)*=0.0	0.0197	0.0290	0.0148	0.0158	0.0599*	0.0380*
	(0.0233)	(0.0202)	(0.0167)	(0.0143)	(0.0218)	(0.0180)
Median, *F(EMPS)*=0.5	0.0228*	0.0274*	0.0116	0.0131**	0.0289*	0.0213*
	(0.0111)	(0.0096)	(0.0079)	(0.0067)	(0.0104)	(0.0085)
High, *F(EMPS)*=1.0	0.0260*[a]	0.0258*	0.0084*[a]	0.0103*[a]	-0.0021[a]	0.0047[a]
	(0.0033)	(0.0031)	(0.0027)	(0.0025)	(0.0029)	(0.0031)
Peer performance						
Low, *F(EMPS)*=0.0	0.0037	-0.0467	0.0016	-0.0462	-0.0302	-0.0503
	(0.0279)	(0.0662)	(0.0199)	(0.0473)	(0.0204)	(0.0485)
Median, *F(EMPS)*=0.5	0.0047	-0.0027	0.0016	-0.0114	-0.0135	-0.0154
	(0.0137)	(0.0321)	(0.0097)	(0.0229)	(0.0100)	(0.0235)
High, *F(EMPS)*=1.0	0.0056*	0.0413*[a]	0.0016	0.0234*[a]	0.0033*[a]	0.0195*[a]
	(0.0019)	(0.0049)	(0.0015)	(0.0036)	(0.0014)	(0.0041)

* Significantly different from zero at the 5% level.
** Significantly different from zero at the 10% level.
[a] Significantly different from the low *F(P)* estimate at the 5% level.

Chart 3.1 — Components of total compensation, unadjusted for taxes and risk 1996-2000.
Pie charts depicting the component parts of total executive compensation for sample firms, over the period 1996-2000, unadjusted for taxes and risk.

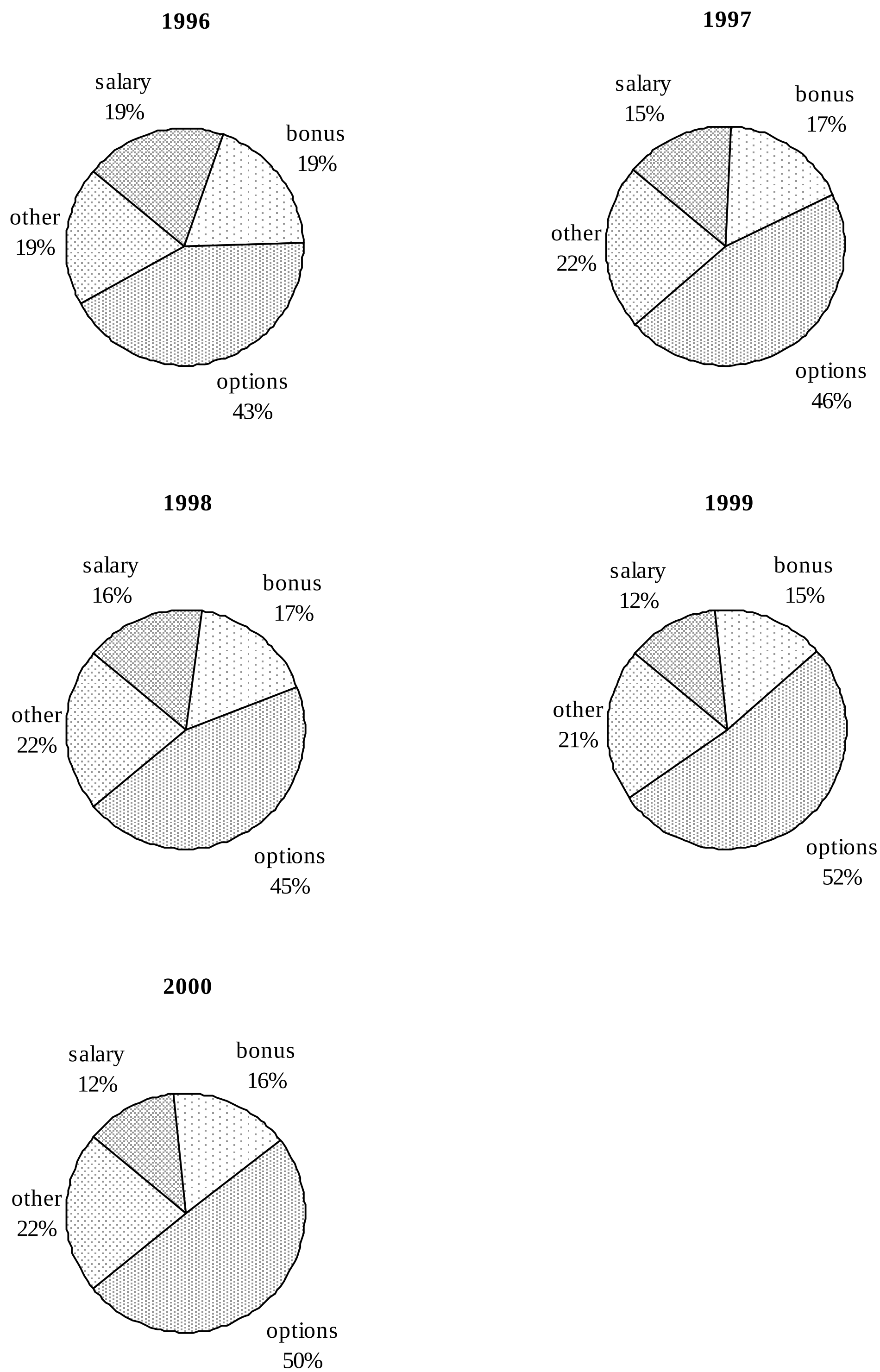
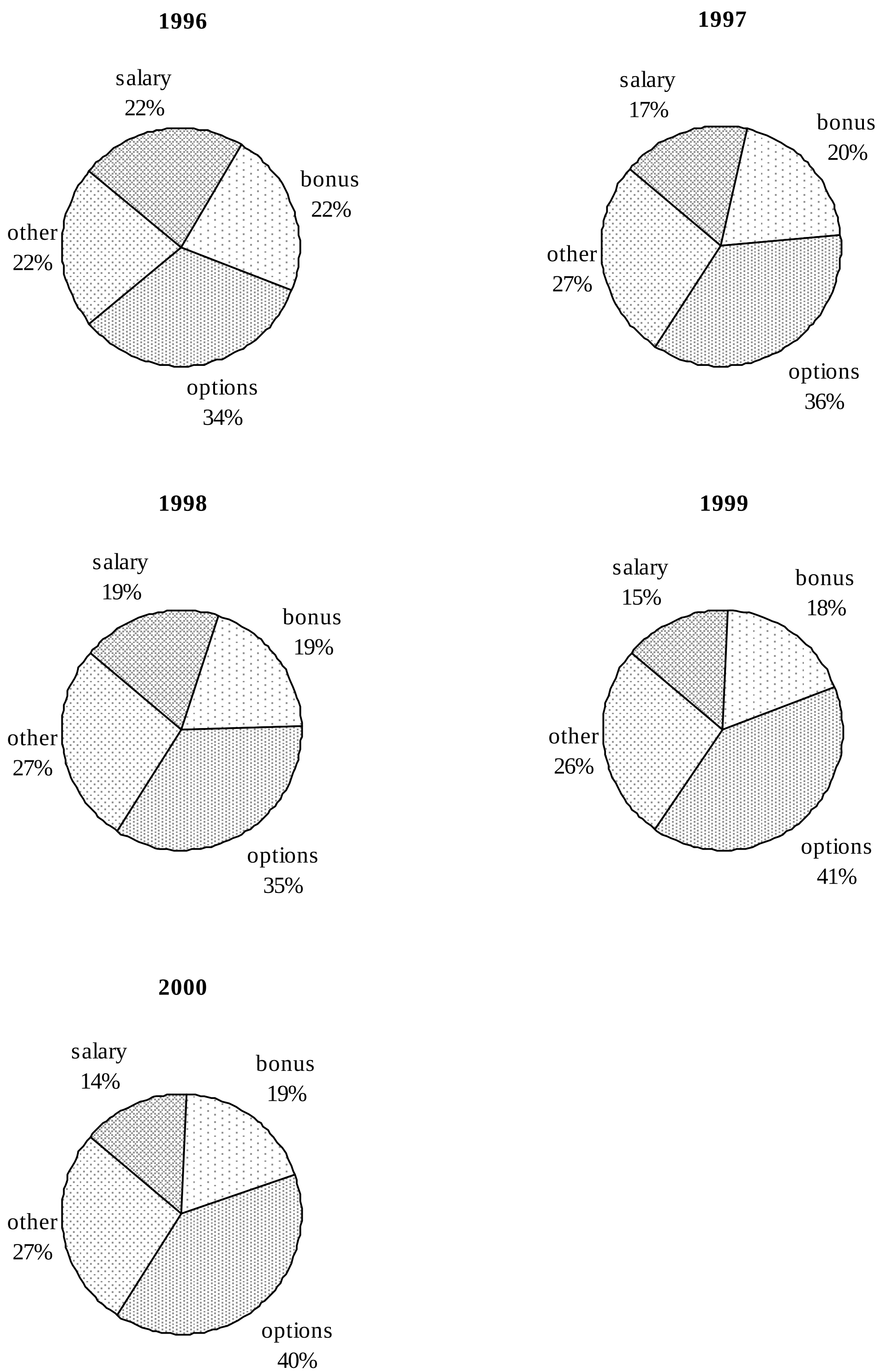


Chart 3.2 — Components of total compensation, adjusted for taxes and risk 1996-2000.
Pie charts depicting the component parts of total executive compensation for sample firms, over the period 1996-2000, adjusted for taxes and risk (i.e., after-tax certainty equivalent compensation).



CHAPTER 4

THE EFFECTS OF THE SARBANES-OXLEY ACT OF 2002 ON CROSS–LISTED FIRMS AND CROSS–LISTING ACTIVITY ON US STOCK MARKETS

4.1 Introduction

A number of studies examine the effects of the Sarbanes-Oxley Act of 2002 (SOX), yet evidence regarding the Act's net benefits remains mixed. On one hand, Akhigbe and Martin (2006) find that SOX-related transparency improvements increased firm values in the financial services industry. Chhaochharia and Grinstein (2004) find that large firms needing to make SOX-related governance changes responded more favorably than other large firms did. Li, Pincus, and Rego (2004) find positive returns associated with events that resolved uncertainty about the Act's final provisions or were informative about its enforcement. They also find that investors expect SOX to improve the accuracy and reliability of financial reports by constraining managements' ability to manage earnings and by enhancing corporate governance.

On the other hand, Engel, Hayes, and Wang (2004) find that the quarterly frequency of going private transactions increased, post-SOX, that small firms with relatively illiquid shares responded less favorably than other firms to SOX announcements, and that small firms with greater inside ownership experienced higher going-private announcement returns, post-SOX, as compared to pre-SOX. Chhaochharia and Grinstein (2004) find evidence that the costs of implementing the new rules exceed the benefits in small firms. Leuz, Triantis, and Wang (2004) find that about 200 US companies went "dark" in response to SOX, choosing to cease filing reports with the SEC by deregistering their securities and moving to the OTC. Cohen, Dey, and Lys (2004) find a significant decrease in the ratio of incentive compensation to gross compensation as well as a significant decline in research and development expenses, post-SOX, suggesting that SOX-related personal liability exposure has affected executives' willingness to take risks. Linck, Netter, and Yang (2005) find that SOX increased board of directors costs, particularly for small firms. They find that boards are larger and more independent, director workload and risk increased, and director and officer insurance premiums more than doubled, post-SOX.

I extend this literature by examining the effects of SOX on cross-listed firms. What makes this sample particularly interesting to study is the suggestion — by Coffee (1999, 2002), Fuerst (1998), and Stulz (1999) — that cross-listed firms create value through adherence to US regulations, which are typically more stringent than other countries'. This study provides new insight into the net benefits of US regulations to these firms.

There are two major parts to the study. In the first part, I study the stock price responses of cross-listed firms to news announcements foreshadowing SOX. Using the event study approach pioneered by Fama, Fisher, Jensen, and Roll (1969), I calculate cumulative abnormal returns around the SOX announcement dates used by Engel, Hayes, and Wang (2004). I then analyze the announcement returns to determine whether there is a relation between the returns and nine firm and home-country characteristics related to SOX and cross-listed firm valuations. Those characteristics include: (i) leverage, (ii) firm size, (iii) growth opportunities, (iv) sales in North America, (v) insider ownership, (vi) profitability, (vii) US market liquidity, (viii) home-country accounting standards, and (ix) home-country shareholder rights.

In the second part, I study cross-listing/de-listing activity in the period immediately surrounding the Act's enactment. Under the *avoiding hypothesis* [Licht (2001, 2003)], the costs of complying with US regulations deter firms from cross-listing the US. This analysis sheds new light on how these costs affect the decision to cross-list or remain cross-listed in the US.

The results of the SOX announcement event study indicate an overall negative and significant response by cross-listed firms to the new regulations. Mean cumulative abnormal returns are a significant -3.81% for the full sample of 424 cross-listed firms. Bifurcating the sample by the level of home-country accounting standards reveals that the firms based in countries with low-level accounting standards responded with an insignificant -1.45% return, while the firms based in countries with high-level accounting standards responded with a significant -6.90% return. Each group's returns are significantly different from one another. Similarly, bifurcating the sample by the level of home-country shareholder rights reveals that the firms based in countries with low-level shareholder rights responded with a significant -2.32% return, while the firms based in countries with high-level shareholder rights responded with a significant -6.56% return. Once again, each group's abnormal returns are significantly different from one another. Cross-sectional analysis of the SOX announcement returns as they relate to the nine key firm and home-country characteristics indicate a positive and significant relation between the returns and firm size, growth opportunities, and US market liquidity, and negative and significant relation between the abnormal returns and profitability and the level of home-country accounting standards.

My findings with respect to cross-listing/de-listing activity indicate a significant decrease in the quarterly average number of new cross-listings in the eight quarters immediately succeeding SOX as compared to the ten quarters immediately preceding SOX (twenty-three per quarter, pre-SOX, versus seven per quarter, post-SOX). Over the same period there was no significant change in the quarterly average de-listing activity (ten per quarter in the pre-SOX period versus nine per quarter in the post-SOX period), however only 20% of de-listings were voluntary (i.e., company decision), pre-SOX, while 35% were voluntary post-SOX. The finding of no significant difference in post-SOX de-listing activity is likely attributable to the US stock exchange de-listing rules and the SEC's deregistration regulations — both of which are exceptionally burdensome to non-US firms.[11]

Overall the results suggest that non-US firm shareholders anticipate negative net benefits from SOX. This is especially true for firms headquartered in countries with high-level accounting standards and shareholder rights already in place. The positive and significant relation between the returns and firm size supports the conjecture, made by Holmstrom and Kaplan (2003), that small firms will have more difficulty accommodating the new SOX-related expenditures than large firms. To the extent that firms with growth opportunities need external financing, the positive and significant relation between the announcement returns and growth opportunities suggests that non-US firms with the greatest need for external finance derive value from the confidence in US markets the new regulations are expected to produce. The positive and significant relation between the returns and US market liquidity suggests the new regulatory compliance costs are more burdensome to firms that are already having difficulty generating investor interest in the US. Finally, the significant downturn in new cross-listings indicates that the costs of complying with US regulations are an important consideration for firms desiring to list their shares on US markets.

The remainder of the paper begins in Section 4.2 with a detailed review of the processes and methods by which non-US firms cross-list in the US. I describe the tumultuous business environment confronting lawmakers immediately preceding SOX, trace the Act's steps through the US Congress on up to the President's desk, summarize the new law's key provisions, and survey the evidence regarding how firms have responded to the new law in Section 4.3. In

[11]See Perino (2005) for more details on NYSE de-listing and SEC deregistration rules.

Section 4.4, I develop the hypotheses that I test using the data and methods described in Section 4.5. I analyze the test results in Section 4.6 and make concluding remarks in Section 4.7.

4.2 Cross-listing in the US

The majority of firms that cross-list in the US do so through a Depositary Receipt (DR) program. Issued by US depositary banks since J.P. Morgan created them in 1927, a DR is a negotiable US security that represents a claim upon a non-US firm's publicly traded equity. US depositary banks create DRs by purchasing a non-US firm's securities on the firm's home market and either holding the securities in-house or arranging for a local custodian bank to hold them. The US depositary bank then issues DRs, which sometimes represent either a fraction or multiple of each non-US share, such that the DR's US dollar price fits into the trading range typical for US stocks. Holders have the right to exchange DRs for the underlying foreign shares at any time, making DRs and their underlying shares virtually perfect substitutes for one another. DRs traded outside the US are known as Global Depositary Receipts (GDRs), while those that trade on a US exchange are known as American Depositary Receipts (ADRs).

Depositary receipt programs may be sponsored or unsponsored. US depositary banks issue unsponsored DRs in response to investor demand, but without a formal agreement with the non-US company. Today, unsponsored DR programs are rarely newly established, however several existing unsponsored DRs continue to trade. Well-known firms with unsponsored DR programs include Japan's Sharp Corporation, Sanyo Electric Company, and Nintendo Company, Heineken, which is based in the Netherlands, and Associated British Foods, makers of Mazola Oil and Karo Syrup, which is based in the United Kingdom.

Depositary banks issue sponsored DRs under differing "levels." Sponsored Level I DRs trade in the US on the over-the-counter (OTC) market with prices published in the "Pink Sheets" and on some non-US exchanges. Level I DR issuers are not required to undergo full SEC registration, comply with the SEC's disclosure rules, or to report under US GAAP. Essentially, firms that cross-list through a Level I DR program become traded in the US without having to change their current reporting processes. Firms that wish to list their DRs on a major exchange (e.g., AMEX, NASDAQ, or NYSE) must issue Level II or Level III DRs. Level II and Level III DR programs require SEC registration, adherence to reporting under US GAAP, and compliance with the listing requirements of the US exchange upon which they are listed. The difference between Level II and Level III DRs is that Level III DRs involve raising new capital while Level II DRs do not.

In addition to the three levels of sponsored DR programs, non-US firms can access US markets through SEC Rule 144A and/or SEC Regulation S DR programs. Rule 144A programs allow firms to raise capital through private placements with Qualified Institutional Buyers (often referred to as "QIBs") in the US, while Regulation S programs provide for raising capital through the placement of DRs, offshore, to non-US investors. Like Level I DR programs, Rule 144A and Regulation S DR programs do not require full SEC disclosure or reporting under US GAAP.

The major advantage of DR programs to investors is that they reduce or eliminate many of the inconveniences associated with international securities trading such as settlement delays and transactions costs. The Bank of New York estimates that a DR investment can save an investor 10–40 basis points annually as compared to the cost of trading and holding ordinary shares outside the US. Growth in both the number of DR programs and DR trading volume increased steadily throughout the 1990's. In 1992, 215 major-exchange-listed (AMEX, NASDAQ, or NYSE) DR programs produced 4.3 billion in annual share trading volume. By

2001, there were 623 major-exchange-listed DR programs producing 31.0 billion in annual share trading volume.[12]

While DRs are still the most popular means by which non-US firms cross-list, a number of foreign issuers have bypassed the DR system altogether by listing directly on a US stock exchange. Known as “Global Shares,” directly listed shares trade on US markets in the same form as they are listed and traded on their home-country market. Improvements in international clearing and settlement systems, which obviate the need for US Depositary Banks to act as intermediaries, have fueled growth in the number of Global Shares issued and traded. Today, many cross-border trades clear and settle without difficulty. For example, the securities clearing and settlements systems of the US and Canada have a relation that is so close that all cross-listed Canadian firms list directly on US exchanges. At present, there are over 75 Canadian companies listed on the both the NYSE and NASDAQ with shares that trade as ordinary shares in both the US and Canadian markets. The US system is also linked with both the German and Swiss clearing systems and links between the US and other clearing systems are developing. As of September 2004, the NYSE reports 135 direct listings by non-US firms while the NASDAQ reports 207.

4.3 The Sarbanes-Oxley Act of 2002

Following several major corporate governance scandals exposed by a string of SEC investigations during 2001 and 2002, the US Congress passed, and on July 30, 2002, President Bush signed into law, the Sarbanes-Oxley Act of 2002 [now known as public law 107–204]. The law, described by President Bush as incorporating, “the most far-reaching reforms of American business practices since the time of Franklin Delano Roosevelt,” intends to give the SEC the

[12]Source: *Solving the ADR Puzzle, The Expert Guide to Building a Successful ADR Program* (The Bank of New York, 2002).

power to, "act against those who have shaken confidence in our markets, using the full authority of government to expose corruption, punish wrongdoers and defend the rights and interests of American workers and investors."[13] In the first part of this section, I describe the tumultuous business environment confronting lawmakers immediately preceding SOX and follow the Act as it works its way through the US Congress on up to the President's desk. In the second part, I summarize the new law's key provisions. In the third part, I survey the evidence regarding how firms and the US stock exchanges have responded to the new law.

4.3.1 Corporate chaos compels Congress

It was the hundreds of class action lawsuits filed by investors seeking damages for securities fraud that initially drew congressional attention to corporate governance issues in early 2001. National media attention was similarly drawn to the issue of corporate governance by the over $3 *billion* in earnings restatements made by Enron Corporation. These unfortunate disclosures caused disastrous losses to Enron's employees and investors and placed additional pressure on lawmakers to put an end the practices blamed for the company's demise. By 2002, scandals at several other high-profile companies such as WorldCom, Adelphia, Arthur Andersen, Baxter, Johnson & Johnson, and Merck came to light, garnering even more media attention and increasing the pressure on lawmakers to reform the current regulatory system. WorldCom, like Enron the previous year, made national headlines with of a series of earnings restatements that eventually totaled about $9.0 *billion* — erasing the reported earnings of the previous two fiscal years combined. Once one of the world's largest telecommunications companies, WorldCom became the largest bankruptcy is US history, with shareholders losing about $180 billion and 20,000 employees losing their jobs. In the end, WorldCom's chief financial officer, Scott

[13]Source: *Remarks by the President at Signing of H.R. 3763, the Sarbanes-Oxley Act of 2002* (Presidential Press Release, Office of the Press Secretary, July 30, 2002).

Sullivan, pled guilty to securities fraud, while a jury convicted its CEO, Bernie Ebbers, of nine felonies. On July 13, 2005, a judge sentenced the sixty-three year-old Ebbers to twenty-five years in prison — the longest sentence ever for an executive accused of committing corporate crimes.

Responding to the outcry for reform that developed in light of these scandals, on February 13, 2002 Congressman Michael D. Oxley introduced into the US House of Representatives, "A bill to protect investors by improving the accuracy and reliability of corporate disclosures made pursuant to the securities laws, and for other purposes; to the Committee on Financial Services."[14] On March 7, President Bush introduced a "Ten-Point Plan to Improve Corporate Responsibility and Protect America's Shareholders" to "improve corporate disclosure, make corporate officers more accountable and develop a stronger, more independent audit system." The House of Representatives passed Congressman Oxley's bill by a vote of 334 to 90 on April 24. Following the House activity, Senator Paul S. Sarbanes introduced into the US Senate "An original bill to improve quality and transparency in financial reporting and independent audits and accounting services for public companies, to create a Public Company Accounting Oversight Board, to enhance the standard setting process for accounting practices, to strengthen the independence of firms that audit public companies, to increase corporate responsibility and the usefulness of corporate financial disclosure, to protect the objectivity and independence of securities analysts, to improve the Securities and Exchange Commission resources and oversight, and for other purposes," on June 24.[15] On June 26, WorldCom announced its first earnings restatement of $3.85 billion, increasing the pressure on the Senate to take action. On July 15, the Senate passed Sarbanes' bill by a 97 to 0 vote. The bill sailed

[14]Source: *Congressional Record – House*, February 14, 2002, p.H523.
[15]Source: *Congressional Record – Senate*, June 25, 2002, p.S6013.

through an almost unanimous conference committee vote and President Bush signed the bill into law on July 30, 2002.

4.3.2 Key provisions

SOX amends the Securities Exchange Act of 1934 by tightening up the rules for financial disclosure and internal controls, prohibiting certain acts and conflicts of interest, establishing a new level of governance for public accounting firms, and by making CEOs, CFOs and other officers and board members criminally liable for fraudulent and misleading statements within their public financial statements. In this section, I summarize the Act's salient provisions.

SOX is divided into eleven parts, each part mandating significant changes to US corporate governance and reporting practices. Specifically, Title I establishes a public company accounting oversight board, responsible for establishing and enforcing rules and standards relating to the preparation of audit reports by public accounting firms. Title II bars auditors from nonaudit work for audit clients, requires rotation of audit partners after five years, requires corporate audit committees to pre-approve outside auditors, and places restrictions upon audits conducted upon firms whose senior management was previously employed by the auditor. Title III focuses on corporate responsibility. Section 301 directs securities exchanges and securities associations to prohibit the listing of any security of an issuer that does not have an audit committee responsible for hiring and oversight of auditors and that is wholly independent. Section 302 holds senior managers accountable for the quality of financial disclosures by requiring CEOs and CFOs to sign financial reports and to certify that the report does not contain any untrue statements of material facts or omit to state any material facts. Section 304 penalizes fraud by requiring executives to return any incentive-based compensation, as well as profits from stock sales, following accounting restatements made necessary by the revelation of executive misconduct. Title IV requires enhancements to public disclosures, such as by requiring firms to disclose all material off-balance sheet transactions. It also prohibits firms from loaning money to executives. Section 404 requires managers to prepare an annual internal control report that states management's responsibility for establishing and maintaining an adequate internal control structure as well as an assessment of the effectiveness of the control structure and the firm's procedures for financial reporting. Title V attempts to eliminate certain conflicts of interest in the investment banking industry by separating the securities analysis function from the investment banking function within investment banks. Title VI increases both the SEC's funding and authority, while Title VII authorizes the General Accounting Office (GAO) to conduct a series of studies relating to accounting firms, credit rating agencies, corporate governance, and investment banking. Finally, Titles VIII through XI increase the penalties for destruction and falsification of corporate records, amend the sentencing guidelines and increase the penalties for white-collar crimes, extend the statute of limitations for securities fraud violations, and provide for enhanced whistleblower protections. Section 906 requires both the CEO and CFO to certify that the firm's financial reports comply with the requirements of the SEC and that the information within the reports fairly presents, in all material respects, the financial condition and results of the issuer. Misconduct under Section 906 is punishable by fines up to $5 million and jail terms of up to 20 years.

4.3.3 Public company responses

Anecdotal and empirical evidence regarding the net benefits from SOX is mixed. A 2005 survey of public company executives by the law firm, Foley & Lardner, LLP indicates that 82% of executives feel that SOX's corporate governance and public disclosure reforms rules are "too strict" and 70% feel that the reforms have increased the overall administrative expenses "a great deal."[16] Furthermore, 44% of respondents reported considering going private or selling or merging with another company in response to SOX. Section 404 compliance, which requires managers to prepare an annual internal control report, was overwhelmingly listed as the area having the most financial impact on public companies; 86% of executives ranked it as the area having the greatest impact on internal and out-of-pocket costs. Following Section 404 compliance as the areas having the greatest impact on expenditures were increases in legal expenses and directors and officers insurance. Overall, the surveyed executives estimated that the annual cost of being public for companies with revenue under $1 billion increased from $1.1 million, pre-SOX, to $2.6 million, post-SOX. The estimated annual cost of being public for companies with annual revenue of $1 billion and over increased from $9.8 million in 2003 to $14.2 million in 2004.

Executives also complain about the indirect costs associated with SOX compliance. Peter Bible, chief accounting officer at General Motors Corporation laments that "The real cost isn't the incremental dollars, it is having people that should be focused on the business focused instead on complying with the details of the rules."[17] E. Follin Smith, CFO of Constellation Energy Group, complains that the new law could eventually make, "fear of personal liability so great that managers are afraid to take risks on innovation."

Complaints about cost increases are not limited to US executives. Martin Hollenhorst, CFO of German biotech company Lion Bioscience AG complains, "Compliance costs are rising exorbitantly, and we have nothing for it."[18] NYSE CEO John Thain has also voiced his disapproval for the new regulations. Regarding the impact of SOX upon non-US firms, he

---

[16] Source: *The Cost of Being Public in the Era of Sarbanes-Oxley* (Foley and Lardner, LLP, June 16, 2005).

[17] Source: *Companies Complain About Cost of Corporate-Governance Rules* (The Wall Street Journal, February 10, 2004).

[18] Source: *Citing Sarbanes, Foreign Companies Flee U.S. Exchanges* (The Wall Street Journal, September 20, 2004).

writes, “companies around the world are voting with their feet. One sign of the negative posture can be seen in the decline in new listings of non-US companies in US financial markets. Between 1996 and 2001, the NYSE listed an average of 50 non-US companies a year. In the past two years, that number dropped to 25, with the decline particularly sharp among European companies, dropping from 19 to 6.” Thain concludes that, “We need to strike a balance between the costs of increased time and resources devoted to compliance, and the incremental benefits they will produce in terms of transparency and governance.”[19] The amount of new capital raised by non-US firms in the US has also declined in the post-SOX era. In 2000, 90% of dollars raised by non-US firms through new stock offerings was raised in New York — rather than in London or Luxembourg, considered the two other main venues. By 2005, 90% of new dollars were raised though new listings in London or Luxembourg, the biggest spread favoring London since 1990. Only 13 non-US firms priced new stock offerings in New York in 2005, while 48 companies sold new shares in London and Luxembourg.[20] New overseas listings on the London Stock Exchange also increased from 7 in 2001 to 18 in 2005.[21]

Several empirical studies support the anecdotal evidence on the costs of SOX compliance. Chhaochharia and Grinstein (2004) find that the costs of implementing the new rules exceed the benefits in small firms. Engel, Hayes, and Wang (2004) find that the quarterly frequency of firms undergoing going private transactions increased, post-SOX, that small firms with relatively illiquid shares responded less favorably than other firms to SOX announcements, and that small firms with greater inside ownership experienced higher going-private announcement returns, post-SOX, as compared to pre-SOX. Leuz, Triantis, and Wang (2004) find that about 200 US companies went “dark” in 2003, choosing to cease filing reports with the

---

[19]Source: *Sarbanes-Oxley: Is the Price Too High?* (The Wall Street Journal, May 27, 2004).
[20] Source: *New York Loses Edge in Snagging Foreign Listings* (The Wall Street Journal, January 26, 2006).
[21] Source: http://www.londonstockexchange.com/en-gb/pricesnews/statistics/listcompanies.

SEC by deregistering their securities and moving to the OTC. Cohen, Dey, and Lys (2004) find a significant decline in the ratio of incentive compensation to gross compensation as well as a significant decline in research and development expenses — suggesting that the personal liability SOX imposes has had an impact upon executives' willingness to take risks. Linck, Netter, and Yang (2005) find that SOX increased board of directors costs, particularly for small firms. They find that boards are larger and more independent, director workload and risk increased, and director and officer insurance premiums more than doubled, post-SOX. Moreover, the returns for the *CRSP* value-weighted index on the SOX announcement dates used in this study are -8.62% — strong indication of disapproval for the new regulations from a broad sample of US-listed firms.

Nevertheless, some non-US firm executives do not consider the additional compliance expenditures excessive. Yong Kyung Lee, CEO of Korean telecommunications service provider KT Corporation says that complying with SOX, "has increased KT's visibility, strengthened our relation with investors, and boosted confidence in the global capital markets." Roy Millionton, corporate secretary for Canadian aluminum products company, Alcan, agrees, stating that his company, "is using Sarbanes-Oxley to make our reporting procedures more efficient and gain more control over how they operate throughout all the levels of our company."[22] Supporting these executives' observations, Li, Pincus, and Rego (2004) find positive returns associated with events that resolved uncertainty about the Act's final provisions or were informative about its enforcement. They also find that investors expect SOX to improve the accuracy and reliability of financial reports by constraining managements' ability to manage earnings and by enhancing corporate governance. Akhigbe and Martin (2006) find that the SOX-related transparency improvements increased firm values in the financial services industry. Chhaochharia and

[22]Source: *The Global Stance on Sarbanes-Oxley* (New York Stock Exchange and Time, Inc, 2003).

Grinstein (2004) find that large firms needing to make governance changes in response to SOX had higher SOX announcement returns than other large firms did.

Overall, evidence regarding the Act's net benefits remains mixed. With an eye toward providing fresh evidence on the costs and benefits of SOX, in the next section I develop the theory that underlies the hypotheses tested in the remainder paper.

4.4 Theory and hypotheses development

Following the analysis in Engel, Hayes, and Wang (2004), I let $value_{cross\text{-}listed}^{pre\text{-}SOX}$ and $value_{cross\text{-}listed}^{post\text{-}SOX}$ be the value of a cross-listed firm before and after the anticipated enactment of SOX, respectively. Since the effect of SOX could be to either increase or decrease value, I can say that

$$value_{cross\text{-}listed}^{post\text{-}SOX} - value_{cross\text{-}listed}^{pre\text{-}SOX} = b - k \tag{4.1}$$

where b and k represent the present values of the anticipated benefits from and costs of SOX compliance, respectively. Equation (4.1) suggests that there is a cross-sectional relation between SOX announcement returns and the anticipated net benefits from SOX compliance.

Equation (4.1) also suggests that non-US firms will cross-list or remain cross-listed, post-SOX, only if

$$k < b + (value_{cross\text{-}listed}^{pre\text{-}SOX} - value_{non\text{-}cross\text{-}listed}^{post\text{-}SOX}), \tag{4.2}$$

which is another way of saying that firms cross-list or remain cross-listed so long as the post-SOX cross-listing premium is positive.

The previous literature on SOX and the costs and benefits of a US cross-listing suggests several variables to consider with respect to cross-sectional analysis of SOX announcement returns. Holmstrom and Kaplan (2003) suggest that small firms will have more difficulty accommodating SOX-related expenditures than large firms because a significant portion of SOX-related costs are fixed; small firms are less able to accommodate these cost increases through scale economies than large firms are. This suggests that there is a positive relation between SOX announcement returns and firm size.

SOX mandates several improvements to corporate governance practices. Firms lacking strong governance before SOX should benefit more than others. Because agency costs only arise to the extent that insiders own less than one hundred percent of the firm's equity, Jensen and Meckling (1976) suggest that the fraction of total shares outstanding held by insiders is a good proxy for the strength of a firm's corporate governance structure. This is because the lower the percentage of the firm owned by insiders, the greater the incentive for insiders to shirk and consume excessive perquisites at the expense of outsiders. This suggests that there is a negative relation between SOX announcement returns and the fraction of the firm owned by insiders.

Sarkissian and Schill (2003) argue that greater market liquidity reduces cross-listed firms' cost of capital and that the level of a firm's exports is highly correlated with cross-listed firm valuation premiums. Both of these characteristics, US market liquidity and export sales, are also measures of cross-listed firms' commitment to US markets. High US market liquidity indicates that there is a lot of interest in the cross-listed firm's stock, while export sales, especially sales to North American customers, indicate a commitment to this market. This suggests that there is a positive relation between SOX announcement returns and US market liquidity and the percentage of total sales made to North American customers.

Under the bonding hypothesis, developed in Coffee (1999, 2002), Fuerst (1998), and Stulz (1999), cross-listed firms trade at a premium relative to their non-cross-listed counterparts because cross-listed firms subject themselves to: (i) the law enforcement powers of the Securities and Exchange Commission (SEC); (ii) an exigent litigation environment; (iii) enhanced financial disclosure rules that require reconciliation to US generally accepted accounting principles

(GAAP); and (iv) increased scrutiny from outsiders such as investors, auditors, analysts, and investment bankers. This suggests that there are cross-sectional differences in SOX announcement returns based on each firm's home-country's level of accounting standards and shareholder rights. Under the bonding hypothesis, firms headquartered in countries with low standards are expected to respond more favorably to SOX than firms based in countries with high-level standards already in place. This is because non-US firms, especially those coming from low-level standards countries, create value by committing themselves to the high-level financial reporting and corporate governance standards prescribed by US regulators. Improvements to those standards, coupled with increases in SEC authority and funding, are therefore good news for firms who cannot obtain such credibility in their home-countries.[23] This suggests that there is a negative relation between SOX announcement returns and home-country accounting standards and shareholder rights.

Finally, Doidge, Karolyi, and Stulz (2004) find that growth opportunities are more highly valued for firms that cross-list in the US, particularly those from countries with poorer investor rights. They suggest that this is because a US listing reduces the extent to which controlling shareholders can engage in expropriation, which improves cross-listed firms' ability to finance their growth opportunities. Because SOX is anticipated to reduce insiders' ability to expropriate, firms with greater growth opportunities are expected to respond more favorably to SOX announcements than those with lesser growth opportunities. This suggests that there is a positive relation between SOX announcement returns and non-US firm growth opportunities.

In sum, the previous literature motivates the first hypothesis:

[23] See Coffee (1999) for a more detailed discussion on the differences between the US and other countries' securities laws.

H1: *SOX announcement returns are positively related to firm size, US market liquidity, North American sales, and firm growth opportunities, and negatively related to the percentage of the firm owned by insiders, and home-country accounting standards and shareholder rights*.

In addition to the above hypothesis, equation (4.2) suggests that non-US firms will cross-list or remain cross-listed only to the extent that the costs of doing so are sufficiently low enough relative to the benefits derived. Analysis of new cross-listing/de-listing activity provides insight into the value that non-US firms place on a US listing both before and after the anticipated enactment of SOX. This analysis motivates the second hypothesis:

H2: *Non-US firm will cross-listing/de-listing activity will be unchanged in response to SOX*.

I test both hypotheses using the data and methods described in the next section.

4.5 Data and method

4.5.1 Sample selection

I identify my sample of cross-listed firms by downloading data on active DR programs from The Bank of New York's website, http://www.adrbny.com. This sample of active DR issues does not include issues previously terminated. Fortunately, The Bank of New York provided me with separate data on terminated DR issues dating back to the beginning 1997. By backfilling the downloaded sample of active DR issues with the set of terminated issues provided directly to me by The Bank of New York, I construct a sample of DR issues outstanding from the beginning 1997 to the end of September 2004 without survivor bias. I exclude unsponsored DRs, Level I DRs, and DRs issued under Rule 144A or Regulation S from the sample because they do not trade on a major exchange, which limits data availability, and because it is not immediately clear that SOX affects these firms' present financial reporting or corporate governance requirements.

I next add to the sample of DR issues a sample of direct listings of Global Shares downloaded from the NYSE and NASDAQ web sites (http://www.nyse.com and http://www.nasdaq.com, respectively); the NYSE data includes terminated issues going back to the beginning of 1999, while the NASDAQ data does not include terminated issues. By combining the Bank of New York DR data with the NYSE and NASDAQ Global Shares data, I form a data set of cross-listed firms that includes all Level II and Level III sponsored DRs listed on either AMEX, NASDAQ, or NYSE from the beginning of 1997 to September 2004, and all direct listings of Global Shares on NYSE and NASDAQ from the beginning 1999 to September 2004, excluding terminated issues of NASDAQ direct listings. Upon deletion of firms from regulated industries (NAICS codes 22, 52, and 92, representing utilities, finance and insurance, and public administration, respectively) I have a final sample that consists of 378 DRs, 122 Canadian direct listings, and 134 other direct listings, for a total of 634 cross-listed firms from 40 countries trading in the US during the SOX announcement period (see tables 4.1 and 4.2 for sample selection).

4.5.2 SOX announcement returns

I analyze non-US firms' response to news announcements foreshadowing SOX using two measures of abnormal returns, *ARSOX* and *ARSOXPASS* — the same two sets of event dates used by Engel, Hayes, and Wang (2004). Specifically, I estimate market model parameters α_j, and β_j over an estimation period that excludes the event dates, letting each non-US firm, j, have a return generating process of the form:

$$R_{jt} = \alpha_j + (\beta_j \times R_{mt}) + \varepsilon_{jt} \tag{4.3}$$

where

α_j, β_j = market model parameters for firm j,

R_{jt}, R_{mt} = returns to firm j and a market portfolio proxy during a 230-day estimation period (minimum 200 observations), and

ε_{jt} = a mean zero disturbance term.

I then use these estimated market model parameters to calculate cumulative abnormal returns as the cumulative prediction error

$$\varepsilon_{jt} = R_{jt} - [\alpha_j + (\beta_j \times R_{mt})] \tag{4.4}$$

over either the 21 event dates (for *ARSOX*) or 9 event dates (for *ARSOXPASS*) specified in appendix A.

To eliminate the possibility that SOX-related announcements enter into my estimation period returns, I end the 230-day estimation period on February 12, 2002, one day before Congressman Oxley introduced his corporate reform bill in the House of Representatives. I also use each firm's home-country market index return from Datastream for the market portfolio benchmark, R_m. In addition, I re-estimate the market model parameters and re-calculate the cumulative abnormal returns using the value weighted market index of US firms from *CRSP* as the market portfolio benchmark.

4.5.3 Cross-sectional analysis

In addition to a univariate analysis of the SOX announcement returns, I analyze the cumulative abnormal returns using multivariable regression analysis. To do this, I estimate cross-sectional ordinary least squares regressions of the form:

$$AR_j = \alpha + \sum_{i=1}^{9} \beta_i \times CHAR_{ij} + \varepsilon_j \tag{4.5}$$

where

AR_j = SOX announcement cumulative abnormal returns for firm j,

α = an intercept coefficient,

β_i = coefficients representing *AR*'s sensitivity to firm and home-country characteristics

$CHAR_{ij}$ = each of (up to) nine firm and home-country characteristics for firm j

ε_j = a mean zero disturbance term.

I test for the predicted relation between the SOX announcement returns and the firm and home-country characteristics specified in **H1** using data gathered from the following sources. I calculate firm size (*LOGMV*), the natural logarithm of the firm's market value of equity, as the fiscal year end number of common shares outstanding times the calendar year end stock price from *COMPUSTAT*. I calculate the year-end percentage of total shares outstanding held by the firm's managers, officers, and directors (*OWNS*) using data from Linck, Netter, and Yang (2005), which was graciously provided by the authors. I estimate US market liquidity (*USTO*) by taking the twelve-month average of the monthly total number of shares traded in the US divided by the monthly average number of shares outstanding in the US, as reported on *CRSP*. To be included in the sample, a firm must have had trading volume data on at least 10 trading days during the month and at least six months worth of data on *CRSP*. I calculate the ratio of North American (i.e., primarily the US, Canada, and Mexico) sales to total sales, *NASLS*, using *COMPUSTAT*'s geographic segments database.[24] I differentiate between each firm's home-country's level of accounting standards and shareholder rights using the "Accounting Standards"

[24]I note here that the calculation of the ratio of North American sales to total sales is slightly imperfect. This is because some firms report their segment data in more detail than others. For example, some firms report sales to the "United States." Some report sales to "North America." Others report sales to "North and South America." I include all of these in the numerator of *NASLS* event though, in the third example, some of the sales are attributable to South America.

and "Shareholder Rights" scores from La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998) [hereafter LLSV (1998)], which I refer to as *ASCORE* and *SSCORE*, respectively. LLSV (1998) calculate their accounting standards scores by rating companies' annual reports on their inclusion or omission of 90 items falling into seven categories: (i) general information, (ii) income statements, (iii) balance sheets, (iv) fund flow statement, (v) accounting standards, (vi) stock data, and (vii) special items. They base their shareholder rights scores on the legal protections each country affords shareholders with respect to their right to vote for the board of directors and on major corporate issues. These legal protections include rules such as: (i) one share, one vote, (ii) proxy by mail allowed, (iii) cumulative voting, and (iv) preemptive rights. I calculate the firm's year-end market-to-book ratio (*MTB*) as the fiscal year end number of common shares outstanding times the calendar year end stock price, all divided by the fiscal year end value of common equity, from *COMPUSTAT*. Finally, I include leverage, *LEV*, and return on net assets, *ROA*, as control variables. *LEV* represents the ratio of the firm's fiscal year end long-term debt to net total assets. *ROA* represents the firm's fiscal year end operating income before depreciation divided by net total assets. The data to calculate both *LEV* and *ROA* are from *COMPUSTAT*.

All of the financial statement variables are as of *COMPUSTAT* year-end 2001.[25] The stock market variables from *CRSP* and the insider ownership data are as of calendar year-end 2001.

I present sample summary statistics for these characteristics in tables 4.3a and 4.3b — sorted by *ASCORE* and *SSCORE*, respectively. Table 4.3a indicates that high *ASCORE* firms are

[25] *COMPUSTAT* year-end data represents the year in which the fiscal year begins if the fiscal year-end is between January and May. If the fiscal year-end is between June and December, then the *COMPUSTAT* year-end data represents the year in which the fiscal year ends. Essentially, the *COMPUSTAT* year-end is the year in which the majority of the months of the fiscal year fall.

significantly smaller in terms of market value than low *ASCORE* firms and have significantly less *USTO*. However, the high *ASCORE* firms have significantly greater *MTB*, *NASLS*, and *OWNS*. Table 4.3b indicates that high *SSCORE* firms are significantly smaller in terms of market value than low *SSCORE* firms and have significantly less *USTO* and lower *MTB*, as well. High *SSCORE* firms have significantly more *NASLS* and *OWNS* than their low *SSCORE* counterparts do.

Sefcik and Thompson (1986) point out that abnormal returns generated on a common event date and used as the dependent variables in cross-sectional regressions give rise to "spatial" autocorrelation in the regression error terms. This unusual type of autocorrelation is attributable to unobservable elements that affect the usual correlations of daily returns between all stocks. Autocorrelation in regression error terms results in biased standard error estimates (but unbiased estimators) and a concomitant loss of efficiency unless researchers either: (i) use the portfolio regression approach in Sefcik and Thompson (1986), or (ii) control for the source of the autocorrelation by including additional independent variables in the regressions. Since the nature of such "spatial" autocorrelation, in the context of this study, is likely attributable to the close economic ties shared between firms located within the same country, the inclusion of *ASCORE* and *SSCORE*, which are country-specific, as independent variables is sufficient to control for this concern.

To alleviate the problem of multicolinearity, attributable to the high degree of correlation between *ASCORE* and *SSCORE* (the Pearson correlation coefficient between the two scores is 0.52), I run separate regressions where I include either *ASCORE* or *SSCORE* as dependent variables. Finally, I refine the standard errors of the regression parameter estimates using the White (1980) correction for heteroskedasticity.

4.5.4 New listing/de-listing analysis

I follow up the cross-sectional analysis of SOX announcement returns with an analysis of post-SOX non-US firm new listing/de-listing activity. This analysis follows in the spirit of previous studies that find that SOX affected firms' going private decisions [Engel, Hayes, and Wang (2004)] and that SOX is the behind the approximately 200 US companies that went dark in 2003 [Leuz, Triantis, and Wang (2004)]. Specifically, I study the new listing and de-listing volume, de-listing announcement returns, *DCAR*, and the de-listing firm characteristics of 179 non-US firms whose securities de-listed (for direct listings) or DR programs terminated (for DRs) between January 1, 2000 and September 30, 2004. I calculate *DCAR* using the usual event study approach with de-listing announcement dates obtained via either an electronic LexisNexis Academic Guided New Search or on Thomson Financial's Securities Data Corporation (SDC) International Mergers and Acquisitions database. I calculate *DCAR* using a 250-day event period (minimum 50 return observations), beginning 46 days before the announcement of the event that ultimately caused the security to be de-listed, and a (-1,+1) event window surrounding the announcement date. I refine the test statistics using the Boehmer, Musumeci, and Poulsen (1991) adjustment for event-induced variance. I divide the sample into two segments, pre-SOX, and post-SOX, based on whether the de-listing occurred before or after June 25, 2002 (the date after which the enactment of SOX was a foregone conclusion). I then use *DCAR* to study whether the de-listing announcement returns are related to the firm characteristics previously identified and whether that relation changed in response to SOX. To do this, I estimate cross-sectional regressions with the de-listing announcement returns as the dependent variable and the firm characteristics as the independent variables along their interaction with a dummy variable that equals zero for pre-SOX firms and one for post-SOX firms. Specifically, this entails estimation of:

$$DCAR_j = \alpha + \sum_{i=1}^{9} \beta_i \times CHAR_{ij} + \sum_{p=1}^{9} \eta_p \times CHAR_{pj} \times POST_j + \varphi \times POST_j + \varepsilon_j \qquad (4.6)$$

where

$DCAR_j$ = the de-listing announcement cumulative abnormal return for firm j,

α = an intercept coefficient,

β_i = each of nine coefficients representing $DCAR_j$'s sensitivity to $CHAR_j$,

$CHAR_j$ = each of nine firm and home-country characteristics for firm j,

η_p = nine coefficients representing $DCAR_j$'s sensitivity to $CHAR_j \times POST_j$,

$POST_j$ = dummy variable that equals one if firm j de-listed post-SOX, and zero otherwise,

φ = coefficient representing $DCAR_j$'s sensitivity to $POST_j$, and

ε_j = a mean zero disturbance term.

As another check on whether SOX affected cross-listed firms' willingness to remain listed on a US stock exchange, I run logistic regressions on my sample of de-listing firms, paired with a matched sample (by home-country and year-end market value of equity) of non-de-listing firms. Specifically, I estimate the model:

$$DUM_j = \alpha + \sum_{i=1}^{6} \beta_i \times CHAR_{ij} \qquad (4.7)$$

where

DUM_j = a dummy variable that equals one if firm j de-listed and zero, otherwise,

α = an intercept coefficient,

β_i = six coefficients representing the log odds ratio that DUM_j equals one. Unit increases in $CHAR_{ij}$ are associated with a β_i change in the log odds of DUM_j equaling one, and

$CHAR_{ij}$ = six of the nine firm characteristics for firm *j*. *ASCORE*, *SSCORE*, and *LOGMV* are not included because I match the sample group to the control group by home-country and firm size.

In the de-listing announcement and logistic regressions, all of the accounting variables are as of the *COMPUSTAT* year-end immediately preceding the de-listing announcement year while the *CRSP* market variables and insider ownership variables are as of the calendar year end immediately preceding the de-listing announcement year.

4.6 Results and analysis

4.6.1 SOX announcement returns — home-country market index

The results of the univariate analysis of SOX announcement returns presented in tables 4.4a and 4.4b indicate that for the entire sample of cross-listed firms, mean cumulative abnormal returns are a significant -3.81% and -1.49% for *ARSOX* and *ARSOXPASS*, respectively. Bifurcating the sample by *ASCORE* (table 4.4a) reveals that firms based in countries with low-level accounting standards responded more favorably to SOX announcements than firms based in countries with high-level accounting standards. The mean *ARSOX* cumulative abnormal returns for high *ASCORE* country firms are a significant -6.90%, while they are an insignificant -1.45% for the low *ASCORE* country firms. The mean *ARSOXPASS* cumulative abnormal returns for high *ASCORE* country firms are a significant -3.67%, while they are an insignificant -0.03% for the low *ASCORE* country firms. The abnormal returns for each group are also significantly different from one another. Bifurcating the sample by *SSCORE* (table 4.4b) reveals that firms based in countries with low-level shareholder rights also responded significantly more favorably to SOX announcements than firms based in countries with high-level shareholder rights. The mean *ARSOX* cumulative abnormal returns for high *SSCORE* country firms are a significant -6.56%, while they are a significant -2.32% for the low *SSCORE* country firms. The mean *ARSOXPASS* cumulative abnormal returns for high *SSCORE* country firms are a significant -3.48%, while they are an insignificant -0.34% for the low *SSCORE* country firms. Once again, the abnormal returns for each group are significantly different from one another.

I present the results of cross-sectional regression analyses of *ARSOX* and *ARSOXPASS* in table 4.5. Notably, the regression results indicate a positive and significant relation between *ARSOX* and *LOGMV*, suggesting that small firms responded less favorably to SOX announcements than large firms. This evidence is consistent with the results in Engel, Hayes, and Wang (2004), who find than smaller firms experienced higher going-private announcement returns in the post-SOX period compared to the pre-SOX period. It is also consistent with the analysis in Holmstrom and Kaplan (2003), who argue that small firms will have more difficulty accommodating the new SOX-related expenditures than large firms. This result also confirms statements made by the chair and CEO of the American Stock Exchange, Neal Wolkoff, who calls SOX a "curse for small-cap companies."[26] Indeed, the SEC recently extended the deadline for small firms (firms having market capitalizations of less than $75 million) to comply with SOX's internal-controls rule to July 2007 in response to several complaints.[27]

Table 4.5 also indicates that there is a positive and significant relation between *ARSOXPASS* and *MTB*. Recalling that the bonding hypothesis suggests that the commitment by

[26]Source: *Sarbanes-Oxley Is a Curse for Small-Cap Companies* (The Wall Street Journal, August 15, 2005).

[27]Source: *Small Firms to Get Another Extension On Sarbanes Rule* (The Wall Street Journal, September 13, 2005).

non-US firms to US-level corporate governance and financial reporting practices enhances their ability to raise outside financing, it is not surprising to find that the firms with the greatest need for external finance, high *MTB* firms, responded more favorably to SOX announcements than firms without such need did. This finding compliments the findings in Reese and Weisbach (2002) who report that both the number and value of equity offerings increase following a US cross-listing and that the increase in issuance is larger for firms based in countries with weaker shareholder protection regulations than the US. The results in table 4.5 also indicate a positive and significant relation between *ARSOX* and *USTURN*. This finding is consistent with the idea that firms come the US to increase their share liquidity, but the costs of complying with US regulations offsets the liquidity benefits.

My final finding from the cross-sectional analysis is a negative and significant relation between *ARSOX* and *ASCORE* — indicating that firms from countries with low-level accounting standards responded more favorably to SOX announcements than firms from countries with high-level accounting standards. Notably, there is no significant relation between *ARSOX* or *ARSOXPASS* and *SSCORE*, whereas **H1** predicts a negative and significant relation.

4.6.2 SOX announcement returns — US market index

I re-calculate *ARSOX* and *ARSOXPASS* using the *CRSP* value-weighted market index of US firms as the return benchmark. I then repeat the cross-sectional analyses using these new, US-market-based measures of abnormal returns. I note here that these new abnormal returns are interpreted as a response *over and above* the response of the average US firm because both the sample firms and the firms comprising the market index are responding to a common event, i.e., SOX-related news announcements. I present the results of the univariate analysis of the US-market-based abnormal returns in tables 4.6a and 4.6b. I present the multivariate analysis results in table 4.7.

The results in table 4.6a indicate that firms from countries with high-level accounting standards responded significantly less favorably to SOX announcements than firms from countries with low-level accounting standards. The mean cumulative abnormal SOX announcement returns (relative to the US market index) for high *ASCORE* firms are a significant -8.44%, significantly different from the 0.00% response by the low *ASCORE* firms. Similarly, table 4.6b indicates that high *SSCORE* firms' cumulative abnormal returns are a significant -8.26%, significantly different than the -0.67% response by low *SSCORE* firms. The cross-sectional regression results in table 4.7 indicate a negative and significant relation between *ARSOX* and *NASLS*, *OWNS*, *ROA*, and *SSCORE*, a negative and significant relation between *ARSOXPASS* and *ROA*, and a positive and significant relation between *ARSOXPASS* and *MTB*. Overall, these results do not change the conclusions reached when the abnormal returns were calculated using each firm's home-country market index as the market portfolio benchmark.

4.6.3 New listing/de-listing analysis

I analyze the quarterly new listing and de-listing activity of non-US firms on the major US stock exchanges in the ten quarters immediately preceding SOX and the eight quarters immediately succeeding SOX (i.e., beginning in January 2000 and ending September 2004, recalling that SOX was enacted in August 2002).

In table 4.8, I present the results of the analysis of new-listing activity. I find a significant decrease in the quarterly average number of new cross-listings on the major US stock exchanges. In the ten quarters immediately preceding SOX, the quarterly average number of new listings is twenty-three. In the eight quarters immediately succeeding SOX, the quarterly average number of new listings is seven. This difference in new listing activity is significant based on the Wilcoxon signed rank test.

In table 4.9, I present the results of the analysis of de-listing activity. I find that there is no significant difference between pre- and post- SOX de-listing activity. In the ten quarters immediately preceding SOX, the quarterly average number of de-listings is ten. In the eight quarters immediately succeeding SOX, the quarterly average number of de-listings is nine. This difference is not significant based on the Wilcoxon signed rank test. However, these results might also be the byproduct of the exchanges' de-listing and the SEC's deregistration rules, which are more burdensome to non-US firms than they are to US firms.

In table 4.10, I bifurcate the de-listing sample by *ASCORE* and *SSCORE*. This analysis reveals that firms from low *ASCORE* countries made up 33% of de-listings in the pre-SOX period and 46% in the post-SOX period, while firms from high *ASCORE* countries made up 58% of pre-SOX and 40% of post-SOX de-listings. Moreover, I find that firms based in low *SSCORE* countries made up 47% of de-listings in the pre-SOX period and 64% in the post-SOX period, while firms based in high *SSCORE* countries made up 50% of pre-SOX, but only 27% of post-SOX de-listings.

Table 4.11 describes the de-listed sample selection. Overall, I found 179 de-listings over the sample period (excluding firms from regulated industries), out of which 82 had no missing data. In table 4.12, I present the results of a univariate analysis of pre- and post-SOX de-listing firms' characteristics. Notably, I find significant differences in *ASCORE* and *SSCORE* between the two samples. The mean *ASCORE* for post-SOX de-listing firms is 68.52 while it is 71.47 for pre-SOX firms. This result indicates that it is the firms located in countries with low level accounting standards that are de-listing in response to SOX. Similarly, I find that the mean

SSCORE of post-SOX de-listing firms is 3.34 as compared to 3.87 for pre-SOX de-listing firms. Once again, the difference is significant. Furthermore, while the post-SOX de-listing announcement returns were not significantly different between the two groups, the returns for the post-SOX de-listing group were a significantly *positive* 6.92% — suggesting that the announcement of a de-listing from the US was good news for non-US firms in the post-SOX period.

In table 4.13, I break down the de-listed firm sample into five categories by reason for de-listing: (i) merger with or tender offer by a US-listed firm, (ii) merger with or tender offer by a non-US listed firm, (iii) company decision, (iv) exchange decision, and (v) other. The first category includes firms that de-listed because the firm combined with a US-listed firm, while the second category includes firms that combined with a non-US listed firm. Firms that simply voluntarily de-listed their shares from US exchanges fall into the third category, while firms that the exchanges involuntarily de-listed for failure to meet their listing requirements fall into the fourth category. The results indicate no change in merger or tender offer activity between the two periods, however, I do find that while 20% of de-listings were voluntary (i.e., attributable to a voluntary decision to de-list) in the pre-SOX period, 35% were made voluntarily in the post-SOX period. This increase came at the expense of de-listings attributable to exchange induced de-listings, which fell from 30% of pre-SOX de-listing to 17% of post-SOX de-listings.

I present the results of the cross-sectional analysis of de-listing announcement returns in table 4.14. The analysis indicates a positive and significant relation between the returns and *USTO*. This result is a little surprising given that a major reason why non-US firms de-list from the US is due to lack of trading volume. The coefficients on the post-SOX interaction terms are all insignificant, indicating no changes in the relevance of the key firm characteristics as they relate to de-listing announcement returns between the pre- and post- SOX periods.

Finally, descriptive statistics for both the de-listed sample and the control group are given in table 4.15. The results of the logistic regressions, wherein I set the dependent variable, DUM_j, to zero for a matched sample (by firm size and home country) of non-de-listing firms and one for the sample of de-listing firms, are given in table 4.16. The results indicate negative and significant coefficients on *USTO*. The negative coefficient on *USTO* is consistent with the statements made by many of the de-listing firms that a lack of trading volume on the US exchange is a major reason why they chose to de-list.

4.7 Conclusion

I study the response of cross-listed firms to the Sarbanes-Oxley Act of 2002 (SOX). Overall, I find significant mean cumulative abnormal returns of -3.81% and -1.87% for the full sample of non-US firms using two sets of event dates corresponding to news announcements foreshadowing SOX. When I bifurcate the sample by home-country level of accounting standards and shareholder legal rights, I find that the mean abnormal returns for the low-standards country firms are significantly greater than those of their high-standards country counterparts.

Cross-sectional analysis of the SOX announcement returns indicates a positive and significant relation between the announcement returns and firm size, growth opportunities, and US market liquidity, and a negative and significant relation with the level of home-country accounting standards. Consistent with the predictions in Holmstrom and Kaplan (2003) and the results in Engel, Hayes, and Wang (2004), these findings suggest that small, non-US firms responded less favorably to SOX than large firms did, possibly due to their inability to accommodate the fixed costs associated with SOX compliance. This finding supports the recent statement by Neal Wolkoff, chair and CEO of the American Stock Exchange, that "Development-stage companies with little or no revenue cannot afford burdensome compliance costs."[28] The positive and significant relation between the announcement returns and firm growth opportunities supports the bonding hypothesis, which predicts that non-US firms with greater growth opportunities will respond favorably to regulatory changes that reinforce their commitment to responsible corporate behavior in the eyes of outside investors. The negative and significant relation with the level of home-country accounting standards also supports the bonding hypothesis — suggesting that firms from countries with low-level standards responded better to the SOX-mandated accounting changes than others.

Analysis of post-SOX cross-listing and de-listing activity indicates a significant decline in the number of new non-US firm listings on the major US stock exchanges. While, in and of itself, this is not direct evidence that SOX has changed non-US firms' choice of overseas listing venue, when combined with both anecdotal and empirical evidence from this and other studies, it certainly seems to suggest that firms are more cautious about their decision to come to the US in the post-SOX era. The firms that de-listed post-SOX were, on average, based in countries with lower level accounting standards and shareholder rights than those that de-listed pre-SOX. Finally, analysis of the reasons that firms gave for giving up their US listing indicates an increase in the percentage of firms that voluntarily de-listed their shares in the post-SOX period as compared to the pre-SOX period.

---

[28]Source: *Sarbanes-Oxley Is a Curse for Small-Cap Companies* (The Wall Street Journal, August 15, 2005).

Appendix A — Key dates used to calculate cumulative abnormal returns.

February 13, 2002

Congressman Oxley introduces in the US House of Representatives, "A bill to protect investors by improving the accuracy and reliability of corporate disclosures made pursuant to the securities laws, and for other purposes; to the Committee on Financial Services."

June 11, 2002–June 13, 2002

The SEC proposes rules requiring CEOs and CFOs to certify that they have read their company's financial statements, that the information presented is true, and that the documents contains all the numbers a "reasonable" investor would deem necessary. The proposed rules also require companies to report significant financial events within two business days, to disclose obligations in off-balance-sheet partnerships, credit-rating changes, triggers that could prompt lenders to call in loans, and lockout periods for employee retirement and stock-ownership plans.

June 17, 2002–June 19, 2002

By a vote of 17 to 4, the Senate Banking Committee approves legislation to heighten oversight of the accounting industry. "We face a very severe challenge in the country," says Senator Paul Sarbanes, the chair of the committee. Calling the nation's accounting problems systemic, the senator went on to say that if Congress does not respond, "We may face a historical verdict that the people's gatekeeper failed its responsibility."

June 25, 2002

Senator Sarbanes introduces into the US Senate, "An original bill to improve quality and transparency in financial reporting and independent audits and accounting services for public companies, to create a Public Company Accounting Oversight Board, to enhance the standard setting process for accounting practices, to strengthen the independence of firms that audit public companies, to increase corporate responsibility and the usefulness of corporate financial disclosure, to protect the objectivity and independence of securities analysts, to improve Securities and Exchange Commission resources and oversight, and for other purposes; from the Committee on Banking, Housing, and Urban Affairs; placed on the calendar."

June 26, 2002

WorldCom, the second-largest long-distance carrier in the US, announces that it had overstated its cash flow by more than $3.8 billion during the last five quarters. In disclosing the problem, WorldCom also announces the firing of its CFO, Scott D. Sullivan, who orchestrated the strategy in which operating costs were booked as capital investments. The SEC calls the disclosures, "accounting improprieties of unprecedented magnitude."

June 27, 2002–June 28, 2002

The SEC orders CEOs and CFOs at the largest 1,000 US companies to certify that their financial statements are true and accurate. SEC chair Harvey Pitt states, "We are committed both in word and in deed to taking every conceivable action to make sure a system that was allowed to go unimproved for far too long is now improved." Mr. Pitt added that if the certification turned out to be false, then the offending executives would, "go to jail."

July 8, 2002–July 12, 2002

The US Senate debates Sarbanes' bill.

July 15, 2002–July 17, 2002

Sarbanes' bill passes in the Senate with a 97-0 vote. The Senate asks the House of Representatives for a conference to merge the Senate's proposal with the House's proposal, which passed the House on April 24, 2002 by a vote of 334-90. The House of Representatives agrees to a conference with the Senate.

July 23, 2002–July 25, 2002

The conference report is filed and agreed to by the House by a vote of 423-3 and by the Senate by a vote of 99-0. The bill becomes public law number 107-204 on July 30, 2002.

Note:

I calculate cumulative abnormal returns with an estimation period that ended on February 12, 2002, one day before Congressman Oxley introduced his bill in the US House of Representatives. I calculated *ARSOX* over the above twenty-one event dates and *ARSOXPASS* over the nine trading days between July 15, 2002 and July 25, 2002.

Table 4.1 — Sample selection.
Sample of cross-listed firms formed by combining data on sponsored Level II and Level III depositary receipt programs from The Bank of New York with direct listing data from NYSE and NASDAQ, excluding firms from regulated industries (NAICS codes 22, 52, and 92).

	US stock exchange		
Listing type:	AMEX / NYSE	NASDAQ	Total
Depositary receipt	232	146	378
Canadian direct	61	61	122
Other direct	30	104	134
Total non-US listings	323	311	634
Less:			
Missing Datastream data	73	137	210
Missing CRSP data	10	2	12
Missing Compustat data	10	6	16
Missing segments data	25	16	41
Missing ownership data	14	14	28
Missing LLSV (1998) data	11	6	17
Firms included in SOX announcement regression	180	130	310

Table 4.2 — Cumulative abnormal returns around SOX announcement dates sorted by home country.
Cross-listed firm cumulative abnormal returns sorted by the firm's home-country (N=424). Abnormal returns are calculated based on each firm's home-country market index from Datastream. ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. ASCORE represents the accounting standards score for each firm's home-country. SSCORE represents the shareholder (antidirector) rights score for each firm's home-country. Both are from LLSV (1998).

	-------------ARSOX-------------			----------ARSOXPASS----------					
Country	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	N	ASCORE	SSCORE
Argentina	-3.92	-6.53	16.42	-5.91	-0.27	13.30	8	45	4
Australia	-5.38	-2.88	11.46	-2.99	-1.53	7.02	14	75	4
Austria	13.76	13.76	0.00	6.71	6.71	0.00	1	54	2
Belgium	-11.29	-11.29	0.00	0.82	0.82	0.00	1	61	0
Brazil	4.52	2.64	7.82	3.50	2.21	6.45	6	54	3
Canada	-6.88*	-5.26	26.47	-4.68*	-2.85	15.03	104	74	5
Chile	0.62	-1.27	8.05	-1.82	-3.28	9.27	10	52	5
China	-0.44	-2.59	13.98	-0.43	-2.15	7.24	11	--	--
Denmark	3.64	3.64	0.00	-4.56	-4.56	0.00	1	62	2
Finland	-6.14	-15.99	19.08	-8.01	-15.92	14.69	6	77	3
France	-0.97	-2.77	13.28	4.97**	4.63	14.23	28	69	3
Germany	-3.16	-4.97	20.82	1.37	0.99	12.66	17	62	1
Greece	-10.64	-10.64	0.00	-11.36	-11.36	0.00	1	55	2
Hong Kong	-13.87*	-14.07	7.46	-10.10*	-10.64	3.33	4	69	5
India	-2.86	-1.98	7.78	3.17**	3.58	3.03	5	57	5
Indonesia	4.07	4.07	0.00	6.67	6.67	0.00	1	--	2
Ireland	-3.73	4.80	27.35	3.42	0.92	9.67	8	--	4
Israel	-2.47	-4.10	6.97	0.85	-0.12	6.64	18	64	3
Italy	-2.95	-3.82	8.55	2.42	-1.75	11.11	6	62	1
Japan	1.58	1.03	6.82	-0.84	0.44	5.45	24	65	4
Luxembourg	4.73	4.73	0.00	10.58	10.58	0.00	1	--	--
Mexico	-5.87**	-2.53	16.20	-0.87	0.71	7.33	24	60	1
Netherlands	-2.64	-1.87	15.14	-0.97	-1.98	13.12	14	64	2
New Zealand	-6.61	-4.53	5.70	1.65	0.86	6.59	3	70	4
Norway	-4.59	-7.04	12.89	0.45	0.09	5.07	5	74	4
Peru	-9.79	-9.79	9.53	-16.16**	-16.16	2.01	2	38	3
Philippines	-1.47	-1.47	0.00	3.49	3.49	0.00	1	65	3
Poland	29.43	29.43	0.00	23.61	23.61	0.00	1	--	--
Portugal	3.35	3.35	0.00	3.21	3.21	0.00	1	36	3
Russia	21.87	11.87	26.57	14.65	10.21	16.23	3	--	--
Singapore	-1.10	-0.45	4.23	-4.53	-4.95	4.03	3	78	4
South Africa	-9.90**	-11.01	11.81	-9.32*	-9.91	7.85	6	70	5
South Korea	7.90	0.55	26.93	0.39	-4.22	7.78	5	62	2
Spain	21.69**	20.38	11.09	4.42	0.37	12.17	3	64	4
Sweden	-11.32**	-8.59	20.32	-5.84**	-6.27	9.98	11	83	3
Switzerland	-7.58**	-5.70	10.50	-5.02	-3.11	10.01	8	68	2
Taiwan	3.90*	4.79	2.18	0.65	-0.19	8.67	5	65	3
Turkey	15.16	15.16	0.00	1.50	1.50	0.00	1	51	2
United Kingdom	-6.70*	-7.37	17.59	-0.81	-2.28	13.55	51	78	5
Venezuela	5.06	5.06	8.44	-3.75	-3.75	2.05	2	40	1

* Significantly different from zero at the five percent level.
** Significantly different from zero at the ten percent level.

Table 4.3a — Descriptive statistics for non-US firms sorted by ASCORE.

ASCORE represents the accounting standards scores for each firm's home country from LLSV (1998). The low-score group includes firms based in countries with accounting standards scores less than 70. The high-score group includes firms based in countries with accounting standards scores greater than or equal to 70. The unclassified group refers to firms based in countries for which there was no accounting standards score in LLSV (1998). The results for the full sample of cross-listed firms are shown in Panel A while the results for those firms with no missing data are shown in Panel B.

	LEV				MV				MTB			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample												
Low-score	16.59	15.16	13.09	193	9,469.95	1,666.26	16,676.11	188	2.69	1.61	3.17	185
High-score	18.86	16.75	18.26	202	7,102.26[a]	1,344.49	23,246.31	202	3.28[a]	2.13[a]	9.01	202
Unclassified	18.83	14.21	18.83	25	6,599.78	728.37	14,175.21	25	2.02	1.07	2.02	25
All	17.81	15.33	16.13	420	8,144.58	1,391.74	20,027.38	415	2.94	1.88	6.68	412
Panel B: Regression sample												
Low-score	16.19	14.93	13.38	144	9,549.53	1,693.08	16,883.35	144	2.76	1.68	3.30	144
High-score	18.96	16.75	18.47	166	7,198.23[b]	1,532.86	21,914.69	166	2.84[a]	2.12[a]	6.73	166
All	17.67	15.28	16.34	310	8,290.45	1,653.51	19,741.86	310	2.80	1.92	5.41	310

	NASLS				OWNS				ROA			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample												
Low-score	30.20	26.03	30.36	174	6.92	1.32	16.45	176	9.36	10.07	10.29	190
High-score	50.93[a]	52.79[a]	35.13	180	14.50[a]	3.89[a]	17.78	191	6.08	9.80	19.37	200
Unclassified	17.90	0.00	31.34	22	11.28	1.00	17.89	20	11.64	11.11	9.55	25
All	39.40	34.69	34.63	376	10.89	1.90	17.54	387	7.92	10.10	15.41	415
Panel B: Regression sample												
Low-score	32.00	26.97	30.28	144	7.31	1.32	17.38	144	9.44	10.02	10.58	144
High-score	51.55[a]	52.35[a]	34.65	166	15.03[a]	4.06[a]	18.16	166	7.26	10.28	18.21	166
All	42.46	37.79	34.07	310	11.44	2.07	18.19	310	8.27	10.10	15.17	310

Table 4.3a — Descriptive statistics for non-US firms sorted by ASCORE, cont'd.

	USTO				ASCORE				SSCORE			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample												
Low-score	20.46	9.08	53.23	188	61.64	64.00	6.86	196	2.70	3.00	1.30	196
High-score	15.91[a]	6.88[a]	33.38	199	75.53[a]	74.00[a]	2.72	203	4.71[a]	5.00[a]	0.61	203
Unclassified	14.59	9.07	19.43	25	--	--	--	0	3.78	4.00	0.67	9
All	17.91	8.08	43.05	412	68.71	70.00	8.67	399	3.72	4.00	1.41	408
Panel B: Regression sample												
Low-score	20.94	9.08	56.38	144	62.15	64.00	6.46	144	2.66	3.00	1.27	144
High-score	14.73[a]	6.97[a]	22.59	166	75.69[a]	74.00[a]	2.66	166	4.71[a]	5.00[a]	0.62	166
All	17.61	8.01	41.87	310	69.40	74.00	8.30	310	3.76	4.00	1.42	310

[a] Significantly different from the low-score group at the five percent level.
[b] Significantly different from the low-score group at the ten percent level.

Table 4.3b — Descriptive statistics for non-US firms sorted by SSCORE.
SSCORE represents the shareholder (antidirector) rights scores for each firm's home country from LLSV (1998). The low-score group includes firms based in countries with shareholder rights scores less than or equal to 4. The high-score group includes firms based in countries with shareholder rights scores greater than 4. The unclassified group refers to firms based in countries for which there was no shareholder rights score in LLSV (1998). The results for the full sample of cross-listed firms are shown in Panel A while the results for those firms with no missing data are shown in Panel B.

	LEV				MV				MTB			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample												
Low-score	17.18	17.03	13.14	224	9,413.74	1,856.75	17,389.52	219	3.08	1.71	6.95	216
High-score	18.47	13.86	18.90	180	6,600.03[a]	1,162.47[a]	23,020.73	180	2.89[a]	2.17[a]	6.62	180
Unclassified	19.26	14.19	20.51	16	8,149.18	816.21	17,296.74	16	1.56	0.74	2.24	16
All	17.81	15.33	16.13	420	8,144.58	1,391.74	20,027.38	415	2.94	1.88	6.68	412
Panel B: Regression sample												
Low-score	17.17	17.00	13.23	165	9,764.63	1,856.75	18,181.92	165	2.65	1.60	2.88	165
High-score	18.25	13.08	19.31	145	6,612.94[a]	1,368.00	21,318.98	145	2.97[a]	2.26[a]	7.30	145
All	17.67	15.28	16.34	310	8,290.45	1,653.51	19,741.86	310	2.80	1.92	5.41	310

	NASLS				OWNS				ROA			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample												
Low-score	30.68	26.55	29.97	204	7.14	1.21	16.59	207	8.34	10.10	12.65	221
High-score	54.16[a]	58.43[a]	35.31	158	15.81[a]	7.11[a]	17.76	168	6.96	9.88	18.63	178
Unclassified	0.00	0.00	0.00	14	6.57	0.05	14.21	12	12.72	11.81	8.31	16
All	39.40	34.69	34.63	376	10.89	1.90	17.54	387	7.92	10.10	15.41	415
Panel B: Regression sample												
Low-score	30.95	26.89	29.03	165	7.07	1.21	17.14	165	8.68	10.00	10.95	165
High-score	55.57[a]	61.11[a]	34.72	145	16.42[a]	7.15[a]	18.12	145	7.81	10.42	18.89	145
All	42.46	37.79	34.07	310	11.44	2.07	18.19	310	8.27	10.10	15.17	310

Table 4.3b — Descriptive statistics for non-US firms sorted by SSCORE, cont'd.

	USTO				ASCORE				SSCORE			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample												
Low-score	20.45	9.31	50.64	219	65.02	64.00	8.60	219	2.71	3.00	1.12	228
High-score	15.58[a]	6.82[a]	33.63	177	73.19[a]	74.00[a]	6.33	180	5.00[a]	5.00[a]	0.00	180
Unclassified	8.77	8.67	3.17	16	--	--	--	0	--	--	--	0
All	17.91	8.08	43.05	412	68.71	70.00	8.67	399	3.72	4.00	1.41	408
Panel B: Regression sample												
Low-score	20.73	9.23	53.85	165	65.72	65.00	8.25	165	2.67	3.00	1.10	165
High-score	14.06[a]	7.02[a]	20.87	145	73.59[a]	74.00[a]	6.09	145	5.00[a]	5.00[a]	0.00	145
All	17.61	8.01	41.87	310	69.40	74.00	8.30	310	3.76	4.00	1.42	310

[a] Significantly different from the low-score group at the five percent level.

Table 4.4a — Cumulative abnormal returns around SOX announcement dates by ASCORE. ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. ASCORE represents the accounting standards scores for each firm's home-country from LLSV (1998). The low-score group includes firms based in countries with accounting standards scores less than 70. The high-score group includes firms based in countries with accounting standards scores greater than or equal to 70. The unclassified group refers to firms based in countries for which there was no accounting standards score in LLSV (1998). The results for the full sample of cross-listed firms are shown in Panel A while the results for those firms with no missing data are shown in Panel B.

	ARSOX				ARSOXPASS			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample								
Low-score	-1.45	-1.73	13.48	196	0.03	0.26	10.15	196
High-score	-6.90*[a]	-5.44[a]	21.93	203	-3.67*[a]	-2.83[a]	13.49	203
Unclassified	2.77	0.48	21.27	25	4.30*	1.11	10.60	25
All	-3.81*	-2.99	18.68	424	-1.49*	-1.17	12.09	424
Panel B: Regression sample								
Low-score	-1.59	-2.09	12.81	144	-0.12	0.26	10.10	144
High-score	-6.87*[a]	-5.38[a]	22.87	166	-3.05*[a]	-1.65[a]	12.49	166
All	-4.42*	-3.23	19.03	310	-1.69*	-1.05	11.52	310

* Significantly different from zero at the five percent level.
[a] Significantly different from the low-score group at the five percent level.

Table 4.4b — Cumulative abnormal returns around SOX announcement dates by SSCORE.
ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. SSCORE represents the shareholder (antidirector) rights scores for each firm's home country from LLSV (1998). The low-score group includes firms based in countries with shareholder rights scores less than or equal to 4. The high-score group includes firms based in countries with shareholder rights scores greater than 4. The unclassified group refers to firms based in countries for which there was no shareholder rights score in LLSV (1998). The results for the full sample of cross-listed firms are shown in Panel A while the results for those firms with no missing data are shown in Panel B.

	ARSOX				ARSOXPASS			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample								
Low-score	-2.32*	-2.26	14.67	228	-0.34	-0.15	10.16	228
High-score	-6.56*[a]	-5.26[a]	22.44	180	-3.48*[a]	-3.03[a]	13.93	180
Unclassified	5.93	-0.77	18.49	16	4.59	0.56	11.63	16
All	-3.81*	-2.99	18.68	424	-1.49*	-1.17	12.09	424
Panel B: Regression sample								
Low-score	-2.86*	-2.63	13.91	165	-0.79	-0.19	10.14	165
High-score	-6.19*[a]	-4.86	23.46	145	-2.71*	-1.64	12.87	145
All	-4.42*	-3.23	19.03	310	-1.69*	-1.05	11.52	310

* Significantly different from zero at the five percent level.
[a] Significantly different from the low-score group at the five percent level.

Table 4.5 — Cross-sectional regression results, non-US market index based abnormal returns.
Cross-sectional regression results for in which the dependent variable is either ARSOX or ARSOXPASS. ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. LEV is the ratio of the firm's fiscal year end long-term debt to net total assets. LOGMV is the natural logarithm of the firm's market value of equity. MTB is the firm's market-to-book ratio. NASLS is the ratio of North American sales to total sales. OWNS is the year-end percentage of total shares outstanding held by the firm's managers, officers, and directors. ROA is the firm's fiscal year end operating income before depreciation divided by net total assets. USTO is the twelve-month average of the monthly total number of shares traded in the US divided by the monthly average number of shares outstanding in the US. ASCORE and SSCORE are the firm's home-country accounting standards and shareholder rights scores from LLSV (1998), respectively. Standard errors are in parenthesis.

	ARSOX	ARSOX	ARSOX	ARSOXPASS	ARSOXPASS	ARSOXPASS
Variable (predicted sign)	(1)	(2)	(3)	(4)	(5)	(6)
INTERCEPT (na)	9.77	8.91	-7.87**	-0.19	0.71	-2.11
	(7.96)	(7.73)	(4.43)	(5.69)	(5.68)	(3.27)
LEV (na)	-0.04	-0.04	-0.03	-0.01	-0.01	-0.01
	(0.06)	(0.06)	(0.07)	(0.04)	(0.04)	(0.04)
LOGMV (+)	1.14*	1.10*	1.12*	0.42	0.46	0.45
	(0.51)	(0.49)	(0.51)	(0.34)	(0.34)	(0.34)
MTB (+)	-0.05	-0.05	-0.03	0.22*	0.22*	0.22*
	(0.17)	(0.17)	(0.16)	(0.10)	(0.10)	(0.10)
NASLS (+)	0.00	0.00	-0.01	0.00	0.00	0.00
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
OWNS (-)	-0.04	-0.04	-0.05	-0.01	-0.01	0.00
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)
ROA (na)	-0.24	-0.24	-0.23	-0.14*	-0.15*	-0.14*
	(0.17)	(0.17)	(0.17)	(0.05)	(0.05)	(0.05)
USTO (+)	0.04*	0.04**	0.03	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
ASCORE (-)	-0.32*	-0.27*		-0.03	-0.08	
	(0.13)	(0.11)		(0.09)	(0.08)	
SSCORE (-)	0.49		-0.43	-0.51		-0.65
	(0.80)		(0.70)	(0.51)		(0.45)
F	2.15	2.38	1.89	1.97	2.11	2.31
N	310	310	317	310	310	317
R^2	0.06	0.06	0.05	0.06	0.05	0.06

* Significantly different from zero at the five percent level.
** Significantly different from zero at the ten percent level.

Table 4.6a — US market index based abnormal returns sorted by ASCORE.
ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. ASCORE represents the accounting standards scores for each firm's home-country from LLSV (1998). The low-score group includes firms based in countries with accounting standards scores less than 70. The high-score group includes firms based in countries with accounting standards scores greater than or equal to 70. The unclassified group refers to firms based in countries for which there was no accounting standards score in LLSV (1998). The results for the full sample of cross-listed firms are shown in Panel A while the results for those firms with no missing data are shown in Panel B.

	ARSOX				ARSOXPASS			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample								
Low-score	0.00	-0.19	17.36	289	0.42	0.21	11.72	289
High-score	-8.44*[a]	-7.95[a]	19.85	231	-5.64*[a]	-4.78[a]	15.22	231
Unclassified	-4.20	-4.24	22.44	75	-3.93*	-4.32	9.69	75
All	-3.80*	-3.47	19.41	595	-2.48*	-1.91	13.28	595
Panel B: Regression sample								
Low-score	-0.40	-0.19	16.29	237	0.33	0.26	11.74	237
High-score	-8.01*[a]	-7.17[a]	19.72	192	-4.59*[a]	-4.28[a]	14.73	192
All	-3.81*	-3.07	18.28	429	-1.87*	-1.27	13.38	429

* Significantly different from zero at the five percent level.
[a] Significantly different from the low-score group at the five percent level.

Table 4.6b — US market index based abnormal returns sorted by SSCORE.
ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. SSCORE represents the shareholder (antidirector) rights scores for each firm's home-country from LLSV (1998). The low-score group includes firms based in countries with shareholder rights scores less than or equal to 4. The high-score group includes firms based in countries with shareholder rights scores greater than 4. The unclassified group refers to firms based in countries for which there was no shareholder rights score in LLSV (1998). The results for the full sample of cross-listed firms are shown in Panel A while the results for those firms with no missing data are shown in Panel B.

	ARSOX				ARSOXPASS			
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N
Panel A: Full sample								
Low-score	-0.67	-0.72	19.18	313	-0.59	-0.64	11.85	313
High-score	-8.26*[a]	-7.12[a]	18.89	221	-4.62*[a]	-4.11[a]	15.56	221
Unclassified	-3.71	-4.25	19.62	61	-4.46*	-4.51	9.30	61
All	-3.80*	-3.47	19.41	595	-2.48*	-1.91	13.28	595
Panel B: Regression sample								
Low-score	-0.76	-0.72	17.46	245	-0.67	-0.76	12.20	245
High-score	-7.86*[a]	-6.00[a]	18.61	184	-3.47*[a]	-3.29[a]	14.68	184
All	-3.81*	-3.07	18.28	429	-1.87*	-1.27	13.38	429

* Significantly different from zero at the five percent level.
[a] Significantly different from the low-score group at the five percent level.

Table 4.7 — Cross-sectional regression results, US market index based abnormal returns.
Cross-sectional regression results for in which the dependent variable is either ARSOX or ARSOXPASS. ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. LEV is the ratio of the firm's fiscal year end long-term debt to net total assets. LOGMV is the natural logarithm of the firm's market value of equity. MTB is the firm's market-to-book ratio. NASLS is the ratio of North American sales to total sales. OWNS is the year-end percentage of total shares outstanding held by the firm's managers, officers, and directors. ROA is the firm's fiscal year end operating income before depreciation divided by net total assets. USTO is the twelve-month average of the monthly total number of shares traded in the US divided by the monthly average number of shares outstanding in the US. ASCORE and SSCORE are the firm's home-country accounting standards and shareholder rights scores from LLSV (1998), respectively. Standard errors are in parenthesis.

Variable (predicted sign)	ARSOX (1)	ARSOX (2)	ARSOX (3)	ARSOXPASS (4)	ARSOXPASS (5)	ARSOXPASS (6)
INTERCEPT (na)	4.87	8.20	2.14	4.15	5.11	-0.07
	(8.68)	(8.56)	(3.52)	(5.70)	(5.63)	(2.44)
LEV (na)	0.08	0.08	0.09	0.00	0.00	0.00
	(0.08)	(0.08)	(0.08)	(0.04)	(0.04)	(0.04)
LOGMV (+)	0.53	0.63	0.52	0.20	0.23	0.20
	(0.42)	(0.42)	(0.41)	(0.29)	(0.29)	(0.28)
MTB (+)	0.12	0.12	0.14	0.19**	0.19**	0.19**
	(0.15)	(0.15)	(0.15)	(0.11)	(0.11)	(0.11)
NASLS (+)	-0.08*	-0.08*	-0.09*	-0.01	-0.01	-0.01
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
OWNS (-)	-0.11*	-0.12*	-0.12*	-0.04	-0.04	-0.03
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
ROA (na)	-0.16*	-0.17*	-0.18*	-0.14*	-0.14*	-0.14*
	(0.07)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)
USTO (+)	0.05	0.05	0.04	0.03	0.03	0.03
	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)
ASCORE (-)	-0.06	-0.19		-0.08	-0.11	
	(0.15)	(0.13)		(0.10)	(0.09)	
SSCORE (-)	-1.44*		-1.54*	-0.41		-0.66
	(0.70)		(0.60)	(0.47)		(0.43)
F	5.49	5.65	6.32	2.75	3.02	3.06
N	429	429	440	429	429	440
R^2	0.11	0.10	0.11	0.06	0.05	0.05

* Significantly different from zero at the five percent level.
** Significantly different from zero at the ten percent level.

Table 4.8 — Non-US firm new listings by quarter.
Sample firm quarterly new listing activity from the major stock exchanges (AMEX, NASDAQ, or NYSE).

Quarter	Frequency	Percentage	Cumulative frequency	Cumulative percentage
2000q1	31	10.26	31	10.26
2000q2	32	10.60	63	20.86
2000q3	41	13.58	104	34.44
2000q4	34	11.26	138	45.70
2001q1	10	3.31	148	49.01
2001q2	16	5.30	164	54.30
2001q3	25	8.28	189	62.58
2001q4	23	7.62	212	70.20
2002q1	10	3.31	222	73.51
2002q2	12	3.97	234	77.48
July 2002	3	0.99	237	78.48
Aug/Sep 2002	9	2.98	246	81.46
2002q4	14	4.64	260	86.09
2003q1	4	1.32	264	87.42
2003q2	3	0.99	267	88.41
2003q3	8	2.65	275	91.06
2003q4	9	2.98	284	94.04
2004q1	3	0.99	287	95.03
2004q2	6	1.99	293	97.02
2004q3	9	2.98	302	100.00

Table 4.9 — Non-US firm de-listings by quarter.
Sample firm quarterly de-listing activity from the major stock exchanges (AMEX, NASDAQ, or NYSE), excluding firms from regulated industries (NAICS codes 22, 52, and 92).

Quarter	Frequency	Percentage	Cumulative frequency	Cumulative percentage
2000q1	7	3.91	7	3.91
2000q2	7	3.91	14	7.82
2000q3	10	5.59	24	13.41
2000q4	10	5.59	34	18.99
2001q1	7	3.91	41	22.91
2001q2	12	6.70	53	29.61
2001q3	9	5.03	62	34.64
2001q4	13	7.26	75	41.90
2002q1	7	3.91	82	45.81
2002q2	14	7.82	96	53.63
July 2002	5	2.79	101	56.42
Aug/Sep 2002	7	3.91	108	60.34
2002q4	13	7.26	121	67.60
2003q1	5	2.79	126	70.39
2003q2	11	6.15	137	76.54
2003q3	11	6.15	148	82.68
2003q4	7	3.91	155	86.59
2004q1	7	3.91	162	90.50
2004q2	12	6.70	174	97.21
2004q3	5	2.79	179	100.00

Table 4.10 — Non-US de-listings by accounting standards and shareholder rights scores. Non-US de-listings sorted by ASCORE in Panel A and SSCORE in Panel B. ASCORE represents the accounting standards scores for each firm's home-country from LLSV (1998). The low-score group includes firms based in countries with accounting standards scores less than 70. The high-score group includes firms based in countries with accounting standards scores greater than or equal to 70. SSCORE represents the shareholder (antidirector) rights scores for each firm's home country as given in LLSV (1998). The low-score group includes firms based in countries with shareholder rights scores less than or equal to 4. The high-score group includes firms based in countries with shareholder rights scores greater than 4. The unclassified category refers to firms based in countries for which there were no accounting standards or shareholder rights scores in LLSV (1998).

	----------Number----------			----------Percentage----------		
	All	Pre-SOX	Post-SOX	All	Pre-SOX	Post-SOX
Panel A: De-listings by accounting standards score						
Low-score	69	33	36	38.5	32.7	46.2
High-score	90	59	31	50.3	58.4	39.7
Unclassified	20	9	11	11.2	8.9	14.1
Total	179	101	78	100.0	100.0	100.0
Panel B: De-listings by shareholder rights score						
Low-score	97	47	50	54.2	46.5	64.1
High-score	72	51	21	40.2	50.5	26.9
Unclassified	10	3	7	5.6	3.0	9.0
Total	179	101	78	100.0	100.0	100.0

Table 4.11 — De-listed sample selection.

Cross-listed firms that de-listed form a major US stock exchange between January 1, 2000 and September 30, 2004, excluding firms from regulated industries (NAICS codes 22, 52, and 92).

	US stock exchange		
Listing type:	AMEX / NYSE	NASDAQ	Total
Depositary receipt	62	88	150
Canadian direct	16	--	16
Other direct	13	--	13
Total non-US listings	91	88	179
Less:			
Missing CRSP data	14	22	36
Missing Compustat data	10	19	29
Missing segments data	10	4	14
Missing ownership data	5	7	12
Missing LLSV (1998) data	4	2	6
Firms included in de-listing announcement regression	48	34	82

Table 4.12—Sample statistics for de-listed issues.

Sample statistics for the select firm variables used in the de-listing logistic regressions. ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. LEV is the ratio of the firm's fiscal year end long-term debt to net total assets. LOGMV is the natural logarithm of the firm's market value of equity. MTB is the firm's market-to-book ratio. NASLS is the ratio of North American sales to total sales. OWNS is the year-end percentage of total shares outstanding held by the firm's managers, officers, and directors. ROA is the firm's fiscal year end operating income before depreciation divided by net total assets. USTO is the twelve-month average of the monthly total number of shares traded in the US divided by the monthly average number of shares outstanding in the US. ASCORE and SSCORE are the firm's home-country accounting standards and shareholder rights scores from LLSV (1998), respectively. DCAR is the de-listing announcement abnormal return. Panel A includes the de-listing data without respect to whether or not some firms were missing data. Panel B includes the data for only those firms without any missing data.

	---------------All de-listings---------------			
	Mean	Median	Std. Dev.	N
Panel A: Full de-listed sample				
ARSOX	-0.20	-2.72	16.28	47
ARSOXPASS	2.73	4.22	12.74	47
LEV	20.47	13.61	24.14	139
LOGMV	3,009.05	310.86	9,288.93	131
MTB	2.44	1.45	7.33	129
NASLS	24.75	6.73	33.14	115
OWNS	8.84	1.15	15.18	149
ROA	2.09	7.54	27.83	135
USTO	17.88	6.93	50.83	163
ASCORE	70.23	74.00	9.25	159
SSCORE	3.64	4.00	1.48	169
DCAR	5.15	2.38	24.89	149
Panel B: Regression sample				
ARSOX	-1.85	-2.13	17.80	28
ARSOXPASS	2.11	4.61	11.67	28
LEV	20.78	16.46	21.00	82
LOGMV	2,639.95	441.89	6,405.98	82
MTB	2.20	1.52	6.97	82
NASLS	25.52	15.15	31.22	82
OWNS	8.73	0.95	16.62	82
ROA	3.47	8.49	28.89	82
USTO	15.86	7.14	39.95	82
ASCORE	71.33	74.00	7.95	82
SSCORE	3.66	4.00	1.45	82
DCAR	7.36	3.89	24.32	82

Table 4.12—Sample statistics for de-listed issues, cont'd.

	Pre-SOX sample				Post-SOX sample				Difference	
	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	N	Mean	Median
Panel A: Full de-listed sample										
ARSOX					-0.20	-2.72	16.28	47		
ARSOXPASS					2.73	4.22	12.74	47		
LEV	20.04	11.53	22.88	77	21.00	15.12	25.81	62	0.96	3.59
LOGMV	3,290.39	292.94	10,187.10	71	2,676.13	362.14	8,172.18	60	-614.26	69.20
MTB	2.18	1.37	7.62	71	2.76	1.55	7.00	58	0.58	0.18
NASLS	24.56	2.52	34.32	60	24.95	15.09	32.11	55	0.39	12.58
OWNS	8.19	1.31	13.33	86	9.72	1.12	17.46	63	1.53	-0.19
ROA	-0.31	6.38	32.96	74	5.00	8.16	19.82	61	5.31	1.78
USTO	20.84	6.68	62.99	90	14.23	7.11	29.75	73	-6.61	0.44
ASCORE	71.47	74.00	8.00	92	68.52	69.00	10.56	67	-2.95[b]	-5.00[a]
SSCORE	3.87	5.00	1.48	98	3.34	3.00	1.43	71	-0.53[a]	-2.00[a]
DELISTCAR	3.74	2.38	26.92	83	6.92	2.29	22.14	66	3.18	-0.10
Panel B: Regression sample										
ARSOX					-1.85	-2.13	17.80	28		
ARSOXPASS					2.11	4.61	11.67	28		
LEV	23.57	16.46	24.90	44	17.55	16.36	15.01	38	-6.02	-0.11
LOGMV	2,842.80	406.66	7,293.04	44	2,405.08	491.53	5,283.73	38	-437.72	84.88
MTB	1.50	1.38	6.04	44	3.02	1.62	7.92	38	1.52	0.24
NASLS	26.64	14.57	34.07	44	24.22	23.22	27.95	38	-2.42	8.64
OWNS	7.96	0.95	13.74	44	9.63	0.91	19.58	38	1.67	-0.04
ROA	0.51	8.93	38.18	44	6.89	8.15	10.53	38	6.37	-0.78
USTO	14.47	6.28	41.45	44	17.47	7.63	38.64	38	3.00	1.34
ASCORE	73.00	74.00	6.70	44	69.39	69.00	8.89	38	-3.61	-5.00
SSCORE	4.09	5.00	1.31	44	3.16	3.00	1.46	38	-0.93	-2.00
DELISTCAR	9.66	6.50	28.68	44	4.70	2.55	18.05	38	-4.95	-3.95

[a] Significantly different from the pre-SOX sample at the five percent level.
[b] Significantly different from the pre-SOX sample at the ten percent level.

Table 4.13 — Frequency of de-listing reason.

De-listing sample broken down by reason for de-listing. The merger or tender offer category includes those firms that de-listed because they either merged with or were purchased by another firm that itself was either listed or not listed on a major US exchange. The company decision category includes those firms that voluntarily de-listed from a major US exchange. The exchange decision includes those firms that were involuntarily de-listed by the exchange, typically for failure to meet the exchange's listing standards.

	Merger or tender offer					
Date range	Acquirer US listed	Acquirer not US listed	Company decision	Exchange decision	Other	Total
Panel A: Full de-listed sample						
1/1/2000 - 7/25/2002	33	16	20	30	2	101
	33%	16%	20%	30%	2%	100%
7/26/2002 - 8/31/2004	25	12	27	13	1	78
	32%	15%	35%	17%	1%	100%
Total	58	28	47	43	3	179
	32%	16%	26%	24%	2%	100%
Panel B: Regression sample						
1/1/2000 - 7/25/2002	16	11	9	8		44
	36%	25%	20%	18%		100%
7/26/2002 - 8/31/2004	14	6	15	3		38
	37%	16%	39%	8%		100%
Total	30	17	24	11		82
	37%	21%	29%	13%		100%

Table 4.14 — Cross-sectional regression results.

Cross-sectional regression results within which the dependent variable is the firm's de-listing announcement return and the independent variables are previously defined firm characteristics. Standard errors are in parenthesis.

Variable	(1)	(2)	(3)
INTERCEPT	19.82	1.42	-24.47
	(47.71)	(45.37)	(15.42)
LEV	-0.14	-0.18	-0.09
	(0.19)	(0.19)	(0.19)
LEV × POST	0.19	0.24	0.17
	(0.35)	(0.35)	(0.33)
LOGMV	1.54	2.07	2.10
	(2.13)	(2.08)	(2.05)
LOGMV × POST	-1.05	-1.60	-2.05
	(3.26)	(3.23)	(3.04)
MTB	0.22	-0.01	0.40
	(0.80)	(0.78)	(0.77)
MTB × POST	-0.83	-0.61	-0.99
	(0.98)	(0.96)	(0.94)
NASLS	0.02	0.07	0.02
	(0.13)	(0.13)	(0.13)
NASLS × POST	0.15	0.09	0.14
	(0.21)	(0.21)	(0.19)
OWNS	0.26	0.26	0.27
	(0.32)	(0.32)	(0.31)
OWNS × POST	-0.09	-0.07	-0.09
	(0.43)	(0.43)	(0.41)
ROA	0.17	0.18	0.17
	(0.13)	(0.13)	(0.12)
ROA × POST	-0.19	-0.19	-0.16
	(0.56)	(0.56)	(0.49)
USTO	0.19**	0.21*	0.19**
	(0.11)	(0.11)	(0.10)
USTO × POST	-0.23	-0.23	-0.23
	(0.16)	(0.16)	(0.16)
ASCORE	-0.57	-0.10	
	(0.71)	(0.61)	
ASCORE × POST	0.32	-0.07	
	(0.90)	(0.79)	
SSCORE	4.89		4.03
	(3.87)		(3.15)
SSCORE × POST	-3.33		-3.07
	(5.03)		(4.30)
POST	-9.59	7.15	21.10
	(58.73)	(56.72)	(22.44)
F	0.82	0.81	1.03
N	82	82	86
R^2	0.20	0.18	0.21

* Significantly different from zero at the five percent level.

** Significantly different from zero at the ten percent level.

Table 4.15 — Descriptive statistics de-listed sample versus control group.

Sample statistics for the select firm variables used in the de-listing logistic regressions. ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. LEV is the ratio of the firm's fiscal year end long-term debt to net total assets. LOGMV is the natural logarithm of the firm's market value of equity. MTB is the firm's market-to-book ratio. NASLS is the ratio of North American sales to total sales. OWNS is the year-end percentage of total shares outstanding held by the firm's managers, officers, and directors. ROA is the firm's fiscal year end operating income before depreciation divided by net total assets. USTO is the twelve-month average of the monthly total number of shares traded in the US divided by the monthly average number of shares outstanding in the US. Panel A includes data on those firms that de-listed on or before June 25, 2002 (pre-SOX). Panel B includes data on those firms that de-listed after June 25, 2002 (post-SOX). None of the firms used in the logistic regression were missing any data.

	---------De-listing sample---------			---------Control sample---------		
	Mean	Median	St. Dev.	Mean	Median	St. Dev.
Panel A: Pre-SOX transactions (N=28)						
ARSOX				-2.42	-0.60	14.05
ARSOXPASS				-0.83	-0.38	12.69
LEV	23.71	15.84	26.41	20.90	19.13	16.78
LOGMV	3,544.94	430.21	8,777.40	12,684.59	1,981.72	32,411.98
MTB	1.19	1.37	7.46	4.21	2.40	5.21
NASLS	29.75	14.57	37.26	36.21	32.73	31.64
OWNS	8.54	1.86	15.30	10.33	2.53	14.97
ROA	-4.06	5.84	43.72	12.54	13.07	10.57
USTO	6.76	4.99	6.36	12.94	7.94	12.69
	---------De-listing sample---------			---------Control sample---------		
	Mean	Median	St. Dev.	Mean	Median	St. Dev.
Panel B: Post-SOX transactions (N=20)						
ARSOX	-1.86	-2.37	19.42	-14.15	-13.17	17.87
ARSOXPASS	-0.85	1.26	12.83	-4.38	-5.10	10.05
LEV	15.51	13.24	16.54	13.84	13.61	10.58
LOGMV	2,691.69	908.76	4,934.98	7,748.39	2,572.19	12,822.34
MTB	5.19	2.31	11.25	2.73	1.88	2.49
NASLS	37.33	25.79	39.80	37.51	31.81	30.77
OWNS	8.00	0.50	15.25	4.35	0.72	9.06
ROA	4.63	8.57	15.82	8.45	8.56	9.56
USTO	15.35	10.02	16.15	25.50	14.31	31.51
	---------De-listing sample---------			---------Control sample---------		
	Mean	Median	St. Dev.	Mean	Median	St. Dev.
Panel C: All transactions (N=48)						
ARSOX				-7.31	-3.57	16.63
ARSOXPASS				-2.31	-1.97	11.68
LEV	20.29	14.11	22.98	17.96	17.40	14.81
LOGMV	3,189.42	705.23	7,367.80	10,627.84	2,382.52	26,000.25
MTB	2.86	1.52	9.34	3.60	2.19	4.32
NASLS	32.91	19.38	38.10	36.75	32.73	30.95
OWNS	8.32	1.29	15.12	7.83	1.61	13.07
ROA	-0.44	7.52	34.90	10.83	12.43	10.26
USTO	10.34	6.75	12.12	18.17	9.77	23.09

Table 4.16 — Determinants of the de-listing decision.
Logistic regression results where the dependent variable equals zero if the firm de-listed their shares from either AMEX, NASDAQ, or the NYSE on or before June 25, 2002 (the pre-SOX period) or one if the firm de-listed after June 25, 2002 (the post-SOX period). LEV is the ratio of the firm's fiscal year end long-term debt to net total assets. MTB is the firm's market-to-book ratio. NASLS is the ratio of North American sales to total sales. OWNS is the year-end percentage of total shares outstanding held by the firm's managers, officers, and directors. ROA is the firm's fiscal year end operating income before depreciation divided by net total assets. USTO is the twelve-month average of the monthly total number of shares traded in the US divided by the monthly average number of shares outstanding in the US. ARSOX (ARSOXPASS) represents the cumulative abnormal returns calculated over the twenty-one (nine) SOX announcement dates specified in appendix A. Standard errors are in parenthesis.

	All	Pre-SOX sample	Post-SOX sample		
	(1)	(2)	(3)	(4)	(5)
INTERCEPT	1.03**	1.76*	2.75**	2.64**	2.24
	(0.55)	(0.82)	(1.60)	(1.57)	(1.45)
LEV	0.00	0.01	-0.05	-0.05	-0.06
	(0.01)	(0.02)	(0.05)	(0.05)	(0.04)
MTB	0.02	-0.05	0.06	0.06	0.09
	(0.04)	(0.07)	(0.07)	(0.07)	(0.06)
NASLS	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
OWNS	0.00	0.00	0.05	0.05	0.04
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)
ROA	-0.05*	-0.04**	-0.09	-0.08	-0.06
	(0.02)	(0.02)	(0.06)	(0.05)	(0.05)
USTO	-0.04*	-0.11*	-0.05**	-0.05**	-0.06*
	(0.02)	(0.05)	(0.03)	(0.03)	(0.03)
ARSOX			0.06**	0.05**	
			(0.04)	(0.03)	
ARSOXPASS			-0.03		0.03
			(0.05)		(0.03)
% CONCORDANT	66.30	76.70	80.80	79.00	76.30
% DISCORDANT	33.30	23.10	19.30	21.00	23.80
% TIED	0.30	0.30	0.00	0.00	0.00
N	96	56	40	40	40

* Significantly different from zero at the five percent level.
** Significantly different from zero at the ten percent level.

CHAPTER 5

CONCLUSION

How do Systematic Risk and Managerial Ability Affect Relative-Performance-Based Compensation?

Executive compensation theory predicts a negative relation between executive pay and peer firm stock returns. Nevertheless, empirical tests yield conflicting results. Interest is increasing in research dedicated to solving this unsolved "puzzle" [Aggarwal and Samwick (1999), Bizjak, Lemmon, and Naveen (2004), Garvey and Milbourn (2003), Himmelberg and Hubbard (2000), Oyer (2004), Rajgopal, Shevlin, and Zamora (2006)].

In the spirit of these studies, I test two alternative hypotheses regarding the impact of peer firm stock returns on executive pay: (i) the noisy evaluation hypothesis, and (ii) the managerial ability hypothesis. The noisy evaluation hypothesis predicts that peer firm stock returns have a greater (more negative) impact on executive compensation in high beta firms. The managerial ability hypothesis predicts that peer firm stock returns have a greater (more negative) impact on the compensation of lower-ability executives.

Based on a sample of 323 CEOs from 1996 to 2000, I find evidence supporting both hypotheses. Specifically, I find that peer firm stock returns have a lesser impact on CEO pay when: (i) the firm's stock return sensitivity to systematic risk is low, and (ii) the executive's managerial ability level is high. Consistent with theories of relative performance evaluation developed in Holmstrom (1979), Lazear and Rosen (1981), Himmelberg and Hubbard (2000), and Oyer (2004), these findings suggest that firms practice relative performance evaluation selectively — when it is more or less consistent with the goals of agency problem resolution and incentive maximization. The findings also suggest that firm- and executive- specific characteristics can counteract the incentive effects of peer-return-based performance criteria.

This study leaves several areas for future research. First, a similar study of the relationship between executive pay and peer firm *accounting* performance would be useful since executive performance is typically not only evaluated using stock returns. A finding of a significant relationship would not be surprising since employment agreements frequently tie annual bonuses to accounting-based performance measures, rather than stock performance. Second, a re-examination of the results from earlier studies of the relation between executive pay and peer firm performance seems justified given our updated understanding of the difference between value of stock options and restricted stock grants to executives and their cost to the firm.

The Effects of the Sarbanes-Oxley Act of 2002 on Cross-Listed Firms and Cross-Listing Activity on US Stock Markets

I study the response of cross-listed firms to the Sarbanes-Oxley Act of 2002 (SOX). Overall, I find significant mean cumulative abnormal returns of -3.81% and -1.87% for the full sample of non-US firms using two sets of event dates corresponding to news announcements foreshadowing SOX. When I bifurcate the sample by home-country level of accounting standards and shareholder legal rights, I find that the mean abnormal returns for the low-standards country firms are significantly greater than those of their high-standards country counterparts.

Cross-sectional analysis of the SOX announcement returns indicates a positive and significant relation between the announcement returns and firm size, growth opportunities, and US market liquidity, and a negative and significant relation with the level of home-country accounting standards. Consistent with the predictions in Holmstrom and Kaplan (2003) and the results in Engel, Hayes, and Wang (2004), these findings suggest that small, non-US firms responded less favorably to SOX than large firms did, possibly due to their inability to accommodate the fixed costs associated with SOX compliance. This finding supports the recent statement by Neal Wolkoff, chair and CEO of the American Stock Exchange, that "Development-stage companies with little or no revenue cannot afford burdensome compliance costs." The positive and significant relation between the announcement returns and firm growth opportunities supports the bonding hypothesis, which predicts that non-US firms with greater growth opportunities will respond favorably to regulatory changes that reinforce their commitment to responsible corporate behavior in the eyes of outside investors. The negative and significant relation with the level of home-country accounting standards also supports the bonding hypothesis — suggesting that firms from countries with low-level standards responded better to the SOX-mandated accounting changes than others.

Analysis of post-SOX cross-listing and de-listing activity indicates a significant decline in the number of new non-US firm listings on the major US stock exchanges. While, in and of itself, this is not direct evidence that SOX has changed non-US firms' choice of overseas listing venue, when combined with both anecdotal and empirical evidence from this and other studies, it certainly seems to suggest that firms are more cautious about their decision to come to the US in the post-SOX era. The firms that de-listed post-SOX were, on average, based in countries with lower level accounting standards and shareholder rights than those that de-listed pre-SOX. Finally, analysis of the reasons that firms gave for giving up their US listing indicates an increase in the percentage of firms that voluntarily de-listed their shares in the post-SOX period as compared to the pre-SOX period.

REFERENCES

Abowd, J.M., Kaplan, D.S., 1999. Executive Compensation: Six Questions That Need Answering. The Journal of Economic Perspectives 13, 145-168.

Aggarwal, R.K., Samwick, A.A., 1999. Executive Compensation, Strategic Competition, and Relative Performance Evaluation: Theory and Evidence. Journal of Finance 54, 1999-2043.

Akhigbe, A., Martin, A.D., 2006. Valuation impact of Sarbanes-Oxley: Evidence from disclosure and governance within the financial services industry. Journal of Banking & Finance 30, 989-1006.

Antle, R., Smith, A., 1986. An Empirical Investigation of the Relative Performance Evaluation of Corporate Executives. Journal of Accounting Research 24, 1-39.

Baker, G.P., Hall, B.J., 2004. CEO Incentives and Firm Size. Journal of Labor Economics 22, 767-798.

Baker, G.P., Jensen, M.C., Murphy, K.J., 1988. Compensation and Incentives: Practice vs. Theory. Journal of Finance 43, 593-616.

Barro, J.R., Barro, R.J., 1990. Pay, Performance, and Turnover of Bank CEOs. Journal of Labor Economics 8, 448-481.

Bebchuk, L.A., Fried, J.M., Walker, D.I., 2002. Managerial power and rent extraction in the design of executive compensation. University of Chicago Law Review 69, 751-846.

Bettis, J.C., Bizjak, J.M., Lemmon, M.L., 2005. Exercise behavior, valuation, and the incentive effects of employee stock options. Journal of Financial Economics 76, 445-470.

Bizjak, J.M., Lemmon, M.L., Naveen, L., 2004. Does the Use of Peer Groups Contribute to Higher Pay and Less Efficient Compensation? Working paper.

Blanchard, O.J., Lopez-de-Silanes, F., Shleifer, A., 1994. What do firms do with cash windfalls? Journal of Financial Economics 36, 337-360.

Boehmer, E., Musumeci, J., Poulsen, A.B., 1991. Event-study methodology under conditions of event-induced variance. Journal of Financial Economics 30, 253-272.

Brown, C., Medoff, J., 1989. The Employer Size-Wage Effect. Journal of Political Economy 97, 1027-1059.

Chance, D.M., Kumar, R., Todd, R.B., 2000. The 'repricing' of executive stock options. Journal of Financial Economics 57, 129-154.

Chhaochharia, V., Grinstein, Y., 2004. Corporate Governance and Firm Value -- the Impact of the 2002 Governance Rules. Working paper.

Coffee, J.C.J., 1999. The Future as History: The Prospects for Global Convergence in Corporate Governance and its Implications. Northwestern University Law Review 93, 641-708.

Coffee, J.C.J., 2002. Racing Towards the Top?: the Impact of Cross-Listings and Stock Market Competition on International Corporate Governance. Columbia Law Review 102, 1757-1832.

Cohen, D.A., Dey, A., Lys, T.Z., 2004. The Sarbanes-Oxley Act of 2002: Implications for Compensation Structure and Risk-Taking Incentives of CEOs. Working paper.

Core, J.E., Holthausen, R.W., Larcker, D.F., 1999. Corporate governance, chief executive officer compensation, and firm performance. Journal of Financial Economics 51, 371-406.

Coughlan, A.T., Schmidt, R.M., 1985. Executive compensation, management turnover, and firm performance: An empirical investigation. Journal of Accounting and Economics 7, 43-66.

Dechow, P.M., Sloan, R.G., 1991. Executive incentives and the horizon problem: An empirical investigation. Journal of Accounting and Economics 14, 51-89.

Doidge, C., Karolyi, G.A., Stulz, R.M., 2004. Why are foreign firms listed in the U.S. worth more? Journal of Financial Economics 71, 205-238.

Engel, E., Hayes, R.M., Wang, X., 2004. The Sarbanes-Oxley Act and Firms' Going-Private Decisions. University of Chicago Working Paper.

Fama, E.F., Fisher, L., Jensen, M.C., Roll, R., 1969. The Adjustment of Stock Prices to New Information. International Economic Review 10, 1-21.

Fuerst, O., 1998. A Theoretical Analysis of the Investor Protection Regulations Argument for Global Listing of Stocks. International Center for Finance at Yale Working Paper.

Garvey, G., Milbourn, T., 2003. Incentive Compensation When Executives Can Hedge the Market: Evidence of Relative Performance Evaluation in the Cross Section. The Journal of Finance 58, 1557-1581.

Gibbons, R., Murphy, K.J., 1990. Relative Performance Evaluation for Chief Executive Officers. Industrial and Labor Relations Review 43, 30S-51S.

Gillan, S.L., Hartzell, J.C., Parrino, R., 2005. Explicit vs. Implicit Contracts: Evidence from CEO Employment Agreements. Working paper.

Grossman, S.J., Hart, O.D., 1983. An Analysis of the Principal-Agent Problem. Econometrica 51, 7-46.

Guidry, F., Leone, A.J., Rock, S., 1999. Earnings-based bonus plans and earnings management by business-unit managers. Journal of Accounting and Economics 26, 113-142.

Hall, B.J., Murphy, K.J., 2002. Stock options for undiversified executives. Journal of Accounting and Economics 33, 3-42.

Hayes, R.M., Schaefer, S., 1999. How much are differences in managerial ability worth? Journal of Accounting and Economics 27, 125-148.

Healy, P.M., 1985. The effect of bonus schemes on accounting decisions. Journal of Accounting and Economics 7, 85-107.

Healy, P.M., Wahlen, J.M., 1999. A Review of the Earnings Management Literature and Its Implications for Standard Setting. Accounting Horizons 13, 365-383.

Himmelberg, C.P., Hubbard, R.G., 2000. Incentive Pay and the Market for CEOs: An Analysis of Pay-for-Performance Sensitivity. Working paper.

Holmstrom, B., 1979. Moral Hazard and Observability. The Bell Journal of Economics 10, 74-91.

Holmstrom, B., 1982. Moral Hazard in Teams. The Bell Journal of Economics 13, 324-340.

Holmstrom, B., Kaplan, S.N., 2003. The State of U.S. Corporate Governance: What's Right and What's Wrong. NBER Working Paper.

Holmstrom, B., Milgrom, P., 1987. Aggregation and Linearity in the Provision of Intertemporal Incentives. Econometrica 55, 303-328.

Holthausen, R.W., Larcker, D.F., Sloan, R.G., 1995. Annual bonus schemes and the manipulation of earnings. Journal of Accounting and Economics 19, 29-74.

Jensen, M.C., Meckling, W.H., 1976. Theory of the firm: Managerial behavior, agency costs and ownership structure. Journal of Financial Economics 3, 305-360.

La Porta, R.L., Lopez-de-Silanes, F., Shleifer, A., Vishny, R.W., 1998. Law and Finance. Journal of Political Economy 106, 1113-1155.

Lambert, R.A., Larcker, D.F., 1987. An Analysis of the use of Accounting and Market Measures of Performance in Executive Compensation Contracts. Journal of Accounting Research 25, 85-125.

Lazear, E.P., Rosen, S., 1981. Rank-Order Tournaments as Optimum Labor Contracts. The Journal of Political Economy 89, 841-864.

Leuz, C., Triantis, A., Wang, T., 2004. Why Do Firms Go Dark? Causes and Economic Consequences of Voluntary SEC Deregistrations. Working paper.

Li, H., Pincus, M., Rego, S.O., 2004. Market Reaction to Events Surrounding the Sarbanes-Oxley Act of 2002. Working paper.

Licht, A.N., 2001. Managerial Opportunism and Foreign Listing: Some Direct Evidence. Working paper.

Licht, A.N., 2003. Cross-Listing and Corporate Governance: Bonding or Avoiding? Chicago Journal of International Law 4, 141-164.

Linck, J.S., Netter, J.M., Yang, T., 2005. Effects and unintended consequences of the Sarbanes-Oxley Act on corporate boards. University of Georgia Working Paper.

Main, B.G.M., 1991. Top Executive Pay and Performance. Managerial and Decision Economics 12, 219-229.

Mirrlees, J.A., 1976. The Optimal Structure of Incentives and Authority within an Organization. The Bell Journal of Economics 7, 105-131.

Mookherjee, D., 1984. Optimal Incentive Schemes with Many Agents. The Review of Economic Studies 51, 433-446.

Murphy, K.J., 1999. Executive Compensation. In: Ashenfelter O & Card D (eds.) Handbook of Labor Economics. North Holland.

Oyer, P., 2004. Why Do Firms Use Incentives That Have No Incentive Effects? Journal of Finance 59, 1619-1649.

Perino, M.A., 2005. American Corporate Reform Abroad: Sarbanes-Oxley and the Foreign Private Issuer. St. John's Legal Studies Research Paper.

Rajgopal, S., Shevlin, T.J., Zamora, V., 2006. CEOs' Outside Employment Opportunities and the Lack of Relative Performance Evaluation in Compensation Contracts. forthcoming, Journal of Finance.

Reese, J., William A., Weisbach, M.S., 2002. Protection of minority shareholder interests, cross-listings in the United States, and subsequent equity offerings. Journal of Financial Economics 66, 65-104.

Rosen, S., 1982. Authority, Control, and the Distribution of Earnings. The Bell Journal of Economics 13, 311-323.

Ross, S.A., 1973. The Economic Theory of Agency: The Principal's Problem. The American Economic Review 63, 134-139.

Sarkissian, S., Schill, M.J., 2003. Are there permanent valuation gains to overseas listing? Evidence from market sequencing and selection. Darden School of Business Working Paper No. 03-03.

Sefcik, S.E., Thompson, R., 1986. An Approach to Statistical Inference in Cross-Sectional Models with Security Abnormal Returns As Dependent Variable. Journal of Accounting Research 24, 316-334.

Shavell, S., 1979. Risk Sharing and Incentives in the Principal and Agent Relationship. The Bell Journal of Economics 10, 55-73.

Shivdasani, A., Yermack, D., 1999. CEO Involvement in the Selection of New Board Members: An Empirical Analysis. The Journal of Finance 54, 1829-1853.

Stulz, R.M., 1999. Globalization, Corporate Finance, and the Cost of Capital. Journal of Applied Corporate Finance 12, 8-25.

White, H., 1980. A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity. Econometrica 48, 817-838.

Wooldridge, J.M., 2002. Econometric Analysis of Cross Section and Panel Data. The MIT Press, Cambridge, Massachusetts; London, England.