Critical Finance Review, 2014, 3: 99–152

Incentive Contracts are not Rigged by Powerful CEOs*

Kam-Ming Wan

School of Accounting and Finance, The Hong Kong Polytechnic University; kmwan@polyu.edu.hk

ABSTRACT

Morse *et al.* (2011), henceforth MNS, interpret the data to suggest that more powerful CEOs *ex post* change their incentive contracts to put greater weight on performance measures that are ex-post more favorable. My paper points out a number of issues with their inference. First and most importantly, MNS do not control for the fact that not just the most powerful but almost all firms change their incentive contracts *ex post*. This is also consistent with an optimal contracting model. Nevertheless, the MNS specification attributes all explanatory power of the average incentive realignment to the cross-coefficient; that is, to more powerful CEOs. When the average level of *ex post* contract change is also controlled for, the MNS cross-coefficient (the additional change attributed to more powerful CEOs) declines by 55% and becomes insignificant. Second, newly-hired CEOs often receive large one-time startup packages. These firms should be broken out, because the MNS theory is not about newly-hired CEOs as new CEOs could not have rigged a previously-set compensation. Third, the results are sensitive to how industry performance is adjusted for. Fourth, the results are sensitive to the level of winsorization.

Keywords: CEO compensation; rigging; stock options; repricing.

JEL Codes: G34, G38, J31, J33

* I appreciate Adair Morse, Vikram Nanda, and Amit Seru for generously sharing certain of their data used in Morse *et al.* (2011), and for commenting on my paper. I am particularly grateful to Ivo Welch

ISSN 2164-5744; DOI 10.1561/104.00000016

1 Introduction

There are two competing views of executive pay. The optimal-contracting hypothesis has argued that executive pay incentives exist to reduce agency conflicts. The managerial-power hypothesis has argued that these incentives create agency conflicts because powerful Chief Executive Officers capture their board of directors and extract rents. In support of the latter, Bebchuk *et al.* (2002) and others have pointed out the spectacular rise of United States executive pay during the 1990s and the resulting, unusually large executive pay. Lie (2005), Lie and Heron (2007), and Narayanan *et al.* (2007) showed that executives manipulated the process to be paid even more than shareholders were told.

Morse *et al.* (2011), henceforth MNS, proposed a new test to examine whether CEOs manipulate their incentive contracts. In particular, they examined whether (more) powerful CEOs *ex post* increased the weights of those components of their incentive structures that worked in their favor (more). MNS call this *ex post* shifting of weights towards advantageous performance measures contract rigging. Their estimates of power-rigging are based on a cross-term between *Max* and *Power* (*Max* × *Power*), where *Max* measures the (*ex post*) realignment of managerial changes in the incentive contracts and *Power* measures the CEO influence.[1] Empirically, they find that the power-rigging effect is strong and accounts for as much as 30% of the pay sensitivity to performance.

MNS graciously made their data on CEO power available, allowing me to revisit their findings and point out four issues:

First, when the generic incentive realignment variable, as measured by the level term *Max*, is added in the regression, the results presented as evidence

(the editor) and an anonymous referee for their constructive and insightful comments. I also thank Nina Baranchuk, John Bizjak, Harold Demsetz, John Graham, Cam Harvey, Wai-Tung Ho, Jun-Koo Kang, Jonathan Karpoff, Benjamin Klein, Ken Lehn, Mike Lemmon, Kevin J. Murphy, Daniel Rogers, James Smith, Avanidhar Subrahmanyam, Wing Suen, Jim Vere, Frances Xu, Yexiao Xu, Harold Zhang, and seminar participants at the Hong Kong Polytechnic University, University of Hong Kong, the 2012 PolyU Summer Research Camp in Finance, the 2012 FMA Annual Meeting, the 2012 China International Conference in Finance, and the 2013 PolyU Mini Conference on Executive Compensation for their useful suggestions.

[1] Specifically, *Max* is the larger value of either the industry-adjusted accounting returns (*zROA*) or industry-adjusted stock returns (*zRET*) of the firm in a given year MNS use three proxies for CEO power (*Power*): (i) CEO personal influence over the board (*PowerIndex*), (ii) percentage of inside directors on the board (*Insider%*), and (iii) percentage of the board appointed during the CEO's tenure (*%Appointed*). Therefore, the power-rigging effect is measured by the coefficient estimate of *Max* × *PowerIndex*, *Max* × *Insider%*, or *Max* × *%Appointed*.

of power-rigging disappears. Specifically, the power-rigging estimate drops by as much as 55%. This is an issue because rigging is not necessarily a sign of board capture in the managerial-power view. Instead, rigging can also be a sign of good incentive contracts in the optimal-contracting view. That is, regardless of CEO power, all companies may want to adjust compensation contracts *ex post* when unforeseen shocks alter the incentive sensitivity, or introduce too much noise into the performance measures. Such *ex post* contract changes have long been considered compatible with the optimal contracting view.[2] For example, Klein (1996), Hart and Moore (1988, and Klein *et al.* (1978) point out that written contract terms are necessarily incomplete due to contracting costs. Kole (1997) and Gillan *et al.* (2009) document that executive compensation contracts are complex and flexible.[3]

A particularly common form of *ex post* contract changes is the lowering of the effective strike price of employee stock options in response to share price drops (see, for example, Saly, 1994; Brenner *et al.*, 2000; Chance *et al.*, 2000; Murphy, 2003). In such cases, companies typically reprice the outstanding options of all employees, and not just those of CEOs (see, for example, Oyer and Schaefer, 2005; Carter and Lynch, 2004; Chidambaran and Prahhala, 2003).

Option repricing also seems to have been more common when the poor stock-price performance was due to a significant decline in the overall stock market, which is beyond the control of CEOs. Examples of this are the stock market crash of October 1987 and the dotcom collapse in 2000. Gilson and Vetsuypens (1993) pointed out that option repricing was also more common among firms that experience financial distress. It therefore seems appropriate not to attribute all the explanatory power in *Max* × *Power* to power-rigging but, instead, to allow for the fact that *Max* can have explanatory power by itself that neither favors one or the other hypothesis. When both the main level and cross variables are included in the same regression, the

---

2 A competing hypothesis of the optimal contracting view is that *Max* reflects the generic contract rigging. A positive coefficient estimate on *Max* indicates that all CEOs, regardless of their power, are entrenched as they enrich themselves by extracting a larger pay than the ones specified in the *ex ante* contracts. At the very least, both interpretations suggest that *Max* is a relevant variable and should be included in the MNS specification.

3 On the other hand, contract renegotiation can alter *ex ante* incentives. This seems to occur only in one direction that favors executives over shareholders, and appears likely in firms with poor corporate governance. For example, Hartzell and Stark (2003) find that CEOs are paid more in firms with low institutional ownership, a proxy of poor corporate governance. However, Smith and Swan (2014) argue that the Hartzell-Stark estimates may not be robust.

cross-variable measures only the unusual ability of more powerful CEOs to renegotiate for a relatively higher compensation *ex post*. On the margin, this cross-variable is then statistically indistinguishable from zero at conventional statistical significance levels.

Second, many companies give large incentive-based compensation to newly-hired CEOs for reasons other than power-rigging. Balsam (2002) argues that this can better align CEO interests with those of shareholders. Congruent with this claim, some companies impose minimum stock ownership targets for their CEOs (see Brown and Li, 2013; Core and Larcker, 2002). For external hires, Fee and Hadlock (2003) show that startup pay packages are used to compensate them for unvested options and restricted stock that these CEOs had to forfeit when leaving their former employers. Thus, it may be better to exclude newly-hired CEOs from a test of the MNS model. This is because the capture view is about how existing powerful CEOs can alter their incentive contracts to extract rents *ex post* and not about how compensation is set for newly-hired executives *ex ante*. Moreover, even if newly-hired CEOs could rig their incentive contracts, MNS's estimates would still not measure this effect properly. This is because MNS measure firm performance over a full one-year period but newly-hired CEOs typically serve only a partial year with their new employers. In the data, being a newly-hired CEO indeed correlates with some of the MNS measures of CEO power. Thus, including newly-hired CEOs in test data of the power hypothesis may muddle the inference on the true power-rigging effect.

Third, the power-rigging effect is sensitive to how industry performance is adjusted for.[4] Like Gibbon and Murphy (1990), Janakiraman *et al.* (1992), Aggarwal and Samwick (1999), and Garvey and Milbourn (2006), my paper uses a broader sample to standardize performance measures. My standardization procedure uses all ExecuComp firms (comprising approximately 25,000 firm-year observations, which is *broad-sample* standardization). In contrast, MNS use only firms in their sample (10,404 firm-year observations, which is *in-sample* standardization). The resulting difference in inference is particularly large for one of their three measures of power-rigging. Specifically, the MNS estimate on *Max* × *Insider%* declines from being statistically

[4] Because the theory offers no conclusive guidance on how to standardize or winsorize variables of interest, the discrepancies reported here could provide implications for other research, too. For example, Lewellen and Metrick (2010) and Johnson *et al.* (2009) disagree on what constitutes a proper sample to compute industry returns. Additional discussion about our discrepancies and their implications will be provided in Section 5.

significant at the 5% level with in-sample standardization to statistically insignificant at the 10% level with broad-sample standardization.

Fourth, MNS winsorize performance variables at the top and bottom 5 percentiles. Winsorizing performance attenuates the effects of contract renegotiation, because it moderates the performance data for those companies that are most likely to renegotiate contracts. Theory does not offer a precise threshold and the MNS choice is defensible. However, choosing a less stringent winsorization at the top and bottom 1 percentiles lowers the power-rigging estimates by as much as 43%. Thus, the inference is not robust to the winsorization choice.

My paper contributes to the executive compensation literature in four ways. First, it disagrees with the MNS perspective that the data speak strongly in favor of the power-rigging hypothesis. Second, it shows that the average-level incentive realignment variable (*Max*) is economically and statistically significant. Third, it shows that it can be important to distinguish between newly-hired and long-standing CEOs. Fourth, it shows how small, and seemingly innocuous, changes in empirical specification can cause large changes in inference in this literature.

2 Sample and Data

My sample begins with the dataset on CEO power taken from Adair Morse's website.[5] This dataset contains three measures of CEO power (*PowerIndex*, *Insider%*, and *%Appointed*), and identifiers for industry, firm, CEO, and year. It is an unbalanced panel of 10,404 firm-year observations from 1992–2003. There are two problems with the MNS data on CEO power, especially *Insider%* and *%Appointed*. First, these two variables are measured with errors. Second, they have missing values for many observations, suggesting that the MNS results are susceptible to sampling bias. In particular, the number of observations for *Insider%* and *%Appointed* are just 7,660 and 7,104, respectively whilst the one for *PowerIndex* is 10,414.[6]

I construct a replicated sample by using the unique CEO-year identifier from the MNS data to match up corresponding records in the 2011 version of the ExecuComp database. To facilitate future studies in replicating my

---

[5] The dataset is available on Adair Morse's Web site at http://faculty.chicagobooth.edu/adair.morse/.

[6] Appendix B contains a detailed description of the extent of the measurement errors in these two variables, and Appendix C addresses the sampling bias problem.

findings, data on other variables are obtained only from the ExecuComp database. To replicate their main results, my paper follows the MNS fixed-effects specification to model CEO total compensation[7]:

$$\text{Log}(CEOComp)_{it} = \gamma_0 + \gamma_1 Power_{it} + \gamma_2 Max_{it}\{zROA_{it}, zRET_{it}\} \times Power_{it} + \phi_1 zROA_{it} + \phi_2 zRET_{it} + X_{it}\beta + \alpha_i + \delta_t + \varepsilon_{it} \quad (1)$$

where the dependent variable is the natural logarithm of CEO total compensation for firm i during the compensation year t. Total compensation is the sum of salary, bonus, restricted stock, Black–Scholes values for stock options, and all other compensation (*TotalComp*; thousands of USD; data item TDC1). The *Power* is the CEO influence and measured by either: (i) *PowerIndex*, which assigns the CEO one additional point for being the chairman of the board, and two additional points for being both the chairman of the board and the president of the company, (ii) *Insider%*, which is the percentage of inside directors sitting on the board of directors, or (iii) *%Appointed*, which is the percentage of the board appointed during the CEO's tenure.

The returns for industry-adjusted accounting and stock performance for firm i during the compensation year are $zROA_{it}$ and $zRET_{it}$, respectively. They are computed as follows: $zROA_{it} = (ROA_{it} - \overline{ROA_t})/\sigma_t^{ROA}$ and $zRET_{it} = (RET_{it} - \overline{RET_t})/\sigma_t^{RET}$, where $\{\overline{ROA_t}, \overline{RET_t}\}$ and $\{\sigma_t^{ROA}, \sigma_t^{RET}\}$ are firm i's industry mean return and standard deviation during the compensation year, for accounting return (*ROA*; data item ROA) and stock return (*RET*; data item TRS1YR). Industry mean returns and standard deviations are from all ExecuComp firms with the same two-digit SIC code during the compensation year. The ExecuComp database has 24,906 and 27,113 valid observations for accounting return and stock return, respectively.

The other explanatory variables are lagged performance variables (*lagged zROA* and *lagged zRET*; industry-adjusted accounting and stock performance in firm i in the year prior to the compensation year), firm assets (*Assets*;

---

[7] Although MNS use two specifications to model the error term, I report the results of only the lagged performance specification throughout this paper for two reasons. The first reason is to conserve space because the results (not reported) in an autoregressive (AR) specification are qualitatively similar to those in a lagged performance specification. Second, in the MNS published results, the lagged performance specification fits the data better than an AR specification. For example, the R^2 of the baseline model is 0.42 in a lagged performance specification and drops to 0.37 in an AR specification (see columns (1) and (2) in MNS's Table III), which is a drop of 11.9%. The drop is large and suggests that the two lagged performance variables are key determinants of CEO compensation, which had become omitted variables in an AR specification. Consistent with this claim, Jensen and Murphy (1990) find that CEO pay is tied strongly and significantly with lagged stock-price performance.

data item ASSETS), prior 5-year average Black–Scholes volatility (*Volatility*; data item BS_ VOLATILITY), shares owned by the CEO (*SharesOwned%*; data item SHROWN_ EXCL_ OPTS deflated by SHRSOUT × 1,000), option holdings of the CEO (*OptionsValue*; data item OPT_ UNEX_ EXER_ EST_ VAL), the years since the CEO began working in the firm (*Tenure*; data item BECAMECEO), and ε_{it} is the random error term, independently and identically distributed.[8]

3 Replicated Results

Table 1 reproduces the summary statistics that are reported in MNS's Table I, the summary statistics from my replicated sample, and the absolute percentage differences between the published and replicated results.

Panel A: Correlations Matrix for CEO Power Variables

	Published Results		
	PowerIndex	*Insider%*	*%Appointed*
PowerIndex	1		
Insider%	−0.033***	1	
%Appointed	0.221***	0.051***	1
	Replicated Results		
	PowerIndex	*Insider%*	*%Appointed*
PowerIndex	1		
Insider%	−0.032**	1	
%Appointed	0.220***	0.051***	1

(*Continued*)

8 I follow MNS to adjust CEO tenure for observations with missing or negative values for this variable. Of these observations, CEO tenure equals to the time lapse since the person joined the firm times the average percentage of time spent in the firm prior to CEO status by year. For the remaining observations with missing data on the date when the CEO joins the firm, CEO tenure is assigned to be three years, which is the 33rd percentile of CEO tenure.

Panel B: Firm and CEO Characteristics			
	PowerIndex	*Insider%*	*%Appointed*
	Published (1)	Replicated (2)	Absolute Percentage Difference (%) (3)
Assets	13,363	13,444	0.61%
Volatility	0.338	0.341	0.89%
ROA	4.39	4.32	1.59%
RET	16.51	16.53	0.12%
TotalComp	4,472	4,486	0.31%
CashComp	1,410	1,417	0.50%
ShareOwned%	0.022	0.022	0.00%
Tenure	7.11	7.16	0.70%
No. of Observation	9,460	9,261	2.10%

Table 1. Summary statistics.

Description: This table reproduces the summary statistics reported in MNS's Table I. It also contains calculations of these summary statistics from my replicated sample and the absolute percentage differences between the published and replicated results. In Panel A, I report the correlation matrix for CEO power variables — *PowerIndex*, *Insider%*, and *%Appointed*. Panel B reports firm and CEO characteristics. The *PowerIndex* assigns the CEO one additional point for being the chair of the board and two additional points for being the chair of the board and also the president of the company; *Insider%* is the percentage of inside directors sitting on the board of directors; and *%Appointed* is the percentage of the board appointed during the CEO's tenure. The variables reported are firm assets (*Assets*), prior 5-year average Black–Scholes volatility (*Volatility*), an accounting measure of performance (*ROA*), a market measure of performance (*RET*), total compensation including salary, bonus, equity-based pay (using Black–Scholes values for option grants), and all other compensation (*TotalComp*; in units of $000), cash compensation including salary and bonus (*CashComp*; in units of $000; ExecuComp data item TOTAL_ CURR), shareholding of the CEO (*SharesOwned%*), and the years since the CEO started working in the firm (*Tenure*). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: For most variables, the summary statistics of the replicated and published values are similar. The simple correlations of CEO power variables are nearly identical. Further, averages of other variables are similar between the replicated and published results.

Panel A shows the correlations between the three measures of CEO power and Panel B reports averages of firm and CEO characteristics in the two samples.

Overall, the replicated results are similar to the published results. Although there are fewer observations in the replication, the average firm and CEO characteristics remain similar. Columns (1)–(4) of Table 2 reproduce MNS's main results from columns (1), (3), and (5) in MNS's Table II, and column (1) in MNS's Table III, respectively. Columns (5)–(8) present the results from replicating MNS's findings using my replicated sample. In sum, Table 2 shows sufficient similarity. For example, the coefficient estimate on *Max* is 0.058 [p-value of 0.026; column (4)] in the published results and 0.057 [p-value of 0.042; column (8)] in the replicated results. Importantly, this generic incentive realignment effect is large and accounts for 34% to 58% of the sensitivity of incentive pay to performance.[9]

3.1 Omitted Variable Bias

To illustrate the omitted variable bias, a full model in Equation (2) excludes all unnecessary terms and subscripts, and includes two level variables (*Power* and *Max*) and their cross term,

$$y = a_0 + a_1 Power + a_2 Max + a_3 Max \times Power \tag{2}$$

The MNS specification excludes the average (*ex post*) incentive realignment variable (*Max*) from the full model,

$$y = \gamma_0 + \gamma_1 Power + \gamma_2 Max \times Power \tag{3}$$

Therefore, MNS assume that a_2 in Equation (2) is zero and exclude the possibility that all, or at least many, companies may want to alter their compensation contracts *ex post* for reasons that are unrelated to CEO power. An alternative interpretation is that MNS allows incentive contracts to be rigged by powerful CEOs only. Nevertheless, if a_2 is not zero, omitting the average

[9] I modify MNS's procedure by using the estimates in column (8) of Table 2 to compute the percentage of incentive pay sensitivity to performance that is attributable to the incentive realignment variable (*Max*). The rigged dollar portion for *Max* with respect to the accounting performance (*zROA*) is 33.7% [$= (\gamma_2/2)/(\phi_1 + \gamma_2/2) = (0.057/2)/(0.056+0.057/2)$], where ϕ_1 and γ_2 are the coefficients for *zROA* and *Max*. Similarly, the rigged dollar portion for *Max* with respect to the stock performance (*zRET*) is 57.6% [$= (\gamma_2/2) / (\phi_2 + \gamma_2/2) = (0.057/2) / (0.021 + 0.057/2)$], where ϕ_2 is the coefficient for *zRET*.

	Published Results				Replicated Results			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PowerIndex	0.059**				0.067***			
	(0.025)				(0.025)			
Insider%		0.102**				0.125**		
		(0.052)				(0.052)		
%Appointed			−0.039				−0.056	
			(0.092)				(0.094)	
Max				0.058**				0.057**
				(0.026)				(0.028)
zROA	0.079***	0.096***	0.088***	0.049**	0.079***	0.097***	0.092***	0.056***
	(0.016)	(0.019)	(0.019)	(0.019)	(0.016)	(0.018)	(0.019)	(0.019)
zRET	0.064***	0.061***	0.064***	0.032*	0.053***	0.060***	0.059***	0.021
	(0.012)	(0.013)	(0.014)	(0.017)	(0.016)	(0.014)	(0.014)	(0.019)
Lagged zROA	0.073***	0.082***	0.087**	0.071***	0.088***	0.089***	0.093***	0.087***
	(0.017)	(0.022)	(0.022)	(0.017)	(0.022)	(0.028)	(0.029)	(0.022)
Lagged zRET	0.046***	0.032**	0.041***	0.047***	0.037***	0.042***	0.044***	0.036***
	(0.011)	(0.014)	(0.013)	(0.011)	(0.012)	(0.014)	(0.014)	(0.012)
N	8,263	6,116	5,797	8,263	8,214	6,030	5,716	8,214
R^2	0.42	0.41	0.41	0.42	0.40	0.41	0.40	0.40
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2. Total CEO compensation.

Description: The results in this table are based on the data in my replicated sample. The empirical model follows MNS: $ln(TotalComp_{it}) = a_0 + a_1 Power_{it} + \phi_1 zROA_{it} + \phi_2 zRET_{it} + [Controls_{it}] + [\text{firm dummies}] + [\text{year dummies}] + \varepsilon_{it}$, except that the CEO power variable is replaced by the incentive-realignment variable in columns (4) and (8). The *TotalComp* is total CEO compensation, *Power* is CEO power and measured by either *PowerIndex*, *Insider%*, or *%Appointed*. The *PowerIndex* assigns the CEO one additional point for being the chair of the board and two additional points for being the chair of the board and also the president of the company; *Insider%* is the percentage of inside directors sitting on the board of directors; and *%Appointed* is the percentage of the board appointed during the CEO's tenure. The *zROA* and *zRET* are the industry-adjusted returns on assets and stock returns, respectively, for the firm during the compensation year. The *lagged zROA* and *lagged zRET* are lagged performance measures and are analogously defined for the year prior to the compensation year. The *Max* is the larger value of either *zROA* or *zRET* for the firm during the compensation year. Controls include: *Assets*, firm assets; *Volatility*, prior 5-year average Black–Scholes volatility; *SharesOwned%* and $SharesOwned\%^2$, shareholding of the CEO and its square; *OptionsValue*, option holdings of the CEO; *Tenure* and $Tenure^2$, the years since the CEO started working in the firm, and its square. All regressions are estimated with the same control variables as in MNS's model plus time and firm fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: When the power-rigging variable is excluded in the MNS model, the replicated results are qualitatively similar to the ones reported by MNS. Additionally, the replicated coefficients on the incentive realignment variable (*Max*) are large and nearly identical to the published value. Further, the better measure of board-capture (*%Appointed*) produces the weakest results. This appears contradictory to the importance of board-capture in the managerial-power view.

incentive realignment variable renders γ_2 to be a biased estimate for the cross-coefficient on *Max* × *Power*. To measure the direction and degree of the bias, say that average incentive realignment variable is correlated with other explanatory variables,

$$Max = b_0 + b_1 Power + b_2 Max \times Power \tag{4}$$

Substituting Equation (4) into the full model in Equation (2) and rearranging terms,

$$y = (a_0 + a_2 b_0) + (a_1 + a_2 b_1) Power + (a_3 + a_2 b_2) Max \times Power, \tag{5}$$

where $\gamma_0 = a_0 + a_2 b_0$; $\gamma_1 = a_1 + a_2 b_1$; and $\gamma_2 = a_3 + a_2 b_2$. Therefore, the MNS estimate of rigging (γ_2) is a biased estimator of the true power-rigging effect (a_3). The bias is $a_2 b_2$, which captures the average incentive realignment effect in the full model (a_2) deflated by the partial correlation between the omitted incentive realignment variable and the cross variable (b_2). It is important to note that the bias is towards overstating the true power-rigging effect because the sign of these two coefficients (a_2 and b_2) are positive. MNS report that the estimates of the average incentive realignment effect are positive [see columns (1)–(2) in their Table III].[10] Furthermore, the omitted variable is positively correlated with the cross variable because *Max* is a common factor of the two variables.[11]

The MNS results appear to contradict the importance of board capture to extract rent in the managerial-power view. Out of the three MNS proxies of CEO influence, it appears that the best measurement of board capture is the percentage of the board appointed during the CEO's tenure (*%Appointed*).[12] According to the managerial-power view, powerful CEOs capture the board

---

[10] Note that the coefficient on *Max* reported by MNS is not exactly the same as a_2 in Equation (2) because the two independent variables (*Power* and *Max* × *Power*) are excluded in those regressions. Nevertheless, my replicated results in columns (10)–(12) of Table 3 show that the point estimates of a_2 are consistently positive.

[11] Unreported results show that the estimated coefficient of b_2 is positive, large, and statistically very different from zero for the three measures of CEO power. Specifically, I regress *Max* on all explanatory variables in the MNS model. The estimated coefficients (b_2) for *Max* × *PowerIndex*, *Max* × *Insider%*, and *Max* × *%Appointed* are +0.29, +1.05, and +0.49, respectively, and each has a p-value of below 0.001. Furthermore, the coefficients of determination of the above regressions are very large. Specifically, the within-R^2 for the regression with *Max* × *PowerIndex*, *Max* × *Insider%*, and *Max* × *%Appointed* as the measure of CEO power is 0.91, 0.85, and 0.87, respectively.

[12] It is unclear if *PowerIndex* and *Insider%* can faithfully measure the degree of board capture in the managerial-power view. This is because the concentration of CEO title (*PowerIndex*) may reflect higher CEO perceived abilities (see Hermalin and Weisbach, 1998). Similarly, the percentage of inside directors on the board (*Insider%*) may reflect CEO succession process. Hermalin and Weisbach (1988) find

by handpicking directors who are willing to give them excessive compensation. If powerful CEOs capture the boards to increase their pay, CEOs in more captured boards would have been paid significantly more than those in less captured boards. However, the published MNS results fail to support this prediction. Specifically, the coefficient estimate on *%Appointed* is negative, albeit statistically insignificantly, when the cross and main-level variables are excluded from the MNS model (see column (3) of Table 2). This suggests that CEOs in more captured boards receive similar pay as CEOs in less captured boards.

In contrast, the average incentive realignment variable (*Max*) is positively and significantly correlated with CEO compensation (see column (4) of Table 2). When MNS use *%Appointed* × *Max* to measure power-rigging, the coefficient estimate on this variable the in MNS model is positive and statistically significant at the 5% level (see column (3) of Table 3). This implies that the economic and statistical significance of *Max* × *%Appointed* is attributable to the generic realignment of managerial incentive contracts *ex post*, an effect that is excluded in the MNS model. Altogether, my results indicate that the omitted variable bias appears serious.

Lastly, the omitted variable problem creates another bias that is also in favor of finding a statistically significant effect on rigging by powerful CEOs. Specifically, omitting a relevant variable decreases the standard errors of the coefficient estimates of the included variables, thereby inflating their corresponding t-statistics.[13]

3.2 Replicated Results with the Average Incentive Realignment Variable

In Table 3, columns (1)–(3) reproduce MNS's main results in columns (3), (5), and (7) in MNS's Table III and columns (4)–(6) present the results from replicating their main results using the replicated sample. The findings in Table 3 show that the power-rigging effect is broadly weaker in statistical and economic terms for the replicated results relative to the ones in the published results, particularly for *Max* × *Insider%*. This implies that discrepancies to construct performance variables can substantially alter our inference on the importance of rigging by powerful CEOs. For example, the point estimate on *Max* × *Insider%* drops from 0.187 [p-value of 0.002; column

that when CEOs plan to retire, firms routinely initiate a succession plan by adding inside directors to the boards. Just after the firm successfully hires a new CEO, inside directors with short tenures are more likely to leave the board.

13 For a proof, please see p. 246 of Greene (1993).

(2)] in the published results to 0.120 [*p*-value of 0.105; column (5)] in the replicated results, so a drop by 35.83%. This implies that the power-rigging effect goes from being statistically significant at the 1% level in the published results to statistically insignificant at the 10% level in the replicated results.

To examine the severity of the omitted variable bias, I add the incentive realignment variable in the MNS specification, and expected that this coefficient would be positive and statistically significant. Columns (7)–(9) of Table 3 show that including the incentive drops the power-rigging cross-variable coefficient estimates drop by as much as 55%. Two implications can be seen in these drops. First, the drops are large, rendering none of the indicators of power-rigging statistically significant at conventional levels. Second, this indicates that the generic incentive realignment mechanism is an important determinant of contract rigging. As expected, including a relevant omitted variable increases the standard error of all the cross variables.

Including the incentive-realignment variable lowers the statistical significance of not just the power-rigging variable but also the omitted variable itself because the two variables are highly collinear. Importantly, the multicollinearity problem is more acute for the incentive realignment variable than for the power-rigging variable. This implies that the statistical insignificance of the incentive realignment variable is due primarily to multicollinearity whereas that of the cross variable is not. Specifically, the results in columns (7)–(9) show that the variance inflation factor (VIF) for the incentive realignment variable is large and ranges from 8.94 to 14.60. In contrast, comparable figures for the product variable are moderate to large and range from 3.81 to 10.40.

Although the multicollinearity also inflates the standard errors of the estimates for the power-rigging variable, the increase is small and insufficient to make this variable statistically insignificant at conventional levels. For example, including the incentive realignment variable increases the variance inflation factors from 2.76 to 3.81 for *Max* × *Insider%* and from 2.78 to 4.28 for *Max* × *%Appointed*. Similarly, the reduction in the point estimate on *Max* × *PowerIndex* is so large that it outweighs the increase in the standard error of the same variable.[14]

[14] Numerically, including the *Max* term lowers the point estimate on *Max* × *PowerIndex* from 0.20 to 0.009, and raises the standard error of the same variable from 0.011 to 0.20. If we ignore the multicollinearity issue and assume that the standard error of *Max* × *PowerIndex* remains identical to the one in MNS's model (which is 0.011) (see column (4) of Table 3), then the implied *t*-statistics of this variable would be just 0.818 (= 0.009/0.011), and would remain statistically insignificant at conventional levels.

	Published Results			Replicated Results								
	Exclude *Max* term			Exclude *Max* term			Include *Max* term			Replace *Max* × *Power* term with *Max* term		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Max							0.036	0.055	0.050	0.055*	0.069**	0.070**
							(0.050)	(0.036)	(0.038)	(0.028)	(0.030)	(0.031)
PowerIndex	0.049**			0.061**			0.064**			0.067***		
	(0.025)			(0.026)			(0.026)			(0.025)		
Max × *PowerIndex*	0.025**			0.020*			0.009					
	(0.011)			(0.011)			(0.020)					
Insider%		0.022			0.083			0.104*			0.126**	
		(0.058)			(0.056)			(0.059)			(0.052)	
Max × *Insider%*		0.187***			0.120			0.062				
		(0.060)			(0.074)			(0.087)				
%Appointed			−0.059			−0.072			−0.064			−0.053
			(0.092)			(0.094)			(0.095)			(0.094)
Max × *% Appointed*			0.060**			0.061*			0.037			
			(0.027)			(0.032)			(0.040)			
zROA	0.053***	0.071***	0.073***	0.062***	0.085***	0.077***	0.056***	0.067***	0.061***	0.056***	0.067***	0.062***
	(0.020)	(0.019)	(0.020)	(0.019)	(0.019)	(0.021)	(0.019)	(0.020)	(0.022)	(0.019)	(0.020)	(0.022)
zRET	0.0368*	0.0391**	0.047***	0.031	0.044***	0.040**	0.022	0.021	0.019	0.022	0.022	0.020
	(0.019)	(0.015)	(0.017)	(0.019)	(0.016)	(0.017)	(0.019)	(0.019)	(0.021)	(0.018)	(0.019)	(0.021)
lagged zROA	0.071***	0.089***	0.086***	0.087***	0.087***	0.093***	0.086***	0.086***	0.091***	0.086***	0.087***	0.091***
	(0.017)	(0.024)	(0.022)	(0.022)	(0.028)	(0.029)	(0.022)	(0.028)	(0.029)	(0.022)	(0.028)	(0.029)
lagged zRET	0.047***	0.036**	0.041***	0.037***	0.042***	0.043***	0.037***	0.042***	0.043***	0.037***	0.042***	0.043***
	(0.011)	(0.014)	(0.014)	(0.012)	(0.014)	(0.014)	(0.012)	(0.014)	(0.014)	(0.012)	(0.014)	(0.014)
N	8,263	6,121	5,797	8,214	6,030	5,716	8,214	6,030	5,716	8,214	6,030	5,716
R^2	0.42	0.42	0.41	0.40	0.41	0.40	0.40	0.41	0.40	0.40	0.41	0.40

(*Continued*)

	Published Results			Replicated Results								
	Exclude *Max* term			Exclude *Max* term			Include *Max* term			Replace *Max* × *Power* term with *Max* term		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VIF(*Max*)	N/A	N/A	N/A	N/A	N/A	N/A	14.60	8.94	10.13	6.02	6.48	6.58
VIF(*Max* × *Power*)	N/A	N/A	N/A	4.29	2.76	2.78	10.40	3.81	4.28	N/A	N/A	N/A
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3. Incentive pay and rigging with total CEO compensation.

Description: This table reports the published and replicated results. The results in this table are based on the data in my replicated sample. The empirical model follows MNS: $ln\ (TotalComp_{it}) = a_0 + a_1 Power_{it} + a_3 Max_{it} \times Power_{it} + \phi_1 zROA_{it} + \phi_2 zRET_{it} + [Controls_{it}]$ + [firm dummies] + [year dummies] $+\varepsilon_{it}$, except that the incentive-realignment variable is included in columns (7)–(9) and the power-rigging variable is replaced by the incentive-realignment variable in columns (10)–(12). Other variables are defined in the notes to Table 2. All regressions are estimated with the same control variables as in MNS's model plus time and firm fixed effects. VIF(*Max*) and VIF(*Max* × *Power*) are variance inflation factor for the incentive-realignment variable and the power-rigging variable, respectively. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: The MNS results presented as evidence of power-rigging are sensitive to how performance variables are constructed. Specifically, the replicated coefficients on power-rigging in columns (4)–(6) are broadly weaker than the ones reported by MNS. These differences are due mainly to the discrepancies to standardize and winsorize performance variables in the replicated and published results. Additionally, when the incentive realignment variable is also controlled for in the MNS specification, the power-rigging estimates drop substantially [columns (7)–(9)]. Further, the incentive realignment variable by itself is an important determinant of CEO compensation [columns (10)–(12)].

The results in columns (10)–(12) of my Table 3 indicate that the incentive realignment variable is highly statistically significant after excluding the power-rigging variable in the MNS model. In contrast, the power-rigging variable is only mildly statistically significant when the incentive realignment variable is excluded (see columns (4)–(6)). This asymmetry implies that the incentive realignment variable seems to be a more important determinant of CEO pay than the power-rigging variable.

3.3 Mean Centering on CEO Power Variables

To further demonstrate that the power-rigging effect is fragile and that the incentive realignment effect is robust, Table 4 explores specifications in which the CEO power variable is mean-centered. This data transformation is similar to including the incentive realignment variable in the MNS model. Specifically, mean centering of the CEO power variable modifies MNS's model to

$$\begin{aligned} y &= c_0 + c_1 Power^{mc} + c_2 Max \times Power^{mc} \\ &= c_0 + c_1(Power - \overline{Power}) + c_2 Max \times (Power - \overline{Power}) \end{aligned} \tag{6}$$

where $Power^{mc} = Power - \overline{Power}$ and $\overline{Power}$ is the sample mean of the CEO power variable. After arranging and collecting terms in Equation (6),

$$y = d_0 + d_1 Power + d_2 Max \times Power + d_3 Max \tag{7}$$

where $d_0 = c_0 - c_1\overline{Power}$; $d_1 = c_1$; $d_2 = c_2$; and $d_3 = -c_2\overline{Power}$.

In Table 4, columns (1)–(6) report regression estimates with mean-centered CEO power variables as in Equation (6) in my replicated sample (with columns (1)–(3) in MNS's model, and with columns (4)–(6) after including the incentive-realignment variable). None of the coefficients on the power-rigging variable are statistically significant at the 10% level after the CEO power variable is mean-centered, regardless of whether the incentive-realignment variable is included or excluded. In this sense, the MNS model is sensitive to which origin a researcher uses to measure the CEO power construct.

Note that mean-centering the CEO power variable only appears to reduce the multicollinearity problem. This is because the variance inflation factor

	Replicated Results — Mean-centered CEO Power Variable					
	Exclude *Max* term			Include *Max* term		
	(1)	(2)	(3)	(4)	(5)	(6)
Max				0.055* (0.028)	0.069** (0.030)	0.070** (0.031)
$PowerIndex^{mc}$	0.064** (0.026)			0.064** (0.026)		
Max × $PowerIndex^{mc}$	0.009 (0.020)			0.009 (0.020)		
$Insider\%^{mc}$		0.103* (0.059)			0.104* (0.059)	
Max × $Insider\%^{mc}$		0.063 (0.088)			0.062 (0.087)	
$\%Appointed^{mc}$			−0.066 (0.095)			−0.064 (0.095)
Max × $\%Appointed^{mc}$			0.035 (0.040)			0.037 (0.040)
zROA	0.079*** (0.016)	0.097*** (0.018)	0.092*** (0.019)	0.056*** (0.019)	0.067*** (0.020)	0.061*** (0.022)
zRET	0.053*** (0.016)	0.060*** (0.014)	0.059*** (0.014)	0.022 (0.019)	0.021 (0.019)	0.019 (0.021)
lagged zROA	0.088*** (0.022)	0.089*** (0.028)	0.094*** (0.029)	0.086*** (0.022)	0.086*** (0.028)	0.091*** (0.029)
lagged zRET	0.037*** (0.012)	0.042*** (0.014)	0.044*** (0.014)	0.037*** (0.012)	0.042*** (0.014)	0.043*** (0.014)
N	8,214	6030	5716	8,214	6,030	5,716
R^2	0.40	0.41	0.40	0.40	0.41	0.40
VIF(*Max*)	N/A	N/A	N/A	6.01	6.48	6.69
VIF(*Max* × $Power^{mc}$)	1.26	1.32	1.29	1.30	1.33	3.66
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes

Table 4. Mean-centered CEO power variables on rigging with total CEO compensation.

Description: This table repeats the regressions displayed in columns (4)–(9) of Table 3, except that each measure of CEO power is mean-centered. The basic empirical model is as follows: $ln(TotalComp_{it}) = a_0 + a_1\ Power^{mc}_{it} + a_3 Max_{it} \times\ Power^{mc}_{it} + \phi_1 zROA_{it} + \phi_2 zRET_{it} + [Controls_{it}]+$ [firm dummies] + [year dummies] $+\varepsilon_{it}$, where $Power^{mc}$ is the mean-centered CEO power variable computed as follows: $Power_{it} - \Sigma Power_{it}/N$, where $Power_{it}$ is the CEO power variable for the firm during the compensation year, $\Sigma Power_{it}/N$ is the sample mean of the CEO power variable, and N is the number of valid observations for the CEO power variable. Other variables are defined in the notes to Table 2. All regressions are estimated

Table 4. (*Continued*)

with the same control variables as in MNS's model plus time and firm fixed effects. VIF(*Max*) and VIF(*Max* × *Power*) are variance inflation factor for the incentive-realignment variable and the power-rigging variable, respectively. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: The MNS results presented as evidence of power-rigging are sensitive to which origin a researcher uses to measure the CEO power construct. When the CEO power variables are mean-centered in the MNS specification, none of the indicators of power-rigging are significant. In contrast, the results presented as evidence of incentive realignment are robust to this mean-centering treatment.

for the cross variable is small with or without the inclusion of the incentive-realignment variable. In contrast, my findings in columns (4)–(6) indicate that the estimates of the incentive-realignment variable are statistically significant. Again, my findings point to the necessity of including the incentive-realignment variable to model CEO compensation.

3.4 Remuneration of Non-CEO Top Executives

Conceptually, if contract rigging is attributable to the influence of CEOs to enrich themselves, this effect should be primarily related to CEO compensation only. In contrast, if contract rigging is motivated by unforeseen firm-specific factors that align or realign the incentive structures of all employees, that effect should also apply to remuneration of other employees. To disentangle these two hypotheses, I extract all data from non-CEO top executives in the ExecuComp database for each observation in the replicated sample. Columns (1)–(3) of Table 5 present the regression results of the natural logarithm of the total compensation for non-CEO top executives in the MNS model. My results indicate that all the estimates of the power-rigging variable are positive and statistically significant at the 5% level.

Columns (4)–(6) of Table 5 present the results after the incentive realignment variable is added to the MNS model. As expected, all the power-rigging estimates drop substantially after the inclusion. In addition, none are statistically significant at the 5% level. In contrast, the estimates of the incentive realignment variables are positive and statistically significant at conventional levels in all regressions (see columns (4)–(7)). This suggests that contract rigging is attributable to factors universal to not just CEOs but also to other top executives.

	Exclude *Max* term			Include *Max* term			Replace *Max* × Power with *Max* term
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Max				0.053*** (0.019)	0.042*** (0.014)	0.036** (0.015)	0.048*** (0.011)
PowerIndex	−0.008 (0.007)			−0.003 (0.007)			
Max × *PowerIndex*	0.013*** (0.004)			−0.003 (0.008)			
Insider%		0.031 (0.026)			0.047* (0.027)		
Max × *Insider%*		0.079*** (0.028)			0.035 (0.032)		
%Appointed			0.007 (0.018)			0.014 (0.019)	
Max × *%Appointed*			0.050*** (0.015)			0.031* (0.017)	
zROA	0.042*** (0.007)	0.058*** (0.008)	0.048*** (0.008)	0.033*** (0.008)	0.045*** (0.009)	0.038*** (0.009)	0.033*** (0.008)
zRET	0.034*** (0.007)	0.030*** (0.006)	0.027*** (0.007)	0.022*** (0.007)	0.013 (0.009)	0.013 (0.009)	0.022*** (0.007)
lagged zROA	0.033*** (0.007)	0.033*** (0.008)	0.033*** (0.008)	0.032*** (0.007)	0.033*** (0.008)	0.032*** (0.008)	0.032*** (0.007)
lagged zRET	0.046*** (0.004)	0.044*** (0.005)	0.049*** (0.005)	0.046*** (0.004)	0.044*** (0.005)	0.049*** (0.005)	0.046*** (0.004)
N	33,516	24,525	23,302	33,516	24,525	23,302	33,516
R^2	0.43	0.45	0.45	0.43	0.45	0.45	0.43
VIF(*Max*)	N/A	N/A	N/A	13.99	8.11	9.00	5.67
VIF(*Max* × *Power*)	4.08	2.65	2.58	10.05	3.68	3.92	N/A
Time and firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5. Rigging with total compensation for non-CEO top executives.

Description: This table repeats the regressions displayed in column (8) of Table 2 and columns (4)–(9) of Table 3 for compensation of non-CEO top executives. The results in this table are based on all non-CEO top executives with data in ExecuComp in my replicated sample. Other variables are defined in the notes to Table 2. All regressions are estimated with the same control variables as in MNS's model plus time

Table 5. (*Continued*)

and firm fixed effects. The only exception is that *Tenure* and $Tenure^2$ are excluded in all regressions. VIF(*Max*) and VIF(*Max* × *Power*) are variance inflation factor for the incentive-realignment variable and the power-rigging variable, respectively. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: The MNS results presented as evidence of power-rigging apply to not just CEOs but also other top executives, indicating that their power-rigging evidence is motivated by factors common to all top executives. Similarly, when the incentive realignment variable is added to the MNS specification, the power-rigging estimates drop substantially. In contrast, the incentive-realignment estimates are significant, implying that the generic incentive realignment is an important determinant of compensation of other top executives.

4 Why do Firms Alter Incentive Contracts of Top Executives *ex post*?

The following sections provide economic justifications for why firms voluntarily alter managerial incentive contracts *ex post*. Specifically, this generic incentive realignment can be explained by two common practices: (i) offering newly-hired CEOs large incentive-based compensation, and (ii) altering incentive contracts of employees *ex post* after poor stock-price performance.

4.1 Newly-hired CEOs versus Continuing CEOs

Table 6 presents Ordinary Least Squares regression results of modeling the natural logarithm of CEO total compensation for newly-hired CEOs (see columns (1)–(3)) and for continuing CEOs (see columns (7)–(9)).[15] An executive is considered to be a newly-hired CEO if he or she has been the CEO for less than one year while an executive is considered to be a continuing CEO if he or she has been the CEO for one year or longer.

The table shows that the estimates of the incentive realignment variable are strong for newly-hired CEOs but weak for continuing CEOs. In the sample of newly-hired CEOs, all coefficients on the incentive realignment variable are positive, large, and statistically significant at the 5% level (see columns (1)–(3)). In contrast, in the sample for continuing CEOs, the

[15] In my replicated sample, the average CEO tenure is 7.17 years, and the sample period is 12 years. This implies that each firm has, on average, one CEO turnover over the sample period. Thus, it is impossible to use within-firm variations (that is, a firm fixed-effects model) to obtain estimates of the level variable. Consequently, I use the OLS model, which relies on between-firm variations, to obtain estimates of this variable.

comparable coefficients are consistently negative and statistically insignificant at the 5% level (see columns (7)–(9)).[16] This is surprising because the managerial-power view would interpret the results to mean that contract rigging is more common or prevalent for newly-hired CEOs than for long-standing CEOs. Yet, newly-hired CEOs could not have rigged a previously-set compensation, because their compensation is typically set *ex ante*.[17] This casts doubt on the empirical validity of the capture view in which long-standing CEOs (rather than newly-hired CEOs) can rig their own compensation due to their pre-existing ties with the board of directors.

There is more information about where the large extra compensation for new CEOs comes from. Unlike continuing CEOs, newly-hired CEOs are routinely given substantial one-time startup incentive packages, particularly in the form of large stock option grants. Awarding large startup option grants to newly-hired CEOs are consistent with the practice of requiring top executives to maintain a minimum stock ownership to align their interests with shareholders' interests. Furthermore, these grants are particularly large for externally hired new CEOs to offset the value of unvested options, and restricted stock they have to forfeit when leaving prior employers.

Columns (4)–(6) of Table 6 present OLS regression results of the natural logarithm of CEO total compensation after excluding stock option grants (B-S value) to newly-hired CEOs. As expected, none of the coefficients of the incentive realignment variable are statistically significant at the 5% level after stock option grants are excluded from CEO total compensation. My findings are again consistent with the optimal-contracting hypothesis. This suggests that large startup option grants are offered to newly-hired CEOs to align their incentives with those of shareholders.

Even if newly-hired CEOs could rig their incentive contracts, the MNS estimates would still not measure this effect properly because contemporaneous

---

[16] Unreported results are qualitatively similar to firm fixed-effects in the regressions, except that the coefficients of the incentive realignment variable change sign and become positive. However, they remain statistically insignificant at the 5% level.

[17] Congruent with this claim, information about compensation of newly-hired CEOs is commonly disclosed at the time of their hiring, particularly for CEOs hired from the outside. A case in point is the compensation to Gilbert Amelio, the former CEO of Apple. Gilbert Amelio was hired as Apple's CEO on February 2, 1996. Ten days later, Apple filed a quarterly report with the Securities and Exchange Commission, completely disclosing the employment contract with Gilbert Amelio. Specifically, Apple fully disclosed a stock option grant that was given to Amelio for joining the company. This option grant was large and covered one million Apple shares. It was valued at approximately $13 million (B-S value), which represented 66% of the total pay awarded to Amelio in his first partial year with Apple.

	Newly Hired CEOs						Continuing CEOs		
	Ln(TotalComp)			Ln(TotalComp Exclusive of Option Grants)			Ln(TotalComp)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PowerIndex		0.004			0.026		0.093***		
		(0.047)			(0.041)		(0.013)		
Insider%			0.072			−0.182		0.027	
			(0.318)			(0.275)		(0.066)	
%Appointed									0.094
									(0.060)
Max	0.223**	0.222**	0.327**	0.057	0.052	0.166	−0.035	−0.070*	−0.059
	(0.113)	(0.113)	(0.139)	(0.100)	(0.101)	(0.106)	(0.036)	(0.041)	(0.043)
zROA	0.004	0.004	−0.052	−0.013	−0.012	−0.040	0.137***	0.157***	0.152***
	(0.071)	(0.071)	(0.083)	(0.063)	(0.064)	(0.077)	(0.025)	(0.030)	(0.031)
zRET	−0.145*	−0.145*	−0.184*	−0.017	−0.013	−0.059	0.073***	0.083***	0.078***
	(0.087)	(0.087)	(0.105)	(0.083)	(0.084)	(0.081)	(0.022)	(0.024)	(0.025)
lagged zROA	0.076	0.076	0.087	0.008	0.010	0.033	0.092***	0.098***	0.101***
	(0.058)	(0.058)	(0.064)	(0.054)	(0.054)	(0.057)	(0.020)	(0.024)	(0.025)
lagged zRET	0.060	0.060	0.019	0.065	0.066	0.023	0.040**	0.043**	0.041**
	(0.050)	(0.050)	(0.054)	(0.041)	(0.041)	(0.040)	(0.017)	(0.020)	(0.021)
LnAssets	0.390***	0.389***	0.407***	0.356***	0.353***	0.348***	0.367***	0.374***	0.374***
	(0.024)	(0.025)	(0.027)	(0.019)	(0.020)	(0.022)	(0.008)	(0.009)	(0.009)
Volatility	1.366***	1.366***	1.219***	0.077	0.073	0.066	1.336***	1.278***	1.246***
	(0.322)	(0.323)	(0.392)	(0.227)	(0.227)	(0.274)	(0.090)	(0.110)	(0.116)
SharesOwned%	−4.936***	−4.948***	−2.935	0.195	0.113	2.021	−3.658***	−3.438***	−4.068***
	(1.561)	(1.569)	(4.610)	(1.636)	(1.639)	(4.751)	(0.762)	(0.818)	(1.024)

(*Continued*)

	Newly Hired CEOs						Continuing CEOs		
	Ln(TotalComp)			Ln(TotalComp Exclusive of Option Grants)			Ln(TotalComp)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SharesOwned%^{2}	7.151**	7.173**	1.849	−6.062**	−5.908*	−10.581	4.893***	4.360**	5.554***
	(2.945)	(2.959)	(18.214)	(3.085)	(3.091)	(18.346)	(1.590)	(1.753)	(2.049)
OptionsValue	13.073***	13.082***	12.959***	8.464***	8.530***	8.282***	6.929***	7.230***	6.538***
	(3.844)	(3.862)	(4.077)	(2.116)	(2.166)	(2.151)	(0.630)	(0.689)	(0.689)
Tenure							0.017***	0.018***	0.009
							(0.004)	(0.004)	(0.008)
Tenure^2							−0.301***	−0.288***	−0.098
							(0.095)	(0.106)	(0.161)
N	617	617	451	617	617	451	7,597	5,579	5,264
R^2	0.45	0.45	0.49	0.45	0.45	0.48	0.44	0.44	0.43
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No	No	No	No

Table 6. Incentive alignment mechanism with total CEO compensation — newly hired vs. continuing CEOs.

Description: The results in this table are based on the data in my replicated sample with columns (1)–(6) for newly hired CEOs and columns (7)–(9) for continuing CEOs. The empirical model is ordinary least squares as follows: $ln(TotalComp) = a_0 + a_1 Power_{it} + a_2 Max_{it} + \phi_1 zROA_{it} + \phi_2 zRET_{it} + [Controls_{it}] +$ [year dummies] $+\varepsilon_{it}$, except that the dependent variable is total CEO compensation after excluding the stock option grants (B-S value) in columns (4)–(6). An executive is considered to be a newly-hired CEO if he or she has been the CEO for less than one year whilst an executive is considered to be a continuing CEO if he or she has been the CEO for one year or above. Other variables are defined in the notes to Table 2. All regressions are estimated with the same control variables as in MNS's model plus time fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: The incentive-realignment estimates for newly-hired CEOs are positive and statistically significant whilst those for continuing CEOs are negative and insignificant. This is surprising because the managerial-power view would interpret the results to mean that contract rigging is more common for newly-hired CEOs than for long-standing CEOs. But, newly-hired CEOs could not have rigged a previously-set compensation as their pay is determined *ex ante*. The statistically significant results for newly hired-CEOs are justified as they are given large startup stock option grants *ex ante* to align their incentives with shareholders. Further, the pay-performance sensitivity varies substantially between newly-hired and continuing CEOs. Specifically, pay is not tied to firm performance for newly-hired CEOs whereas this relation is positive and significant for continuing CEOs.

firm performance is an inappropriate measure of accomplishments of such CEOs. In the MNS data, firm performance is measured over a one-year period, whereas newly-hired CEOs typically serve less than a full year by the end of the first fiscal year with their new employers. This mismatch suggests that part of firm performance is attributable to the performance of the outgoing CEO, which was typically poor enough to trigger CEO turnover. Therefore, some extant studies have excluded newly-hired CEOs when modeling CEO compensation because compensation for new CEOs are not necessarily tied to firm performance (see, for example, Garvey and Milbourn, 2006; Fee and Hadlock, 2003; Murphy, 2002; Barro and Barro, 1990). Consistent with this claim, my results in Table 6 indicate that pay for newly-hired CEOs is tied neither to contemporaneous nor to lagged firm performance, whereas pay for continuing CEOs is tied positively and strongly to both of these performance variables.

4.2 Firms Alter Incentive Contracts *ex post* Amid Poor Stock-price Performance

The second non-capture explanation is that firms alter the incentive contracts of their managers *ex post* after they experience poor stock-price performance. This explanation is consistent with the increased use of stock options to reward employees during the MNS 1992–2003 sample period (see Hall and Liebman, 1998; Hall and Murphy, 2003). Declining stock prices rendered the value of outstanding stock options worthless, which means that employees were more likely to leave the company. Those that staid had financial incentives that had become less closely aligned with shareholder value.

To retain and motivate employees, firms routinely reprice employee stock options. Oyer and Schaefer (2005) and Carter and Lynch (2004) find that employee turnover was lower in firms that repriced stock options. Similarly, repricing might help to retain top executives. For instance, Chidambaran and Prahhala (2003) find that CEO turnover was lower in firms that repriced the CEO's options than for firms that repriced non-CEO executive's options only.

To empirically examine the contract realignment effect in firms with poor performance, I create a dummy variable for poor stock-price performance (*LowRET*) and another dummy variable for poor accounting performance (*LowROA*). *LowRET* takes the value one if the stock return of firm i in the compensation year is below the median stock return of all ExecuComp firms during the same year, and zero otherwise. *LowROA* is analogously defined using the accounting return in place of the stock return. Next, I create two interaction terms to capture the effect of incentive realignment amid poor stock-price performance and poor accounting performance. Specifically, I interact the incentive realignment variable with the two performance variables to create *Max* × *LowRET* and *Max* × *LowROA*. To precisely account for the pure influence of poor company performance on incentive realignment, newly-hired CEOs are excluded from the replicated sample.[18]

Table 7 presents the regression results of modeling the total compensation of CEOs (see columns (1)–(3)) and non-CEO top executives (see columns (4)–(6)) in the replicated sample after including these variables for poor firm performance. The table shows that top executives are paid significantly less when firms perform badly. The coefficient estimates of the two poor-performance variables are negative and statistically significant at the 5% level.

	CEOs			Non-CEO Top Executives		
	(1)	(2)	(3)	(4)	(5)	(6)
PowerIndex	0.078**			−0.004		
	(0.030)			(0.007)		
Insider%		0.102*			0.062***	
		(0.052)			(0.023)	
%Appointed			0.036			0.025
			(0.093)			(0.018)
Max × *LowRET*	0.055**	0.063**	0.068**	0.049***	0.053***	0.045***
	(0.028)	(0.025)	(0.031)	(0.011)	(0.012)	(0.013)
LowRET	−0.062***	−0.062**	−0.068**	−0.036***	−0.043***	−0.044***
	(0.019)	(0.025)	(0.026)	(0.008)	(0.009)	(0.009)
Max × *LowROA*	0.009	0.052	0.053	−0.002	0.001	0.009
	(0.033)	(0.038)	(0.037)	(0.012)	(0.015)	(0.015)
LowROA	−0.095*	−0.157***	−0.138***	−0.096***	−0.103***	−0.091***
	(0.049)	(0.031)	(0.032)	(0.010)	(0.013)	(0.012)
Max	0.014	−0.008	−0.005	0.022	0.021	0.026
	(0.034)	(0.042)	(0.042)	(0.014)	(0.015)	(0.016)

(*Continued*)

[18] Not reported, the results are similar if newly-hired CEOs are included.

	CEOs			Non-CEO Top Executives		
	(1)	(2)	(3)	(4)	(5)	(6)
zROA	0.041*	0.047**	0.042*	0.004	0.011	0.009
	(0.023)	(0.021)	(0.021)	(0.008)	(0.009)	(0.009)
zRET	0.015	0.023	0.015	0.020**	0.011	0.006
	(0.022)	(0.025)	(0.026)	(0.009)	(0.010)	(0.011)
lagged zROA	0.084***	0.087***	0.091***	0.030***	0.031***	0.030***
	(0.023)	(0.031)	(0.031)	(0.007)	(0.008)	(0.008)
lagged zRET	0.033***	0.035**	0.038**	0.042***	0.040***	0.045***
	(0.012)	(0.014)	(0.015)	(0.004)	(0.005)	(0.005)
N	7,597	5,579	5,264	33,516	24,525	23,302
R^2	0.41	0.42	0.41	0.45	0.46	0.46
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes

Table 7. Incentive realignment mechanism for firms with poor stock-price performance.

Description: To the extent poor firm performance induces contract renegotiation, one would expect that firms would realign incentive structures in such circumstances to increase executive pay. Therefore, I modify the empirical model to include measures of poor firm performance and possibilities of incentive realignment amid poor firm performance as follows: $ln(TotalComp_{it}) = a_0 + a_1 Power_{it} + a_2 Max_{it} + \xi_1 Max_{it} \times LowRET_{it} + \xi_2 LowRET_{it} + \xi_3 Max_{it} \times LowROA_{it} + \xi_4 LowROA_{it} + \phi_1 zROA_{it} + \phi_2 zRET_{it} + [Controls_{it}] +$ [firm dummies] + [year dummies] $+\varepsilon_{it}$. The results in this table are based on the data in my replicated sample, separated into CEOs in columns (1)–(3) and non-CEO top executives in columns (4)–(6). The *LowRET* takes the value of one if stock return of the firm during the compensation year is below the median stock return of all ExecuComp firms during the same year and zero if otherwise. *LowROA* is analogously defined using the accounting return in place of the stock return. Other variables are defined in the notes to Table 2. All regressions are estimated with the same control variables as in MNS's model plus time and firm fixed effects. The only exception is that *Tenure* and $Tenure^2$ are excluded in all regressions for non-CEO top executives. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: The results on incentive realignment after poor stock-price performance are large and significant for not just CEOs but also other top executives. Further, these results are consistent with the widespread practice of option repricing in firms experiencing declining stock prices. In contrast, the results on incentive realignment after poor accounting performance are weak and insignificant.

Overall, my results show that the intensity of incentive realignment during poor stock-price performance is strong for top executives. The coefficient estimates on *Max* × *LowRET* are statistically significant at the 5% level for both CEOs and other top executives. My findings are consistent with the widespread practice of option repricing amid poor stock-price performance because the urge to reprice outstanding stock options is only strong when firms are experiencing declining stock prices. Declining stock prices (rather than declining accounting returns) render the value of outstanding stock options lower than they were negotiated at. As expected, my results indicate that none of the coefficients of $Max \times LowROA$ are statistically significant at the 5% level.

5 Discrepancies to Measure Industry-adjusted Performance

In this section, I report the impact of changes in the measure of industry-adjusted performance on the estimated power-rigging effect.

5.1 In-Sample vs. Broad-Sample Standardization of Performance Measures

MNS use their own sample to standardize performance measures. Their standardization procedure is unusual in two ways. First, they use a relatively small sample on which to perform standardization (their own sample), whereas other papers often use a broader sample (for example, all firms in the ExecuComp or Compustat database). Second, they include the firm of interest to compute industry or peer performance, whereas other papers often exclude it from the computation (for example, Gibbon and Murphy, 1990; Janakiraman *et al.*, 1992; Aggarwal and Samwick, 1999; Bertrand and Mullainathan, 2001; Garvey and Milbourn, 2006; Goyal and Wang, 2012).[19]

To standardize performance measures, my paper now relies on a broader sample of approximately 25,000 observations in the ExecuComp, which is significantly larger than 10,404 observations used in the MNS sample. This benchmark sample and my different standardization procedures have three advantages. First, it is more representative. The MNS benchmark excludes many well-known companies (including Microsoft, Dell, and Cisco). Second, because the firm of interest is included in the calculation of industry performance, the broader sample reduces the (unwanted) influence of the test firm on the industry benchmark. Third, this industry benchmark is invariant to changes in sample size. In my robustness tests, I examine the sensitivity of the MNS results to a larger and more representative sample. In particular,

[19] The sensitivity of MNS's main results to the inclusion of the firm of interest to compute industry statistics is beyond the scope of this paper.

I use a newly-constructed expanded CEO sample of 14,208 observations (to be described in the robustness tests), which is significantly larger than the largest subsample used in the MNS regressions of 8,263 observations. My standardization procedure ensures that data on performance variables are constant across my replicated sample and my expanded CEO sample. This invariance property enables me to disentangle how the larger sample size influences MNS-like inference.

To explore the sensitivity of the power-rigging effect to the benchmark, I apply the MNS model to my replicated sample when performance measures are computed with either in-sample or broad-sample standardization methods. These results are reported in columns (4)–(9) of Table 8. For ease of comparison, columns (1)–(3) of the same Table reproduce MNS's main results in columns (3), (5), and (7) of MNS's Table III.

The table shows that the statistical inference of the power-rigging effect were higher with the MNS in-sample standardization. Specifically, the point estimates on *Max* × *PowerIndex* and *Max* × *Insider%* decline from statistical significance at the 5% level to statistical insignificance with my broad-sample standardization. The improvement in statistical significance is mainly due to the smaller standard errors of the estimates with in-sample standardization.[20]

5.2 Winsorization of Industry-Adjusted Performance

MNS winsorize the 5% extreme values of their standardized performance measures. Because there is no theoretical guidance on the proper trimming level for winsorization, I first searched all articles published in the *Journal of Finance* from 1988 to 2011 for "winsorize". Of 59 articles, I eliminated seven for brevity.[21] Figure 1 is the histogram describing the winsorization breakpoints that are used in these studies. The most common trim levels are 0.5% and 1%, which comprise 15% and 79% of these articles. Thus, in

[20] Not reported, the findings are robust to other methods of standardizing performance measures. Specifically, to adjust for the location of the distribution, medians (rather than means) are used to compute industry-adjusted performance. Similarly, to adjust for the scale of the distribution, data on industry-adjusted performance variables are winsorized at the 0.5% or 1% level (to be discussed in the next section). Again, our results are qualitatively identical, implying in-sample standardization improves the likelihood of inferring the power-rigging effect.

[21] Three articles are eliminated because they use either multiple breakpoints or a breakpoint on only one tail of the distribution. The remaining four articles are excluded because they use either absolute levels or standard deviations from the mean as cutoffs to winsorize variables of interest. My inference remains qualitatively similar if these articles are included in the sample.

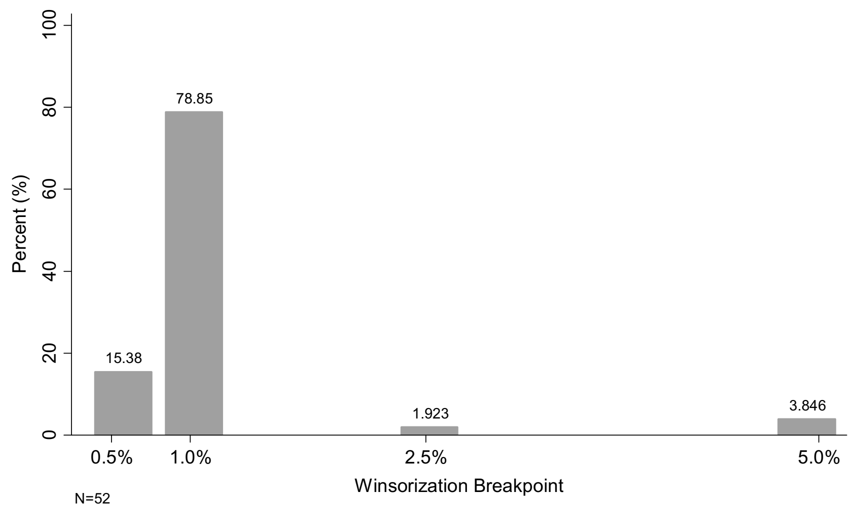

Figure 1. Histogram of winsorization breakpoints for articles published in the journal of finance, 1988–2011.

19 out of 20 studies, the trim levels were less than or equal to 1% extremes. The MNS 5% trim level is unusually high.

To further explore winsorization in the MNS sample at the 5% level, I follow MNS and apply an in-sample standardization method to construct the two performance variables in the replicated sample. Next, I plot the distribution of each variable in unwinsorized data. Figures 2A and 2B present the histograms of the industry-adjusted accounting performance (*zROA*) and stock performance (*zRET*), respectively. Except for several outliers, the performance variables appear normally distributed. A visual examination indicates that winsorization at the 1% level appears sufficient to remove these outliers. Specifically, the smallest and largest values of *zROA* are –2.93 and 2.29, respectively, while those for *zRET* are –2.01 and 2.86, after these variables are winsorized at the 1% level.[22]

Columns (10)–(12) of Table 8 present the results after the two performance variables are winsorized at the 5% level in the replicated sample. My findings indicate that a high winsorization level increases the estimated regression coefficients on the power-rigging variable. Specifically, the coefficients are at least 30% larger in winsorized data compared to those in

[22] After 1% extreme values are winsorized, the smallest and largest values of the two performance variables are close to those under the standard normal distribution, that is, –2.33 and 2.33.

	Published Results			Replicated Results								
	in-sample standardization — data on performance measures are winsorized at the 5% level			in-sample standardization — no winsorization			broad-sample standardization — no winsorization			broad-sample standardization — data on performance measures are winsorized at the 5% level		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PowerIndex	0.049**			0.059**			0.061**			0.059**		
	(0.025)			(0.025)			(0.026)			(0.027)		
Max × *PowerIndex*	0.025**			0.023**			0.020*			0.026*		
	(0.011)			(0.009)			(0.011)			(0.015)		
Insider%		0.022			0.074			0.083			0.073	
		(0.058)			(0.056)			(0.056)			(0.057)	
Max × *Insider%*		0.187***			0.110**			0.120			0.192**	
		(0.060)			(0.050)			(0.074)			(0.092)	
%Appointed			−0.059			−0.065			−0.072			−0.066
			(0.092)			(0.094)			(0.094)			(0.094)
Max × *%Appointed*			0.060**			0.043*			0.061*			0.087**
			(0.027)			(0.025)			(0.032)			(0.040)
zROA	0.053***	0.071***	0.073***	0.033**	0.062***	0.055***	0.062***	0.085***	0.077***	0.089***	0.117***	0.112***
	(0.020)	(0.019)	(0.020)	(0.016)	(0.015)	(0.016)	(0.019)	(0.019)	(0.021)	(0.031)	(0.030)	(0.032)
zRET	0.0368*	0.0391**	0.047***	0.024	0.042***	0.043***	0.031	0.044***	0.040**	0.041*	0.054***	0.051***
	(0.019)	(0.015)	(0.017)	(0.017)	(0.014)	(0.015)	(0.019)	(0.016)	(0.017)	(0.022)	(0.018)	(0.019)
lagged zROA	0.071***	0.089***	0.086***	0.064***	0.073***	0.077***	0.087***	0.087***	0.093***	0.104***	0.131***	0.126***
	(0.017)	(0.024)	(0.022)	(0.016)	(0.022)	(0.022)	(0.022)	(0.028)	(0.029)	(0.030)	(0.041)	(0.040)
lagged zRET	0.047***	0.036**	0.041***	0.041***	0.038***	0.039***	0.037***	0.042***	0.043***	0.049***	0.047***	0.049***
	(0.011)	(0.014)	(0.014)	(0.010)	(0.013)	(0.013)	(0.012)	(0.014)	(0.014)	(0.015)	(0.018)	(0.018)
N	8, 263	6,121	5,797	8,182	6,010	5,696	8,214	6,030	5,716	8,214	6,030	5,716
R^2	0.42	0.42	0.41	0.40	0.41	0.40	0.40	0.41	0.40	0.40	0.41	0.40

(*Continued*)

	Published Results			Replicated Results								
	in-sample standardization — data on performance measures are winsorized at the 5% level			in-sample standardization — no winsorization			broad-sample standardization — no winsorization			broad-sample standardization — data on performance measures are winsorized at the 5% level		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 8. In-sample vs. broad-sample standardization procedures on rigging with total CEO compensation.

Description: This table presents results in MNS's model to determine the sensitivity to their main results to various procedures to standardize and winsorize the two performance measures. The results in this table are based on the data in my replicated sample. Columns (1)–(3) reproduce the results of columns (3), (5) and (7) from MNS's Table III; columns (4)–(6) and (7)–(9) present the results when industry-adjusted performance measures are constructed using in-sample standardization procedure and broad-sample standardization procedure, respectively; and columns (10)–(12) present the results when industry-adjusted performance measures are constructed using broad-sample standardization procedure with the upper and bottom 5 percentiles of performance measures are winsorized. Other variables are defined in the notes to Table 2. All regressions are estimated with the same control variables as in MNS's model plus time and firm fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: The MNS results presented as evidence as power-rigging are sensitive to how performance variables are constructed. In their specification, when no winsorization is performed on performance variables in the replicated sample, the power-rigging estimates are statistically more significant with in-sample standardization than those estimated with broad-sample standardization. Further, winsorization of performance variables at the upper and bottom 5 percentiles of performance variables further improve the statistical significance of the power-rigging variables.

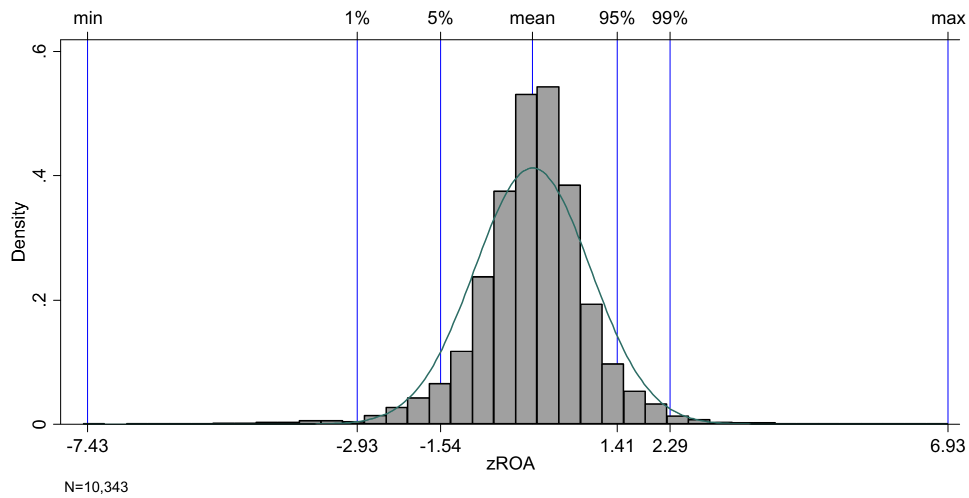

Figure 2A. Industry-adjusted accounting performance — *zROA*.

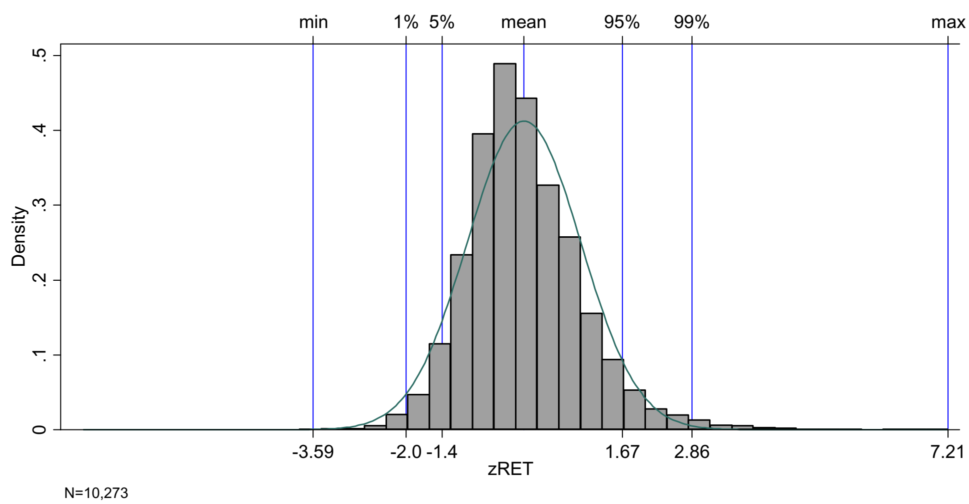

Figure 2B. Industry-adjusted stock-price performance — *zRET*.

unwinsorized data as shown in columns (7)–(9). However, winsorization also increases the standard errors of these estimates. Overall, the increase in the estimated regression coefficients outweighs the increase in their standard errors. Therefore, my findings suggest that winsorization increases (at best) and does not decrease (at worst) the likelihood of statistical inference of the power-rigging effect. The enhancement in statistical inference is particularly noticeable for *Max* × *Insider%* and *Max* × *%Appointed* because they go from being statistically significant at the 5% level in winsorized data to statistically in significant at the same level in unwinsorized data.

Next, I apply the MNS model in my replicated sample when data on industry-adjusted performance measures are winsorized at the 0%, 0.5%, 1%, 5%, and 10% levels. The results are reported in Table 9. My findings show that none of the coefficients on the power-rigging variables are statistically significant at the 5% level when data are winsorized at the 0.5% and 1% levels. In contrast, two of the three are statistically significant at the 5% level when the data are winsorized at the 5% level, and all are significant when the data are winsorized at the 10% level.

	Winsorization Breakpoints				
Panel A: *PowerIndex*	(1)	(2)	(3)	(4)	(5)
	unwinsorized data	0.5% extremes	1% extremes	5% extremes	10% extremes
Max × *PowerIndex*	0.020*	0.021*	0.021*	0.026*	0.040**
	[0.080]	[0.093]	[0.095]	[0.085]	[0.030]
N	8,214	8,214	8,214	8,214	8,214
R^2	0.40	0.40	0.40	0.40	0.40
Time and firm FE	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes
Panel B: *Insider%*	(1)	(2)	(3)	(4)	(5)
	unwinsorized data	0.5% extremes	1% extremes	5% extremes	10% extremes
Max × *Insider%*	0.120	0.141*	0.141*	0.192**	0.254**
	[0.107]	[0.069]	[0.075]	[0.038]	[0.017]
N	6,030	6,030	6,030	6,030	6,030
R^2	0.41	0.41	0.41	0.41	0.41
Time and firm FE	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes

(*Continued*)

Panel C: *%Appointed*	Winsorization Breakpoints (1)	(2)	(3)	(4)	(5)
	unwinsorized data	0.5% extremes	1% extremes	5% extremes	10% extremes
Max × *%Appointed*	0.061*	0.062*	0.061*	0.087**	0.128***
	[0.058]	[0.063]	[0.069]	[0.029]	[0.009]
N	5,716	5,716	5,716	5,716	5,716
R^2	0.40	0.40	0.41	0.40	0.40
Time and firm FE	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes

Table 9. Degree of winsorization on rigging with total CEO compensation.

Description: The results in this table are based on the data in my replicated sample when data on industry-adjusted performance measures are winsorized at various breakpoints: unwinsorized in column (1), and at 0.5%, 1%, 5%, and 10% in columns (2)–(5), respectively. The empirical model follows MNS: $ln(TotalComp_{it}) = a_0 + a_1 Power_{it} + a_2 Max_{it} \times Power_{it} + \phi_1 zROA_{it} + \phi_2 zRET_{it} + [Controls_{it}] +$ [firm dummies] + [year dummies] $+\varepsilon_{it}$, where *TotalComp* is total CEO compensation and *Power* is CEO power and measured by either *PowerIndex* in Panel A, *Insider%* in Panel B, or *%Appointed* in Panel C. Other variables are defined in the notes to Table 2. All regressions are estimated with the same control variables as in MNS's model plus time and firm fixed effects. For brevity, only the power-rigging variables are presented in this table. *P-values* are reported in brackets. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: In the MNS specification with broad-sample standardization of the two performance variables, a higher winsorization level of these performance variables increases the statistical significance on the power-rigging variable.

To examine how winsorization changes the incentive realignment effect during poor stock-price performance, I also perform similar regression analyses in the replicated sample as in columns (1)–(3) of Table 7 using winsorized data. The results are reported in columns (4)–(6) of Table 10. For comparison purposes, I reproduce the results in columns (1)–(3) of Table 7 and in columns (1)–(3) of Table 10. As expected, the incentive-realignment effects during poor stock-price performance are broadly weaker in winsorized data. Winsorization renders the coefficient estimates on *Max* × *LowRET* to be statistically insignificant at the 5% level (see columns (4)–(6)). The drop in statistical significance is mainly the result of the sharp increase

	unwinsorized data on *zROA* and *zRET*			winsorized data on *zROA* and *zRET*		
	(1)	(2)	(3)	(4)	(5)	(6)
PowerIndex	0.078**			0.077**		
	(0.030)			(0.030)		
Insider%		0.102*			0.105**	
		(0.052)			(0.052)	
%Appointed			0.036			0.045
			(0.093)			(0.093)
Max × LowRET	0.055**	0.063**	0.068**	0.079*	0.091*	0.097*
	(0.028)	(0.025)	(0.031)	(0.046)	(0.048)	(0.057)
LowRET	−0.062***	−0.062**	−0.068**	−0.063***	−0.068***	−0.075***
	(0.019)	(0.025)	(0.026)	(0.019)	(0.024)	(0.025)
Max × LowROA	0.009	0.052	0.053	0.046	0.069	0.071
	(0.033)	(0.038)	(0.037)	(0.045)	(0.059)	(0.058)
LowROA	−0.095*	−0.157***	−0.138***	−0.100**	−0.155***	−0.135***
	(0.049)	(0.031)	(0.032)	(0.046)	(0.031)	(0.031)
Max	0.014	−0.008	−0.005	−0.022	−0.029	−0.023
	(0.034)	(0.042)	(0.042)	(0.062)	(0.075)	(0.077)
zROA	0.041*	0.047**	0.042*	0.056	0.054	0.050
	(0.023)	(0.021)	(0.021)	(0.037)	(0.036)	(0.036)
zRET	0.015	0.023	0.015	0.027	0.032	0.020
	(0.022)	(0.025)	(0.026)	(0.025)	(0.030)	(0.031)
lagged zROA	0.084***	0.087***	0.091***	0.097***	0.131***	0.125***
	(0.023)	(0.031)	(0.031)	(0.031)	(0.043)	(0.041)
lagged zRET	0.033***	0.035**	0.038**	0.047***	0.041**	0.045**
	(0.012)	(0.014)	(0.015)	(0.015)	(0.018)	(0.019)
N	7,597	5,579	5,264	7,597	5,579	5,264
R^2	0.41	0.42	0.41	0.41	0.41	0.41
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes

Table 10. Winsorization on incentive realignment mechanism for firms with poor stock-price performance with total CEO compensation.

Description: This table repeats the regressions in columns (1)–(3) of Table 7 in the replicated sample, except that the two industry-adjusted performance measures (*zROA* and *zRET*) are winsorized at the upper and bottom 5 percentiles. The *LowRET* takes the value of one if stock return of the firm during the compensation year is below the median stock return of all ExecuComp firms during the same year and zero if otherwise. *LowROA* is analogously defined using the accounting return in place of the stock return. Other variables are defined in the notes to Table 2. Other variables are defined in the notes to Table 2. All regressions are estimated with the same control variables as in MNS's model plus time and firm fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: Winsorization of the two performance variables at the top and bottom 5 percentiles lowers the statistical significance on the estimates of incentive realignment after poor stock-price performance.

in standard errors of these estimates, although winsorization broadly and mildly inflates the coefficient estimates.

6 Conclusion and Future Research

My paper has shown that the data do not speak as strongly in favor of the power-rigging hypothesis as MNS have argued. This is not to argue that CEOs do not necessarily rig their incentives; it is merely to argue that the MNS evidence cannot distinguish empirically between the optimal contracting view and the power-rigging view.

Although MNS have constructed a theoretical model, their empirical test did not examine the contract renegotiation behavior directly. Specifically, MNS did not examine how the level and components of CEO pay *ex post* deviated from those specified in the *ex ante* CEO employment agreement. Future research could investigate whether terms specified in the *ex ante* CEO employment contract deviated from the actual pay reported in the company financial statements. It will be interesting to learn how contract renegotiation varies with CEO influence and firm performance.

It would also help to have better theoretical guidance on what proper benchmarks and winsorization levels are. At least in this study's case, a seemingly small change in how industry performance benchmarks were constructed caused meaningful changes in inferences. This is not just an MNS-specific issue, but one of broader concern. It is worth to ask whether there are other studies that are sensitive to the winsorization level.

In the end, the optimal-contracting view and the power-rigging view will remain difficult to disentangle, even conceptually. In light of substantial monitoring costs, it could be optimal *ex ante* to allow CEOs to rig their compensation *ex post* because shareholders are too diffuse to prevent it. Then again, shareholders may be diffuse *ex ante* because allowing CEOs to rig *ex post* is a commitment device to reduce shareholders' monitoring efforts. The optimal contracting view is so flexible that it is difficult to understand what behavior would not be consistent with some form thereof. In any case, future research will have to improve the identification and power of the tests, especially with respect to the origin of CEO power, if it seeks to understand the two views.

Appendix A: Robustness Tests

To examine the robustness of the incentive realignment effect, I include the power-rigging variable in all the regressions that are presented in this paper. Not reported, the results are robust to this inclusion. In sum, after including the incentive realignment variable, none of the power-rigging variables are statistically significant at the 5% level.[23] My results remain qualitatively similar if I exclude Apple Inc. from my replicated sample because Guthrie *et al.* (2012) find that compensation to Steve Jobs is an outlier.

My findings are robust and qualitatively similar in MNS's own data.[24] When the incentive realignment variable is included in the MNS model, none of the power-rigging variable are statistically significant at conventional levels. Similarly, estimates on the incentive realignment variable are statistically significant at the 5% level while those on the power-rigging variable are not when the CEO power variable is mean-centered.[25] Again, my findings are equally robust to compensation of other top executives.

As expected, the incentive realignment effect during poor stock-price performance is considerably weaker in the MNS data because they winsorize data on industry-adjusted performance. Specifically, none of the estimates on *Max* × *LowRET* are statistically significant at conventional levels.

To further examine the sensitivity of the power-rigging effect in a more representative sample, I use all ExecuComp firms with valid observations from 1992 to 2003, which is the same time period covered by the MNS data (herein referred to as the expanded CEO sample). Because ExecuComp has no data on *Insider%* and *%Appointed*, my measure of CEO power is just *PowerIndex* in the expanded CEO sample. The expanded CEO sample is an unbalanced panel of 14,028 CEO-year observations, which has approximately 70% more observations than the largest subsample that is used in MNS's regressions. In the expanded CEO sample, performance measures are

---

23 The only exception is that the coefficient estimate on *Max* × *%Appointed* in the regression to examine the incentive alignment mechanism for firms experiencing poor stock-price performance (see column (6) of Table 7) is significant at the 5% level in the sample of non-CEO top executives.

24 The journal editor assisted me in obtaining MNS's data, enabling me to replicate their main results.

25 The only exception is that the estimate on *Max* × *Insider%* remains significant at the 5% level after the inclusion of the incentive realignment variable. This effect is robust to whether the CEO power variable is mean-centered. Despite this, the power-rigging effect inferred from this CEO power variable should be interpreted with caution because *Insider%* may reflect CEO succession process. Furthermore, this variable is measured with errors and is vulnerable to sampling bias. Appendices B and C contain a detailed discussion of the data problems of this variable.

computed using the broad-sample standardization method with no treatment for winsorization.

Table A.1 presents regression results in the expanded CEO sample. Columns (1)–(3) report regression estimates with untreated CEO power variables, whereas columns (4)–(5) report those after the CEO power variable is mean-centered. In the expanded CEO sample, the results are qualitatively similar to those reported in my replicated sample (see columns (4), (7), and (10) in Table 3, and columns (1) and (4) of Table 4). First, the power-rigging effect is economically and statistically significant in the MNS model (see column (1)). After including the incentive realignment variable, this effect decreases substantially (see column (2)), and becomes statistically insignificant at the 5% level. My results indicate that when the CEO power variable is mean-centered, the incentive realignment estimate is statistically significant at the 5% level, whereas the power-rigging variable is not statistically significant at the same level (see columns (4)–(5)).

To explore whether incentive realignment is more prevalent for newly-hired CEOs in a more representative sample, the expanded CEO sample is divided into two groups: newly-hired CEOs and continuing CEOs. Columns (1)–(2) of Table A.2 presents the OLS regression results for two measures of compensation for newly-hired CEOs: total compensation and total compensation exclusive of stock option grants. As expected, I find that the coefficient estimate of the incentive-realignment variable is positive and statistically significant for newly-hired CEOs. However, this estimate is considerably less positive and statistically insignificant after stock option grants are excluded from total compensation. Again, these results suggest that large initial stock option grants are given to newly-hired CEOs to align their incentives with those of shareholders.

Column (3) presents the OLS regression results for total compensation for continuing CEOs, whereas those in a fixed-effects model are presented in column (4). As expected, I find that the incentive realignment variable is statistically insignificant in the OLS model. Similarly, in a fixed-effects model, this estimate is 33.33% smaller than the one reported in the expanded CEO sample in column (3) of Table A.1. Furthermore, the estimate is marginally significant at the 10% level. These results are consistent with results reported earlier as they show that it is newly-hired CEOs (rather than continuing CEOs) who appear to rig their own compensation. Again, this statistically significant relation is spurious.

	untreated CEO Power: *PowerIndex*			mean-centered CEO power: *PowerIndex*^{mc}	
	(1)	(2)	(3)	(4)	(5)
Max		0.016	0.057***		0.059***
		(0.031)	(0.022)		(0.022)
PowerIndex	0.035*	0.036*	0.044**	0.037*	0.036*
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Max × *PowerIndex*	0.027***	0.023*		0.021*	0.023*
	(0.009)	(0.013)		(0.013)	(0.013)
zROA	0.036**	0.033**	0.034**	0.054***	0.033**
	(0.015)	(0.016)	(0.016)	(0.013)	(0.016)
zRET	0.033**	0.028*	0.028*	0.065***	0.028*
	(0.013)	(0.015)	(0.015)	(0.010)	(0.015)
lagged zROA	0.063***	0.063***	0.063***	0.065***	0.063***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
lagged zRET	0.051***	0.051***	0.051***	0.052***	0.051***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
N	14,028	14,028	14,028	14,028	14,028
R^2	0.36	0.36	0.36	0.36	0.36
VIF(*Max*)	N/A	12.24	5.74	N/A	5.73
VIF(*Max* × *Power*)	3.82	8.16	*N/A*	1.21	1.21
Time and firm FE	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes

Table A.1. Rigging with total CEO compensation in expanded CEO sample.

Description: This table repeats regressions in columns (4), (7) and (10) of Table 3 and columns (1) and (4) of Table 4, except that the data are based on my expanded CEO sample. In constructing this sample, I follow MNS's data requirements to extract all observations available in ExecuComp. My expanded CEO sample has 14,028 observations. This implies an increase of approximately 70% in sample size relative to the largest subsample used in the MNS regressions. Columns (1)–(3) report the results when data on *PowerIndex* are untreated for mean-centering and columns (4)–(5) report the ones when data on *PowerIndex* are mean-centered. All regressions are estimated with the same control variables as in MNS's model plus time and firm fixed effects. Other variables are defined in the notes to Table 2. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: My results using the replicated sample are robust and qualitatively similar in a larger and more representative sample. In particular, the MNS results presented as evidence of power-rigging are sensitive to the inclusion of the incentive realignment variable and to whether CEO power variables are mean-centered. The power-rigging estimates drop significantly after including the incentive realignment variable in the MNS specification.

	Newly-hired CEO		Continuing CEOs	
	(1)	(2)	(3)	(4)
	Ln(Total Comp)	Ln(TotalComp Exclusive of Stock Option Grants)	Ln(Total Comp)	Ln(Total Comp)
PowerIndex	0.042	0.037	0.090***	0.051**
	(0.043)	(0.037)	(0.010)	(0.021)
Max	0.275***	0.097	0.034	0.038*
	(0.091)	(0.075)	(0.025)	(0.023)
zROA	−0.100*	0.002	0.087***	0.058***
	(0.057)	(0.044)	(0.017)	(0.016)
zRET	−0.160**	−0.005	0.029*	0.041***
	(0.072)	(0.062)	(0.017)	(0.015)
lagged zROA	0.056	−0.022	0.073***	0.067***
	(0.036)	(0.033)	(0.014)	(0.014)
lagged zRET	0.028	0.064**	0.050***	0.056***
	(0.034)	(0.028)	(0.012)	(0.010)
N	1,018	1,017	13,010	13,010
R^2	0.38	0.36	0.40	0.37
Time FE	Yes	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Other Controls	Yes	Yes	Yes	Yes

Table A.2. Incentive realignment on total CEO compensation for newly hired versus continuing CEOs — expanded CEO sample.

Description: The results in this table are based on the data in my expanded CEO sample with columns (1)–(2) for newly hired CEOs and columns (3)–(4) for continuing CEOs. The empirical model is OLS and follows the one in Table 6, except that firm fixed-effects are included for the regression in column (4). The dependent variable is total CEO compensation, except that total compensation exclusive of stock option grants (B-S value) is used for the regression in column (2). All regressions are estimated with the same control variables as in MNS's model plus time and/or firm fixed effects. Other variables are defined in the notes to Table 2. Robust standard errors are reported in parentheses. ***, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: My results on the disparity of incentive realignment and pay-performance sensitivity between newly-hired and continuing CEOs in the replicated sample are robust and qualitatively similar in a larger and more representative sample. The incentive-realignment estimates for newly-hired CEOs are large and statistically significant whilst those for continuing CEOs are small and insignificant. Further, the incentive-realignment estimates drop considerably after stock options grants are excluded from total compensation of newly hired CEOs. Further, pay is not tied to firm performance for newly-hired CEOs whereas this relation is positive and significant for continuing CEOs.

	Continuing CEOs	
	(1)	(2)
PowerIndex	0.052**	0.045**
	(0.021)	(0.022)
Max × *PowerIndex*		0.019
		(0.013)
Max × *LowRET*	0.068***	0.070***
	(0.020)	(0.020)
LowRET	−0.066***	−0.065***
	(0.016)	(0.016)
Max × *LowROA*	−0.017	−0.018
	(0.022)	(0.022)
LowROA	−0.116***	−0.115***
	(0.033)	(0.033)
Max	0.021	−0.014
	(0.026)	(0.036)
zROA	0.020	0.019
	(0.018)	(0.018)
zRET	0.032*	0.032*
	(0.018)	(0.018)
lagged zROA	0.064***	0.064***
	(0.015)	(0.015)
lagged zRET	0.051***	0.051***
	(0.009)	(0.009)
N	13,010	13,010
R^2	0.38	0.38
Time and firm FE	Yes	Yes
Other controls	Yes	Yes

Table A.3. Incentive realignment on total compensation for continuing CEO in firms with poor stock-price performance — expanded CEO sample.

Description: The data of this table are based on my expanded CEO sample. Column (1) reports the regression displayed in Table 7 and column (2) reports results when the interaction variable of rigging is included in the empirical model. The dependent variable is log total CEO compensation. The *LowRET* takes the value of one if stock return of the firm during the compensation year is below the median stock return of all ExecuComp firms during the same year and zero if otherwise. *LowROA* is analogously defined using the accounting return in place of the stock return. Other variables are defined in the notes to Table 2. All regressions are estimated with the same control variables as in MNS's model plus time and firm fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: My results on incentive realignment after poor stock-price performance variable in the replicated sample are robust and qualitatively similar in a larger and more representative sample. The results on incentive realignment amid poor stock-price performance variable are positive and statistically significant.

Table A.3 presents the regression results of the total compensation for continuing CEOs after including variables for poor firm performance. My results are again qualitatively similar to those reported in the replicated sample (see column (1) of Table 7). The coefficient of the contract realignment amid poor stock-price performance remains positive and statistically significant at the 5% level. In contrast, the coefficient of the incentive-realignment variable is statistically insignificant. Similarly, the contract renegotiation effect is asymmetric because incentive contracts are not altered *ex post* in firms experiencing poor accounting performance. My findings imply that firms alter incentive contracts *ex post* only when they experience poor stock-price performance.

In the expanded CEO sample, the results (not reported) and conclusions are robust to various standardization procedures. Specifically, my findings are qualitatively similar when performance measures are standardized using an in-sample or broad-sample procedure. This implies that MNS's main results are fragile and vulnerable to sampling bias.

Contrary to my expectations, winsorization may substantially weaken the power-rigging effect in the expanded CEO sample. I apply the MNS model in the expanded CEO sample when data on industry-adjusted performance measures are winsorized at the 5% level. These results (not reported) indicate that in winsorized data the power-rigging estimate is 22% smaller than the one reported in column (1) of Table A.1. This estimate is marginally significant at the 10% level compared to statistical significance at the 1% level in unwinsorized data. In contrast, estimates on the incentive realignment variable are somewhat robust, albeit more weakly, to this winsorization. The one exception is that the incentive realignment variable is no longer an important determinant of total compensation for newly-hired and continuing CEOs.

Appendix B: Measurement Errors

An examination of MNS's own data on CEO power suggests that measurement errors appear to be considerable for the percentage of inside directors on the board of directors (*Insider%*), and the percentage of the board appointed during the CEO's tenure (*%Appointed*). For example, I follow the MNS definitions to reconstruct the three measures of CEO power based on information extracted from the proxy statements of Abbott Laboratory from 1994 to 2003.[26] Next, I use these reconstructed measures as benchmarks to compute measurement errors (in percentages) for the three measures of CEO power from MNS's data. Table B.1 presents these measurement errors for Abbott Laboratory. With the exception of *PowerIndex*, the measurement errors for *Insider%* and *%Appointed* are substantial.[27] The mean measurement errors for *Insider%* and *%Appointed* are 0.1993 and 0.3248, respectively. They are sizable and even larger than their respective means constructed from the proxy statements.

The year-to-year change in *Insider%* is drastic in MNS's data and inconsistent with the existing findings that a corporate board structure changes slowly over time (see, for example, Hermalin and Weisbach, 1998; Denis and Sarin, 1999). For example, in their data, *Insider%* for Abbott Laboratory was 25% in 1998, fell to 9.52% in 1999, increased to 100% (that is, a board comprised entirely of inside directors) in 2000 and 2001, and fell to 25% in 2002 (see column (5)).

Furthermore, in the MNS data, there are 194 firm-year observations of boards consisting entirely of inside directors, or 2.53% of the total for the sample with nonmissing data of *Insider%*. Of these, 186 (95.88% of the total) are from the period 1996–2003. To cross-check this figure, I extracted data on director characteristics of every company from the Investor Responsibility Research Center (IRRC) database during the same period.[28] Out of a total of 13,064 firm-year observations in the IRRC data from 1996 to 2003, I find that only one observation has a board that consists entirely of inside directors, which is 0.077% of the total.[29]

In addition, the MNS measure of *Insider%* appears unnecessarily complicated, unique, and prone to bias. To construct this variable, MNS extract

---

[26] The first available proxy statement for Abbott Laboratory from the SEC EDGAR database is 1994.

[27] The *PowerIndex* is measured fairly accurately in MNS's data. Out of a total of 10,404 observations with data on *PowerIndex*, I find that my measurement is different from MNS's in only 17 cases, which is 0.16% of the total.

[28] The first available data on director characteristics from the IRRC database is from 1996. The firm characteristics between the IRRC data and MNS's data should be similar because the IRRC data also covers S&P 1500 firms, which MNS used to construct their sample.

[29] This observation is likely to be an error because the company had only one board member during the year. Additionally, boards rarely consist entirely of inside directors for two reasons. First, the surge in shareholder activism during the 1990s created intense pressure on board members — particularly on large companies such as S&P1500 companies — to increase the representation of independent directors on their boards. Second, the 1993 congressional tax code requires that compensation committees must have two or more outside directors or performance-based executive pay in excess of $1 million is not tax deductible (see Anderson and Bizjak, 2003; Hall and Murphy, 2003).

		PowerIndex			*Insider%*			*%Appointed*		
		(1)	(2)	(3) Difference	(4)	(5)	(6) Difference	(7)	(8)	(9) Difference
Year	CEO Name	Proxy	MNS	(2)–(1)	Proxy	MNS	(5)–(4)	Proxy	MNS	(8)–(7)
1994	Duane Burnham	2	2	0	0.1538	0.1538	0.0000	0.3077	1.0000	0.6923
1995	Duane Burnham	2	2	0	0.1538	0.1538	0.0000	0.3077	1.0000	0.6923
1996	Duane Burnham	2	2	0	0.1538	0.1538	0.0000	0.3846	1.0000	0.6154
1997	Duane Burnham	2	2	0	0.1667	**NA**	undefined	0.3333	**1.0000**	0.6667
1998	Duane Burnham	2	2	0	0.3125	0.2500	−0.0625	0.5000	1.0000	0.5000
1999	Miles White	1[#]	2	1	0.1538	0.0952	−0.0586	0.0769	0.0000	−0.0769
2000	Miles White	2	2	0	0.1667	1.0000	0.8333	0.0833	0.0000	−0.0833
2001	Miles White	2	2	0	0.1538	1.0000	0.8462	0.2308	0.1000	−0.1308
2002	Miles White	2	2	0	0.2143	0.2500	0.0357	0.2857	0.3333	0.0476
2003	Miles White	2	2	0	0.2308	NA	undefined	0.3077	NA	undefined
	Mean	1.89	2.00	0.11	0.1810	0.3821	0.1993	0.2789	0.6037	0.3248
	Std. Dev.	0.32	0.00	0.32	0.0524	0.3849	0.3967	0.1277	0.4797	0.3732
	Minimum	1.00	2.00	0.00	0.1538	0.0952	−0.0625	0.0769	0.0000	−0.1308
	Maximum	2.00	2.00	1.00	0.3125	1.0000	0.8462	0.5000	1.0000	0.6923

Note: NA denotes that data are missing in the MNS data and undefined denotes calculation of the difference in values is impossible due to the missing data. *PowerIndex* takes the value one in 1999 according to the company 1999 proxy statement because Miles D. White was named only the Chief Executive Office of Abbott Laboratory on January 1, 1999. Yet, ExecuComp recorded his title as both Chairman of the Board and CEO of the company in that year. This discrepancy is possible because newly appointed CEOs might take up additional titles shortly after their CEO appointments. In the case of Miles D. White, he took the chairman of the board position on April 23, 1999, approximately four months after his CEO appointment. The ExecuComp database updated his title to reflect his additional position as the chairman of the board.

Table B.1. Measurement errors for measures of CEO power for abbott laboratory in MNS's data, 1994–2003.

Description: This table presents the three measures of CEO power for Abbott Laboratory in the MNS data (columns (2) for *PowerIndex*, (5) for *Insider%* and (8) for *%Appointed*) and my reconstructed measures on the same variables based on information from the company proxy statements (columns (1), (4) and (7)). *PowerIndex* assigns the CEO one additional point for being the chair of the board and two additional points for being the chair of the board and also the president of the company; *Insider%* is the percentage of executive directors sitting on the board of directors; and *%Appointed* is the percentage of the board appointed during the CEO's tenure. In my reconstructed measure of *%Appointed*, the CEO is considered as an appointed director.

Interpretation: In the MNS data, *Insider%* and *%Appointed* are measured with errors. The measurement errors are large relative to values constructed from the company's proxy statements.

data from two databases, although the same variable may be constructed using only one database. Specifically, MNS obtain data on board size from the Compact Disclosure database and the number of inside directors from the ExecuComp database. Nevertheless, the two data items can be extracted solely from the Compact Disclosure database. Table B.2 presents the data structure on director characteristics for Abbott Laboratory in the June 1997 version of the Compact Disclosure database. In fact, most studies construct the same variable using data only from the Compact Disclosure database (for example, Coles *et al.*, 2008; Linck *et al.*, 2008). Furthermore, MNS's measure of *Insider%* underestimates the true representation of inside directors on the board. This is because ExecuComp only contains data for top executives, whereas Compact Disclosure contains data for all executives of the firm.

To conduct a systematic analysis on the extent of measurement errors for MNS's measures of *Insider%* and *%Appointed*, for each firm-year observation in the MNS data from 1996 to 2003, I construct the same variables using data from the IRRC database. For each observation, a paired difference is computed by subtracting the value of *Insider%* in their data from its corresponding value constructed from the IRRC data. A paired difference for *%Appointed* is computed analogously. In the IRRC data, *Insider%* refers to the fraction of executive directors sitting on the board of directors and *%Appointed* refers to the fraction of the board appointed during the CEO's tenure. I classify a CEO as an appointed director in the IRRC data. My results are qualitatively similar, even if CEOs are not considered appointed directors in the IRRC data. The measurement error for *%Appointed* is even smaller if CEOs are considered appointed directors.

Table B.3 presents the results of the extent of measurement errors for *Insider%* and *%Appointed* during the period of 1996–2003 in the MNS data and relative to those constructed using the Investor Responsibility Research Center (IRRC) database. Panels A and B compare *Insider%* and *%Appointed*, respectively, between their data and the IRRC data. I find that the mean paired differences of these variables between the two data sources are positive. This suggests that the two measures of CEO power are, on average, biased upward compared to those in the IRRC data. In addition, the mean paired differences are significantly different from zero at the 1% level, which implies that their measurement errors are nontrivial. The two measures of CEO power in the MNS data exhibit substantially more variability than those in the IRRC data. For example, the standard deviation of *Insider%* is 0.1852

ABBOTT LABORATORIES
TICKER SYMBOL: ABT
CUSIP NO: 002824100

DIRECTORS (NAME/AGE/TITLE/REMUNERATION): (SOURCE: PROXY 03/11/97)

AUSTEN, K. FRANK/ 68/ NOMINEE / NA
BURNHAM, DUANE L./ 55/ CHAIRMAN OF THE BOARD, CHIEF EXECUTIVE OFFICER, NOMINEE /$3,761,308
FULLER, H. LAURANCE/ 58/ NOMINEE / NA
HODGSON, THOMAS R./ 55/ PRESIDENT, CHIEF OPERATING OFFICER, NOMINEE /$2,266,238
JONES, DAVID A./ 65/ NOMINEE / NA
OWEN, DAVID/ 58/ NOMINEE / NA
POWELL, BOONE, JR./ 60/ NOMINEE / NA
RAND, ADDISON BARRY/ 52/ NOMINEE / NA
REYNOLDS, W. ANN/ 59/ NOMINEE / NA
SMITHBURG, WILLIAM D./ 58/ NOMINEE / NA
WALTER, JOHN, R./ 50/ NOMINEE / NA
WEISS, WILLIAM L./ 67/ NOMINEE / NA

Table B.2. Data structure on director information in compact disclosure.

Description: This table presents the data structure on director characteristics for Abbott Laboratory in the June 1997 version of the Compact Disclosure database. In the fiscal year 1997, the board of Abbott Laboratory was consisted of 12 directors, two of which were executive directors (bold-faced).

Interpretation: The MNS measure of *Insider%* appears unnecessarily complicated as this variable can be constructed solely from data in the Compact Disclosure database. Specifically, board size can be computed by counting the total number of directors on the board whilst the number of inside directors can be calculated by counting the number of directors who are hired by the company (in bold-faced).

in their data (see column (2), Panel A), which is 57% larger than the one found in the IRRC data.

The greater variability in the MNS data is not attributable to a few outliers. Figures B.1 and B.2 show histograms for *Insider%* and *%Appointed*, using the MNS data (upper panel), the IRRC data (middle panel), and the paired

Panel A: *Insider%* (N = 4,744)				
	(1) IRRC	(2) MNS	(3) Paired Difference	(4) Paired T-statistics
Mean	0.2080	0.2259	0.0179	6.87***
Std. Dev.	0.1180	0.1852	0.1797	
Minimum	0.0000	0.0000	−0.5385	
Maximum	0.7273	1.0000	0.9524	
Panel B: *%Appointed* (N = 4,434)				
	(1) IRRC	(2) MNS	(3) Paired Difference	(4) Paired T-statistics
Mean	0.4470	0.4997	0.0527	14.78***
Std. Dev.	0.2927	0.3906	0.2375	
Minimum	0.0000	0.0000	−1.0000	
Maximum	1.0000	1.0000	1.0000	

Table B.3. Comparing *Insider%* and *%Appointed* in MNS and IRRC Data, 1996–2003.

Description: Panel A reports the summary statistics of *Insider%* in the IRRC data [column (1)], MNS's data [column (2)], the paired difference of *Insider%* between the two data sources [column (3)], and the corresponding paired t-statistics [column (4)]. Similarly, Panel B reports the similar statistics analogously for *%Appointed*. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: Additional evidence on *Insider%* and *%Appointed* in the MNS data are measured with errors. These variables in MNS's data are compared with those constructed from the Investor Responsibility Research Center (IRRC) database in the period of 1996–2003. Averages of the two variables in the MNS data are significantly larger than the ones constructed from the IRRC data. In addition, these measures exhibit substantially more variability in the MNS data than those in the IRRC data.

difference of the same variable between the two data sources (lower panel). The lower panels of Figures B.1 and B.2 show that the variability of the paired differences in *Insider%* and *%Appointed* is large, which suggests that these variables are measured with significant errors in their data.

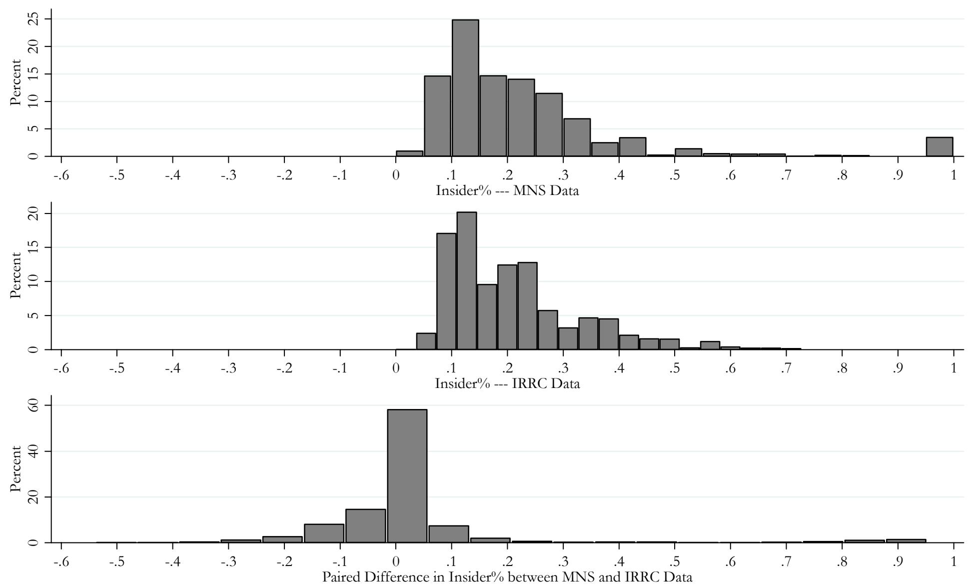

Figure B.1. Comparing *Insider%* in MNS and IRRC Data, 1996–2003.

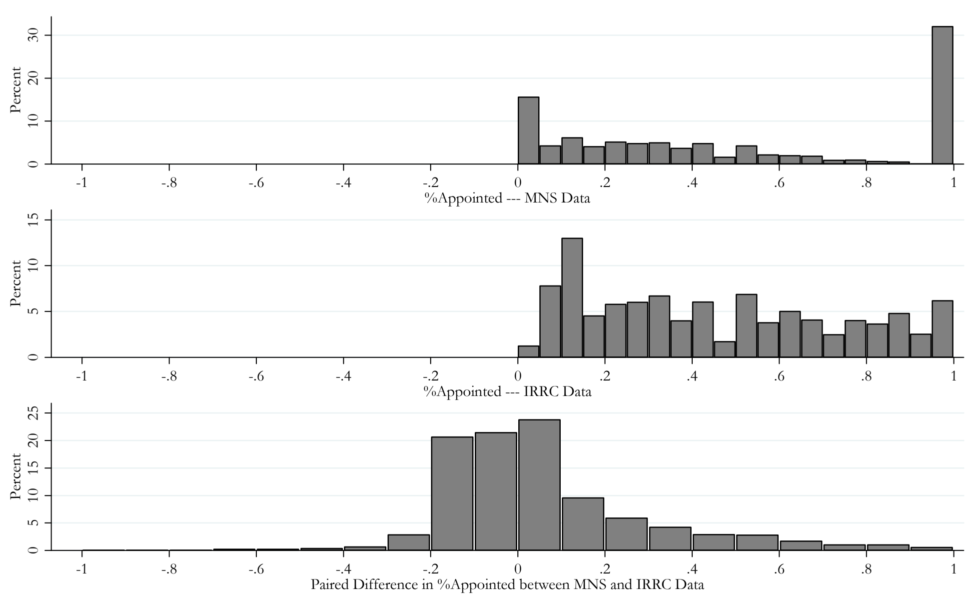

Figure B.2. Comparing *%Appointed* in MNS and IRRC Data, 1996–2003.

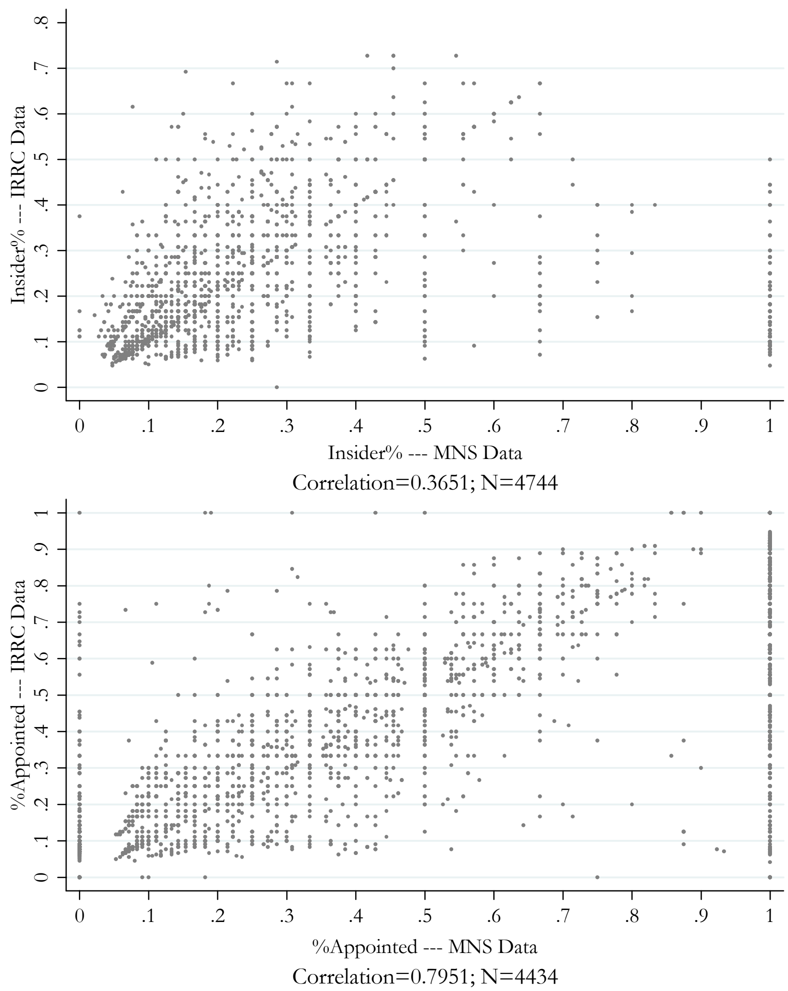

Figure B.3. Plot of *Insider%* and *%Appointed* in MNS vs. IRRC Data, 1996–2003.

Figure B.3 provides an additional perspective on the extent of measurement error in the MNS data compared to the IRRC data. The upper panel of Figure B.3 displays a scatter plot of *Insider%* in their data compared to that in the IRRC data and the lower panel shows the same plot for *%Appointed*. The correlation of *Insider%* between the two data sources is merely 0.37, which suggests that *Insider%* in the MNS data are measured with considerable error.

Appendix C: Sampling Bias

The large number of missing data for *Insider%* and *%Appointed* in the MNS data is an issue because of the possibility of sampling bias. Furthermore, the pattern of missing observations is inconsistent in the MNS data, which suggests that their data may be prone to coding errors. Although data on certain variables are obtained from the same database, several observations in their data show a missing value for a variable and a nonmissing value for another variable, such as the missing value of *Insider%* for Abbott Laboratory in 1997. In their data, data on *%Appointed* are available but data on *Insider%* are missing for Abbott Laboratory in 1997 (see columns (5) & (8) of Table B.1). Because data on *%Appointed* are available, this suggests that MNS must have data on board size from the Compact Disclosure database. Analogously, because data on *Insider%* are missing, this implies that data on the number of inside directors should be missing from the ExecuComp database.[30] However, I find that the ExecuComp database does contain this information.

To examine why missing data are so prevalent for the two variables, I construct a sample of all the observations from the Compact Disclosure database for 23 months, or a total of above 230,000 firm-year observations.[31] For many companies that have missing data for *Insider%* and *%Appointed* in the MNS data, there are in fact data on director characteristics found in the Compact Disclosure database.

To examine to what extent the possibility of sampling bias might affect their findings, column (1) of Table C.1 presents the results from my replicated sample, which replace the cross variable with the incentive realignment variable in the MNS specification. In columns (2) and (3) of Table C.1, I drop observations with missing data on *Insider%* and *%Appointed*. The estimate of the incentive realignment variable is significantly more positive in the samples with data on *Insider%* and *%Appointed* compared to the one in the MNS regression with the largest observation. The greater significance of these results suggests that restricting the sample to only

---

[30] MNS measure *Insider%* by dividing the number of inside directors from the ExecuComp database by the board size of the company from the Compact Disclosure database.

[31] My sample is based on all the observations extracted from the Compact Disclosure database for the June version from 1993–1998, the December version from 1993–1997, the July and November versions in 2000, the February version in 2001, the August–November versions in 2002, and the January–May versions in 2003.

	MNS Full Sample (1)	Drop observations with missing data on *Insider%* (2)	Drop observations with missing data on *%Appointed* (3)
Max	0.057**	0.069**	0.070**
	(0.028)	(0.030)	(0.031)
zROA	0.056***	0.067***	0.061***
	(0.019)	(0.020)	(0.022)
zRET	0.021	0.022	0.019
	(0.019)	(0.019)	(0.021)
lagged zROA	0.087***	0.087***	0.091***
	(0.022)	(0.028)	(0.028)
lagged zRET	0.036***	0.041***	0.042***
	(0.012)	(0.014)	(0.014)
N	8,214	6,030	5,716
R^2	0.40	0.41	0.40
Time and firm FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes

Table C.1. Sampling bias — rigging with total CEO compensation.

Description: This table reports estimates from regressions that use the natural logarithm of the CEO total compensation as a dependent variable using the replicated sample. Columns (1)–(3) present the estimates including the incentive realignment variable in the samples with nonmissing data on *PowerIndex Insider%* and *%Appointed*, respectively. *zROA* and *zRET* are the standardized returns on assets and stock returns, respectively; and *Max* is Max $\{zROA, zRET\}$ for each firm in a given year. The *PowerIndex* assigns the CEO one additional point for being the chair of the board and two additional points for being the chair of the board and also the president of the company; *Insider%* is the percentage of inside directors sitting on the board of directors; and *%Appointed* is the percentage of the board appointed during the CEO's tenure. All regressions are estimated with the same control variables as in MNS's model plus time and firm fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Interpretation: In regressions when *Insider%* and *%Appointed* are used to proxy for CEO power, MNS use a considerably smaller sample size. Thus, restricting the sample to only observations for which the former two variables are available creates a degree of sampling bias. Specifically, the incentive-realignment estimates in the restricted samples in columns (2)–(3) are significantly larger than the one in the full sample in column (1). Consequently, this sampling bias may overstate MNS's power-rigging estimates in their own specification.

observations for which the former two variables are available creates a degree of sampling bias, notwithstanding the greater sample size for the MNS largest-observation sample. Specifically, the magnitude of the coefficient on *Max* increases from 0.057 (p-value of 0.044; column (1)) in their largest-observation sample to 0.069 (p-value of 0.024; column (2)) in the sample with data on *Insider%*, which equals 21.05%. In light of the measurement error problem and the possibility of sampling bias, I put more emphasis on *PowerIndex* as the measure of CEO power when comparing findings between the replicated and published samples.

References

Aggarwal, R. and A. Samwick. 1999. "The Other Side of the Trade-off: The Impact of Risk on Executive Compensation." *Journal of Political Economy* 107: 65–105.

Anderson, Ronald C. and John M. Bizjak. 2003. "An Empirical Examination of the Role of the CEO and the Compensation Committee in Structuring Executive Pay." *Journal of Banking and Finance* 27: 1323–1348.

Balsam, Steven. 2002,. *An Introduction to Executive Compensation*, 1st edn., Academic Press.

Barro, Jason R. and Robert J. Barro. 1990. "Pay, Performance, and Turnover of Bank CEOs." *Journal of Labor Economics* 8(4): 448–481.

Bebchuk, Lucian, Jesse Fried and David Walker. 2002. "Managerial Power and Rent Extraction in the Design of Executive Compensation." *University of Chicago Law Review* 69(3): 751–846.

Bertrand, M. and S. Mullainathan. 2001. "Are CEOs Rewarded for Luck? The Ones Without Principals Are." *Quarterly Journal of Economics* 116: 901–932.

Brenner, M., R. Sundaram, and D. Yermack. 2000. "Altering the Terms of Executive Stock Options." *Journal of Financial Economics* 57(1): 103–128.

Brown, Kareen and Si Li. 2013. "Target Ownership Plans and Corporate Policies." Unpublished Working Paper, Wilfrid Laurier University.

Chance, D., R. Kumar, and R. Todd. 2000. "The 'Repricing' of Executive Stock Options." *Journal of Financial Economics* 57(1): 124–154.

Chidambaran, N. and N. Prabhala. 2003. "Executive Stock Option Repricing, Internal Governance Mechanisms, and Management Turnover." *Journal of Financial Economics* 69: 153–189.

Carter, M. E. and L. J. Lynch. 2004. "The Effect of Stock Option Repricing on Employee Turnover." *Journal of Accounting and Economics* 37: 91–112.

Coles, J., N. Daniel, and L. Naveen. 2008. "Boards: Does One Size Fit All?" *Journal of Financial Economics* 87: 329–356.

Core, J. E. and D. F. Larcker. 2002. "Performance Consequences of Mandatory Increase in Executive Stock Ownership." *Journal of Financial Economics* 64: 317–340.

Denis, David J. and A. Sarin. 1999. "Ownership and Board Structures in Publicly Traded Corporation." *Journal of Financial Economics* 52: 187–223.

Fee, Edward and Charles Hadlock. 2003. "Raids, Rewards and Reputations in the Market for Managerial Talent." *Review of Financial Studies* 16(4): 1315–1357.

Garvey, Gerald and Todd Milbourn. 2006. "Asymmetric Benchmarking in Compensation: Executives are Paid for Good Luck for Bad." *Journal of Financial Economics* 82(1): 197–225.

Gillan, S. L., J. C. Hartzell, and R. Parrino. 2009. "Explicit vs. Implicit Contracts: Evidence from CEO Employment Agreements." *Journal of Finance* 64: 1629–1655.

Gilson, Stuart C. and Michael R. Vetsuypens. 1993. "CEO Compensation in Financially Distressed Firms: An Empirical Analysis." *Journal of Finance* 48: 425–458.

Goyal, Vidhan. K. and W. Wang. 2012. "Provision of Management Incentives in Bankrupt Firms." Working Paper.

Greene, William H. 1993. *Econometric Analysis*, Second edn., Prentice Hall.

Guthrie, Katherine, Jan Sokolowsky, and Kam-Ming Wan. 2012. "CEO Compensation and Board Structure Revisited." *Journal of Finance* 67(3): 1149–1168.

Hall, B. and J. B. Liebman. 1998. "Are CEOs Paid Like Bureaucrats?" *Quarterly Journal of Economics* 113: 653–691.

Hall, Brian J. and Kevin J. Murphy. 2003. "The Trouble with Stock Options." *Journal of Economic Perspectives* 17(3): 49–70.

Hart, O. and J. Moore. 1988. "Incomplete Contracts and Renegotiation." *Econometrica* 56: 755–785.

Hartzell, Jay C. and Laura T. Starks. 2003. "Institutional Investors and Executive Compensation." *Journal of Finance* 58: 2351–2374.

Hermalin, B. and M. Weisbach. 1998. "Endogenously Chosen Boards of Directors and their Monitoring of the CEO." *American Economic Review* 88: 96–118.

Hermalin, B. and M. Weisbach. 1988. "The Determinant of Board Composition." *RAND Journal of Economics* 19(4): 589–606.

Janakiraman, Surya N., Richard A. Lambert, and David F. Larcker. 1992. "An Empirical Investigation of the Relative Performance Evaluation Hypothesis." *Journal of Accounting Research* 30(1): 53–69.

Jensen, M. and K. Murphy. 1990. "Performance Pay and Top-Management Incentives." *Journal of Political Economy* 98: 225–264.

Johnson, S. A., T. C. Moorman, and S. Sorescu. 2009. "A Reexamination of Corporate Governance and Equity Prices." *Review of Financial Studies* 22: 4753–4786.

Klein, Benjamin. 1996. "Why Hold-Ups Occur: The Self-Enforcing Range of Contractual Relationships." *Economic Inquiry* 34: 444–463.

Klein, Benjamin, Robert G. Crawford and Armen A. Alchian. 1978. "Vertical Integration, Appropriable Rents, and the Competitive Contracting Process." *Journal of Law and Economics* 21: 297–326.

Kole, Stacey R. 1997. "The Complexity of Compensation Contracts." *Journal of Financial Economics* 43: 79–104.

Lewellen, Stefan and Andrew Metrick. 2010. "Corporate Governance and Equity Prices: Are Results Robust to Industry Adjustments." Working Paper.

Lie, Erik. 2005. "On the Timing of CEO Stock Option Awards." *Management Science* 51: 802–812.

Lie, Erik, and Randall Heron. 2007. "Does Backdating Explain the Stock Price Pattern Around Executive Stock Option Grants?" *Journal of Financial Economics* 83: 271–295.

Linck, James, Jeffry Netter, and Tina Yang. 2008. "The Determinants of Board Structure." *Journal of Financial Economics* 87: 308–328.

Morse, Adair, V. Nanda, and A. Seru. 2011. "Are Incentive Contracts Rigged by Powerful CEOs?" *Journal of Finance* 66(5): 1779–1821.

Murphy, Kevin J. 2002. "Explaining Executive Compensation: Managerial Power versus the Perceived Cost of Stock Options." *University of Chicago Law Review* 69(3): 847–869.

Murphy, Kevin J. 2003. "Stock-Based Pay in New Economy Firms." *Journal of Accounting and Economics* 34(1–3): 129–147.

Narayanan, M. P., C. Schipani, and Nejat Seyhun. 2007. "The Economic Impact of Backdating of Executive Stock Options." *Michigan Law Review* 105(8): 1589–1642.

Oyer, P. and S. Schaefer. 2005. "Why Do Some Firms Give Stock Options to All Employees?: An Empirical Examination of Alternative Theories." *Journal of Financial Economics* 76(1): 99–133.

Saly, P. J. 1994. "Repricing Executive Stock Options in a Down Market." *Journal of Accounting and Economics* 18(3): 325–356.

Smith, Gavin S. and Peter L. Swan. 2013. "Do Concentrated Institutional Investors Really Reduce Executive Compensation Whilst Raising Incentives?" *Critical Finance Review*, forthcoming.