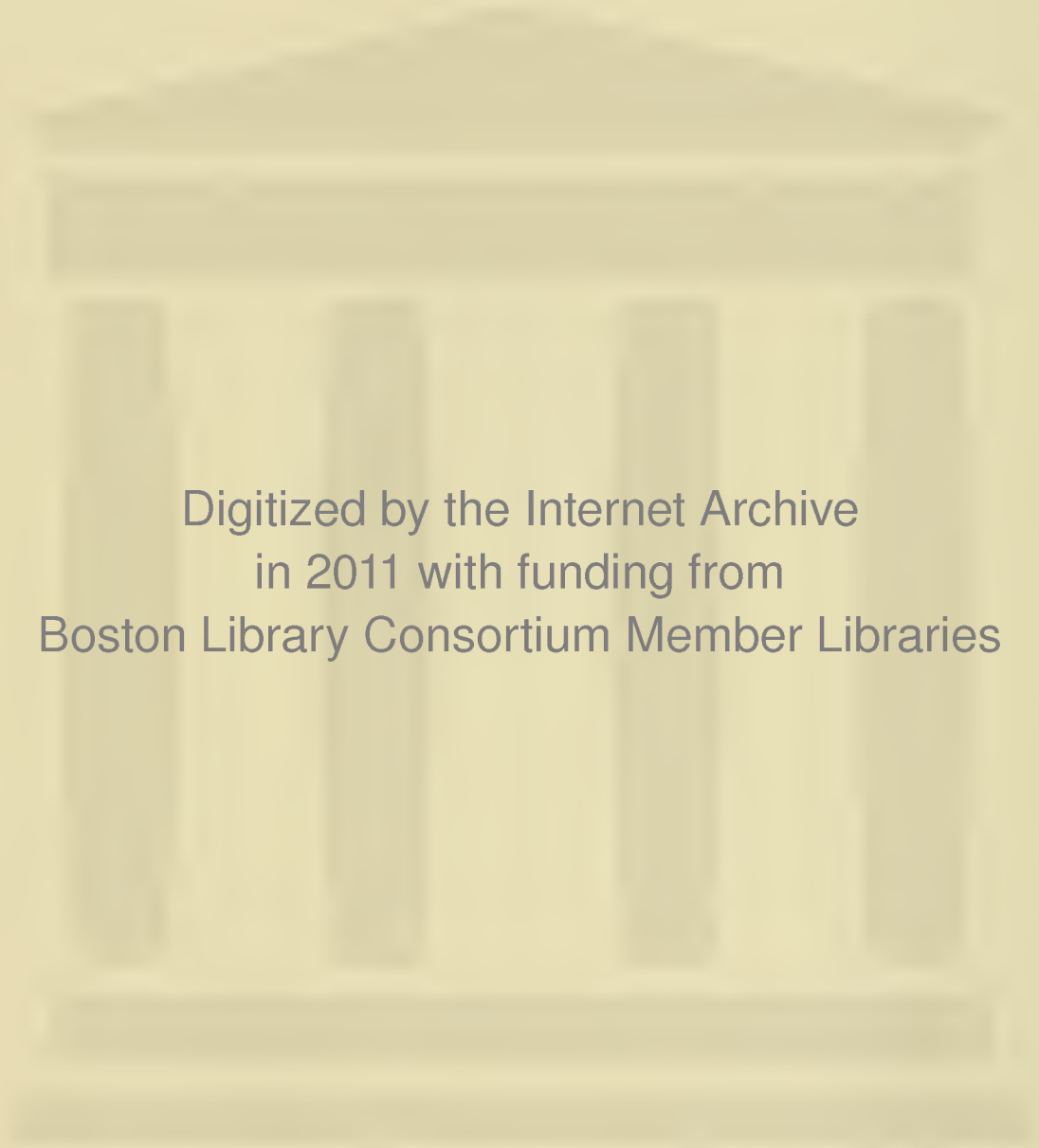

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Executive Compensation and Incentives:

The Impact of Takeover Legislation

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Abstract

We investigate the impact of changes in states' anti-takeover legislation on executive compensation. We find that both pay for performance sensitivities and mean pay increase for the firms affected by the legislation (relative to a control group). These findings are partially consistent with an optimal contracting model of CEO pay as well as with a skimming model in which reduced takeover fears allow CEOs to skim more. We compute *lower bounds* on the relative risk aversion coefficients implied by our findings. These lower bounds are relatively high, indicating that the increase in mean pay may have been more than needed to maintain CEOs' individual rationality constraints. Under both models however, our evidence shows that the increased pay for performance offsets some of the incentive reduction caused by lower takeover threats. (*JEL* D21, D80, G3, J41)

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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. They provide a natural testing ground for theories of executive pay because CEO compensation is a primary alternative method for providing incentives to CEOs. This paper empirically examines the impact of the anti-takeover legislation on executive compensation.[1]

State legislation differed in date of passage and stringency, providing two sources of variation for our empirical methodology. We use the time variation to implement a differences-in-differences methodology. 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.[3]

[1]Clearly, other corporate control mechanisms, such as leverage, board composition or executive turnover may respond to the legislation. In this paper, however, we will focus exclusively on the impact on executive compensation.

[2]In 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 III for a discussion of our empirical methodology.

[3]Two papers have independently examined the effect of takeover threats on CEO compensation. Agrawal and Knoeber (1997) proxy the threat of a takeover with the actual industry incidence of takeovers. 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. Using a matched sample, Borokhovich, Brunarski, and Parrino (1997) show that firms that adopt anti-takeover amendments have higher salaries and more valuable options grants. The key difference between our paper and the other two is our source of identification. Both of these papers rely on potentially endogenous firm decisions —firm adoption of anti-takeover amendments or golden parachutes. On the other hand, state-level legislation generates more exogenous sources of variation. Thus, while the other two papers use more straightforward but more endogenous sources of variation, we use less straightforward but less endogenous sources. In this sense, our paper complements these.

Our results identify two effects of antitakeover legislation on executive pay. Following these laws, we find (1) an increase in mean pay and (2) an increase in the pay performance correlation. CEO pay rises by approximately 5% and the pay performance sensitivity rises by between 20 and 60% for most measures of performance. The estimates are statistically significant for a variety of performance measures and the point estimates are roughly unchanged when checked for robustness. 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.[4]

Our findings are consistent with an optimal contracting model of CEO pay, wherein shareholders represent rational principals and the CEO represents the agent. In such a model, shareholders should tradeoff incentive devices to optimize total incentives.[5] If the power of one incentive device is weakened, the principal should now rely more on the other.[6] Hence, the first prediction is an increased use of explicit pay for performance. This increased pay for performance raises the variance of pay and thereby lowers the utility of a risk averse CEO. To maintain the individual rationality constraint, the principal must increase the risk free component of pay. This logic of the optimal contracting model does not extend completely to our study since takeover legislation also raises the utility of CEOs directly by lowering the risk of takeover. Hence, the prediction on mean pay becomes ambiguous because the CEO's individual rationality constraint becomes easier to satisfy. Thus, the predictions of contract theory about how a principal would respond to a weakening of

[4]See Karpoff and Malatesta (1989).

[5]Gibbons and Murphy (1992) also examine the maximization of total incentives. Proxying for career concerns with age, they find that the pay for performance correlation increases as CEOs approach retirement. The main difference between our approach and theirs is that takeover legislation provides direct variation in the strength of an incentive device, whereas many things other than career concerns vary with age.

[6]This implicitly assumes that the two mechanisms are substitutes. This is clearly based on the work by Brickley and James (1987) on the substitution hypothesis.

the takeover pressure are: (i) the sensitivity of pay to performance will increase; and (ii) the impact on mean pay is ambiguous.

An *increase* in mean pay is predicted by a model where CEOs skim higher pay. In such a model, anti-takeover legislation raises discretion by insulating management from raiders.[7] This increased discretion may allows CEOs to raise their pay to the extent that they control it. The skimming theory implies that these laws raised the welfare of CEOs. In contrast, under a contract theory interpretation, the CEO is not made better off by the laws: the change in mean pay exactly compensates for the change in risk.

To assess whether the skimming hypothesis is consistent with our empirical results, we ask if the rise in mean pay is "commensurate" with the rise in pay for performance. We compute *lower bounds* on the relative risk aversion coefficients that would be required to justify the joint increase in mean pay and incentive pay under the optimal contracting hypothesis. We can only measure the increase in risk caused by the increase in pay for performance and not the decrease in risk due to the reduced takeover threat.[8] As we overestimate the risk increase, we underestimate the relative risk aversion coefficients. Our lower bound estimates of risk aversion vary from 5 to 6.6. Many would consider these lower bounds unreasonably high.[9] Large estimates of relative risk aversion imply that the rise in mean pay was likely more than enough to compensate for the rise in risk. One can therefore plausibly argue that part of the increase in mean pay may be caused by skimming.

[7]Bertrand and Mullainathan (1996) find that anti-takeover legislation raises employee wages by 1-2%. This is consistent with takeover legislation increasing managerial discretion and managers using this discretion to pay higher wages. Of course, *net* discretion can rise in both the contracting and skimming models. In the contracting model, shareholders offset some of the increased discretion, but not necessarily all of it.

[8]The low probability of takeover incidence would make any measure of takeover threat reduction very noisy.

[9]Those who believe the equity premium estimates of relative risk aversion to be correct may not find these numbers large at all. Three points must be made. First, the estimates are lower bounds. Second, they do not include the possibility for CEOs to offset the increased risk by diversifying their wealth portfolio. As estimates of risk aversion for diversifiable risk, these numbers are large. Finally, even with all of these considerations, we recognize that some may still find these numbers reasonable and, hence, the tentative nature of our conclusions about the mean pay effect.

The skimming story can also provide an interpretation of the increased pay for performance. If CEOs can only skim when performance is high, we might expect to observe an increased pay for performance relationship.[10] Thus increased pay for performance might arise either because of a calculated response by shareholders or because of constrained skimming. However, *under either story, the CEO still faces a tighter incentive scheme.* A rise in the value of the firm still results in a larger rise in pay and the CEO's incentives for value maximization increase.[11] Hence, irrespective of the interpretation, the increased pay for performance sensitivity partially offsets the decreased disciplinary role played by takeovers.

This paper has three main findings. First, we find that mean pay rises following anti-takeover legislation. Second, we find that the relationship between pay and performance strengthens as the threat of takeovers is reduced. Finally, we argue that while these laws increased pay incentives, they may also have left CEOs better off. This paper is an important contribution to the literature because we use direct variation in the efficacy of incentive devices to test the impact of takeover threats on CEO compensation.

Section I of the paper describes the anti-takeover laws and discusses prior work on their impact. Section II describes the data set. Section III outlines our empirical methodology. Section IV presents the results. Section V discusses the potential endogeneity of these laws and the possibility that political economic forces bias our estimates. Section VI discusses the skimming interpretation more carefully. Section VII concludes.

[10] This would happen if a disproportionate amount of the skimming induced by the takeover legislation happened during high performance periods. Alternatively, if CEOs skim options as well as compensation, we would observe an increased pay for performance. The second alternative is not possible in our empirical technique since we cannot use the cumulated options grants in our pay for performance regressions. See Section III.

[11] Of course the two stories differ on whether such an increase is optimal. Evaluating this optimality is clearly beyond the scope of this paper, unless we make *ad hoc* assumptions about the importance of CEO effort.

I State Takeover Laws

I.A Description

Serious regulation of modern tender offer activity in the United States begins with the Williams act, a federal statute passed in 1968.[12] 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 were declared to be constitutional by the Supreme Court in 1987 (*CTS v. Dynamics Corp.*).[13] 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).[14] CSAs give noninterested shareholders the right to decide whether a large shareholder has any voting rights. 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

[12] Before 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.

[13] First 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.

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

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.[15] 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.[16] 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 I.C.

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.

I.B Impact of Laws

I.B.1 Evidence

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.[17] 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

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

[16] Specified 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.

[17] New 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).

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 one study that examines 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.

Several papers have attempted to establish the effect of these laws on stock prices.[18] Most papers focus on a single law using an event study methodology. Many of these papers find negative share price effects, some find insignificant negative share price effects, and some find no 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.[19] 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,

[18]See 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).

[19]See Karpoff and Malatesta (1989), Pound (1987), and Szewczyk and Tsetsekos (1992).

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.[20] The evidence to date suggests the quantitative importance of these laws to both takeovers and share prices.

I.C 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 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.[21] 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.[22] FPs and BCs in contrast

[20] See Section I.C.

[21] See Grossman and Hart (1980).

[22] Moreover, 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)

will likely have stronger effects on disciplinary takeovers by placing in the directors' hands the right to refuse a takeover.[23] Since incumbent management greatly influences the board, BCs and FPs grant management a great deal of control regarding the success of takeovers.[24]

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 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

[23] Sometimes, *both* the board and shareholder approval are required in the FPs.

[24] The 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).

represented mature regulatory activity with heightened stringency.

II Data

Our initial CEO compensation data consists of 792 different corporations over the 1984-1991 period.[25] 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 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.[26] 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

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

[26] Ideally, 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. Romano (1993) discusses some of these issues.

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.[27]

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.[28] Assets is the total start of year assets in 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.[29] 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.[30] 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

[27]We 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.

[28]Options 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.

[29]All nominal values are deflated using the CPI (1991=100).

[30]See Table 2 for a list.

more carefully in Section III, 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 arise from time-varying differences correlated with passage of laws. We discuss how we deal with these problems in the next section.

III 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.[31] By analogy with the experimental terminology, we refer to firms incorporated in states passing anti-takeover legislation as *treatment* firms and firms incorporated in other states as *control* firms. In the first level of differences, we subtract outcomes (y) before the law from outcomes after the law. Since we do this for both treatment and control, we get two sets of differences: $\Delta^T y$ for the treatment group and $\Delta^C y$ for the

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

control group. $\Delta^T y$ contains an estimate of the law's effect, but might also contain other shocks contemporaneous with the law. To control for this, we introduce a second level of differences. If contemporaneous shocks affect treatment and control groups in roughly similar ways, then those shocks should also be contained in $\Delta^C y$. One can therefore subtract $\Delta^C y$ from the first difference, $\Delta^T y$, to estimate the effect of the law.

This approach can be easily understood with an example. Suppose we wish to estimate the effect of the Pennsylvania law passed in 1989. We would difference outcomes before and after 1989 for Pennsylvania firms. However, other things in 1989, such as a recession, may have affected Pennsylvania 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 their outcomes would be a measure of the severity of that recession. We would, therefore, compare the difference in outcomes 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 law's effect.

In practice, we compute the diffs-in-diffs estimate in a regression framework in order to control for changing observables:

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

where i indexes firms, t indexes time, y_{it} is the outcome variable of interest, X_{it} represents controls, $Treatment_i$ is a dummy variable for treatment firm (1 if firm i is incorporated in a state that 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 $After_t$ pick up any fixed differences between treatment and control and any common shocks contemporaneous with the law respectively.[32] Our estimate

[32]In practice, $After_t$ allows for a one year delay in the law's effect. This allows for lags in implementation as well

of the law's effect is δ, the coefficient on the interaction term: change in outcomes specific to states passing legislation at the time of the legislation.[33]

Two problems must still be solved before estimating this regression. First, it 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 problem, 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:

$$y_{it} = \alpha_t + \beta_i + \gamma X_{it} + \delta Treatment_i * After_{it} + \epsilon_{it} \tag{2}$$

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 eventually adopts a BC law (California and Texas being the most notable exceptions), they adopt them at different times.[34] For a list of state and year of enactment for the three different laws, see Table 2.[35]

as the fact that some fiscal data may reflect results from the previous calendar year.

[33]Some 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.

[34]The 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).

[35]As 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

Two problems arise with any diffs-in-diffs estimator: (i) shocks differentially affecting the treatment group contemporaneous with the law pollute the estimates and (ii) the passage of laws at time t may be correlated with the error term in $t+1$. The first difficulty is especially severe when treatment and control are different on important dimensions. Shocks affecting those dimensions will then differentially affect treatment and control groups. For example, if treatment firms were larger, shocks affecting large firms' pay would affect treatment firms more than control firms. If by chance these shocks happen at the same time as these laws, the estimated effect of the laws would be biased. To deal with this, we allow the coefficients on a variety of observable characteristics to be different for each year. This amounts to allowing γ to vary by year. Suppose our results were driven merely by contemporaneous shocks to large firms, of which the treatment group has more. In this case, our estimates are merely picking up on the time-varying effects of size. By directly allowing the coefficient on size to vary by time, we can control for this effect. In practice, when we allow for the coefficients on observables to vary over time, we find that our estimates do not change.

The second difficulty arises from the political economy of these laws. For example, Besley and Case (1994) have argued that basing tests on state legislation can bias results since passage of laws is often correlated with current and future economic outcomes. We argue in Section V that such political economy bias is unlikely to drive our results.

The estimation procedure so far has used time and state variation. As we have discussed, anti-takeover legislation also varied in strength. To incorporate this variation, we proceed in two ways. First, we estimate equation (2) for each kind of takeover statute: BC, FP, and CSA, ignoring the

increase due to the reduced sample size, but the point estimates stay qualitatively the same.

other laws. Second, we estimate the effect of the three laws simultaneously:

$$y_{it} = \alpha_t + \beta_i + \gamma X_{it} + \delta^{BC} BC_i * After_{it}^{BC} + \delta^{FP} FP_i * After_{it}^{FP} + \delta^{CSA} CSA_i * After_{it}^{CSA} + \epsilon_{it} \quad (3)$$

where the superscript $\{BC, FP, CSA\}$ indicates type of law and *Treatment* has been replaced by BC, FP, and CSA. For example, BC_i is a dummy indicating one if i is incorporated in a state passing a business combination statute. Since states may pass several kinds of takeover legislation, the BC, FP and CSA dummies are not exclusive, nor are the $After$ dummies.[36] As discussed before, previous results and *a priori* reasoning allow us to order the laws by stringency. As a robustness check, we will verify that our estimates follow this order. We expect to find that $\delta^{BC} > \delta^{FP} > \delta^{CSA}$.

As with the stock price results presented of Karpoff and Malatesta (1989), BC laws will be the ones in our data to have significant effects. For most of our regressions, therefore, we will focus on them. We will estimate the effects of these laws on two different outcome variables: mean pay and pay for performance. Estimation of the mean effect follows directly from equation (2):

$$log(Comp_{it}) = \alpha_t + \beta_i + \gamma X_{it} + \delta BC_i * After_{it} + \epsilon_{it} \quad (4)$$

where $log(Comp_{it})$ will be the log of total CEO compensation in firm i in year t. The X_{it} will include a set of control variables such as log assets, log employment and CEO characteristics.

Estimating the effect on pay for performance is more difficult. The standard regression found in Murphy (1985) and Jensen and Murphy (1990) for estimating pay for performance relationships is:

$$Wealth_{it} = a_t + b_i + cX_{it} + dPerf_{it} + \epsilon_{it} \quad (5)$$

where $Wealth_{it}$ is CEO wealth and $Perf_{it}$ is some performance measure in *levels*, such as share-

[36] This raises the issue of whether these laws interact with each other. That is, does the effect of a CSA law depend on whether the state already has a BC law? We investigated these issues by allowing interaction terms between all pairs of laws, and even a three-level interaction. These interaction terms were all insignificant, and no consistent pattern emerged. We, therefore, ignore such considerations in what follows.

holder wealth.[37] We modify this regression in three ways. First, CEO wealth data is not available to us. Instead, we have total annual pay. Thus, we replace the stock measure, wealth, by a flow measure, compensation in that year. Since CEO wealth increases not only because of compensation but also because options granted in previous years change in value, this will mismeasure the true change in wealth. As is common in this literature, this will lead to an underestimate of the pay for performance link. Since we have transformed the left hand side into a flow, we must also transform the right hand side into a flow. We therefore use performance measures that are flows, such as rate of return on the stock or net income. Second, we estimate our regressions in *log* of compensation, not levels.[38] The data definitely prefers the log specification in terms of goodness of fit (see Table 5). We will also use performance measures that are either in logs, such as changes in the log of shareholder wealth or form quasi rates of return such as net income divided by assets. Third, and most importantly, we are not interested in the coefficient on the pay for performance per se. We are instead interested in how that coefficient changes following the laws. We modify the differences-in-differences estimation procedure of equation (1) so that the differences-in-differences is on d, the pay for performance relationship, instead of on the constant, or the mean pay:

$$log(Comp_{it}) = \alpha_t + \beta_i + \gamma X_{it} + \delta BC_i * After_{it} + \psi_t Perf_{it} + \phi BC_i * Perf_{it} + \theta BC_i * After_{it} * Perf_{it} + \epsilon_{it} \quad (6)$$

This 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 effect, and θ will be the estimate of the laws' effects on pay for performance.

[37] This is typically estimated in first differences.

[38] We do estimate one regression in levels to replicate the Jensen and Murphy (1990) regression. See Table 5.

IV Results

IV.A Effect on Mean Pay

Table 3 estimates equation (3) for mean pay. We control for CEO characteristics with CEO tenure, tenure squared, age and age squared. We also include log employment and log assets. 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.[39] We find that the BC laws lead to a $4-5\%$ increase in mean pay.[40]

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, but log employment remains significant. Since the two variables log assets and log employment are very related, the log employment term, evidents serves as a better proxy for size. Even under this interpretation, however, this result is somewhat surprising. Even when controlling for firm fixed effects, short-run movements in size seem to impact pay.

Table 4 investigates the robustness of our estimates of the effects of 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

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

[40] When we decompose pay (in unreported regressions), we find that most of this increase is via increased award of stock options. The point estimates on all three components of pay (options, salary and bonus, and other compensation) are positive, but it is the options component that is largest and most significant. By increase in options, recall that we mean an increase in the value of the options *granted*, according to the Black-Scholes formula.

differentially affect firms with more assets or more employment and so on. For example, BC firms may be larger and the effects on 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 each of the returns to assets, employment, book value, market value, and experience as CEO to vary over time. Our estimates of the effects of BC laws are insensitive to these additions. We still find approximately a 5% effect for these laws. Column (6) allows the returns to all the variables—assets, book value, market value, and experience as CEO—to vary over time.[41] 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 V, 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.

IV.B Effect on Pay for Performance

From now on, we concentrate on BC legislation. Table 5 presents our basic estimates of the change in incentive pay. The regression framework follows equation (6). Each regression includes CEO demographic controls and controls for assets and employment as explained in Section IV.A.

Because we do not know which indicator of performance shareholders use, we try five different candidates. Both share performance and accounting measures of performance have been previously

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

used in the pay for performance literature. Theoretical justifications can be found for both types of measures. If the firm's owners care about long term performance or worry about executives manipulating accounting numbers, they should evaluate performance on the basis of market returns. If on the other hand, stock prices are very noisy measures of CEO effort or if CEOs do not influence the long term prospects of the company through day to day decisions, accounting measures may be better measures of performance. Both have been shown important determinants of CEO compensation (e.g., see Joskow and Rose (1994)). Columns (1) to (3) use accounting measures while columns (4) and (5) use market performance. In column (1), we use the ratio of net income over total assets as the performance measure. We see that incentive pay increases after passage of these laws. Compared to the base year (1984), the incentive slope is about 30% bigger. The exact same pattern is delivered in column (2) where we use the ratio of net income to book value as the performance indicator. The result is, however, less significant. Column (3) measures performance with the change in the book value of total stockholders' equity. The effect on the slope of the incentive pay line is again positive and very significant. Compared to the base year incentive level, the increase is quite large. The slope of the incentive scheme indeed more than doubles. Columns (4) and (5) use market return as performance measures: change in log market value of shareholders' wealth and change in market value of shareholders' wealth, respectively. While column (4) still uses log of compensation as a LHS variable, column (5) follows Jensen and Murphy (1990) and adopts a level specification. Again, both regressions deliver an economically important increase in the pay for performance slope. The increase is of about 20% compared to the base year in the log specification. It is, however, not significant. The level specification shows a statistically significant increase in slope of about 65% compared to 1984.

Table 6 reports some robustness checks. For each performance measure, we allow the returns to

assets, employment, and experience as a CEO to change over time.[42] We also include a trend term specific to states passing BC laws. Like the mean effect estimation, the incentive pay estimation is broadly unchanged by relaxing the specification. These robustness checks lessen worries that our results are driven by changes in the returns to observables or pre-existing trends in states passing laws.

V Political Economy of Anti-Takeover Laws

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. 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 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.[43] Typically,

[42] The returns to book value and market value are already allowed to change over time in columns (3) and (4) of Table 5.

[43] "Greyhound said, 'Jump,' and we said, 'How high?"' said state Rep. Jim Skelly, Chairman of the House Judiciary

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 shown in Tables 4 and 6, adding such trends does not change our coefficients in both the pay for performance and 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 7 estimates regression (4) with the addition of dynamics:

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

which includes two $Before_{it}$ terms to capture leads. $Before^0_{it}$ is a dummy for the year the law passed and $Before^{-1}_{it}$ is a dummy for the year before the law passed. Table 7 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 $After^s_{it}$ refers to a dummy for s years after the law and $After^{\geq s}_{it}$ 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 after their enactment. We received some hint of this in the cases discussed in Section I. 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.

Committee in Arizona (Los Angeles Times, September 15, 1987).

Inclusion of treatment trends does not change the coefficient. Similarly, we find no effect of the laws before the laws were actually passed.

VI Interpretation

The results to this point can be easily interpreted in an optimal contracting framework. Since takeovers discipline management, BC legislation represents a weakening of one incentive device faced by CEOs. If shareholders act as optimal principals, they should respond to the weakening of one incentive device by relying more on other ones. Since explicit pay for performance is the major alternative incentive device, we would expect the pay for performance relationship to tighten. The increase in incentive pay raises the variance of pay and, thereby, lowers the utility of a risk averse CEO. On the other hand, the laws themselves raise the utility of the CEOs by reducing the risk of takeover. Therefore, agency theory predicts an increase in pay for performance and an ambiguous effect on mean pay. We do find an increase in mean pay for performance. We also find an increase in mean pay. To account for this later finding, an agency model would require that the reduction in risk due to takeover protection be smaller than the increase in risk due to heightened pay for performance.

An alternative interpretation of our findings is provided by the popular literature on CEO compensation.[44] Some argue that CEOs essentially set their own pay, skimming what they can from relatively powerless shareholders. Under this theory, the decreased threat of takeover further weakens shareholders, thereby increasing the amount CEOs can skim. This is consistent with increased mean pay. To explain the increased correlation between pay and performance, one could argue that CEOs can only skim when performance is high. In that case, the difference between

[44]See Crystal (1991) for a discussion of these issues.

such a "constrained" skimming interpretation and a contract theory interpretation becomes subtle. The increased pay for performance caused by constrained skimming still implies that the CEO has stronger incentives to perform well. Even if pay is more tightly linked to performance only because of skimming, the CEO still faces a tighter incentive scheme.[45]

The skimming interpretation does provide a substantively different interpretation of our findings on mean pay. Recall that under the contract theory interpretation, mean pay rises in order to maintain the individual rationality constraint. Such a pay increase makes the CEO no better off since it exactly compensates for the increased risk. Under the skimming interpretation, the increase in mean pay is the result of increased discretion which does make the CEO better off.[46]

To assess the importance of skimming, we investigate the reasonableness of the incentive-insurance tradeoff implied by our estimates. Using the estimated increase in pay for performance, we can compute the increase in variance of pay. Combining the rise in variance with the rise in mean pay allows us to compute the level of risk aversion that leaves the CEO indifferent in the optimal contracting model. However, since the threat of takeover also disappears, the increase in variance of pay represents an *overestimate* of the increased risk caused by the laws. Hence, our estimates of the risk aversion coefficients are actually *lower bounds*. The CEOs true rise in risk is smaller than our estimated rise, and therefore, the rise in mean pay need not be as large for any given risk aversion coefficient. If much of the mean pay rise is skimming, we would then expect unreasonably high risk aversion coefficients. The mean pay increase would more than compensate for the rise in variance unless CEOs were very risk averse.

[45]These two theories do differ on one dimension. In the constrained skimming story, the CEO's pay for performance will increase even when the CEO's performance matters very little. More generally, a constrained skimming story implies that the pay for performance increase depends on the skimming constraint, not on the importance of CEO's effort or other contract theory considerations.

[46]Under constrained skimming, she faces only upside risk. The increased pay performance relationship happens only because the CEO can skim more during good times. There is no need to compensate for this increased "risk".

How do we use our estimates to compute the rise in variance of pay? In equation (6), θ represents the increased sensitivity of pay to performance. All other things equal, the variance of pay has increased by

$$Var[\theta * Perf] = \theta^2 * \sigma^2_{Perf} \tag{7}$$

Estimates of θ are provided in Table 5. To estimate σ^2_{Perf}, we use two different measures: the *within* firm variability in performance for only BC firms and for all firms.[47] We use the within firm variability rather than the total variability in the sample since otherwise we confound the cross-sectional variation with the true variability any given CEO faces.

In order to estimate a relative risk aversion coefficient, we must specify the CEO utility function. We use a constant relative risk aversion utility function:[48]

$$U = -c^{-r}$$

where c is compensation and r is a parameter related to risk aversion. This utility function can be reconciled with an incentive scheme linear in log compensation, which is the functional form implicitly used in our regression framework. Holmstrom and Milgrom (1987) present a model in which the optimal incentive scheme is linear in compensation under the assumption of a CARA utility function, $-e^{-rc}$. But since

$$U = -c^{-r} = -e^{-r*log(c)}$$

their results translate into an incentive scheme linear in *log* compensation for our utility function.

Assuming log normality for compensation and normality for the performance measure makes computations easy. Let c_0 be the old pay, distributed log normally with mean δ_0 and variance θ_0.

[47] We do not actually use the sample variance since the sample variance is a downwardly biased estimate of the true variance.

[48] We have chosen a CRRA utility function rather than CARA utility function because we find the assumption that absolute risk aversion decreases with pay realistic in this case.

Similarly, let c_1 be the new pay, also distributed log normally with mean δ_1 and variance θ_1. We have estimated $\delta_1 - \delta_0 = \hat{\delta}$ and $\theta_1 - \theta_0 = \hat{\theta}$. We wish to ask which r leaves the individual indifferent between the two lotteries $log(c_0)$ and $log(c_1)$. To do this, we equate the utility associated with the two lotteries:

$$-e^{-r\left[\delta_0 - \frac{1}{2}r^2\theta_0^2\sigma_{Perf}^2\right]} = -e^{-r\left[\delta_1 - \frac{1}{2}r^2\theta_1^2\sigma_{Perf}^2\right]}$$

Equating the terms in brackets and solving for r gives:

$$r = \sqrt{2 * \frac{\delta_1 - \delta_0}{\sigma_{Perf}^2(\theta_1 - \theta_0)(\theta_1 + \theta_0)}}$$

From our regression we can substitute $\hat{\delta} = \delta_1 - \delta_0$: this is the estimated insurance effect of the takeover laws. Also from our regression we can substitute $\hat{\theta} = \theta_1 - \theta_0$: this is the estimated incentive effect of the takeover laws. We have already discussed different techniques for estimating σ_{Perf}^2. Finally, we rewrite

$$\theta_1 + \theta_0 = 2 * \theta_0 + \hat{\theta}$$

We take θ_0 to be the pay for performance relationship in the base year for the BC firms.

The Arrow-Pratt relative risk aversion coefficient for this utility function is easily calculated:

$$-c\frac{u''(c)}{u'(c)} = -c\frac{r(-r-1)c^{-r-2}}{rc^{-r-1}} = r + 1$$

Thus our estimate of the relative risk aversion coefficient is:

$$r = \sqrt{2 * \frac{\hat{\delta}}{\sigma_{Perf}^2(\hat{\theta})(\hat{\theta} + 2\theta_0)}} + 1$$

Table 8 presents estimates for each of the specifications in Table 5. We do not include column (5) since those estimates are in level of compensation and do not fit into the framework outlined above. Columns represent the different measures σ_{Perf}^2. Rows represent the different performance measures. Our lower bounds on the relative risk aversion coefficients are between 5 and 6.6.

These may be unreasonably large levels of risk aversion for CEOs for two reasons.[49] First, recall that these are lower bound estimates. When the reduction of takeover risk is large, these estimates will greatly underestimate the implied risk aversion. Second, these estimates do not allow for the possibility for the CEO to diversify the increased risk caused by pay for performance. As estimates of risk aversion for potentially diversifiable risk, they are less plausible.[50] If we accept these arguments, we can reject the hypothesis that the rise in mean pay exactly compensated CEOs for the rise in risk. These estimates, therefore, raise the serious possibility that CEOs were made better off by these laws.

VII Concluding Remarks

We have provided some evidence that state anti-takeover laws raised mean CEO pay and increased the sensitivity of CEO pay to performance. These findings are consistent with a contract theory model in which the principal optimally trades off two instruments with which to discipline the agent: takeover threats and explicit incentive pay. Increased sensitivity of pay to performance is the consequence of a principal optimizing total incentives. Increased mean pay is the consequence of the incentive-insurance tradeoff inherent in contract theory. The mean pay effect is also consistent with a world view in which CEOs set their own pay. Our calculations of relative risk aversion coefficients do not allow us to reject this view. While these laws unambiguously resulted in CEOs facing tougher pay incentives, they may well have left them better off on the whole.

[49]These bounds do fall within the range of Haubrich (1994), but mainly because of the noisiness of his estimate. He finds that r between 1.1 and 9.9 are needed to explain Jensen and Murphy's (1990) estimated pay for performance relationships.

[50]The bias induced by diversifiability is magnified by the possibility that the risk caused by pay for performance may be more diversifiable than the risk of human capital loss caused by takeover threats.

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Table I: Summary Statistics[a]

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

[a]Notes:

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 II: 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)	Mississipi (1985)	Mississipi (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)		

[a]Source: *Annotated State Codes*, various states and years.

Table III: 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**	—	—	.045*
	(.025)			(.026)
FP*After$_t^{FP}$	—	.040	—	.031
		(.027)		(.030)
CSA*After$_t^{CSA}$	—	—	−.021	−.031
			(.030)	(.031)
Log Assets	.037	.038	.038	.038
	(.030)	(.030)	(.030)	(.030)
Log Employment	.185****	.183****	.184****	.185****
	(.032)	(.032)	(.032)	(.032)
Age	.038*	.038*	.037*	.038*
	(.020)	(.020)	(.020)	(.020)
Age^2 * 100	−.035**	−.035**	−.037**	−.038**
	(.017)	(.017)	(.017)	(.017)
Tenure	.016****	.016****	.016****	.016****
	(.004)	(.004)	(.004)	(.004)
Tenure^2 * 100	−.046***	−.046***	−.046***	−.047***
	(.016)	(.016)	(.016)	(.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

[a]Notes:

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 III. Log Assets is the log of total start of year assets in millions, Log Employment is log of total employment in 1000s, Age and Age^2 are for the age of the CEO, Tenure and Tenure^2 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 IV: 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**	.052**	.053**	.057**	.056**	.056**
	(.025)	(.025)	(.025)	(.025)	(.025)	(.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

[a]Notes:

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 III. $BC * Year$ 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, Age^2, Tenure and Tenure^2.
3. * denotes significance at the 10%;** at the 5%;*** at the 1%;**** at the .1%.

Table V: Effects of BC Legislation on Pay for Performance[a]

Dependent Variable: See Footnotes

	(1)	(2)	(3)	(4)	(5)
BC*After$_t^{BC}$	.054**	.055**	.046*	.054**	311.05*
	(.024)	(.025)	(.026)	(.025)	(188.68)
BC*After$_t^{BC}$*Performance	.891**	.226	.307**	.066	.232**
	(.406)	(.164)	(.140)	(.077)	(.127)
Performance Measure	2.657****	.605***	.231	.309**	.375*
	(.794)	(.219)	(.200)	(.101)	(.213)
Performance Measure*YD	Yes	Yes	Yes	Yes	Yes
Year Dummies(YD)	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	.711	.709	.711	.709	.160
Sample Size	4566	4566	3983	4524	4533

[a]Notes:

1. Dependent Variable is Log Total CEO Compensation in Columns (1) to (4); Total CEO Compensation in column (5). Performance is: the ratio of Net Income over Total Assets in Column (1), the ratio of Net Income over the Book Value in Column (2) , the change in log Book Value in column (3), the change in log Market Value in column (4), the change in Market Value in column (5). All the performance measures are demeaned. All regressions include controls for log total assets, log employment, Age, Age^2, Tenure as CEO and Tenure^2.
2. 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 III. 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, and Net Income is sales minus costs, all in millions.
3. All regressions include performance times year dummies. The reported performance coefficient is for the base year (1984).
4. * denotes significance at the 10%;** at the 5%;*** at the 1%;**** at the .1%.

Table VI: Effects of BC Legislation on Pay for Performance: Robustness Checks[a]

Dependent Variable: See Footnotes

	(1)	(2)	(3)	(4)	(5)
BC*After$_t^{BC}$	.044	.046*	.044	.054**	295.516
	(.028)	(.028)	(.029)	(.025)	(209.974)
BC*After$_t^{BC}$*Performance	.933**	.235	.287**	.057	.240*
	(.408)	(.164)	(.139)	(.077)	(.128)
Performance Measure	2.452***	.518**	.177	.289***	.348*
	(.825)	(.222)	(.203)	(.102)	(.214)
Performance Measure*YD	Yes	Yes	Yes	Yes	Yes
Log Assets*YD	Yes	Yes	Yes	Yes	Yes
Log Employment*YD	Yes	Yes	Yes	Yes	Yes
Tenure*YD	Yes	Yes	Yes	Yes	Yes
BC* Year	Yes	Yes	Yes	Yes	Yes
Year Dummies(YD)	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	.712	.710	.713	.711	.166
Sample Size	4566	4566	3983	4524	4533

[a]Notes:

1. Dependent Variable is Log Total CEO Compensation in Columns (1) to (4); Total CEO Compensation in column (5). Performance measures are: the ratio of Net Income to Total Assets in Column (1), the ratio of Net Income to the Book Value in Column (2), the change in log Book Value in column (3), the change in log Market Value in column (4), the change in Market Value in column (5). All the performance measures are demeaned. All regressions include controls for log total assets, log employment, age, age^2, tenure as CEO and tenure^2.
2. 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 III. 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, and Net Income is sales minus costs, all in millions. BC*Year is a time trend for the states passing BC legislation.
3. All regressions include performance times year dummies. The reported performance coefficient is for the base year (1984).
4. * denotes significance at the 10%;** at the 5%;*** at the 1%;**** at the .1%.

Table VII: Effects of BC Legislation on Mean Pay: Dynamics[a]

Dependent Variable: Log of Total CEO Compensation

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

[a]Notes:

1. All regressions also include demographic controls of Age, Age^2, Tenure and Tenure^2 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 III. *After* and *Before* dummies are defined in Section V Log Assets is the log of total start of year assets in millions, Log Employment is log of total employment in 1000s, Age and Age^2 are for the age of the CEO, Tenure and Tenure^2 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 VIII: Estimated

Relative Risk Aversion Coefficients[a]

Specification	σ^2_{Perf}	$\sigma^2_{Perf}\|BC_i = 1$
$\frac{\text{Net Income}}{\text{Assets}}$	5.28	5.15
$\frac{\text{Net Income}}{\text{Book Value}}$	6.59	6.47
Δlog(Book Value)	5.12	5.01
Δlog(Market Value)	5.72	5.64

[a]Notes:

1. For computation, see Text.
2. Column (1) uses within variance for the whole sample, (2) within variance for BC firms.
3. Row (1) uses Net Income over assets, row (2) uses Net Income over Book Value, row (3) uses change in book value and row (4) uses market return. See Table 5 for details.