CEO Compensation and Board Structure Revisited*

Katherine Guthrie
College of William and Mary
(katherine.guthrie@mason.wm.edu)

Jan Sokolowsky
University of Michigan
(jansoko@umich.edu)

Kam-Ming Wan
University of Hong Kong
(kmwan@econ.hku.hk)

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Abstract

Chhaochharia and Grinstein (JF, 2009) estimate that CEO pay decreases by 17% more in firms that were not compliant with the recent NYSE/NASDAQ board independence requirement than in firms that were compliant. We document that 74% of this magnitude is attributable to two outliers out of 865 sample firms. In addition, we find that the compensation committee independence requirement increases CEO total pay, particularly in the presence of effective shareholder monitoring. Our evidence casts doubt on the effectiveness of independent directors in constraining CEO pay as suggested by the managerial power hypothesis.

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JEL Classifications: G34, G38, J31, J33

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Whether board composition affects executive pay has been a contentious issue for decades. Proponents of the managerial power hypothesis (e.g., Bebchuk, Fried, and Walker (2002)) argue that managers' power and influence over their directors allows them to extract rents, for example through excessive pay. An implication of the theory is that making boards more independent from management is key to improving corporate governance.

The spectacular rise in executive pay over the 1990s has made the managerial power hypothesis, with all its implications, a popular view among politicians, regulators, academics, and the media. In the wake of the accounting scandals that led to the Sarbanes-Oxley Act of 2002, the NYSE and NASDAQ revised their listing standards to improve corporate governance. The stock exchanges now require boards to have a majority of independent directors, as well as fully independent nominating, compensation, and auditing committees.

If independent directors are indeed better monitors of CEOs, then CEO pay should decline according to the managerial power hypothesis. To test this prediction, Chhaochharia and Grinstein (2009) (henceforth CG) utilize firms' compliance status before the rule change to identify the causal effect of board composition on CEO pay. The independence mandate is an exogenous constraint imposed by the stock exchanges and provides a quasi-experimental setting. The main advantage to CG's difference-in-difference approach is that they circumvent the endogeneity problem identified by Hermalin and Weisbach (1998, 2003) that has plagued the empirical literature on the effects of board characteristics.

CG find that CEO pay decreases by 17% more in noncompliant firms than in compliant firms, which they interpret as the causal effect of improvements in board independence. Their findings are consistent with the managerial power hypothesis, namely that non-independent directors allow CEOs to extract rents in the form of higher pay.

We reexamine the impact of the new independence mandate on CEO pay using CG's data and methodology. We document that CG's main results are mostly attributable to the decrease in pay for just two CEOs, namely Steve Jobs at Apple and Kosta Kartsotis at Fossil. We argue that Jobs' and Kartsotis' pay are outliers, because they unduly impact the mean estimate of the noncompliance effect and because they do not fit the story of the causal effect of board independence on CEO pay. Dropping just these two firms from the full sample of 865 firms (i.e. 12 out of 5,190 firm-years) reduces the point estimate of the effect of board independence by 74%, rendering it economically insignificant and statistically indistinguishable from zero even at the 20% significance level. As such, the mean causal effect from board independence on CEO pay as identified by CG is not generalizable to large publicly traded firms.[1]

CG's results for compensation committees are also sensitive to the outliers. Excluding the two outliers uncovers an *increase* in CEO pay in firms whose compensation committees are not fully independent prior to the new listing requirements relative to compliant firms. Moreover, the increase in CEO pay is most pronounced in the presence of stronger shareholder monitoring (i.e. blockholder directors and concentrated institutional ownership). These findings are inconsistent with the view that independent directors successfully prevent managers from extracting rents in the form of excessive pay.[2]

[1] The IRRC definition of independence is stricter than those of the NYSE/NASDAQ. In an attempt to adjust for the discrepancies, CG reclassify former employees as independent if three or more years have passed since termination. While reclassifying former employees, however, CG ignore other IRRC disqualifications of independence, such as business relationships. Therefore, they end up treating business relationships inconsistently: former employees with business ties to the firm are considered independent, while directors with business ties who were not formerly employed are not considered independent. An anonymous referee finds that in all the cases he/she checked, the relationships are immaterial under the NYSE/NASDAQ rules. If we treat all such relationships as immaterial, the number of firms not compliant with the new board independence requirements in CG's sample will decrease from 142 to 50. Since the misclassification does not alter our conclusions, we use CG's definition of noncompliance. We plan to investigate this issue in more detail in separate research.

[2] Our findings are also congruent with Anderson and Bizjak (2003) and Wan (2003) in that director independence has little effect on executive pay.

I. The Effect of Board Independence on CEO Pay: Replication of CG's Estimates

Column (1) of Table I reproduces CG's main result on board independence from Table II, column (1) of the published paper: the estimated effect of *noncompliant board* × *after* is −0.192 (with an implied *p*-value of 2.6%). In columns (2) and (3) we present the results from replicating CG's main result *using their data*.[3] The estimates do not match the published results.

Reconciling the differences requires two modifications of the estimates. First, one would have to combine the estimates from two regressions, namely the larger point estimate from the regression that controls for CEO tenure (column 2) and the smaller standard error from the regression that excludes CEO tenure (column 3). As some firms have missing data on CEO tenure, controlling for CEO tenure reduces the sample size from 5,190 to 4,956 observations. Using the full sample by dropping tenure from the regression lowers the magnitude of the estimate slightly to −0.173 (*p*-value of 4.6%). Second, one would have to truncate the estimates after the third decimal place, instead of rounding them to the nearest thousandth.[4, 5]

[Insert Table I here.]

To minimize the possibility that sample selection is driving our subsequent results (and to avoid contention over which sample to use), we use CG's full sample (i.e. 5,190 observations) as the benchmark for comparison throughout our paper. To this end, we complement their data by hand-collecting missing observations on CEO tenure from various sources such as companies'

[3] We also construct our own sample following CG's data requirements (6 years of director data from IRRC, 6 years of CEO pay data, but allowing for missing observations on tenure). Our final sample contains 909 firms (including Apple and Fossil). Our findings become qualitatively stronger when we use our own sample instead of CG's, which suggests that CG's results are also sensitive to sample selection effects. The results are available in Appendix 2 to keep the focus of the paper on the effect of outliers.

[4] CG's reported R^2 is the overall-R^2. We report within-R^2, which is maximized by the fixed effects estimator.

[5] To account for serial correlation within firms, CG use clustered standard errors at the firm-period level. The clusters, however, are not nested within firm-level-panels. As such, the degrees of freedom should be adjusted using the *dfadj* option in Stata. On average, the adjustment increases the estimates of the standard errors by about 10%, but we omit the adjustment to make our results more comparable to CG's. Using the placebo technique suggested by Bertrand, Duflo, and Mullainathan (2004) yields similar results.

websites and proxy statements, Hoover company records, news and reports from Forbes and Business Week.[6] Column (4) of Table I contains the replicated results from the full sample, in which missing observations on CEO tenure have been replaced with hand-collected data. Overall, the magnitude and statistical significance of the coefficient of CG's main result remain quantitatively similar to that in the regression without the tenure variable.[7] The coefficient of −0.179 translates into a drop in CEO pay of 16.4% (*p*-value of 4.1%). Note that after the corrections and additions to CEO tenure observations, the coefficient on tenure becomes statistically significant at conventional levels.

II. The Impact of Outliers on CG's Estimates

Fig. 1 presents the histogram of the change in CEO pay for noncompliant firms. As only 142 sample firms are noncompliant with the new listing requirements, estimates of the mean effect of noncompliance are particularly susceptible to outliers among the noncompliant firms. CG read the proxy statements for some of the noncomplying firms that had the largest drop in compensation. They find that the drop in CEO pay at Adobe Systems and Compuware – which rank 4th and 5th in the distribution – appears to be linked to a reevaluation of incentive pay. However, there is no mention of the three firms with the largest decrease in CEO pay: Steve Jobs at Apple, Kosta Kartsotis at Fossil, and Jack Welch at GE. Apple and Fossil are clearly identifiable outliers among the noncompliant firms.[8,9] We argue that the decrease in CEO pay at

[6] To allow future replication of our results, Appendix I presents the year when the executive was first appointed as the CEO of the company for observations with either missing or incorrect data on CEO tenure in the ExecuComp database.
[7] Throughout the paper, our results are robust to using the actual sample that CG used in their main results (4,956 observations) and to excluding the tenure variable completely from the regressions.
[8] Apple and Fossil constitute outliers based on z-scores exceeding ±3.3, which corresponds to a probability of less than 0.1% of those values occurring (assuming that the change in ln(pay) is normally distributed).
[9] Oracle is a large positive outlier among the compliant firms. Excluding Oracle from our analyses further strengthens our results.

Apple and Fossil makes them outliers for two reasons. First, the decrease is very large in magnitude, particularly for Apple. As such, those two firms unduly influence CG's estimate of the effect of board independence on CEO pay. Second, Apple's and Fossil's CEO pay is idiosyncratic in nature. In a nutshell, Kartsotis insisted on the decrease himself; and Jobs' pay is erratic and tied to unusual circumstances. In other words, neither the magnitude nor determinants of the change in CEO pay at Fossil and Apple are representative of other firms in CG's sample or driven by the recent board mandate. We devote section III to an in-depth look at the circumstances of Kartsotis' and Jobs' pay cuts, but first explore the sensitivity of CG's main result to the outliers.

[Insert Fig. 1 here.]

In column (5) of Table I, we account for the outliers' excessive influence on CG's mean estimate by excluding Apple and Fossil from the regression. The magnitude of the coefficient on *noncompliant* × *after* drops from −0.179 to −0.047, or by 74%.[10] CG's main result becomes statistically insignificant even at the 20% level due to the decrease in its magnitude, and despite the large decrease in its standard error (*p*-value of 23%). Interestingly, excluding the outliers also affects the coefficients and standard errors of all other explanatory variables appreciably, even though the empirical model constrains the coefficients to be equal for compliant and noncompliant firms. In other words, removing Apple and Fossil significantly affects the mean estimates derived from 865 firms. This implies that the relationship between CEO pay and the explanatory variables at Apple and/or Fossil is fundamentally different from the other sample firms.

[10] Our results remain quantitatively similar throughout all our analyses in the paper even if we drop only Apple in the regressions. In this particular regression, the magnitude of the coefficient of CG's main result drops from −0.179 to −0.068 (*p*-value of 9.8%), or by 62%.

An alternative solution to dealing with outliers is to use the least absolute deviations method (i.e. median regression) for estimation, which is less sensitive to extreme observations. The main benefit to using a median regression is that we need not explicitly (and perhaps subjectively) identify outliers. In column (6) of Table I, we present results from the median regression (including Apple and Fossil). Since the inclusion of a large number of variables exponentially increases the time required to obtain quantile regression estimates, we account for firm fixed effects by demeaning all variables and including industry-period dummies instead of industry-year dummies. We estimate bootstrapped standard errors to allow for heteroskedasticity and clustering at the firm-period level (Petersen (2009)). We find that CG's main result is weakened to −0.045 (*p*-value of 28%). We conclude that the median effect of board independence on CEO pay is economically and statistically insignificant.

Conceptually, if board independence indeed affects CEO compensation decisions, its influence should extend to the remuneration of non-CEO top executives. After all, the same directors who negotiate or approve CEO compensation are also responsible for the compensation of other top executives. If board independence strengthens directors' bargaining position vis-à-vis top executive officers, then we would also expect non-CEO executives' pay to decrease in noncompliant firms. In contrast, if CG's main result is driven mainly by outliers in CEO pay, then the board independence requirement should have no effect on the remuneration of non-CEO top executives. Column (7) presents the results. We include all non-CEO top executives with non-missing pay data in ExecuComp in our regression, including those of Apple and Fossil. Again, CG's main result is weakened substantially to −0.031 and remains statistically insignificant at conventional levels (*p*-value of 28%).[11]

[11] The results are robust to excluding some outliers in non-CEO pay changes (the influence of any one outlier on the mean estimate is mitigated by the larger number of observations). Our results also remain quantitatively similar if

To summarize, we provide strong empirical evidence that CG's main finding on the effect of board independence on CEO pay is fragile. In particular, CG infer the effect of noncompliance for a broad sample of 865 large and publicly traded firms primarily from the change in Steve Jobs' and Kosta Kartsotis' pay. We conclude that the mean effect documented by CG is not representative of the sample firms.

III. Do the Outliers Fit the Story?

Panels A and B of Table II present the various components of the total pay to Kosta Kartsotis and Steve Jobs. Kartsotis earned approximately $255,000 annually in years 2000–2004, but his pay dropped to nearly zero in 2005. Similarly, Jobs' total pay ranged from $75 million to $600 million per year between 2000 and 2003, and dropped to a symbolic $1 per year in 2004 and 2005.

[Insert Table II here.]

As the boards of Apple and Fossil did not comprise a majority of independent directors prior to the passage of the board reform, the substantial drop in their CEOs' pay during the post-reform period is – at first sight – consistent with the claim that the board independence requirement indeed affected CEO compensation decisions. Alternatively, it might have been coincidence that CEO pay dropped in these firms and that the drop is related to factors other than the board independence requirement. To disentangle these two competing hypotheses, we examine the individual pay to Kosta Kartsotis and Steve Jobs during our sample period. The plunge in their pay seems to be driven by CEO/firm-specific factors other than the board independence requirement.

we use the average pay of these non-CEO top executives or just the highest-paid non-CEO top executive in our analysis.

A. Kosta Kartsotis

Kosta Kartsotis is the brother of Tom Kartsotis – founder of Fossil, former CEO, and chairman of the board in 2005.[12] Kosta and Tom are the firm's largest shareholders at the beginning of fiscal year 2005, owning about 30% of the firm's shares. Given the Kartsotis' continuing and pervasive influence on the firm – holding the positions of founder, chief executive officer, chairman of the board, and largest shareholders between them – it is highly implausible that the pay cut was caused by the director independence mandate.

In fact, it was Kosta Kartsotis himself, rather than the compensation committee or the board as a whole, who initiated and insisted that his base salary be cut from $255,000 to $0 in 2005. The voluntary cut was motivated by his concern about Fossil's recent stock price performance. In 2005, the stock price of Fossil dropped by about 16%, compared to a 1% increase for the industry average. The pay cut can be described as symbolic, as Kosta Kartsotis' stake in the company exceeded $200 million in 2005.

Finally, even prior to becoming majority-independent, the board followed Kartsotis' recommendation on pay. Kosta Kartsotis was appointed as the CEO of Fossil in October 2000. During the period of 2001–2004, the cumulative return on Fossil's stocks was 398%, while that for the industry average was 207%. Despite outperforming his industry peers, and the compensation committee's explicit recognition in the firm's proxy statements for years 2002–2004 that Kartsotis' pay was below the market median, Kartsotis repeatedly requested that his pay not be raised. His refusal to accept pay increases is contradictory to the claim that the board independence requirement is important to compensation decisions at Fossil.

[12] The information on Fossil comes from its DEF 14A filings (available from http://www.sec.gov/edgar.shtml).

B. Steve Jobs

Steve Jobs went from earning more than $600 million in 2000 to $1 in 2005. What happened at Apple for Steve Jobs to experience such a drastic reduction in pay?[13] Paradoxically, the decline in pay does not reflect a pay cut, but rather temporarily abnormal pay to Jobs in the early sample period 2000–2003. His base salary has remained unchanged at $1 per year since he rejoined the company as interim CEO in September 1997. As Jobs' annual total pay typically consists only of the base salary, his total pay was merely $1 per year in 1998 and 1999; and it returned to $1 per year in 2004–2008.

Jobs' erratic compensation reflects four events. First, in fiscal year 2000, upon accepting the position of permanent CEO at Apple, Jobs was granted options with a Black-Scholes value of $600 million, which was the second largest annual pay ever rewarded to a corporate executive in the U.S. at that time. Later that year, Apple's stock price dropped precipitously, rendering the options worthless. Second, in fiscal year 2001, Jobs received a $90 million bonus for his success as interim CEO during fiscal years 1997–1999. Third, in 2002, the board decided to grant Jobs additional options valued at $90 million, because the previously granted options no longer tied Jobs' pay to firm performance (these new options carried an exercise price of $18.30). Fourth, in 2003, Jobs voluntarily canceled his outstanding options when Apple's shares traded around $14.50. The board chose to replace those options with restricted stocks worth $75 million. In 2004 and 2005, Jobs did not receive any additional stocks or options – his pay went back to the token $1 salary he earned in years 1998 and 1999.

[13] The information on Apple comes from its DEF 14A filings (available from http://www.sec.gov/edgar.shtml), supplemented with information on historical stock prices from CRSP, and disclosure on political contributions from http://www.opensecrets.org.

The firm fixed-effects model used by CG is inadequate to explain the erratic timing and magnitude of Jobs' pay.[14] Specifically, the time series variation in Jobs' total pay violates the matching principle, i.e. Jobs' pay is typically not timed to match his contributions and services rendered during the period. More importantly, Jobs' recorded pay does not always reflect the decisions of the board in that year, but those of prior years. During the entire period of September 10, 1997 to December 1999, Jobs took a total pay of merely $2 for serving as the interim CEO of the company, while the value of Apple's shares more than quadrupled. To reward him for his outstanding achievement during that period, the board granted Jobs a special executive bonus in the form of an aircraft in December 1999. The total cost of the aircraft (including tax benefits) was approximately $90 million and was eventually reported as income to Jobs in 2001 and 2002, because the aircraft was not physically transferred to him until 2001.

Jobs' $600 million option grant in 2000 also violates the matching principle, because it provided him with multiple years' worth of annual stock options at once.[15] As such, this mega grant is equivalent to an early payment for the services that Jobs had not yet rendered, but was expected to render in future years.

In addition, Apple's poor stock performance contributed to Jobs' high pay in years 2000–2003. When Apple's stock price was declining, the board replaced underwater options to maintain incentives. After 2003, when Apple's stock price was rising, no further grants were necessary. This negative relationship between pay and stock performance at Apple contrasts sharply with the empirical relationship found in Table 1: the positive coefficient on stock returns

[14] To obtain a reliable estimate from the firm fixed-effects model, one requires a stable relationship between time-varying economic factors (e.g., firm sales, firm performance, and tenure) and CEO total pay. However, such a relationship is unstable (or at best very weak) for Jobs. As such, the time series variation in his pay is poorly captured by the firm fixed-effects model.

[15] On March 18, 2008, Jobs gave a deposition to the SEC regarding the option backdating case against two top executives at Apple, in which he described that the mega option grant was designed to provide four years' worth of equity upfront. The full text of the deposition is available at http://images.forbes.com/media/2009/04/24/jobs-deposition.pdf.

shows that, on average, CEOs' pay increases with prior year stock returns. Instead, some of Jobs' pay was contingent on prior pay becoming worthless (i.e. new grants were made only because previous grants ended up not costing Apple's shareholders anything).

Despite earning only a $1 salary in 2004 and 2005, Jobs was well compensated for his effort. The market value of his stock holdings increased from $75 million in March 2003 (date of stock grant) to $540 million in September 2005 (end of sample period). Furthermore, the impact of the fluctuations in the value of stock and option grants on CG's finding is exacerbated by Jobs' token salary of $1 over the sample period (small changes in the dollar value of pay lead to large changes in the log value of pay at low income levels). Overall, the magnitude of CG's result seems to reflect a temporary restructuring of incentive pay at Apple rather than a systematic adjustment to the level of pay in large, publicly traded firms in the U.S.[16]

To summarize, we find that including the large drops in pay for Kosta Kartsotis and Steve Jobs leads to false inferences about the effect of board independence on CEO pay for most other firms. The changes at Apple and Fossil do not fit the story of board independence causing a drop in CEO pay.

IV. CEO Compensation, Board Independence, and Shareholder Monitoring

CG contend that effective shareholder monitoring mutes the effect of board independence on CEO pay. We follow CG in allowing the noncompliance effect to vary (i) between firms with

[16] The board room dynamics at Apple also illustrate the shortcomings of formal director independence. Jobs personally contributed $50,000 to the Democratic National Committee on November 1, 2000 (a soft money campaign contribution; after his options became worthless), but not to the Republicans. Then, in September 2002, former vice president Al Gore joined Apple's board and compensation committee as an independent director. Also, from 2003 onward, continuing director Millard Drexler was deemed an independent director, despite his and Jobs' prior interlocking relationship. Until 2002, Jobs was CEO of Apple and served on Gap's board, while Drexler was CEO of Gap and served on Apple's board. Perhaps not coincidentally, Drexler also joined the compensation committee at Apple starting in 2003. Therefore, it is questionable whether the increase in formal board independence made Apple's directors more effective monitors.

and without a non-employee blockholder on the board (as suggested by Core, Holthausen, and Larcker (1999)), and (ii) with institutional ownership concentration (as suggested by Hartzell and Starks (2003)).

A. Blockholder Directors

The data set supplied to us by CG does not contain their measure for the presence of blockholder directors. We follow CG in identifying the presence of director blockholders based on non-employee directors holding 5% or more of their firms' shares in 2002.[17] Column (1) in Panel A of Table III reproduces CG's main result on block ownership from Table VII, column (1) of the published paper: the estimated effect of *noncompliant board* × *after* × *no blockholder* is −0.270 (*p*-value of less than 0.01%). Column (2) presents the results from replicating CG's main result. Again, we fail to replicate CG's published results, particularly the standard errors. If we reversed the standard errors of the point estimates for noncompliant firms with and without blockholder directors, then we would obtain results reasonably close to CG's. In column (3) we present the replicated results based on the full sample. The magnitude of the coefficient of CG's main result drops slightly to −0.262 (*p*-value of 1.3%). CG's results indicate that the board independence requirement led to a large decrease in CEO pay in noncompliant firms, but only in the absence of blockholder directors. Their findings suggest that blockholder directors are an effective monitoring substitute for board independence.

[Insert Panel A of Table III here.]

[17] CG offer two conflicting definitions for blockholder directors. On page 254 of the published article, they define a blockholder director as a nonemployee director who owns *5% or more* of the outstanding shares. However, on page 255, CG write that the ownership cutoff is *more than 5%* of the outstanding shares. We identify 35 noncompliant firms with blockholder directors under the first definition, compared to 34 noncompliant firms with blockholder directors under the second definition. The results are insensitive to which definition we use.

As neither Apple nor Fossil have non-employee blockholder directors on their boards in 2002, we investigate the sensitivity of CG's estimates to the outliers in columns (4)–(6). In column (4), we account for the outliers' excessive influence on CG's mean estimate by excluding them from the regression. The magnitude of the coefficient drops to –0.100, or by 62%, but it maintains its statistical significance with a *p*-value of 2.5%. Contrary to CG's findings, however, we also find a significant increase in CEO pay in noncompliant firms with non-employee blockholder directors (coefficient of 0.111; *p*-value of 7.7%). This result suggests that the board independence requirement has had the unintended consequence of increasing CEO pay in firms with blockholder directors.[18,19]

We present results from the median regression and from non-CEO top executive pay in columns (5) and (6) as alternative ways to examine the robustness of CG's main result to outliers. Our estimate from the median regression is –0.074.[20] The pay of non-CEO top executives responds even less to the board independence mandate (coefficient of –0.039). Neither estimate is statistically distinguishable from zero at conventional significance levels, indicating that outliers drive CG's published result.

B. Institutional Ownership Concentration

[18] In additional robustness tests, we also control for changes in blockholder presence and institutional concentration, as well as interactions between the substitute monitor classifications prior to the rule change with a dummy identifying the period after the rule change. The results remain similar.

[19] Using our own data set (as opposed to CG's), the noncompliance effect is also positive and highly significant in firms with blockholder directors (coefficient of 0.160). However, the coefficient for noncompliant firms without blockholder directors is of very small magnitude (–0.005) and not distinguishable from zero. The estimation results are available in Appendix 2.

[20] The absence of blockholder directors is the only instance in which using industry-year dummies rather than industry-period dummies in a median regression with demeaned data yields an estimate of the effect of noncompliance that is statistically significant at the 10% level (coefficient of -0.079, *p*-value of 7.2%).

We follow CG in defining institutional ownership concentration as the sum of shares held by the five largest institutional investors relative to total institutional shareholdings in the firm.[21] Note that CG deviate from Hartzell and Starks (2003) in classifying high and low ownership concentration as belonging to the top and bottom quartile of the distribution in their sample, whereas Hartzell and Starks use concentration as a continuous measure. For the sake of comparability to CG's results, we follow CG's classification into quartiles based on all sample firms' 2002 observations.[22]

Column (1) in Panel B of Table III reproduces CG's results on institutional ownership concentration from Table VII, column (2) of the published paper. They find that noncompliant firms with low concentration of institutional holdings decrease CEO pay by 21.2% more than compliant firms (coefficient of -0.238; implied *p*-value of 2.6%). However, CG acknowledge that the decrease in CEO pay in noncompliant firms with low concentration of institutional ownership is not statistically different from the decrease for firms with high concentration, for which they report a statistically insignificant point estimate of –0.176.

[Insert Panel B of Table III here.]

Column (2) in Panel B of Table III displays our replication estimates. The estimates differ greatly from CG's published results, as we find that neither noncompliant firms with high institutional concentration nor noncompliant firms with low institutional concentration have decreased CEO pay relative to compliant firms, as neither estimate is distinguishable from zero at conventional significance levels.

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[21] The data set supplied by CG contains measures of institutional ownership by the top 5 institutions and total institutional ownership, but some observations are missing. We were able to match every firm in CG's data set with institutional ownership data from TFN. To keep the sample consistent throughout the paper, we proceed using our measure of institutional ownership concentration. For each firm, we calculate the average value of institutional concentration over the four quarters in calendar year 2002. The results based on CG's measures are similar.

[22] Our findings remain qualitatively identical when we use the continuous measure of institutional ownership concentration.

Why are our estimates so different from CG's published results?[23] We suspect that the difference stems from a discrepancy in CG's research design and their implementation of it. The research design for institutional ownership concentration – as laid out in their published paper – is problematic. CG allow the effect of noncompliance to differ between firms with high and low institutional ownership concentration (i.e. firms in the top and bottom quartiles), but implicitly constrain the noncompliance effect of firms with institutional concentration in the interquartile range to be zero (i.e. those firms are treated like compliant firms). However, in the implementation CG appear to treat all firms in the bottom three quartiles as having low institutional ownership concentration.

The next two columns present results based on modifications of CG's original methodology. For column (3), we allow the effect of noncompliance to differ across all four quartiles of institutional concentration. As before, we find that noncompliant firms in the top and bottom quartiles of institutional concentration do not decrease CEO pay by more than compliant firms. Since both outliers – Apple and Fossil – belong to the third quartile, it is not surprising that the decrease in CEO pay is concentrated there. The point estimate of –0.804 is unrealistic, as it suggests that board independence in the presence of medium-to-high ownership concentration causes CEO pay to drop by over 55%. For column (4), we redefine low institutional concentration to encompass all noncompliant firms in the bottom three quartiles of the distribution. These results are closest to CG's published results. Note that the low institutional concentration group now includes Apple and Fossil, which drive both the magnitude and significance of CG's result.

[23] Without access to CG's coding or clarifications on the implementation, we are unable to replicate their published results (even using CG's data on institutional ownership). Our replication attempts rely on trial and error. The following section is based on the replication that yields estimates closest to the published results.

As before, we account for the outliers' influence on CG's mean estimate by excluding them from the regression (column (5)); using a median regression (column (6)); and evaluating the effect of board independence on non-CEO executive pay (column (7)). We continue to group the bottom three quartiles into the low concentration category. In all cases, the estimates of the effect of noncompliance on executive pay are economically negligible and statistically indistinguishable from zero.

To summarize, our results indicate that CG's main findings on substitute monitors are not robust and are driven by the decrease in CEO pay at Apple and Fossil. We find no support for the hypothesis that the board independence requirement led to a decrease in CEO pay in noncompliant firms with low (or high) institutional ownership concentration.

V. Compensation Committees

Once we account for the effect of outliers, we find that the requirement for a fully independent compensation committee, rather than that for a majority of independent directors on the board, affects the level of CEO pay.

Column (1) of Table IV reproduces CG's main result from Table II, column (2) of the published paper: the estimated effect of *compensation committee noncompliant* × *after* (hereafter referred to as CC-noncompliant) is –0.014 (*p*-value of 83%). Column (2) of Table IV contains the estimates from the full sample. The magnitude and statistical significance of the coefficient on CC-noncompliance are nearly zero, which if true suggests that the requirement for independent compensation committees has no influence on CEO compensation.

[Insert Table IV here.]

In column (3), we remove Apple and Fossil from the regression.[24] Without the downward influence of Jobs' huge pay decrease on the estimate, the magnitude of the coefficient increases and turns positive. Specifically, the logarithm of CEO pay increases by an additional 0.069 in firms not compliant with the compensation committee independence rule relative to compliant firms, and the result is statistically significant at the 5% level (*p*-value of 2.3%). This result indicates that the requirement for compensation committee independence has not only been ineffective at reducing CEO pay, but has had the presumably unintended consequence of raising CEO pay. We obtain a similar estimate, albeit smaller in magnitude (0.046, *p*-value of 4.3%), from the pay of non-CEO executives (column (4)). While using committees instead of boards impacts the noncompliance coefficients dramatically, the standard errors remain similar (compared to columns (5) and (7) of Table I).[25]

To examine how CEO pay changes in the presence or absence of monitoring substitutes, we follow CG in allowing the CC-noncompliance effect to vary with the presence of blockholder directors and the concentration of institutional investor ownership. In column (5) of Table IV, we find that the increase in CEO pay is concentrated in CC-noncompliant firms with blockholder directors. Numerically, CEO pay in those firms increases by an additional 15% (coefficient of 0.136, *p*-value of 0.4%) when compared to the CC-compliant firms. The effect is economically meaningful and statistically significant at well below the 5% level. We find a similar, but again smaller effect for non-CEO executives. Our results in column (6) show that non-CEO pay rises by an additional 8% (*p*-value of 4.3%) in those firms relative to CC-compliant firms. However, the effect of CC-noncompliance differs only marginally between firms with high and low

[24] Apple's compensation committee did not comply with the new requirements in 2002, but Fossil's did. We continue to remove both firms from the regression for consistency in the presentation of our results. The results are virtually unchanged if we keep Fossil in the sample, as its impact on the estimate for compliant firms is negligible.

[25] Using median regressions, we find no significant effect of compensation committee independence on executive compensation levels throughout this section.

institutional concentration (columns (7) and (8)), but it remains positive. Based on CG's sample, we conclude that CC-noncompliance leads to an increase in CEO pay, regardless of institutional shareholder concentration.[26]

Taken together, our findings indicate that the requirement for compensation committee independence produces a perverse effect on executive remuneration in noncompliant firms, particularly in the presence of blockholder directors.[27]

VI. Conclusion

Using corporate governance listing requirements imposed by the US stock exchanges as a quasi-natural experiment to examine whether board structure influences CEO remuneration, Chhaochharia and Grinstein (2009) find that CEO pay decreases by about 17% in firms with noncompliant boards relative to firms with a majority of independent directors.

We reexamine CG's evidence using their data and methodology and find that the results are fragile. Their results are driven by two outlier firms in CEO pay, namely Apple and Fossil. After excluding the two outlier firms (12 firm-year observations) from the full sample of 865 firms (5,190 firm-year observations), our results indicate that (i) board independence does not affect the level of CEO pay; (ii) compensation committee independence causes CEO pay to increase; and (iii) the increase in CEO pay occurs only in the presence of blockholder directors

[26] We obtain economically and statistically highly significant estimates for compensation committee noncompliance when we use our own data set. The CC-noncompliance effect is concentrated in firms with blockholder directors (coefficient of 0.137) and with high institutional concentration (coefficient of 0.167). On the other hand, the coefficients for firms without blockholder directors and low institutional concentration are of very small magnitude (0.037 and 0.026) and not distinguishable from zero. These results imply that the compensation committee independence requirement has had the unintended consequence of increasing CEO pay, especially in firms with effective shareholder monitoring. The estimation results are available in Appendix 2.

[27] These empirical findings stand in sharp contrast to the prediction of the managerial power hypothesis that director independence effectively curbs rent extraction in the form of excessive CEO pay. Two plausible explanations of these findings are that (i) independent directors are less able or inclined to rein in CEO pay than incumbent nonindependent directors (e.g., due to smaller ownership stakes); and (ii) in response to the Sarbanes-Oxley Act of 2002, directors shifted their priorities from reining in CEO pay toward other tasks, especially in noncompliant firms.

or high institutional ownership concentration, both of which are considered to be monitoring substitutes. We draw similar conclusions based on median regressions and the change in pay for non-CEO executives. These results are based on CG's sample selection criteria, definitions, and methodology and may not be generalizable.

Taken together, there is little evidence that the board reforms have had any meaningful effect on the level of CEO pay. While it is tempting to reject the managerial power hypothesis, the evidence alternatively calls into question the effectiveness of director independence in corporate governance or the importance of reducing CEO pay. The possibility remains of intended and unintended consequences in other dimensions.

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Figure 1: Histogram of Changes in CEO Pay for Noncompliant Firms

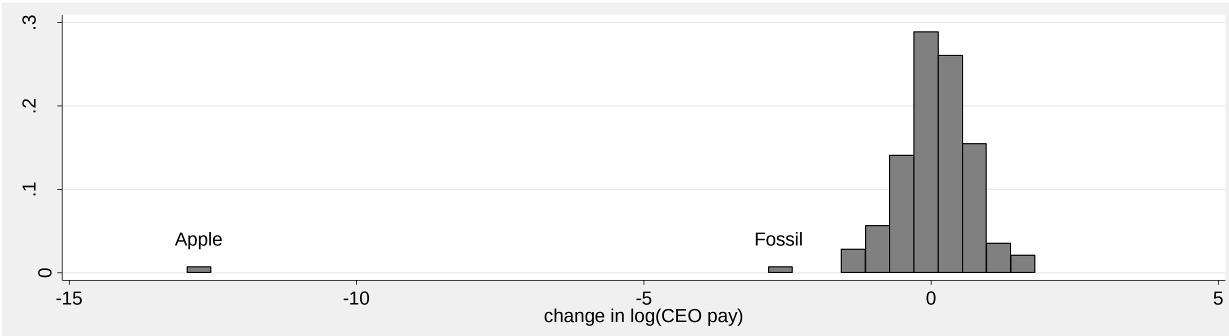

Table I: Board Independent Requirement

The results in this table are based on the data used and supplied by CG. The empirical model also follows CG: *ln(CEO pay) = a0 + a1*D(noncompliant board '02)*$_i$ ** D('03-'05)*$_t$ *+ [controls*$_{it}$*] + [FE*$_i$*] + [FE*$_{jt}$*] + e*$_{it}$, where CEO pay is total CEO compensation (variable *tdc1* in ExecuComp); *Noncompliant Board* is a binary variable that takes the value of one if the firm did not have a majority of independent directors on the board in 2002 and zero otherwise. A director is defined as an independent director if the director was not an employee of the firm during the previous 3 years, did not have any family affiliation of the officers of the firm, and did not have any material business transactions with the firm. *before* and *after* are period indicators, taking the value of one if the observation is in the pre-mandate (2000–2002) or post-mandate period (2003–2005), and zero otherwise. Controls include: *Sales* is the natural log of company sales (Compustat data item 12); *ROA* is the natural log of one plus net income before extraordinary items (data item 18) scaled by the book value of assets (data item 6) – all measured in (t−1); *RET* is the natural log of one plus the annual stock return (with dividends reinvested), measured in year (t−1); *Tenure* is the natural log of one plus the number of years the CEO served in the firm; *Adj_Tenure* is CEO tenure, with hand-collected corrections and additions to replace missing *Tenure* observations. *Sales*, *ROA*, and *RET* are interacted with the period indicators *before* and *after*. We include firm fixed-effects (*FE*$_i$) and industry-year dummies (*FE*$_{jt}$) in the regressions. *Industry-year dummies* are based on the Fama/French 48-industry classification (Fama and French (1997)) interacted with year dummies. All nominal variables are adjusted for inflation using 2002 as the base year. The numbers in parentheses are heteroskedasticity-robust standard errors, clustered at the firm-period level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Published and Replicated Results				Effect of Outliers		
	(1) Published Results	(2) Replicated Results	(3) Replicated Results w/o tenure	(4) Replicated Results w/ tenure adj.	(5) Excluding Apple & Fossil	(6) Median Regression	(7) Non-CEO Top-Executives
Noncompliance × after	-0.192** (0.086)	-0.193** (0.094)	-0.173** (0.086)	-0.179** (0.087)	-0.047 (0.039)	-0.045 (0.042)	-0.031 (0.028)
Sales × before	0.305*** (0.066)	0.306*** (0.066)	0.319*** (0.063)	0.326*** (0.063)	0.379*** (0.048)	0.356*** (0.083)	0.292*** (0.035)
Sales × after	0.268*** (0.072)	0.268*** (0.073)	0.280*** (0.069)	0.287*** (0.069)	0.355*** (0.048)	0.416*** (0.048)	0.272*** (0.035)
ROA × before	0.321 (0.399)	0.322 (0.399)	0.290 (0.389)	0.311 (0.390)	0.164 (0.375)	0.620* (0.335)	0.209 (0.182)
ROA × after	0.260* (0.150)	0.260* (0.150)	0.268* (0.149)	0.278* (0.152)	0.209* (0.120)	0.172 (0.155)	0.016 (0.088)
RET × before	0.123*** (0.033)	0.124*** (0.034)	0.117*** (0.033)	0.117*** (0.034)	0.118*** (0.033)	0.183*** (0.029)	0.094*** (0.026)
RET × after	0.269*** (0.048)	0.270*** (0.049)	0.273*** (0.047)	0.276*** (0.047)	0.302*** (0.042)	0.228*** (0.030)	0.189*** (0.029)
Tenure	-0.034 (0.022)	-0.034 (0.023)					
Tenure (adj.)				-0.046** (0.022)	-0.034* (0.020)	0.024 (0.020)	
# firm-years	5,190	4,956	5,190	5,190	5,178	5,190	22,736
# firms	865	841	865	865	863	865	865
Adj. R^2	0.260	0.103	0.104	0.105	0.124		0.062

Table II: CEO Pay at Fossil and Apple in 2000-2005

Panel A: Compensation for Kosta Kartsotis at Fossil

Year	Salary	Bonus	Restricted Stocks	Options Grant (Black-Scholes value)	All Other Compensation[a]	Total Pay
2000	$255,000	$0	$0	$0	$35	$255,035
2001	$255,000	$0	$0	$0	$21	$255,021
2002	$255,000	$0	$0	$0	$17	$255,017
2003	$255,000	$0	$0	$0	$324	$255,324
2004	$255,000	$0	$0	$0	$220	$255,220
2005	$0	$0	$0	$0	$180	$180

Panel B: Compensation for Steve Jobs at Apple

Year	Salary	Bonus	Restricted Stocks	Options Grant (Black-Scholes value)	All Other Compensation	Total Pay
2000	$1	$0	$0	$600,347,400	$0	$600,347,351
2001	$1	$43,511,534[b]	$0	$0	$40,484,594[b]	$83,996,129
2002	$1	$2,268,698[b]	$0	$89,444,690	$1,302,795[b]	$93,016,179
2003	$1	$0	$74,750,000	$0	$0	$74,750,001
2004	$1	$0	$0	$0	$0	$1
2005	$1	$0	$0	$0	$0	$1

[a] All other compensation refers to the premiums paid by the company on term life insurance policies.

[b] In December 1999, Jobs was awarded a special executive bonus in the form of an aircraft for serving as the company's interim CEO between September 1997 and December 1999. The total cost of the aircraft was about $90 million. The entire cost of the aircraft was initially reported as a bonus to Jobs in 2000. Later, however, the bonus was reclassified into four different income components to Jobs in 2001 and 2002, because the aircraft was not physically transferred to him until 2001. The purchase price of the aircraft involved two payments: approximately $40.5 million and approximately $2.7 million to be made in 2001 and 2002, respectively. These payments were reported as bonuses to Jobs in 2001 and 2002. The company also made two corresponding payments to settle related tax obligations, reported as all other compensation of $40.5 million in 2001 and $1.3 million in 2002.

Table III: Shareholder Monitoring

To the extent that the presence of monitoring substitutes mutes the effect of board independence, one would expect the decrease in pay to be concentrated in noncomplying firms without monitors. The empirical model of Table I has been modified to allow the effect of noncompliance to differ between the presence and absence of substitute monitors. *Blockholder* is a binary variable and takes the value of one if a firm has any non-employee directors who own more than 5% of the company's shares and zero if otherwise. *High concentration* is a binary variable and takes the value of one if a firm's institutional ownership concentration falls into the top quartile. The other concentration variables – *upper middle*, *lower middle*, and *low concentration* – are also binary variables indicating the lower three quartiles of institutional concentration. Note that low concentration encompasses the bottom quartile in columns (1)–(3), and the bottom three quartiles in columns (4)–(7). *Concentration of institutional ownership* is the proportion of institutional investor ownership accounted for by the five largest institutional investors in the firm. See section IV.B for more details. All other variables are defined in Table I. The numbers in parentheses are robust standard errors, clustered at the firm-period level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

Panel A: Blockholder Directors

	Published and Replicated Results			Effect of Outliers		
	(1) Published Results	(2) Replicated Results	(3) Replicated Results w/ tenure adj.	(4) Excluding Apple & Fossil	(6) Median Regression	(5) Non-CEO Top-Executives
Noncompliance × after						
× blockholder	0.054	0.053	0.075	0.111*	0.044	-0.006
	(0.106)	(0.073)	(0.067)	(0.063)	(0.064)	(0.046)
× no blockholder	-0.270***	-0.273**	-0.262**	-0.100**	-0.074	-0.039
	(0.063)	(0.114)	(0.106)	(0.044)	(0.048)	(0.033)
Sales	0.333***	0.314***	0.334***	0.384***	0.357***	0.293***
× before	(0.055)	(0.066)	(0.062)	(0.048)	(0.085)	(0.036)
Sales	0.298***	0.278***	0.296***	0.361***	0.425***	0.273***
× after	(0.056)	(0.072)	(0.067)	(0.048)	(0.048)	(0.035)
ROA	0.285	0.339	0.331	0.178	0.614	0.211
× before	(0.256)	(0.399)	(0.390)	(0.375)	(0.334)	(0.182)
ROA	0.249	0.253*	0.271*	0.204*	0.170	0.015
× after	(0.161)	(0.149)	(0.150)	(0.119)	(0.161)	(0.088)
RET	0.122***	0.123***	0.116***	0.117***	0.181***	0.093***
× before	(0.037)	(0.034)	(0.034)	(0.033)	(0.029)	(0.026)
RET	0.265***	0.273***	0.279***	0.304***	0.229***	0.190***
× after	(0.051)	(0.048)	(0.046)	(0.042)	(0.030)	(0.029)
Tenure	-0.034	-0.036				
	(0.022)	(0.023)				
Tenure (adj.)			-0.048**	-0.035*	0.021	
			(0.022)	(0.020)	(0.020)	
# firm-years	5,190	4,956	5,190	5,178	5,190	22,736
# firms	865	841	865	863	865	865
Adj. R^2	0.280	0.105	0.107	0.125		0.062

Panel B: Institutional Ownership Concentration

	Published and Replicated Results				Effect of Outliers		
	(1) Published Results	(2) Top vs. Bottom Quartiles	(3) All Quartiles	(4) Top vs. Bottom 3 Quartiles	(5) Excl. Apple & Fossil	(6) Median Regression	(7) Non-CEO Top-Executives
Noncompliance × after							
× high conc	-0.176	-0.062	-0.092	-0.096	-0.083	-0.046	-0.046
	(0.112)	(0.058)	(0.059)	(0.059)	(0.056)	(0.061)	(0.045)
× upp-mid conc			-0.804**				
			(0.350)				
× low-mid conc			0.133*				
			(0.073)				
× low conc	-0.238**	-0.038	-0.072	-0.224*	-0.027	-0.042	-0.023
	(0.107)	(0.075)	(0.077)	(0.124)	(0.047)	(0.049)	(0.034)
Sales × before	0.304***	0.312***	0.310***	0.327***	0.379***	0.356***	0.292***
	(0.065)	(0.067)	(0.065)	(0.062)	(0.048)	(0.082)	(0.035)
Sales × after	0.266***	0.278***	0.270***	0.289***	0.354***	0.416***	0.272***
	(0.071)	(0.071)	(0.071)	(0.068)	(0.048)	(0.048)	(0.035)
ROA × before	0.326	0.327	0.325	0.309	0.164	0.620*	0.210
	(0.398)	(0.396)	(0.381)	(0.389)	(0.376)	(0.338)	(0.182)
ROA × after	0.258	0.268*	0.279*	0.280*	0.208*	0.172	0.016
	(0.150)	(0.149)	(0.153)	(0.153)	(0.120)	(0.157)	(0.088)
RET × before	0.123***	0.117***	0.114***	0.116***	0.118***	0.183***	0.094***
	(0.036)	(0.034)	(0.033)	(0.033)	(0.033)	(0.029)	(0.026)
RET × after	0.270***	0.276***	0.274***	0.276***	0.303***	0.227***	0.189***
	(0.048)	(0.047)	(0.047)	(0.047)	(0.042)	(0.030)	(0.029)
Tenure	-0.033						
	(0.003)						
Tenure (adj.)		-0.043**	-0.038*	-0.045**	-0.034*	0.024	
		(0.021)	(0.021)	(0.022)	(0.020)	(0.020)	
# firm-years	5,190	5,190	5,190	5,190	5,178	5,190	22,736
# firms	865	865	865	865	863	865	865
Adj. R^2	0.280	0.103	0.115	0.106	0.124		0.062

Table IV: Independent Compensation Committee Requirement

This table repeats the regressions displayed in Tables I and III, except that we determine firms' noncompliance status from their compensation committee independence. *Noncompliant* now takes the value of one if the firm did not have a fully independent compensation committee in 2002 and zero otherwise. *Blockholder* and *High institutional concentration* are dummies indicating the presence of substitute monitors prior to the independence mandate (see Table III for more details). *Low institutional concentration* encompasses firms in the bottom three quartiles. All other variables are defined in Table I. The numbers in parentheses are robust standard errors, clustered at the firm-period level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Published and Replicated Results		Effect of Outliers		Block Ownership		Concentration of Institutional Ownership	
	(1) Published Results	(2) Replicated Results w/ tenure adj.	(3) Exclude Apple & Fossil	(4) Non-CEO Execs	(5) Exclude Apple & Fossil	(6) Non-CEO Execs	(7) Exclude Apple & Fossil	(8) Non-CEO Execs
Noncompliance	-0.014	-0.000	0.069**	0.046**				
× after	(0.064)	(0.061)	(0.031)	(0.023)				
× blockholder					0.136***	0.080**		
					(0.047)	(0.039)		
× no block					0.049	0.036		
					(0.035)	(0.026)		
× high inst conc							0.073	0.037
							(0.047)	(0.037)
× low inst conc							0.067*	0.049*
							(0.037)	(0.026)
Sales	0.290***	0.310***	0.370***	0.286***	0.372***	0.287***	0.370***	0.287***
× before	(0.068)	(0.064)	(0.048)	(0.035)	(0.048)	(0.035)	(0.048)	(0.035)
Sales	0.259***	0.277***	0.350***	0.269***	0.352***	0.270***	0.350***	0.269***
× after	(0.073)	(0.070)	(0.048)	(0.035)	(0.048)	(0.035)	(0.048)	(0.035)
ROA	0.346	0.331	0.183	0.223	0.178	0.221	0.182	0.224
× before	(0.404)	(0.394)	(0.377)	(0.183)	(0.377)	(0.183)	(0.377)	(0.183)
ROA	0.248*	0.267*	0.199*	0.011	0.198*	0.010	0.199*	0.011
× after	(0.148)	(0.150)	(0.119)	(0.087)	(0.118)	(0.087)	(0.119)	(0.087)
RET	0.124***	0.117***	0.119***	0.094***	0.118***	0.094***	0.119***	0.094***
× before	(0.034)	(0.034)	(0.033)	(0.026)	(0.033)	(0.026)	(0.033)	(0.026)
RET	0.269***	0.276***	0.303***	0.190***	0.303***	0.190***	0.303***	0.190***
× after	(0.048)	(0.047)	(0.042)	(0.029)	(0.042)	(0.029)	(0.042)	(0.029)
Tenure	-0.029							
	(0.024)							
Tenure (adj.)		-0.042**	-0.032		-0.032		-0.032	
		(0.021)	(0.020)		(0.020)		(0.020)	
# firm-years	5180	5190	5178	22,736	5178	22,736	5178	22,736
# firms	865	865	863	865	863	865	863	865
Adj. R^2	0.260	0.103	0.124	0.062	0.125	0.062	0.124	0.062

Appendix 1: CEOs' First Year in Office for Observations with Missing/Incorrect Data on CEO Tenure

GVKEY	Company Name	CEO Name	Year Became CEO
8431	AMERICAN FINANCIAL GROUP INC	Carl Henry Lindner	1959
17197	AMERICREDIT CORP	Clifton H. Morris, Jr.	1988
1633	ANALOGIC CORP	Bernard M. Gordon	1995
1633	ANALOGIC CORP	John W. Wood, Jr.	2003
2184	BEST BUY CO INC	Richard M. Schulze	1983
12123	BIG LOTS INC	Steven S. Fishman	2005
22794	BJ SERVICES CO	J. W. Stewart	1990
65105	BJ'S WHOLESALE CLUB INC	John J. Nugent	1997
13839	CAMBREX CORP	James A. Mack	1995
2803	CASCADE NATURAL GAS CORP	W. Brian Matsuyama	1995
28320	CDW CORP	Michael P. Krasny	1984
61404	CENTRAL PARKING CORP	Monroe J. Carell, Jr.	1980
12756	COCA-COLA ENTERPRISES INC	Lowry F. Kline	2001
16784	COMMERCE BANCORP INC/NJ	Vernon W. Hill, II	1982
3246	COMMERCIAL METALS	Stanley A. Rabin	1979
2849	COMPASS BANCSHARES INC	D. Paul Jones Jr.	1991
2577	CTS CORP	Joseph P. Walker	1988
3786	DATASCOPE CORP	Lawrence Saper	1964
14489	DELL INC	Michael S. Dell	1984
3964	DILLARDS INC	William Dillard II	1998
4060	DOW CHEMICAL	William S. Stavropoulos	1995
4065	DOWNEY FINANCIAL CORP	Daniel D. Rosenthal	1998
65671	DRIL-QUIP INC	Gary D. Smith	1981
25495	DYNEGY INC	Charles L. Watson	1985
4230	EDWARDS (A G) INC	Benjamin F. Edwards III	1983
12796	FIRST AMERICAN CORP/CA	Parker S. Kennedy	1993
25157	FIRST DATA CORP	Henry C. Duques	1989
25807	GTECH HOLDINGS CORP	W. Bruce Turner	2000
12788	HARMAN INTERNATIONAL INDS	Bernard A. Girod	1998
30146	INVESTMENT TECHNOLOGY GP INC	Raymond L. Killian Jr.	1994
6207	JLG INDUSTRIES INC	L. David Black	1991
6379	KELLY SERVICES INC	Terence E. Adderley	1987
14954	LINDSAY CORP	Gary D. Parker	1984
20075	MAF BANCORP INC	Allen H. Koranda	1989
13561	MBIA INC	Joseph W. Brown, Jr.	1999
6865	MDC HOLDINGS INC	Larry A. Mizel	1988
7506	MOLEX INC	Frederick A. Krehbiel	1988
31607	NATIONAL INSTRUMENTS CORP	James J. Truchard	1976

7798	NBTY INC	Scott Rudolph	1994
15202	NORTH FORK BANCORPORATION	John Adam Kanas	1976
2290	OFFICEMAX INC	George J. Harad	1994
28180	O'REILLY AUTOMOTIVE INC	Greg Henslee	2005
8293	PAXAR CORP	Arthur Hershaft	1980
8402	PAYCHEX INC	B. Thomas Golisano	1971
61325	PEDIATRIX MEDICAL GROUP INC	Roger J. Medel	1979
13200	PHOTRONICS INC	Constantine S. Macricostas	1974
12945	PLEXUS CORP	Peter Strandwitz	1979
8898	RAYMOND JAMES FINANCIAL CORP	Thomas A. James	1983
10121	SLM CORP	Albert L. Lord	1997
5087	SPX CORP	John B. Blystone	1995
25434	STARBUCKS CORP	Howard D. Schultz	1985
10124	STURM RUGER & CO INC	William B. Ruger	1949
17233	SUSQUEHANNA BANCSHARES INC	Robert S. Bolinger	1982
13041	SYNOVUS FINANCIAL CORP	James H. Blanchard	1971
10374	TECHNITROL INC	James M. Papada III	1999
10420	TELLABS INC	Michael J. Birck	1975
10498	TEXAS INDUSTRIES INC	Robert D. Rogers	1970
12395	TOLL BROTHERS INC	Robert I. Toll	1967
17248	UNITED BANKSHARES INC/WV	Richard M. Adams	1984
63927	UNITED NATURAL FOODS INC	Michael S. Funk	1999
23088	VITAL SIGNS INC	Terry D. Wall	1972
11313	WATSCO INC	Albert H. Nahmad	1973
14253	WESTAMERICA BANCORPORATION	David L. Payne	1989
13597	ZENITH NATIONAL INSURANCE CP	Stanley R. Zax	1978
11687	ZIONS BANCORPORATION	Harris H. Simmons	1990

Appendix 2: Replication of the Results from Tables I, III, and IV Using Our Data

The results in this table are based on our own sample. In constructing this sample, we follow all of CG's data requirements (6 years of director data from IRRC, 6 years of CEO pay data from ExecuComp, but allowing for missing observations on tenure) and definitions. Our final sample contains 909 firms (including Apple and Fossil). All variables are as defined in Tables I, III, and IV. Columns (1)–(4) are based on firms' compliance status with the board majority independence requirement in 2002; and columns (5)–(7) are based on compensation committee independence. We differentiate the effect of noncompliance (columns (1), (2), and (5)) by the presence of substitute monitors in columns (3), (4), (6), and (7). The numbers in parentheses are heteroskedasticity-robust standard errors, clustered at the firm-period level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Boards				Compensation Committees		
	(1) Baseline All firms	(2) Baseline Excl. A&F	(3) Block Dir Excl. A&F	(4) Inst Conc Excl. A&F	(5) Baseline Excl. A&F	(6) Block Dir Excl. A&F	(7) Inst Conc Excl. A&F
Noncompliance	-0.098	0.037			0.060*		
× after	(0.091)	(0.043)			(0.034)		
× blockholder			0.160***			0.137**	
			(0.062)			(0.057)	
× no blockholder			-0.005			0.037	
			(0.051)			(0.038)	
× high inst conc				0.004			0.167***
				(0.060)			(0.056)
× low inst conc				0.047			0.026
				(0.051)			(0.038)
Sales	0.320***	0.354***	0.357***	0.353***	0.354***	0.355***	0.356***
× before	(0.059)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)
Sales	0.293***	0.342***	0.346***	0.341***	0.342***	0.344***	0.346***
× after	(0.063)	(0.051)	(0.051)	(0.051)	(0.050)	(0.051)	(0.051)
ROA	0.464	0.374	0.383	0.375	0.378	0.379	0.367
× before	(0.334)	(0.336)	(0.335)	(0.336)	(0.334)	(0.335)	(0.333)
ROA	0.202	0.124	0.122	0.124	0.120	0.119	0.121
× after	(0.135)	(0.106)	(0.105)	(0.106)	(0.105)	(0.105)	(0.105)
RET	0.084***	0.087***	0.086***	0.087***	0.087***	0.087***	0.087***
× before	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
RET	0.285***	0.318***	0.320***	0.319***	0.319***	0.319***	0.319***
× after	(0.049)	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)
Tenure	-0.016	-0.007	-0.008	-0.007	-0.007	-0.007	-0.007
	(0.018)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
# firm-years	5,318	5,306	5,306	5,306	5,306	5,306	5,306
# firms	909	907	907	907	907	907	907
Adj. R^2	0.096	0.121	0.121	0.121	0.121	0.121	0.121