THREE ESSAYS ON CORPORATE GOVERNANCE, MANAGERIAL INCENTIVES, AND GOVERNMENT EXPENDITURES

By

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Chapter 1

Introduction

A relatively new area of economics seeks to understand the decision making and internal processes of organizations. In the context of firms, there are usually laws and contracts that control the firm's operations. These controls help secure firms' future outside financing. Understanding how and whether the control mechanisms work is therefore very important, and has become an active field of economic and financial research.

The first two essays in this dissertation are devoted to studying the determinants of corporate control instruments. The first chapter examines the role of large investors, in particular that of financial institutions who are often considered to be passive on control issues. An important implication from theory indicates that a large investor improves a firm's performance through monitoring. The empirical literature provides some evidence for enhanced performance at firms with existing large investor, but due to the empirical approach it does not tell us much about how the large investors emerge, while the theory has also implied that the incentive to acquire large holdings should increase when the demand for monitoring goes up, as credible monitoring requires large shareholding. To test the hypothesis that large shareholding will be more likely when the value of monitoring rises, I identify proxies for value of monitoring based on agency problems. Using data on institutional ownership and firm characteristics for large U.S. public companies from 1992 to 2001, I find that the firms where shareholder monitoring could be more valuable are indeed more likely to have a large institutional holding. To rule out the possibilities that large institutions arise for non-monitoring reasons somehow related to the agency measures, I further verify that large institutional holdings indeed lead to more monitoring. I find that large institutions are linked to less empire building behavior,

as evident in both capital expenditure and acquisition activities. I do not find strong evidence for large institutions facilitating value-increasing takeovers, although they do have small negative effect on firm's takeover defense. Together with the result that institutions choose to acquire large holdings of firms where their monitoring could be profitable, this study gives further support for institutional investors' active role in corporate governance.

The second essay tests how market competitiveness affects the structure of managerial compensation contracts. Agency theory predicts the use of relative performance evaluation (RPE) in incentive contracts – that RPE helps filter a common industry or market effect by evaluating an individual firm's performance relative to other firms facing the same effects. The literature suggests that product market competition enhances the usefulness of RPE, because in more competitive environments managers are more likely to face similar uncertainties, have more peers, and are less likely to affect other firms' output. This paper empirically confirms this argument using the 1996 deregulation of the U.S. telecommunications industry as a natural experiment. This event exogenously intensified competition in the industry, which helps overcome the empirical limitations common to existing studies. We adopt a difference-in-differences design using size- and performance-matched manufacturing firms as the benchmarks. The results indicate that, relative to the benchmark firms, telecommunications firms strengthened the (negative) tie between managerial compensation and peer performance after the Act. The effect is significant for peer stock returns, and in the same direction, though not significant, for peer accounting return on equity. The results suggest that relative performance evaluation became more valuable for telecommunication firms after the Act.

The third essay studies the response of state spending to windfall revenue using tobacco settlements in the U.S. in the late 1990s as a natural experiment. The settlements commit tobacco companies to making annual payments to perpetuity, with total payments over the first 25 years estimated at several hundred billion dollars. The magnitude and duration of the payments provide a unique natural experiment to study individual state spending responses to the windfall receipts, and re-evaluate the "flypaper effect". Since the tobacco

payments are not only unconditional non-earmarked lump-sum transfers to state governments, but also exogenous to policy variations, this study avoids potential estimation bias that is often criticized in the "flypaper effect" literature. We adopt a first differencing approach and find strong evidence of the flypaper effect. State direct spending increases by about $1 in response to a one dollar increase in settlement funds, and by about 8 cents for a similar increase in private income. States also respond differently to the windfall revenue. Those with a higher adult smoking prevalence rate, or with a large tobacco-manufacturing sector, exhibit a lower spending propensity.

Chapter 2

Institutional Investor Activism Revisited: Investment Targeting and Evidence of Control

2.1 Introduction

We are often concerned that managers at public corporations may not work in the best interest of shareholders. They need to be monitored sometimes replaced. Theory has suggested that shareholders, especially large ones, can play the function of monitoring the managers, affect the decision-making and ultimately the value of their investment[1]. Is this really the case? Existing empirical literature tests the theoretical implication, and some studies find that the existence of a large investor does enhance firm performance. This provides partial evidence of the theory though, because it only shows what happens to firm performance when a large investor already exists. The empirical evidence does not tell us much about how the large investors emerge, while the theory has also implied that the incentive to acquire large holdings should increase when the demand for monitoring goes up, as credible monitoring requires large shareholding.

In this chapter, I empirically test the latter implication. I look at financial institutions (such as mutual funds, etc) in particular, not only because institutional ownership in the

[1] This is because monitoring in public companies with dispersed ownership has public good properties. By resolving the free-rider problem associated with costly monitoring, the presence of large shareholders can enhance firm value. There are different mechanisms by which large shareholder enhances value. For instance, the study by Shleifer and Vishny (1986) shows large shareholders improve firm values by bringing in value-increasing takeovers. Chidambaran and John (1998) present a mechanism where the large investor acquires private information and conveys that to other smaller investors. Admati and Pfleiderer (2006) argue the threat of exit-by-feet or "Wall Street Walk" can also be a form of activism, contrary to the common belief that liquidity hinders investor monitoring. In practice, shareholder activism often takes the form of involvement in the regulation of institutional investors, voting in board elections, submitting proxy proposals, directly negotiating with the management, or targeting firms publicly through media.

U.S. has become so important[2], but also that the way they invest is quite puzzling. As financial intermediaries who aggregate smaller investments allowing individual investors to benefit from diversification and lower transaction cost, they are most likely to remain passive on corporate control issues (often labeled by following the "Wall Street Rule"). However, what we have observed in recent years is quite the opposite. Institutions often hold significant positions within large firms that may be hard to justify by risk and return only. This study thus contributes to the existing empirical literature on investor activism by addressing the motivation of block institutional holding that is usually missing in the literature, and providing more supportive evidence of activism. I also incorporate a richer set of determinants for ownership structure, with measures related to the agency problems due to firm's underlying uncertainty, but also related to agency problems that are caused by the lack of alternative control instruments. These measures later serve as instruments for the ownership variable when I study the evidence of control.

My approach to the problem consists of two steps. I first test whether the tendency of having large stake is correspondent with the potential benefits offered by monitoring. To assess how large the return is for monitoring, I use proxy variables that are related to the degree of information asymmetry between management and owners. I then examine whether we can draw inferences of monitoring for firms with large institutional investor. For the first step, I identify the presence of a large institutional investor (defined as those with block holdings of at least 5% of the outstanding equity) in nearly 1400 large US public corporations during the years 1992-2001. In order to test that these large institutional holdings arise from potential benefits of monitoring, variations in the value of monitoring are necessary. There are two main comparative statics. First, the limit to takeover threat calls for a bigger role of shareholder monitoring. As the discipline of takeover is reduced at some firms due to their legislative arrangements that fostered stronger anti-takeover protection, the return for internal monitoring becomes greater. I measure the takeover pressure through an index developed by Gompers et al (2003) that quantifies anti-takeover related amendments and legislation at both state and firm levels. The second prediction is that, uncertainty at the individual firm level makes shareholder

[2] The percentage shareholding among U.S. public companies has surpassed 50% in the mid 1990s.

monitoring more rewarding. When there is little idiosyncratic uncertainty, firm performance is revealing of manager's effort input, and it would not pay for the institutions to monitor. When firm-specific uncertainty or risk is high, it becomes difficult for the market to determine whether poor performance is driven by manager slacking or unfavorable shocks. And monitoring, if generates additional information on management beyond what is revealed from a public signal such as firm profit, would help strengthen managerial incentives. Firm-specific uncertainty is proxied by the residual standard deviation from the CAPM regression. However, large shareholding for monitoring purpose involves the opportunity cost of less diversification, and uncertainty exacerbates the problem. I expect this cost of risk exposure is relatively low compared to the benefit of monitoring, when uncertainty is low or firms are small.

The empirical results are consistent with the predictions. Firms are more likely to have large institutional investors when institutional arrangements make takeovers harder. Firm-specific risk has a positive impact, but the effect is weaker at a higher level of risk, consistent with an increasing cost due to deviation from perfect diversification. Comparative statics for other firm characteristics related to capital structures are also presented. The results are not sensitive to alternative specifications or the presence of endogeneity bias.

To exclude possibilities that institutions hold large stake for reasons other than monitoring, I further test whether larger institutional holdings indeed relate to firm outcomes in ways that are consistent with monitoring. Various venues through which large institutions improve the firm's status quo are investigated. There is some evidence that large institutions affect firm's real decision making: I find large institutions are linked to less empire building behavior, as evident in both capital expenditure and acquisition activities. I do not find strong evidence for large institutions facilitating takeovers, although they do have small negative effect on firm's takeover defense. Together with the result that institutions choose to acquire large holdings in firms where their monitoring could be profitable, this study gives further support for institutional investors' active role in corporate governance.

The chapter is organized in the following way. In the next section, I summarize the relevant empirical research. I then outline a theoretical framework, followed by a discussion of the empirical method and data used. The main empirical results are presented in 2.4. Section 2.5 presents the ex post evidence of monitoring. And 2.6 concludes.

2.2 Related Literature

Prior empirical research on investor activism usually examines the impact on firm performance, due to large outside investors that are already in place. In the common approach, the presence of a large shareholder is taken as given, and used as a proxy for shareholder monitoring efforts. If a positive correlation is found between the presence of large shareholders and firm performance, it is interpreted as the evidence for effects of monitoring. This empirical approach does not give much consideration for the potential endogeneity in the ownership variable, and therefore makes the interpretation of results more difficult. For instance, if firms without large investor are found to be associated with poorer performance, one can argue that investors are probably attracted to firms with better outlook. Does the relation merely reflect underlying investor preference? Does it mean that lower ownership concentration is an inferior arrangement, while it could just be the case that there is little room for improvement hence tighter control is not necessary. Due to the presumption of existing large stakes, the empirical studies do not provide a complete picture of the relationship between ownership structure and monitoring, and leave open the question of what motivates large shareholding. As examples of the empirical studies in this line, McConnell and Servaes (1990), Holderness and Sheehan (1988) link institutional ownership to firm profitability and shareholder value. Among them, some rely on event-study methods to examine excess returns around the announcement date of outsiders' large equity acquisitions (such as Mikkelson and Ruback (1985), Holderness and Sheehan (1985), Barclay and Holderness (1991). In general no significant abnormal returns are found. Some studies examine the market reaction to shareholder proxy proposal submission or other public targeting methods.

Using publicly observable activity as evidence of control offers a direct measure for monitoring, but is also subject to certain limitations[3].

Other than firm performance, studies have also looked at whether large investor presence has an impact on real decision making of firms, especially on the extent of agency problems. Bertrand and Mullainathan (1999), Bertrand and Mullainathan (2001), and Hartzell and Starks (2003) examine managerial incentives, and find that the existence of large investors is negatively associated with manager's discretion to set their own pay. Bushee (1998) shows that institutional ownership correlates to smaller chance of managers myopically cutting R&D expenses to meet short-run earnings benchmarks. In a study of U.S. M&A activities, Qiu (2008) uses instrumental variable approach and finds that the presence of large public pension fund ownership discourages managers acquiring others firms, especially in cash-rich firms. These studies all show that large investor helps mitigate agency problems, but are silent on whether existing agency problems attract large shareholding to solve the problems.

There exists a smaller literature that explores the endogenous determination of block ownership. Himmelberg et al (1999) focus exclusively on managerial stock ownership. They show that the level of managerial ownership is endogenously determined by the scope of moral hazard problems[4]. Demsetz and Lehn (1985) acknowledge the benefits of control as one important determinant of ownership concentration, and show how ownership concentration varies systematically with firm characteristics for a cross-section of large U.S. firms during 1976-1980.

There are some major differences that distinguish this study. First, institutional investors are not only important for studying U.S. ownership structure nowadays, but also serve as a good benchmark for studying the motivation for large holdings. We can not learn much

[3] Under the circumstance that control takes the form of private communication between the management and the investor, the extent of activism may be understated. On the other hand, a public targeting method such as proposal submission may signal that the communication with incumbent manager fails or problems are harder to fix, and cause negative market reaction to the event.

[4] Among the measures are firm size, scopes of discretionary spending (proxied by R&D, advertisement spending), and risk.

of what determines the block holding by financial institutions from Himmelberg et al (1999) and Demsetz and Lehn (1985), either because the data covered the time before institutional ownership became important, or due to a small perspective of ownership structure under study (managerial ownership). Secondly, monitoring activities could contain helping set the proper managerial incentives, among many other things. As Himmelberg et al (1999) show, level of managerial incentive results from an equilibrium contractual arrangement that reflects the existence of moral hazard problems. However, the incentive scheme is only one of the possible outcomes that can infer the effects of monitoring. A more general question therefore is: are investors more likely to monitor when moral hazard problems are more serious? If so, is there any evidence of monitoring activity? This is what this study tries to answer. I provide evidence of monitoring by checking a list of variables that may gauge the outcomes of monitoring, in case firm profitability is too noisy as a measure for monitoring.

2.3 Determinants of Institutional Holding -- Theoretical Framework

It has been widely recognized that the separation of ownership and control is associated with agency problems, because self-interested corporate managers tend to make decisions that are not in the best interests of shareholders. A conventional case is about manager's effort provision, since providing effort generates private cost for the mangers while most of the benefits accrue to the shareholders[5]. Another common scenario of agency conflicts arises with acquisitions that reduce bidder shareholder value, which happens when unconstrained managers enjoy building empires instead of investing in the best interest of the shareholders.

Earlier studies suggest that the existence of agency problems has negative impact on future firm value[6]. The study of corporate governance is centered on the problem of how to minimize the costs related to agency conflicts. To understand when monitoring

[5] The issue of managerial shirking is reflected from the observations that mangers often seek "quiet life". For example, they forego positive net present value investment projects because investments impose private cost on the managers (e.g., Aggarwal and Samwick (2006), Bertrand and Mullainathan (2003), Bohren et al (2007)).
[6] An example is the study by Gompers et al (2003).

becomes more valuable and when institutional investors establish large stakes to monitor, it is important to recognize the simultaneous existence of alternative control mechanisms. The theoretical framework for this study is adapted from Holmstrom's well-known theory of monitoring which shows that monitoring adds value by reducing the information asymmetry problem. From there, I derive investor's net benefit from monitoring, and identify the exogenous factors that influence investor's incentive to acquire large stake and monitor.

2.3.1 The Agency Problem

Benchmark Case

First recall the conventional incentive provision problem with information asymmetry and differential risk tolerance between the principal (owner) and the agent (manager). As usual, assume that owners are risk-neutral. This assumption can be justified if owners hold well-diversified portfolios.

The risk-averse manager chooses effort e and receives w in compensation. Her cost of effort is given by $C(e) = \frac{e^2}{2}$. Assume her utility takes the form of $U_M = 1 - e^{-r_M(w - C(e))}$, where r_M is the coefficient of absolute risk aversion. The manager's outside option gives her a reservation utility of 0, and she has no bargaining power.

The firm's profit is determined by manager's effort input and a random shock: $y = e + \varepsilon$, where $\varepsilon \sim N(0, \sigma_\varepsilon^2)$. Without monitoring, firm profit is the only verifiable signal on which the compensation contract can be based. The manager's pay contract takes the form $w = \beta_0 + \beta_1 y$.

Manager chooses her effort taking the contract terms as given, at a level $e^* = \beta_1$. The manager's negative exponential utility function allows us to write the certainty-equivalent of her utility in terms of the mean and variance of her compensation:

$$CE_M = \beta_0 + \frac{\beta_1^2}{2} - \frac{r_M}{2}\beta_1^2\sigma_\varepsilon^2 .$$

The owner's maximization problem is to choose the set of parameters of pay contracts that maximizes $E[y-w]$ subject to manager's incentive constraint $e^* = \beta_1$ and participation constraint $CE_M = 0$. Under this standard construction, the solution of owner's maximization problem provides an optimal contract characterized by:

[2.1] $$\beta_1^* = \frac{1}{1+r_M\sigma_\varepsilon^2} \text{ and } \beta_0^* = -\frac{1-r_M\sigma_\varepsilon^2}{2(1+r_M\sigma_\varepsilon^2)^2} .$$

In equilibrium, manager's effort is given by $e^* = \frac{1}{1+r_M\sigma_\varepsilon^2}$. Owner's expected payoff equals $V^*_{Benchmark} = \frac{1}{2(1+r_m\sigma_\varepsilon^2)}$.

The main implication from this model is that the principal and agent have to compromise between optimal provision of incentives and optimal allocation of risk. When there exist gains from insurance, the first best outcome (in which $e^* = 1$) is generally not achievable. As the manager becomes more risk-averse (represented by higher r_M), weaker incentive will be provided. And the more severe the asymmetry of information (higher σ_ε^2), the weaker the incentive; in the extreme, the manger's pay should not be based on the signal when it does not tell owners anything about the performance of the manager. Meanwhile the manager demands a higher risk premium (in the form of β_0) as a compensation for being exposed to firm risk. The need for insurance therefore creates agency cost. However, this agency problem can be mitigated with owners who monitor.

With Monitoring

Monitoring refers to the activities to help better infer managerial inputs beyond the publicly observable firm profit. Monitoring activity in this context includes gathering information, or researching about the management. Suppose the monitor can observe a second signal of managerial effort input $s = e + u$, where $u \sim N(0, \sigma_u^2)$. The informativeness principle from Holmstrom (1979) implies that the signal should be

incorporated into the incentive contract as long as it contains additional information about managerial effort. This is the case when ε and u are not perfectly correlated. For simplicity, assume the correlation between them is zero.

The optimal wage contract now takes the form

$$w = \beta_0'' + \beta_1'' y + \beta_2'' s .$$

Manager's incentive constraint (IC) is given by:

[2.2] $$e'' = \beta_1'' + \beta_2'' .$$

The certainty-equivalent of manager's utility thus becomes:

$$CE_M = \beta_0 + \frac{1}{2}(\beta_1'' + \beta_2'')^2 - \frac{r_M}{2}(\beta_1''^2\sigma_\varepsilon^2 + \beta_2''^2\sigma_u^2) .$$

And her participation constraint (PC) gives:

[2.3] $$\beta_0'' = \frac{r_M}{2}(\beta_1''^2\sigma_\varepsilon^2 + \beta_2''^2\sigma_u^2) - \frac{1}{2}(\beta_1'' + \beta_2'')^2 .$$

Suppose monitoring cost is M. Then monitoring owner's problem is to maximize

$V_{Monitor} = E(y'' - w'' - M) = e'' - \beta_0'' - (\beta_1'' + \beta_2'')e'' - M$ subject to (IC) and (PC), which yields an optimal contract with the following parameters:

[2.4] $$\beta_1''^* = \frac{\sigma_u^2}{\sigma_\varepsilon^2 + \sigma_u^2 + r_M\sigma_\varepsilon^2\sigma_u^2}, \text{ and } \beta_2''^* = \frac{\sigma_\varepsilon^2}{\sigma_\varepsilon^2 + \sigma_u^2 + r_M\sigma_\varepsilon^2\sigma_u^2} .$$

These equations imply that the weight assigned to one signal in the compensation contract is smaller when the signal is noisier (or when the other signal is less noisy), as the signal becomes less informative and only introduces more risk exposure to the manager. In the extreme, when monitoring is perfectly revealing, i.e. $s = e$, the contract will be based solely on signal s: $w = \beta_0'' + \beta_1'' e$. In this special case, the optimal contract becomes:

[2.5] $$\beta_1''^* = 1 \text{ and } \beta_0''^* = -\frac{1}{2} .$$

The first-best effort level is achieved: $e''^* = 1$, and the owner's expected payoff becomes

[2.6] $$V_{Monitor}^* = \frac{1}{2} - M .$$

It is easy to show that monitoring adds firm value by improving performance incentives. This is because monitoring helps tie managerial rewards more closely to manager's actual performance rather than to firm profit, which is usually influenced by random events outside the control of managers. This can be shown by a positive change in firm's expected profit stream from the case without monitoring:

$$Ey^{"*} - Ey^{*} = Ee^{"*} - Ee^{*} = \beta_1^{"*} + \beta_2^{"*} - \beta_1^{*} = \frac{r_M \sigma_\varepsilon^4}{(\sigma_\varepsilon^2 + \sigma_u^2 + r_M \sigma_\varepsilon^2 \sigma_u^2)(1 + r_M \sigma_\varepsilon^2)} > 0 .$$

The owner will find it beneficial to monitor if her expected payoff with monitoring exceeds that from not monitoring:

$$V_{Monitor}^{*} \geq V_{Benchmark}^{*} \text{ or if}$$

[2.7] $$\frac{r_M \sigma_\varepsilon^4}{2(\sigma_\varepsilon^2 + \sigma_u^2 + r_M \sigma_\varepsilon^2 \sigma_u^2)(1 + r_M \sigma_\varepsilon^2)} \geq M .$$

2.3.2 The Decision to Monitor

The decision to monitor depends on her relative wellbeing under monitoring and not-monitoring. The extent of existing agency problems lowers the benchmark return and could increase the incentive to monitor.

Firm-specific Risk

From [2.7], it follows that the net benefit of monitoring increases with risk (σ_ε^2). This is because when there is little uncertainty, firm performance is fully revealing of managerial effort and the information asymmetry problem is less severe, it would not pay for institutions to monitor; when uncertainty becomes greater, it is harder for the owner (or the market) to determine whether a bad firm outcome is due to manager being lazy or some unfavorable shock.

However, when significant shareholding is a prerequisite for credible monitoring, the impact of potential diversion from diversification on the incentive to monitor should be

also considered. Monitoring has public-good property and is subject to the free-riding problem in public companies with dispersed ownership. Large shareholding can enhance the incentive to monitor by internalizing the benefit from monitoring. Credible monitoring thus requires large shareholding; however, this may force financial institutions to forego some benefits from diversification. The risk-neutrality assumption justified by well-diversified owner fails to capture an investor that monitors.

To illustrate the implication this may have on the monitoring decision, the above monitoring problem is modified for a risk-averse owner (as in Heinrich (2002)). The details are in Appendix A. The modified decision rule for the investor becomes:

[2.8] $$\frac{1}{4r_O\sigma_\varepsilon^2}\left(\frac{1}{2}-\frac{1}{1+r_M\sigma_\varepsilon^2}\right)\geq M$$

From [2.8], firm-specific uncertainty (or firm risk, given by σ_ε^2) affects the monitoring decision but the effect is less straightforward. Higher risk has two countervailing effects on the incentive to monitor. First, firm risk makes providing performance incentive more costly, hence monitoring becomes more valuable. When there is little uncertainty, firm profit or stock return fully reveals managerial performance; there remains little room for monitoring. While uncertainty increases, it becomes more difficult for the market to determine whether firm performance is driven by managerial effort or by exogenous factors; the agency problem becomes more acute. Secondly, higher firm risk exposes the investor to more risk when she holds a significant stake for monitoring purposes. I expect this cost of holding large stake varies with the level of risk itself, or with the size of the firm. At lower level of uncertainty, the cost of risk-bearing may be low relative to the benefit from monitoring; while at higher level of risk, the cost of risk-bearing may dominate. Firm size could reflect the share of wealth that the financial institution put into the given firm. The larger the firm, a given stake will imply a larger share of wealth, and greater risk exposure for the institutional investor.

Takeover Threat

The market for corporate control can be a solution for agency problems through hostile takeovers. In a hostile takeover the raider makes an offer to buy all or a fraction of

outstanding shares at a stated price. The takeover is successful if the raider obtains more than 50% of the voting shares which grants him effective control of the company and thus be able to appoint manager. In the literature for takeovers as solution for agency problems, takeover is generally modeled to change the distribution of information between the owners and managers. For example, Scharfstein (1988) shows takeover threats add value when prospective raiders gain informational advantage over incumbent shareholders[7]. There also exist models of takeover as source of extra agency costs. The counter-argument involves takeover threats inducing managerial myopia that leads to efficiency loss (in terms of sacrificed long term firm value): since the benefit partly goes to the new management, takeover induces manager to under-invest in profitable long-term projects with delayed returns.

Despite the different views towards takeover, existing empirical evidence converges to support the disciplinary view of takeover threats on firm management. In the current context, the existence of an active takeover market can translate to smaller net benefits of monitoring. In a setup where takeover threat discourages manager underperformance, I show how the incentive to monitor changes with the presence of takeover threat, and how this relationship is affected by legislative hurdles to takeovers. For takeovers presenting a threat to the manager, managers are allowed to earn firm-specific rent. Otherwise the manager can earn the same amount elsewhere in case he gets fired following a change in control, and therefore has no incentive to avoid being fired. Moreover for takeover threats to elicit greater effort from the manager, assume takeovers happen with positive probability and the probability of successful takeover depends on the market price of the firm without a takeover, such that by working harder, the manager can raise the market price and reduce the takeover risk.

[7] Inefficiency arises when managers have private information about the environment in which the firm operates, and the manger is not penalized enough for shirking because of the insurance provided by his incentive contract. An informed raider can mitigate this inefficiency problem by penalizing manager for bad effort.

Suppose the manager is able to extract rent *R* with the current firm. This could be the case when the manager has bargaining power[8]. The takeover threat is introduced after the contracting stage. The raider will be able to implement the first-best effort $e = 1$ after takeover and becoming the new manager. The cost of takeover *T* is stochastic and follows a normal distribution function $F(\mu, \upsilon^2)$. Suppose the raider, in order to successfully take over the firm, will have to pay the shareholder the market value without takeover: $E[y - w] = e - \beta_0^{'} - e\beta_1^{'}$, with the contract terms given. She will then take over the firm *iff* $1 - e(1 - \beta_1^{'}) + \beta_0^{'} > T$, and the probability of successful takeover p is given by $\Phi[\frac{1 - e(1 - \beta_1^{'}) + \beta_0^{'} - \mu}{\upsilon^2}]$. Taking the contract terms given, the manager can increase her effort level in order to reduce the takeover risk, up to the level where the marginal benefit of an effort increase $p'(e) * R$ equals the marginal cost due to extra effort $C'(e)$. Takeover threat thus improves performance incentives. This will leave less room for improvement. For the potential investors, the return as a non-monitoring owner is rather high and would not pay for the investors to monitor. When a takeover threat is not present, the agency problem persists. Shareholder monitoring can potentially improve incentive provision and add value. Therefore the incentive for institutional investors to acquire large stake and monitor increases.

The institutional changes toward stronger anti-takeover protection introduced an exogenous shock to costs for successful takeovers. If the change is treated as mean-shifting in the takeover cost distribution function, a higher μ implies that a smaller increase in effort is needed for the same reduction in takeover risk, and leaves more room for monitoring.

In addition to the two major parameters discussed here, there are other parameters in the model that can affect investor's decision to monitor. They include manager's risk averseness r_m and monitoring cost *M*. However, these variables are not easy to measure.

[8] It can be shown that optimal contract has the same pay-performance sensitivity when the manager has the bargaining power.

The model does not consider investor's wealth constraint, because it is not as important for institutional investors[9].

2.4 Empirical Framework and Results

From the above theoretical framework, I derive the decision rule by the investor to monitor and identify several factors that can move the potential benefits from monitoring. Since large shareholding is a prerequisite for credible monitoring, I now estimate how firm's likelihood of having a large institution varies with the variables identified in the previous section. The estimation problem is formulated as follows.

In any given period, we observe a binary variable whether there exists any large institutional investor in a firm. The outcome of the variable *y* depends on the net benefit of monitoring being non-negative: $y = 1(y^* \geq 0)$, where 1(.) is the indicator function. Net benefit y^* is a function of determinants, and define the function in a linear form $y^* = x'\beta + \delta$, $x = (\sigma_\varepsilon^2, (\sigma_\varepsilon^2)^2, r_o, ...)$. Under standard normal distribution assumption for δ, this breaks down to estimating a probit model $\Pr(y = 1 \mid x) = \Phi(x'\beta)$.

2.4.1 Measurement

From the theoretical framework, I have identified firm risk and the strength of takeover pressure as two major determinants for the monitoring incentives. These two factors affect the severity of agency problems and thus the room for improvements through shareholder monitoring. Here I discuss the measurement issues of these variables, as well as other firm characteristics that also influence the scope of agency problems.

The theoretical framework implies that the disturbance term in firm profit (σ_ε^2) has countervailing effects on the incentive to monitor. To measure the degree of uncertainty, only firm-specific shocks are relevant. This is because if the shock to firm profit is only due to some market-wide systematic risks that are common to all firms, relative firm

[9] The average size of institutions is 7 times the average size of firms in our sample.

performance will still be fully revealing of managerial input and incentive provisions are less of a concern. This in turn gives little room for shareholder monitoring. Firm specific risk (or idiosyncratic risk; the terms are used interchangeably) is calculated by taking the standard deviation of firm's residual stock return (the residual term of a CAPM regression of daily stock return on market return). The residual reflects firm return after separating the co-variation with the market, or the idiosyncratic component of daily stock return.

I have also shown that the incentive to monitor is greater when the takeover threat is missing: in the presence of takeover threats, the incentive for institutional investors to monitor is rather low; and when takeover pressure does not exist, shareholding monitoring can improve performance incentives and investors will find monitoring more rewarding. The strength of anti-takeover defenses can serve as a proxy for the lack of takeover threat.

Most U.S. states introduced anti-takeover legislations in the late 1980s following the hostile takeover wave. One focus of the legislations was to limit what bidders can do. For example, "business combination" statutes restrict the right of a successful bidder to merge with, break up, or liquidate the target firm, which particularly deters highly leveraged bids; "fair price" statutes raise the costs of takeovers, by requiring the bidder to pay all shareholders the highest price paid to any during a specific time period and thus ruling out two-tier offers[10]. The legislations also allowed management to adopt defensive tactics, such as poison pills. The arrangements in poison pills give the shareholders (other than the bidder) the right to purchase stock at a steep discount, making the target unattractive or diluting the acquirer's voting power. Among the other common provisions are supermajority amendments[11], special meeting limitations[12], classified board[13], and

[10] In which a higher price is offered for the first shares tendered and thus induces shareholders to tender.
[11] This amendment raises the majority rule above 50% -- typically from 66% to 80% -- for a merger to be approved.
[12] The limits restrict or eliminate shareholders' ability to call special meetings. Such provisions add extra time to proxy fights.
[13] This defense staggers the terms and elections of directors and can be used to slow down a hostile takeover. It postpones the time at which the raider can gain full control of the board after a takeover.

golden parachutes[14]. These amendments in general make it more costly to successfully complete a hostile tender offer. Their effect on actual takeovers and takeover attempts is studied for instance by Borokhovich et al (1997), which suggests that many protections were adopted at the beginning of a concerted effort to deter takeover. Their findings show these efforts are effective, as the firms that adopted anti-takeover defenses received significantly fewer takeover bids during the years following the adoption than the control firms. Regarding the effect of the anti-takeover defenses on managerial incentives, the studies in general support the view of takeover threats as disciplinary devices[15] (for example, Meulbroek et al (1990) reports evidence that R&D efforts decline following the adoption of anti-takeover amendments; research by Borokhovich et al (1997), Bertrand and Mullainathan (1999) and Bertrand and Mullainathan (2003) found that the anti-takeover defenses help managers receive above-market compensation levels and enjoy "quiet life"). The implication is therefore, when disciplinary takeover threats do not exist because of the institutional arrangements, shareholder monitoring becomes more important to improve incentive provision. Another possible venue for shareholder activism is to facilitate takeovers. This could happen when large shareholders help weaken anti-takeover defenses, also takeovers are easier to complete with more concentrated shareholdings.

To quantify the lack of takeover threats, I adopt the governance index developed by Gompers et al (2003). The index groups the 22 firm-level anti-takeover related amendments and 6 state anti-takeover laws intro five categories: tactics for delaying hostile bids (such as classified board and special meeting), voting rights (such as supermajority), director/officer protection (e.g., golden parachutes), state laws (such as business combination laws) and other takeover defenses. The index adds up the number

[14] These are severance payments (cash and non-cash) to senior executive upon a change in corporate control and do not require shareholder approval.

[15] We discussed earlier of the alternative view towards the ex ante efficiency of takeovers, that takeover may be a source of extra agency costs. The discussion is also related to the motivation of the anti-takeover defenses. In an efficient contracting view, some anti-takeover provisions can be justified as a means of re-establishing managerial incentives to pursue long term objectives. In an entrenchment view, the defenses are put in place by managers of firms with weak corporate governance structures and are against shareholders' interest: by making it more costly for shareholders to replace management through voting, it increases the ability of managers to engage in self-serving behavior at the expense of shareholders.

of anti-takeover provisions and laws for each firm, assuming different amendments or laws are essentially equivalent[16]. A higher index, as the authors suggest, reflects weaker discipline of the managers from the outside control market. As a result, un-disciplined managers are able to effectively seize control of the board to make favorable decisions at the cost of shareholders. They find positive correlation between the governance index and investment measures (capital expenditure and acquisition frequency), and interpret it as evidence for excessive agency cost associated with the anti-takeover protections.

I identify several firm level control variables that are related to the extent of agency problems. The moral hazard problem in the theoretical context can be exaggerated when there are a lot of investment opportunities around for the firm – with managerial shirking (or sometimes labeled by seeking "quiet life"), firms may forego some investment projects of positive present value; firms with more investment opportunities thus suffer more from managerial shirking than those with fewer opportunities. A few studies have shown the tendency of under-investing instead of the usual assumption that managers tend to over-invest ("empire-building"). For instance, Bertrand and Mullainathan (2003) studies the goals pursued by managers when they face weak discipline from the takeover market, and find that managers prefer a "quiet life" instead of building empires. Bohren et al (2007) also find that better corporate governance not only pushes a higher level of investment but also improves investment efficiency (that investment becomes more in line with investment opportunities and less sensitive to internal financial constraints). Aggarwal and Samwick (2006) show that the equilibrium relationships between compensation incentives, investment and firm performance are consistent with models of underinvestment in which managers have private costs of investment. However, "quiet life" may be less of a concern if there is abundant internal cash such that managers do not have to actively pursue external funding for the opportunities. A common measure for investment opportunities is Tobin's Q, which equals to the ratio of market value of assets

[16] The study by Jarrell and Poulsen (1987) documented that different types of amendments had various effect on the share price of the firms announcing amendments. Non fair price amendments on average had significant negative effect, while the fair price amendment had insignificant effect.

to the book value of assets[17]. Although free cash flow is not directly observable, presumably it is correlated with operating income.

An alternative form of agency problems related to investment is the free cash flow problem introduced by Jensen. In Jensen's free cash flow theory, managers derive private benefit from controlling more assets and tend to take wasteful unprofitable projects ("empire-building"). The free cash flow a firm possesses therefore opens up opportunities for discretionary spending and could imply higher agency cost. The theory also predicts that firms with more free cash but less investment opportunities tend to waste more of it; thereby cash-rich low-growth opportunities firms suffer even greater agency costs. A large investor has a potential role in reducing the agency problems related to wasteful spending by differentiating the quality of projects and discouraging managers from selecting bad ones (thus improving overall investment quality). On the other hand, the free cash flow problem can also be mitigated by debt and dividend, because the cash payout requirement can curb manager's ability of discretionary spending.

Larger firms may be subject to higher agency cost (Himmelberg et al (1999)). Meanwhile from the previous discussions, firm size may reflect more of risk aversion considerations[18], since holding large stakes in larger firms is more costly than owning the same percentage in smaller corporations. And I expect the risk consideration is more important than the monitoring consideration. Therefore firm size has a negative effect on the tendency to hold large stakes.

2.4.2 Data Description

I draw the sample from CompuStat, CDA/Spectrum, and IRRC datasets for the years 1992-2001. The CompuStat dataset covers a universe of around 2700 firms (including

[17] Alternatively, the ratio of sales to capital is used in other studies to avoid the potential mispricing problem of stocks associated with the market to book ratio of assets. Average sales growth in the previous years has also been used to proxy for future growth.

[18] In previous studies such as Demsetz and Lehn (1985), the consideration of firm size is motivated by controlling for investor's wealth constraint. In the context of institutional investors, wealth constraint concern is less important because of the size of the institutional investors (those in the sample are at least 7 times as large as the invested firms).

S&P 1500 firms) with detailed information of the balance sheet items. Stock return information is obtained from CRSP. Thomson Financial's CDA/Spectrum 13f dataset contains quarterly shareholding information for all 13f institutions [19] in over 4300 companies. From this data, I am able to identify large shareholdings by institutional investors in each sample firm. I extract annual holding information using reported holding of the quarter ending on June 31st, the quarter ending on December 31st, and the average holding among the 4 quarters.

Table 2.1 shows the overall trends in both total institutional ownership (in percentage term) and percent of firms with block institutional ownership, for a merged sample from the above mentioned sources with 12400 firm-year observations. Overall institutional holdings grew from 46% in 1993 to 60% in 2001 of firm's outstanding equity. As the overall holdings have grown, large holdings by institutional investors have also become more common from the beginning of the sample period toward the later years. In 1993, about one third of the firms did not have large institution with at least 5% block ownership. Then in 2001 only 22% of the firms did not have block institution[20].

The governance index is obtained from Investor Responsibilities Research Center covering S&P 1500 companies. To ensure that the same voting rights are attached to the stocks within the same firm, I exclude a small number of firms with two or more classes of common stock (with different voting schemes attached). For every other year, IRRC reports governance data including the takeover defense provisions of firms, state takeover laws, and the governance index based on Gompers et al (2003). For years not reported I use the most adjacent-year information. The index is calculated based on the number of 24 unique[21] firm-level provisions and state anti-takeover laws, with one point represents the existence of each provision/law.

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[19] 13f institutions include all types of investment companies that are required to file Form 13f with the SEC within 45 days after the last day of every quarter. Institutional investment managers with over $100 million in equity portfolio are required to file, and must report all holdings in excess of 10,000 shares and/or with a market value over $200,000.

[20] If we look at 10% block ownership, less than 30% have large institution.

[21] They consider firm anti-takeover amendment and state law with the same function as one unit.

The firm-specific idiosyncratic risk is calculated based on the residual standard deviation from the CAPM regression of daily stock returns on the market return. Since the market return has very small variation in the sample period, the derived firm specific risk measure is very similar to the annualized stock volatility measure from the CRSP data (standard deviation of stock return over the past 60 months). Investment opportunity or Tobin's Q is calculated as the market value of assets, which equals market value of equity (CompuStat item #199 * item #25) plus book value of liabilities (item #6 - item #60), over the book value of assets (item #6). I use the market value of equity as the measurement for firm size. Free cash flow is proxied by the ratio of earnings (item #18 + item #14) to assets. I also use the ratio of leverage, defined by total debt (item #9 + item #34) divided by asset, and the ratio of dividend (item #21) to total assets as controls for firm's capital structure.

After merging the different datasets for the period 1992-2001 and dropping the firm-year pairs without observations on the main variables (those identified in the last section for the comparative analysis), I get a final sample for regression analysis with nearly 6740 firm-year observations for some 1380 firms. Firms are missing half observations over the years due to two main reasons. First, since I am using lagged variables to predict the variation in institutional holding, the first year observations will be excluded. Secondly and as a more significant source of missing observations, IRRC does not have information on anti-takeover related measures for some of the firms. And since I interpolate the anti-takeover index variable only based on the adjacent report-year information, about 4000 firm-year observations are lost. On top of the two main sources for missing year observations, stock return information is not available in some years due to de-listing, and hence a small number of observations are missing on the firm idiosyncratic risk which is derived based on daily stock return data. I examine the potential selection problem by comparing the sample used in regression analysis and the full sample along main firm characteristics. In fact, the firms used in the regression are larger on average, with higher overall institutional ownership and lower investment opportunities. Later as robustness check I test the main qualitative results are not affected by the differences in the samples.

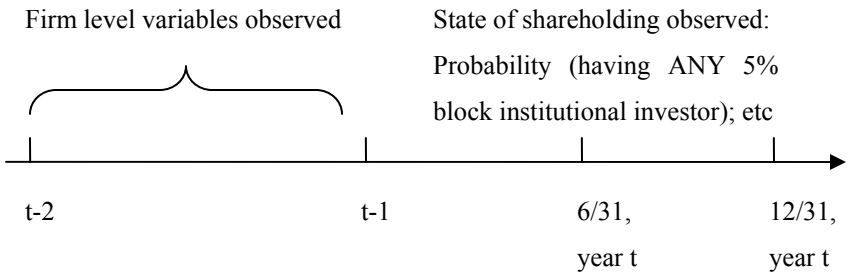
Focusing on the final sample, the summary statistics for the main firm characteristics is presented in Table 2.3. The median value of firm equity is about $1845 million and median total assets are valued at about $1872 million (in 1992 dollar). The governance index has a median value of 10, and the variable does not vary much over time (not shown in the table) during our sample period since most of the adoptions clustered at the end of 1980s. On average, institutions hold almost 60% of the outstanding shares for sample firms, and 76% of the firms have large institution. Table 2.4 provides the correlation coefficients between large institutional presence indicator and selected variables that serve as the measures for monitoring needs. The directions of correlations are largely consistent with our expectations. The indicator for large institutional presence is positively and significantly correlated to the level of firm idiosyncratic risk. It is correlated to the size of firm negatively and significantly. The correlations with anti-takeover measure and with cash flow are of expected signs, but are not significant.

2.4.3 Estimation and Results

I have motivated the establishment of large shareholding as related to monitoring, and identified the factors that encourages or discourages monitoring. The main testable relationships are: (1) large shareholdings become relatively more attractive with higher firm-specific risk, but becomes less so at higher level of risk due to increasing cost associated with institutions being further deviated from perfect diversification; large holding becomes less attractive at larger firms, as the cost due to risk bearing increases with firm size which also partly reflects the relative size of holdings to the institution's total wealth; (2) the tendency of large shareholding increases with the lack of takeover threat measured by the prevalence of anti-takeover protections; (3) controlling for risk and anti-takeover protections, large shareholding is more attractive at firms with more free cash flow; and the relationship is even stronger when firms with greater cash flow also face less investment opportunities. I thus examine the decision to acquire large shares by the institutional investors, based on their observation of the state of firms and evaluation of the benefits versus costs of monitoring. Empirically, the state of institutional shareholdings (whether there exists at least one block holder, where block is

defined as 5% of the outstanding equity) in year t is estimated as a function of the variables in year t-1. The focus in this study is the existence of ANY block holder, instead of the number of block holders.

The timeline of our empirical analysis is as follows:



The following probit regression is estimated:

[2.9]

$$\Pr ob(Y_{it} = 1) = \Phi[\beta_0 + \beta_1 Risk_{i,t-1} + \beta_2 (Risk_{i,t-1})^2 + \beta_3 AT_{i,t-1} + \beta_4 \log(MVEQ)_{i,t-1} + \beta_5 Cashflow_{i,t-1} + \beta_6 TobinQ_{i,t-1} + \beta_7 (Cashflow_{i,t-1} * TobinQ_{i,t-1}) + \beta_8 Capital_{it} + \gamma YD_{it}]$$

where the dependent variable is a dummy variable which equals one if there exists at least one large institution for firm i in year t. The *Risk* variable is firm idiosyncratic risk (for interpretation purpose I use the empirical cumulative distribution of the variable $\Phi(Risk)$ in the regression). $Risk^2$ term helps capture the countervailing effects of firm-specific risk on the incentive of large shareholding – at higher level of risk, the cost of large shareholding can offset the benefit of better monitoring. I therefore expect β_1 to be positive and β_2 negative. *AT* stands for the anti-takeover protections with an expected positive coefficient. *Log(MVEQ)* is the log of market value of equity and I expect its coefficient to be negative. The ratio of cash flow controls for the agency cost arising from discretionary spending, and its interaction with Tobin's Q allows for varying agency cost when cash-rich firms are faced with different levels of investment opportunities. I expect β_5 to be positive and a negative β_7, as monitoring is more desirable at firms with high cash flow and low investment opportunities. Variable *Capital* measures firm's capital structure such as debt and dividend payout, which is usually considered as a

solution to the free cash flow problem -- by keeping managers on a steady payout and thus reducing the cash flow available for discretionary spending, these instruments help limit the agency problem for instance associated with empire-building. I would expect shareholder monitoring has similar disciplinary effect as capital structure, if monitoring helps detect the ex ante quality of potential investment projects and prevents managers from pursuing bad deals. However, prior empirical evidence usually finds a positive correlation between ownership concentration and the level of debt. Year effects are captured by year dummies *YD* to control for the overall trend in large institutional holding over time.

Table 2.5 summarizes the results from estimating the probit regression in [2.9]. The coefficients shown reflect the estimated marginal effects on the probability of having any large institution. The estimates overall are consistent with the predictions.

Firms are more likely to have large institutional investors when firm-specific risk increases. The tendency to have large institutions grows slower with risk level. For instance, a rise in risk level from 25th percentile to the median is related to a 7 percentage-point increase (from 0.12 to 0.19) in likelihood; whereas an increase in risk from the median to the 75th percentile only correlates to a 2 percentage-point higher likelihood (from 0.19 to 0.21). Then at certain levels of uncertainty (around the 70th percentile of risk variable) the positive relationship reverses. This is consistent with the theory that cost of risk bearing offsets the benefit from close monitoring and deters monitoring, when risk is sufficiently high.

I've shown that the incentive to monitor rises with the lack of takeover threat. The results show as the anti-takeover protections measure grows larger, large institutional holding becomes more likely. The effect is quite small in magnitude though: one more anti-takeover related firm provision or state law is related to less than 1 percentage-point increase in the likelihood of having a large institutional investor. However, this effect may be under-stated due to endogeneity.

I have discussed the two possible effects of firm size on the incentive for large institutional holding: on one hand, large shareholding in a larger firm at any given risk level may imply more deviation from diversification; and on the other hand, agency issues could be graver in larger firms and retaining control makes more sense. The results show that larger firms are less likely to have large institutional investors – a one percent increase in market value of outstanding equity is related to 0.5 percentage-point decrease in the tendency of large institutional holding, at the significance level of 1%. This implies the cost from holding large stakes in a larger firm dominates the value arising from tighter control.

As discussed earlier, there are two possible sources of agency problems related to investment: "quiet life" and "empire building". Growth opportunities amplify the moral hazard problem due to managers seeking "quiet life", but the effect may diminish with the availability of internal cash. Therefore we would expect monitoring becomes more valuable, and large institutional investor is more likely to exist, with more growth opportunities. However, the agency problems arising from managerial slacking at firms with more investment opportunities could be alleviated with the internal cash available, as managers seeking quiet life may be reluctant to raise external funds to avoid the scrutiny and invest only when internal funds are available. The estimated coefficients of Tobin's Q ($\hat{\beta}_6$ = 0.01, with p-value < 0.05) and of the interaction term Tobin's Q * Cash Flow Ratio ($\hat{\beta}_7$ = -0.09, with p-value < 0.01) confirm the "quiet life" concern[22]. Under the "empire building" hypothesis, firms are more likely to engage in wasteful investment, especially in cash-rich firms with less investment opportunities. These firms are subject to higher agency cost and thus merit closer monitoring through large holding. The estimates from Table 2.5 are also consistent with the "empire building" hypothesis. Cash flow itself has an estimated effect of 0.56 (with p-value < 0.01) on the probability of large shareholding. This means an increase in cash flow ratio from the 25th percentile to the

[22] Note that the coefficient of interaction terms in probit model does not give cross-partial derivative of the probability with respect to the interacted variables. In some situations, it may not have the same sign as predicted by the linear model. As a specification check, I also run the regression in a linear probability form, and find coefficients of the quadratic and interaction terms are similar to those from the probit regression.

median translates to an increase of 2 percentage points (0.56*(0.09-0.05) = 0.02) in likelihood of having large institution. After allowing for interaction between cash flow and investment opportunities, the likelihood becomes more sensitive to cash flow at lower level of investment opportunities ($\hat{\beta}_7$ = -0.09, with p-value < 0.01).

Debt and dividend ratios are included as alternative discipline for empire building. By saying so, we would expect both variables are negatively related to the tendency of having large institutional holding. I find that higher leverage ratio is positively related to the likelihood of having large institutional holding ($\hat{\beta}_{LEV}$ = 0.08, with p-value < 0.01), and dividend negatively related ($\hat{\beta}_{DIV}$ = -0.7, with p-value =0.01)[23]. The relationship between debt and ownership is puzzling, yet in line with prior findings. However, explaining this empirical puzzle goes beyond the main thesis of the current study.

Overall the estimates for regression [2.9] are quite consistent with the predictions from the theory: institutional investors acquire large holdings where monitoring is valuable.

2.4.4 Over-Time Change in Ownership Structure

I also present time-series evidence of large institutional holding as determined by the firm's monitoring needs. Table 2.6 presents the estimated marginal effects on the probability of having additional large institutions over time from the Probit regression. The dependent variable is a dummy variable taking value one if the number of block institution holders increases over time, and equaling zero otherwise. I include the existence of large institution in the previous period as an additional proxy for the expected return of monitoring. The regression specification remains otherwise the same as in equation [2.9]. I observe existing large institutional investor has indeed deterred others to acquire large stakes, consistent with the hypothesis that the return to monitoring would be lower for incoming institutional investors. Meanwhile, the possibility to attract block institutional investor still responds to the measures for benefit of monitoring,

[23] The capital structure variables are endogenous and may contaminate the other estimates. Dropping them from the regression does not change results significantly.

controlling for existing institutional block ownership. However comparing to Table 2.5, the marginal effects of the variables (especially of *Risk*, *Anti-takeover*, *Market Value* and *Cash Flow*) are much smaller in the magnitude. A potential explanation is that other investors are expecting the incumbent large institution to do some monitoring[24], but not sufficient.

2.4.5 Robustness Checks

The explanatory variables, especially with the anti-takeover defense measure, may be subject to endogeneity concerns. With the anti-takeover variable, the provisions that form the construction of the measure were chosen by firms[25]. Lagged independent variables help mitigate the concern to some degree. Furthermore, the findings in prior studies suggest that the endogeneity problem with this variable, if any, should lead us to under-estimate the effects of takeover restrictions. In particular, Jarrell and Poulsen (1987), Brickley et al (1988) have found large institutional shareholders are more likely to vote against management-initiated anti-takeover amendments, especially those that hurt shareholder value (while insider holding has opposite effect on the adoption of the amendments). So even if we worry large institutional investors can select over anti-takeover protections (or over unobserved firm characteristics related to the protections), they will prefer firms with less protections and will use their power to keep such provisions from being adopted. Therefore reverse causation can not explain our finding that stronger anti-takeover protections are associated with a higher probability of having large institutional holder.

The firm-specific risk variable could be endogenous to the ownership structure as well. For example, large institutions cross-hold in firms and are less likely to sell their holdings, that tend to stabilize the stock prices at the cross-holding firms. However this

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[24] Evidence of monitoring is provided later in the paper.

[25] Alternatively we can rely only on the number of state laws to measure the strength of anti-takeover protections. However the cross-section variation will be very small, given that there are only 6 state-level laws and almost half of the firms incorporated in Delaware. Another possibility is to compare the holdings before and after the passage of state laws in Delaware, using other states as a control group.

will predict a negative relationship between large holding and idiosyncratic risk and implies our estimate is biased toward zero.

I test whether the main results are sensitive to alternative ways of measuring large institutional ownership. The qualitative relationships are robust to using a continuous measure of institutional ownership concentration. When the holding variable is constructed, only the holding information of the last quarter in the calendar year is used; however results are very similar if using reported holding information in the middle of the year (end of the 2nd quarter), or using averages among the four quarters.

I used interpolation to fill in the missing observations on the anti-takeover measure. In fact if I drop those years when the governance data was not reported by IRRC, regression results are similar. And since during sample selection a significant number of observations were lost because of the anti-takeover measure, I re-estimate the regression in [2.9] without this particular variable on the full sample and this does not cause large moves in the estimated coefficients of other variables.

2.5 Evidence of Control

I find that having large institutional holding is more likely where the benefit from monitoring is great relative to the forgone benefit from diversification, keeping other things constant. What, then happens, to the firm once the large institution is aboard? In the rest of the paper, I look for evidence of better monitoring at firms with large institutions. The purpose is to keep institution's large stake holding motives in check. It helps rule out alternative explanations for large institutional holding: for example, Barclay and Holderness (1989) has documented evidence for private benefits associated with block institutions holding (such as consuming corporate resources through their voting power, describing benefits that are not shared among other shareholders)[26], the magnitude of such private benefits could also be increasing with agency problems.

[26] They show substantial premiums that large blocks are traded to the post-announcement exchange price, and that the premiums tend to be larger with the block size and firm performance before the trade.

Monitoring effort is hard to observe directly. A common measure for monitoring used in the literature is firm performance or shareholder value, although in general no strong correlation has been found. And similar to many existing studies, we find block institutional holding has insignificant effect on firm accounting profit[27]. The submission of shareholder proposals is sometimes considered as more direct evidence for monitoring; however the incident could also indicate the failure of initial control efforts within the firm.

The focus here is more on investigating the possible mechanisms through which large institutions can potentially improve the firm's status quo. Institutions can involve actively and affect real decision making, especially firm's investment decisions. This is the case when institutions help investment becoming more aligned with the available opportunities. Institutions could also involve in a less active way, by facilitating value-increasing takeovers. On one hand, large holding makes takeover easier because of the holding concentration. On the other hand, large institutions have the capability of voting to remove some of the anti-takeover related provisions. Therefore I look at both over-time change in anti-takeover index and the actual incidence of being acquired at firms with large institutional investors.

2.5.1 Firm Investment Decisions

I have shown earlier that the likelihood of firms having large institutions responds to measures that capture the agency problems associated with investment choice (labeled by managers seeking "quiet life" or "empire building"). Here I examine whether large institution presence is associated with improved firm investment decisions, in particular, if manages invest more in line with available investment opportunities (as in Bohren et al (2007)). The following investment equation is estimated:

[2.10] $$Inv_{i,t} = a_0 + a_1 L_{i,t-1} + a_2 InvOpp_{i,t-1} + a_3 Cash_{i,t-1} + a_4 Debt_{i,t-1} + \nu_{i,t}.$$

[27] The result is not tabulated but available on request. Firm performance measure in time t is regressed on indicator for the existence of large institutional investor in time t-1. I measure profitability by the return on assets (earnings divided by the book value of assets). The control variables include lagged values of firm size, R&D expenses and advertise expenses relative to sales (as in McConnell and Servaes (1990)), industry and years effects.

The dependent variable is the capital expenditure ratio (relative to the capital stock), and investment opportunity is proxied by Tobin's Q as in regression [2.9]. The sensitivity of investment ratio to Tobin's Q is allowed to vary between firms with and without large institution. The regression controls for firm's financial constraints by including cash flow and leverage ratio. Lagged explanatory variables are used to reduce the effect of simultaneity.

Table 2.7 reports the estimation results for equation [2.10], including year and firm fixed effects. There are two main observations for firm with large institutional holders, compared to similar firms without large institution. Firstly, investment is lower on average in firms with large institutional holder. Secondly, investment responds more steeply (about 60% steeper from the fixed effect estimation) to the measure of investment opportunities. The coefficients on the control variables have the expected signs as measures for financial constraint. Cash flow is positively related to investment ratio; while leverage ratio has a negative coefficient, which supports debt as a disciplinary force on firm investment[28].

I further evaluate the above equation for corporate M&A activities specifically. M&A reflects an important strategic decision and can have a substantial influence on firm value. Many studies have shown that M&A can be motivated by managerial incentives and reduce shareholder wealth. To measure firm's acquisition behavior, SDC dataset provides specific information on domestic deals including the identity of acquirers and dates of announcement. Only completed transactions are included and after merging with the existing sample there are about 3350 cases of completed acquisitions for the period 1995-2001. Sample firms that did not appear in the SDC data are considered as those without acquisition activities within the year. I use the specification in equation [2.10], this time the dependent variable becomes the indicator for acquisitions. As shown in Table 2.8, the estimated relationships are very similar qualitatively to those for capital

[28] If we further allow investment-leverage sensitivity to vary across firms, the sensitivity is less negative at firms with large institution. This is consistent with large institution partly substitute for the disciplinary role of debt on investment.

expenditure: firms with large institutions are less likely to acquire other firms; moreover, the acquisition possibility is more aligned with the measure for investment opportunities. Combined with the observations for capital expenditure, the empirical relationships imply improved investment efficiency and less empire-building behavior at firms with large institutions.

Some might argue that the above finding could be a result of institutions being attracted to firms with better investment efficiency (and less empire building). This endogeneity concern is very common in the corporate governance literature. I argue that this should not create too much concern, since we have already established that large institution is more likely to arise when the firm is in fact more prone to empire building, rather than less.

2.5.2 Facilitating Takeovers?

Institutions could also involve in a less active way, by facilitating value-increasing takeovers. This can occur in several ways. The existence of large holding already makes takeover easier because higher holding concentration helps resolve the free-riding issue in the takeover process. Further, large institutions have the capability of voting to remove anti-takeover related provisions. Prior findings in Jarrell and Poulsen (1987), Brickley et al (1988) have shown large institutional shareholders are more likely to vote against management initiated anti-takeover amendments. Here I check if large institution presence help weaken firm's anti-takeover defense.

In order to test this, I look at the future over-time change in anti-takeover index at firms with large institutional investors. Table 2.9 shows the results from examining the changes in firm's total number of takeover-related provisions from year t-1 to t and from year t-1 to t+1. The regressions control for firm size, the current number of provisions and year effects (results are very similar if firm fixed effects are included). The coefficient on the indicator variable for large institution implies large institution tends to have downward pressure on the number of adopted provisions but the effect seems to be small.

I also examine the actual incidence of being acquired for firms with large institutional investors. The SDC dataset provides detailed deal information for M&A activities. I only count the completed transactions and there are about 809 cases of completed acquisitions (being acquired) among sample firms for the period 1995-2001. Sample firms that were not recorded as targets in the SDC database are treated as no announcement had ever been made toward them. A dummy variable takes on 1 if announcement was made during the year (and the deal is successful later on) and 0 otherwise. Control variables are firm characteristics that may affect the acquisition likelihood such as firm size, prior firm performance and the cost of takeover (anti-takeover measure). Table 2.10 summarizes the results from the probit estimation for successful acquisition likelihood within a year. The coefficient of large institutional presence is not economically or statistically significant. It is probably because the facilitating effect due to large institutional holding is offset by the effect of a more efficient management that tends to discourage takeover bids. Overall we can not draw a conclusion that large institutions actually facilitate takeovers. And it also makes the argument that institutions simply choose to hold large position where firms are more likely to be takeover targets less credible.

2.6 Concluding Remarks

This paper presents empirical evidence that large stakes by institutional investors are endogenously determined, and, in particular, that they arise to resolve the agency issues within firms. Using measures for the benefit of monitoring directly related to agency problems, and the cost of monitoring in terms of foregone benefit from diversification, I find the benefit and cost of monitoring explain the cross-firm variations in large institutional holdings. The results confirm earlier studies that ownership structure is endogenous and reflects the outcome of investor's optimization problem. I then investigate the venues through which large institutions improve the status quo. Large institutions are linked to less empire building behavior, as evident in both capital expenditure and acquisition activities. I do not find strong evidence for large institutions facilitating value-increasing takeovers, although they do have small negative effect on firm's takeover defense. Together with the result that institutions choose to acquire large

holdings in firms where their monitoring could be profitable, this study gives further support for institutional investors' active role in corporate governance.

It is possible that institutions with specific characteristics hold large stakes, and this raises the concerns of matching issues. These are interesting issues yet not important to this study, since the focus is on whether agency problems attract ANY institution to assume monitoring role and mitigate problems. Another interesting issue that is not addressed here is the existence of multiple large investors and the problem of monitoring provision by several large investors. In the current study, monitoring potential is used to explain the variation in ownership structure from zero to a positive number of large investors, not to explain the variation in terms of different number of large investors.

By documenting evidence that large institutional holding is related to monitoring potential, this study adds to the literature on investor activism. It also contributes to the literature on the determinants of US ownership structure, showing that monitoring potential could be one other important factor.

Table 2.1: Variations in Institutional Holding

	1993	2001
Mean % Institutional Ownership	46%	60%
% of firms with ANY block institutional investor	67%	78%

Note: This table shows the over-time change in the prevalence of institutional holding and of large institutional holding in the full sample (before excluding missing observations for the variables that are used in the regression analysis). Mean % institutional ownership refers to the average percentage ownership by institutional investors, and in the second variable block investor is defined as investor with at least 5% ownership stake.

Table 2.2: Variable Definitions

Variables	Definitions
Market Value	arket value of equity (in millions)
Total Assets	book value of assets (in millions)
Cash Flow Ratio	earnings/assets
Tobin's Q	market value of assets/book value of assets
Firm Idiosyncratic Risk	residual standard deviation from CAPM regression of daily stock return on market return
Leverage Ratio	debt/assets
Anti-takeover defense	governance index (Gompers et al 2003) that quantifies the number of anti-takeover amendments
Exist Large Institution	dummy variable equals one if there exists ANY institutional investor with ownership stake 5% or larger
Total Institutional Holding	percentage of ownership hold by institutional investors
Concentration of Institutional Holding	Herfindahl Index of all institutional holding

Table 2.3: Summary Statistics

Variables	Mean	S. D.	25th Percentile	50th Percentile	75th Percentile
Market Value	7783.266	25190.25	640.271	1845.189	5470.863
Total Assets	9637.682	34136.18	683.236	1872.445	6544.75
Cash Flow Ratio	0.089	0.126	0.051	0.091	0.136
Tobin's Q	1.997	1.625	1.158	1.480	2.178
Firm Idiosyncratic Risk	0.024	0.012	0.015	0.021	0.029
Leverage Ratio	0.244	0.175	0.112	0.237	0.351
Anti-takeover defense	9.603	2.709	8	10	12
Exist Large Institution	0.761	0.427	1	1	1
Total Institutional Holding	57.807	17.927	46.108	59.349	71.093
Concentration of Institutional Holding	209.177	297.297	89.643	165.323	263.571

Note: The statistics are based on a sample of firms extracted from Thomson Financial, CompuStat and IRRC over the years 1992-2001. The final sample for main regression analysis includes 1381 companies and 6737 firm-year total observations. All the financial variables are based on 1992 dollars.

Table 2.4: Correlations

	Exist Large Institution	Risk	Log (MV)	Anti-takeover
Idiosyncratic Risk	0.138*			
Log (Market Value)	-0.217*	-0.330*		
Anti-takeover	0.010	-0.213*	0.059*	
Cash Flow Ratio	0.006	-0.197*	0.181*	-0.002

Note: This table presents the coefficients of pair wise correlations among selected variables for the final sample, with * indicating the correlation coefficients significant at the 5% level or higher.

Table 2.5: Determinants for Having a Large Institutional Investor

	Dep Var =1 if there exists at least one block institutional investor; 0 o/w
Idiosyncratic Risk $_{t-1}$	0.497
	(0.072)***
(Idiosyncratic Risk $_{t-1}$)^2	-0.337
	(0.074)***
Anti-takeover Defense $_{t-1}$	0.008
	(0.002)***
Log (Market Value) $_{t-1}$	-0.050
	(0.004)***
Cash flow Ratio $_{t-1}$	0.561
	(0.096)***
Tobin's Q $_{t-1}$	0.011
	(0.005)**
Tobin's Q $_{t-1}$ * Cash flow Ratio $_{t-1}$	-0.094
	(0.027)***
Leverage Ratio $_{t-1}$	0.074
	(0.031)**
Dividend Ratio $_{t-1}$	-0.882
	(0.377)**

Note: This table lists the regression results from estimating the tendency of having any large institutional investor (with at least 5% holding), based on the Probit regression as specified in [2.9]. The dependent variable is a dummy taking value 1 when there is at least one large institution and 0 otherwise. The reported coefficients are the marginal effects of independent variables on the probability. All regressions include year effects. Robust standard errors are in parentheses, with *, **, *** indicating significance at 10%, 5%, and 1% levels, respectively.

Table 2.6: Change in Institutional Holding over Time

	Dep Var =1 if there are more block institutional investors between t-1 and t; 0 o/w
Exist Large Institution $_{t-1}$	-0.112
	(0.013)***
Idiosyncratic Risk $_{t-1}$	0.358
	(0.077)***
(Idiosyncratic Risk $_{t-1}$)^2	-0.209
	(0.077)***
Anti-takeover Defense $_{t-1}$	0.004
	(0.002)**
Log (Market Value) $_{t-1}$	-0.018
	(0.004)***
Cash flow Ratio $_{t-1}$	0.368
	(0.105)***
Tobin's Q $_{t-1}$	0.006
	(0.005)
Tobin's Q $_{t-1}$ * Cash flow Ratio $_{t-1}$	-0.073
	(0.032)**
Leverage Ratio $_{t-1}$	0.082
	(0.033)**
Dividend Ratio $_{t-1}$	-0.126
	(0.269)

Note: This table examines the change in block institutional shareholding over time. The dependent variable is a dummy taking value 1 when the firm is gaining additional large institutions between year t-1 and t, and 0 otherwise. The reported coefficients are the marginal effects of independent variables on the probability. All regressions include year effects. Robust standard errors are in parentheses with *, **, *** indicating significance at 10%, 5%, and 1% levels, respectively.

Table 2.7: Large Institutional Presence and Investment Behavior

	(1)	(2)	(3)	(4)
Tobin's Q $_{t-1}$	0.029	0.013	0.019	0.010
	(0.004)***	(0.002)***	(0.004)***	(0.002)***
Exist Large Institution $_{t-1}$	-0.005	-0.022	-0.051	-0.037
	(0.038)	(0.012)*	(0.042)	(0.014)***
L $_{t-1}$ * Tobin's Q $_{t-1}$			0.018	0.006
			(0.006)***	(0.003)*
Cash flow Ratio $_{t-1}$	0.016	0.200	0.013	0.192
	(0.038)	(0.068)***	(0.039)	(0.068)***
Leverage Ratio $_{t-1}$	-0.555	-0.425	-0.541	-0.422
	(0.029)***	(0.086)***	(0.027)***	(0.086)***
Firm Fixed Effects	No	Yes	No	Yes

Note: This table reports the regression results based on equation **[2.10**]. The dependent variable is the investment-capital ratio (gross capital expenditure divided by the stock of net property, plant and equipment). The regression includes year effects. Robust standard errors are in parentheses with *, **, *** indicating significance at 10%, 5%, and 1% levels, respectively.

Table 2.8: Large Institutional Presence and Acquisition Possibility

	(1)	(2)	(3)	(4)
Tobin's Q $_{t-1}$	0.010	0.008	0.005	0.005
	(0.001)***	(0.002)***	(0.001)***	(0.002)***
Exist Large Institution $_{t-1}$	-0.016	0.008	-0.040	-0.011
	(0.009)*	(0.010)	(0.011)***	(0.012)
L $_{t-1}$ * Tobin's Q $_{t-1}$			0.010	0.008
			(0.002)***	(0.003)***
Cash flow Ratio $_{t-1}$	0.091	0.195	0.085	0.181
	(0.028)***	(0.038)***	(0.028)***	(0.038)***
Leverage Ratio $_{t-1}$	-0.020	-0.332	-0.010	-0.327
	(0.023)	(0.047)***	(0.023)	(0.047)***
Firm Fixed Effects	No	Yes	No	Yes

Note: This table reports the regression results from linear regression models of the probability of acquiring other firms. The acquisition data is from SDC database. Only completed deals are included. I treat those firms in the sample that did not appear in SDC as the ones without acquisition activities in a year. The dependent variable equals one if there is acquisition within a year, and zero otherwise. The regression includes year effects. Robust standard errors are in parentheses with *, **, *** indicating significance at 10%, 5%, and 1% levels, respectively.

Table 2.9: Large Institutional Presence and Anti-takeover Provisions

	Δ Anti-takeover $_{t-1, t}$	Δ Anti-takeover $_{t-1, t+1}$
Exist Large Institution $_{t-1}$	-0.046	-0.051
	(0.015)***	(0.024)**
Anti-takeover Defense $_{t-1}$	-0.039	-0.080
	(0.002)***	(0.004)***
Log (Market Value) $_{t-1}$	-0.022	-0.047
	(0.004)***	(0.007)***

Note: This table shows the effect of large institution on the over-time changes in the number of the anti-takeover provisions. Lagged firm market value and year effects are also included. Robust standard errors are in parenthesis, with *, **, *** indicating significance at 10%, 5%, and 1% levels, respectively.

Table 2.10: Large Institutional Presence and Takeover Possibility

	Dep Var =1 if being target; 0 if not
Exist Large Institution $_{t-1}$	0.008
	(0.007)
Log (Market Value) $_{t-1}$	-0.001
	(0.002)
Stock Return $_{t-1}$	-0.019
	(0.007)**
Anti-takeover Defense $_{t-1}$	-0.001
	(0.001)

Note: This table reports the effect of large institution on the actual probability of being acquired. The dependent variable is a dummy variable which equals one if the firm receives announcement during the year (and the deal is successful later). The reported coefficients are the marginal effects of independent variables on the probability. The information on acquisitions is from SDC database. Only completed deals are included. We treat those firms in our sample that are not occurring in SDC as the ones without acquisition activities in a year. The coefficients are the marginal effects of independent variables on the probability of being acquired. Year effects are included. Robust standard errors are in parenthesis, with *, **, *** indicating significance at 10%, 5%, and 1% levels, respectively.

Chapter 3

The Role of Peer Performance in Managerial Compensation Surrounding the 1996 Telecommunications Act [29]

3.1 Introduction

Relative performance evaluation (RPE) receives considerable attention in the accounting, finance, and economics literature (e.g., Antle and Smith (1986); Janakiraman et al (1992); Gibbons and Murphy (1990); DeFond and Park (1999)). Prior studies identify product market competition as a determinant of RPE, but these studies focus on cross-sectional variations in competition (e.g., industry concentration), and hence, provide little evidence on whether and how firms change RPE as the competitive environment changes (e.g.,Nalebuff and Stiglitz (1983); Aggarwal and Samwick (1999); DeFond and Park (1999); Joh (1999)). We extend the literature by examining the dynamics of RPE surrounding the U.S. 1996 Telecommunications Act.

As a milestone in the evolution of the telecommunications industry, the Act undoubtedly changed the competition environment of the industry, although the direction of the change is still at debate (e.g.,Hazlett (2000); Sidak (2003)). To examine the impact of the Act on RPE, we compare the changes in the role of peer performance for telecommunications firms against the corresponding changes for size- and performance-matched benchmark firms from the manufacturing sector. Our final sample consists of 45 pairs of telecommunications firms and their benchmarks, both identified from the EXECUCOMP dataset over 1992-2001. The results indicate that the association between

[29] This chapter is based on a joint work with Shijun Cheng, now published in the Journal of Accounting and Public Policy 2006.

peer stock performance and executive compensation is insignificant before the Act and negatively significant after the Act. These changes in the role of peer performance are significant standing alone, or relative to the benchmark manufacturing firms. The weight on peer accounting performance also declined significantly post-Act, but the change is statistically insignificant relative to the corresponding changes of the benchmark firms. Overall, these results show that RPE became more valuable for telecommunications firms in the post-Act period.

3.2 Background and Hypothesis

3.2.1 The 1996 Telecommunications Act

Historically the U.S. communication policy evolved to keep communication services separated, but technology developments in the mid 1990s facilitated convergence in different modes of communications and made regulatory separation between content and conduit increasingly difficult. The 1996 Telecommunications Act signed by President Clinton on February 8, 1996, aims at an opening of market to competitors and a reduction of market power through facilitating communication companies to enter each other's market, especially in the monopolized local exchange markets. Under the Act, local telephone companies, long-distance carriers and cable television operators can enter each other's markets. Local exchange carriers can offer video programming services themselves or carry other video programming services. The Act also allows the seven regional Bell operating companies to offer long-distance telephone service for the first time since the 1984 breakup of AT&T, under the condition that there is "sufficient competition" in the local markets. At the same time, long distance companies and cable operators are allowed to provide local exchange service in direct competition with the regional Bell operating companies. The Act thus fosters cross-penetration in the telecommunications industry. Proponents of the Act expected such cross-penetration to increase competition in the industry, whereas others argue that the Act is an abysmal failure due to unintended responses from the telecommunications companies, such as horizontal mega-mergers in the industry (Hazlett (2000); Sidak (2003)).

3.2.2 The Hypothesis

As suggested by Holmstrom (1982), shareholders may use peer performance as a benchmark to filter out effects of common market or industry shocks, and therefore be able to evaluate managerial performance more accurately. As such, RPE predicts a negative association between peer performance and managerial compensation, and a positive association between own performance and managerial compensation. While enhancing risk-sharing between shareholders and managers, RPE also has incentive effects. For example, RPE may provide managers with incentives to compete against peer firms because their compensation decreases with peer performance (Aggarwal and Samwick (1999); Joh (1999)), making it harder for the group being evaluated to enforce a collusive agreement. On the other hand, RPE may also provide an incentive to collude by either making sure no one sticks out too far, or letting there be a big winner and splitting the gains, perhaps by taking turns in being the big winner.

As the competition environment of the telecommunications industry changes after the Act, it is very likely that when determining RPE, shareholders change the relative weight placed on their considerations of risk-sharing and encouraging/discouraging collusive behavior in the product market. However, absent a theory predicting such relative weight, as well as a consensus regarding the impact of the Act on the competition level of the industry, *ex ante*, we do not have a clear prediction for how RPE changes after the Act. We therefore state the hypothesis in null form:

> **Hypothesis:** The association between peer performance and managerial compensation remains the same in the post-Act period as in the pre-Act period.

3.3 Sample and Data

3.3.1 Sample and Data

We start our sample selection with the 68 firms in the telecommunications industry (SIC code 48) covered by the EXECUCOMP dataset over the period 1992-2001. This sample period not only witnessed the U.S. Telecommunications Act of 1996, but also includes several years before and after the passage of the Act, and thus, is suitable for studying the

impact of the Act. To control for secular trends and other confounding factors, for each telecommunications company we identify a benchmark firm from the manufacturing industry (Standard Industrial Classification codes 20-39). We select benchmark firms based on data of 1995, the year before the Act. 46 of the telecommunications firms have data on both firm size (book value of assets) and performance (return on common equity). For each of these 46 telecommunication firms, we select a manufacturing firm whose book value of asset is between 70% and 130% of the book value of assets of the telecommunications firm and also with closest magnitude of return on equity.[30] One of the 46 matched pairs do not have other data required for our main regressions discussed in Section 4, and our final sample consists of 45 telecommunications firms and 45 benchmark firms. Our data on executive compensation and ownership are from the EXECUCOMP. Data on stock and financial items are from CRSP and COMPUSATA respectively.

3.3.2 Measurement

Following prior studies (e.g.,Aggarwal and Samwick (1999); Garvey and Milbourn (2004); Hartzell and Starks (2003); Joh (1999)), we focus on executive annual total pay. Specifically, executive cash pay (*C_PAY*) is the sum of the executive's salary and annual bonus. Executive total pay (*T_PAY*) is the sum of the executive's cash pay, cash payouts from long-term incentive plans, value of stock and option grants (based on the Black and Scholes (1973) model adjusted for dividends), and all other annual pay. Following Aggarwal and Samwick (1999), among others, we consider top executives as a team and include all executives with valid compensation data from the EXECUCOMP dataset in our analyses, but allow CEOs to have different compensation levels and sensitivities to own and peer performance.

We use both accounting and stock performance measures as compensation determinants (Murphy (1999)). We measure accounting performance as accounting return on common equity (*ROE*), defined as income before extraordinary items (COMPUSTAT item #18)

[30] The matching method is a common technique and is used in Barber and Lyon (1996), Kole and Lehn (1999), and Parrino et al (2003).

divided by the average book value of common equity (COMPUSTAT item # 60). Our stock performance measure is the firm's annual stock returns (*RET*). For each firm, we define its peer group as all other firms in the same four-digit SIC code industry. Accordingly, following Gibbons and Murphy (1990) and Aggarwal and Samwick (1999) we measure peer stock (accounting) performance as the value-weighted average of *RET* (*ROE*) for all other firms in the same industry, i.e.,

$$\sum_{j \neq i} RET_{j,t} V_{j,t-1} / \sum_{j \neq i} V_{j,t-1} \; (\sum_{j \neq i} ROE_{j,t} V_{j,t-1} / \sum_{j \neq i} V_{j,t-1}),$$

where *j* indexes firms and *t* indexes fiscal years, and *V* is the market value of equity (book value of common equity), and we denote peer stock (accounting) performance by *Ind_RET* (*Ind_ROE*).

We include a number of variables to control for factors that prior studies identify as having effects on executive compensation (Murphy (1999)). We use *ASSET*, book value of assets (COMPUSTAT item #6), to control for firm size. We use market-to-book value of assets (*MTB*) to control for growth opportunities of the firm, where *MTB* is calculated as market value of equity (COMPUSTAT item #25 ×item #199) plus book value of liabilities (COMPUSTAT item #6 – item #60), divided by *ASSET*. We include annualized stock volatility (*VOLAT)*, measured as standard deviation of stock returns over the past 60 months, to control for firm risk. We use financial leverage (*LEV*), defined as total debt (COMPUSTAT item #9 + item #34) divided by *ASSET*, to control for capital structure.

3.3.3 Analyses and Results

Table 0.2 presents summary statistics of the 45 telecommunications versus 45 benchmark firms based on data of 1995. The results largely suggest that in the year before the passage of the Act, the telecommunications firms and their benchmarks are not different from each other along most important dimensions.

Following prior studies, we regress managerial compensation on own firm performance and peer performance, controlling for other factors (e.g., Gibbons and Murphy (1990); Aggarwal and Samwick (1999); Joh (1999)). We use a difference-in-differences design

and compare the changes in the role of peer performance in executive pay for telecommunications firms against the corresponding changes for the size- and performance-matched benchmark firms. This design helps mitigate the effects of factors other than the Act affecting the role of peer performance in all firms. We specify the model below.

[0.1]

$$
\begin{aligned}
COMP_{i,j,t} = {} & \beta_0 + \beta_1 RET_{j,t} + \beta_2 Ind\_RET_{j,t} + \beta_3 D_t \times RET_{j,t} + \beta_4 D_t \times Ind\_RET_{j,t} \\
& + \beta_5 T_j \times RET_{j,t} + \beta_6 T_j \times Ind\_RET_{j,t} + \beta_7 D_t \times T_j \times RET_{j,t} + \beta_8 D_t \times T_j \times Ind\_RET_{j,t} \\
& + \beta_9 ROE_{j,t} + \beta_{10} Ind\_ROE_{j,t} + \beta_{11} D_t \times ROE_{j,t} + \beta_{12} D_t \times Ind\_ROE_{j,t} \\
& + \beta_{13} T_j \times ROE_{j,t} + \beta_{14} T_j \times Ind\_ROE_{j,t} + \beta_{15} D_t \times T_j \times ROE_{j,t} + \beta_{16} D_t \times T_j \times Ind\_ROE_{j,t} \\
& + \beta_{17} CEO_{i,j,t} + \beta_{18} CEO_{i,j,t} \times RET_{j,t} + \beta_{19} CEO_{i,j,t} \times Ind\_RET_{j,t} \\
& + \beta_{20} CEO_{i,j,t} \times ROE_{j,t} + \beta_{21} CEO_{i,j,t} \times Ind\_ROE_{j,t} + CONTROLS + Error\ Term_{i,j,t}.
\end{aligned}
$$

Similar to Aggarwal and Samwick (1999), executive i works at firm j in year t. $COMP_{i,j,t}$ is the compensation of executive i at firm j in year t, defined as the natural log of *C_PAY* or *T_PAY*. T_j is equal to 1 if the firm is a telecommunications firm and 0 otherwise, and D_t is an indicator equal to 1 for the post-Act period (1997-2001), and 0 for the pre-Act period (1992-1995).[31] $CEO_{i,j,t}$ is an indicator equal to 1 if executive i is the CEO of firm j in year t, and 0 otherwise. *CONTROLS* denotes the set of control variables, including *ASSET*, *MTB*, *VOLAT*, *LEV*, and firm and year indicators. Because the model controls for firm fixed effects, it is effectively similar to changes models in prior studies (e.g., Gibbons and Murphy (1990)). The model also accounts for the possibility that CEOs have different pay levels and pay-performance sensitivities.

The coefficient on $D_t \times T_j \times Ind\_RET_{j,t}$ (β_8) captures the change in the role of peer stock performance for telecommunications firms, relative to the benchmark firms. Similarly, the coefficient on the coefficient on $D_t \times T_j \times Ind\_ROE_{j,t}$ (β_{16}) captures the change in the role of peer accounting performance for telecommunications firms, relative to the benchmark firms. To illustrate, the combined coefficient on peer performance $Ind\_RET_{j,t}$ for the benchmark firms is β_2 pre-Act and $\beta_2 + \beta_4$ post-Act, and the change following the

[31] Our results remain unchanged if we extend D_t by defining D_t as 1 or 0 for 1996.

Act is β_4. For the telecommunications firms, the combined coefficient on peer performance $Ind\_RET_{j,t}$ is $\beta_2 + \beta_6$ pre-Act and $\beta_2 + \beta_6 + \beta_4 + \beta_8$ post-Act, with $\beta_4 + \beta_8$ as the change in the combined coefficient on $Ind\_RET_{j,t}$ following the Act. Thus, relative to the benchmark firms the change in the combined coefficient on $Ind\_RET_{j,t}$ for telecommunications firms following the Act is β_8.

Table 0.3 reports the estimates of Equation [0.1] and Table 0.4 summarizes the results to facilitate the interpretations of the results. While the results for executive cash pay and total pay are basically similar, our interpretation is based on the results for executive total pay, because it is a better proxy for executive compensation. The results in Table 0.3 indicate that the coefficient on $D_t \times T_j \times Ind\_RET_{j,t}$ (β_8) is negatively significant for executive total pay (-0.87, $p < 0.01$). Table 0.4 demonstrates this coefficient as post- versus pre-Act change in the weight on *Ind_RET* for telecommunications firms minus the corresponding change for the benchmark firms. In fact, for the telecommunications firms the weight on *Ind_RET* in executive total pay is negative but insignificant pre-Act (-0.175), and negatively significant post-Act (-0.753, $p < 0.01$), with a significant change of -0.578 ($p < 0.01$). The corresponding change for the benchmark firms is 0.295 (insignificant). Thus, the difference between the changes for the telecommunications and benchmark firms is -0.753 - 0.295 = -0.873 ($p < 0.01$), which is the coefficient on $D_t \times T_j \times Ind\_RET_{j,t}$ (β_8) shown in Table 0.3. The results imply that holding others constant, if peer performance (*Ind_RET*) increases by one standard deviation of 0.58, executive total pay (*T_PAY*) will decrease by 0.175 × 0.58 = 10% pre-Act, and by 0.753 × 0.58 = 44% post-Act.

In short, Table 0.4 shows that for executive total pay the weight on peer stock performance is insignificant pre-Act and negatively significant post-Act for the telecommunications firms, and this change is negatively significant relative to the corresponding change for the benchmark firms. The change in the weight on peer accounting performance is also negatively significant for the telecommunications firms, but relative to the benchmark firms, it is insignificant. Together, these results show that the telecommunications firms (relative to the benchmark firms) significantly reduced the

weight on peer performance in compensating executives. In other words, RPE became more valuable after the Act.[32]

There are alternative interpretations of the findings. One interpretation is that in the spirit of Nalebuff and Stiglitz (1983), the Act changed competition in such a way that peer performance is more informative about managerial performance post-Act. Another interpretation is that after the Act, shareholders strengthened RPE in managerial compensation to discourage collusion with competitors in the product market (Aggarwal and Samwick (1999); Joh (1999)). However, this interpretation is subject to the following possibility discussed earlier: managers may have incentives to cooperate so that no one will stick out too far. In this exploratory study, we do not distinguish between these alternative interpretations.[33]

Nevertheless, our findings are not inconsistent with the criticisms that the Act only led to modest improvement in competitiveness or even turned out to be an abysmal failure due to unintended responses from the telecommunications companies (Hazlett (2000); Sidak (2003)). One widely recognized example of such response is the horizontal mega-mergers in the post-Act period aimed at reducing competition pressure created by the Act (e.g., Warf (2003)). The motivations of these mergers are not necessarily anti-competitive, and in fact, our results from unreported additional analyses show that horizontal mergers actually hurt peer performance.

Furthermore, our main results reported in Table 0.3 and Table 0.4 are robust to a number of sensitivity checks. Our results are very similar when we include CEOs only in our analyses, or when we calculate peer performance based on two-digit or three-digit SIC code industries. The results are also insensitive to our choice of benchmark firms; the

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[32] In additional analyses, the results indicate that the role of peer performance in indirect compensation executives derive from changes in the value of their existing equity portfolio, another important component of executive incentives, did not change significantly post-Act. This sensitivity check mitigates the concern that our results are driven by changes in the role of peer performance in executive equity holdings.

[33] Compensation contracts can possibly affect competition strategies, but they are not the only factor with such an effect. There could be other agreements between the firms or between shareholders and managers that are unobservable to us. Therefore we need to be cautious to infer the firms' competition strategies from the compensation contracts examined.

results remain unchanged when we use all the firms from the manufacturing sector or when we use all non-telecommunications firms covered in the EXECUCOMP dataset over 1992-2001. We also use stock performance only or accounting performance only when estimating our empirical model, and for each performance measure we find results similar to those reported in Table 0.3. Finally, the results do not change when we use executive pay levels as our dependent variables, whether we use all observations, eliminate outliers, or use median regressions (which are less sensitive to outliers).

3.4 Conclusion

We use the introduction of the U.S. 1996 Telecommunications Act as our empirical setting to examine the dynamics of the role of peer performance in managerial compensation. The results from 45 pairs of telecommunications and size- and performance-matched benchmark firms indicate that the weight on peer performance declined post-Act, suggesting that RPE became more valuable for the telecommunication firms following the Act. Besides shedding some light on the impact of the Act, our results show how managerial incentives in general, and the role of peer performance in particular, changed with the firm's competition environment. Our empirical setting is relatively exogenous, and hence, helps mitigate concerns with omitted variables and endogeneity. Our focus on managerial incentives complements prior studies about the telecommunication sector reforms examining factors such as product prices and industry concentration (e.g., Parker and Roller (1997); Warf (2003)), because these factors and managerial incentives are likely to interact with each other.

Table 0.1: Variable Definitions

Variables	Definitions
C_PAY	Executive cash pay (in thousands), including salary and annual bonuses
T_PAY	Executive total annual pay (in thousands), including cash pay, value of equity grants during the year, fringe benefits, and cash payouts from other long-term incentive plans
RET	annual stock return
Ind_RET	value-weighted average of annual stock returns of all other firms in the same four-digit SIC code industry
ROE	income before extraordinary items (COMPUSTAT item #18) divided by average book value of common equity (COMPUSATA item #60)
Ind_ROE	value-weighted average of ROE of all other firms in the same four-digit SIC code industry
ASSET	book value of assets (in millions, COMPUSTAT item #6)
MTB	market to book value of assets, defined as COMPUSTAT [(item #25 × item 199) + (item #6 – item #60)]/ item #6
VOLAT	annualized stock volatility, measured as the standard deviation of stock returns over 60 months
LEV	total debts divided by total assets, defined as COMPUSTAT (item #9 + item #34)/ item #6

Table 0.2: Summary Statistics of the Telecommunications versus Benchmark Firms

	Mean of Telecommunications Firms of 1995	Mean of Benchmark Firms of 1995	P-value of equal means
C_PAY	1,130.75	973.08	0.21
T_PAY	2,507.31	2,541.44	0.93
RET	0.52	0.32	0.10
Ind_RET	0.30	0.27	0.89
ROE	0.07	0.10	0.47
Ind_ROE	0.08	0.10	0.89
ASSET	11,356.66	8,307.59	0.34
MTB	2.07	1.94	0.58
VOLAT	0.28	0.32	0.30
LEV	0.66	0.57	0.04

Note: The statistics presented in this table are based on a sample of 45 telecommunications firms (with two-digit SIC code 48) and 45 size- and performance- matched (matched using data in 1995) manufacturing firms (with two-digit SIC codes 20-39) identified from the EXECUCOMP dataset over 1992-2001 excluding 1996, the year the Telecommunication Act was enacted.

Table 0.3: Regressions of Executive Annual Pay on Own and Peer Performance

Dependent Variable	$Ln(C\_PAY_{i,j,t})$	$Ln(T\_PAY_{i,j,t})$
Independent Variable	(1)	(2)
$RET_{j,t}$	0.692	0.395
	(0.000)***	(0.025)**
$Ind\_RET_{j,t}$	-0.765	-0.298
	(0.000)***	(0.128)
$D_t \times RET_{j,t}$	-0.651	-0.248
	(0.000)***	(0.161)
$D_t \times Ind\_RET_{j,t}$	0.880	0.295
	(0.000)***	(0.188)
$T_j \times RET_{j,t}$	-0.556	-0.089
	(0.000)***	(0.652)
$T_j \times Ind\_RET_{j,t}$	0.733	0.123
	(0.000)***	(0.550)
$D_t \times T_j \times RET_{j,t}$	0.596	0.174
	(0.000)***	(0.327)
$D_t \times T_j \times Ind\_RET_{j,t}$	**-0.932**	**-0.873**
	(0.000)* **	**(0.000)*** **
$ROE_{j,t}$	1.421	1.036
	(0.019)**	(0.010)**
$Ind\_ROE_{j,t}$	-0.952	-1.347
	(0.109)	(0.021)**
$D_t \times ROE_{j,t}$	-1.239	-0.933
	(0.049)**	(0.187)
$D_t \times Ind\_ROE_{j,t}$	1.024	1.079
	(0.111)	(0.234)
$T_j \times ROE_{j,t}$	-1.184	-1.803
	(0.063)*	(0.000)***
$T_j \times Ind\_ROE_{j,t}$	0.085	2.386
	(0.888)	(0.007)***
$D_t \times T_j \times ROE_{j,t}$	1.047	1.971
	(0.102)	(0.005)***
$D_t \times T_j \times Ind\_ROE_{j,t}$	**-0.739**	**-1.912**
	(0.304)	**(0.115)**
Control Variable		
$CEO_{I,j,t}$	0.744	0.857
	(0.000)***	(0.000)***
$CEO_{I,j,t} \times RET_{j,t}$	0.014	-0.079
	(0.558)	(0.128)
$CEO_{I,j,t} \times Ind\_RET_{j,t}$	-0.049	0.078
	(0.220)	(0.215)
$CEO_{I,j,t} \times ROE_{j,t}$	0.176	0.139
	(0.005)***	(0.200)
$CEO_{I,j,t} \times Ind\_ROE_{j,t}$	0.024	-0.046
	(0.912)	(0.862)
$Ln(ASSET)_{j,t-1}$	0.178	0.303
	(0.001)***	(0.000)***
$MTB_{j,t-1}$	0.065	0.117
	(0.000)***	(0.001)***
$VOLAT_{j,t-1}$	-0.236	-0.456
	(0.323)	(0.097)*
$LEV_{j,t-1}$	-0.506	-0.871
	(0.119)	(0.007)***

$D_t \times T_j$	0.234	0.564
	(0.009)***	(0.001)***
CONSTANT	4.931	4.478
	(0.000)***	(0.000)***
Firm Fixed Effects	Included	Included
Year Fixed Effects	Included	Included
Sample Size	1687	1687
Adjusted R^2	0.758	0.750

Note: This table presents the regression results of executive annual pay on own and peer performance surrounding the 1996 Telecommunications Act, using manufacturing firms as benchmarks. The regression is based on equation [0.1], where i indexes for executive, j for firm and t for year. P-values (in parentheses) are based on standard errors corrected for heteroskedasticity and auto-correlations with *, **, *** indicating significance at 10%, 5%, and 1% levels, respectively.

Table 0.4: Comparing Combined Coefficients on Own and Peer Performance

	Telecommunications Firms			Benchmark Firms			Difference for Telecommunications firms minus difference for benchmark firms
	1992-1995	1997-2001	Difference: 1997-2001 vs. 1992-1995	1992-1995	1997-2001	Difference: 1997-2001 vs. 1992-1995	
Dependent Variable is Ln(C_PAY):							
Combined Coefficient on RET	0.136***	0.081***	-0.055***	0.692***	0.041**	-0.651***	0.596***
Combined Coefficient on Ind_RET	-0.032	-0.084**	-0.052	-0.765***	0.115***	0.88***	**-0.932*****
Combined Coefficient on ROE	0.237***	0.045	-0.192***	1.421**	0.182*	-1.239**	1.047
Combined Coefficient on Ind_ROE	-0.867**	-0.582**	0.285	-0.952	0.072	1.024	**-0.739**
Dependent Variable is Ln(T_PAY):							
Combined Coefficient on RET	0.306***	0.232***	-0.074***	0.395**	0.147**	-0.248	0.174
Combined Coefficient on Ind_RET	-0.175	-0.753***	-0.578***	-0.298	-0.003	0.295	**-0.873*****
Combined Coefficient on ROE	-0.767***	0.271***	1.038***	1.036**	0.103	-0.933	1.971***
Combined Coefficient on Ind_ROE	1.039**	0.206	-0.833*	-1.347**	-0.268	1.079	**-1.912**

Note: The combined coefficients are based on the results in Table 0.3. P-values (in parentheses) are based on standard errors corrected for heteroskedasticity and auto-correlations with *, **, *** indicating significance at 10%, 5%, and 1% levels, respectively.

Chapter 4

The Flypaper Effect: Evidence from the Tobacco Settlement Payment to States [34]

4.1 Introduction

Late 1990s witnessed the great success in the U.S. public health history of attacking enormous problems posed by tobacco use. On November 23, 1998, 46 states and other U.S. territories signed the Master Settlement Agreement (MSA) with the five largest U.S. tobacco manufacturers, joining four states (Mississippi, Texas, Florida and Minnesota) that had reached earlier, individual settlements. This multi-state settlement ended four years of litigation against the tobacco industry by the states seeking for recovery of medical costs incurred in the past when treating smoking-related diseases. The settlement commits tobacco manufacturers to make annual payments to the states in perpetuity, with total payments over the first 25 years estimated at several hundred billion dollars. The scope of the settlement and the magnitude of the payments involved provide a natural experiment to study the flypaper effect.

The flypaper effect is an empirical puzzle at odds with the theoretical prediction regarding the response of public spending to intergovernmental grants or windfalls. A simple median-voter model in which the private income and grants or windfalls are assumed to be fully fungible predicts that government's proclivity to spend on public goods should not depend on the source of income. In other words, marginal propensity of public spending out of the private income should be equal to that out of fiscal grants or

[34] This chapter is based on a joint work with Xin Li.

windfalls. Nevertheless, numerous empirical studies have generally found that fiscal grants or windfall money boosts government spending more than an equivalent increase in private income. The puzzle is referred as the flypaper effect since money "sticks where it hits" (Gramlich et al (1973)).

Fisher (1982) provides an excellent survey on the earlier literature. Wyckoff (1991) and Hines, James R. Jr. and Thaler (1995) review more recent studies. Table 0.1 presents the summary results from commonly cited studies since the 1990s. In these studies, higher marginal propensity to spend from grants than from private income is found as evidence for the flypaper effect. The estimated income effect from private source is in the ballpark of 0.01-0.05. The estimated effect of grants income on public spending varies from 0.25 (Gramlich et al (1973)) to 1.70 (Grossman (1990)) while the majority of the recent findings narrowed it down to the range of from 0.6 to 1.

Explanations on the puzzle generally fall into two categories, estimation bias or voter misperception. The common type of estimation bias relates to endogeneity of some explanatory variables. One typical example is the so-called "price effect" associated with matching grants. Lower level jurisdictions usually need to commit their own income resource to support their government spending in order to obtain subsidies from a higher level jurisdiction at certain rates, which creates a spurious positive correlation between public spending and grants. Failure to control for this price effect can result in overestimation of the fiscal responsiveness to grant receipts. In addition, policy endogeneity is introduced from political economy literature and generalized by Besley and Case (2000).[35] They argue that empirical methodologies that exploit policy changes cross jurisdiction and over time to study the effect of policies on economic outcomes may be problematic since policies are themselves determined through bargaining processes by political representatives whose actions may reflect underlying constituent preferences.

[35] Earlier studies on policy endogeneity include Schneider and Moon Ji (1990) and Ahmad (1997). The former provides theoretical grounds for endogenous grants by analyzing factors that affect local government's decision to seek intergovernmental aid. The latter investigates grant schemes in OECD and developing countries and finds that lower-level governments have incentives to pursue policies that increase the probability of obtaining cheap resources from central authorities.

Hence, failure to identify and control for the forces that lead policies to change biases the estimates of a policy's incidence. Knight (2002) applies this idea to investigate the fiscal outcome of federal highway grants. As in Besley and Case (2000), he illustrates that both federal grants and state spending are determined through a political process which constituent preferences underlie. He develops a legislative bargaining model to incorporate the positive correlation between grant receipts and the unobserved preferences for state spending and uses the political power of state congressional delegations as the instrument variable for the endogenous grant. After controlling for the policy endogeneity, the flypaper puzzle is eliminated.

The second type of estimation bias is due to the omitted variable. Case et al (1993) formalize the situation in which states tailor their fiscal practice after behaviors of similarly situated states. They find that the omitted fiscal spillover effect leads to a substantial upward bias in the estimated responsiveness of state spending to federal grants, although the corrected marginal propensity to spend out of grant money still significantly exceeds the estimated effect of private income. The third type of estimation bias is caused by improperly used econometric model. Moffitt (1984) demonstrates that the seemingly persistent flypaper effect is associated with some incorrectly used OLS specification. He formally models the non-linear budget constraint in the presence of AFDC grant and finds that the puzzle is resolved.

The other branch of explanation features voter misperception. It is possible that voters confuse the average and marginal price effect of unconditional grants and this confusion leads to overspending on public goods and services (Courant et al (1979); Oates (1979)). Alternatively, Filimon et al (1982) tell a story that voters suffer from imperfect information and underestimate the amount of grant. The lack of information by voters makes it possible for budget-maximizing officials to set expenditures beyond the socially preferred level. More recent study by Strumpf (1998) considers the fiscal response of suburban Philadelphia communities to various public windfalls including the "earned income tax" windfall from in-commuting non-residents. He provides further evidence that imperfect voter information plays an essential role in explaining the flypaper effect.

The administrative overhead index that he proposes as a gauge of voter control over fiscal decisions helps explain heterogeneous spending elasticities out of various public windfalls.

While the vast body of the literature focuses on intergovernmental grants, there are a few exceptions. In addition to Strumpf (1998), researchers have been trying to utilize exogenous windfalls other than policy changes as the source of variation in non-private income, which helps avoid the price bias and the policy endogeneity problem as discussed above. Olmsted et al (1993) analyze the impact of bond issue retirement on school spending in Missouri. The retirement of bond issue reduces debt service rates and has an effect on spending similar to that from a lump-sum aid. They find that a dollar increase in lump-sum state aid increases per-pupil spending by 58 cents whereas additional dollar in median income leads to 5 cents increase in per-pupil spending. Ladd (1993) uses the federal tax base expansion due to the Tax Reform Act of 1986 as a natural experiment and finds that an additional dollar of TRA86 windfalls had led to an average increase of 40 cents in state spending.

In this paper, we exploit the tobacco settlement between the U.S. states and major tobacco manufacturers in the late 1990s as another natural experiment to further investigate the flypaper puzzle. Our analysis possesses a few advantages in comparison to most of the existing research. Most importantly, it is immune from the potential price effect since the payment is purely a lump sum transfer from tobacco manufacturers to states. It is also least subject to voter incomplete information because the payment amount and adjustment factors are widely publicized. Moreover, the settlement funds each state has received or will be receiving are predetermined based on a set of fixed allocation percentages agreed by state attorneys general. The fact that policy changes cross jurisdictions plays little role here prevents our estimates from the policy endogeneity problem.[36] Last but not least, the influx of tobacco funds that is favorable to

[36] Other endogeneity concerns may exist, however. For example, the settlement allocation shares were determined partially based on the historical level of Medicaid spending that is trending over time. In addition, states bargained over the allocation percentages before reaching the consensus. We shall return to these issues when discussing the empirical strategy below.

both voters and state government agencies is not earmarked for any specific usage.[37] Each state has total discretion on where to spend the money. It thus serves as a clean natural experiment to study the change in public good provision at the state level in response to a windfall. Using the fixed-trend approach to resolve some potential endogeneity issues, we find that state direct expenditure increases by about $1 in response to one dollar increase in settlement revenue, while it does so only by about 55 cents to federal grants and 8 cents to private income. The evidence supports a strong flypaper effect associated with the settlement windfall.

We also look into how voter preference contributes to heterogeneous spending behaviors among states.[38] State smoking prevalence and the engagement of state economy in tobacco manufacturing are two factors that we conjecture can considerably influence voter preferences on how to spend the windfall money. Our finding shows that states with a higher percentage of adult smoking population or major tobacco-manufacturing exhibit lower spending propensity from the tobacco revenue.

A closely related study to ours is Singhal (2008) who proposes and tests a different explanation for the non-fungibility of government spending from discretionary grants. She examines the influence on grant distribution of special interest groups with the ability to raise funds for local governments. Using tobacco settlement windfall data, she tests the fungibility of windfall revenues for state tobacco-control spending. The results suggest that the marginal propensity to spend on the state tobacco-control spending is $0.2 per dollar of settlement revenue, and states that did not file law suits spend significantly less post-settlement. She concludes that the observed spending pattern is consistent with the prediction from the interest group model proposed in her study.

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[37] Voters may perceive differently the net benefit from the settlement depending on the extent to which they are affected by the payment financially. For instance, smokers and tobacco growers will eventually bear the financial burden of the payment via forward and backward cost transfers, respectively. This impact on voter self-interest may affect the marginal proclivity of state spending out of the tobacco windfalls. We shall return to this issue when studying heterogeneous responses.

[38] Existing research has generally examined two types of heterogeneous fiscal responses. One involves asymmetric responses to the rise and fall in grants or windfall (Ladd (1993); Stine (1994); Levaggi and Zanola (2003)). Another relates to different sizes of spending responses to variation in external incomes of different sources (Strumpf (1998)).

Our study also uses the tobacco settlement data to study government spending responses, however, our primary goal is to test for the existence of flypaper effect utilizing tobacco settlement's unique feature that it is not subject to common critiques against most flypaper studies. We document flypaper effect on a much larger scale in the *overall* state spending, while the non-fungibility of settlement revenues found in Singhal's study only applies to the specific category of tobacco prevention spending, which represents only a very small portion of the total windfall receipts[39].

The remainder of the paper is organized as follows. Section 2 provides background information on the tobacco settlement payments. In Section 3, we lay out the empirical strategy, comment on potential endogeneity issues, and discuss the data variations in the tobacco payment. The results and implications are presented in Section 4. Section 5 concludes.

4.2 Tobacco Settlement Agreements

Led by state Attorney General Michael Moore, Mississippi sued the tobacco industry on behalf of taxpayers seeking for compensation for state tax money spent on smoking-related illnesses in 1994. The lawsuit, the first of its kind, led to a settlement for nearly $3.6 billion on July 3, 1997.[40] Florida, Texas and Minnesota followed and had their lawsuits individually settled for $11.3 billion, $15.3 billion and $6.1 billion in August 1997, January and May 1998, respectively. The Master Settlement Agreement (MSA), a comprehensive settlement signed in November 1998, involved the rest of the 46 states, the District of Columbia and other U.S. territories. According to the MSA, tobacco manufacturers were committed to pay approximately $206 billion over the following 25 years to states as reimbursement for past Medicaid expenditures states incurred treating

[39] For example, spending on tobacco prevention and control represented only 5% of the total settlement revenue in fiscal year 2000-2001, according to Health policy Tracking Service, National Conference of State Legislatures, August 2002.

[40] The $3.6 billion settlement was about 1%, the estimated Mississippi's share, of the proposed national settlement.

smoking related illnesses. The settlement agreements also imposed limited restrictions on the marketing of tobacco products.

There are different types of payments for the states that signed either individual agreements or the master agreement. The largest two payments are the up-front payments and the annual payments. We take the payments made to the 46 MSA states for illustration purposes. According to the master agreement, up-front payments are made in five installments of $2.4 billion annually between 1998 and 2003 adjusted for a 3 percent inflation factor.[41] Annual payments beginning with a $4.5 billion payment on April 15, 2000 will continue in perpetuity.[42] Both of these types of payments are distributed based on fixed allocation percentages agreed to by state attorneys general. The information on allocation percentages is presented in Table 0.2.

The allocation shares were resulted from negotiations that began with a complex formula. The formula was composed of a series of factors including smoking-related Medicaid expenditures, population, population densities and smoking prevalence.[43] We will return for more discussions about this formula. A third type of payment, known as the Strategic Contribution Fund payment, will begin in 2008 and continue through 2017. These are intended to reflect the level of the contribution each state made toward final resolution of the lawsuits. In addition, tobacco growers and producers in states that grow cigarette tobacco also receive Phase II payment through a separate agreement from 1999 through 2010 that is intended to provide compensation for financial losses due to the anticipated decline in cigarette consumption.

The payments actually received by each state potentially differ from the originally estimated amount due to various adjustments, most important of which are inflation adjustment, volume adjustment and non-participating manufacturers adjustment. The

[41] The initial payments are as follows: $2.400 billion in 1998, $2.472 billion in 2000, $2.546 billion in 2001, $2.623 billion in 2002 and $2.700 billion in 2003. There was no up-front payment in 1999.
[42] The annual payment schedule is as follows: $4.5 billion in 2000, $5 billion in 2001, $6.5 in each of 2002 and 2003, $8 billion in each year 2004-2007, $8.139 billion annually in 2008-2017 and $9 billion in 2018 and thereafter.
[43] General Accounting Office report, "States Use of Master Settlement Agreement Payments", June 2000.

inflation adjustment equals the actual inflation rate for the preceding year or three percent, whichever is greater. The volume adjustment applies to reflect the fluctuation in tobacco manufacturers' operating income from the cigarette sales within each state. It's estimated that cigarette consumption has declined by about 6.5% in 1999 alone mostly due to one-time increases in cigarette prices after the master agreement took effect. And it is predicted that the decline in cigarette consumption will continue by nearly two percent annually, which will result in lower tobacco payments than originally expected. The non-participating manufacturer adjustment is to address market share losses attributable to the provisions in the agreement. If the aggregate market share of the companies that participate in the agreement declines by greater than two percent, their annual payment is reduced by three percent for each percent lost over the two percent threshold. Thus, the actual amount that states have received or will receive fluctuates over time due to these adjustment factors even though their shares of the payment stay fixed.

One of the crucial features of the settlement funds is that they are not earmarked and the settlement agreements do not dictate how states should spend the money. Because of complicated legal requirements states had to fulfill, funds from the master settlement were not available to the 46 states until January 2000 when their economies were at their strongest. Since then, the MSA states have received about 22 billions from fiscal year 2000 to 2002. California and New York had received the largest amount, nearly $2.7 billion each. California, Illinois, Michigan, New York, Ohio, and Pennsylvania together received about 50 percent of the master agreement payment. During the same period, Florida, Minnesota, Mississippi and Texas that had started to receive their individual payments in earlier years received over $7 billion. On average, the settlement fund received by each state amounted to 1.3% of annual state direct spending or $34 per capita from fiscal year 2000 to 2002.

The unprecedented large amount of the settlement payments created an opportunity for states to establish new programs and expand existing programs in a variety of policy areas. Because claims for compensation for the past smoking-related Medicaid costs were

the basis for the tobacco lawsuits filed by the states, high priority has been given to health related funding and tobacco control programs.[44] Based upon the statistics provided by the National Conference of State Legislatures, a bit less than $7 billion or 30 percent of the total payment had been dedicated for health related purposes among the 46 states that signed the master agreement from fiscal year 2000 to 2002. These allocations included funding for Medicaid, State Children's Health Insurance Program, tobacco use prevention and etc. About $2 billion or nine percent of the tobacco revenue was placed in education and children and youth programs while five percent was allocated to tobacco control efforts.[45] In addition, three percent of the MSA fund was assigned for assistance to tobacco growers and economic development projects. Rainy day funds and endowments were also established to fund program activities with interest earned. The vast majority of these endowments will provide funds for health care services. A few states, for example, Connecticut and Illinois, used the payment explicitly to fund tax reduction. Specifically, Connecticut used a total of 38 percent, or $50 million per year, of its tobacco payment for property tax reductions. Illinois used 50 percent, $316 million, for an earned income tax credit and a one-time property tax reduction. The figures represent states' reported spending from settlement revenue, and do not take into account potential crowd out from other parts of the state budget.

4.3 Empirical Strategies

To test for the existence of the flypaper effect associated with the settlement revenue, we estimate state marginal propensities to spend from voter private income and settlement windfalls, respectively. As in most studies, we make use of the state budget constraint

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[44] In June 1997, the AG of Indiana sent a memo to all suing Attorneys General which made clear that Medicaid recoveries were central to the state lawsuits. The memo explains that "states are in business of administering Medicaid, and Medicaid reimbursement was the primary element of damages for most, if not all, suing States." In a footnote, the memo elaborates: "We realize, of course, that most States also sued on other theories such as antitrust, RICO, and consumer protection. States have in common, however, the desire for Medicaid reimbursement." Source: Attorney General Jeff Modisett, Memorandum to All Suing Attorneys General, June 23, 1997, p.3.

[45] Education includes funding for kindergarten through grade 12 education and tuition for and scholarships to community colleges, colleges and universities. Children and youth program includes funding for early childhood programs, after-school adolescent programs and juvenile justice programs.

and allow the slope coefficients of different types of income to vary. The analysis is based on state government financial data from Government Finances, Census Bureau, as well as settlement revenue allocation information. The sample contains annual observations of 50 states for fiscal years 1992-2002.[46] Summary statistics for the variables used in the study are presented in Table 0.3.

We use a fixed-trend model as specified in equation [0.1].

[0.1] $$Spend_{it} = \alpha_i + \lambda_t + \alpha_i \cdot t + \beta_1 Income_{it} + \beta_2 Settle_{it} + \beta_3 Aid_{it} + \varepsilon_{it}$$

The dependent variable is state direct expenditure that is defined as the general expenditure exclusive of intergovernmental transfer payments, utility, liquor store and insurance trust expenditures. The key independent variables are income variables from various sources. $Income_{it}$ is personal disposable income, $Settle_{it}$ state settlement receipts and Aid_{it} federal grants that are used to control for other income resources available for state governments. All dollar amounts are in per capita real terms deflated using year 1992 CPI. The symbol i indicates state and t year. The variables α_i and λ_t are state and year dummies, respectively. The variable t is the time trend. States are allowed to have different spending trend over time, which is captured by $\alpha_i \cdot t$.

The β's measure the marginal propensities of state public spending out of private income, settlement funds and federal grants. We are particularly interested in the comparison between the marginal propensity to spend from income of private source and that from settlement windfalls. The flypaper effect predicts that the tobacco revenue tends to stimulate more public spending than private income, i.e. $\beta_1 < \beta_2$. The size of the fiscal impact of settlement receipts is also interesting. Estimated β_2 that falls between zero and one represents the extent to which settlement revenue crowds out revenues of state own source in public spending. If β_2 is greater than one it implies that, on average, state government spends more than one dollar in response to a dollar of tobacco windfall and hence needs to match high public spending with incomes from other sources.

[46] We exclude the District of Columbia and the U.S. Commonwealths that are participants in the MSA. Throughout this study, fiscal year refers to the state's fiscal year. In most states, the fiscal year begins on July 1 and ends on June 30. The exceptions are in Alabama and Michigan where the fiscal year begins on October 1 and in New York where the fiscal year begins on April 1.

The fixed-trend specification is adopted to deal with a potential endogeneity problem in the variable *Settle* due to the dependence of settlement revenue on the historical Medicaid expenditure. Specifically, the allocation process of settlement payments among states indicates that state historical Medicaid expenditure was one of the major determinants in the settlement allocation as the settlement was intended to compensate states for their high historical spending when treating smoking-related illnesses. The higher Medicaid cost a state had incurred in the past, the larger share of settlement revenue it received. On the other hand, Medicaid expenditure comprises about one third of state direct expenditures annually and is also trending over time. This leads to a spurious link between settlement receipts and state spending, since both are positively correlated with past Medicaid cost. The fixed-effect model would fail to address this endogeneity issue and lead to an overestimation of the causal effect of *Settle*. The fixed-trend model, on the other hand, can resolve the endogeneity concern since the settlement receipts depend only on historical *level* of Medicaid cost rather than its *growth*. The linkage of endogeneity that operates through levels of settlement payments and state spending is broken in the fixed-trend model, provided that the changes in settlement receipts by states over years does not depend on the growth of contemporaneous Medicaid cost or that of state aggregate public spending. Simply put, allowing the state fixed trend is equivalent to the approach that implicitly controls for state specific propensity to spend on Medicaid, which should resolve the endogeneity issue due to the dependence of *Settle* on the state spending history of Medicaid.

The fixed-trend approach doesn't come without any caveat, however. Particularly, the data variation in the fixed trend model primarily comes from transitory deviations from the trend of spending and these transitory deviations may be subject to contamination of unexplained noise. In other words, the marginal propensities of public spending, β's, are now determined by changes in income sources that lead to transitory deviations in state spending from its trend. Since the time trend picks up the effect of permanent factors in income shocks and leaves only transitory variation to be explained by changes in various

income sources, the coefficients of those income variables may be estimated with larger standard errors in comparison to those in other models, for example, a fixed effect model.

An additional source of endogeneity we are potentially concerned with *Settle* is regarding the bargaining process among the 46 MSA states before reaching the final agreement on the splitting shares. If bargaining indeed played a crucial role in the settlement fund allocation and was correlated to certain unobservable elements such as spending needs or preferences that affect growth of state public spending, our analysis can be potentially subject to a policy endogeneity problem discussed intensively in Knight (2002).

One may conjecture that state effort during the collective lawsuits may have been rewarded through greater allotment percentage of initial and annual payments. This can very likely lead to a policy endogeneity problem since the willingness to engage great effort in final resolution of the lawsuits may reflect underlying needs or preferences for public spending by the state government. Fortunately, effort was not among the factors that determined the shares obtained by states. In stead, a separate category of payments, the Strategic Contribution Fund, is intended to compensate for contribution each state made during the litigation process. An annual amount of 861 million dollars of the Strategic Contribution Fund payments will be made from year 2008 through 2017 to states. The percentages vary from 0.8 to 5.8, based upon a separate formula developed by a panel of former state attorneys general. Specifically, State of Washington will receive 5.8%, New York 5.5% and California 5.2% whereas Alabama, Arkansas, Delaware, Idaho, Kentucky, Nebraska, South Dakota, Tennessee, Virginia and Wyoming get 0.8% each.

As mentioned earlier, the allocation allotments of initial and annual payments were determined based upon a formula developed by the attorney generals. The process was followed by negotiation as some smaller states argued that they should receive a larger percentage to enable them to fund smoking cessation programs because they did not have the same economy of scale as larger states. The allocation fractions were then adjusted

accordingly.[47] Although we are unable to obtain the formula we explore the determinants of the allocation percentages by conducting a simple cross section OLS analysis as elaborated next. We conclude that there was little room for bargaining to play a big role in the fund allocation process.[48]

The allocation percentages are run against the estimated smoking-attributable Medicaid expenditure in year 1998, average total population, average population density and average adult smoking rate from 1991 through 1998. The explanatory variables are chosen according to the agreement documents.[49] The regression results are presented in Table 0.4. Smoking-related Medicaid expenditure and state population have significant and positive effect on the allocation shares. It implies that more populous states with higher Medicaid spending in the past were compensated with a greater share. And these two factors alone can explain 99 percent of the variations in the percentages across state. Population density also has a positive effect that is statistically significant at the 1% level although it contributes little to the model's estimating power. The direction of the effect of the adult smoking prevalence rate depends on the specification of the model but it is not significantly different from zero after we control for the Medicaid spending and population. We also augment the model by incorporating state characteristics that are related to anti-smoking policy. One attempt is to include an index variable for the six states whose economy heavily relies on tobacco production and to see whether their shares differ significantly from the non-tobacco states.[50] Our conjecture is that lenient anti-tobacco policies adopted by tobacco states, for example, much lower cigarette excise tax rates than other states, are very likely to undermine their bargaining power. Column (4) of Table 0.4 suggests that tobacco states received a lower fraction of settlement receipts and the effect is statistically significant at the 5% level. Inclusion of this

[47] General Accounting Office. Tobacco Settlement: States' Use of Master Settlement Agreement Payments, page 11. (GAO-01-851, June 2001.)
[48] Singhal (2008) also shows that negotiations among states only resulted in some small adjustments to the base percentages. Her study uses simulation to replicate the allocation percentages based on the formula that she obtained. The correlation between the simulated results and the percentages under the settlement is 0.99. The differences are small.
[49] General Accounting Office. Tobacco Settlement: States' Use of Master Settlement Agreement Payments, page 11. (GAO-01-851, June 2001.)
[50] The six major tobacco states are Georgia, Kentucky, North Carolina, South Carolina, Tennessee and Virginia.

indicator, however, neither has any crucial impact on estimated coefficients of other variables nor increases the explanatory power of the model since the adjusted R-squared barely changes. In summary, the allocation percentages almost perfectly reflect state historical smoking-related medical expenditures, indicating that the negotiation played little role during the allocation process. Therefore, policy endogeneity doesn't pose a large concern in our analysis.

So far we have identified and attempted to resolve various potential endogeneity problems associated with *Settle*. The federal aid variable, *Aid*, should also be handled with caution due to its aggregate nature. The federal aid to states usually consists of open-ended matching grants like Medicaid which is the largest part of federal aid, closed-ended matching grants as highway aid, and block grants such as Title I grant or TANF.[51] Block grants are lump-sum transfers, the amount of which does not depend on the level of state spending and hence its effect is purely income effect. Matching grants, however, work as subsidies to state governments by effectively lowering the price of public goods provision.[52] The amount of matching grants that states receive is thus endogenous to how much they actually spend. The fixed-trend specification alleviates the endogeneity problem. Nonetheless, there can still be a potential bias transmitted from *Aid* to *Settle* as long as the correlation between these two variables exists. Since our focus is the settlement revenue rather than the federal grant in this study we show in the next section that our results, particularly the coefficient of *Settle*, are robust to the exclusion of the grant variable from the regression. Furthermore, we are able to pin down the range for the income effect of *Settle*. Hence, we conclude that bias from estimating the coefficient of *Aid*, β_3, does not transmit an important bias to our coefficient of interest.

4.4 Results

The basic results are presented in Table 0.5. We investigate further the heterogeneous impacts of various income sources on state spending in Table 0.6. The data period is from fiscal year 1992 to 2002. Data from the pre-settlement period are included to control for

[51] Medicaid accounted for 42% of total federal aid to states in the fiscal year 2002.
[52] For closed-ended matching, the price incentive is effective only when the expenditure is below the cap.

the time trend in state public spending. Heteroskedasticity-robust standard errors are reported. P-values from F-tests on the hypothesis that settlement windfalls stimulate more public spending than voter private income are laid out in the bottom of the tables for the corresponding regressions.

Results of the baseline fixed-trend model in Table 0.5 show that the estimated coefficient of settlement revenue is 1.182, significantly different from zero at the 1% level but not different from one. This suggests that state direct spending increases by $1.182 in response to a one dollar settlement payment. One dollar increase in per capita personal income, however, leads to about 8 cents of increase in state spending and the effect is also significantly different from zero at the 1% level. The F-test further shows that the income impact of exogenous settlement windfalls is significantly greater than that of an equivalent increase in voter personal income and thus is supportive of the existence of the flypaper effect.

Not surprisingly, state spending also responds to the amount of federal aid received. The magnitude of the response is 0.549, statistically significant at the 1% level. Although this coefficient estimate is in the ballpark of findings from other studies in the literature we suspect it is still somewhat overestimated due to the endogeneity problem elaborated early. The primary concern of the potential bias in the federal aid variable is that the coefficient estimates of other variables may be affected due to transit bias. So we examine the sensitivity of other coefficient estimates to the potential endogeneity in the federal aid variable by excluding it from the regression. The results in column (2) of Table 0.5 show that the estimates without the federal aid variable (*Aid*) in which case the effect of *Settle* are underestimated. The magnitude for the estimated effect of settlement drops slightly to 0.991. The standard error changes little, and so does the estimate for the personal income coefficient. Since the true effect of federal aid must lie between 0 and 0.549 we are confident that the true effect for the settlement payment will be in the range of 0.991 and 1.182. The supporting evidence comes from a series of constrained regressions that we run by pre-imposing the coefficient of *Aid* at various values within the range between 0 and 0.549. This exercise allows us to observe the changes in the

coefficient of *Settle* and the associated standard error, and to numerically examine the transit bias. Results of the exercise show that the estimated coefficient of *Settle* changes monotonically with the imposed value of the federal aid coefficient, and the size is bounded between 0.991 and 1.182.[53] At the same time, the standard error almost remains the same. The estimated effect from personal income exhibits a similar pattern. For the purpose of inference on the flypaper effect of *Settle*, we therefore conclude that the endogeneity bias associated with *Aid* due to the feature of matching grants does not exert practically important impact on our main results.

Compared with Ladd's natural experiment study which utilizes TRA86 windfalls as exogenous income shock, we find more marked flypaper effect associated with tobacco settlement. To the best of our understanding, this difference in the sizes of spending response can be explained by differences in voter information and voter perceptions on the windfall allocation. However, these two attributes have exactly the opposite predictions. On one hand, voters are most likely to be better informed about the size of settlement payment their states obtained through the publicized litigation process and publicly available documents of the settlement agreement. In contrast, making a precise prediction on the magnitude of the TRA86 windfall is very difficult for both policy makers and voters since the tax windfalls were automatically generated through revenue-sharing and also subject to various uncertainties in the macroeconomic environment.[54] According to Strumpf (1998) reasoning that limited voter information and control over the fiscal decisions (imbedding in high overhead levels) causes great rates of spending out of the public windfall, the tobacco settlement payment is expected to have a smaller flypaper effect than TRA86 tax windfalls. On the other hand, the TRA86 windfalls and the tobacco settlement payments stemmed from entirely different income sources from the voter standpoint. One was due to the changes in the federal tax codes and the other was paid as a compensation for past smoking-related Medicaid spending that states

[53] Results of these regressions aren't reported in the paper but are available from the authors upon request.
[54] In Ladd (1993), the TRA86 windfalls are generated by using different simulation models including TAXSIM, ACIR and NASBO/NCSL. The TAXSIM estimates are substantially higher than the ACIR and NASBO/NCSL estimates, with the total ranging from $10.5 to $11.9 billion in contrast to the $5.2 billion for the ACIR and $5.9 billion for NASBO/NCSL.

should not have incurred had the smoking rates been low. As a consequence voters may have dramatically different opinions on how to allocate these revenues. In the former case, taxpayers to whom the tax windfalls to their states come directly from their personal income may be more naturally inclined toward retaining the revenue rather than spending them. In the latter, voters are more likely to support spending the windfalls on anti-smoking activities, health programs and other categories that had been crowded out by smoking-related Medicaid expenses in the past. Our results show that the flypaper phenomenon is more substantial in the tobacco settlement case, which is probably not a surprise considering fiscal crises most states have experienced since the year when they started to receive the payment.

We further add interaction terms to the baseline model and explore whether the income effect of *Settle* varies across states. In other words, we use certain state characteristics to examine potential heterogeneity in the degree of the flypaper effect. The results are presented in Table 0.6.

In column (2), fifty states are categorized into two groups based upon the extent to which state economy relies on tobacco related production. We conjecture that the settlement revenue may be spent differently in the six major tobacco states than in other states. In those states whose economic growth heavily relies on tobacco industry, settlement payment is less likely to be viewed as the pure windfall. Instead it may be perceived as a mandatory transfer from tobacco manufacturers to the state government, the cost of which will eventually be borne by voters, especially those tobacco growers.[55] If fiscal decisions are made to reflect the voter interest we expect that the settlement revenue is more likely to be retained for the tobacco states and thus boosts less public spending. Consistent with our expectation, the result in column (2) shows that the six tobacco states have a much lower spending response to settlement receipts. One dollar of the payment is associated with about zero increase in public spending in tobacco states as opposed to

[55] Phase II payments are used particularly to compensate tobacco growers. However, the magnitude of compensations is too small to parallel the cost borne by these tobacco growers due to the settlement.

$1.26 of increase in non-tobacco states. We can not reject the hypothesis that the difference in spending responses equals to zero since the standard error is relatively large.

Furthermore, we investigate how voter characteristics affect the magnitude of flypaper effect. One important implication from the median-voter model is that not only median voter income but also her socio-economic characteristics are determinants of the provision level of public services since her characteristics capture her preferences regarding public spending. A simple measure we use here is whether on average voters are more or less likely to be smokes or not. For smoking voters, the settlement payment looks less like free money, as the burden of the payment will eventually be transferred to and at least partially borne by cigarette consumers through higher prices. In fact, evidence has shown that the settlement payments are indeed financed by higher cigarette prices. The net wholesale price was increased by 36 percent from $62.7 to $85.2 per 1,000 premium brand cigarette following the master agreement in November 1998.[56] In states where median voters are less likely to smoke, the settlement payment is presumably perceived as windfalls to a greater extent and boosts more public spending. Hence our hypothesis is that the spending propensity out of settlement money is lower for states with smoking median voters. In our regression analysis, we use state adult smoking rates to proxy for probabilities of median voters being smokers. Statistics of adult smoking rates are obtained from CDC's Behavioral Risk Factor Surveillance System (BRFSS). Current smokers are defined as persons who reported having smoked more than 100 cigarettes and currently smoked every day or some days during the survey period. Adult smoking rates are computed by state over year and then averaged through 1998 and 2002 by state. Then an index variable is generated to represent the group of states whose adult smoking rates exceed the national average rate. The high smoking index variable is further interacted with per capita settlement revenue in the regression listed in column (3) of Table 0.6. As shown, states with the high percentage of smokers tend to spend a lot less out of settlement money, 52 cents in response to one dollar of windfalls compared to $1.534 by those with low smoking rates, although the difference is

[56] Tobacco Briefing Room, Economic Research Service, United Stated Department of Agriculture. URL: http://www.ers.usda.gov/Briefing/tobacco/Data/table09.pdf.

not statistically significant at the conventional level due to the relatively large standard error.

The estimates of income effects from the personal income and federal grant are robust to the inclusion of the interaction terms. F-tests show a persistent flypaper effect in the heterogeneity analyses.

For robustness tests, we exclude the federal aid variable from the heterogeneity analyses. Columns (4) – (6) of Table 0.6 illustrate the estimated coefficients for the three heterogeneity regressions without the federal aid variable. As shown, the results are by and large robust and the main implications persist.

4.5 Conclusion

Tobacco settlement payments made by the major tobacco manufactures to the U.S. states in the late 1990s serve as a natural experiment for studying the responsiveness of state government spending to windfall receipts. The unique features of the payments provide us with a natural experiment to analyze fiscal decisions on exogenous income shock, an advantage over earlier work in the flypaper literature. Results suggest a strong flypaper effect. We find that state governments have greater spending proclivity from the settlement windfalls than from personal income. Specifically, state direct spending increases by about $1 on average in response to one dollar increase in the settlement funds.

We also find that state heterogeneous responses to the windfall income reflect their socio-economic characteristics. States with higher proportion of adult smoking population or major tobacco manufacturing industries exhibit lower spending propensity from the tobacco windfall.

Table 0.1: Measures of the Flypaper Effect

Author	Sample	MPS from income	MPS from grants
Grossman (1990)	State and federal grants to 136 Virginia local governments, 1981	0.01	1.70
Ladd (1993)[a]	TRA86 windfalls to states	0.03	0.98
Olmsted et al (1993)	State and federal grants to 344 Missouri school districts, 1980	0.05	0.58-1.15
Case et al (1993)	Federal grants to states 1970-85	0.11-0.17	0.65-1.04
Becker (1996)	Federal grants to state and local governments, 1977-1986 (panel)	0.06	0.61
Gamkhar and Oates (1996)	Federal grants to state governments, 1953-1991 (time-series)	0.11	0.62
Strumpf (1998)[b]	Various windfall revenues to Pennsylvania municipalities in the Philadelphia metropolitan area including state liquid fuels highway aid fund	0.24-0.43	0.53-0.57
Knight (2002)	Federal highway grants to 47 state governments, 1983-1997 (panel)	0.01	1.14

Note: This table provides some representative estimates of marginal propensity to spend on public Services. Part of contents in the table is adapted from Hines, James R. Jr. and Thaler (1995).
[a] MPS from the TRA86 tax windfall is 0.4.
[b] Strumpf (1998) reports income elasticity rather than MPS.

Table 0.2: State Allocation Percentages

State	Percentages	State	Percentages
Alabama	1.616131	Nebraska	0.594983
Alaska	0.341419	Nevada	0.609935
Arizona	1.473885	New Hampshire	0.665934
Arkansas	0.828066	New Jersey	3.866996
California	12.76396	New Mexico	0.59639
Colorado	1.370861	New York	12.76203
Connecticut	1.856537	North Carolina	2.332285
Delaware	0.39547	North Dakota	0.366014
Georgia	2.454458	Ohio	5.03751
Hawaii	0.601865	Oklahoma	1.036137
Idaho	0.363263	Oregon	1.147658
Illinois	4.654247	Pennsylvania	5.746859
Indiana	2.039803	Rhode Island	0.718905
Iowa	0.869667	South Carolina	1.176352
Kansas	0.833671	South Dakota	0.348946
Kentucky	1.761159	Tennessee	2.440895
Louisiana	2.255353	Utah	0.444887
Maine	0.769351	Vermont	0.411185
Maryland	2.260457	Virginia	2.044745
Massachusetts	4.038979	Washington	2.053258
Michigan	4.351948	West Virginia	0.88646
Missouri	2.274601	Wisconsin	2.072039
Montana	0.424759	Wyoming	0.248345

Source: The Master Settlement Agreement Exhibit A

Table 0.3: Descriptive Statistics

Variable	# Observations	Mean	Std. Dev.	Min	Max
PC state general direct expenditure	550	2,004	788	1,204	6,865
PC personal disposable income	550	18,793	2,574	13,319	27,718
PC federal grant	550	771.4	231.9	386.6	1,886
PC settlement revenue	155	27.6	14.8	9.6	129.7
Average adult smoking rate (1991~1998)	50	23.2	2.63	14.9	29.7
MSA allocation percentage	46	2.135	2.668	0.248	12.764

Note: All financial variables are deflated using year 1992 CPI.

Table 0.4: Determinants of State Shares of Settlement Payments

	(1)	(2)	(3)	(4)
Smoking-related Medicaid expenditure (1998)	1.909	1.856	1.859	1.825
	(0.102)***	(0.091)***	(0.093)***	(0.088)***
Total population (average 1991~1998)	0.255	0.255	0.255	0.260
	(0.013)***	(0.012)***	(0.012)***	(0.011)***
Population density (average 1991~1998)		0.537	0.532	0.519
		(0.148)***	(0.151)***	(0.142)***
Average adult smoking rate (1991~1998)			-0.004	0.006
			(0.013)	(0.013)
Tobacco states				-0.264
				(0.107)**
Constant	0.013	-0.062	0.038	-0.176
	(0.054)	(0.052)	(0.321)	(0.315)
Observations	46	46	46	46
Adjusted R-squared	0.990	0.992	0.992	0.993

Note: Dependent variable is stateshares of settlement payments, in percentage terms. Heteroskedasticity-robust standard errors are in parentheses. Significance level notation: * at 10%; ** significant at 5%; *** significant at 1%.

Table 0.5: Estimation of the Income Effects on the State Public Spending (1992-2002)

	(1)	(2)
PC personal income	0.077	0.078
	(0.022)***	(0.023)***
PC settlement	1.182	0.991
	(0.416)***	(0.387)**
PC federal grant	0.549	
	(0.139)***	
Observations	550	550
Adjusted R-squared	0.999	0.999
P-value for F-test	0.004	0.010

Note: Dependent variable is per capita state direct expenditure. Heteroskedasticity-robust standard errors are in parentheses. Coefficients of the state and year dummies and the state fixed trends are omitted. Significance level notation: * at 10%; ** significant at 5%; *** significant at 1%. F-test H_0: $\beta_2 = \beta_1$; H_1: $\beta_2 > \beta_1$

Table 0.6: Heterogeneous Income Effects on State Public Spending

	(1)	(2)	(3)	(4)
PC personal income	0.076	0.074	0.077	0.075
	(0.022)***	(0.023)***	(0.023)***	(0.024)***
PC settlement	1.261	1.534	1.080	1.313
	(0.475)***	(0.569)***	(0.428)**	(0.480)***
PC federal grant	0.548	0.552		
	(0.139)***	(0.140)***		
(PC settlement)*	-1.257		-1.402	
(tobacco states)	(1.642)		(1.449)	
(PC settlement)*		-1.011		-0.928
(high adult smoking)		(0.775)		(0.761)
Observations	550	550	550	550
Adjusted R-squared	0.999	0.999	0.999	0.999
P-value for F-test	0.004	0.030	0.028	0.025

Note: The dependent variable is the per capita state direct expenditure. Heteroskedasticity-robust standard errors are in parentheses. Coefficients of the state and year dummies and the state fixed trends are omitted. Significance level notation: * at 10%; ** significant at 5%; *** significant at 1

Chapter 5Conclusions

The first two essays in this dissertation examine the determinants of corporate control instruments. The first essay proposes potential benefits from monitoring as important determinants of institutional block holding, where the measures for monitoring benefits are identified from the information asymmetry problem. I show that agency problems attract large institutional holdings for monitoring purpose, and the investors later mitigate the problems. The study thus contributes to the existing literature on investor activism, which is usually limited to studying the effect of an existing large investor. The study can be generalized to cross-country studies on the differences in corporate control structures.

In the second essay, we use the introduction of the U.S. 1996 Telecommunications Act as our empirical setting to examine the dynamics of the role of peer performance in managerial compensation. The results from 45 pairs of telecommunications and size- and performance-matched benchmark firms indicate that the weight on peer performance declined post-Act, suggesting that RPE became more valuable for the telecommunication firms following the Act. Besides shedding some light on the impact of the Act, our results show how managerial incentives in general, and the role of peer performance in particular, changed with the firm's competition environment. The use of a natural experiment helps resolve empirical problems concerned with existing studies. Last but not the least, our focus on managerial incentives complements the literature on telecommunication sector reforms, as those studies usually examine factors such as product prices and industry concentration, which likely to interact with managerial incentives.

The first two essays are within my main research agenda in organizational economics and corporate finance. As a reflection of my other research interest, the third essay contributes to the public finance literature by identifying a natural experiment to study the response of state spending to windfalls. Our analysis possesses important advantages in comparison to most of the existing studies because of the unique features of the tobacco settlement funds as exogenous windfall. Results show that one dollar increase in the settlement revenue boosts state direct spending by about one dollar, a significantly larger spending response than the spending propensity out of the personal disposable income. In addition, we find that state heterogeneous responses to the windfall income reflect their socio-economic characteristics.

APPENDICES

Appendix A

Theoretical Insert for Chapter 2

Here I consider the monitoring problem again for a risk-averse owner, in order to illustrate how the monitoring consideration interacts with risk. Her utility function is given by $U_O(x) = 1 - e^{-r_O[x]}$, where r_O is her risk aversion coefficient. If she monitors, the owner allocates her wealth by holding a share of α in the firm and the rest in a market portfolio. Taking the holding α as given, and assume fixed monitoring cost *M*, her expected payoff expressed in certainty equivalent is given by:

[A.1] $$V_{monitor} = \alpha E[y''' - w''] - M - \frac{1}{2} r_O \alpha^2 VAR[y'' - w'']$$

$$= \alpha[\beta_1''^* - (\beta_0''^* + \beta_1''^* e''^*)] - M - \frac{1}{2} r_O \alpha^2 \sigma_\varepsilon^2 .$$

Since manager's effort choice is unaffected by owner's attitude towards risk, the investor's problem is to maximize [A.1] subject to the constraints [2.2] and [2.3]. The maximization leads to the same optimal contract given in [2.4]. The effect of α reflects a trade-off for the owner between optimal monitoring incentives and risk diversification: the owner must forego some gains from diversification to hold a significant stake in the firm. To simplify the calculation for optimal shareholding and payoff, I take the special case of perfect monitoring, with the optimal contract given by [2.5]. The problem then breaks down to:

$$\max \frac{1}{2}\alpha(1 - r_O \alpha \sigma_\varepsilon^2) - M \text{ wrt. } \alpha$$

which produces an optimal stake $\alpha^* = \frac{1}{2 r_O \sigma_\varepsilon^2}$, and correspondingly $V^*_{monitor} = \frac{1}{8 r_O \sigma_\varepsilon^2} - M$.

I further assume concentrated ownership has no other benefit beyond facilitating monitoring: without monitoring, the optimal ownership structure will be perfectly dispersed. Hence for the owner, the relevant benchmark to monitoring is the case of holding a diversified portfolio with the same stake in the firm that is not monitored (assume the return equals the market return). The expected payoff for the owner becomes:

$$V_{non-monitor} = \alpha E[y - w] = \frac{1}{2}\alpha \frac{1}{1 + r_M \sigma_\varepsilon^2},$$

where y and w are given by the optimal contract in the benchmark case without monitoring. With the same size of holding as in the monitoring case, the expected pay-off for the non-monitoring investor becomes $V^*_{non-monitor} = \frac{1}{2}\frac{1}{2r_O\sigma_\varepsilon^2}\frac{1}{1 + r_M \sigma_\varepsilon^2}$.

The investor will decide to acquire a large stake and monitor *iff*

$$\frac{1}{8r_O\sigma_\varepsilon^2} - M \geq \frac{1}{2}\frac{1}{2r_O\sigma_\varepsilon^2}\frac{1}{1 + r_M \sigma_\varepsilon^2}, \text{ or}$$

[A.2] $$\frac{1}{4r_O\sigma_\varepsilon^2}\left(\frac{1}{2} - \frac{1}{1 + r_M \sigma_\varepsilon^2}\right) \geq M .$$

Appendix B

Data Source for Chapter 4

1. Data for state general direct expenditures and federal grants are obtained from the Census Bureau publication *State Government Finances* (fiscal year 1992~2002).

2. The information on the financial management and appropriation of tobacco settlement revenue is obtained from *State Management and Allocation of Tobacco Settlement Revenue 2002* published by the National Conference of State Legislatures.

3. Data for personal disposable income and total population are taken from the Regional Economic Information System, Bureau of Economic Analysis for various years. The online source is http://www.bea.doc.gov/bea/regional/spi/.

4. Data on adult smoking rates are obtained from CDC's Behavioral Risk Factor Surveillance System surveys.

5. Data on smoking-related Medicaid expenditure are taken from CDC's publication *State Tobacco Control, Highlights* 1999, 2001 and 2002.

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