Co-opted Boards

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We develop two measures of board composition to investigate whether directors appointed by the CEO have allegiance to the CEO and decrease their monitoring. *Co-option* is the fraction of the board comprised of directors appointed after the CEO assumed office. As *Co-option* increases, board monitoring decreases: turnover-performance sensitivity diminishes, pay increases (without commensurate increase in pay-performance sensitivity), and investment increases. *Non-Co-opted Independence*—the fraction of directors who are independent and were appointed before the CEO—has more explanatory power for monitoring effectiveness than the conventional measure of board independence. Our results suggest that not all independent directors are effective monitors. (*JEL* G32, G34. K22)

The board of directors of a corporation is meant to perform the critical functions of monitoring and advising top management (Mace 1971). Conventional wisdom holds that monitoring by the board is more effective when the board consists of majority of independent directors. The empirical evidence on the connection between board independence and firm performance, however, is mixed and weak, as is the evidence on the relation between board independence and other organizational and governance attributes, such as managerial ownership.[1]

One potential reason for the paucity of consistent, significant results is that many directors are co-opted and the board is captured. In practice, CEOs are

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[1] See, for example, Coles, Daniel, and Naveen (2008), Adams, Hermalin, and Weisbach (2010), and Coles, Lemmon, and Wang (2011).

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likely to exert considerable influence on the selection of all board members, including nonemployee directors. Carl Icahn, activist investor, asserts quite directly (*Business Week Online*, November 18, 2005) that ". . . members of the boards are cronies appointed by the very CEOs they're supposed to be watching." Likewise, Finkelstein and Hambrick (1989) allege that CEOs can co-opt the board by appointing "sympathetic" new directors. Hwang and Kim (2009) suggest that CEOs favor appointees who share similar views or social ties or because there is some other basis for alignment with the CEO.

Reflecting similar concerns about board capture, subsequent to the Sarbanes Oxley Act of 2002 (SOX), NYSE and NASDAQ adopted listing requirements that substantially reduced the direct influence of the CEO in the nominating process. Nonetheless, CEOs are likely to continue to be able to exert some influence on the board nomination process. At the very least, they approve the slate of directors, and this slate is almost always voted in by shareholders (Hermalin and Weisbach 1998; Cai, Garner, and Walkling 2009).[2]

In this paper, we propose and implement two new measures of board composition, which we term *Co-option* and *Non-Co-opted Independence*. *Co-option* is meant to capture board capture. *Non-Co-opted Independence*, on the other hand, is meant to refine the traditional measure of board independence as a proxy for the monitoring effectiveness of the board.

We define *Co-option* as the ratio of the number of "co-opted" (or captured) directors, meaning those appointed after the CEO assumes office, to board size. The idea is that such co-opted directors, regardless of whether they are classified as independent using traditional definitions, are more likely to assign their allegiance to the CEO because the CEO was involved in their initial appointment. Our measure is meant to reflect the additional behavioral latitude and managerial discretion afforded a CEO when that CEO has significant influence over some directors on the board. A related interpretation of *Co-option* is that it captures the disutility to the board from monitoring the CEO. Along these lines, Hermalin and Weisbach (1998), in their model of CEO bargaining with the board, specify director utility as a function of, among other things, a distaste for monitoring (κ in their model), which for a director is reflected in a ". . . *lack* of independence, at least in terms of the way he or she behaves" (101). *Co-option* can be thought of as capturing director aversion to monitoring and lack of independence aggregated to the board level. Intuitively, *Co-option* reflects what the CEO can get away with.

Co-option ranges from 0 to 1, with higher values indicating greater co-option and board capture and greater insulation of the CEO from various efficiency pressures. In our sample, mean *Co-option* is 0.47, indicating that on average nearly half of the directors on a board joined the board after the CEO assumed office.

[2] Of course, CEO influence on the nomination process is substantially lower in the relatively few instances in which directors are put up for election by dissident shareholders in proxy fights.

We predict that a CEO who has co-opted a greater fraction of the board will be less likely to be fired following poor performance, will receive higher pay, will have lower sensitivity of pay to performance, and will be able to implement preferred or pet projects even if they are suboptimal from a shareholder-value perspective. Our findings generally are consistent with these hypotheses.

First, we find that the sensitivity of forced CEO turnover to firm performance decreases with co-option. For example, our parameter estimates indicate that CEO-turnover-performance sensitivity is attenuated by about two-thirds for a one-standard-deviation increase in *Co-option*. Second, we find that CEO pay levels increase with board co-option. Of course, higher pay being associated with higher co-option is not symptomatic of entrenchment if it is compensation for higher risk borne by the CEO through higher pay-performance sensitivity. Additional evidence, however, suggests that this is not the case: we find that the sensitivity of CEO pay to firm performance is generally unrelated to board co-option and even is negatively related to co-option in some specifications. Finally, we find that investment in tangible assets (the ratio of capital expenditure to assets) increases with co-option. This is consistent with the idea that CEOs who have co-opted the board can invest in ways they otherwise would not. For example, in the absence of effective board monitoring, executives are likely to satisfy their preferences for scale and span of control, preferences that arise in larger firms for reasons of higher compensation, control over more resources, and enhanced stature in the industry and community (Jensen 1986). Overall, the evidence on turnover, pay, and investment is consistent with the idea that co-option reduces the monitoring effectiveness of the board.

In all specifications we control for the proportion of independent directors on the board (*Independence*), which traditionally has been understood to be a measure of board monitoring.[3] We find that *Independence* has little power to explain CEO turnover-performance-sensitivity, CEO pay, CEO pay-performance-sensitivity, and investment. If there were a statistical horse race between *Co-option* and *Independence*, *Co-option* would appear to be more successful.

In light of this result, a natural question is whether independent directors who are co-opted by the CEO are different in monitoring effectiveness from those who are not co-opted. To address this question, we calculate the fraction of the board that is comprised of independent directors appointed after the CEO assumed office ("*Co-opted Independence*"). Our results using this measure as an explanatory variable are similar to what we find with *Co-option*. Specifically, we find that *Co-opted Independence* is associated with lower sensitivity of CEO turnover to performance, higher pay levels, lower sensitivity of pay to performance, and greater investment. Thus, co-opted independent directors,

[3] See, for example, Weisbach (1988), Byrd and Hickman (1992), Brickley, Coles, and Terry (1994), Dahya, McConnell, and Travlos (2002), Hermalin and Weisbach (2003), Dahya and McConnell (2007), Coles, Daniel, and Naveen (2008), and Dahya, Dimitrov, and McConnell (2008).

though independent of the CEO in the conventional and legal sense, behave as though they are not independent in the function of monitoring management. This is likely to explain why the literature has not found consistent evidence with respect to the monitoring effectiveness of independent directors.

To formally test the monitoring effectiveness of independent directors who are not co-opted, we introduce a second new measure of board composition: *Non-Co-opted Independence*. We define this measure as the fraction of the board comprised of independent directors who were already on the board when the CEO assumed office. In our sample, mean *Non-Co-opted Independence* is 0.35, indicating that on average about a third of the board is comprised of independent directors who are truly independent, having not been co-opted by the CEO. Of course, on most issues faced by the board, the majority rules, so there is a significant possibility that the subset of independent directors who are not co-opted is not influential. Nonetheless, consistent with our conjecture that independent directors who are not co-opted are the monitors that matter, we find that *Non-Co-opted Independence* is associated with higher sensitivity of CEO turnover to performance, lower pay levels, higher sensitivity of pay to performance, and lower investment.

In sum, not all independent directors are equally effective at monitoring. Those who are co-opted by the CEO are associated with weaker monitoring, whereas the independent directors who join the board before the CEO assumes office, that is, the directors who hired the CEO, are associated with stronger monitoring.

Our results on board capture are robust to two alternative definitions of *Co-option*. Our first alternative proxy, *Tenure-Weighted Co-option* (*TW Co-option*), accounts for the possibility that directors appointed by the CEO become even more co-opted through time and that the influence of co-opted directors increases with their tenure on the board.[4] We define *TW Co-option* as the sum of the tenure of co-opted directors divided by the total tenure of all directors, so an increase likely indicates higher board co-option. Our second alternative proxy is designed to address the possible concern that co-option increases mechanically with CEO tenure and that our results on co-option may be capturing the effect of CEO tenure. We estimate *Residual Co-option* as the residual from a regression of *Co-option* on CEO tenure. We similarly estimate *Residual TW Co-option* as the residual from a regression of *TW Co-option* on CEO tenure. By construction, these residual measures are uncorrelated with CEO tenure. We find qualitatively similar results using these alternative definitions of co-option.

Our results also are robust to our best attempts to address endogeneity. All of our base case regressions include firm fixed effects to control for biases introduced by unobserved, firm-specific, time-invariant, omitted variables that are correlated with co-option. Endogeneity could still arise, however, either

[4] Per Nell Minow, quoted in Hymowtiz and Green (2013), "What you want from directors is for them to really push the CEO for answers and, just by human nature, that gets harder the longer they're on a board."

because the omitted variable is not firm-specific or varies through time, or because reverse causation runs from our firm policy variables, such as pay, to co-option. We exploit exchange-rule changes enacted in 2002 to address such concerns. Because these rules were adopted shortly after the passage of Sarbanes-Oxley (SOX), we refer to the postrules period as the post-SOX period. Firms that pre-SOX were not compliant with subsequent listing requirements to have a majority of independent directors on the board chose to appoint new independent directors (Linck, Netter, and Yang 2009), thereby causing an exogenous increase in board co-option for such firms. To isolate the causal impact of co-option, we apply a modified difference-in-differences approach. We continue to find results on the effects of co-option that by-and-large are consistent with the evidence described above.

1. Motivation, Related Literature, and Hypotheses Development

1.1 CEO turnover-performance sensitivity

One of the key functions of the board is to evaluate the CEO and to replace him if his performance is poor (Mace 1971). Whereas early studies find that the likelihood of CEO turnover decreases in firm performance, subsequent studies suggest that this relation between turnover and performance is weaker when the firm's governance is weaker.[5] Along similar lines, Hermalin and Weisbach (2003) suggest that turnover-performance sensitivity is weaker if the CEO captures the board. This implies that, for a given level of performance, CEOs of firms with more co-opted boards should be less likely to be fired. Thus, we expect that

H1: All else equal, the sensitivity of forced CEO turnover to firm performance decreases with co-option.

1.2 CEO pay level

A second important function of the board is to set the structure of CEO pay. Many studies argue that entrenched CEOs and CEOs of firms with weaker monitoring receive higher pay (Borokhovich, Brunarski, and Parrino 1997; Core, Holthausen, and Larcker 1999). We extend this reasoning to argue that if co-opted boards are more sympathetic to the CEO, then CEO pay should increase with co-option. This leads to our second hypothesis:

H2: All else equal, CEO pay level increases with co-option.

1.3 CEO pay-performance sensitivity

Pay contingent on performance is a means to align executive incentives with shareholder interests (e.g., Jensen and Murphy 1990; Bizjak, Brickley, and

[5] See Coughlan and Schmidt (1985), Warner, Watts, and Wruck (1988), Weisbach (1988), Huson, Parrino, and Starks (2001), Kang and Shivdasani (1995), and Kaplan and Minton (2012).

Coles 1993). Thus, we also examine the impact of co-option on CEO pay-performance sensitivity (PPS or "delta"). Hartzell and Starks (2003) show that the CEO pay-performance sensitivity is higher when institutions hold more shares and argue that this is consistent with higher institutional holdings being good for shareholders. Falaye (2007) finds lower PPS for CEOs of firms with staggered boards and argues that staggered boards are associated with CEO entrenchment. Thus, we expect that, if co-option results in lower efficiency pressures on the management team, then pay-performance sensitivity should decrease in co-option.[6]

H3: All else equal, CEO pay-performance sensitivity decreases with co-option.

1.4 Investment policy

A large body of literature addresses managerial incentives to overinvest and to engage in empire building. Jansen (1986, 323), for example, notes that "growth increases managers' power by increasing the resources under their control. It is also associated with increases in managers' compensation, because changes in compensation are positively related to the growth (see Kevin Murphy 1985)." Moreover, the scale and span of control can enhance the stature of the CEO in the industry and community. When the CEO has significant influence over some directors on the board and, accordingly, is permitted additional behavioral latitude and managerial discretion, such overinvestment is more likely. All else equal, co-option will be associated positively with investment.

H4: All else equal, firm investment increases with co-option.

2. Data and Summary Statistics

We start with the RiskMetrics database, with coverage of directors of S&P 500, S&P MidCap, and S&P SmallCap firms over the period 1996–2010. RiskMetrics does not provide a unique firm-level or director-level identifier over the entire time period. In the Appendix we describe how we associate unique identifiers with each record on RiskMetrics.[7] We obtain compensation data from Execucomp, accounting data from Compustat, and stock return data from CRSP. We exclude firms incorporated outside the United States. We define below our key variables.

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[6] Empirically, the papers mentioned in this subsection use varying methodologies to capture PPS. For example, Hartzell and Starks (2003) use PPS from new option grants only as the dependent variable. Coles, Lemmon, and Wang (2011) use the pay performance sensitivity derived from the total portfolio of accumulated stock and option holdings net of dispositions. Falaye (2007) uses Aggarwal, and Samwick-type (1999) regressions of changes in annual pay on dollar returns and interprets the coefficient on dollar returns as PPS.

[7] RiskMetrics provides two different director identifiers, neither of which is fully populated for all directors. Between 23%–27% of director-years have missing identifiers. We combine both to create a unique identifier for all director-year observations. Importantly, if only one of these identifiers is used, it will result in incorrect estimates of board size, independence, co-option, etc. Upon request, the authors can provide the unique director identifier created, as well as the unique firm identifiers (GVKEY and PERMNO) for each record on RiskMetrics.

2.1 CEO forced turnover

The logic underlying our measure of co-option is most applicable for forced turnover. Unfortunately, classifying turnover as forced or voluntary is difficult. Very often, even forced turnovers are reported to the press as voluntary. Nevertheless, we use an approximate classification scheme, similar to that used in other papers (such as Denis and Denis 1995) to separate turnovers into forced or voluntary. We define *Forced Turnover* as one if the departing CEO is less than 60 years old, and zero otherwise.

2.2 CEO pay

Our measure of CEO pay is total annual compensation (Execucomp variable *TDC1*). This includes the value of annual stock option grants, salary and bonus, value of annual restricted stock grants, other annual compensation, long-term incentive payouts, and all other compensation. We discuss in the Appendix how the changes in compensation reporting following FAS 123R and new SEC disclosure requirements affect the reporting of pay. We compute an adjusted pay measure (discussed in more detail in the Appendix) that accounts for these changes in reporting. Our results are similar using this adjusted pay measure.

2.3 CEO pay-performance sensitivity

Pay performance sensitivity is estimated as the sensitivity of CEO wealth to stock price, otherwise termed as CEO delta, based on the entire portfolio of stock and options held by the CEO. Specifically, the semielasticity form of delta is the expected dollar change in CEO wealth for a 1% change in stock price. We calculate delta using the approach of Core and Guay (2002) but with adjustments to Execucomp data as specified in the Appendix. Also see Coles, Daniel, and Naveen (2013) for details on data and on calculation of incentive measures in the presence of changing financial reporting requirements and formats.

2.4 Investment

Our proxy for investment is capital expenditures scaled by book value of assets.

2.5 Co-option

Our principal measure of co-option is based on the number of directors elected after the CEO takes office. We refer to such directors as "co-opted" directors.

$$Co\text{-}option = \frac{\#\,Co\text{-}opted\ directors}{Board\ size}$$

This variable ranges from 0 to 1, with higher values indicating greater co-option.[8]

[8] In contemporaneous work independent of ours, Morse, Nanda, and Seru (2011) develop a measure of CEO power based on three elements, one of which is similar to our measure of co-option. They show that more powerful

In some specifications, we use an alternative measure of co-option, *Tenure-Weighted Co-option* (*TW Co-option*), which is the sum of the tenure of co-opted directors divided by the total tenure of all directors. Thus,

$$TW\,Co\text{-}option = \frac{\sum_{i=1}^{boardsize} Tenure_i \times Co\text{-}opted\,Director\,Dummy_i}{\sum_{i=1}^{boardsize} Tenure_i},$$

where $Co\text{-}opted\ Director\ Dummy_i$ equals one if the director "i" is a co-opted director, and equals zero otherwise. $Tenure_i$ refers to the tenure of the director "i" on the board. This alternative measure accounts for the increase of influence of co-opted directors on board decisions through time, as such directors work alongside the CEO and previously appointed directors. This measure assumes that the greater the tenure of co-opted directors, the greater their influence on board decisions. Again, this measure can vary from 0 to 1, with a higher value indicating greater board capture.

Our third measure of co-option is *Residual Co-option*, which is defined as the residual from a regression of *Co-option* on CEO tenure. Our final measure of co-option is *Residual TW Co-option*, which is the residual from a regression of *TW Co-option* on CEO tenure. These two measures remove the positive correlation between CEO tenure and co-option.

For each firm-year, RiskMetrics provides the date of the annual meeting and the slate of directors up for election. The directors on the slate almost always obtain sufficient support to be elected (Hermalin and Weisbach 1998; Cai, Garner, and Walkling (2009)). The majority of the sample firms hold their annual meeting during the first 3–4 months of the fiscal year. Thus, because these directors constitute the board for the majority of the fiscal year, we assign directors on the slate at the annual meeting in a given fiscal year as the directors for that year.

For CEO turnover events, we are careful to identify the board in place before the CEO was dismissed because this board is the one responsible for replacing the CEO. Thus, the CEO turnover date relative to the meeting date is important for our purpose. Figure 1 illustrates the time line. If a CEO turnover occurred after the annual meeting date, then the board that determined the replacement was the board elected for that year. That is, turnover and co-option are measured contemporaneously. If a CEO turnover occurred before the annual meeting date, then the board responsible for replacing the CEO is the one elected in the previous year, so we use lagged measures of co-option in the turnover regression. In nonturnover years, because both the lagged and contemporaneous

CEOs (CEOs who have the titles of Chairman, CEO, and President, CEOs of firms with insider-dominated boards, or CEOs with a greater proportion of directors appointed during their tenure) rig their pay contracts by increasing the weights on the better performing measures.

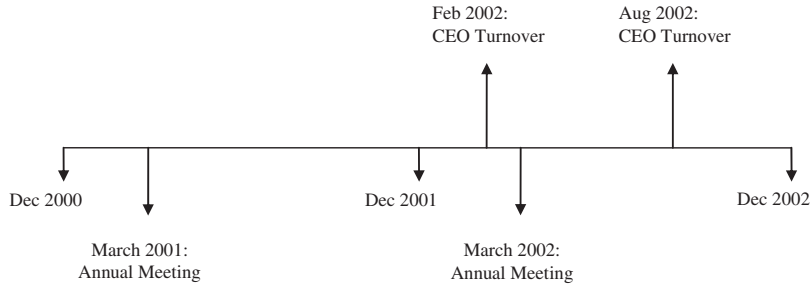

Figure 1
Time line
The example above illustrates how we estimate the relevant board co-option associated with any given outcome (such as turnover-performance sensitivity, pay, pay-performance sensitivity, and investment). Consider a firm that has a December fiscal year-end and has its annual meeting in March. Co-option for year 2001 is based on the slate of directors presented at the March 2001 annual meeting, because this is the board in place for the majority of the year. Thus, by definition, co-option is based on the board prevailing at the end of the year rather than at the beginning of the year. Similarly, co-option for the year 2002 is based on the slate of directors presented at the March 2002 meeting. Assume that a CEO turnover event occurs after the annual meeting date (say, August 2002). Then the board that determined the CEO turnover is the board in place since March 2002, and hence turnover and co-option are measured contemporaneously. If the turnover occurs before the annual meeting (say, February 2002), then the board that determined the turnover is the board elected as of March 2001, and the lagged co-option measure is used in regressions. For nonturnover years, because both the lagged and contemporaneous boards decide on the CEO's "nonreplacement," we use the average of the lagged and contemporaneous values of co-option. For regressions of CEO pay, CEO pay-performance sensitivity, and investment, we use the contemporaneous co-option measure because this is based on the board that is in place for the majority of the year and also because performance-based pay will be decided by the board at the end of the fiscal year.

boards decide on the CEO's "nonreplacement," we use the average of the lagged and contemporaneous values of co-option.

For regressions explaining variation in CEO pay, CEO delta, and investment, we use the contemporaneous co-option measure, because this is based on the board that is in place for the majority of the year and also because performance-based pay (which is a significant component of overall pay) will be decided by the board at the end of the fiscal year.

2.6 Independence

Independence is the ratio of the number of independent directors on the board to total board size. Independent directors are those who are neither inside nor gray directors (Weisbach 1988; Byrd and Hickman 1992; Brickley, Coles, and Terry 1994).

2.7 Summary statistics

Table 1 provides the summary statistics. Throughout the paper, we winsorize all variables at the 1st and 99th percentiles to minimize the influence of outliers.[9]

[9] Our results are similar if we winsorize all variables at the 0.5 and 99.5 percentiles instead. Our results are also similar if we drop the observations in the top and bottom 0.5 percentiles from the analyses.

Table 1
Summary statistics

	Obs.	Mean	Median	SD
Firm-level variables				
Sales ($ million)	18,987	5,341	1,505	11,426
Investment	18,405	0.052	0.038	0.051
Board-related variables				
Board Size	18,987	9.6	9.0	2.7
Co-option	18,196	0.47	0.44	0.32
TW (Tenure-weighted) Co-option	18,196	0.31	0.17	0.33
Residual Co-option	18,196	0.00	−0.03	0.22
Residual TW Co-option	18,196	0.00	−0.07	0.23
Co-opted Independence	18,196	0.35	0.33	0.25
Co-opted Non-Independence	18,196	0.12	0.09	0.15
Non-Co-opted Independence	18,196	0.34	0.33	0.26
Independence	18,987	0.69	0.71	0.17
CEO-related variables				
Forced Turnover	18,860	0.025	0.000	0.156
CEO Pay ($ '000s)	18,870	4,934	2,853	6,088
CEO pay-performance sensitivity (Delta; $ '000s)	17,436	789	250	1,802
CEO Tenure (years)	18,401	8.1	6.0	7.1

Co-option is the number of directors appointed after the CEO assumed office ("co-opted" directors) divided by the board size. *TW Co-option* is the sum of tenure of co-opted directors divided by the sum of tenure of all directors. *Residual Co-option* and *Residual TW Co-option* are the residuals from regressions of *Co-option* on CEO tenure and *TW Co-option* on CEO tenure. *Co-opted Independence* is the proportion of the board that consists of co-opted directors who are independent. *Co-opted Non-Independence* is the proportion of the board that consists of co-opted directors who are not independent. *Non-Co-opted Independence* is the proportion of the board that consists of independent directors who were already on the board when the CEO assumed office. *Independence* is the proportion of the board that consists of independent directors. *Forced Turnover* equals one if the CEO departs and is younger than 60 years of age and equals zero otherwise. *CEO Pay* is the total annual pay (Execucomp: TDC1). *CEO Delta* is the expected dollar change in CEO wealth for a 1% change in stock price, where components of pay-performance sensitivity (delta) arise from CEO holdings of own-firm stock and options, per Core and Guay (2002). *Investment* is capital expenditure scaled by assets. Reported values are based on data winsorized at the 1st and 99th percentiles.

The average firm in the sample is large, with sales of $5.3 billion. This is not surprising given that our sample is S&P 1500 firms. The average board has about ten directors. *Co-option* has a mean value of 0.47, whereas mean *Independence* is 0.69. Thus, on average, although more than two-thirds of the directors are technically independent, our calculations indicate that nearly half of the board has been co-opted by the CEO. Average *Tenure-Weighted (TW) Co-option* is 0.31, implying that whereas co-opted directors make up nearly half the board, their influence, after accounting for their tenure on the board, is a bit lower at 31%. Not surprisingly, *Co-option* and *TW Co-option* are similar, with a correlation of 0.93 ($p < 0.0001$). *Co-option* and *TW Co-option* are dissimilar to board independence ($\rho = -0.07$ and $\rho = -0.09$, respectively).

The unconditional rate of forced turnover is 0.025. For comparison, the equivalent number is 0.019 in Hazarika, Karpoff, and Nahata (2012) (inferred from their Table 1) and is 0.030 in Mobbs (forthcoming). On average, CEOs receive $4.9 million in total annual compensation, have a delta of $789,000, and have about eight years of tenure. On average, investment is 5.2% of total book assets.

3. Co-option and Monitoring Ineffectiveness: Empirical Results

3.1 Co-option and CEO turnover-performance sensitivity

Our first hypothesis, H1, is that the sensitivity of CEO turnover to performance decreases with co-option. To test this, we estimate the following logistic regression:

$$\begin{aligned} &ln[Prob(Forced\ Turnover)/(1 - Prob(Forced\ Turnover))] \\ &= \alpha_0 + \alpha_1 Co\text{-}option \times Performance \\ &\quad + \alpha_2 Performance + \alpha_3 Co\text{-}option + \alpha_4 Independence + f(Other\ Controls) + \varepsilon_1. \end{aligned}$$

Our proxy for performance is *Prior Abnormal Return*. For turnover years, this is measured as the firm stock return (including dividends) in the year leading to the actual date of CEO turnover minus the value-weighted market return over that period. For nonturnover years, this is measured as the stock return over the previous fiscal year minus the value-weighted market return over that period. It is well-documented that, in practice, performance is negatively related to the likelihood of CEO turnover or that α_2 is negative (Weisbach 1988; Warner, Watts, and Wruck 1988; Parrino 1997; Kaplan and Minton 2012). Our hypothesis is that turnover-performance sensitivity is attenuated by co-option or that α_1 is positive. All specifications include *Independence*. Other control variables (*Other Controls*) include firm size, CEO tenure, and governance variables. The governance variables are CEO ownership; CEO duality, an indicator variable that equals one if the CEO also has the title of chairman; outside director ownership; GIM index, the governance index of Gompers, Ishii, and Metrick (2003); board size; female director, an indicator variable that equals one if the firm has a female director on board; and (in some models) terms interacting governance variables with prior performance.[10] We include firm fixed effects to control for any omitted firm-specific and time-invariant variables that are correlated with co-option. We include year fixed effects to control for variation in common influences through time. In general, our control variables are based on those in Adams and Ferreira (2009), Hwang and Kim (2009), Fich and Shivdasani (2007), and Dittmar and Mahrt-Smith (2007).

Table 2 reports the results. In models 1 and 2, the key independent variable is the interaction term of *Co-option* with *Prior Abnormal Return*. For each independent variable, we report the coefficient estimates (Row 1), z-statistics (Row 2), and the marginal effects (Row 3). We report the marginal effects because there is no ready economic interpretation of the coefficients in nonlinear regressions. The marginal effect is presented in semi-elasticity form. For continuous variables, the marginal effect represents the percentage change in the probability of *Forced Turnover* for a one unit change in the underlying variable, holding all other variables at their mean values. For indicator variables,

[10] For the four CEO-related variables, the values correspond to the departing CEO in the year of turnover. Also, we do not include CEO age because *Forced Turnover* is automatically zero when the CEO is over 60.

Table 2
Effect of co-option on CEO turnover-performance sensitivity

	Dependent variable = Forced Turnover					
	Co-option measure used:					
	Co-option (1)	Co-option (2)	TW Co-option (3)	TW Co-option (4)	Residual Co-option (5)	Residual TW Co-option (6)
Co-option measure × Prior Abnormal Return (α_1)	**2.021***** **(3.4)** ***0.856***	**1.610**** **(2.5)** ***0.535***	**2.304***** **(4.4)** ***0.961***	**2.557***** **(4.1)** ***0.751***	**1.558*** **(1.8)** ***0.327***	**2.250***** **(3.4)** ***0.407***
Prior Abnormal Return (α_2)	−2.153*** (−5.3) *−1.331*	−2.473*** (−2.6) *−1.257*	−2.062*** (−6.1) *−1.539*	−2.974** (−2.6) *−1.550*	−1.163*** (−5.6) *−1.165*	−1.337*** (−5.7) *−1.347*
Co-option measure	3.817*** (6.9) *3.797*	3.804*** (6.9) *3.723*	2.475*** (3.8) *2.571*	2.517*** (3.8) *2.628*	3.832*** (6.9) *3.854*	2.539*** (3.8) *2.629*
Independence$_{t-1}$	−0.674 (−0.8) *−0.650*	−0.723 (−0.9) *−0.597*	−0.555 (−0.6) *−0.545*	−0.555 (−0.6) *−0.462*	−0.655 (−0.8) *−0.643*	−0.527 (−0.6) *−0.517*
Firm Size$_t$	−0.264 (−0.9) *−0.255*	−0.235 (−0.8) *−0.221*	−0.289 (−0.9) *−0.284*	−0.296 (−1.0) *−0.288*	−0.248 (−0.9) *−0.244*	−0.274 (−0.9) *−0.269*
CEO Tenure$_t$	0.018 (0.7) *0.018*	0.021 (0.8) *0.020*	0.106*** (3.2) *0.104*	0.106*** (3.2) *0.103*	0.132*** (6.8) *0.130*	0.174*** (7.3) *0.171*
CEO Ownership$_t$	−0.053 (−1.2) *−0.051*	−0.090* (−1.7) *−0.078*	−0.066 (−1.5) *−0.064*	−0.071 (−1.5) *−0.067*	−0.042 (−1.0) *−0.041*	−0.054 (−1.3) *−0.053*
CEO Duality$_t$	−1.148*** (−5.3) *−1.110*	−1.127*** (−5.2) *−1.066*	−1.344*** (−5.8) *−1.321*	−1.370*** (−5.8) *−1.378*	−1.162*** (−5.4) *−1.140*	−1.370*** (−5.9) *−1.350*
Outside Director ownership$_{t-1}$	0.015 (0.8) *0.015*	0.016 (0.8) *0.014*	0.023 (1.2) *0.022*	0.023 (1.2) *0.023*	0.016 (0.9) *0.016*	0.025 (1.3) *0.024*
GIM Index$_{t-1}$	−0.079 (−0.7) *−0.076*	−0.085 (−0.8) *−0.078*	−0.093 (−0.8) *−0.091*	−0.091 (−0.8) *−0.083*	−0.085 (−0.8) *−0.084*	−0.099 (−0.9) *−0.097*
Board Size$_{t-1}$	−0.009 (−0.1) *−0.009*	−0.006 (−0.1) *0.001*	−0.032 (−0.4) *−0.032*	−0.021 (−0.3) *−0.012*	−0.011 (−0.2) *−0.011*	−0.026 (−0.4) *−0.026*
Female Director$_{t-1}$	−0.072 (−0.3) *−0.070*	−0.087 (−0.3) *−0.092*	0.069 (0.2) *0.068*	0.057 (0.2) *0.038*	−0.031 (−0.1) *−0.03*	0.101 (0.3) *0.099*
Independence$_{t-1}$ × Prior Abnormal Return		0.295 (0.3) *0.108*		0.278 (0.2) *0.082*		
CEO Ownership$_{t-1}$ × Prior Abnormal Return		0.099* (1.8) *0.259*		0.013 (0.2) *−0.003*		
CEO Duality$_{t-1}$ × Prior Abnormal Return		0.100 (0.3) *0.059*		−0.449 (−1.0) *−0.490*		
Outside Director Ownership$_{t-1}$ × Prior Abnormal Return		−0.035 (−1.2) *−0.198*		−0.023 (−0.7) *−0.116*		
GIM Index$_{t-1}$ × Prior Abnormal Return		−0.012 (−0.2) *−0.003*		0.039 (0.4) *0.142*		
Board Size$_{t-1}$ × Prior Abnormal Return		0.045 (0.5) *0.010*		0.056 (0.5) *0.192*		

(*Continued*)

Table 2
Continued

	Dependent variable = Forced Turnover					
	Co-optionmeasure used:					
	Co-option (1)	Co-option (2)	TW Co-option (3)	TW Co-option (4)	Residual Co-option (5)	Residual TW Co-option (6)
Female Director$_{t-1}$ × Prior Abnormal Return		−0.051 (−0.1) *−0.108*		−0.020 (−0.0) *−0.104*		
Fixed effect	firm, year	firm, year	firm, year	firm, year	firm, year	firm, year
Observations	2,152	2,152	1,899	1,899	2,152	1,899
Turnover-Performance Sensitivity at Co-option measure = max value	−0.092	−0.203	0.213	0.415	−0.088	0.465

The table presents results from logistic regressions of *Forced Turnover*, which equals one if the CEO departs and is younger than 60 years of age and equals zero otherwise. *Co-option* is the number of directors appointed after the CEO assumed office ("co-opted" directors) divided by the board size. *TW Co-option* is the sum of tenure of co-opted directors divided by the sum of tenure of all directors. For a departing CEO, co-option effectively is the co-option of the board at the time of dismissal. In the year in which turnover occurs, if the turnover occurs after the date of the firm's annual meeting, then the contemporaneous co-option measure is used; if the turnover occurs before the firm's annual meeting date, then the lagged co-option measure is used. In nonturnover years, we take the average of contemporaneous and lagged values of co-option measure. *Residual Co-option* and *Residual TW Co-option* are the residuals from regressions of *Co-option* on CEO tenure and *TW Co-option* on CEO tenure. For turnover years, *Prior Abnormal Return* is measured as the annual stock returns in the year leading up to the actual date of CEO turnover minus the value-weighted market returns over that period. For nonturnover years, *Prior Abnormal Return* is measured as the stock returns over the previous fiscal year minus the value-weighted market returns over that period. In the year of turnover, all CEO variables correspond to that of the departing CEO; in nonturnover years, they are measured contemporaneously. All other control variables are defined in the Appendix. For each independent variable, we present the coefficient estimate (row 1), the z-statistics (in parentheses; row 2), and the marginal effect (in italics; row 3). The marginal effect for a continuous variable is the percentage change in the conditional expected probability of *Forced Turnover* for a 1% change in the continuous variable, holding all other variables at their means (i.e., it is the elasticity). The marginal effect for an indicator variable is the percentage change in the conditional expected probability of *Forced Turnover* when the indicator variable goes from 0 to 1, holding all other variables at their means. The marginal effect for the interaction term of *Prior Abnormal Return* with a continuous variable is the change in the elasticity of *Forced Turnover* with *Prior Abnormal Return* when the continuous variable moves by one standard deviation from its mean value, holding all other variables at their means. The marginal effect for the interaction term of *Prior Abnormal Return* with an indicator variable is the change in the elasticity of *Forced Turnover* with *Prior Abnormal Return* when the indicator variable moves from zero to one, holding all other variables at their means. The last row of the table gives the elasticity of *Forced Turnover* with *Prior Abnormal Return* for a fully co-opted board (i.e., at *Co-option* = 1 for Columns 1 and 2, *TW Co-option* =1 for Columns 3 and 4, *Residual Co-option* = 0.67 for Column 5, and *Residual TW Co-option* = 0.84 for Column 6). ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

we report the percentage change in the probability of *Forced Turnover* when the indicator variable moves from zero to one (holding other variables at their mean values).[11]

Consistent with our hypothesis, the coefficient on the interaction term of *Co-option* and *Prior Abnormal Returns* (α_1) is positive and statistically significant (= 2.021, z-statistic = 3.4), indicating that an increase in *Co-option* is associated with a decrease in the sensitivity of CEO turnover to firm performance. To estimate the effect of the interaction term, we compute the marginal effect of

[11] Ai and Norton (2003) note that interpretation of interacted variables in nonlinear models is not straightforward. Stata (v. 11) has since introduced the *margins* statement, which correctly computes the marginal effects in nonlinear models with interaction terms. We use this statement to compute all reported marginal effects.

Prior Abnormal Return at two different levels of *Co-option*: at the mean as well as mean plus one standard deviation (holding all other variables at their mean values). The difference indicates how the sensitivity of turnover to performance changes with co-option. As can be seen in model 1, the sensitivity of turnover to firm performance decreases by 0.856, from –1.331 at the mean value of *Co-option*, to –0.476 when *Co-option* increases by one standard deviation from its mean value (for ease of presentation in the table, we report only the difference). In other words, the sensitivity of turnover to performance goes down by almost two-thirds when *Co-option* moves by one standard deviation from its mean value. If *Co-option* increases even further to the maximum possible value of one, then the sensitivity of forced turnover to performance is even smaller (= –0.092; see the last row of Table 2).[12] Thus, the results in model 1 are consistent with H1. Turnover-performance sensitivity decreases as co-option increases.

In model 1, we allow only *Co-option* to affect the turnover-performance sensitivity (i.e., we include only the interaction term of *Co-option* with *Prior Abnormal Return*). In model 2 we allow all governance-related variables (*Independence*, CEO ownership, CEO duality, outside director ownership, GIM index, board size, and female director) to affect the turnover-performance sensitivity. Two results are worth noting. First, the coefficient on the interaction of *Co-option* with *Prior Abnormal Return* remains significantly positive. Second, *Independence* does not appear to have a significant impact on turnover-performance sensitivity. Board co-option, rather than board independence, has explanatory power for turnover-performance sensitivity.

In models 3 and 4, we use the same specifications as in models 1 and 2, respectively, but include *TW Co-option*, rather than *Co-option*. The estimated coefficient on the interaction of *TW Co-option* with *Prior Abnormal Return* remains significantly positive in both specifications. In terms of economic significance, the results in model 3 indicate that when *TW Co-option* increases by one standard deviation from its mean value, the sensitivity of turnover to performance changes from –1.539 to –0.577 (the table reports the difference = 0.962) . The last row in the table shows that when *TW Co-option* increases to one, the sensitivity of turnover to performance is altered further to 0.213, which is *positive*. Results from model 4 are similar.

A potential issue arises because our two co-option measures (*Co-option* and *TW Co-option*) are positively correlated with CEO tenure. Thus, multicollinearity could be a concern. To address this concern, we replace *Co-option* with *Residual Co-option*, which is the residual from a regression of *Co-option* on CEO tenure. Model 5 reports the results. The coefficient on the interaction of *Residual Co-option* with *Prior Abnormal Returns* is significantly

[12] In the model, α_2 represents the effect of *Prior Abnormal Return* on *Forced Turnover* when *Co-option* is zero. When STATA reports the marginal effect of *Prior Abnormal Return*, however, it reports the total effect of *Prior Abnormal Return* on *Forced Turnover* at the mean of all variables.

positive, indicating that the effect of *Co-option* on CEO turnover-performance sensitivity documented in model 1 is not due to the correlation between *Co-option* and CEO tenure. Finally, in model 6, we replace *Co-option* with *Residual TW Co-option*, which is the residual from a regression of *TW Co-option* on CEO tenure. Once again, our results are similar to those in model 3.[13]

In terms of the other control variables, our results across the various models show that CEO duality is significantly negatively related to CEO turnover (as in Goyal and Park 2002). In contrast, the other governance variables, in general, are not consistently significant across the various specifications.

The number of observations is much smaller in our turnover regressions because the use of firm fixed effects means that firms that never had a forced turnover during the sample period are excluded from the regression. To ensure that our results are not driven by any sample selection, we estimate the same regression models without firm fixed effects, but with industry fixed effects, and obtain very similar results for all six specifications on a much larger sample.

In all tables that follow, we report t-statistics based on standard errors adjusted for heteroscedasticity and clustering at the firm level (Petersen 2009). This option, however, is not available for the fixed effects logistic regression models in Table 2. As a robustness check, we bootstrap the standard errors using 200 replications. We find qualitatively similar results using the bootstrap.

Overall, the results indicate that, consistent with H1, turnover-performance sensitivity is attenuated as measures of board co-option increase.

3.2 Co-option and CEO pay level

Our second hypothesis, H2, predicts that CEO pay increases with co-option. To test this, we estimate regressions of *CEO* pay on co-option and controls.

$$CEO\ Pay = \theta_0 + \theta_1 Co\text{-}option + \theta_2 Independence + g(Other\ Controls) + \varepsilon_2.$$

H2 asserts that the coefficient on *Co-option* (θ_1) will be positive. The control variables, based on prior literature (see Murphy 1999 for a comprehensive review of CEO compensation), include board independence, firm size, firm performance (both stock and accounting), CEO tenure, governance variables, and firm and year dummies.[14] We do not include CEO turnover years and require that the CEO's tenure be at least one year. This is because CEO pay in a turnover year is likely to reflect compensation only for part of the year. Also, CEOs in their first year may receive higher than average stock compensation

[13] We also estimate models 5 and 6, including interactions of all governance variables with prior abnormal returns (as in models 2 and 4). When we re-estimate model 6 in this manner, the results are statistically and economically similar to our main results. When we re-estimate model 5, the results are economically similar but are statistically weaker. The sensitivity of turnover to performance decreases from –1.62 at the maximum value of *Residual Co-option* to –0.26 at the minimum value. The interaction of *Residual Co-option* with prior abnormal returns is positive but is insignificant ($p = 0.195$).

[14] The results are robust to using industry and year fixed effects instead.

Table 3
Effect of co-option on CEO pay

	Dependent variable = logarithm of CEO Pay			
	Co-option measure used:			
Coefficient estimate (t-statistic)	Co-option (1)	TW Co-option (2)	Residual Co-option (3)	Residual TW Co-option (4)
Co-option measure$_t$ (θ_1)	**0.223*****	**0.133***	**0.227*****	**0.130***
	(3.9)	**(2.0)**	**(3.9)**	**(1.9)**
Independence$_t$	0.141	0.163*	0.141	0.163*
	(1.5)	(1.7)	(1.5)	(1.7)
Firm Size$_t$	0.327***	0.324***	0.327***	0.324***
	(10.1)	(9.9)	(10.1)	(9.9)
Stock Return$_t$	0.103***	0.104***	0.103***	0.104***
	(5.9)	(5.9)	(5.9)	(5.9)
ROA$_t$	1.220***	1.226***	1.220***	1.226***
	(7.2)	(7.2)	(7.2)	(7.2)
CEO Tenure$_t$	−0.008**	−0.004	−0.001	−0.000
	(−2.4)	(−1.3)	(−0.4)	(−0.1)
CEO Ownership$_t$	−0.012**	−0.013***	−0.012**	−0.013***
	(−2.6)	(−2.6)	(−2.6)	(−2.6)
CEO Duality$_t$	0.037	0.045*	0.037	0.045*
	(1.5)	(1.9)	(1.5)	(1.9)
Outside Director Ownership$_t$	−0.001	−0.001	−0.001	−0.001
	(−1.4)	(−1.3)	(−1.4)	(−1.3)
GIM Index$_t$	0.011	0.011	0.011	0.011
	(1.0)	(1.0)	(1.0)	(1.0)
Board Size$_t$	−0.005	−0.003	−0.005	−0.003
	(−0.7)	(−0.4)	(−0.7)	(−0.4)
Female Director$_t$	0.013	0.014	0.013	0.014
	(0.5)	(0.5)	(0.5)	(0.5)
Fixed effects	firm, year	firm, year	firm, year	firm, year
Observations	12,036	12,036	12,036	12,036
R^2	0.167	0.166	0.168	0.166

The table presents regression results where the dependent variable is logarithm of *CEO Pay*. We drop firm-years in which a turnover occurred and require that the CEO's tenure be at least one year. This ensures that we do not consider pay for fractional years. *CEO Pay* is the total annual pay (Execucomp: *TDC1*). *Co-option* is the number of directors appointed after the CEO assumed office ("co-opted" directors) divided by the board size. *TW Co-option* is the sum of tenure of co-opted directors divided by the sum of tenure of all directors. *Residual Co-option* and *Residual TW Co-option* are the residuals from regressions of *Co-option* on CEO tenure and *TW Co-option* on CEO tenure. All other control variables are defined in the Appendix. Intercept is included but not reported. t-statistics, given in parentheses, are based on standard errors corrected for heteroscedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

(to align their incentives) and higher bonus (including signing bonuses). We use the logarithm of annual compensation as the dependent variable because compensation data are skewed.[15]

Table 3 presents the results. In model 1, the coefficient on *Co-option* is significantly positive, implying that CEO pay increases with co-option.[16] The coefficient of 0.223 on *Co-option* indicates that moving from zero to full co-option would be associated with an increase in CEO pay of 22.3%. A less

[15] We obtain similar results using unlogged compensation.

[16] This result is consistent with Core, Holthausen, and Larcker (1999), who, using a sample of 495 firm-years from 1982–1984, find that CEO total pay is positively related to the proportion of the board composed of new outside (both independent and affiliated) directors.

extreme measure of economic significance is the change in pay when *Co-option* increases by one standard deviation. In this case, we find that CEO pay increases by 7% relative to the mean pay. This corresponds to about $345,380 annually for the CEO.

In model 2, we use *TW Co-option* rather than *Co-option*. As with *Co-option*, we find that the coefficient on *TW Co-option* is significantly positive. Finally, in models 3 and 4, we use *Residual Co-option* and *Residual TW Co-option*, and the results are similar. The coefficients on both measures are significantly positive, indicating that co-option is associated with higher pay, and this effect is not driven by the positive correlation between co-option and tenure.

Board independence has no explanatory power for CEO pay in two of the four specifications. In the other two models, the coefficient on *Independence* is positive, which is inconsistent with greater independence leading to better monitoring of rent extraction.[17] For the other control variables, as expected firm size and performance are strongly positively associated with pay. Overall, the evidence is consistent with CEO pay increasing in co-option (H2).

3.3 Co-option and CEO pay-performance sensitivity

Pay-performance-sensitivity—otherwise known as delta—is seen as aligning the incentives of managers with the interests of shareholders. Higher delta can mean that managers will work harder or more effectively because managers share gains and losses. Thus, we now examine the influence of co-option on CEO delta. The representative specification is

$$\textit{CEO Pay-Performance Sensitivity} = \gamma_0 + \gamma_1 \textit{Co-option} + \gamma_2 \textit{Independence} + h(\textit{Other Controls}) + \varepsilon_3.$$

Our control variables are based on the prior literature on the determinants of delta (Core and Guay 1999; Coles, Daniel, and Naveen 2006) and the governance variables used in the preceding regressions.

Table 4 presents the results. As in Table 3, our independent variables are *Co-option* (model 1), *TW Co-option* (model 2), *Residual Co-option* (model 3), and *Residual TW Co-option* (model 4). In models 1 and 3, the estimated coefficients on *Co-option* and *Residual Co-option* are negative (consistent with our hypothesis) but insignificant at conventional levels (p = 0.107 and 0.103, respectively). In models 2 and 4, the estimated coefficient on *TW Co-option* and *Residual TW Co-option* are negative and significant, albeit at the 10% level. The coefficient on *Co-option* in model 1 indicates that when *Co-option* increases by one standard deviation from its mean, pay-performance sensitivity decreases by 12% from its mean value. When *Co-option* increases from zero to

[17] As a robustness check, instead of using contemporaneous values of our co-option measures, we also use the average of the contemporaneous and the lagged values, because the lagged board also may be partly responsible for CEO compensation. Our results are robust to this change.

Table 4
Effect of co-option on CEO pay-performance sensitivity (CEO delta)

	Dependent variable = CEO Pay-performance sensitivity			
	Co-option measure used:			
	Co-option (1)	TW Co-option (2)	Residual Co-option (3)	Residual TW Co-option (4)
Co-option measure$_t$ (γ_1)	−296.485	−366.150*	−299.503	−363.054*
	(−1.6)	(−1.7)	(−1.6)	(−1.8)
Independence$_t$	−558.298**	−578.303**	−558.695**	−578.854**
	(−2.4)	(−2.5)	(−2.4)	(−2.5)
Firm Size$_t$	199.686***	201.071***	199.643***	201.065***
	(3.1)	(3.1)	(3.1)	(3.1)
Tobin's q_t	378.526***	378.745***	378.469***	378.609***
	(7.1)	(7.2)	(7.1)	(7.2)
R&D/Assets$_t$	−1,644.191	−1,729.813	−1,640.445	−1,722.186
	(−1.4)	(−1.5)	(−1.4)	(−1.5)
Investment$_t$	1,338.528*	1,355.289*	1,337.425*	1,350.299*
	(1.7)	(1.7)	(1.7)	(1.7)
Leverage$_t$	−200.384	−197.445	−200.717	−197.515
	(−1.0)	(−1.0)	(−1.0)	(−1.0)
Log(Unsystematic Risk$_t$)	−93.625***	−91.989***	−93.651***	−91.938***
	(−2.9)	(−2.9)	(−2.9)	(−2.9)
CEO Tenure$_t$	82.321***	83.992***	72.924***	72.232***
	(5.1)	(5.2)	(6.5)	(6.6)
CEO Duality$_t$	156.116***	152.332***	156.272***	151.993***
	(2.7)	(2.7)	(2.7)	(2.7)
Outside Director Ownership$_t$	0.083	0.062	0.084	0.060
	(0.1)	(0.1)	(0.1)	(0.1)
GIM Index$_t$	11.241	9.324	11.222	9.349
	(0.3)	(0.3)	(0.3)	(0.3)
Board Size$_t$	2.524	−1.028	2.514	−1.058
	(0.1)	(−0.1)	(0.1)	(−0.1)
Female Director$_t$	−11.763	−14.439	−11.837	−14.363
	(−0.2)	(−0.3)	(−0.2)	(−0.3)
Fixed effects	firm, year	firm, year	firm, year	firm, year
Observations	11,539	11,539	11,539	11,539
R^2	0.196	0.196	0.196	0.196

The table presents regressions of *CEO Pay-performance sensitivity* (or *Delta*), defined as the expected dollar change in CEO wealth for a 1% change in stock price, where components of delta arise from current CEO holdings of own-firm stock and options, per Core and Guay (2002). *Co-option* is the number of directors appointed after the CEO assumed office ("co-opted" directors) divided by the board size. *TW Co-option* is the sum of tenure of co-opted directors divided by the sum of tenure of all directors. *Residual Co-option* and *Residual TW Co-option* are the residuals from regressions of *Co-option* on CEO tenure and *TW Co-option* on CEO tenure. Other control variables are defined in the Appendix. Intercept is included but not reported. t-statistics, given in parentheses, are based on standard errors corrected for heteroscedasticity and firm-level clustering.***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

one, sensitivity of pay to performance decreases by $296,485 or by 38% from its mean value.

The coefficient on board independence is negative and significant in models 1–4. This result, which is similar to that in Coles, Lemmon, and Wang (2011), suggests that board monitoring and CEO delta may well be substitutes in organization design.

For robustness, as we do with CEO pay, we use the average of the contemporaneous and the lagged values of the co-option measures instead of the contemporaneous values alone. Also, we use industry year fixed effects

instead of firm year fixed effects. The results in both cases are similar to our base case result, in that the coefficient on the co-option measures continues to be negative but insignificant at conventional levels.

In sum, we find weak evidence in support of the hypothesis (H3) that higher co-option is associated with lower CEO pay-performance sensitivity (PPS). CEO pay and PPS, however, cannot be viewed as independent of each other. CEOs would demand higher pay if greater risk is imposed on them in the form of higher PPS. Instead, if anything, co-option is associated with lower exposure of CEO wealth to risk. Thus, our finding that co-option is associated with higher pay, but similar or even lower PPS, is consistent with co-opted boards adopting more liberal compensation policies that are favorable to the CEO.

3.4 Co-option and investment

H4 proposes that co-option is positively associated with investment. We examine this using the following specification:

$$Investment = \mu_0 + \mu_1 Co\text{-}option + \mu_2 Independence + j(Other\ Controls) + \varepsilon_4.$$

The dependent variable is capital expenditure scaled by assets. In addition to board independence, the other key independent variables are based on Coles, Daniel, and Naveen (2006) and include vega, delta, cash compensation, CEO tenure, Tobin's q, firm size, free cash flow to assets, sales growth, leverage, and stock return.

Table 5 shows the results. In model 1, we use *Co-option* as our key variable of interest. The coefficient on *Co-option* is positive and statistically significant (= 0.005, p-value = 0.014). In terms of economic significance, the coefficient indicates that when *Co-option* increases by one standard deviation, investment increases by 3% relative to the mean. When *Co-option* increases from zero to one, investment increases by 10% relative to its mean value.

In model 2, as the dependent variable we use *TW Co-option* instead of *Co-option*. Once again, the coefficient on *TW Co-option* is positive and statistically significant. In the last two columns we use *Residual Co-option* and *Residual TW Co-option*, respectively. Our results are similar in sign and significance.

In all specifications, we find that the fraction of independent directors is negatively associated with investment. The results on the other control variables are consistent with prior literature. Consistent with Coles, Daniel, and Naveen (2006), we find the coefficient on vega is negative (although not significant at conventional levels), and the coefficient on delta is positive. Higher Tobin's q and higher free cash flow are associated with more investment.

As with CEO pay and pay-performance sensitivity, we confirm that the results are qualitatively similar if we use the average of the contemporaneous and the lagged values of the co-option measures instead of the contemporaneous values alone. When we use industry-year fixed effects instead of firm-year fixed effects, however, we find that the coefficient on co-option is not significant.

Table 5
Effect of co-option on investment

	Dependent variable = Investment			
	Co-option measure used:			
	Co-option (1)	TW Co-option (2)	Residual Co-option (3)	Residual TW Co-option (4)
Co-option measure$_t$ (μ_1)	0.005**	0.006**	0.005**	0.006**
	(2.5)	(2.2)	(2.4)	(2.0)
Independence$_t$	−0.007*	−0.006*	−0.007*	−0.006*
	(−1.9)	(−1.8)	(−1.9)	(−1.7)
CEO Vega$_{t-1}$ ($\times 10^{-6}$)	−2.730	−2.60	−2.730	−0.260
	(−1.2)	(−1.2)	(−1.2)	(−1.2)
CEO Delta$_{t-1}$ ($\times 10^{-6}$)	0.830**	0.830**	0.831**	0.825**
	(2.0)	(2.0)	(2.0)	(2.0)
CEO Tenure$_t$	−0.000	−0.000	0.000	0.000
	(−1.5)	(−1.4)	(0.0)	(0.2)
CEO Cash Compensation$_t$ ($\times 10^{-6}$)	0.509**	0.504**	0.509**	0.506**
	(2.2)	(2.2)	(2.2)	(2.2)
Tobin's q_t	0.005***	0.005***	0.005***	0.005***
	(8.6)	(8.5)	(8.6)	(8.5)
Firm Size$_t$	0.002	0.002	0.002	0.002
	(1.6)	(1.6)	(1.6)	(1.6)
FCF/Assets$_t$	0.034***	0.034***	0.034***	0.034***
	(5.3)	(5.3)	(5.3)	(5.3)
Sales Growth$_t$	0.001	0.001	0.001	0.001
	(0.9)	(0.9)	(0.9)	(0.9)
Leverage$_t$	−0.017***	−0.017***	−0.017***	−0.017***
	(−3.6)	(−3.6)	(−3.6)	(−3.6)
Stock Returns$_t$	−0.010***	−0.010***	−0.010***	−0.010***
	(−14.4)	(−14.4)	(−14.4)	(−14.4)
Fixed effects	firm, year	firm, year	firm, year	firm, year
Observations	16,041	16,041	16,041	16,041
R^2	0.148	0.147	0.147	0.147

The table presents regressions of *Investment*, defined as capital expenditure scaled by assets. *Co-option* is the number of directors appointed after the CEO assumed office ("co-opted" directors) divided by the board size. *TW Co-option* is the sum of tenure of co-opted directors divided by the sum of tenure of all directors. *Residual Co-option* and *Residual TW Co-option* are the residuals from regressions of *Co-option* on CEO tenure and *TW Co-option* on CEO tenure. Other control variables are defined in the Appendix. Intercept is included but not reported. t-statistics, given in parentheses, are based on standard errors corrected for heteroscedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Overall, the results in this subsection support the hypothesis that CEOs that have captured the board to a greater extent are able to invest more than otherwise would have been the case. At this juncture, based on our results, we are unable to discern whether such investment, which likely increases firm size and the economic span of control of top management, is necessarily inconsistent with shareholder interests. On this question, however, in separate, independently developed work, Pan et al. (2013) document that investment increases with the extent of CEO control of the board, as proxied by a measure of co-option similar to ours. They also find that the quality of investment (captured by the market reaction to acquisition announcements) deteriorates over the CEO's tenure and that this deterioration is related to the CEO's control of the board. Thus, the relation we document between CEO investment and *Co-option* potentially arises because ". . . the CEO overinvests when he gains more control over his board" (Pan et al. 2013, their abstract).

4. Endogeneity

Endogeneity is an important concern in any study on corporate governance (Coles, Lemmon, and Wang 2011). In particular, it is possible that both co-option and pay are high because of an unobserved (and hence omitted) variable. Because we include firm fixed effects in all our specifications, we control for omitted variables that are firm-specific and time-invariant. If the omitted variable is time-varying or not firm-specific, however, and is correlated with co-option, this would cause the error term in the outcome equation to be correlated with co-option, rendering OLS invalid. Another source of endogeneity is that both co-option and our variables of interests, such as pay, are determined in equilibrium simultaneously. One solution would be a valid instrument for the endogenous variable (*Co-option*). It is difficult, however, to find an instrument that is related to co-option but is not related to CEO pay or the other outcomes we examine. As an alternative to firm fixed effects specifications, we turn to a natural experiment to help us address endogeneity concerns.

We exploit the rules enacted in 2002 by NASDAQ and NYSE, requiring all listed firms to have a majority of independent directors on their board.[18] Because these rules were adopted shortly after the passage of SOX, we refer to the period following the proposal of the new stock exchange rules (2002–2010) as the post-SOX period. Pre-SOX noncompliant firms were required to increase board independence after implementation of the new listing requirements, and these firms chose to add new independent directors to the board (Linck, Netter, and Yang 2009). This resulted in an exogenous increase in co-option.[19]

To isolate the causal impact of co-option, we somewhat modify the Bertrand and Mullainathan (2003) difference-in-differences (DID) methodology. The key difference is that we allow for the possibility that SOX and associated exchange provisions have a direct effect on turnover-performance-sensitivity, pay, pay-performance-sensitivity, and investment, as well as an effect through co-option. This is because other regulations and political pressure arising from SOX were likely to have affected monitoring through numerous channels.[20] For example, under SOX and the associated exchange provisions: complete independence was mandated for the compensation, audit, and monitoring committees; a director with financial expertise was required on the audit committee; in addition to their regular sessions, boards were required to meet without management; CEO/CFO certification of accounting statements was required; and there was a general increase in media scrutiny of all firms.

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[18] A detailed time line is available in Chhaochharia and Grinstein (2007).

[19] We are particularly grateful to the referee for suggesting this specific line of attack and for shaping some of the other aspects of our approach to ameliorating endogeneity concerns.

[20] Indicative evidence on the effects of SOX on pay and turnover is presented in Chhaochharia and Grinstein (2007), Carter, Lynch, and Zechman (2009), and Kaplan and Minton (2012).

Because of this complication, we modify the typical DID setup to isolate the effect of co-option (we term this the "clean" effect). The typical DID setup for examining pay, for example, would be to regress pay on three dummy variables: *Post-SOX*, *Noncompliant*, and the interaction term *Post-SOX* × *Noncompliant*, where *Post-SOX* is an indicator variable that equals one if the year is 2002 or later, and equals zero otherwise, and *Noncompliant* is an indicator variable that equals one if the firm was not in compliance in 2001, and equals zero otherwise. *Co-option* is not included in the above specification, and the focus is on the coefficient on *Post-SOX* × *Noncompliant*. This coefficient, however, captures both the effect we want to isolate (through the exogenous shock to co-option) and direct effect (through other channels) of SOX. To assess the impact of co-option, we estimate the modified regression, which includes *Co-option* and the interaction of *Co-option* with the three dummy variables:

$$\begin{aligned} Pay = {} & \beta_0 + \beta_1 Co\text{-}option + \beta_2 Post\text{-}SOX \times Co\text{-}option \\ & + \beta_3 Noncompliant \times Co\text{-}option + \beta_4 Post\text{-}SOX \times Noncompliant \times Co\text{-}option \\ & + \beta_5 Post\text{-}SOX + \beta_6 Noncompliant + k(Other\ Controls) + \varepsilon_5 \end{aligned}$$

The controls in the specification include the independent variables used in the pay regressions in Table 3 and the individual dummies, as well as the interactions of all the independent variables with the three key dummy variables: *Post-SOX*, *Noncompliant*, and *Post-SOX* × *Noncompliant*.

Panel A of Table 6 provides an estimate of the sensitivity of pay to co-option for the four subsamples of firms: compliant firms in the pre-SOX period, noncompliant firms in the pre-SOX period, compliant firms in the post-SOX period, and noncompliant firms in the post-SOX period. The effects are estimated by taking the partial derivative of *Pay* with respect to *Co-option* in the equation above. As can be seen from the table, β_1 and $\beta_1+\beta_3$ represent the sensitivities for compliant and noncompliant firms, respectively, in the pre-SOX period. Both sensitivities include the bias due to endogeneity. The sensitivity of *Pay* to *Co-option* for compliant firms in the post-SOX period is given by $\beta_1 + \beta_2$, and this includes not only the effect of bias but, in addition, the direct effects of SOX. The sensitivities for firms in all three groups are subject to bias because of the standard set of reasons that give rise to the endogeneity problem. We allow this bias to differ by whether the firm was compliant (superscript C) or not compliant (superscript NC) pre-SOX, though we do restrict Bias^C to be the same both pre- and post-SOX.

The subsample of primary interest is the noncompliant post-SOX group. This group contains firms facing the exogenous shock to co-option. The sensitivity for this subsample ($= \beta_1 + \beta_2 + \beta_3 + \beta_4$) is contaminated by the SOX effects through channels other than co-option and thus represents the combined effect of both co-option and SOX on the variable of interest (= "Clean + SOX"). As can be seen from the table, the typical DID estimate reported in the lower right cell (β_4) does not yield the clean estimate, but rather the negative of Bias^{NC}. The "clean" estimate, arising from the exogenous increase in co-option, forced on noncompliant firms through a mandated increase in board independence, is given by $\beta_1 + \beta_3 + \beta_4$.

Table 6
A natural experiment

Panel A: Representative example of a regression of CEO Pay on co-option

	Estimated coefficient (= ∂Pay/∂Co-option):		
	Pre-SOX period (1996–2001)	Post-SOX period (2002–2010)	Difference
Compliant	β_1 (Clean + Bias^C)	$\beta_1+\beta_2$ (Clean + Bias^C + SOX)	β_2 (SOX)
NonCompliant	$\beta_1+\beta_3$ (Clean + Bias^{NC})	$\beta_1+\beta_2+\beta_3+\beta_4$ (Clean + SOX)	$\beta_2+\beta_4$ (SOX – Bias^{NC})
Difference	β_3 (Bias^{NC} – Bias^C)	$\beta_3+\beta_4$ (–Bias^C)	β_4 (–Bias^{NC})

Clean = (Clean + SOX) – SOX = $\beta_1+\beta_2+\beta_3+\beta_4-\beta_2=\beta_1+\beta_3+\beta_4$

Panel B: Estimates of the clean effects of co-option

Coefficient estimate (z-statistic/t-statistic)	Results from base case	"Clean" estimate
Table 2: Model 1, α_1 (turnover-performance sensitivity)	2.00*** (3.4)	8.92* (1.7)
Table 3: Model 1, θ_1 (pay)	0.23*** (4.0)	0.84*** (3.3)
Table 4: Model 1, γ_1 (pay-performance sensitivity)	−284.85 (−1.6)	836.34 (1.2)
Table 5: Model 1, μ_1 (investment)	0.006** (2.5)	0.010 (1.0)

This table reports the effect of co-option on turnover-performance sensitivity, pay, pay-performance sensitivity, and investment using a natural experiment. As a basis for a natural experiment, we exploit the 2002 NYSE/NASDAQ listing requirement that a majority of the board be comprised of independent directors. Firms reacted by adding independent directors, thus imposing an exogenous shock to co-option on firms that were noncomplaint as of 2001. We essentially follow the difference-in-differences methodology, but with one key difference. We allow for the possibility that SOX and associated listing provisions have a direct effect on turnover-performance sensitivity, pay, pay-performance sensitivity, and investment, in order to isolate the "clean" or causal effect of co-option on these four attributes. We illustrate our methodology using pay.

$$Pay = \beta_0 + \beta_1 Co\text{-}option + \beta_2 Post\text{-}SOX \times Co\text{-}option + \beta_3\, Noncompliant \times Co\text{-}option$$

$$+ \beta_4 Post\text{-}SOX \times Noncompliant \times Co\text{-}option + \beta_5 Post\text{-}SOX + \beta_6 Noncompliant + k(other\ controls) + error$$

Post-SOX is an indicator variable that equals one if it is the post-SOX period, 2002–2010. *Noncompliant* is an indicator variable that equal one if the firm was noncompliant as of 2001. The controls in the specification include the usual independent variables used in the pay regressions, year fixed effects, and the interactions of all the independent variables with the three key dummy variables: *Post-SOX*, *Noncompliant*, and *Post-SOX* × *Noncompliant*. Panel A of Table 6 provides an estimate of the sensitivity of pay to co-option for the four subsamples of firms: compliant firms in the pre-SOX period, noncompliant firms in the pre-SOX period, compliant firms in the post-SOX period, and noncompliant firms in the post-SOX period. The firms in the noncompliant post-SOX group face the exogenous shock to co-option. The sensitivities are estimated by taking the partial derivative of *Pay* with respect to *Co-option*. "SOX" represents how changes in the listing requirements affected pay through avenues other than through co-option. "Bias" arises from the standard endogeneity problem. We allow this bias to differ by whether the firm was compliant (superscript C) or not compliant (superscript NC) pre-SOX, though we do restrict Bias^C to be the same both pre- and post-SOX. Panel B estimates the "clean" effect. The corresponding base case estimates from Model 1 from Tables 2–5 are reported for comparison purpose. t-statistics, given in parentheses, are based on standard errors corrected for heteroscedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Panel B of Table 6 provides the results. For brevity, we present only the clean estimates for the total impact of co-option on each of the four variables of interest (turnover-performance sensitivity, pay, pay-performance sensitivity, and investment). For ease of comparison, we report results from the base case regressions (model 1 of Tables 2–5). In terms of the notation in the regression specifications defined above, we report clean estimates of α_1,θ_1,γ_1, and μ_1.

For turnover and pay level, relative to the base case, the estimates based on an exogenous shock to co-option have the same sign and similar (though not quite as strong) statistical significance. The clean estimate pertaining to the effect of co-option on CEO PPS, like that in the base case, is statistically insignificant. The clean effect of co-option on investment policy is still positive, but statistically weaker (t-statistic = 1.0 vs. 2.5) than the estimated coefficient from the base case.

5. Are All Independent Directors Equally Relevant for Board Monitoring?

In this section, we examine whether the monitoring effectiveness of directors varies depending on whether or not they are independent and by whether or not they are co-opted.

5.1 Co-option: Independent versus nonindependent directors

Our results thus far indicate that board capture is associated with weaker monitoring. The measures of co-option used above, however, do not differentiate between directors who are independent versus those who are not. Indeed, the notion that employee and affiliated directors are co-opted is the basis for using board independence as a measure of monitoring (e.g., Weisbach 1988; Byrd and Hickman 1992). The question remains as to whether co-option blunts the monitoring effectiveness of independent directors. If we find that co-opted directors who are independent are also weak monitors, then it would suggest that the independence measure traditionally used in the literature does not capture the disposition of the board to provide effective oversight and monitoring and thus can be improved. To examine this question, we further refine *Co-option*. *Co-opted Independence* is defined as the proportion of the board that consists of co-opted directors who are independent, whereas *Co-opted Non-independence* is defined as the proportion of the board that consists of co-opted directors who are not independent. These two measures differentiate between directors who are employees or affiliated versus those who are supposedly independent.

Table 7 documents the results. In Panel A, for ease of comparison, we reproduce our base case results on *Co-option* (model 1 of Tables 2–5). We report the coefficients and associated t-statistics only for our primary variables. Panels B and C report our results wherein we replace *Co-option* by *Co-opted Independence* and *Co-opted Non-Independence*, respectively. In Panel B, we find that *Co-opted Independence* is associated with attenuated turnover-performance sensitivity, higher pay, lower CEO delta, and higher investment. Per Panel C, *Co-opted Non-Independence* is associated with

Table 7
Are all co-opted directors similar? Co-opted independent versus co-opted nonindependent directors

	Dependent variable:			
Coefficient estimate (*z*-statistic/*t*-statistic)	Forced Turnover (1)	log(CEO Pay) (2)	CEO Delta (3)	Investment (4)
Panel A: Base case (co-option)				
Co-option × Prior Abnormal Return	**2.021***** **(3.4)**			
Co-option	3.817*** (6.9)	**0.223***** **(3.9)**	**−296.485** **(−1.6)**	**0.005**** **(2.5)**
Panel B: Co-opted Independence				
Co-opted Independence × Prior Abnormal Return	**3.138***** **(3.7)**			
Co-opted Independence	4.137*** (6.1)	**0.262***** **(3.9)**	**−499.608**** **(−2.3)**	**0.005**** **(2.1)**
Panel C: Co-opted Non-Independence				
Co-opted Non-Independence × Prior Abnormal Return	**2.306***** **(2.7)**			
Co-opted Non-Independence	2.245** (2.0)	**0.130** **(1.1)**	**350.636** **(1.1)**	**0.010**** **(2.0)**
Other controls for all panels as in:	Table 2, model 1	Table 3, model 1	Table 4, model 1	Table 5, model 1
Observations	1,899	12,036	11,539	16,041

The table reports results for experiments in Model 1 of Tables 2–5 re-estimated using *Co-opted Independence* (Panel B) and *Co-opted Non-Independence* (Panel C) rather than *Co-option*. For ease of comparison, Panel A reproduces the results from base case results. *Co-option* is the number of directors appointed after the CEO assumed office ("co-opted" directors) divided by the board size. *Co-opted Independence* is the proportion of the board that consists of co-opted directors who are independent. *Co-opted Non-Independence* is the proportion of the board that consists of co-opted directors who are not independent. Column 1 reports results on turnover-performance sensitivity. Column 2 reports results on CEO pay. Column 3 addresses CEO pay-performance sensitivity. Column 4 estimates specifications for investment. *t*-statistics, given in parentheses, are based on standard errors corrected for heteroscedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

attenuated turnover-performance sensitivity and higher investment but has no effect on pay or pay-performance sensitivity. Thus, our overall results on co-option appear to be driven by independent co-opted directors, rather than by nonindependent co-opted directors. We conclude that, once a director is co-opted, the independence of the director does not matter from a monitoring perspective. This likely explains why the literature has found little uniform evidence on the relation between board independence and various measures of firm performance and structure.

5.2 Independence: Co-opted versus non-co-opted directors

Our results to this point suggest that (i) independent directors typically do not have an effect on monitoring effectiveness in the presence of co-option (Tables 2–5) and (ii) co-opted directors, even those that are independent, are

Table 8
Are non-co-opted independent directors more effective monitors?

Panel A: Comparison of co-option and independence measures

		Co-opted? yes		Co-opted? no	
Independent?	yes	Co-opted Independence	+	Non-Co-opted Independence	= Independence
		+			
	no	Co-opted Non-Independence		Non-Co-opted Non-Independence	
		= Co-option			

Panel B: Results for Model 1 (Tables 2–5) using *Non-Co-opted Independence* as primary explanatory variable

	Dependent variable:			
	Forced Turnover (1)	log(CEO Pay) (2)	CEO Delta (3)	Investment (4)
Non-Co-opted Independence × Prior Abnormal Return	**−2.899*** **			
	(−3.4)			
Non-Co-opted Independence	−4.314***	**−0.210*** **	**358.878***	**−0.006** **
	(−6.2)	**(−3.2)**	**(1.9)**	**(−2.4)**
Other controls as in:	Table 2, model 1	Table 3, model 1	Table 4, model 1	Table 5, model 1
Observations	1,899	12,036	11,539	16,041

Panel A depicts the overlap and dissimilarity between co-option and independence. *Co-option* is the number of directors appointed after the CEO assumed office ("co-opted" directors) divided by the board size. *Independence* is the proportion of the board that consists of independent directors. *Co-opted Independence* is the proportion of the board that consists of co-opted directors who are independent. *Co-opted Non-Independence* is the proportion of the board that consists of co-opted directors who are not independent. The sum of *Co-opted Independence* and *Co-opted Non-Independence* equals *Co-option*. *Non-Co-opted Independence* is the proportion of the board that consists of independent directors who were already on the board when the CEO assumed office. The sum of *Co-opted Independence* and *Non-Co-opted Independence* equals *Independence*. *Non-Co-opted Non-Independence* is the proportion of the board that consists of nonindependent directors who were already on the board when the CEO assumed office. Panel B reports results for experiments in Model 1 of Tables 2–5 re-estimated by replacing *Co-option* and *Independence* using *Non-Co-opted Independence* as the key explanatory variable. Column 1 reports results on turnover-performance sensitivity. Column 2 reports results on CEO pay. Column 3 addresses CEO pay-performance sensitivity. Column 4 estimates specifications for investment. *t*-statistics, given in parentheses, are based on standard errors corrected for heteroscedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

weak monitors (Table 7). It is likely, therefore, that only independent directors who are not co-opted by the CEO are effective monitors. To test this formally, we introduce a second new measure of board composition, *Non-Co-opted Independence*. We define this as the proportion of the board that consists of independent directors who were already on the board when the CEO assumed office.

Panel A of Table 8 depicts the overlap and dissimilarity between our measures of co-option and independence. As can be seen, the sum of *Co-opted Independence* and *Co-opted Non-Independence* equals *Co-option*, whereas the sum of *Co-opted Independence* and *Non-Co-opted Independence* equals *Independence*.

Figure 2 plots how the various board composition measures described above change over CEO tenure. As expected, *Co-option* increases with CEO tenure. This is because in each director election cycle the CEO has the opportunity to

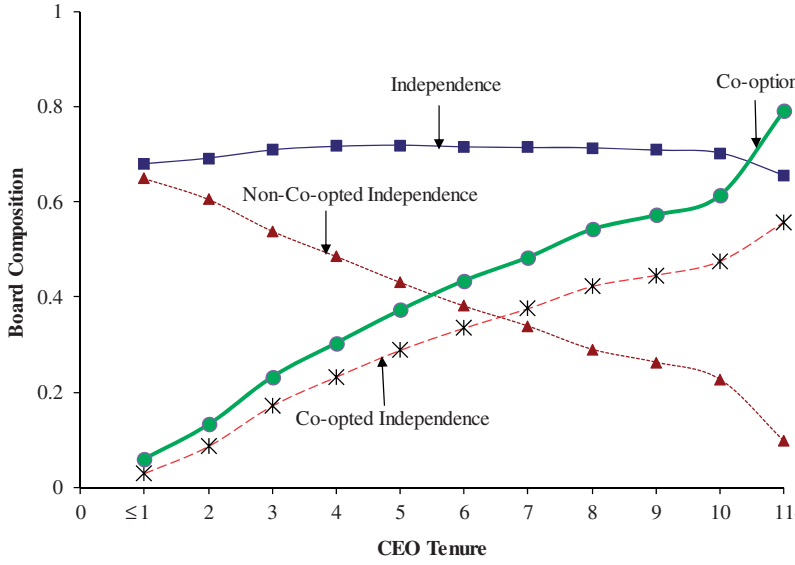

Figure 2
Board composition and CEO tenure
The figure plots various measures of board composition and co-option against CEO tenure. *Co-option* is the number of directors appointed after the CEO assumed office ("co-opted" directors) divided by the board size. *Independence* is the proportion of the board that consists of independent directors. *Co-opted Independence* is the proportion of the board that consists of co-opted directors who are independent. *Non-Co-opted Independence* is the proportion of the board that consists of independent directors who were already on the board when the CEO assumed office. The sum of *Co-opted Independence* and *Non-Co-opted Independence* equals *Independence*. CEOs are put in eleven groups based on their tenure, where CEO tenure is measured as fiscal year-end date minus the date at which the executive became the CEO. All CEOs with $\leqslant$1 year are put in the first group, . . . , CEOs with tenure greater than 9 years but $\leqslant$10 years are put in the tenth group, and CEOs with greater than 10 years are put in the eleventh group.

affect the nomination of directors to the board. *Independence*, however, remains more or less constant (at around 69% in our sample). Thus, while on the surface it appears that board independence is high, the composition of the board as represented by co-option gradually tilts in the CEO's favor over time. Further, a closer look at the two components of *Independence* indicates that as CEO tenure increases, *Co-opted Independence* increases, whereas *Non-Co-opted Independence* decreases. This arises as the CEO replaces previously appointed independent with new independent directors. This suggests that the monitoring effectiveness of the board weakens over the CEO's tenure. We explore this issue by estimating our base case regressions with both *Co-option* and *Independence* replaced by *Non-Co-opted Independence* as the key dependent variable.

Panel B of Table 8 reports the results. Consistent with the idea that independent directors who are not co-opted are better monitors, we find *Non-Co-opted Independence* is associated with higher sensitivity of CEO turnover to performance, lower pay levels, higher sensitivity of pay to performance, and lower investment.

Overall, these results are consistent with non-co-opted directors being effective monitors. Moreover, it appears that not all independent directors are the same. Differentiating among independent directors by whether or not they are co-opted appears to be a more incisive way to explain monitoring intensity of the board. Independent directors whose selection was influenced by the CEO appear to be more sympathetic to the CEO. On the other hand, non-co-opted independent directors appear to be effective monitors. Relative to aggregate board independence, the representation of non-co-opted independent directors on the board appears to be a sharper measure of monitoring effectiveness.

6. Alternative Interpretations and Other Robustness Checks

6.1 Is Co-option capturing the effect of CEO tenure?

Figure 2 shows that *Co-option* increases over the CEO's tenure. It is likely that CEO power increases with tenure (e.g., see Weisbach 1988; Ryan and Wiggins 2004). Thus, CEO tenure may be correlated with both power and *Co-option*, and it is possible that our results on the effect of co-option are due to the positive association between co-option and CEO tenure. We perform three tests and conclude that CEO tenure is not causing our results.

First, our base case specifications already include CEO tenure as a control variable in all our regression specifications. Thus, the effect of co-option that we document earlier is after controlling for CEO tenure.

Second, in the specifications in Tables 2–5 (model 1), instead of *Co-option*, we use *Residual Co-option*, which is the residual from a regression of *Co-option* on CEO tenure. The residual now no longer proxies for power arising from tenure but is a proxy for power related to co-option of the board. We find that all results on *Residual Co-option* are similar to our results on *Co-option*.[21]

Third, we drop *Co-option* from all specifications and use only tenure as our measure of the CEO's power over the board. If it is true that our results on *Co-option* somehow obtain only because of the positive correlation between tenure and *Co-option*, and do not reflect the true effect of board capture, then when we drop *Co-option* from the regressions, our results on tenure should be similar to what we reported earlier with *Co-option*. That is, we should find that tenure decreases CEO turnover-performance sensitivity, increases CEO pay, decreases CEO pay-performance sensitivity, and increases investment. The results, however, do not support the idea that co-option is only capturing the effect of CEO tenure on monitoring. We find that CEO tenure has a positive effect on turnover-performance sensitivity, similar to the effect of co-option. In contrast to the effect of co-option, however, CEO tenure has no effect on pay or investment and a *positive* association with pay-performance sensitivity.

[21] The results are similar when we use *Residual TW Co-option* (the residual from a regression of *TW Co-option* on CEO tenure) instead. In the interests of conciseness, we do not tabulate the results here or below. All results are available from the authors on request.

Overall, the analysis in this section suggests that our results on *Co-option* are not driven by the effects of CEO tenure.

6.2 Is Co-option capturing the effect of director inexperience?

Another possibility is that our measures of co-option reflect the inexperience of co-opted directors, as indicated by the average tenure of directors on the board. Higher co-option could mean that there are several new directors and these new directors are less experienced (at least on that board). They may not be sympathetic to the CEO but, perhaps, they are slower to react to poor performance and more ready to award favorable pay packages and approve investment projects proposed by the CEO. Average director tenure, thus, would be correlated with co-option as well as with director inexperience. To isolate the effect of co-option, as with CEO tenure, we perform three different tests. First, we include average director tenure in our baseline specifications. The results are similar to our base case regressions. Second, we estimate the residual from a regression of *Co-option* on average director tenure and then include this residual (termed "*Director-Tenure-Adjusted Co-option*") in the regressions instead of *Co-option*, along with necessary interactions. We find that our results on the adjusted co-option measure are similar to our results on *Co-option*. Finally, we exclude *Co-option* from our baseline specifications but include average director inexperience, as well as the necessary interaction terms. We find that average director tenure has no effect on turnover-performance sensitivity, pay-performance sensitivity, or investment, and has a *negative* effect on pay.

Overall, there is no evidence that director inexperience causes a spurious relation between co-option and monitoring effectiveness.

6.3 Is Co-option capturing the "Involved CEO" effect of Shivdasani and Yermack?

Shivdasani and Yermack (1999) construct an indicator variable, *Involved CEO*, which equals one if the firm has a nominating committee and the CEO is part of the committee or if the firm has no nominating committee. In the latter instance, the presumption is that if there is no nominating committee the CEO will be more centrally involved in director selection. Shivdasani and Yermack show that when a CEO is involved in director selection as part of the nominating committee, then he or she tends to appoint more gray outside directors and fewer independent outside directors. Shivdasani and Yermack do not examine the effect of *Involved CEO* on CEO turnover-performance sensitivity, pay, pay-performance sensitivity, or firm investment. It is possible, however, that our co-option variable does not provide additional explanatory power relative to *Involved CEO* because both variables proxy for the same underlying construct, the extent of the CEO's influence on the board.

The data do not support this conjecture. First, following the 2002 NYSE and the NASDAQ listing requirements for completely independent nominating committees, the Shivdasani-Yermack measure should, by definition, be zero for

all firms. Consistent with this, we find that the mean of *Involved CEO* drops from about 54% in 1998 (the first year for which RiskMetrics has data on subcommittees) to 1.9% in 2010, and the correlation between *Co-option* and *Involved CEO* over the entire time interval is only 0.11.

To verify that our co-option measure provides additional explanatory power relative to *Involved CEO*, we perform three different tests. First, we include *Involved CEO* as an additional explanatory variable in all our regressions. We find that the results on *Co-option* are qualitatively similar. Second, we estimate the residual from a regression of *Co-option* on *Involved CEO*. The residual, which we term *Involved-CEO-Adjusted Co-option*, is uncorrelated with *Involved CEO* by construction. We include this residual along with the usual interactions in model 1 of Tables 2–5. We find that the results using *Involved-CEO-Adjusted Co-option* are qualitatively similar to our results using *Co-option*. Third, we drop *Co-option* from our regressions but include *Involved CEO* and relevant interactions. We find that *Involved CEO* is not associated with turnover-performance sensitivity, pay levels, and pay-performance sensitivity but is positively associated with investment. Overall, the evidence does not support the notion that *Involved CEO* drives our results. This in no way negates the results found in Shivdasani and Yermack. Indeed, our findings on co-option are consistent with the premise of their paper. Our results suggest that rule changes enacted subsequent to the sample period they analyze reduce the relevance and explanatory power of their measure.

7. Conclusion

The economics of director selection and performance are more complex than standard, widely used measures of board structure, such as board independence, would suggest. Director co-option, by way of loyalty and allegiance to the CEO who is associated with appointment of a director, is likely to affect what that director actually does. Directors who are appointed by a CEO, regardless of whether they are independent in the legal sense, are more sympathetic to that CEO (Mace 1971; Lorsch and MacIver 1989; Shivdasani and Yermack 1999).

We propose and empirically examine two new measures of board composition that reflect the extent of the CEO's influence on directors. *Co-option* is the fraction of the board comprised of directors appointed after the CEO assumes office. We provide evidence that co-option reduces the monitoring effectiveness of the board of directors. We find that (i) the sensitivity of CEO turnover to performance decreases with co-option, (ii) CEO pay increases with co-option, but the sensitivity of CEO pay to firm performance is unaffected, and (iii) investment increases with co-option.

Testing robustness, the results are similar for various alternative measures of co-option. These results obtain after we control for the traditional measure of board independence (the fraction of independent directors). Our results

are robust to our best attempts to address endogeneity. We use firm fixed effects in our main specifications. Alternatively, we use the changes in NYSE and NASDAQ listing requirements, which led to an exogenous increase in co-option in firms that were noncompliant as of the announcement. Using this clean source of identification, we find qualitatively similar results on the effects of co-option on turnover, pay, and pay-performance sensitivity, although we find a weaker estimated effect on investment.

Further analysis reveals that independent directors that are co-opted behave as though they are not independent. Thus, we refine the conventional measure of board independence to be the proportion of board members who are independent directors but are not co-opted. This second measure, *Non-Co-opted Independence*, focuses on independent directors who are most likely to be truly independent. We find that boards with a higher proportion of non-co-opted independent directors provide more effective monitoring. Higher *Non-Co-opted Independence* is associated with higher sensitivity of CEO turnover to performance, lower pay levels, higher sensitivity of pay to performance, and lower investment.

In terms of future research, it is possible that there are offsetting economic benefits to co-option. For example, CEOs with more co-opted boards may be more insulated from the threat of dismissal and, consequently, may invest more in firm-specific human capital (Burkart, Gromb, and Panunzi 1997) or in risky projects with long term payoffs (Deangelo and Rice 1983; Stein 1988). Proxies for such human capital intensity (HCI) include within-industry heterogeneity in growth rates, R&D intensity, and firm-specific risk. In regression results not reported here, we find some preliminary evidence that *Co-option* is positively associated with these HCI proxies and that Tobin's *q* increases in *Co-option* for high-HCI firms. We defer presenting those empirical results in detail until we develop defensible conceptual and econometric foundations for the empirical analysis.

More broadly, beyond turnover-performance sensitivity, pay level, executive pay-performance-sensitivity, and investment policy—the issues we examine herein—there is a large literature on the importance of board monitoring in empirical corporate finance. When appropriate, researchers can re-examine prior research questions and the corresponding experiments using new measures, *Co-option* and *Non-Co-opted Independence*. For example, experiments that regress structure on structure would relate our measures of board capture to takeover bids (hostile or friendly, as bidder or target), takeover defenses (such as poison pills, antitakeover charter amendments, staggered boards, blank-check preferred stock, and supermajority voting rules), state takeover law (as determined by domicile), product market characteristics (including monopoly power and regulatory structure), financial policy (e.g., capital structure, cash holdings, and hedging), institutional ownership, investment policy (such as firm focus and asset intangibility), dividend policy, and other firm characteristics (such as size and whether

it is organized around product markets or functional areas). Performance-on-structure designs, assuming endogeneity concerns can be overcome, would examine the relation between Tobin's q, accounting performance, or stock market performance, for example, and *Co-option* or *Non-Co-opted Independence*. Finally, our co-option measures may well have power to explain cross-sectional variation in event study returns to announcement of M&A events or adoption of defensive devices. There appears to be significant remaining empirical opportunity for researchers interested in board monitoring.

Appendix

A.1. Variable Definitions

The governance data used in this study are from RiskMetrics (formerly Investor Responsibility Research Center or IRRC) for the period 1996–2010. Compensation data are from Execucomp; accounting data are from Compustat; and stock return data are from CRSP. We also give the mnemonics used by Compustat to define these variables.

A.1.1 Co-option-related variables

Variable	*Definition*
Co-opted director	Director who joined the board after the CEO assumed office
Co-option	Number of co-opted directors / Board Size = Co-opted Independence + Co-opted Non-Independence
TW Co-option (Tenure-Weighted Co-option)	Sum of tenure of co-opted directors divided by the sum of tenure of all directors
Residual Co-option	Residual from a regression of Co-option on CEO tenure
Residual TW Co-option	Residual from a regression of TW Co-option on CEO tenure
Co-opted Independence	Number of co-opted independent directors / Board Size
Co-opted Non-Independence	Number of co-opted non-independent directors / Board Size
Non-Co-opted Independence	Number of independent directors who were already on the board before the CEO assumed office / Board Size

A.1.2 Governance-related variables

Variable	*Definition*
Board Size	Total number of directors on the board
Independence	Number of independent directors / Board size = Co-opted Independence + Non-Co-opted Independence
Outside Director Ownership	Cumulative share ownership of outside directors (available from 1998)
GIM Index	Governance index as defined in Gompers, Ishii, and Metrick (2003)
Female Director	= 1 if board has a female director; = 0 otherwise (available from 1997)
Average Director Tenure	Average tenure of all directors on board
CEO Ownership	Shares held by the CEO / Number of shares outstanding
CEO Duality	= 1 if CEO is also the Chairman; = 0 otherwise

A.1.3 CEO-related variables

Variable	*Definition*
Forced Turnover	= 1 if the departing CEO was younger than 60 years of age; = 0 otherwise
CEO Pay	Total annual pay (Execucomp: TDC1)
CEO Pay-Performance Sensitivity (Delta)	Expected dollar change in CEO wealth for a 1% change in stock price (using entire portfolio of stocks and options) computed as in Core and Guay (2002)
CEO Vega	Expected dollar change in CEO wealth for a 0.01 change in stock return volatility (using entire portfolio of options) computed as in Guay (1999)
CEO Tenure	Tenure as CEO of the firm = fiscal year end date minus date became CEO
CEO Cash Compensation	Sum of salary and bonus (Execucomp: TCC)
Involved CEO	= 1 if the firm has no nominating committee, or if the firm has a nominating committee and the CEO is a member; = 0 otherwise

A.1.4 Firm-level variables

Variable	**Definition**	**Mnemonic**
Investment	Capital Expenditure / Assets	capx / at
Return	Stock return over the fiscal year	RET variable in Execucomp
ROA	Return on Assets = EBITDA/Assets	oibdp / at
FCF/Assets	Free cash flow / Assets	(oancf – dvc – dvp) / at
Leverage	Total Debt / Assets	(dltt + dlc) / at
Firm Size	Log(sales)	Log(sale)
Tobin's q	(Assets – Book equity + Market equity) / Assets	(at – ceq + prcc_f×csho) / at
Unsystematic Risk	Standard deviation of residuals from Fama-French regression using daily stock returns	
R&D/Assets	R&D expenditure / Assets	xrd / at
Sales Growth	Sales / Lagged Sales	sale / Lagged(sale)

A.2. RiskMetrics Adjustment

Our governance data are drawn from RiskMetrics. One complication with this dataset is that there is no unique firm identifier or unique director identifier across all the years. Although RiskMetrics has CUSIP, TICKER, NAME, and RT_ID as company identifiers, none of these are unique across the entire sample period. Similarly, RiskMetrics has LEGACY_DIRECTOR_ID and DIRECTOR_DETAIL_ID as director identifiers, but these are also not unique or consistent across the entire sample period. The Wharton Research Data Services (WRDS) states on its website "Each time IRRC - ISS - RiskMetrics was acquired, new identification methods were introduced . . . Constructing a complete time series in the directors data is not easy. There is no single variable that is populated for all companies and all years." We explain below how we resolve the lack of unique firm and director identifiers. In the process, we also associate each firm-year with unique PERMNO and GVKEY, enabling us to merge the RiskMetrics dataset with CRSP and Compustat. In what follows below, variables we create are indicated in italics, whereas variables drawn from standard databases are in uppercase.

The authors are happy to provide the exact GVKEY – PERMNO – YEAR – CUSIP – MEETINGDATE – LEGACY_DIRECTOR_ID – DIRECTOR_DETAIL_ID – *Our Director ID* combinations on request, where

YEAR = fiscal year of data (from Compustat)

CUSIP = firm CUSIP (from RiskMetrics)

MEETINGDATE = annual meeting date (from RiskMetrics)

LEGACY_ DIRECTOR_ID, DIRECTOR_DETAIL_ID

= Director ID variables (from RiskMetrics)

Our Director ID = unique director ID that we create

Researchers can then merge our dataset with the RiskMetrics dataset using CUSIP – MEETINGDATE – LEGACY_DIRECTOR_ID – DIRECTOR DETAIL ID to obtain director data, use GVKEY – YEAR to merge with Compustat, use PERMNO (and MEETINGDATE if required) to merge with CRSP, use GVKEY or PERMNO to uniquely identify firms, and use *Our Director ID* to uniquely identify directors.

A.2.1 Issues with firm identifier

Starting in 2007, RiskMetrics changed the methodology used to collect data. As a result, data on directors are provided in two datasets in WRDS: a "Directors Legacy" dataset that provides data from 1996–2006 and a "Directors" dataset that provides information from 2007 and onward.

For records from 1996–2006, the CUSIP provided is the 6-digit CUSIP (we refer to this as *Cusip6*) and for records on or after 2007, the CUSIP provided is the 9-digit CUSIP. We use only the first 8 digits of this (we refer to this as *Cusip8*) because CRSP provides only 8-digit CUSIP. Additionally, *Cusip6* is the header CUSIP, which means that each time the database was updated the most recent CUSIP was applied to the entire history for each company. Since this dataset was not updated after 2007, the *Cusip6* was the 6-digit CUSIP for the firm as of 2006 (or, as of the last date the firm was included in the database). *Cusip8*, on the other hand, is the actual CUSIP for the firm for that year. In both datasets, RiskMetrics also provides the ticker (TICKER) and company name (NAME) associated with the firm for that year. We use these in addition to CUSIP to identify firms.[22]

We obtain the PERMNOs from CRSP for each firm using the methodology below. We start with the dataset DSENAMES from CRSP, which contains, for each PERMNO, the full history of changes in NCUSIP (the historic CUSIP), firm ticker (TICKER), and firm name (COMNAM). From this dataset, we create a dataset of unique NCUSIP-PERMNO combinations along with the corresponding date range over which this combination is valid (which we denote as *Start Date* and *End Date*). These dates are obtained using NAMEDT and NAMEENDT variables from CRSP. We do this separately for 8-digit NCUSIPs (*NCusip8* – PERMNO – *Start Date* – *End Date*) as well as for 6-digit NCUSIPS (*NCusip6* – PERMNO – *Start Date* – *End Date*). Similarly, we obtain, for each PERMNO, all the tickers associated with it and its relevant date range (TICKER – PERMNO – *Start Date* – *End Date*) and all the firm names associated with it and its relevant date range (COMNAM – PERMNO – *Start Date* – *End Date*).

[22] RiskMetrics includes 2 company identifiers: LEGACY_PPS_ID (before 2004) and RT_ID (after 2004). Neither of these uniquely identify firms across the entire time period. Further, they cannot be used to merge with other databases such as CRSP etc. because they are created by IRRC and by WRDS. Therefore we do not use these IDs.

We start with 23,242 unique firm-year observations in the RiskMetrics dataset (downloaded as of November 2011; recent downloads will clearly have a different number of observations). Of this, 5,810 observations correspond to the post-2007 period. We match the post-2007 RiskMetrics data with the *NCusip8* - PERMNO – *Start Date* – *End Date* dataset that we created above using *Cusip8* and MEETINGDATE variables of RiskMetrics. We obtain a unique PERMNO for each firm by ensuring that MEETINGDATE is between the *Start Date* and *End Date*. We obtain a match for 5,366 of the 5,810 observations.

For the remaining 444 (= 5,810 – 5,366) observations that do not match using *Cusip8*, we match using *Cusip6*. Specifically, we match the RiskMetrics data with the *NCusip6* - PERMNO – *Start Date* – *End Date* dataset. Again, we impose the condition that the MEETINGDATE should be between the *Start Date* and *End Date* for which the *NCusip6* is valid. We find duplicate matches for firms that have multiple traded securities, particularly dual class shares. We identify the correct PERMNO in these cases using data on dual-class shares from Gompers, Ishii, and Metrick (2010) (available through WRDS) and through Web searches. This yields 109 firm-year observations that match on *Cusip6*.

For the remaining 335 (= 444 - 109) observations, we match using RiskMetrics variable TICKER. We then identify firms for which the RiskMetrics TICKER matches with the CRSP TICKER.[23] After imposing the condition that MEETINGDATE should be between the *Start Date* and *End Date* for which the TICKER is valid, and eliminating duplicates as before, we obtain a match for 319 firm-year observations.

Finally, for the remaining 16 (= 335 - 319) observations, we match the RiskMetrics company name (NAME) with the CRSP name (COMNAM). We do not find any matches after imposing the condition that MEETINGDATE should be between the *Start Date* and *End Date* for which COMNAM is valid.

We attempt to populate these 16 observations as follows. If two firm-years have the same *Cusip8* around the year for which we have not been able to find a PERMNO, then we assign the PERMNO of the adjoining year for the missing observation. This procedure yields a match for another 10 observations.

For the pre-2007 period, we start with 17,432 firm-year observations on RiskMetrics. We follow a very similar procedure as we did for the post-2007 data, but with one minor difference. Because the *Cusip6* is the header CUSIP, we cannot match based on the actual MEETINGDATE as we did with the post-2007 data. We therefore first identify the last year for which the firm was in the RiskMetrics legacy dataset. We label this as *LastMeetingDate* and we assign this for all years for that firm.

We then merge the RiskMetrics data with our *NCusip6* - PERMNO – *Start Date* – *End Date* dataset and require the *LastMeetingDate* to be between the *Start Date* and *End Date*. We obtain a match for 17,089 observations. We pick up duplicates (308 firms on RiskMetrics match with more than one PERMNO on CRSP) because of dual-class shares. As before, we check each of these manually to identify the correct PERMNO and eliminate all duplicates.

We next match the 343 (= 17,432 - 17,089) remaining observations using TICKER. We require the MEETINGDATE to be between the *Start Date* and *End Date* (unlike *Cusip6*, which is the header CUSIP on RiskMetrics, TICKER and NAME are historic). After eliminating duplicate matches by manually checking the observations, we identify the correct PERMNOs for 312 firm-years. That leaves 31 observations, which we match using NAME and MEETINGDATE. We find one match here. Finally, as before, for the 30 observations for which we could not find a match, we identify the correct PERMNOs for 23 observations by imposing the condition that if two firm-years have the same *NCusip6*, then they should have the same PERMNO.

[23] RiskMetrics ticker includes a period character in the case of dual class securities. For example, Berkshire Hathaway's ticker is BRK.B. CRSP does not follow the same nomenclature. We therefore remove the period and any characters after the period (".B" in the Berkshire example) from the RiskMetrics ticker before matching with CRSP ticker. Also, before matching by name, we remove the following characters – space, period, ampersand, and comma – in both CRSP and RiskMetrics names.

Overall, we are able to find a unique PERMNO for 5,804 of the 5,810 firm-year observations post-2007 and for 17,425 of the 17,432 observations pre-2007. We check these 13 unmatched observations by hand and find that many of them do not match because of data entry errors by RiskMetrics, and in some cases, the MEETINGDATE precedes the *Start Date* by a few days. Once we correct for these, we are able to obtain matching PERMNOs for all 13 of these observations. Thus, our procedure finds a matching PERMNO for every observation on RiskMetrics.

We finally match this data with the CRSP-Compustat combined database to obtain GVKEY and YEAR (fiscal year of the data) associated with each firm-year on RiskMetrics using PERMNO and MEETINGDATE by ensuring that MEETINGDATE falls between the fiscal year beginning and ending dates. Either the GVKEY or PERMNO then uniquely identifies the sample firms.

A.2.2 Issues with director identifier

There is no unique director identifier (ID) across the two RiskMetrics directors' datasets. Two sets of director IDs are maintained. The first is LEGACY_DIRECTOR_ID, and the second is DIRECTOR_DETAIL_ID. The latter variable is supplied by WRDS from 2004 and onward. All directors in the RiskMetrics database as of 2004 (both directors who started in 2004 and directors who started prior to 2004 but are still on the board as of 2004) have a valid DIRECTOR_DETAIL_ID from 2004 and onward. WRDS assigned the same ID to directors in earlier years, where possible, through matching names, director age, etc. Directors in the database who quit before 2004 do not have a valid DIRECTOR_DETAIL_ID. Thus, prior to 2004, some directors have no DIRECTOR_DETAIL_ID. Over the sample period, 23% of the director-year observations have missing DIRECTOR_DETAIL_ID.

Pre-2004, the unique director ID is LEGACY_DIRECTOR_ID. In contrast to DIRECTOR_DETAIL_ID, this variable is populated only for those directors who were on the database prior to 2004. For all years, the directors are on the database, they will continue to have LEGACY_DIRECTOR_ID. Directors who joined on or after 2004 will not have LEGACY_DIRECTOR_ID. Over the sample period, 27% of the director-year observations have missing LEGACY_DIRECTOR_ID. Eventually, as more years of data are added and directors who started serving prior to 2004 are no longer on the boards of firms, this data item will not be relevant.

In sum, for directors who enter the database on or after 2004, only DIRECTOR_DETAIL_ID is available. For directors who exit the database before 2004, only LEGACY_DIRECTOR_ID is available. For directors who joined before 2004 and are still on the board after 2004, both IDs are available. Thus, it is imperative that both IDs are used to create a unique director ID to ensure that all directors are included in the study.

We first confirm that for directors for whom both IDs are available, there is a one-for-one match between the two IDs. Second, when both IDs should be available, but only one is given in the database, we make necessary adjustments. There are times in which WRDS should have been able to assign a DIRECTOR_DETAIL_ID to a given director based on the director's name and age, but it does not do so. For example, director Joe Smith may have served on firm ABC, Inc., from 2003–2005. Suppose his LEGACY_DIRECTOR_ID is 5555 and his DIRECTOR_DETAIL_ID is 6666. Then WRDS should have assigned this DIRECTOR_DETAIL_ID for Joe Smith for every year from 2003–2005. But we find that this is not always true; for example, Joe Smith has a missing DIRECTOR_DETAIL_ID in 2004. We know he is the same director because the LEGACY_DIRECTOR_ID is the same. Therefore, we assign Joe Smith the same DIRECTOR_DETAIL_ID of 6666 in 2004.

Year	LEGACY_DIRECTOR_ID	DIRECTOR_DETAIL_ID	Corrected DIRECTOR_DETAIL_ID
2003	5555	6666	6666
2004	5555	–	6666
2005	5555	6666	6666

The reverse is also true: sometimes the LEGACY_DIRECTOR_ID is not filled in completely even though DIRECTOR_DETAIL_ID is available for all the years. For these director-year observations, we do a similar assignment and correct the LEGACY _DIRECTOR_ID (we call this Corrected LEGACY_DIRECTOR_ID).

We then create our own unique director ID, which we term as *Our Director ID*, as follows: if Corrected DIRECTOR_DETAIL_ID is available, then we use this as the unique ID, because this is the ID that WRDS will maintain going forward. If Corrected DIRECTOR_DETAIL_ID is not available (which implies the director exited the database prior to 2004), we define an ID that is Corrected LEGACY_DIRECTOR_ID $\times$ 100. The multiplication by 100 ensures that the ID we create is unique and will not clash with the WRDS ID. An example of what we do follows below.

Joe Smith, director at ABC Inc., from 2003–2005

Year	Corrected LEGACY_ DIRECTOR_ID	Corrected DIRECTOR_ DETAIL_ID	*Our Director ID*
2003	5555	6666	6666
2004	5555	6666	6666
2005	5555	6666	6666

Jane Adams, director at ABC Inc., from 2004–2006

Year	Corrected LEGACY_ DIRECTOR_ID	Corrected DIRECTOR_ DETAIL_ID	*Our director ID*
2004	–	6667	6667
2005	–	6667	6667
2006	–	6667	6667

John Doe, director at ABC Inc., from 2001–2003

Year	Corrected LEGACY_ DIRECTOR_ID	Corrected DIRECTOR_ DETAIL_ID	*Our Director ID*
2001	5556	–	555600
2002	5556	–	555600
2003	5556	–	555600

By using both IDs, we ensure that we account for all directors in the database. The final dataset (after cleaning) has 220,963 director-years. If a researcher uses only LEGACY_DIRECTOR_ID, she would have only 195,547 director-year observations. Similarly, if a researcher uses only DIRECTOR_DETAIL_ID, she would have only 170,068 director-year observations. Importantly, if one does not consider both IDs, then board size and co-option and all of the board measures (independence, busyness) will be incorrectly estimated as we would be ignoring a large sample of directors.

A.2.3 Other issues with DIRECTOR_DETAIL_ID

There are some problems specifically with the DIRECTOR_DETAIL_ID of 35025. It appears to have been wrongly assigned in certain years. We find that this ID appears in 772 observations, of which 719 are in 2004, 43 are in 2005, and the remaining 10 occur in years 2006–2010. We examine these ten observations and find that they all belong to the same director, H. Paulett Eberhart (or slight variations of this name, such as H. Eberhart).

To fix this, we flag observations in which the firm (identified using GVKEY), director name (FULLNAME), and the director's joining year (DIRSINCE) are the same in two consecutive years, but the DIRECTOR_DETAIL_ID is different. This happens because there is incorrect assignment of DIRECTOR_DETAIL_ID of 35025 to directors other than H. Paulett Eberhart. For example, in Metlife and General Dynamics, director JOHN M KEANE appears in 2004 with a DIRECTOR_DETAIL_ID of 35025, but in subsequent years with a DIRECTOR_DETAIL_ID of 35937. For this director, we assign a Corrected DIRECTOR_DETAIL_ID of 35937 in 2004. Where we are not able to correct this mistake, we drop the firm completely from the analysis. Finally, there are data entry errors in RiskMetrics. For example, MARY L GOOD, director of ACXIOM Corp. is named as MARY L GOOD PHD in one year and MARY L GOOD in another, but she is given different DIRECTOR_DETAIL_IDs of 112331 and 9612. We correct such data errors to the best of our ability. The manual correction provides the correct DIRECTOR_DETAIL_ID to some of the directors who are mistakenly assigned a DIRECTOR_DETAIL_ID of 35025 and also corrects mistakes in which the same director in the same firm is assigned multiple DIRECTOR_DETAIL_IDs.

A.2.4 Issues with MEETINGDATE

RiskMetrics provides two variables to identify the year for which the observation is valid. One is the MEETINGDATE (date of the annual meeting), and the other is YEAR. In theory, YEAR in RiskMetrics is the year of the MEETINGDATE (this is not to be confused with YEAR in Compustat mentioned earlier in the Appendix, which refers to the fiscal year). We find that 482 director-year observations have the YEAR variable not equal to the year indicated by the MEETINGDATE. Of this, 427 observations have the year of MEETINGDATE equal to 1960, whereas the YEAR is given as 2008. For example, for Zimmer Holdings, the YEAR is given as 2008, but the MEETINGDATE is given as 1/1/1960. We correct the year indicated by the MEETINGDATE variable for such observations to be 2008. That is, in this example we replace MEETINGDATE by 1/1/2008. This is important because when we obtain PERMNOs from CRSP, we use the MEETINGDATE variable. The remaining 55 observations (= 482 - 427) have a difference of either one year or three years between the year indicated by MEETINGDATE and YEAR. We check each of these and find that the YEAR variable is correct, but not the MEETINGDATE. We therefore change the year of the MEETINGDATE to be the same as YEAR.

A.3. Discussion of Changes in Reporting of Annual Pay (TDC1)

Execucomp recently changed its compensation data reporting in line with accounting changes imposed by the Financial Accounting Standards Board (FASB) as well as expanded compensation disclosure requirements (for pension, severance, change-in-control payouts, and equity based compensation) imposed by the Securities and Exchanges Commission (SEC). For fiscal years 1992–2005, all companies on Execucomp report using the old reporting format. The data format reporting code is identified using the Execucomp variable OLD_DATAFMT_FLAG. For fiscal years 2007 and later, all firms on Execucomp report compensation using the new format. For 2006, 16% of firms report using the old format, whereas the remainder report under the new format. We loosely refer to these different reporting formats as the pre-2006 format (OLD_DATAFMT_FLAG = 1) and the post-2006 format (OLD_DATAFMT_FLAG = 0).

The annual compensation (TDC1) pre- and post-2006 is not strictly comparable for at least three reasons. (i) The components and the composition of the components included in TDC1 differ pre- and post-2006. (ii) Pre-2006, equity-based long-term (multi-year) incentive awards were reported based on actual payouts during the fiscal year, whereas post-2006, they are reported based on target (or expected) levels of payout. (iii) Pre-2006, Execucomp computed the Black-Scholes value of option grants (which is one component of TDC1), whereas post-2006, Execucomp provides the fair value (which may or may not be the Black-Scholes value) of option grants as reported by

firms. Unfortunately, reconciling pre- and post-2006 TDC1 numbers without making some major assumptions is impossible. We elaborate below.

$$\text{Pre}-2006,\ \text{the Execucomp definition of TDC1 was}$$

$$\text{TDC1} = \text{SALARY} + \text{BONUS} + \text{LTIP} + \text{RSTKGRNT} + \text{OPTION\_AWARDS\_BLK\_VALUE} + \text{OTHANN} + \text{ALLOTHTOT} \quad \text{(A1)}$$

$$\text{Post}-2006,\ \text{Execucomp definition of TDC1 is}$$

$$\text{TDC1} = \text{SALARY} + \text{BONUS} + \text{NONEQ\_INCENT} + \text{STOCK\_AWARDS\_FV} + \text{OPTION\_AWARDS\_FV} + \text{OTHCOMP} + \text{DEFER\_RPT\_AS\_COMP\_TOT} \quad \text{(A2)}$$

We provide below a quick recap of the key changes in each of these components from pre- to post-2006. Our understanding is based on talks with Standard & Poor officials (who supply the Execucomp database), on "A User's Guide to the SEC's New Rules for Reporting Executive Pay" issued by Moody's Investors Service (2007), and on explanations provided by WRDS.

Execucomp variable	**Pre-2006 definition**	**Post-2006 definition**
SALARY	Base annual salary	Base annual salary
BONUS	Performance-based pay that is • earned during the year; • based on nonformulaic (also called "discretionary" or "guaranteed") as well as formulaic plans; • based on annual (as opposed to multi-year) performance targets; and • cash-based (as opposed to equity-based).	Performance-based pay that is • earned during the year; • based on nonformulaic plans; • based on annual performance targets; and • cash-based.
NONEQ_INCENT	Not reported pre-2006	Performance-based pay that is • earned during the year; • based on formulaic plans; • based on annual as well as multi-year performance targets; and • cash-based.
LTIP	Performance-based pay that is • earned during the year; • based on formulaic plans; and • cash-based when using multiyear performance targets or equity-based when using either annual or multiyear performance targets.	Not reported post-2006

(*Continued*)

Execucomp variable	Pre-2006 definition	Post-2006 definition
RSTKGRNT	Value of restricted stock granted during the year (estimated by Execucomp assuming July 1st as grant date)	Not reported post-2006
STOCK_AWARDS_FV	Not reported pre-2006	Fair value (estimated by the company as of the grant date) of restricted stock grants and performance-based pay that is yet unearned but will result in stock awards in the future. For performance-based pay, the fair value is, typically, based on target payouts.
OPTION_AWARDS_BLK_VALUE	Value of options granted during year (estimated by Execucomp using Black-Scholes methodology assuming July 1st as grant date)	Not reported post-2006
OPTION_AWARDS_FV	Not reported pre-2006	Fair value (estimated by the company as of the grant date) of option grants and performance-based pay that is yet unearned but will result in option awards in the future. For performance-based pay, fair value is, typically, based on target payouts.
OTHANN	Execucomp defines this as "perquisites and other personal benefits, above market earnings on restricted stock, options/SARs or deferred compensation paid during the year but deferred by the officer, earnings on long-term incentive plan compensation paid during the year but deferred at the election of the officer, tax reimbursements, dollar value of difference between the price paid by the officer for company stock and the actual market price of the stock under a stock purchase plan that is not generally available to shareholders or employees of the company."	Not reported post-2006

(*Continued*)

Execucomp variable	Pre-2006 definition	Post-2006 definition
ALLOTHTOT	Execucomp defines this as "severance payments, debt forgiveness, imputed interest, payouts for cancellation of stock options, payment for unused vacation, tax reimbursements, signing bonuses, 401K contributions, and life insurance premiums."	Not reported post-2006
OTHCOMP	Not reported pre-2006	Execucomp defines this as "other compensation received by the executive including perquisites and other personal benefits, termination or change-in-control payments, contributions to defined contribution plans (e.g. 401K plans), life insurance premiums, gross-ups and other tax reimbursements, discounted share purchases, etc."
DEFER_RPT_AS _COMP_TOT	Not reported pre-2006	Execucomp defines this as "deferred compensation earnings as reported in "summary compensation table."

As seen from the discussion above, the only component of compensation that is directly comparable across the two reporting regimes is SALARY.

A.3.1 Performance-based pay

A big difference pre- and post-2006 is in the reporting of performance-based pay. Below for ease of understanding, we present the information in a different manner, where the focus is on how the various components of performance-based pay are reported pre- and post-2006.

Component of performance-based pay (Plan, performance target, payout type)	Pre-2006	Post-2006
Nonformulaic, annual, cash	BONUS	BONUS
Formulaic, annual, cash	BONUS	NONEQ_INCENT
Formulaic, multiyear, cash	LTIP	NONEQ_INCENT
Formulaic, annual/multiyear, stock	LTIP	STOCK_AWARDS_FV
Formulaic, annual/multiyear, option	LTIP	OPTION_AWARDS_FV

Clearly comparing pre- and post-2006 is difficult. BONUS is comparable across the two periods only if we assume that all cash-based payments from formulaic annual plans are zero post-2006. Similarly BONUS pre-2006 is comparable to BONUS + NONEQ_INCENT post-2006 only if we assume that cash-based payments from formulaic multiyear plans are zero in the pre-2006 and/or in the post-2006 period. There is no way to compare performance-based pay that is paid in the form of shares or options because pre-2006 such noncash performance-based pay was recorded in

the year it was actually paid, whereas post-2006 it is recorded at fair value when the performance-based plan is introduced, and not when the payouts are earned by the executive. Firms report the threshold, target, and maximum number of units (cash, stock, or options) that can be earned by the executive under the plan, as well as a fair value estimated based on target-level grants. The threshold is the number of units that will be received by the executive if the performance crosses a pre-specified threshold. The plan also specifies the maximum awards that can be received by the executive and level of performance necessary to achieve this maximum. The target is the level of awards that the board expects the executive to earn. We read several proxy statements that stated that executives did not actually earn these awards because of the failure of the executives to meet targets.

The data needed to evaluate the value of these awards are not available in Execucomp and are sometimes not available in the proxies either.[24] Adding to this lack of data, there are several instances in which Execucomp makes mistakes in terms of recording these awards. There appear to be at least three types of mistakes: (i) The firm actually reports the dollar value of threshold, target, and maximum award levels, but Execucomp wrongly records these as number of threshold, target, and maximum award levels (examples: Briggs & Stratton for 2008 and 2010, Nacco Industries for 2006). (ii) The firm gives performance-based option awards, but Execucomp records these as stock grants (examples: SLM Corp in 2008, Epicor in 2008, and Mentor Graphics in 2006). (iii) The award is in performance units, where one unit is not equivalent to one share, but Execucomp records these as number of shares (e.g., JM Smucker in 2007–2010).

To estimate the correct fair value for such awards, we need to know the performance measure being used by the firm and the level of performance needed to earn these awards. Because this information is not available in Execucomp, following the methodology adopted by some firms, we estimate the fair value of performance-based stock awards as the target level of awards multiplied by the closing stock price on the date the plan is put in place. Similarly, for performance-based option awards, we estimate the fair value using the target number of options, the reported exercise price, time-to-maturity, and other variables needed for the Black-Scholes value, such as volatility, dividend yield, and risk-free rate. The volatility is the annualized standard deviation of stock returns estimated over the previous 60 months prior to the beginning of the fiscal year period, the dividend yield is the average dividend yield over the current year and the previous two years, and the risk-free rate is the rate of return on a Treasury security with comparable maturity. We then compare our estimates with the fair value estimates provided by the firms. Not surprisingly, we find only a 23% correlation between the two values.

The low correlation is not only because of the data errors mentioned above, but also because of variation in the way firms estimate the value of these awards. (i) While most firms evaluate the fair value at the target payout level, some like AIG (see proxy for 2008) evaluate the fair value at the maximum payout level.[25] (ii) Firms may estimate the fair value based on the probability that the executive will achieve the performance level that will trigger the target payout. (iii) Some firms assume that the awards will be paid out at the target level three years from the grant date (the typical performance evaluation period) and discount this back at some appropriate discount rate. (iv) Some firms adjust for the dividend paid on the shares as the unearned shares do not qualify for dividends.

[24] For example, IBM indicates in its 2013 proxy statement that the performance criteria for its executives are revenue growth, operating net income, operating EPS, and free cash flow, but does not indicate what levels of these measures have to be achieved for the awards to vest. The firm states "IBM is not disclosing specific targets under the annual and long-term plans because it would signal IBM's strategic focus areas and impair IBM's ability to leverage these areas for competitive advantage."

[25] The SEC amended its reporting rules in 2009 by requiring that performance-based awards be reported on the basis of the probable outcome of the performance condition, rather than the amount payable for maximum performance.

A.3.2 Stock grants and option grants

Not only is performance-based pay not comparable, the value of stock and option grants is also not comparable. RSTKGRNT cannot be compared to STOCK_AWARDS_FV and OPTION_AWARDS_BLACK_VALUE cannot be compared to OPTION_AWARDS_FV because of two reasons. (i) RSTKGRNT and OPTION_AWARDS_BLACK VALUE are the value of annual stock and options grants only. STOCKS_AWARDS_FV includes the value of annual stock grants as well as unearned stock (which will be earned only when certain performance conditions are met). Similarly, OPTION_AWARDS_FV includes the value of annual option grants as well as unearned options. This strictly is not a problem because firms break out fair values of earned and unearned awards separately.[26] (ii) Pre-2006, Execucomp estimated the value of grants, whereas post-2006, firms estimate and report the fair value themselves. This creates problems because the methodology used by firms need not be the same across firms and need not be consistent within firms over time. For options, even if all firms used the most widely used Black-Scholes model, firms could use different assumptions about maturity because some firms could give a maturity haircut to reflect early exercise (e.g., due to executive risk aversion). Moreover, firms could use different assumptions about volatility.

To understand the magnitude of the problem, we first identify the restricted stock grants portion of STOCK_AWARDS_FV. We then value these restricted stock grants as the number of stocks granted times the closing stock price as on grant date. We find a 92% correlation between our estimates and the corresponding fair values reported by firms. Similarly, we first identify the option grants portion of OPTION_AWARDS_FV. We then estimate the Black-Scholes value of these option grants as described earlier. We find a 91% correlation between our estimates and the fair values reported by firms for option grants.

A.3.3 Adjusted TDC1

Finally, we compute an adjusted measure of TDC1. We use Equations (A1) and (A2) given earlier for TDC1 computation. For firm-years using the pre-2006 reporting format, we compute our own estimate of the value of restricted stock grants (in lieu of RSTKGRNT) and the value of option grants (in lieu of OPTION_AWARDS_BLK_VALUE) as discussed in Section A.3.2. The values of other variables needed for TDC1 are taken from Execucomp. For firm-years using the post-2006 reporting format, we estimate the fair value of stock grants and unearned stock awards (in lieu of STOCK_AWARDS_FV) and the fair value of option grants and unearned option awards (in lieu of OPTION_AWARDS_FV) using the method discussed in Sections A.3.1 and A.3.2. Again, the values of other variables are taken from Execucomp. Overall, the correlation between our adjusted TDC1 estimate and the TDC1 reported by Execucomp is over 99% in the pre-2006 period and 44% in the post-2006 period.

That TDC1 in the pre- and post-2006 periods are so differently defined matters if the annual change in TDC1 is of interest to the researcher. In 2006, with the vast majority of firms (86%) switching to the new reporting format, the change in TDC1 relative to the previous year is misleading. This is true even in 2007, when the remaining 14% of firms reported under the new format. One way around this issue is to compute the adjusted TDC1 as we describe here. But we acknowledge that this measure still has drawbacks, particularly, our inability to estimate the value of performance-based pay using Execucomp data. Another way is to ignore 2006 altogether, as well as 2007, if the firm had reported in the old format in 2006.

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[26] Execucomp provides details of each type of award (stock grants, option grants, and unearned stock awards, unearned option awards) in the "Plan Based Awards" table along with their fair value (FAIR_VALUE). The sum of this variable across all stock grants and unearned stock awards is indicated as STOCK_AWARDS_FV and the sum across all option grants and unearned option awards is indicated as OPTION_AWARDS_FV in the "Annual Compensation" table.

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