Pay without Performance: A Market Equilibrium Critique

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Bebchuk and Fried's book will get a lot of attention, and frankly it should.[1] As Jesse Fried's remarks indicated, this area has received a lot of popular attention. I want to come back to talk about outrage perhaps a little bit later. At the same time I have to be honest with you that I am worried that research in corporate finance, and to some extent law and economics, may have too big an impact on the popular outrage, and in fact, may be feeding this populism.

At one level, as an economist just looking at this problem and not being a specialist in securities law, my first question would be: Why should I care? If markets work very well (which of course isn't the thesis of the book, which I will address in a moment), why should we care? There is a distribution argument to be sure, but if markets are working well, there's not an economic efficiency question. There are many individuals in society who receive more compensation than others, and that's not usually an efficiency concern. In other words, in the language of economists, this "high pay," even if it were there, would not be a social cost. In addition, this topic is important when considering public policy. For example, we acquired tax distortions in executive compensation in the early 1990s, and there are many executive compensation restrictions being contemplated in various parts of the government today.

But, of course, that is not the thesis of the book. We should care about pay, in the eyes of the book, because there is something wrong with the markets themselves. As economists, of course, we must care about the market problem. If markets are not working, economists, legal academics, and policymakers should have something to say about it. But, before signing on, let me start with a market that has only received a little bit of discussion thus far, the labor market.

Executive compensation is often talked about as a disembodied corporate finance topic. But we are talking about a market for labor in a particular area. Let me begin with a story. I started my teaching career at Northwestern University in 1983, to date myself. I remember my salary rising at a very rapid clip in the early years, and I was very proud of myself because I said, "You know, it must be this article or this seminar that did it." So I went to my department chairman for approval: "Why are you so proud of me?" He said, "Well, the market's going up. Business schools are growing; pay is going up for economists. You're here at the right time." Now, many academics and many businesspeople might have the experience and belief that what moves our compensation

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1. *See* LUCIAN BEBCHUK & JESSE FRIED, PAY WITHOUT PERFORMANCE: THE UNFULFILLED PROMISE OF EXECUTIVE COMPENSATION (2004).

is our own individual performance. However, part of it is demand-and-supply conditions in the market in which we sit. So, the question, "Are CEOs paid too much?," is a question that cannot be answered outside of a specific model of the labor market. This inability to answer is particularly true if one wants to make enough strong allusions to relative performance evaluation to ask: How high is high?

A paper that Charles Himmelberg and I wrote a few years ago stresses quite heavily the importance of the managerial labor market.[2] In that paper, we cast doubt as to whether CEOs are paid too much in any meaningful sense, if one's definition of "too much" is simply a departure from relative performance evaluation. I will review some evidence in a moment, but we did not find that the currently fashionable view that there is a lot of skimming going on was supported by the data. Cross-sectional patterns—that is, compensation patterns across firms—tend to have a lot more to do with shifts in the value of next best alternatives for CEOs, something with which we would all be familiar in our own labor markets.

Now, in this approach to CEO pay, CEOs from a finance and legal perspective have figured prominently in many studies. Some have argued that there are a whole variety of governance mechanisms, and if only we could "get them right" we would raise the value of the firm. In particular, some early work stressed the value of increasing managerial ownership, although my research casts doubt on that notion. That is, there is very little evidence in the data that suggests that simply changing a governance mechanism, like share ownership or independent directors, has much effect on value. This pattern simply indicates that markets try to get it right across an entire range of mechanisms.

There is certainly anecdotal evidence that managers work hard to entrench themselves. Any of us in the room who have been on boards may have seen that, but the question from an economic perspective is not, "Can you find the stories?," but rather, "Are there some high pay levels and failures of relative performance evaluation that can be traced rigorously to entrenchment?"

In the work that Himmelberg and I did, we started with patterns in executive compensation because any model with which we are working should be able to describe these patterns in the data. There is a significant cross-sectional size elasticity of pay with respect to either relative performance or market returns; that is, there's a very big difference in the sensitivities of big firms and small firms. Looking at data across a wide range of publicly traded companies, the data shows that these companies want to log somewhat linear relationships between measures of compensation and measures of performance.

The first characteristic of a labor market that seems to be missing in much of the popular discussion of executive pay is the importance of scale-of-operations effects. That is, the classic superstar argument of the late Sherwin Rosen and others,[3] that small differences in talent in running organizations and in designing strategies for organizations have very different effects on value, depending on the size of the organization.

2. Charles P. Himmelberg & R. Glenn Hubbard, *Incentive Pay and the Market for CEOs: An Analysis of Pay-for-Performance Sensitivity* (Working Paper, 2000), *available at* http://www0.gsb.columbia.edu/faculty/ghubbard/Papers/ceo10.pdf.

3. *See* Sherwin Rosen, *The Economics of Superstars*, 71 AM. ECON. REV. 845 (1981) (discussing the superstar argument that claims there is a "phenomenon of superstars," or relatively small group of people who dominate their fields and earn a great deal of money).

Individuals with scarce skills in those areas have very high alternative value in other corporations and in other industries. In those models, a labor market equilibrium matches CEO talent to firms that value that talent the most.

Now, the twist that Himmelberg and I began in our own work was to observe that in the standard agency model of compensation, CEOs compete in trying to work for firms, and they get some *exogenous reservation utility*, which in economic terms is a reservation level of wellbeing. That would not be what you would expect, of course, in the labor market that I described earlier; changes in the market and booms and busts will change the CEOs' next best alternative—their ability to go run another company—so that their compensation would be pro-cyclical. In booms, alternatives are worth a lot; in busts, they are worth much less. Thus, one would expect to see a correlation between compensation and aggregate stock returns that do not necessarily reflect skimming. Himmelberg and I recorded that version of the standard agency problem and used data from ExecuComp during the 1990s to test if we could find significant evidence to support the story that there are substantial cross-section differences in pay performance sensitivity. Large-firm CEOs tend to have their salaries closely linked to market returns as well as their own idiosyncratic returns. This finding is consistent with the argument I advanced, as it is in large firms that CEOs have the most valuable next best alternatives.

We found that the cash compensation paid to a CEO and relative performance are correlated exactly as agency theory would suggest, but that there is also a correlation between any number of various measures of compensation and market returns. This body of evidence suggests that merely demonstrating an aggregate effect on compensation–that is, one makes more money in booms–does not necessarily imply skimming. I will return to that observation later.

Himmelberg and I found little convincing evidence to support this. Is pay-performance sensitivity too low? One would expect to see low sensitivity for long, tenured managers, but the data showed no statistically significant effects for long or short tenure, or for those promoted from within versus those hired from without. We found no evidence in the data, as opposed to the anecdotes one often hears, that governance variables posed by state takeover laws, such as business combination rules, control share acquisition rules, or fair-price provisions, have any meaningful interaction in those sensitivity tests. We found little evidence that a CEO's ability to obtain higher pay when the market is high is somehow a function of a lax governance environment.

These reservations about findings not withstanding, there is important work in this book, and it suggests areas where economists, finance specialists, and legal academics need to look further. First, on the labor market side, we need to think much harder about what the labor market equilibrium looks like. In the standard models, we talk about polar extremes, that is, fixed reservation wages at one extreme and total competition for the executives at the other extreme. The truth likely lies in between. As Jesse Fried already mentioned in his remarks, there are very interesting questions about the dynamics of boards and the evolution of bargaining power in boards. While reviewing work by Ben Hermalin and Michael Weisbach is suggestive, additional work on what boards actually do would be useful.

One final empirical point I will make is in the handout I distributed. At the simplest level, the labor market story is again about correlations between individual pay as a CEO, and changes in the aggregate environment, as opposed to the firm. My earlier work with

Himmelberg covered a period of a generally rising stock market. More recent data cover generally declining equity values. Now critics have said about those correlations (since most of these papers, including my own, were done prior to the stock market collapse): "How can you simply separate the effect of the stock market booms?" You're getting the correlation that CEO pay rose with the stock market, but distinguishing skimming and labor market equilibrium stories is different. In a declining stock market, one would expect to see the flip side. One would expect to see meaningful declines in CEO pay during these periods. In fact, it occurred during 2001, 2002, and again in 2003 with the recovering market. These patterns underscore the desirability of thinking about a labor market and not a stealing-cash-from-the-till story.

Let me close with an observation about public policy. It is appropriate to have conferences in academia, but we should be very careful about policy implications. At the moment, the policymaking environment, particularly at the SEC, does not give economics the appropriate attention, and policy-directed limits on pay can be quite counterproductive. I agree with what I understand is a big punch line of the book—the far better route is to think about ways to energize boards, and, in particular, institutional investors.