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Lucian Bebchuk and Jessie Fried. Pay without Performance: The Unfulfilled Promise of Executive Compensation. Cambridge: Harvard University Press, 2004, 278 pages, $24.95, hardcover.

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The authors, both law professors, note at the very outset of their book that the average pay of a CEO of a large, public corporation in 2003 was 500 times the pay of an average worker. That ought to be unconscionable to anyone with any notion of sensible and equitable pay and the practical limits on what any CEO can do single handedly to boost corporate performance (the authors cite one study showing only 30 percent of stock price changes reflect corporate performance). Having argued for a high-low income ratio of nine to one in his time, I wonder if Plato would be indignant today.

Although acknowledging “outrage costs” when shareholders and the general public learn of give-away compensation, the authors deliberately avoid taking any moral stand on the issue. Their perspective, instead, is theoretical and practical. It is theoretical because the authors espouse a theory they claim explains why executive performance is in their view insensitive to that part of compensation intended to drive performance, namely equity-based incentives, or stock ownership. It is practical because, if their theory is valid, a better understanding of executive pay setting could lead, in their opinion, to a re-coupling of executive pay and performance and a significant increase in shareholder wealth.

Their theory, the “managerial power model,” competes with at least five alternative models all seeking to explain why and how financial incentives are becoming a larger part of executives’ compensation. Theorists naturally defend their theories, so throughout the book the authors rebut rival theories. The debate does not stop at the end of the book, as the authors have since engaged in an “exchange” with a management professor who presents some compelling counterarguments (Bebchuk & Fried, 2006; Conyon, 2006).

The dominant rival is known as the “principal/agent model.” It underlies existing corporate law and governance arrangements and has, the authors note, guided most empirical research. Basically this model argues that compensation committees of boards of directors, who represent the principals (i.e., dispersed owners), negotiate “at arm’s length” with the agents (i.e., CEOs) to set executive pay. Supposedly by keeping their distance and objectivity and by recognizing their obligations to the principals, committees establish cost-effective contracts with agents that are intended to motivate agents to manage the corporation well and to increase the principals’ wealth.

It does not work that way at all according to the authors. What really happens, they argue, is that CEOs, through their powerful influence, set their own pay and increase their own wealth. As for shareholders, they are taken to the cleaners. Compensation committees have had, as I ad lib the authors’ model, “their arms broken by strong-armed robbers,” or have been overwhelmed by, as I can imagine author and political commentator Arianna Huffington saying (Huffington, 2003), the tough “pigs at the trough” (I’m trying to inject some humor into this review because the book is dreadfully dry and definitely not a page turner).

Well, maybe the authors are right and their rivals are not. But how do the authors know that compensation committees' arms are broken? They do not know which is precisely why they have a theory. They do not sit in on negotiations. They do not "cross examine" CEOs and committees. They simply believe that the presence of costly but useless incentives is evidence of the absence of arms-length contracting and that their model best explains why that is so.

In case you are curious about the four other rivals, and I think you should be if you are at all interested in the subject, and before I finish this review, here they are in a nutshell as I label and understand them. One is the market model that predicts changes in executive pay when there are changes in the supply of and demand for CEOs. Another is the better board model that predicts CEOs will justifiably demand more incentives when boards are making them work harder. A third is the turbulence model that predicts greater executive compensation when the corporation is buffeted by strong outside forces such as major technological changes in the industry. The fourth is the accounting model that predicts changes in executive pay based on how strict or lenient is the accounting for the equity component of pay.

Remember, now, that the authors are lawyers, so if you read this book, be ready to wade through a lot of analytical arguments. For instance, the authors consume one chapter just to offer plausible reasons to doubt that outside and supposedly independent directors on the board and its compensation committee (a criterion of the better board model) will engage in arm's-length bargaining. The authors' basic rebuttal is that a CEO can easily make outsiders dependent on the CEO in over a dozen ways, such as, to cite just a few, influencing nominations of directors who want to keep their seats, encouraging or discouraging increases in director pay, and lavishing perks and other rewards on the directors. The authors consume another chapter just to rebut the accounting model's explanation of the widespread failure of corporations to reduce "windfall" options, and yet another chapter to rebut the market model. And so on it goes.

The author's model naturally is given prime exposure throughout the book. Besides their rebuttals of the alternative models, the authors cite numerous findings from the research literature which they believe can only be fully explained or predicted by their model. For instance, once anti takeover defenses are deployed against hostile bidders, the subsequently less vulnerable CEOs extract significantly greater pay concessions. These CEOs, the authors argue, have used their influence to protect and enhance themselves even further.

Such defensive measures are just one example of the unscrupulous ways in which CEOs are presumed to abuse their power over compensation committees. Other examples the authors discuss include "camouflaging" compensation contracts to minimize outrage costs, obtaining "stealth wealth" that is never reported, increasing the value of options when stock prices fall, trading on inside information, securing a generous but sham consulting contract upon retiring, and arranging for a "soft landing" (i.e., with a "golden parachute") if dismissed (the last two examples seem to demonstrate CEO power even when it is going out the door). In the broadest sense if you ask me any element of executive pay without performance, especially excessive pay, is fraudulent and not unlike embezzlement (that may be the moralist coming out in me).

Although the book's main aim is to explain rather than solve the problem, some "partial remedies" are offered in the last two chapters for how to improve compensation packages and corporate governance. Legislative reform is not proposed because the authors contend that the judiciary is “ill equipped” to judge the matter and thus usually defers to the “business judgment rule.” But neither the judiciary nor that rule need be taken as sacrosanct. For lawyers who author a critical analysis not to go on to propose legal reforms as one means to curb abusive power is perplexing in light of arguments by some authoritative critics that legal reforms are absolutely necessary (see, e.g., Drutman & Cray, 2004).

The book ought to be titled “pay without performance management.” The authors are so narrowly focused on financial incentives being “insensitive” to performance that they fail to determine what should be the role of the board in setting performance expectations for CEOs and in appraising their performance against those expectations. The right kind of expectations and an honest appraisal ought to make oversized pay for undersized performance less likely or at least more conspicuous. Of course, there is every reason to believe that CEOs who operate according to the author’s theory will control the entire performance management cycle, not just the pay decisions.

The authors’ view of performance is also too narrow. To them, performance that does not increase shareholder wealth is unacceptable. But increased shareholder wealth is neither the sine qua non of corporate greatness nor even in some cases a legitimate corporate objective. How could, for instance, increased shareholder wealth created through corporate wrongdoing be considered legitimate?

The authors would welcome good compensation contracts. To me, however, any compensation contract is an inherently a bad management practice. It should be abolished and the role of compensation committees minimized. Contracts and committees accommodate and legitimize a CEO’s manipulative schemes and are no substitute for genuine performance management conducted by the whole board in “arms’-length collaboration” with the CEO.

Common to all of the theoretical models, including the authors,’ is an underlying premise of financial incentives that puzzles me. It is presumed that CEOs are “risk averse” and thus must be financially motivated by performance incentives to take risks to increase shareholder wealth. Yet financial incentives in the form of stock offerings are considered risky, particularly compared to base salary that is normally kept disconnected from firm performance, and thus CEOs are presumed to “prefer a smaller amount of risk-free cash to risk incentive compensation” (and apparently there is a move away from stock options to cash incentives). Go figure. Obviously, though, there is little or no risk if CEOs actually do set the parameters and are smart and devious enough in doing it!

An I/O psychological perspective on performance management at the top might be illuminating but this perspective is nowhere to be found in the book, and almost all of the nearly 40 pages of footnotes represent the fields of law and economics. Had the authors’ perspective been broader the prominent work by Ed Lawler and his colleagues probably would have been mentioned (e.g., Conger, et al., 2001; Lawler, 1990; 2000).

I am ambivalent about this book but have decided to endorse it anyway. I would like to think there are a few I/O psychologists who would be interested in examining all six theories, adding their own, and keeping company with Lawler et al! If you are one of them or if you should decide to read the book for whatever reason, consider reading also the exchange articles I mentioned. Conyon’s arguments and citation of evidence contrary to the managerial power model are hard to dismiss. Nevertheless, my intuition says this model makes the most sense among the rival theories and also seems consistent with all of the literature I have read about imperial CEOs and abusive corporate power (see, e.g., Korten, 2001).

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