

Corporate Governance and Executive Pay: Evidence from Takeover Legislation

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Abstract

We examine the effects of anti-takeover legislation on CEO pay. Since these laws altered an important component of governance, the threat of takeover, they provide a natural testing ground for theories of executive compensation. Under skimming models, where entrenched CEOs pay themselves, we would expect mean pay to rise as less governed CEOs manage to skim more. Under contracting models, where a principal optimally sets pay, we would expect a fall (or no effect) in mean pay since CEOs no longer need to be compensated for the risk of takeover. We might expect, however, a rise in use of pay for performance to offset the reduced incentives. Consistent with skimming, we find that mean pay rose in firms affected by the laws (relative to a control group). Moreover, the rise in pay was largest in firms that did not have a large shareholder present prior to the law. Turning to pay for performance, we find evidence of a rise in pay for performance on accounting measures. This rise, however, seems to have been concentrated in firms with large shareholders. Together, these results suggest that firms without large shareholders match more closely the skimming model. The optimal contracting model, on the other hand, may have more relevance in the presence of a large shareholder who can serve as the principal these models posit.

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1 Introduction

In the 1980s, many states passed anti-takeover legislation which erected legal barriers to the hostile takeover of firms. Since shoddy management can precipitate a hostile takeover and loss of job, the threat of takeover keeps managers in check, giving them incentives to maximize shareholder wealth. By reducing this threat, these laws changed the incentive structure faced by CEOs, providing a natural testing ground for theories of executive pay. This paper empirically examines the impact of anti-takeover legislation on executive compensation.

Two prominent theories shape the way we would think about the effects of these laws on pay. In “skimming” models, where entrenched CEOs skim whatever pay they can from their shareholders, a reduction in takeover threat should raise pay. Uncontrolled CEOs can now increase their rents because of the weakened market discipline (Borokhovich, Brunarski and Parrino, 1997, and Agrawal and Knoeber, 1998). In optimal contracting models, where a principal optimally sets CEO pay, we might expect mean pay to fall. Indeed, an important source of risk that the CEO faces has now been removed and a compensating differential for that risk no longer needs to be paid (Knoeber, 1986). Moreover, the simple contracting model would predict a rise in pay for performance, as the principle attempts to offset a weakening of one incentive scheme by strengthening another.¹

Two papers have independently examined the effect of takeover threats on CEO compensation. Agrawal and Knoeber (1998) proxy the threat of a takeover with the actual industry incidence of takeovers or with a variable that measures whether a firm will *actually* be taken over in the

¹This increased incentive itself clearly generates some risk, which may offset some of the reduction in risk caused by falling takeover probabilities. An analysis of this risk alone (excluding the declining takeover probabilities) can be found in Bertrand and Mullainathan (1998). Other empirical work has examined the idea that incentive schemes can substitute for reduced governance. For example, Brickley and James (1987) find some evidence on such substitution in the case of banking. Hubbard and Palia (1995) provide a clever test for substitution by using legislative changes that increased competition in the banking sector. For a broader set of firms, Gibbons and Murphy (1992) find that as career concerns fall (with closeness to retirement), pay for performance rises, suggesting substitution of incentives.

future. In a cross-section, they examine the differential impact of their takeover threat measure on firms with and without golden parachutes and other explicit employment contracts. They find that pay increases with their measure of takeover threat and that it increases mainly in firms without golden parachutes. On the other hand, Borokhovich, Brunarski, and Parrino (1997) show that firms that adopt anti-takeover amendments have higher salaries and more valuable options grants. A key difference between our paper and these other two lays in the identification strategy. Both of these papers rely on sources of variation that could generate serious endogeneity and omitted variable biases: a firm’s decision to adopt anti-takeover amendments, a firm’s decision to adopt golden parachutes, or the mean level of takeover activity in an industry. In fact, the differences between the findings of these two papers may be due to conflicting estimation biases. In contrast, state-level legislation generates more exogenous variation. Thus, while the other two papers use more straightforward but more endogenous sources of variation, we use a less straightforward but less endogenous source.

State anti-takeover legislation differed in date of passage and stringency, providing two sources of variation for our empirical methodology. Our methodology exploits both of these sources of variation. We compare changes in CEO compensation before and after the laws between firms incorporated in states enacting laws and firms incorporated in other states. The first level of differences—before and after the law—eliminates any fixed differences between states passing laws and states not passing laws. The second level of differences—change in compensation in “passing” states minus change in compensation in “non passing” states—eliminates any common (aggregate) shocks contemporaneous with the laws.² We use the variation in anti-takeover law stringency to further ensure that we identify the effects of the laws: more stringent laws should have larger effects.

²In practice, we exploit the panel nature of our micro data more thoroughly, allowing for firm fixed effects and a variety of other controls. See Section 4 for a discussion of our empirical methodology.

Using this methodology, we find that total CEO compensation increased by approximately 5%. These results are robust to a variety of specification checks including attempts to deal with the political economy of the laws. We also find that Business Combination statutes, one particular type of anti-takeover legislation, had the biggest impacts, Fair Price statutes had smaller impacts, and Control Share Acquisition statutes had no impact. This is comforting since empirical research on the share price reaction to these laws finds the same ranking.³ This first set of results supports the skimming argument.

The reduction in takeover threat may lead to a particularly strong increase in managerial entrenchment among firms that have only weak alternative governance mechanisms in place. One such alternative form of governance is provided by large shareholders (Shleifer and Vishny, 1986). Because large shareholders actively monitor corporate activity, they may be able to offset some of the reduction in governance that followed the laws. We test this prediction in the paper. We separate the firms in our sample into two groups based on whether there was a large shareholder present or not prior to the passage of the laws. Large shareholders may be able to offset some of the reduction in governance (Shleifer and Vishny, 1986). Consistent with this idea, we find that the pay of CEOs in firms with at least one large shareholder grew by a (statistically insignificant) 2%. In contrast, CEO pay grew by a (statistically significant) 7.5% in firms without large shareholders.

We also find some evidence that pay for performance rose following these laws, especially for accounting measures of performance. The relationship between total compensation and accounting rates of return rose by approximately 30%. However, we also find that the increase in pay for performance sensitivity was concentrated in firms *with* large shareholders.

In summary, our paper shows that firms without large shareholders experience large increases

³See Karpoff and Malatesta (1989).

in mean pay following the reduction in takeover threat, whereas firms with large shareholders experience increases in pay for performance but small changes in mean pay. These results suggest that a skimming model of CEO compensation works well for firms without large shareholders. The predictions of the contracting model seem to fit better for firms that have large shareholders who can serve as active principals.

The rest of the paper is laid out as follows. Section 2 of the paper describes the anti-takeover laws and discusses prior work on their impact. Section 3 describes the data set. Section 4 outlines our empirical methodology. Section 5 presents the results. We conclude in Section 6.

2 State Takeover Laws

2.1 Description

Serious regulation of modern tender offer activity in the United States begins with the Williams act, a federal statute passed in 1968.⁴ The Williams Act provided for detailed disclosure requirements, an antifraud system, and other measures to protect shareholders during the tender offer process. Individual states greatly extended the Williams Act by passing their own statutes in the 1970s. These are known as the “first generation” of anti-takeover laws. They were deemed unconstitutional by the Supreme Court in 1982 (*Edgar v. Mite Corp.*) primarily because of their excessive jurisdictional reach, applying far beyond corporations chartered in the state. In response to the Supreme Court decision, states hesitantly began a second wave of anti-takeover statutes which tried to deal with some of the constitutionality issues. To the surprise of many involved, these statutes

⁴Before the 1960s, the primary method of hostile takeover was the proxy fight which was regulated by the Securities Act of 1933 and the proxy rules of the Securities Exchange Act of 1934. Cash tender offers, however, escaped regulation until the Williams Act.

were declared to be constitutional by the Supreme Court in 1987 (*CTS v. Dynamics Corp.*).⁵ This decision triggered a third generation of even more stringent state laws regulating takeovers.

The second and third generation statutes are of three general types: (i) Control Share Acquisition (CSA), (ii) Fair Price (FP) and (iii) Business Combination (BC).⁶ CSAs give noninterested shareholders the right to decide whether a large shareholder has any voting right. The acquirer of a certain threshold percentage of shares outstanding must request a vote of the non-interested shareholders and retains voting rights only if a majority of them approve. CSAs impede takeover by hindering a raider in a proxy fight. FPs require shareholders acquiring beyond a threshold level to pay a “fair price” for all stocks acquired unless the board approves otherwise.⁷ FPs impede takeovers because they put limits on two tier offers commonly used by raiders. BCs impose a moratorium (3 to 5 years) on specified transactions between the target and a raider holding a certain threshold percentage of stock unless the board votes otherwise.⁸ BCs impede highly leveraged takeovers, a trademark of the 1980s, since these are financed by selling some of the target’s assets. We delay discussion on the relative stringency of these laws until Section 2.3.

The history of these laws provides one advantage for our analysis. Since constitutionality was an important concern, many states imitated existing statutes leading to laws that varied on a fixed set of dimensions. This makes laws in different states roughly comparable, allowing us to pool together the laws on the basis of type.

⁵First generation laws were declared unconstitutional because they violated the commerce clause and to a lesser extent the supremacy clause of the U.S. Constitution. The second generation laws were deemed constitutional primarily because they restricted the jurisdiction of the laws to only firms incorporated in that state. With this precedent in place, challenges to third generation laws never reached the Supreme Court, even though they were much more stringent in practice.

⁶Less common types of statutes were passed by a few states, but we do not consider them here.

⁷Fair prices are usually defined as some function of the highest price paid to any shareholder (for some time period) prior to the takeover announcement.

⁸Specified transactions include sale of assets, mergers and business relationships between raider and target. Thresholds are typically set at 20%, 33.3%, or 50% for CSAs, 10% for FPs, and 10% as well for BCs. There is, however, a little state to state to variation in these thresholds.

2.2 Impact of Laws

Anecdotal evidence on the importance of the state anti-takeover laws is plentiful. A mass of cases often followed each law where raiders attempted to argue against the law.⁹ This indicates that target companies understood the laws well enough to use them as defenses and that raiders felt the laws as a large enough deterrent to success to challenge them in court. Moreover, these laws received extensive coverage by both the popular press and legal practitioners.

Empirical work on the laws typically falls under two categories: studies of their impact on takeovers and studies of their impact on stock prices. Perhaps because the data is less easily available, we know of only two studies that examine the impact of these laws on the number of takeovers. Hackl and Testani (1988) perform a straightforward differences-in-differences analysis for laws up to 1988 and find that these laws lessen takeover activity. States passing laws experienced approximately 48% smaller rise in takeover attempts in this period. They also find that the proportion of takeover attempts using tender offers went down, as well as the number of tender offer attempts that were successful. On the other hand, Comment and Schwert (1995) find little evidence that anti-takeover laws reduced the frequency of takeover activities though they do find that takeover premia went up after the passage of these laws.

Several papers have attempted to establish the effect of these laws on stock prices.¹⁰ Most papers focus on a single law using an event study methodology. Many of these papers find significant negative share price effects, some find insignificant negative share price effects, and some find no

⁹New Jersey's law, for example, was tried in *Bilzerian Partners, Ltd. v. Singer Co*, No. 87-4363 (D.N.J. Dec.2, 1987). Delaware's law was immediately challenged in *Black & Decker Corp. v. American Standard Inc.*, 679 F. Supp. 422 (D.Del. 1988) and *CRTF Corp. v. Federated Dept. Stores, Inc.*, 683 F. Supp. 422 (S.D.N.Y. 1988). These are only a few of the many cases revolving around these laws. Courts consistently found the laws applicable. See Matheson and Olson (1991).

¹⁰See for example, Karpoff and Malatesta (1989), Pound (1987), Szewczyk and Tsetsekos (1992), Romano (1987), Margotta et. al. (1990), Schumann (1989), and Block et. al. (1986).

share price effect at all. The main difficulty is in choosing the date at which the effect of these laws should be impounded into prices since information about the legislation can be incorporated into expectations and stock prices before it is formally revealed. Some papers use dates of law passage, some use press announcements, and some use dates of law introduction. As a rule, the papers that find the most negative impacts on stock price use press announcements.¹¹ Others use time averages of price for years after the law rather than looking for a treatment date. Choosing specific announcement dates biases coefficients towards zero because information about passage might have leaked out before the passage and expectations about passage might already be incorporated into prices. Using time averages, on the other hand, reduces power because of the high variability of stock prices. For us, the problem of choosing a treatment date is less problematic since CEO pay is reported and decided upon on an annual basis.

Easterbrook and Fischel (1991) summarize the literature on stock price reactions up to that point. They argue that on average the value of firms covered by these laws fell by .5%. In dollar terms, these are quite large losses. Applied to the entire New York Stock Exchange, they imply a loss of \$10 to \$20 billion dollars. Moreover, Easterbrook and Fischel likely underestimate the effect for our purposes since we focus on BC laws rather than all laws.¹² The evidence to date suggests the quantitative importance of these laws to both takeovers and share prices.

2.3 Relative Stringency of Laws

While the evidence so far indicates that as a group these laws had impacts, more reasoning and evidence is required to discern which specific laws had the largest impacts. Since we wish to proxy for changes in the incentive structure facing management, we need laws that hinder takeovers

¹¹See Karpoff and Malatesta (1989), Pound (1987), and Szewczyk and Tsetsekos (1992).

¹²See Section 2.3.

aimed at disciplining management. A primary distinction between these laws is who has the right to “veto” a takeover. In CSAs, shareholders retain the right to block out a large shareholder since they are the ones who vote on whether a large acquirer gets voting rights. While CSAs may deter takeovers because of the transactions costs they impose, they potentially solve the collective action problems that any raider faces by forcing the shareholders to vote, in essence, on the takeover.¹³ Most importantly, they likely have small effects on takeovers aimed at disciplining management since it is unclear why shareholders would vote against such a move.¹⁴ FPs and BCs in contrast will likely have stronger effects on disciplinary takeovers by placing in the directors’ hands the right to refuse a takeover.¹⁵ Since incumbent management greatly influences the board, BCs and FPs grant management a great deal of control regarding the success of takeovers.¹⁶

Empirical work on share price reactions to these laws support the idea that FPs and BCs are detrimental to managerial performance and hence share value while CSAs are not. Karpoff and Malatesta (1989) examine stock price reactions to all laws passed before 1987. They choose the effective date to be the first date on which they find a press announcement for the law. Their study is unique in comprehensively analyzing each type of law. They find significant negative reactions to the passage of BCs, resulting in a loss of value of approximately $-.467\%$. This is likely an

¹³See Grossman and Hart (1980).

¹⁴Moreover, one technique used by entrenched management to escape takeover, avoiding a proxy vote, is now weakened. For example, “Many corporate lawyers expressed concern that control share statutes, instead of protecting against abusing takeovers, actually facilitate takeovers by providing a mechanism for a mandatory shareholder vote, which, together with the resulting publicity, provides an inexpensive and simple mechanism for putting a company into play.” (Sroufe and Gelband, 1990, p. 897)

¹⁵Sometimes, *both* the board and shareholder approval are required in the FPs.

¹⁶The legal rulings on these laws generally reflect the idea that CSAs do not change the balance of power between management and shareholders, while FPs and especially BCs change the balance in favor of management. In the initial *CTS v. Dynamics Corp.* ruling on the constitutionality of CSA legislation, the court carefully weighed neutrality of the statute, concerned that the Williams act mandated shareholder management and bidder management neutrality. In contrast, in *Amanda Acquisition Corp. v. Universal Food Corp.*, a landmark case on BC legislation, the court ruled that while BCs did indeed violate neutrality, the Williams Act did not mandate it. Justice Schwartz, deciding on the Delaware BC law, concluded that it altered the balance of power between management and raider, “perhaps significantly.” See Sroufe and Gelband (1990). One commentator noted that one implication of the Wisconsin decision was that “The Seventh Circuit’s Amanda opinion asserts that a law, such as Wisconsin’s business combination statute, can be both economic folly and constitutional” (New York Law Journal, September 14, 1989).

underestimate because of the difficulty in choosing an effective date, as discussed earlier. They find less negative ($-.274\%$) and insignificant responses to FPs. Finally, they find no reaction to the adoption of CSAs. This is the most comprehensive and careful stock price to date. It conforms well to *a priori* reasoning and further tells us that BCs had more impact than FPs.

The genealogy of these laws supports this ordering. Early second generation statutes were by and large CSAs, with some states passing FPs. When constitutionality of the second generation was established, states became more aggressive. Following the example of New York, third generation laws were often BCs and some FPs. CSAs and to some extent FPs represented the early incursion of states into tender offer regulation. On the other hand, BCs and again to some extent FPs represented mature regulatory activity with heightened stringency.

3 Data

Our initial CEO compensation data consists of 792 different corporations over the 1984-1991 period.¹⁷ Compensation data was collected from the corporations' SEC Proxy, 10-K, and 8-K filings. Other data was transcribed from the *Forbes* magazine annual survey of CEO compensation as well as from SEC Registration statements, firms' Annual Reports, direct correspondence with firms, press reports of CEO hires and departures, and stock prices published by Standard & Poor's. Firms were selected into the sample on the basis of their *Forbes* rankings. *Forbes* magazine publishes annual rankings of the top 500 firms on four dimensions: sales, profits, assets and market value. To qualify for the sample a corporation must appear in one of these *Forbes* 500 rankings at least four times between 1984 and 1991. In addition, the corporation must have been publicly

¹⁷The data set was graciously made available to us by David Yermack and Andrei Shleifer. It is extensively described in Yermack (1995).

traded for four consecutive years between 1984 and 1991. Yermack's data has exceptionally high quality compensation information. Much of the CEO literature uses the *Forbes* data. One problem with that data, not present in Yermack's, is the miscoding of options. *Forbes* includes in the annual compensation the value of options *exercised* in that year. Instead, one would want the value of options *granted* in that year. Yermack's data provides this.

Our empirical test relies on knowing a firm's state of incorporation.¹⁸ Since this information is not available in Yermack's CEO data set, we matched the data to COMPUSTAT. The matching was done using company names since our version of the original data set did not contain CUSIP numbers. The matching process eliminates some firms, most often because the state of incorporation was not available or because, in a few cases, the firm name could not be unambiguously matched to one in COMPUSTAT. We are left with 611 corporations over the sample period, 1984-1991. Firm births, deaths and missing data translate this into 4,566 data points for most regressions. Missing performance measures sometimes lead to fewer observations in the pay for performance regressions.¹⁹

Table 1 presents means of the variables of interest and compares treatment and control groups. Total CEO Compensation is the sum of salary and bonus, value of options *granted* in that year and other compensation which includes stock awards, fringe benefits, and cash payouts from long term compensation plans, all in thousands of 1991 dollars.²⁰ Assets is the total start of year assets in

¹⁸Ideally, we would like state of incorporation at some time before the laws were passed, but we only have available to us the state of incorporation in 1994. Anecdotal evidence indicates that changes in state of incorporation are quite rare, especially for the very large companies in our sample. See Romano (1993). To further verify this, we randomly sampled 75 firms from our sample and checked, using Moody's Industrial Manual, whether they had changed state of incorporation in our sample period. Consistent with previous evidence, we found only three changes in state of incorporation and these were all to Delaware (two in 1985 and one in 1986), thus predating the 1988 law change.

¹⁹We also investigated the impact of dropping utilities, financial firms and other firms from regulated industries. The magnitude of the point estimates for both mean pay and pay for performance increased.

²⁰Options are valued using the Black-Scholes formula. This potentially understates the value of an option to the CEO since he or she may be able to use inside information to better time their exercises.

millions, Employment is total employment in thousands as reported in COMPUSTAT, Book Value is the total book value of the firm (total assets minus liabilities), and Market Value is the stock market value of the firm, both in millions.²¹ Age is the age of the CEO, Tenure refers to number of years as CEO. Each firm is assigned to one or more of the groups (BC, FP, CSA, NBC) based on state of incorporation. The state of incorporation, derived from COMPUSTAT, refers to the state of incorporation in 1994 if the company was still alive or the last state of incorporation if the company died before 1994. BC, FP, and CSA refer to the *set* of firms incorporated in states passing (by 1990) Business Combination laws, Fair Price laws and Control Share Acquisition laws respectively.²² NBC refers to the set of firms incorporated in states passing no Business Combination laws. It is clear from Table 1 that many of the firms in our sample are located in states passing laws. As we describe more carefully in Section 4, this does not cause our control group to be too small. Since the states *staggered* their passage of laws, our control group for any given year is the set of states not passing laws in that year. We present our summary statistics in logs since almost all our regressions are in logs. To gain an understanding of the characteristics of our sample, we discuss the raw numbers. The mean CEO compensation in our sample is \$1,607,204 and the median \$1,097,527. Mean assets for these firms are \$9,758 million, and median assets are \$3,650 million. The mean firm employs 29,500 people while the median firm employs 12,900. These numbers obviously show that the firms in our sample are quite large.

The table does make clear that the BC firms are bigger on average than firms in the full sample. They have more employees, more assets and pay their CEOs more. The differences are not large in logs. They are potentially larger in levels. By using fixed effects, our empirical methodology will of course deal with any fixed differences between BC and non-BC firms. Other problems potentially

²¹All nominal values are deflated using the CPI (1991=100).

²²See Table 2 for a list.

arise from time-varying differences correlated with passage of laws. We discuss how we deal with these problems in the next sections.

4 Empirical Methodology

We implement our test using a *differences-in-differences* methodology now common in labor economics and similar to the event study methodology in finance.²³ This approach can be easily understood with an example. Suppose we wish to estimate the effect of the Pennsylvania law passed in 1989 on the pay of the CEOs of firms incorporated in Pennsylvania. We would difference average CEO pay before and after 1989 for Pennsylvania firms. However, other things in 1989, such as a recession, may have affected Pennsylvania firms and CEO outcome in these firms. Choosing a control state, for example New Jersey, would help control for changing economic conditions. If New Jersey firms were also subject to this recession, the change in pay for their CEOs would be a measure of the severity of that recession. We would, therefore, compare the difference in CEO pay in Pennsylvania, before and after 1989, to the difference in New Jersey, before and after 1989. The difference of these two differences would serve as our estimate of the laws' effect.

In practice, we compute this estimate in a regression framework in order to control for changing observables. One could then derive the mean pay effect of the laws by estimating the following equation:

$$\log(Comp_{it}) = a + bX_{it} + cTreatment_i + dAfter_t + \delta Treatment_i * After_t + \epsilon_{it} \quad (1)$$

where i indexes firms, t indexes time, $\log(Comp_{it})$ is the logarithm of total CEO compensation, X_{it} is a vector of control variables such as log assets, log employment and CEO characteristics, $Treatment_i$ is a dummy variable for treatment firm (1 if firm i is incorporated in a state that

²³See Heckman and Hotz (1989) or Gruber (1994) for a clear exposition of differences-in-differences.

passes anti-takeover legislation), $After_t$ is a dummy variable for after the law (1 if the law has been passed by time t). Here $Treatment_i$ and $After_t$ respectively pick up any fixed difference between treatment and control and any common shock contemporaneous with the law.²⁴ Our estimate of the law's effect on mean CEO pay is δ , the coefficient on the interaction term: change in outcomes specific to states passing legislation at the time of the legislation.²⁵

Two issues must still be dealt with before estimating this regression. First, the specification does not fully exploit the panel nature of our data set. Second, by assuming a common $After_t$ dummy across firms, we restrict ourselves to laws passed in the same year. In practice, the laws were passed in different years. To deal with the first issue, we extend the specification to include firm fixed effects. To deal with the second, we allow for laws to be passed at different times. The non-interacted $After_t$ dummy, therefore, would be replaced by a set of $After_t$ dummies, one for each year in which a law is passed. More generally, we include year dummies for all the years to better control for aggregate conditions. Finally, since each firm faces a different treatment date, the $After_t$ dummy in the interaction term is replaced by a firm specific $After_{it}$ dummy:

$$\log(Comp_{it}) = \alpha_t + \beta_i + \gamma X_{it} + \delta Treatment_i * After_{it} + \epsilon_{it} \quad (2)$$

where we drop $Treatment_i$ because the firm fixed effect makes it redundant.

One important implication of staggered passage dates is that we no longer need our control group to be states that do not pass laws. The above specification can be estimated even if all states eventually passed a law. It implicitly takes as the control group for a law at time t all firms incorporated in states not passing a law at time t , even if they have already passed one or will pass one later. This is especially important for our work using BC laws. While almost every state

²⁴In practice, $After_t$ allows for a one year delay in the law's effect. This allows for lags in implementation as well as for the fact that some fiscal data may reflect results from the previous calendar year.

²⁵Some of the states gave firms the choice to opt out of coverage under the law. We choose not to use this information. Since a firm's choice to opt out is endogenous, excluding firms that opt out can induce a sample selection bias.

eventually adopts a BC law (California and Texas being the most notable exceptions), they adopt them at different times.²⁶ For a list of state and year of enactment for the three different laws, see Table 2.²⁷

By analogy, the basic specification we use to estimate pay for performance effect is as follows:

$$\log(Comp_{it}) = \alpha_t + \beta_i + \gamma X_{it} + \delta BC_i * After_{it} + \psi_t Perf_{it} + \phi BC_i * Perf_{it} + \theta BC_i * After_{it} * Perf_{it} + \epsilon_{it} \quad (3)$$

where $Perf_{it}$ is some performance measure for firm i in period t and all the other variables are defined as above. Note that this specification controls for fixed differences in pay for performance between states passing and not ($BC_i * Perf_{it}$) and differences in pay for performance over time ($\psi_t * Perf_{it}$). In this framework, δ is the estimate of the mean pay effect, and θ is the estimate of the laws' effect on pay for performance.

5 Results

5.1 Effects on Mean Pay

Table 3 estimates equation (2). We control for CEO characteristics with CEO tenure, tenure squared, age and age squared. We also include log employment and log assets. The coefficients on the CEO controls are intuitive. These estimates show positive returns to tenure and total labor market experience. In the presence of firm fixed effects, it is no surprise that the significance of the coefficient on log assets disappears. However, even when controlling for firm fixed effects, short-run movements in employment seem to impact CEO pay.

²⁶The proportion of sample points in a year that are covered by BC legislation is: 0% (1984), 0% (1985), 8% (1986), 15% (1987), 18% (1988), 69%(1989), 83%(1990), 90%(1991).

²⁷As one can see in that list, Delaware is a BC firm. Since many firms are incorporated in Delaware, one might worry that this one law drives our results. When we run our regressions excluding Delaware, our standard errors increase due to the reduced sample size, but the point estimates stay qualitatively the same.

The estimated effects of the laws are in concordance with Karpoff and Malatesta (1989) and our a priori reasoning. There are positive and significant rises in CEO pay following BC laws. Neither CSA, nor FP produce significant effects on CEO pay, though FP produces larger point estimates. The same picture emerges in column (4) where the effect of the laws are estimated simultaneously.²⁸ We find that the BC laws lead to a 4 – 5% increase in mean pay. Note that when we decompose pay (in unreported regressions), we find that most of this increase in pay following a BC law happens is via increased awards of stock options. The point estimates on all three components of pay (option grants, salary and bonus, and other forms of compensation) are positive, but it is the option component that is the largest and most significant.²⁹

5.1.1 Robustness Checks

We focus on the BC laws in the rest of the paper. Table 4 investigates the robustness of our estimates on the effects of the BC laws. In this table, we allow the returns to observables to vary by year. This serves as an important specification check. One might be concerned that our results are driven by shocks contemporaneous with the law that differentially affect firms with more assets or more employment and so on. For example, we know that BC firms are larger and the effects of firm size on CEO pay may be rising over time. Allowing the coefficient on size and other variables to vary with time addresses such concerns. If these effects corrupted our results, we would expect the estimated treatment effect to diminish when we allow the effects of covariates to change over time.

In Columns (1)-(5), we allow for interactions of assets, employment, book value, market value,

²⁸As discussed earlier, we ran some specifications with interaction terms between the laws, but none of these terms were significant.

²⁹By increase in options, recall that we mean an increase in the value of the options *granted* according to the Black-Scholes formula.

and experience as CEO, with year dummies. Our estimates of the effects of BC laws are insensitive to these interactions. We still find approximately a 5% effect for the BC laws. Column (6) allows the returns to all the variables—assets, book value, market value, and experience as CEO—to vary over time.³⁰ More importantly, it includes a trend term specific to states passing BC laws. Once again, the coefficient is not affected by this inclusion. As we discuss in Section 5.1.2, this provides some evidence that our estimates are not polluted by the endogeneity of the laws. If passage of the laws were the result of changing conditions in BC states, they should be reflected to some extent in differential trends for these states. Allowing for different pre-existing trends should lower the coefficient, contrary to what we find.

5.1.2 Political Economy and Endogeneity Checks

Up to this point, we have taken these laws as exogenous. In practice, they might be the result of changing economic conditions and such changes might be correlated with CEO compensation.³¹ In this section, we investigate whether political economic considerations bias our results. We use two techniques in this investigation: (1) we see the effects of including a treatment trend in our regression—a trend specific to states passing legislation; and (2) we introduce leads to see if the “effect” of the laws occurred before their passage.

Romano (1987) investigates the political context in which some of the anti-takeover laws were passed. She uses the Connecticut laws as a case study. She concludes: “The spur behind the passage of the Connecticut statute was not a broad-based political coalition. Rather, the bill was promoted by a corporation incorporated in Connecticut, the Aetna Life and Casualty Insurance

³⁰When we allow all the returns to vary and do not include a trend term, we continue to find the same estimates.

³¹Besley and Case (1994) for example strongly argue the point that basing tests on state legislation can bias results since the passage of laws is often correlated with current and future economic conditions.

Company (Aetna), which enlisted the support of the most important business association in the state, the Connecticut Business and Industry Association (CBIA)” (Romano, 1987, pp. 122-123). In many cases, the bills were lobbied for even more exclusively. The Arizona statute, for example, was called the “Greyhound Bill” since it was all but written by Greyhound executives.³² Typically, as was the case with Greyhound, the corporation lobbying in favor of the law perceived a current takeover threat. In fact, many of these laws were passed in emergency sessions. These case studies raise the possibility that political economy plays an important role in the passage of those laws. Two pieces of evidence tend to show that the laws are nonetheless exogenous with respect to CEO pay.

First, if underlying trends give rise to these laws, we would expect our estimates to drop when we explicitly allow for trends in legislating states. Such trend terms would likely capture some of the pre existing trends in these states. As we have already shown in Table 4, adding such trends does not change our coefficients in the mean pay regressions.

Second, if we feel short term changes in economic conditions give rise to these laws, we would expect to find some “effect” of the laws prior to passage. Table 5 estimates the following regression:

$$\log(Comp_{it}) = \alpha_t + \beta_i + \gamma X_{it} + \delta BC_I * After_{it} + \delta_0 BC_I * Before_{it}^0 + \delta_{-1} BC_I * Before_{it}^{-1} + \epsilon_{it}$$

which includes two $Before_{it}$ terms to capture leads. $Before_{it}^0$ is a dummy for the year the law passed and $Before_{it}^{-1}$ is a dummy for the year before the law passed. Table 5 demonstrates that neither of the lead terms is significant. They are both quantitatively small and statistically insignificant. We also break apart the $After$ dummy into dynamic terms where $After_{it}^s$ refers to a dummy for s years after the law and $After_{it}^{>s}$ refers to a dummy that is one if the law was passed more than s years ago. From these regressions, it is clear that the laws take place rather quickly

³² “Greyhound said, ‘Jump,’ and we said, ‘How high?’” said state Rep. Jim Skelly, Chairman of the House Judiciary Committee in Arizona (Los Angeles Times, September 15, 1987).

after their enactment. We received some hint of this in the cases discussed in Section 2. Many of those cases were filed as soon as the laws were passed, indicating the speed of reactions.

In conclusion, we find no evidence that our results are driven by endogeneity of the laws. Inclusion of treatment trends does not change the coefficient. Similarly, we find no effect of the laws before the laws were actually passed. Our results demonstrate that the reduction in a threat of an hostile takeover that followed the passage of BC statutes led to a about a 5% increase in CEO total compensation. This is consistent with the view that CEOs set their own pay, skimming what they can from relatively powerless shareholders. Under this theory, the decreased threat of a takeover further weakens shareholders, thereby increasing the amount CEOs can skim from them. We next investigate the role that large shareholders might play in limiting the rise in skimming.

5.2 The Role of Large Shareholders

5.2.1 Effects on Mean Pay

By monitoring firms more effectively, large shareholders are often thought to be a good governance mechanism (Shleifer and Vishny, 1986). In fact, takeover threat and large shareholders may be substitutes: the presence of a large shareholder that monitors managerial actions makes takeover discipline less necessary. Under that view, one might expect that the rise in skimming and increase in CEO pay that takes place after the passage of the BC laws will be weaker for companies that have at least one large shareholder. We test this prediction in Table 6.

For the purpose of this test, we split our original data set into two subgroups. For each company in the original sample, we compute the number of holders of a block of at least five percent of common share at the beginning of the sample period (1984). We include in the calculation blockholders that are directors but also blockholders that are not represented on the board. We

exclude from the calculation large blocks that are owned by the CEO herself. We then separate companies that have at least one large shareholder prior to the takeover laws (column (1) in Table 6) and companies that are not monitored by a large shareholder (column (2) in Table 6). Note that we concentrate on the number of large shareholders at the beginning of the sample period in order to avoid future changes in ownership structure that may be endogenous to the passage of the laws.

The regressions in Table 6 replicate column (1) of Table 3. The results strikingly support our original prediction.³³ The increase in total CEO compensation among the companies with large shareholders is only about 2.5% is not statistically different from 0. On the other hand, companies that do not benefit from the monitoring of a large shareholder witness an increase in CEO compensation of 7.5% after the passage of the BC statutes.

5.2.2 Effects on Pay for Performance

The increase in mean pay documented in Section 5.1 is hard to reconcile with an optimal contracting model of CEO pay. Because the takeover legislation reduces the threat of an hostile takeover and the probability of a costly job loss, risk-averse CEOs need less of a compensating differential. Average CEO pay should thus go *down*, not up, after the passage of the laws.

In the optimal contracting theory, however, part of the decline in the risk of a job loss might have been matched by an increase in the sensitivity of CEO pay to firm performance. This might happen if takeover discipline and pay-for-performance are substitute mechanisms for corporate governance. In that case, the weakening of the takeover mechanism will lead principals to rely more extensively on pay for performance. Such an increase in the correlation between CEO compensation and firm performance might then ask for less of a reduction in mean pay than is implied by a pure decline in

³³In regressions not reported here, we have verified the robustness of these findings along the lines discussed in sections 5.1.1 and 5.1.2.

takeover threat. We investigate these effects in Table 7, both for the entire sample and separately for firms with and without large shareholders.

The regressions estimated in Table 7 are based on equation (3). We consider two different performance measures. We measure accounting performance by the ratio of net income over total assets. We also consider the logarithm of shareholder wealth as a measure of financial performance. In the regressions presented in Table 7, both of these performance measures have been demeaned.

Column (1) in Table 7 shows that the sensitivity of pay to accounting rate of return went up after the legislation.³⁴ Compared to the base year (1984), the incentive slope is about 30% bigger. There is however no sign of an increase in the sensitivity of pay to financial performance (column (2)).

In the next four columns, we reestimate these pay for performance regressions separately for firms with and without at least one large shareholder at the beginning of the sample period. While neither group of firms experience a change in the sensitivity of CEO pay to financial performance (columns (5) and (6)), the findings on accounting performance are very interesting. We find that most of the increase in incentive slope documented in column (1) is driven by the group of firms with a large shareholder (column (3)). The change in pay for performance is much smaller and not statistically significant among the firms that do not have a larger shareholder.

These last results, in combination with the results of Table 6, lead us to draw a sharp distinction between the two types of firms. In the absence of large shareholders, skimming seems to be the best model to describe the determination of CEO compensation. As protection from the takeover market increases, CEOs increase their expropriation of the now even weaker shareholders

³⁴In regressions not reported here, we have found that these findings are broadly robust to allowing the returns to assets, employment, and CEO tenure to change over time and also to including a trend term specific to states passing BC laws.

and pay themselves higher compensation. The passiveness of shareholders in this case is further reflected by the lack of a significant increase in pay for performance sensitivity after the anti-takeover laws are passed. In the presence of large shareholders, however, mean CEO pay does not significantly increase after the legislative changes and there is some evidence of an increase in the sensitivity of pay to accounting performance. In other words, large shareholders limit the extent of the expropriation and also actively react to the weakening of the takeover disciplining device by strengthening the link between pay and performance, as would be predicted by optimal contracting theory.

6 Concluding Remarks

Current views on the link between CEO pay and takeover threat are conflicting. This results, we believe, from the lack in previous work of a clear source of variation in takeover probabilities across firms. Because the test implemented here exploits a plausibly exogenous decline in takeover market discipline, it pushes this debate one step further. More generally, this paper informs us about the role of corporate governance mechanisms in the determination of CEO compensation.

We have provided some evidence that state anti-takeover laws on average raised the total compensation for CEOs. This finding is consistent with the view that CEOs expropriate what they can from relatively powerless shareholders and pay themselves more when takeover discipline goes down and shareholders are further weakened. It is inconsistent with the view that the average shareholder in a large US corporation acts as an optimal principal. This finding confirms that the skimming view of CEO compensation, made popular by writers such as Crystal (1991), will deserve more attention by future researchers.

We have also shown that the presence of large shareholders in a company substantially affects the determination of CEO compensation. More specifically, large shareholders help limit the rise in mean pay after the passage of the takeover legislation. They also seem to introduce stronger pay for performance incentives to compensate for the weakened market discipline. This last set of findings suggest that the optimal contracting view of CEO compensation, which has been the one favored by economists for many years, may be of more relevance for firms with large shareholders that are ready to behave like active principals.

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Table 1: Summary Statistics^a

| | All | BC | FP | CSA | NBC |
|------------------------|----------------|----------------|----------------|----------------|----------------|
| Total Compensation | 1098 (3443) | 1665 (3618) | 1321 (1310) | 1298 (1974) | 1166 (1443) |
| Log Total Compensation | 7.03 (.768) | 7.06 (.755) | 6.89 (.733) | 6.82 (.730) | 6.71 (.786) |
| Log Total Assets | 8.28 (1.27) | 8.28 (1.29) | 8.26 (1.19) | 8.31 (1.12) | 8.28 (1.11) |
| Log Employment | 2.53 (1.33) | 2.57 (1.35) | 2.45 (1.30) | 2.25 (1.39) | 2.19 (1.14) |
| Log Market Value | 7.33 (1.20) | 7.38 (1.19) | 7.23 (1.20) | 7.03 (1.17) | 7.05 (1.22) |
| Log Book Value | 6.81 (1.12) | 6.83 (1.13) | 6.74 (1.11) | 6.59 (1.05) | 6.62 (1.05) |
| Age of CEO | 57.7 (6.8) | 57.8 (6.76) | 57.8 (6.6) | 56.9 (6.4) | 57.0 (7.04) |
| Tenure of CEO | 9.2 (8.11) | 9.2 (8.3) | 9.0 (8.1) | 8.2 (6.8) | 9.1 (6.6) |
| Sample Size | 4566 | 4040 | 1927 | 1147 | 526 |

^aNotes:

1. Total CEO Compensation is the sum of salary and bonus, value of options granted and other compensation which includes stock awards, fringe benefits, and cash payouts from long term compensation plans, all in thousands. Log Assets is the log of total start of year assets in millions, Log Employment is log of total employment in thousands, Log Book Value is the total book value of the firm (total assets minus liabilities), Log Market Value is the market value of the firm, both in millions. All numbers are deflated using the CPI (1991=100).
2. Standard Deviation in parenthesis.
3. BC, FP, CSA, and NBC are respectively the set of firms incorporated in states passing a BC statute, a FP statute, a CSA statute and no BC statute.

Table 2: State Anti-Takeover Legislation^a

| Business Combination | Fair Price | Control Share Acquisition |
|-----------------------------|-----------------------|----------------------------------|
| Arizona (1987) | Arizona (1987) | Arizona (1987) |
| Connecticut (1989) | Connecticut (1984) | Hawaii (1985) |
| Delaware (1988) | Georgia (1985) | Idaho (1988) |
| Georgia (1988) | Idaho (1988) | Indiana (1986) |
| Idaho (1988) | Illinois (1984) | Kansas (1988) |
| Illinois (1989) | Indiana (1986) | Louisiana (1987) |
| Indiana (1986) | Kentucky (1989) | Maryland (1988) |
| Kansas (1989) | Louisiana (1985) | Massachusetts (1987) |
| Kentucky (1987) | Maryland (1983) | Michigan (1988) |
| Maine (1988) | Michigan (1984) | Minnesota (1984) |
| Maryland (1989) | Mississippi (1985) | Mississippi (1991) |
| Massachusetts (1989) | Missouri (1986) | Missouri (1984) |
| Michigan (1989) | New Jersey (1986) | Nebraska (1988) |
| Minnesota (1987) | New York (1985) | Nevada (1987) |
| Missouri (1986) | North Carolina (1987) | North Carolina (1987) |
| Nebraska (1988) | Ohio (1990) | Oklahoma (1987) |
| New Jersey (1986) | Pennsylvania (1989) | Oregon (1987) |
| New York (1985) | South Carolina (1988) | Pennsylvania (1989) |
| Ohio (1990) | South Dakota (1990) | South Carolina (1988) |
| Pennsylvania (1989) | Tennessee (1988) | South Dakota (1990) |
| Rhode Island (1990) | Virginia (1985) | Tennessee (1988) |
| South Carolina (1988) | Washington (1990) | Utah (1987) |
| South Dakota (1990) | Wisconsin (1985) | Virginia (1988) |
| Tennessee (1988) | | Wisconsin (1991) |
| Virginia (1988) | | Wyoming (1990) |
| Washington (1990) | | |
| Wisconsin (1987) | | |
| Wyoming (1989) | | |

^aSource: *Annotated State Codes*, various states and years.

Table 3: Effects of Anti-Takeover Legislation on Mean Pay^a

| Dependent Variable: Log of Total CEO Compensation | | | | |
|---|--------------------|--------------------|--------------------|--------------------|
| | (1) | (2) | (3) | (4) |
| BC*After _t ^{BC} | .054** (.025) | — | — | .045* (.026) |
| FP*After _t ^{FP} | — | .040 (.027) | — | .031 (.030) |
| CSA*After _t ^{CSA} | — | — | -.021 (.030) | -.031 (.031) |
| Log Assets | .037 (.030) | .038 (.030) | .038 (.030) | .038 (.030) |
| Log Employment | .185**** (.032) | .183**** (.032) | .184**** (.032) | .185**** (.032) |
| Age | .038* (.020) | .038* (.020) | .037* (.020) | .038* (.020) |
| Age ² * 100 | -.035** (.017) | -.035** (.017) | -.037** (.017) | -.038** (.017) |
| Tenure | .016**** (.004) | .016**** (.004) | .016**** (.004) | .016**** (.004) |
| Tenure ² * 100 | -.046*** (.016) | -.046*** (.016) | -.046*** (.016) | -.047*** (.016) |
| Year Dummies | Yes | Yes | Yes | Yes |
| Firm F.E. | Yes | Yes | Yes | Yes |
| Adjusted R ² | .702 | .702 | .701 | .702 |
| Sample Size | 4566 | 4566 | 4566 | 4566 |

^aNotes:

1. Dependent variable is log of total CEO Compensation, which is the sum of salary and bonus, value of options granted and other compensation which includes stock awards, fringe benefits, and cash payouts from long term compensation plans, all in thousands. BC, FP and CSA, and *After_t* are defined in the text in Section 4. Log Assets is the log of total start of year assets in millions, Log Employment is log of total employment in 1000s, Age and Age² are for the age of the CEO, Tenure and Tenure² refer to number of years as CEO. The data used is described in the text.
2. * denotes significance at the 10%; ** at the 5%; *** at the 1%; **** at the .1%.

**Table 4: Effects of BC Legislation on Mean Pay:
Robustness Checks^a**

| Dependent Variable: Log Total CEO Compensation | | | | | | |
|--|------------------|------------------|------------------|------------------|------------------|------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| BC*After _t ^{BC} | .055** (.025) | .052** (.025) | .053** (.025) | .057** (.025) | .056** (.025) | .056** (.024) |
| Log Assets*YD | Yes | No | No | No | No | Yes |
| Log Employment*YD | No | Yes | No | No | No | Yes |
| Log Book Value*YD | No | No | Yes | No | No | Yes |
| Log Market Value*YD | No | No | No | Yes | No | Yes |
| Tenure*YD | No | No | No | No | Yes | Yes |
| BC*Year | No | No | No | No | No | Yes |
| Firm F.E. | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Dummies (YD) | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted R ² | .703 | .703 | .702 | .722 | .702 | .724 |
| Sample Size | 4566 | 4566 | 4566 | 4566 | 4566 | 4566 |

^aNotes:

1. Dependent variable is log of total CEO Compensation, which is the sum of salary and bonus, value of options granted and other compensation which includes stock awards, fringe benefits, and cash payouts from long term compensation plans, all in thousands. BC and *After_t* are defined in the text in Section 4. *BC * Year* is a time trend for states passing BC legislation. Log Assets is the log of total start of year assets in millions, Log Employment is log of total employment in thousands, Log Book Value is the total book value of the firm (total assets minus liabilities), Log Market Value is the market value of the firm, both in millions.
2. All regressions also include demographic controls of Age, Age², Tenure and Tenure².
3. * denotes significance at the 10%; ** at the 5%; *** at the 1%; **** at the .1%.

**Table 5: Effects of BC Legislation on Mean Pay:
Dynamics^a**

Dependent Variable: Log of Total CEO Compensation

| | (1) | (2) | (3) |
|-----------------------------------|------------------|-----------------|-----------------|
| BC* <i>Before</i> ⁻¹ | — | .003 (.028) | .003 (.028) |
| BC* <i>Before</i> ⁰ | — | -.005 (.031) | -.005 (.032) |
| BC* <i>After</i> ¹ | .055** (.027) | — | .053 (.037) |
| BC* <i>After</i> ² | .051 (.032) | — | .049 (.042) |
| BC* <i>After</i> ^{>2} | .059 (.038) | — | .056 (.051) |
| BC* <i>After</i> | — | .052 (.035) | — |
| Year Dummies | Yes | Yes | Yes |
| Firm F.E. | Yes | Yes | Yes |
| Adjusted <i>R</i> ² | .702 | .702 | .701 |
| Sample Size | 4566 | 4566 | 4566 |

^aNotes:

1. All regressions also include demographic controls of Age, Age², Tenure and Tenure² as well as as controls for log Assets and log Employment.
2. Dependent variable is log of total CEO Compensation, which is the sum of salary and bonus, value of options granted and other compensation which includes stock awards, fringe benefits, and cash payouts from long term compensation plans, all in thousands. BC is defined in the text in Section 4. *After* and *Before* dummies are defined in Section 5.1.2 Log Assets is the log of total start of year assets in millions, Log Employment is log of total employment in 1000s, Age and Age² are for the age of the CEO, Tenure and Tenure² refer to number of years as CEO. The data used is described in the text.
3. * denotes significance at the 10%; ** at the 5%; *** at the 1%; **** at the .1%.

**Table 6: Effects of BC Legislation on Mean Pay:
The Role of Large Shareholders^a**

| Dependent Variable: Log of Total CEO Compensation | | |
|---|--------------------|--------------------|
| | (1) | (2) |
| <i>Large Shareholder?</i> | <i>Yes</i> | <i>No</i> |
| BC*After _t ^{BC} | .026 (.040) | .075** (.031) |
| Log Assets | .087* (.045) | -.050 (.042) |
| Log Employment | .207**** (.051) | .157**** (.040) |
| Age | .040 (.026) | .013 (.033) |
| Age ² * 100 | -.037* (.022) | -.013 (.029) |
| Tenure | .008 (.006) | .024**** (.006) |
| Tenure ² * 100 | -.027 (.024) | -.064*** (.021) |
| Year Dummies | Yes | Yes |
| Firm F.E. | Yes | Yes |
| Adjusted R ² | .633 | .768 |
| Sample Size | 2281 | 2268 |

^aNotes:

1. "Large Shareholder" is a dummy variable that equals 1 ("Yes") if the firm has a strictly positive number of blocks of at least five percent of common shares in the base year (1984), whether the block holder is or is not a director. Blocks of at least five percent that are owned by CEOs are excluded.
2. Dependent variable is log of total CEO Compensation, which is the sum of salary and bonus, value of options granted and other compensation which includes stock awards, fringe benefits, and cash payouts from long term compensation plans, all in thousands. BC, FP and CSA, and *After_t* are defined in the text in Section 4. Log Assets is the log of total start of year assets in millions, Log Employment is log of total employment in 1000s, Age and Age² are for the age of the CEO, Tenure and Tenure² refer to number of years as CEO. The data used is described in the text.
3. * denotes significance at the 10%; ** at the 5%; *** at the 1%; **** at the .1%.

Table 7: Effects of BC Legislation on Pay for Performance^a

| Dependent Variable: Log Total CEO Compensation | | | | | | |
|--|---------------------|--------------------|---------------------------|--------------------|--------------------|---------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| <i>Sample:</i> | <i>All</i> | | <i>Large Shareholder?</i> | | | |
| | | | <i>Yes</i> | <i>No</i> | <i>Yes</i> | <i>No</i> |
| BC*After _t ^{BC} | .054** (.025) | .053** (.025) | .017 (.040) | .085*** (.030) | .020 (.040) | .072**** (.031) |
| BC*After _t ^{BC*} | .891** (.406) | — | 1.126** (.582) | .316 (.584) | — | — |
| Acc. Rate of Return | 2.657**** (.794) | — | 2.352** (1.116) | 2.533*** (1.08) | — | — |
| BC*After _t ^{BC*} | — | .002 (.021) | — | — | -.011 (.041) | -.003 (.023) |
| Ln(Sh. Wealth) | — | .277**** (.050) | — | — | .262**** (.089) | .267**** (.0615) |
| Acc. Rate of Return*YD | Yes | No | Yes | Yes | No | No |
| Ln(Sh. Wealth)*YD | No | Yes | No | No | Yes | Yes |
| Year Dummies(YD) | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm F.E. | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted R ² | .711 | .722 | .641 | .782 | .653 | .787 |
| Sample Size | 4566 | 4566 | 2281 | 2268 | 2281 | 2268 |

^aNotes:

1. All regressions include controls for log total assets, log employment, Age, Age², Tenure as CEO and Tenure².
2. "Large Shareholder" is a dummy variable that equals 1 ("Yes") if the firm has a strictly positive number of blocks of at least five percent of common shares in the base year (1984), whether the block holder is or is not a director. Blocks of at least five percent that are owned by CEOs are excluded.
3. CEO Compensation is the sum of salary and bonus, value of options granted and other compensation which includes stock awards, fringe benefits, and cash payouts from long term compensation plans, all in thousands. BC and *After_t* are defined in the text in Section 4. Accounting Rate of Return is the ratio of Net Income over Total Assets. Both performance measures, Accounting Rate of return and Ln (Shareholder Wealth), have been demeaned.
4. All regressions include performance times year dummies. The reported performance coefficients are for the base year (1984).
5. * denotes significance at the 10%; ** at the 5%; *** at the 1%; **** at the .1%.