CEO Compensation and Board Structure – Rejoinder

KATHERINE GUTHRIE, JAN SOKOLOWSKY, and KAM-MING WAN[*]

Following the new board independence requirements introduced by the major U.S. stock exchanges in 2003, Chhaochharia and Grinstein (2009, henceforth CG) find a 17% decrease in CEO compensation in previously noncompliant firms relative to compliant firms. Moreover, they show that the decrease in pay is particularly pronounced among firms without outsider blockholder directors and firms with low institutional investor ownership concentration.

In Guthrie, Sokolowsky, and Wan (2012, henceforth GSW), we show that 74% of the magnitude documented by CG is attributable to two outliers out of 865 sample firms using CG's data and methodology. We argue that the outliers do not lend support to CG's conclusion of board independence having a causal effect on the level of CEO pay. Our results also hold for compensation of non-CEO executives. Furthermore, removal of the outliers uncovers a significant increase in CEO pay in firms with noncompliant compensation committees relative to those with compliant compensation committees, particularly in the presence of outsider blockholder directors and high institutional ownership concentration.

In their reply to our critique ("CG-R"), Chhaochharia and Grinstein (2012) suggest that (i) Apple is a prime example of how board regulations affect CEO pay and should therefore not be excluded from the study, and (ii) their original results are robust to excluding the outliers when extending the pre-event sample period from 2000 to 2002 back to 1996. In this rejoinder, we (i) dispute that Apple is a fitting example to illustrate the causal effect of board independence on CEO pay, (ii) caution against drawing conclusions about the robustness of the results from the new regression results in CG-R (e.g., due to lack of relevance, sample selection issues, and more outlier effects), and (iii) argue that important omissions in CG-R cast further doubt on the conclusions advocated by CG. While not the focus of our debate, it is worth pointing out that contrary to CG's original findings, committees do in fact appear to play an important role in the CEO compensation process. In a nutshell, the existing evidence simply does not support the view that mandated board independence helps rein in executive compensation.

The remainder of this discussion is organized as follows. In Section I, we address Apple. In Section II, we discuss the drawbacks to extending the sample, sample selection issues, and the problems in identifying noncompliant firms. In Section III, we revisit the results on boards vs. committees. We conclude in Section IV.

I. Apple Does Not Fit the Story

A. Apple Did Not Need to Replace or Add any Directors to Become Compliant

CG-R spend the majority of their reply arguing that Apple's board was captured by Steve Jobs prior to the independence requirement. We completely agree with this assessment (ironically, Jobs closest allies on the board – Campbell and Ellison – were considered independent directors). What CG-R fail to mention is what happened to Apple's board after the rule change. Despite being noncompliant in 2002, Apple did not need to replace or add any directors to become compliant by 2003. From 2003 onward, continuing director Millard Drexler was deemed an independent director, as his and Jobs' interlocking relationship terminated.[1]

---

[1] The interlocking relationship dissolved in 2002 because Millard Drexler resigned as CEO from Gap Inc. in late September 2002, and a few days later Jobs quit his directorship at Gap.

B. Apple's Press Release Does Not Constitute Credible Evidence

Interestingly*,* CG-R describe a press release issued by Apple on March 20, 2003 (titled "Apple Enhances Corporate Governance") as "perhaps the most direct evidence that Jobs's change in compensation was a governance measure." We disagree. Put simply, this disclosure comes from the same company and board members that tolerated nondisclosure of option backdating – it could be cheap talk and therefore cannot be relied upon as evidence.

First, according to the press release, Apple planned to add two independent directors to the board. While two new members joined the board, one of them was clearly not independent. Fred Anderson joined the board in 2004 only days after retiring as CFO of Apple, but had to resign in 2006 for backdating option grants at Apple in 2001.[2] Thus, the formal independence of the nominating committee did not lead to the selection of truly independent directors at Apple (which, by extension, casts doubt on CG-R's argument that new committee procedures and greater scrutiny of directors by the stock exchanges had much impact at Apple).

Second, the press release explains that Jobs voluntarily exchanged his 27.5 million underwater options for 5 million shares. CG-R argue that Jobs' loss of about $87 million in the option-for-stock swap is indicative of increased board power. We contend that CG-R's estimate is highly sensitive to their choice of 70% stock volatility to value Jobs' option holdings as of March 20, 2003. According to Ivy DB OptionMetrics, the average implied volatility of outstanding Apple options was about 42% during the month of March, with the implication that Jobs' options and stocks would have had equal value.[3]

C. Confounding Events That Could Explain the Change in Jobs' Pay

Even if we could attribute the changes in Jobs' compensation structure at Apple to the change in Apple's governance practices, it would be impossible to differentiate the impact of the new independence requirements from concurrent events. For example, in anticipation of the implementation of accounting standard FAS 123R, many companies began to voluntarily expense options in their financial statements as early as 2002, while others announced that they would abandon options altogether.[4] Further, it is possible that the press release was (at least in part) a response to negative media exposure. Apple had been heavily criticized for its poor corporate governance practices. For example, it was named to *Business Week*'s list of worst boards published in October 2002 and was harshly criticized by Arthur Levitt, former chairman of the Securities Exchange Commission, in his book "*Take on the Street*" released in October 2002.[5] Joe, Louis, and Robinson (2009) further show that firms that appear on *Business Week*'s list of ineffective boards increase the representation of outsiders on the board compared to a matched control group. Therefore, we cannot distinguish the effect of the board independence requirement from the effects of contemporaneous events on the structure of Jobs' pay.

---

[2] The other addition was Al Gore, who was the beneficiary of a $50,000 soft-money campaign contribution to the Democratic National Committee by Jobs in 2000.

[3] We suspect that CG-R's high volatility estimate subsumes the period around the 52% drop in Apple's stock price on September 29, 2000, when Jobs' $600+ million option grant turned worthless.

[4] According to a report by Bear Stearns dated February 28, 2003, 175 firms had announced to voluntarily expense options since June 2002 (http://www.citizenworks.org/corp/options/bearstearns.pdf). Firms that committed to voluntarily expense options represented 29.4% of the market capitalization of the S&P 500 index.

[5] The *Business Week* list is available at http://www.businessweek.com/magazine/content/02_40/b3802004.htm.

D. Was Missing the Statistical Outliers an Accidental Oversight?

In CG-R, Chhaochharia and Grinstein conclude with

"… if one were to pick a firm that was significantly affected by the governance regulations, one should probably choose Apple. GSW's claim that Apple should not be in the sample because the large shifts in compensation to the CEO of Apple were because his options were simply underwater is therefore incorrect."

Regardless of whether Apple fits the story advocated by CG or whether there were large shifts in compensation, there is no doubt that Apple is an influential outlier in the statistical sense. Dropping Apple from the full sample reduces the point estimate of the effect of board independence on CEO pay by 62%. For this reason alone, Apple should not have been included in the original sample when the goal was to assess whether board independence affects CEO pay for a broad sample of large, publicly traded firms in the U.S.

If Apple were a leading example to illustrate the impact of board independence reform on CEO pay as argued in CG-R, we find it curious that this point was not made in the published article. Perhaps the authors simply overlooked the outlier. Here is how Chhaochharia and Grinstein explain their oversight of the influential outliers in footnote 5 of their reply:

"CG also examined extreme changes in compensation in noncomplying firms. However, their focus was on changes in compensation between 2002 (the year before the rule) and 2003 (the first year after the rule). The outliers identified by GSW did not have a large change in compensation between these two years and therefore they were not detected by CG."

A closer look at the data casts doubt on the accuracy of this statement. CG write in their published article that "To get a better sense of whether the large drops in compensation were due to other factors, we read the proxy statements between 2003 and 2005 for some of the noncomplying firms that had the largest drop in compensation." CG go on to single out Adobe and Compuware. Table I lists the 20 noncompliant firms with the largest decrease in ln(*pay*) based on the change from 2002 to 2003 as well as on the change in the averages over the 2000 to 2002 and 2003 to 2005 periods.

Adobe and Compuware rank 3rd and 22nd when ranked by the change in pay from 2002 to 2003, and 4th and 5th when ranked by the change in pay from the 2000 to 2002 period to the 2003 to 2005 period. We do not think that ranking 22nd out of 142 qualifies Compuware as belonging to the noncompliant firms with the largest drop in CEO pay.[6,7]

---

[6] Using the change in CEO pay from 2002 to 2003 to detect potential outliers does not adequately reflect CG's research design. CG's methodology infers the effect of noncompliance from the average change in CEO pay from the pre- to post-independence mandate period, not from an abrupt change between 2002 and 2003. Apple ranks only 47th out of 142 based on the change in pay from 2002 to 2003. The 62% decrease in the estimate of the noncompliance effect just by excluding Apple is not reconcilable with using the change in pay from 2002 to 2003 to detect outliers.

[7] Note that the residual analysis offered in CG-R is not intended to identify outliers with a large impact on the coefficient estimates, but rather firms whose variation in CEO pay is not well explained by the empirical model.

Table I
List of Noncompliant Firms with the Largest Change in ln(*CEO pay*)

Rank	Change from 2002 to 2003	Δln(*pay*)	Change from 2000-2002 to 2003-2005	Δln(*pay*)
1	Abercrombie & Fitch	-2.739	**Apple Computer**	**-12.952**
2	Swift Transportation	-2.553	**Fossil**	**-2.487**
3	**Adobe Systems**	**-2.015**	General Electric	-1.374
4	General Communication	-1.831	**Adobe Systems**	**-1.344**
5	Transaction System Architects	-1.806	**Compuware**	**-1.283**
6	Dillard's	-1.788	Synopsys	-1.196
7	Claire's Stores	-1.525	Protein Design Labs	-1.104
8	Acxiom	-1.335	Millennium Pharmaceuticals	-0.904
9	Insight Enterprises	-1.303	Universal Health Services	-0.828
10	AmeriCredit	-1.286	Abercrombie & Fitch	-0.770
…	…	…		
22	**Compuware**	**-0.703**		
47	**Apple Computer**	**-0.235**		
69	**Fossil**	**-0.015**		

We conclude that Apple is not the "prime example of the effect of the new exchange rules on noncomplying firms" that CG-R suggest it is. Rather, it is an outlier that should be (and should have been) excluded from the study. It does, however, make for an illuminating case study for why corporate governance regulations end up being ineffective.

II. Addressing the Further Evidence in CG-R

A. Drawbacks to Extending the Sample

For the original paper, CG chose a priori sensible sample selection criteria and empirical specifications. We believe that sticking with the previous empirical implementation is essential to avoid introducing hindsight and sample selection biases. Any changes to the implementation need to be particularly well justified. We are not convinced that using additional observations from four or more years prior to the new regulations improves the quality of the inferences. While adding past observations may yield a more precise estimate of the average value of volatile option grants in the pre-event period, those observations are not necessary to address our main critique – that the results are driven by outliers – and come at the cost of being less relevant to the relationship between CEOs and boards around the new exchange requirements. First, over 60% of the sample firms employ a different CEO in 2003 than in 1996. Second, as board structure is endogenously determined, we expect board independence to vary over time. Consequently, extending the sample period makes it more difficult to ascribe compensation practices in the pre-event period to board compliance status in 2002.

After removing the outliers, neither CG-R nor we find a significant decrease in pay from the 2000 to 2002 pre-event average in firms with noncompliant boards relative to firms with compliant boards. Yet after expanding the sample back to 1996, CG-R find a marginally significant differential change in CEO pay. Taken together, these findings suggest that the relative decrease in CEO pay among noncompliant firms shown in CG-R is attributable to relatively higher pay in the 1996 to 1999 period. This observation is difficult to reconcile with

CG's interpretation that the independence mandate improved the bargaining position of noncompliant boards.

B. Sample Selection Issues

In our experience, the estimates have proven to be highly sensitive to sample selection. For example, as shown in GSW, using the data set that we construct following the selection criteria set forth by CG (which contains 909 firms), we obtain estimates that are quite different from those based on CG's original data. We also conduct numerous robustness tests, both on the original CG sample and on our own sample: various cutoffs for pre/post-rule changes (e.g., fiscal vs. calendar year, SOX compliance date), alternative years for identifying noncompliant boards (e.g., board independence trended upward before 2003), perturbations to sample restrictions (e.g., not insisting on IRRC data over the entire sample period), and variations in the definition of compliance (e.g., the board independence classification used in the original CG article potentially misclassifies 92 out of 142 firms as noncompliant).[8,9] None of these tests indicates that CEO pay decreased in noncompliant firms relative to compliant firms. At best, one should conclude that the results are (i) highly sensitive to reasonable variations in the empirical specification and (ii) specific to the firm-years included in the sample (i.e., they are not generalizable to the broader population).

CG-R themselves provide an illustration of just how sensitive their main result is to sample selection. In an attempt to show that the result is not driven by volatile option grants, CG-R restrict the noncompliance effect to firms that do not grant any options to their CEOs over the 2000 to 2002 period (e.g., Table V in CG-R). Note that their table shows the effect of noncompliance *only for firms that did not grant options*, that is, it offers no insights about the noncompliance effect *in option-granting firms* (CG-R's specification implicitly restricts the change in pay for compliant and noncompliant option-granting firms to be the same). A better specification would be to separately estimate the effect of noncompliance for the subsamples of firms that do and do not pay with options, or at least to include additional interaction terms for the option-granting firms. Table II presents our estimates using CG's original data.[10]

---

[8] Some of those tests are described in more detail and reproduced in a 2009 working paper version of the critique by Guthrie and Sokolowsky, available from SSRN at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1673007.

[9] The erratum posted by CG on the *Journal of Finance* website (http://www.afajof.org/afa/all/Chhaochharia-Grinstein-errata.pdf) is also erroneous. In point (a) CG acknowledge missing observations for tenure, and a smaller resulting sample size, but they fail to mention other instances in which their sample size deviates from their published values. For example, the regression for the change in bonus pay contains only 4,113 observations. Correction (b) does not reflect the actual definition of independence used by CG in the original sample. In reclassifying former employees, CG forget to honor the business relationship disqualification to independence. Specifically, CG consider former employees with business ties as independent, but non-employees with business ties as non-independent.

[10] Further inspection of the data reveals that the magnitude of the estimate among firms that did not grant options is once again highly influenced by just one firm. Excluding Oracle (a compliant firm) from the sample lowers the estimate of the noncompliance effect by 43%. The influence of Oracle is more pronounced in the restricted sample, because there are just 37 compliant firms that do not grant options, compared to 723 compliant firms in the full sample.

Table II
Examining the Noncompliance Effect on Option Payers vs. Nonpayers

The results in this table are based on the data used and supplied by CG. The empirical model also follows CG: ln(*CEO pay*) = $a0 + a1 \times$ D(*noncompliant board* '02)$_i \times$ D('03-'05)$_t$ + [*controls$_{it}$*] + [*FE$_i$*] + [*FE$_{jt}$*] + e_{it}. See CG and GSW for further details. 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. Column (1) is estimated over the subsample of firms that granted options to their CEOs between 2000 and 2002; columns (2) and (3) only include firms that did not grant options.

	Option Payers	Option Non-Payers	
	Excluding Apple (1)	Excluding Fossil (2)	Excluding Fossil & Oracle (3)
Noncompliance × after	-0.052 (0.042)	-0.354** (0.175)	-0.203 (0.142)
Sales × before	0.386*** (0.049)	-0.197 (0.241)	-0.045 (0.243)
Sales × after	0.360*** (0.048)	-0.113 (0.264)	-0.084 (0.249)
ROA × before	0.462 (0.372)	-5.512* (2.960)	-1.378 (1.239)
ROA × after	0.172 (0.115)	1.538 (1.861)	1.118 (1.676)
RET × before	0.119*** (0.034)	0.328* (0.191)	0.243 (0.177)
RET × after	0.305*** (0.045)	0.156 (0.166)	0.052 (0.159)
# firm-years	4,824	354	348
# firms	804	59	58
Adj. R^2	0.135	0.560	0.559

The first thing to note from Table II is that there are 805 option-granting firms (including Apple), but only 60 non-option-granting firms (including Fossil and Oracle). Second, the noncompliance effect is of economically relevant magnitude only for firms that did not grant options (coefficient of -0.203 vs. -0.052 for option-granting firms; neither estimate is statistically different from zero at conventional significance levels). Thus, the sample of firms from which CG-R deduce a significant effect of board independence on CEO pay constitutes a rather limited and nonrepresentative subset of large publicly traded companies in the U.S.

In light of the backdating and earnings manipulation scandals tied to stock options and lack of board oversight, it is surprising to find that the effect of board independence on CEO pay is particularly pronounced in firms that did not grant options. Moreover, noncompliant firms make up 38% of the non-option-granting subsample, whereas they represent only 15% of the option-granting firms. These observations are not easily reconciled with the managerial power hypothesis and the view that nonindependent boards were weak.

C. Problems in Identifying Which Firms Were Affected by the Independence Requirement

Even if one were to accept the premise that the small subsample of non-option-granting firms can deliver insights about the effect of board independence on CEO pay more broadly,

those estimates remain subject to other serious data problems. In particular, the number of firms classified as noncompliant is highly sensitive to how one identifies noncompliance with the new regulations. The IRRC definition of independence is stricter than the definitions implemented by the stock exchanges. First, IRRC always treats former employees as nonindependent directors, whereas the exchanges consider former employees independent provided that three or more years have passed since the end of their employment. Second, IRRC disqualifies directors with any business ties to the firm from being nonindependent, whereas the exchanges only do so for material business relationships.

Table III shows the fraction of boards with a majority of independent directors over time for various definitions, calculated from the data supplied to us by CG. Column (1) shows the evolution of board independence according to the definition used by IRRC. CG attempt to correct the first problem by reclassifying qualifying former employees as independent (subject to the caveat described in footnote 9), but cannot reclassify nonindependent directors with minor business relations as independent because the size of these business transactions is not captured in the IRRC database (column (2)). The implicit assumption by CG is that all business relations are material under the NYSE/NASDAQ standards. In column (3), CG assume the opposite – namely, that all business relations documented in IRRC are immaterial (in fact, in all the cases checked by an anonymous referee who read the proxy statements, the business relations turned out to be immaterial). Column (4) employs the independence classification by ISS (we do not have access to ISS data ourselves, but the variable was part of the data set shared by CG; it is available for 703 firms).

Table III
Fraction of Majority-Independent Boards over the Sample Period

Year	IRRC	CG	NYSE/ NASDAQ	ISS
	(1)	(2)	(3)	(4)
2000	0.753	0.794	0.923	
2001	0.780	0.780	0.927	0.828
2002	0.794	0.836	0.942	0.835
2003	0.847	0.897	0.973	0.912
2004	0.901	0.933	0.992	0.970
2005	0.914	0.936	0.993	0.984

Based on CG's definition of independence, $(1-0.936) \times 865 \approx 55$ firms out of 142 sample firms identified as noncompliant in 2002 were still noncompliant by the time the rules were binding in 2005. The gap is substantially smaller for the alternative independence definitions in columns (3) and (4).[11] The data suggest one of two things: either (i) CG employ a definition of independence that entails a high degree of misclassification (see footnote 1 in GSW for further details), or (ii) many firms found a way to comply with the letter, but not the intent, of the new regulations. This is an important issue, because the interpretation of causality advocated by CG relies on the exogeneity of the shock to board independence. Yet many of their noncompliant firms that in reality were compliant with the new rules were free to choose whether to increase the representation of independent directors or to become compliant without restructuring their

---

[11] There is substantial disagreement on which firms are deemed noncompliant between the CG and ISS definitions in 2002, despite similar compliance rates.

boards. Interestingly, we find no significant effect of noncompliance on CEO pay when we use either one of the alternative independence definitions available in CG's data set.[12]

In sum, to establish that there is an effect of board structure on CEO pay after all, one would have to first carefully address all the data issues and perhaps even consider improving on the methodology (e.g., along the lines of Guo and Masulis (2011), who also take into account self-selection of firms into being compliant or noncompliant prior to the new rules).

III. Influence of Board vs. Committee on Executive Pay

Though not the focus of CG's contribution or our debate, it is worth pointing out that removing the outliers yields different results regarding the relative importance of boards vs. committees in setting executive compensation. In columns (3) and (4) of Table II of CG's published article, the effect of the nomination committee is weak and statistically insignificant at the 10% level. According to Table II, Panel B of CG-R, nomination committee independence appears to have a moderately negative yet statistically significant effect on CEO pay.

The new estimates imply a pivotal change in CG's position on the influence of boards vs. committees on executive pay. One of CG's original conclusions was that board-level attributes are more important than committee-level attributes, which they attribute to boards being ultimately more powerful than committees, for example, due to their decision rights about who serves on which committee and board ratification of committee recommendations.

The conflicting findings in CG and CG-R have different implications about the internal working of the firm. On the one hand, if boards and committees evolve endogenously to improve decision-making efficiency within organizations, one would expect decisions about executive pay to be delegated to compensation committees and decisions about the selection of new directors to be delegated to the nominating committee (see Williamson (1985) and Alchian and Demsetz (1972)). On the other hand, if corporate boards are captured by powerful CEOs such that they determine the makeup of the boards as well as the committees, then one would expect key decisions to be made at the board level rather than the committee level (e.g., Bebchuk and Fried (2003)).

The results in CG and CG-R fail to convey a clear picture about the internal governance of the firm. In contrast, the empirical results we present in GSW (2012) are consistent with the view that boards and committees evolve endogenously.[13] From this perspective, the change in committees and boards due to the independence requirements upset the optimal structure achieved within firms, with the unintended consequence of leading to higher CEO pay, particularly in the presence of powerful monitoring substitutes (e.g., outsider blockholder directors).

---

[12] We do not further address the results in CG-R that exclude options or equity pay, because we do not see how excluding important components of compensation could possibly help shed light on the effect of board independence on the *level of CEO pay*. We do find the effects of the board independence mandates on *CEO incentives* interesting, but that question is addressed in contemporaneous work by Chung (2008).

[13] Our results are also congruent with findings in existing literature, for example, Hermalin and Weisbach (1988), Boone et al. (2007), Linck, Netter, and Yang (2008), and Lehn, Patro, and Zhao (2009).

IV. Conclusion

In GSW (2012) we raised objections that CG-R do not address. We repeat these objections here, as the new evidence undermines the conclusions advocated by CG. First, after removing the outliers, the effect of board independence is no longer concentrated among firms without monitoring substitutes, despite CG's assertion that this should be the case (these findings were even highlighted in the abstract of the published article). Second, board independence has no effect on pay of other top executives, despite the fact that the same directors who negotiate or approve CEO compensation are also responsible for the compensation of other top executives.

We conclude that the board independence mandate in 2003 did not lead to lower managerial pay as predicted by the managerial power and bargaining hypotheses advocated by Bebchuk and Fried (2003) and Hermalin and Weisbach (1998). However, the evidence is also consistent with the views that mandated board independence was ineffective (e.g., because powerful CEOs were able to circumvent the intent of the board independence requirement) or that restraining CEO pay was not a high priority item for newly independent boards.

REFERENCES

Alchian, Armen, and Harold Demsetz, 1972, Production, information costs, and economic organization, *American Economic Review* 62, 777–795.

Bebchuk, Lucian, and Jesse Fried, 2003, Executive compensation as an agency problem, *Journal of Economic Perspectives* 17, 71–92.

Boone, Audra, Laura Field, Jonathan Karpoff, and Charu Raheja, 2007, The determinants of corporate board size and composition: An empirical analysis, *Journal of Financial Economics* 85, 66–101.

Chhaochharia, Vidhi, and Yaniv Grinstein, 2009, CEO compensation and board structure, *Journal of Finance* 64, 231–261.

Chhaochharia, Vidhi, and Yaniv Grinstein, 2012, CEO compensation and board structure — There is an effect after all, *Journal of Finance,* forthcoming.

Chung, Hae Jin, 2008, Board independence and CEO incentives, Working paper, New York University.

Guo, Lixiong, and Ronald Masulis, 2011, Board structure and monitoring: New evidence from CEO turnover, Working paper, University of New South Wales.

Guthrie, Katherine, Jan Sokolowsky, and Kam-Ming Wan, 2012, CEO compensation and board structure revisited, *Journal of Finance* 67, 1149–1168.

Hermalin, Benjamin, and Michael Weisbach 1988, The determinants of board composition, *RAND Journal of Economics* 19, 589–606.

Hermalin, Benjamin, and Michael Weisbach 1998. Endogenously chosen boards of directors and their monitoring of the CEO, *American Economic Review* 88, 96–118.

Joe, Jennifer, Henock Louis, and Dahlia Robinson, 2009, Managers' and investors' responses to media exposure of board ineffectiveness, *Journal of Financial and Quantitative Analysis* 44, 579–605.

Lehn, Kenneth, Sukesh Patro, and Mengxin Zhao, 2009, Determinants of the size and structure of corporate boards: 1935–2000, *Financial Management* 38, 747–780.

Linck, James, Jeffry Netter, and Tina Yang, 2008, The determinants of board structure, *Journal of Financial Economics* 87, 308–328.

Williamson, Oliver, 1985, *The Economic Institutions of Capitalism: Firms, Markets, and Relational Contracting* (Macmillan, New York).