Asymmetric benchmarking in compensation: Executives are rewarded for good luck but not penalized for bad*

Gerald T. Garvey[†] Todd T. Milbourn[‡]

(Received 13 May 2003; accepted 14 January 2004)

* *JEL classification:* D8; G3; J3 *Keywords:* CEO compensation; benchmarking; pay for luck Thanks to Sendhil Mullainathan (American Finance Association discussant), Harold Mulherin, Bill Schwert (editor), Richard Smith, Yisong Tian, Steven Todd, conference participants at the 2004 AFA meetings, seminar participants at Claremont McKenna College, Indiana University, Southern Methodist University, University of Arizona and York University, and especially an anonymous referee for very useful comments. Remaining errors are our own.

[†]Barclays Global Investors, San Francisco, CA 94105, USA

[‡]John M. Olin School of Business, Washington University, St. Louis, MO 63130, USA

Abstract

Principal-agent theory suggests that a manager should be paid relative to a benchmark that removes the effect of market or sector performance on the firm's own performance. Recently, it has been argued that such indexation is not observed in the data because executives can set pay in their own interests; that is, they can enjoy "pay for luck" as well as "pay for performance." We first show that this argument is incomplete. The positive expected return on stock markets reflects compensation for bearing systematic risk. If executives' pay is tied to market movements, they can only expect to receive the market-determined return for risk-bearing. This argument, however, assumes that executive pay is tied to bad luck as well as to good luck. If executives can truly influence the setting of their pay, they will seek to have their performance benchmarked only when it is in their interest, namely, when the benchmark has fallen. Using industry benchmarks, we find significantly less pay for luck when luck is down (in which case, pay for luck would reduce compensation) than when it is up. These empirical results are robust to a variety of alternative hypotheses and robustness checks, and they suggest that the average executive loses 25-45% less pay from bad luck than is gained from good luck.

1. Introduction

Both the level and the stock-price sensitivity of executive compensation have increased dramatically since the 1980s (Hall and Liebman, 1998). Academics (Bertrand and Mullainathan, 2001; Bebchuk and Fried, 2003) and practitioners (Crystal, 1991; Rappaport, 1999) are concerned that the boom in pay was at least in part a windfall. That is, executives were enriched for merely tracking a bull market. Consistent with this view, Bertrand and Mullainathan (2001) show that pay is as sensitive to exogenous forces (henceforth, luck) as it is to firm-specific performance and that the linkage is stronger when shareholders are diffuse and arguably more passive. (A similar finding appears in the empirical literature on relative performance evaluation. See Antle and Smith, 1986 and Aggarwal and Samwick, 1999a, 1999b.)

Our starting point is the observation that an executive whose pay is tied to market movements has relieved her shareholders of some of the firm's systematic risk. By definition, the systematic component of returns provides the fair-market rate of compensation for such services. The argument applies a fortiori to exogenous but unsystematic shocks such as those that affect only a single industry. Tying compensation to such shocks *reduces* an executive's expected utility because the market provides no compensation for bearing such risks. Thus, the Bertrand and Mullainathan (2001) result that the sensitivity of pay to exogenous luck is stronger when a large outside shareholder is absent does not necessarily support their conclusion that such executives are able to set pay in their own interests.

The above argument is based on the estimated sensitivity of compensation to various performance measures. A typical regression coefficient implies that executives' compensation changes by $\$X$ for every \$1,000 change in the market value of the firm's equity, regardless of whether that change is the result of firm-specific performance or exogenous luck. Our argument that pay for luck only provides the executive with the market return in exchange for bearing systematic risk is only valid if compensation *retains* its sensitivity to performance when luck turns out to be bad. In this paper, we find evidence that executives are in fact insulated from bad luck, while they are rewarded for good luck.[1]

[1]Betrand and Mullainathan (2001) note the appearance of such asymmetry but do not pursue the issue system-

The most straightforward interpretation of the data is as follows. At the beginning of any compensation period, luck-based pay will be at best a zero-net present value (NPV) investment from an executive's point of view. It is equivalent to a zero-dollar investment position that yields the right to participate in subsequent gains or losses in a set of market-priced assets. The expected return on this position simply compensates the holder for the systematic risk involved. But compensation is not completely determined in this ex ante fashion. Instead, it is decided at the end of each fiscal year by the compensation committee of the board of directors (see, e.g., Crystal, 1991), at which point it is known whether exogenous forces (luck) have turned out favorably or unfavorably for the firm. At this stage, the executive's self-interest is straightforward: Emphasize benchmarking and remove exogenous influences from compensation only if the benchmark is down. This effectively transforms the executive's investment in the luck component of performance from a zero-NPV appreciation right into a valuable option. The presence of this valuable option is exactly what we find in the data.

Two additional tests further our understanding of the process whereby asymmetric benchmarking takes place. First, the effect is stronger when corporate governance is weaker, according to the measure introduced by Gompers, Ishii, and Metrick (2003). We argue that this makes sense because executives strictly prefer asymmetric benchmarking to simple pay for luck. Second, part of the phenomenon is attributable to stock option granting practices. O'Byrne (1995) and Hall (1999) distinguish two alternative approaches to option grants: fixed-number and fixed-value. Fixed-number granting policies give an executive the same number of options every year. Thus, if the stock price goes up, the executive receives a more valuable grant as well as a capital gain on her outstanding options. As Bertrand and Mullainathan (2001) point out, such a granting policy automatically strengthens pay for luck. But executives prefer weaker pay for luck when luck turns out to be bad. Fixed-value option granting policies increase the number of options granted when the stock price has fallen in an attempt to maintain the total value of the grant. The usual justification is to reduce the prospect of unwanted turnover (something that O'Byrne, 1995 terms "retention

atically. Our measures of luck include industry stock returns at the two-digit standard industrial classification code level. Thus, luck can be more broadly interpreted as the set of exogenous factors affecting a firm's return, which naturally includes the effects of the market portfolio on average industry returns.

risk"). We find evidence that option-granting policies are closer to fixed-value when luck is bad than when it is good. Put another way, compensation seems more concerned about retention risk when luck is bad than with overpayment risk when luck is good.

Our empirical finding that top managers enjoy stronger pay for luck when luck is good is robust to a host of potential benchmark portfolios and specifications. Nonetheless, there are several viable alternative explanations. First, what we have in essence shown is a departure from linearity in the pay for luck relationship. The slope is less steep below zero than it is above zero. But optimal compensation need not be linear in stock market performance. If it is not, and firms do not bother benchmarking, the asymmetry that we document could simply mirror the shape of the optimal relationship between pay and firm-specific performance (henceforth, we refer to this portion of firm returns as "skill").[2] We find, however, that the asymmetries in pay for luck and skill are statistically different from one another.

Oyer (2004) and Himmelberg and Hubbard (2000) argue that benchmarking is not observed in compensation because the value of executives' outside opportunities are also market sensitive. This can explain the absence of benchmarking when the market is up; the executive's market opportunities are also up and she would quit if the firm tried to benchmark her. But we find that executives are benchmarked when the market is down. The Oyer (2004) story can explain our results only if there is also an asymmetry in the sensitivity of executives' outside opportunities to the market. A related version of the labor market argument is that executives possess outside opportunities that are not related to the overall market at all. Examples include working in the non-profit sector, writing a book, or pursuing some other form of outside personal interest. This would imply that in a down market, the firm still needs to provide the executive with a minimum level of compensation to keep her from taking such opportunities. An observationally equivalent idea is that the Chief Executive Officer (CEO) is essentially infinitely risk-averse with respect to reductions in pay. This minimum-pay hypothesis, however, has the same empirical implication as the mirror-image hypothesis outlined above. If the manager has a fixed minimum amount of

[2] Jin (2002) shows that the lack of indexation for the average executive is consistent with efficient contracting. Moreover, Garvey and Milbourn (2003) show that indexation occurs in circumstances in which it would appear efficient, providing managers with insurance when they can least provide for it themselves.

compensation she must receive, her pay will show the same asymmetric response to luck and to skill. We are able to reject this hypothesis in the data.

Another alternative explanation is that executives do indeed suffer losses from bad luck, but such losses take the form of dismissal or turnover. We find, however, that turnover has no statistically significant association with luck and thus no evidence that dismissal is the punishment for bad luck. Finally, the asymmetry between pay for good versus bad luck could reflect an asymmetry in firms' underlying exposure to luck. Firms with appropriate flexibility can reap the gains to good markets while leaving some of the losses from bad luck to be borne by their less flexible rivals. We show that this can explain our results only if flexible firms have stronger pay for performance. We find no evidence to support this condition in a simple test exploiting the fact that flexible firms will also tend to show systematically higher levels of skill.

The remainder of this paper is organized as follows. Section 2 presents a simple model in which pay for luck need not be desired by executives and then delineates our empirically testable hypotheses. Section 3 contains our primary empirical results and several robustness checks, while in Section 4 we provide tests of alternative hypotheses. Section 5 contains additional tests of the source of the skimming results we uncover. Concluding remarks are in Section 6.

2. A simple model of pay for luck

To fix ideas, we first provide a simple model of executive pay in order to tease out our empirical hypotheses.

2.1. Basic results

We consider a one-period model of an all-equity firm employing a single manager. The executive is paid according to firm value, where initial value is denoted V and its end-of-period value is denoted

$$V_1 = V(1 + \beta r_m + \delta L + \varepsilon). \quad (1)$$

For simplicity, we normalize the risk-free rate of interest to zero and set the scale factor $V = 1$. Following standard notation, r_m represents market returns and β is the firm's sensitivity to this market factor. The second shock L is luck and could represent returns to the firm's industry, oil prices, exchange rates, or any other objective index that affects the value of the specific firm. The term δ represents the sensitivity of firm value to luck, and ε is firm-specific risk (or skill), unrelated either to the market or to what we term luck. We assume that market risk is priced but that both luck and skill are diversifiable, so that $E(r_m) > 0$ and $E(L) = E(\varepsilon) = 0$. All shocks are assumed to be independently and normally distributed. The results in this section generalize straightforwardly to a setting with multiple systematic factors and multiple luck factors.

Because the terms r_m and L are both verifiable, the executive's pay can be separately conditioned on both factors. To economize on notation, we assume that the firm chooses to link some of the manager's pay to firm value, but do not model the underlying incentive problem that motivates such a compensation arrangement. The manager's pay is assumed to take the form

$$W = w + a(1 + \varepsilon) + b_m r_m + b_L L, \tag{2}$$

where w represents fixed pay, a is the sensitivity of pay to idiosyncratic firm value (skill), b_m is the extent to which the manager is paid for market movements and b_L is the sensitivity of pay to luck. In a standard principal-agent setting, the term a would represent the marginal reward the manager receives for effort given that her efforts cannot be disentangled from other firm-specific determinants of value.

If the firm makes no attempt to distinguish exogenous forces (market and luck) from firm-specific outcomes, we will observe $b_m = a\beta$ and $b_L = a\delta$. That is, the manager is paid as much for market movements and luck as she is for firm-specific performance. Bertrand and Mullainathan (2001) find evidence that this is the case and argue that this is evidence that at least some executives have captured the pay process, thereby enabling them to enrich themselves at the expense of shareholders. To analyze this claim, however, it is not sufficient to point to cases in which either βr_m is large and positive (i.e., the market has gone up and the firm has a positive beta) or that W increased because of good luck (δL). If an executive has in fact captured the pay process, she will certainly exploit this fact to increase her expected utility. To address this question, we assume that the manager has negative exponential utility with a coefficient of risk-aversion given by k. This allows us to write the certainty-equivalent of her utility in terms of the mean and variance of her compensation, given by

$$U = w + a + b_m \beta E(r_m) - \frac{k}{2}\left[a^2\sigma_e^2 + b_m^2\beta^2\sigma_m^2 + b_L^2\delta^2\sigma_L^2\right]. \quad (3)$$

Bertrand and Mullainathan (2001) show that, on average, managers are paid for luck. That is, b_m and b_L are both positive. However, whether such an arrangement increases the CEO's expected utility depends on the signs of the following[3]

$$\frac{\partial U}{\partial b_m} = \beta E(r_m) - k b_m \beta^2 \sigma_m^2 \text{ and} \quad (4)$$

$$\frac{\partial U}{\partial b_L} = -k b_L \delta^2 \sigma_L^2 . \quad (5)$$

The first term ($\frac{\partial U}{\partial b_m}$) could be positive or negative. Linking the executive's pay to market-wide luck increases her expected utility only if $b_m < \frac{E(r_m)}{k\beta\sigma_m^2}$. This result has a natural interpretation. Because the expected market risk premium is positive, the executive desires some exposure to its shocks. She gains from such exposure so long as it is not excessive, given her risk aversion. The results on pay for luck are even simpler. The term $\frac{\partial U}{\partial b_L}$ is unambiguously *negative*. The executive strictly prefers to be insulated from luck because it is risky and the market provides no compensation for bearing such risks. If an executive has captured the pay process, we would ultimately expect $b_L = 0$.

2.2. Results when executives have access to capital markets

The results above are similar to those in the standard principal-agent literature in which pay is set to maximize shareholder wealth, subject to participation and incentive constraints, rather than to enrich the executives. The interpretation is that it is inefficient to have the manager bear risks that the shareholders can bear at lower costs if there are no incentive effects. Here, the interpretation is that even if the manager can choose her own pay, she will not choose to expose herself to risk when the compensation is insufficient.

Our results imply that the Bertrand and Mullainathan findings are not necessarily consistent with their conclusion that at least some managers have captured the pay process. We say 'not

[3] We take partial derivatives with respect to b_m and b_L and do not consider the possibility that the fixed component w changes to compensate for any utility losses or gains. We ignore this because to implement it empirically would require direct estimates of executives'cash compensation, which are observable, but also requires direct estimates of their outside opportunity wages, which are unobservable.

necessarily' because our model implies that $b_L = 0$, but $b_m = \frac{E(r_m)}{k\beta\sigma_m^2}$. That is, the manager not only will choose pay to insulate herself fully from luck, but also to give herself an optimal positive exposure to the market. Betrand and Mullainathan cannot reject the hypothesis that $b_m = a/\beta$. That is, pay is equally sensitive to market and to idiosyncratic shocks to value. Absent information about the manager's risk-aversion k, we cannot reject the hypothesis that managers are receiving their most preferred exposure to market shocks through their pay arrangements.

Jin (2002) and Garvey and Milbourn (2003) point out that the above analysis ignores the fact that executives can and do choose securities such as mutual funds for their own private portfolios. However, thus far the analysis assumes that they can invest in various markets only implicitly through their compensation. Suppose to the contrary that the manager can also choose to invest c_m dollars of her risk-free wealth w in the market and c_L dollars in a security that tracks the luck factor (an industry index or oil futures, for example). Because the risk-free rate is zero, we can now write her expected utility as

$$U = w + a + (b_m\beta + c_m)E(r_m) - \frac{k}{2}\left[a^2\sigma_e^2 + (b_m\beta + c_m)^2\sigma_m^2 + (b_L\delta + c_L)^2\sigma_L^2\right]. \tag{6}$$

Her private investment choices will satisfy the following first-order conditions

$$\frac{\partial U}{\partial c_m} = E(r_m) - k(b_m\beta + c_m)\sigma_m^2 = 0 \text{ and} \tag{7}$$

$$\frac{\partial U}{\partial c_L} = -k(b_L\delta + c_L)\sigma_L^2 = 0. \tag{8}$$

In light of her optimal private holdings, the effect of pay for luck on her expected utility is given by

$$\frac{\partial U}{\partial b_m} = \beta E(r_m) - \beta k(b_m\beta + c_m)\sigma_m^2 = 0 \text{ and} \tag{9}$$

$$\frac{\partial U}{\partial b_L} = -k\delta(b_L\delta + c_L)\sigma_L^2 = 0. \tag{10}$$

As long as the executive can make her optimal private investment choices c_m and c_L, she is indifferent to the sensitivity of her pay to either market shocks or luck. She can always create her most desired exposure to the market or to luck, regardless of how her pay is determined. This result also carries over if the executive believes she has private information about either the market or luck. She can equally well take bets through her compensation or through her own private investments.

Garvey and Milbourn (2003) recognize that at least some executives will not be able to freely choose their own desired investment positions. Legal restraints, transaction costs, or limited personal wealth, for example, could prevent her from taking large short positions in either the market or her industry. Our basic point still holds, however. Markets and industries go down as well as up, and an executive gains no obvious benefit from being exposed to such risks through her pay. Sometimes she will prosper from good luck, but other times she will suffer from bad luck.

2.3. Second thoughts: asymmetric benchmarking?

Bertrand and Mullainathan (2001) refer to the phenomenon of pay for luck as managerial "skimming." This terminology suggests that perhaps managers skim off only the gains stemming from good luck. Bertrand and Mullainathan note the appearance of an asymmetry for oil price shocks but do not present systematic tests. The fact that executive stock option exercise prices are frequently revised downward after price declines but never, to our knowledge, upward to offset price increases suggests a similar asymmetry (see, e.g., Chance, Kumar, and Todd, 2000). Similar cases exist in the determination of bonuses. Under existing accounting standards, a portion of the investment gains and losses on pension plan assets are recorded as income. During a market upswing, pension gains help increase a firm's net income and ultimately the CEO's bonus payout. However, as markets fall, pension losses reduce net income and, in theory, the CEO's bonus. In response to the economic downturn, many firms, including GE, Delta Air Lines, AK Steel, and Verizon, apparently amended the compensation formula (*Wall Street Journal, 2003*). One particularly glaring example was identified at AK Steel, where "[i]n January 2003, two weeks before announcing the full-year loss for 2002, the company amended the terms of its annual bonus plan, so that bonuses would be pegged to net income 'excluding special, unusual and extraordinary items.' The company similarly amended its long-term incentive plan."

Affording a board of directors some discretion over bonus payouts need not result in such apparently perverse results. Discretion can be optimal in situations in which monitors (e.g., the board of directors) have access to valuable, yet subjective, information surrounding a manager's performance. Murphy and Oyer (2003) document several cases in which bonuses owed to top executives based on objective criteria were disallowed by the board based on "poor individual performance [or] underperformance relative to similar firms." Thus, there exist individual cases where benchmarks are both used appropriately and where they appear instead to enrich executives. Our tests help identify which of the two is more likely to be observed in the broader marketplace.

The following simple model details and clarifies the empirical implications of our argument. The starting point is the recognition that actual compensation, including the use of benchmarks, is not completely determined by a contract. Instead, it is chosen by the compensation committee of the board each year, looking back at past performance. This shifts the focus from ex ante contracting and welfare to ex post outcomes. Here, ex post means after the realization of luck has been revealed. To simplify, focus on one dimension of luck L and associated pay for luck b. Based on the evidence that little benchmarking exists on average, suppose that the default arrangement is one in which pay is as sensitive to luck as it is to skill ($b = a$). However, the executive can induce the board to evaluate and pay her relative to the benchmark L by exerting influence at a cost of I to the executive. Thus, she can shift from a pay for luck regime in which $b = a$ to a benchmarking regime in which $b = 0$. It is rational for the executive to incur the cost I only if it is exceeded by the loss she avoids by being benchmarked, an amount equal to aL. Thus, below the critical luck level of $-\frac{I}{a}$, we will observe benchmarking and for $L > -\frac{I}{a}$ we will observe pay for luck. This is illustrated in Panel A of Fig. 1.

The above argument not only helps lay out our empirical implications, but it also indicates the barriers to successful testing. First, we cannot observe the influence cost I, although we follow the logic of Bertrand and Mullainathan (2001) in arguing that I is lower when governance is weaker. Second, there is no reason to presume that the costs of influence are all fixed. If there are increasing marginal influence costs, we will see a smooth movement from $b = 0$ to $b = a$ as L increases. This is illustrated in Panel B of Fig. 1 for the case in which influence activities reduce b one for one at the strictly increasing cost of $I^2/2$. Third, if the default is full benchmarking and not pay for luck, the critical luck value in Panels A and B would be the positive number $\frac{I}{a}$, at which point the executive exerts effort to shift her pay from benchmarking toward pay for luck. In all cases, however, we have the result that there should be less pay for luck when $L < 0$ (benchmark is down) than when $L > 0$ (benchmark is up).

Insert Fig. 1 near here

3. Empirical analysis

In this section, we describe our data, lay out our empirical methodology, and ultimately provide our main results.

3.1. Data and descriptive statistics

Our data are drawn from two sources. Firm returns and estimates of their volatility come from the Center for Research in Security Prices (CRSP), and the compensation data are drawn from Standard and Poor's ExecuComp. Our sample period covers the years 1992 through 2001, and Panel A of Table 1 summarizes the basic compensation and firm-specific variables for the full sample. These summary statistics cover each firm's executive identified by ExecuComp as the CEO given by the CEOANN field. We drop firms with fiscal years ending in any month other than December because we need to use peer group performance as a benchmark. Finally, because we need to analyze year-to-year changes in compensation, we drop firm-years in which the CEO was changed and in which the CEO served for less than two full years. We address the resulting selection issues in two ways. First, Panel A of Table 1 presents summary statistics from the full ExecuComp sample, whereas Panel B contains the same summary statistics for the subsample we employ in our analyses. Our subsample is not qualitatively different from the full sample. Second, we explicitly analyze the effects of what we estimate as luck and skill on CEO turnover in Section 4.

Insert Table 1 near here

Focusing on our usable subsample in Panel B, salary and bonus represent the CEO's yearly salary and bonus values, respectively, and average to approximately \$634,000 and \$703,000. Option grants represents the Black and Scholes value of the options granted to the CEO in the year and average just under \$2 million. CEO age is the CEO's age in the data year, and CEO tenure is calculated as the difference in years between the fiscal year-end of the current year and the date at which the executive became CEO, as given by the became_CEO field. Stock return is the one-year percentage return for the firm over the calendar year (which also is the firm's fiscal year because we focus only on December fiscal year-ends), and market cap of equity is the firm's market capitalization at the end of the year. The standard deviation of stock returns are computed using the monthly returns of the five years preceding the data year. Not surprisingly, requiring multiple firm-years with the same CEO tends to favor more established firms with more fixed pay, slightly lower volatility and returns, and slightly higher market values. All differences are slight, however. The more important feature of both the full sample and the subsample is the enormous right skewness in the data. For instance, the maximum value of option grants is over \$60 million, and the median value is approximately one-fourth of the mean. To reduce the effects of such outliers, our variables of empirical interest are all winsorized at the 1% level and we estimate robust standard errors.[4] We ignore changes in the value of the CEO's *existing* shares and options because by definition they move only with the stock price and cannot have distinct sensitivities to luck versus skill.

While our theoretical model distinguishes between two exogenous shocks to firm value (market and luck), actual firms are subject to a large number of such shocks. In Table 2, we provide summary statistics for the two benchmarks we use. These include both the equal-weighted and value-weighted industry returns, in which a firm's industry is given by the remaining ExecuComp firms in the same two-digit standard industrial classification (SIC) code. Critical to our ability to test the hypothesis that managers opportunistically benchmark their pay is the fact that the benchmark can take both positive and negative values. To that end, Table 2 summarizes the percentage of the observations for each benchmark that are positive, as given in the column denoted "percent positive". Not surprisingly for our sample period, a large proportion of our sample contains positive benchmark

[4] Our regression results are also robust to the use of median and robust regressions, which also minimize the effect of outliers on coefficient estimates. We report the estimates of median regressions in Tables 5 and 6.

returns.

Insert Table 2 near here

It is important to note that our empirical results are robust to a wide variety of luck measures, including broader market-based measures such as the value-weighted and equal-weighted market return, size decile-based returns, and even more focused measures of luck such as oil price movements. All of these are imperfect measures of luck, and empirically we cannot be certain that we have adequately distinguished between the portion of a firm's return given by luck and that which is affected by the executive's actions and decisions (i.e., her skill). We will inevitably misclassify some luck as skill, and vice versa, and such measurement error will tend to push the estimated pay coefficients on luck and skill closer together. Because our tests are based on estimating different coefficients on luck and skill, simple measurement error biases us against finding this result. We also deal in some detail with a related alternative explanation in which firms' exposures to luck are not linear as we assume in our model specification (see Section 4.1).

We provide simple correlations of some key variables in Table 3. Not surprisingly, both option and total compensation have a strong and positive association with firm size as given by its market capitalization and also with percentage stock returns and risk. Remaining correlations are typical of other compensation studies.

Insert Table 3 near here

3.2. Pay for luck confirmed

We begin our analysis by confirming the result that the average executive receives compensation for exogenous as well as firm-specific performance. We follow the Bertrand and Mullainathan (2001) approach of breaking the test into two stages. First, firm performance (given here by the firm's raw stock return) is regressed on exogenous components, given by the equal-weighted and value-weighted industry returns, with the resulting predicted value representing what we coin luck and the residual representing skill (i.e., the firm-specific component of firm performance).[5] This provides a

[5]We do not include market returns because our estimates all include time dummies. Because market-wide returns vary only by year, they add no information in this setting.

natural and parsimonious way to deal with the large set of potential indices. In terms of the model, we estimate the regression

$$V_1 - 1 = \beta r_m + \delta L + \gamma X + \varepsilon, \tag{11}$$

where ε is the residual from the regression and X represents the indices plus year dummies.[6] The model normalizes beginning of period value to one. In our regressions, we scale both firm returns and the returns on the indices by each firm's market capitalization at the beginning of the year. We then compute the luck component of firm returns as

$$\lambda = \widehat{\beta} r_m + \widehat{\delta} L + \widehat{\gamma} X, \tag{12}$$

where $\widehat{\beta}$, $\widehat{\delta}$, and $\widehat{\gamma}$ are the estimated coefficients from Eq. (11). Summary statistics on our empirical estimates of luck and skill are contained in Panel B of Table 4.

Insert Table 4 near here

To test the effect of luck versus skill on compensation, we follow the approach of Aggarwal and Samwick (1999a) on the pay for performance relationship.[7] First, we use changes in compensation as the dependent variable. Second, we use dollar performance measures for both luck and skill, which involves multiplying predicted and residual stock returns by market capitalization at the beginning of the year. Third, and most important, we follow Aggarwal and Samwick and depart from Bertrand and Mullainathan (2001) by not fixing the sensitivity of pay to either luck or skill to be the same for all firms. Instead, we include interactions between luck and skill and the cumulative distribution function (cdf) of their own respective volatilities. Aggarwal and Samwick show that ignoring this

[6] Bertrand and Mullainathan (2001) also include firm fixed effects in their performance regressions. This effectively says that each firm's average performance over the sample period, even after controlling for market and industry effects, was due to exogenous luck. While our results are qualitatively unaffected by adopting their specification, we omit firm dummies from the performance regression.

[7] Bertrand and Mullainathan (2001), along with many other compensation studies, also use log-log specifications. As stressed by Baker and Hall (2004), theory does not dictate which specification is preferable and our results are similar with the log specification. We report results with the linear specification for two reasons. Empirically, we estimated Box-Cox specifications in which compensation is transformed to $(Comp^{\theta} - 1)/\theta$ and the maximum-likelihood estimate of θ was 1.12. This estimate of θ was insignificantly different from one (implying that the underlying relationship is linear), but was significantly different from zero (implying that the underlying relationship is not log). If we applied the transformation to both compensation and performance (luck and skill), the θ estimate was 0.92, again indistinguishable from one, but statistically different from zero. Theoretically, our hypotheses about asymmetric benchmarking essentially point to a local convexity in the compensation function, which shows up most naturally if we do not perform a concave transformation on the data.

heterogeneity systematically understates the strength of the pay-performance relationship. (We use dollar variance because we are regressing dollar compensation on dollar stock returns.) Thus, our variance measure captures both size and percentage risk effects. This is not a problem for Bertrand and Mullainathan because their question is whether pay is equally sensitive to luck and skill and also because we find their result carries over to the case in which the sensitivity is allowed to differ according to risk. But we need to estimate the attenuation of the pay for luck relationship when luck is down, and thereby require an accurate estimate of this relationship as a first step. In terms of our empirical model, we estimate

$$W = w + aV\varepsilon + bV\lambda + cV\varepsilon \times cdf(Var(V\varepsilon)) + dV\lambda \times cdf(Var(V\lambda)) + gY, \tag{13}$$

where Y contains controls for firm fixed effects, year effects, total risk, and the CEO's tenure.

In the first regressions of Panel A of Table 4, we define W as the change in total direct compensation, which reflects salary, bonus, long-term incentive payments, all other compensation, the market value of restricted stock granted, and the Black, Scholes and Merton value of options granted. In the second and third regressions, we separately treat the two main sources of incentive pay: bonuses and option grants.

We immediately confirm the Bertrand and Mullainathan (2001) finding that executive pay is positively and significantly related to both luck and firm-specific performance. That is, we estimate that both $\widehat{b} > 0$ and $\widehat{a} > 0$. Consequently, the conclusion to be drawn from Panel A of Table 4 is the standard one: indexation (or benchmarking) is not an important feature of the average executive's compensation. The point estimates of the coefficients imply that for a CEO of a firm with median risk, an additional \$1,000 in firm value from skill (which is unrelated to industry or market conditions) will increase total compensation by 96 cents $[1.823 - (\frac{1}{2} \times 1.725)]$, bonus payouts by just under 31 cents, and new option grants by 42 cents. While this represents substantial pay for performance (and significantly understates actual incentives because we omit the ongoing incentives resulting from previously granted shares and options), the same executive will gain 74 cents in total compensation, 18 cents in bonus, and 35 cents in larger option grants for performance that is entirely the result of market or industry factors (i.e., from luck). In no case are the sensitivities to luck and skill statistically different from one another.

These findings are similar to those that led Bertrand and Mullainathan (2001) to conclude that at least some executives skim the gains to luck. However, as argued in our model, such a finding is also consistent with executives bearing exogenous risks that neither increase their expected utility nor decrease shareholder wealth. Below, we present our main findings of *differential* indexation in compensation as a function of realized luck.

3.3. Evidence of asymmetric indexation

As shown in Section 2.3, the most durable and robust prediction of our idea is that there will be less benchmarking when the benchmark is down than when it is up. The actual break point need not be at zero, and we take this point up in Subsection 3.4.

As indicated in the Introduction, optimal contracting provides an alternative explanation for the asymmetry. The optimal incentive contract based on skill could have a nonlinearity near zero. Such a contract could be optimal to encourage managerial risk-taking, or because such outcomes are revealing of the agent's effort (see Hart and Holmstrom, 1987). The firm could also decide that it is optimal not to benchmark the executive (see Jin, 2002, and Garvey and Milbourn, 2003). If so, the apparent nonlinearity in pay for luck is simply the mirror image of an optimal incentive contract based on skill. Our first test of these hypotheses involves allowing the sensitivity of pay to luck and skill to differ depending on whether luck or skill, respectively, is up or down. Specifically, we add the following interaction terms to our specification in Eq. (13):

$$a_D V\varepsilon \times D_1(\varepsilon < 0) + b_D V\lambda \times D_2(\lambda < 0), \tag{14}$$

where $D_1(\varepsilon < 0)$ is an indicator variable taking on the value one if skill is negative, and zero otherwise, and $D_2(\lambda < 0)$ is the analogous indicator variable for luck. The skimming hypothesis predicts that $b_D < 0$. The optimal contracting hypothesis can also explain an empirical finding that $b_D < 0$ but would also imply that $a_D = b_D$. We empirically examine these coefficients next.

In Table 5, we summarize the estimated coefficients from three specifications using changes in total direct compensation, followed by estimates of models of changes in the two most important discretionary components of compensation: bonus payouts and option grants. Not surprisingly, salary and the other components of compensation show negligible sensitivity to either luck or skill. The first column is our primary specification, allowing for both pay for luck and pay for skill to be asymmetric around zero. The second column omits the skill interaction term to ensure that the asymmetry in pay for luck remains even when we restrict pay for skill to be linear. The third column estimates a median regression (minimizing the sum of absolute errors instead of squared errors) to ensure that our results are not driven by outliers. The last two columns repeat our primary specification restricting attention to bonuses and to option grants, respectively.

Insert Table 5 near here

As in the results of Panel A of Table 4, we estimate a positive and significant relationship between changes in total executive pay and the realizations of luck and skill, with both effects weakening as the firm becomes riskier. More importantly, we estimate a negative coefficient for b_D that is both statistically and economically significant whether or not we allow for asymmetry in pay for skill and regardless of the estimation method. Moreover, we consistently reject the hypothesis that the asymmetry in pay for bad luck and pay for bad skill are equal (i.e., $a_D = b_D$). The results also continue to hold if we remove those cases in which compensation (total, bonus, or option grants) is at its lower bound of zero. Thus, our finding that executives are insulated from bad luck is not driven by the fact that they cannot receive negative compensation when luck is particularly bad. However, they can still receive their punishment for bad luck in the form of dismissal, and we take up this alternative explanation later.

The findings summarized in Panel A of Table 4 suggest that, on average, the executive's pay is affected by luck. However, the results of Table 5 show that the executive is rewarded *more* for good luck than she is punished for bad luck. Our point estimates imply that the executive at a firm with median risk receives approximately 79 cents $[1.413 - (\frac{1}{2} \times 1.243)]$ in additional compensation for every additional \$1,000 increase in shareholder wealth because of luck. On the other hand, she loses only 60 cents (given by the sum of the median pay for luck sensitivity of 79 cents and the estimated coefficient on $b_D = -19.2$ cents) for every \$1,000 loss in shareholder wealth due to bad luck, a reduction of almost 25%. The proportional reduction in exposure to bad luck is greater still when we use median regressions, because estimated pay for luck falls more than the estimated asymmetry at zero luck. Equally important, no such favorable asymmetry (from the executive's perspective) is evident in the skill component. In some specifications, the manger is punished somewhat more for bad skill than she is rewarded for good skill.[8]

The primary contribution of our work is that we empirically test the most natural implication of the skimming approach that provides a strictly different prediction than standard agency theory. That is, if top managers can in fact influence the form of their compensation, they will seek indexation (i.e., insurance) only when it is to their advantage to do so. Ex post, insurance is valuable to the manager only when unfavorable outcomes are realized, and this is what our first set of tests strongly suggest. The remainder of the paper supplements these results. We first estimate piecewise linear regressions to push the theory's predictions to their limit and then address alternative explanations for the results. Finally, we attempt to uncover possible sources of the skimming we have identified.

3.4. Testing the break point and functional form

The ex post model (and accompanying Fig. 1) sketched at the end of Section 2 suggests that the relationship between b and L is neither globally concave nor convex. Instead, there should be a significant increase in the estimated coefficient on luck (b) moving from large and negative realizations of luck to values that are closer to zero, with little subsequent increase thereafter. Unfortunately, the appropriate breakpoints are not clear a priori because of the unobservability of both the managerial influence costs (I) and the underlying relationship between these influence costs and the resulting sensitivity of pay to luck. Therefore, in Table 6, we estimate piecewise linear models using various breakpoints for luck that bracket the value of zero. Using the summary statistics on luck and skill from Panel B of Table 4 as a guide, we consider two sets of breakpoints based on percentiles. In each of these cases, the critical value of zero lies strictly between the breakpoints.

[8] This is not to say that the average executive has incentives to reduce risk. We have ingored convexity and pay-performance incentives stemming from options already granted because these must have the same sensitivity to luck and skill. This is also why our estimated sensitivities are less than those in Aggarwal and Samwick (1999a) anbd Milbourn (2003).

Insert Table 6 near here

We amend Eq. (13) by segmenting the contribution of exogenous factors (luck) to the firm's dollar returns ($bV\lambda$) into

$$b_{Low}V\lambda_{\{Low\}} + b_{Med}V\lambda_{\{Med\}} + b_{High}V\lambda_{\{High\}}, \tag{15}$$

where

$$\lambda_{\{Low\}} = \begin{cases} \lambda & \text{if } \lambda < 10^{\text{th}} \text{ percentile of } \lambda \\ 0 & \text{otherwise,} \end{cases} \tag{16}$$

$$\lambda_{\{High\}} = \begin{cases} \lambda & \text{if } \lambda > 90^{\text{th}} \text{ percentile of } \lambda \\ 0 & \text{otherwise, and} \end{cases} \tag{17}$$

$$\lambda_{\{Med\}} = \begin{cases} \lambda & \text{if } \lambda \in [10^{\text{th}}, 90^{\text{th}} \text{ percentiles of } \lambda] \\ 0 & \text{otherwise.} \end{cases} \tag{18}$$

We similarly segment the contribution of firm-specific factors (skill) in that we also include

$$a_{Low}V\varepsilon_{\{Low\}} + a_{Med}V\varepsilon_{\{Med\}} + a_{High}V\varepsilon_{\{High\}} \tag{19}$$

in some specifications. Given the lack of theoretical basis for the exact breakpoint, we also report results for the less extreme case in which low luck is defined as luck below the 20^{th} percentile and high luck is above the 80^{th} percentile.

Our chief hypothesis is whether the pay for luck coefficient in the lowest range of luck is less than the others because it contains values in which the executive would strictly prefer no pay for luck. The secondary hypothesis is that the medium coefficient is less than the high coefficient because the executive is willing to expend more resources to tie her pay to luck as luck increases. To test these hypotheses, we rely on median regressions as in Aggarwal and Samwick (1999a) that minimize the sum of absolute instead of squared errors. Results are qualitatively similar if we use ordinary least squares (OLS), but the coefficients on high and low are strongly affected by extreme values, while the medium coefficient is unaffected. Such issues are unavoidable because by construction the $\lambda_{(Low)}$ range contains the extreme negative values of luck and the $\lambda_{(High)}$ range contains the extreme positive values. Aggressively trimming the outliers is unappealing because of the potential loss of valuable and scarce information from the extreme values of luck. We therefore adapt our estimation technique instead of the data to suit the task at hand.

In the first two columns of Table 6, we designate the extreme deciles as high and low luck (and, respectively, high and low skill, in the case of the second column). In the third column we expand the definition of high and low luck to include the next highest and lowest deciles. In all cases, the coefficient on low luck (b_{Low}) is substantially lower than that on b_{Med} and b_{High}, although all are positive and significantly different from zero. F-tests reject the hypotheses that the coefficient on low luck is equal to those on either medium or high luck at the 1% level. This buttresses our findings from Table 5 (where our results are tied to the breakpoint of zero) that there is less pay for luck when luck turns out to be bad (i.e., when the realized value of luck lies in the lower range). Equally important, we find no such pattern with the skill component of performance and thus no support for the mirror-image explanation of our results.

While we find strong evidence that executives are insulated from bad luck, we do not find evidence that they reap extraordinarily high rewards for good luck. In no case can we reject the hypothesis that b_{Med} and b_{High} are equal to one another. This pattern is entirely consistent with the model sketched in Fig. 1, and indicates the empirical relevance of the constraints we have assumed on managerial skimming. First, executives do not seem able to fully avoid the consequences of bad luck. The coefficient on low luck is about 50% smaller than that on higher levels of luck but is still positive.[9] Second, executives are not able to exploit the gains of good luck to an increasing extent as luck improves. Instead, pay for luck is clearly bounded on the upside by the strength of the pay for skill relationship. That is, executives can reap rewards for good luck only to the extent that their pay is also tied to skill.

4. Alternative explanations

The results thus far strongly support the conclusion that executives can reap the rewards of good luck without sharing proportionately in the losses from bad luck. In this section, we examine

[9] An alternative explanation for the finding that executives are not fully insulated from bad luck is measurement error in the first-stage decomposition of performance into luck and skill. If the true relationship is that the executive is fully insulated from bad luck, we could get our results simply by misclassifying some skill as luck and vice versa.

explanations other than executive skimming for this result. Section 5 then attempts to draw out some of the mechanisms that underlie the ability of at least some executives to gain more from good luck than they lose from bad luck.

4.1. Performance nonlinearities

A logical alternative explanation for our results is that pay is effectively linear in both luck and skill, but firm performance is not. Specifically, a skillful manager could be able to adjust quickly to exploit good luck or to avoid some of the consequences of bad luck or both. Of course, this cannot be true for all firms in an industry or market; some must be in the opposite position of being disproportionately exposed to bad luck.[10] The simplest possible model of this argument would be as follows. Consider an industry with two firms, one common shock L, and no firm-specific shocks. The common shock $L \in \{-1, 0, 1\}$. The firms are identical in size, but the flexible firm's CEO manages to capture all of the positive shock and avoid all of the negative shock. Thus, the flexible firm's returns take on the values $V_f \in \{0, 0, 1\}$ while that of the inflexible firm takes on the values $V_i \in \{-1, 0, 0\}$. Panel A of Fig. 2 depicts the possible outcomes and the consequences of ignoring nonlinearity in performance in estimating each firm's luck. When luck is good, the flexible firm has a dollar return of one and the inflexible firm has a return of zero; our linear estimate will indicate that both had *luck* worth 1/2 and ascribes the remaining performance to *skill* (1/2 for the flexible firm, −1/2 for the inflexible firm). Similarly, when luck is bad, we will estimate $luck = -1/2$ for both firms with the flexible firm again having $skill = 1/2$ (because it achieved a zero return when luck was bad) and the inflexible firm again having $skill = -1/2$.

Insert Fig. 2 near here

To draw out the consequences for our estimated pay relationship, suppose that both firms link pay to performance. Specifically, suppose the flexible firm pays its CEO a bonus f if its returns are one and not zero, and the inflexible firm penalizes its CEO an amount $-i$ if its returns are negative one and not zero. Panel B of Fig. 2 depicts the resulting relationship that we will observe

[10] We thank the anonymous referee for suggesting the alternative explanation and for developing the idea to this point. However, we bear full responsibility for the developments that follow.

between pay and what we estimate as luck. Because we control for executive fixed effects, it is reasonable to normalize pay to zero. When luck is good, the flexible CEO receives a bonus of f and the inflexible CEO receives no bonus. When luck is zero, there are no bonuses or penalties. When luck is bad, the inflexible firm penalizes its CEO $-i$ but the flexible firm does not because it has not suffered from the bad luck. We will therefore estimate a pay for luck sensitivity of f in the good luck state (expected bonus is $f/2$ for $luck = 1/2$) and a pay for luck sensitivity of i in the bad luck state (expected bonus is $-i/2$ for $luck = -1/2$).

It is immediately apparent that performance nonlinearities can explain our observation of asymmetric benchmarking if the flexible firm uses stronger incentives than the inflexible firm ($f > i$), which is arguably a plausible hypothesis. Milbourn (2003) finds evidence that firms with better past performance tend to tie their managers' pay more closely to subsequent performance, but this is not an appropriate test of the condition that $f > i$. Our observation is that flexible firm-CEO pairs will tend to have positive residuals in our luck-skill regressions, and we can use this to classify firms to then test their contemporaneous pay-performance relationships.[11] Milbourn's (2003) evidence implies that $f > i$ only if there is strong persistence in abnormal stock performance over multiple years, a condition that is not supported by the literature on momentum (e.g., Jegadeesh and Titman, 1993), which finds that good performance tends to reverse after approximately six months.

Before constructing a test of the condition $f > i$, it is important to note that our finding that there is no systematic asymmetry in the pay-skill relationship is evidence against the nonlinear performance explanation. The reason is that pay for what we measure as skill in this model is exactly the same as pay for luck in Panel B of Fig. 2. The flexible firm has skill of 1/2 when luck is good and when it is bad. When luck is good, its manager receives a bonus of f but no bonus when luck is bad. Thus, the estimated return for good skill is also f. Similarly, the inflexible firm has skill of $-1/2$ when luck is either good or bad but only bears the loss $-i$ when luck is bad, so the estimated loss for a unit of low skill is $-i$, just as for the case of bad luck.

[11] In presenting the model it was not necessary to carefully distinguish between firm and CEO because they formed unique pairs. In the empirical tests, we focus on the skill of unique firm-CEO pairs.

Table 7 presents an additional test of the nonlinearity explanation, focusing on the condition that flexible firms use stronger incentives than inflexible firms. The additional observation is that flexible firms will tend to have systematically higher skill measures. In our model, the flexible firm has skill of $1/2$ when luck is good or bad, while the inflexible firm has skill of $-1/2$ in both cases. For each CEO-firm pair in our sample, we compute the median value of skill and ask if firms with skillful CEOs have systematically higher pay for performance, when performance now makes no distinction between luck and skill.[12] The first column estimates an interaction term between performance and the CEO-firm pair's median skill level and finds no systematic evidence that higher skill CEOs (and potentially more flexible employing firms) receive stronger pay-performance as required by the asymmetric performance explanation for our results. The last two columns divide the sample into CEOs with high and low median skill levels and show that, if anything, low-skill CEOs have somewhat stronger pay-performance although the difference is not statistically significant. In no case do we find empirical support for the asymmetric performance explanation's key condition that $f > i$. However, one reason we perhaps do not find it is that we are not directly measuring firm flexibility, but instead proxy for it with median (or average) performance.

Insert Table 7 near here

4.2. External labor market forces

The skimming approach views compensation as an arena in which rent extraction takes place. The specific ways in which managers extract rents are primarily through influence over the compensation committee's and regulators' information and incentives. A related approach suggested by Oyer (2004) and Himmelberg and Hubbard (2000) focuses on the managers' threat of quitting not only ex ante (as in the traditional principal-agent model), but also ex post, after performance has been observed. This is a viable explanation for the existence of pay for luck if the manager's outside opportunities fluctuate with market-wide outcomes. But they do not explain the asymmetry between good and bad luck. An up-market could strengthen an executive's outside opportunities and hence her bargaining power, but a down-market should *weaken* such opportunities.

[12] Similar results are obtained if we use average skill levels instead of median levels, but the classification of firms is driven more by extreme values.

An alternative labor-market explanation is that the executive has some market-insensitive, outside opportunity to which she can revert, such as working at a nonprofit organization or writing her memoirs. An observationally-equivalent explanation is that we have optimal ex ante incentive contracts and executives are strongly averse to reductions in their pay below some minimum (see, e.g., Scharfstein, 1988). This minimum pay hypothesis not only suggests why the executive should be insulated from bad luck, but also implies that she should be insulated from firm-specific downturns. We systematically rejected this hypothesis in the previous sections.

4.3. Job loss as punishment for bad luck

The labor market explanations assume that the managers' outside opportunities provide her with bargaining power through the threat of quitting and the associated turnover costs. There is an alternative interpretation of turnover that could explain our results.[13] Suppose that executives are likely to be dismissed when the firm is experiencing bad luck. Because our sample contains only those executives who remain on the job, and compensation cannot be negative, we systematically underestimate the true punishment for bad luck.[14]

To test this explanation, we return to the full sample and code a dummy variable for years in which the CEO position changes hands. We then ask if bad luck is associated with abnormally high turnover in a probit estimate controlling for luck, skill, age, CEO tenure, firm size, and year effects. The first column of Table 8 shows, consistent with the results in Barro and Barro (1990) for bank executives and Brickley, Coles, and Linck (1999) for board members, that the turnover decision appears to be benchmarked. That is, skill has a significant negative effect on the probability of turnover, whereas luck has no discernible effect. Our second column asks whether bad luck or bad skill have distinct effects on turnover, and suggests that if anything, there is asymmetric benchmarking in dismissal as well as in compensation.

Insert Table 8 near here

[13] Thanks again to an anonymous referee for suggesting this explanation.

[14] In our sample, approximately 15% of the bonus observations, 20% of the option grant observations, and 0.2% of the total compensation observations are zero. All results continue to hold if we omit these observations, which is to some degree expected at this point because any asymmetry induced by the zero minimum would also imply asymmetric pay for skill, counter to our results.

Dismissal is, a priori, a viable alternative explanation for our finding that pay increases in good luck but does not symmetrically decrease with bad luck. Extreme punishment effectively requires some kind of job change because pay cannot be negative. Given that the CEO is at the top of the organization, the only unfavorable job change is to be dismissed. These facts undercut our conclusion that executives are insulated from bad luck only if executives are disproportionately likely to lose their jobs when luck is bad. We find no evidence to support this condition and therefore maintain the conclusion that executives lose less from bad luck than they gain from good luck.

5. Additional tests: how does skimming take place?

In this section, we provide two additional empirical tests to further explore how CEO pay skimming actually takes place. To this end, we first examine whether skimming takes place more often in situations marked by weaker corporate governance. Next, we explore whether boards alter their option-granting policies as means of insulating CEOs against bad luck outcomes.

5.1. Governance and skimming

Bertrand and Mullainathan (2001) show that skimming (paying for luck) is weaker in firms for which corporate governance is stronger. They proxy for the efficacy of corporate governance in several ways: the presence of a large shareholder (holding a claim of 5% of the shares or more), CEO tenure, board of directors size, and the fraction of board members who are insiders. The presence of a large shareholder provides their strongest result, and they find that pay for luck is reduced by as much as 33% in firms for which there is a large shareholder. They also find that the presence of a large shareholder becomes more important for reducing CEO pay skimming as CEO tenure increases.[15]

Absent readily available data on the presence of a large shareholder and both board composition and size, we turn to the Corporate Governance Index constructed by Gompers, Ishii, and Metrick (2003).[16] This index is based on the prevalence of various corporate governance provisions at each

[15] Hermalin and Weisbach (1997) provide a theory that suggests CEOs with longer tenures will have greater influence over the board of directors. However, we find that the severity of the asymmetric relationship of pay for luck is insensitive to CEO tenure in our sample.

[16] We thank Andrew Metrick for graciously providing us with these data.

firm and is inversely related to the strength of shareholder rights. That is, lower values of the index correspond to greater shareholder (weaker management) rights, whereas higher values of the index are associated with weaker shareholder (stronger management) rights. The key determinants are takeover defenses, state laws, shareholder voting rights, and protection of directors and officers through insurance and severance arrangements. In our sample, the index ranges from 2 to 17, with a median and average value of 9 for the 5,377 firm-year observations for which we could match our ExecuComp sample with that of Gompers, Ishii and Metrick. (The governance index spans the range 2 through 18 in Gompers, Ishii, and Metrick, 2003.)

To form our testable hypotheses we adapt the logic of the Bertrand and Mullainathan (2001) governance tests. CEOs should be better able to insulate themselves from bad luck outcomes when they are employed by a firm with weaker shareholder rights (i.e., by firms with higher values of the governance index). In the first and second columns of Table 9, we first replicate our analysis of pay for luck in Table 4 for two subsamples of the data. In the first column, we examine the pay for luck relationship in firms for which corporate governance can be characterized as strong ($G \leq 6$). In the second column, we examine the same relationship in firms in which corporate governance is weak ($G \geq 12$).[17] Similar to the results of Table 4, pay is positively and significantly related to both luck and skill in firms with strong and weak corporate governance, respectively. This is consistent with the original Betrand and Mullainathan (2001) findings. However, further consistent with their findings, in the case of firms with stronger corporate governance, pay for luck is only marginally significant and economically smaller than the associated pay for skill coefficient. This is in contrast to the results for firms with weaker corporate governance where the estimated pay for luck coefficient is strictly greater than the associated pay for skill coefficient.

We turn now to an examination of whether asymmetric indexation is in fact more prevalent in firms with weaker corporate governance. In the third and fourth columns of Table 9, we replicate the analysis of Table 5 on the strong and weak corporate governance subsamples by including the

[17] Gompers, Ishii, and Metrick (2003) rely on deciles of the index in their analysis of the effects of governance on corporate performance. Here, we rely on quintiles so as to leave a sufficient number of firm-year observations (roughly 1,000) in each subsample. Qualitatively similar, yet statistically weaker, results are obtained using deciles as the breakpoints.

interaction of luck with an indicator of whether luck was negative. In the case of strong corporate governance, we estimate a positive and significant coefficient (b_D) on this interaction, consistent with the notion that CEOs in better governed firms are not insulated from bad luck as in the average firm. However, in the case of weaker corporate governance, CEOs are partially insulated from bad luck with nearly 25% of the effects of bad luck removed from their ultimate pay. Thus, we conclude on the basis of these results that while the average CEO's pay is asymmetrically indexed (see Table 5), it appears to be marginally more prominent in situations in which the CEO could have greater influence over her pay, such as is exemplified in a firm with weaker corporate governance.

This statement is further supported by the results contained in the fifth column of Table 9, when we employ the full sample of firms. Here, we estimate the effects of asymmetric indexation across three groups by including two additional interactions with our primary interaction of luck with the indicator of whether luck was negative. We interact this primary interaction with an indicator variable denoting whether the firm had strong corporate governance ($G \leq 6$) and also with an indicator of weaker corporate governance ($G \geq 12$). The asymmetric indexation for pay at firms with intermediate quality of corporate governance is captured by the estimated coefficient b_D on the primary interaction. The natural prediction is that we should observe the least asymmetric indexation in firms with stronger governance, moderate asymmetric indexation in firms of intermediate governance strength, and the most severe asymmetric indexation in firms with the weakest governance. Turning to our empirical findings, we see that the data are consistent with the first two predictions, but not the third. That is, we observe no asymmetric benchmarking for firms with $G \leq 6$, significant asymmetric benchmarking for firms of intermediate governance, and insignificant asymmetric benchmarking for firms with $G \geq 12$. That said, we can strongly reject the hypotheses that the relevant b_D coefficient for firms with good corporate governance is equal to those at firms with either intermediate or weak corporate governance. We cannot, however, reject the hypothesis that asymmetric benchmarking is the same at firms of intermediate and weak corporate governance. Our inability to differentiate between these types of firms could simply reflect that once firms move away from the strong corporate governance set, an executive's ability to influence her pay strategically perhaps does not vary significantly. In any event, the results suggest that the strength of shareholder rights does tend to restrain managerial pay-skimming practices.

5.2. Fixed value versus fixed number option granting policies

Hall (1999) shows that, on average, executives receive more valuable option grants when past performance is better, but this is not universally the case. He distinguishes two basic alternative option-granting policies. There are fixed-number granting policies, in which executives get more valuable grants as firm value grows, and fixed-value granting policies, which attempt to hold the yearly value of the option grant fixed even when price falls or rises. According to compensation consultants (O'Byrne, 1995), the apparent goal in this latter policy is to maintain a constant ratio of stock-based pay to fixed pay. Our asymmetric benchmarking story implies that firms would tend to use a fixed-number granting policy when the stock price is driven up by exogenous forces, but attempt to maintain the value of the option grant when luck is bad by increasing the number of options granted.

The most direct test would be to regress the number of options granted on measures of luck and skill, when the hypothesis is that the executive gets a distinct boost when luck is down. But this would add little to our results in Table 5 where we show that the value of new option grants is not much affected by bad luck. Since the value of a new (at-the-money) option is lower when luck is bad, the total value of the grant can only be maintained by granting more options. This is the essence of a fixed-value granting policy. Table 10 presents an alternative test based on the recognition that if firms follow a strict fixed-value granting policy, a regression of the value of option grants to a given CEO in year t on option grants to the same CEO in the previous year will yield a coefficient of one. The first column regresses grants on lagged grants and all the empirical controls used in Panel A of Table 4 (including risk, luck, skill, interactions of luck and skill with risk, tenure, and year dummies). The exception is that we do not use firm fixed effects.[18] Instead, we control for two-digit SIC effects and also include other firm-specific determinants such as firm size (measured by both the book value of total assets and the market value of the firm), the book-

[18]The reason is that, in this case, the lagged grant measure would capture only deviations from the firm's own sample mean grant value. If we employ firm fixed effects, we estimate a negative coefficient because, on average, an above-average grant is followed by a below-average grant.

to-market ratio, and debt-to-total assets (see, e.g., Yermack, 1995). We then obtain the sensible result that the coefficient on lagged option grants is positive and highly significant (firms do have distinct and identifiable granting levels) and is also significantly different from one, implying that some firms follow a fixed-number policy and, more generally, take into account other factors in granting options.

The second column of Table 10 includes interactions between lagged option grants and both bad luck and bad skill. The hypothesis is that if firms revert to fixed-value granting policies when luck is bad, lagged option grants will be a stronger determinant of current option grants in such times. Consistent with this, we estimate a positive coefficient on the interaction of bad luck and lagged option grants. Bad luck drives the coefficient on lagged option grants about 10% closer to the fixed-value level of one. Consistent with our previous estimates of the effects of bad luck versus bad skill, we find that when firm-specific performance is bad, a tendency exists to depart further from fixed-value granting policies and we can reject equality between the coefficients on bad luck and bad skill interactions at the 1% level. The final column of Table 10 confirms that the evidence that granting policies are closer to fixed-value when luck is bad does not rely on the inclusion of the bad skill asymmetry term and its negative coefficient.

6. Concluding remarks

We find that executive pay is most sensitive to industry or market benchmarks when such benchmarks are up. This is consistent with the view that important aspects of executive compensation are not chosen as part of an ex ante efficient contracting arrangement, but rather as a way to transfer wealth from shareholders to executives ex post. Normatively, the message is that the choice of whether to remove luck from an executive's compensation package is less important than is consistency in this choice across years. That is, if the board chooses to use external benchmarks to evaluate performance, these benchmarks should be applied when they have risen as well as when they have fallen. Alternatively, if it is decided that benchmarking is impractical or too costly, this should be applied when benchmarks are down as well as when they are up.

The corporate governance results help flesh out the story. Firms where shareholders are more influential are more likely to use benchmarks consistently across up and down markets. The option granting results suggest that some firms could inadvertently practice asymmetric benchmarking by increasing the number of options granted in down markets while not reducing them in up markets. Further work is necessary to more clearly distinguish rent-seeking from efficient contracting views of executive compensation. Our results lend some empirical support to the rent-seeking approach and also indicate some of the forces that shape and constrain rent-seeking in the compensation process.

References

Aggarwal, R., Samwick, A., 1999a. The other side of the trade-off: the impact of risk on executive compensation. Journal of Political Economy 107, 65-105.

Aggarwal, R., Samwick, A., 1999b. Executive compensation, strategic competition, and relative performance evaluation: theory and evidence. Journal of Finance 54, 1999-2043.

Antle, R., Smith, A., 1986. An empirical investigation of the relative performance evaluation of corporate executives. Journal of Accounting Research 24, 1-39.

Baker, G. P., Hall, B., 2004. CEO incentives and firm size. Journal of Labor Economics 22, 767–798.

Barro, J. R., Barro, R. J., 1990. Pay, performance, and turnover of bank CEOs. Journal of Labor Economics 8, 448-481.

Bebchuk, L. A., Fried, J. M., 2003, Executive compensation as an agency problem. Journal of Economic Perspectives 17-3, 71-92.

Bertrand, M., Mullainathan, S., 2001. Are executives paid for luck? The ones without principals are. Quarterly Journal of Economics 116, 901-932.

Brickley, J. A., Coles, J., Linck, J., 1999. What happens to CEOs after they retire? Evidence on career concerns and CEO incentives. Journal of Financial Economics 52, 341-377.

Chance, D. M., Kumar, R., Todd, R. B., 2000. The "repricing" of executive stock options. Journal of Financial Economics 57, 129-154.

Crystal, G. S., 1991. In Search of Excess: The Overcompensation of American Executives. W.W. Norton, New York.

Garvey, G. T., Milbourn, T. T., 2003. Incentive compensation when executives can hedge the market: evidence of relative performance evaluation in the cross-section. Journal of Finance 58, 1557-1582.

Gompers, P. A., Ishii, J. L., Metrick, A., 2003. Corporate governance and equity prices. Quarterly Journal of Economics 118, 107-155.

Hall, B., 1999. The design of multi-year stock option plans. Journal of Applied Corporate Finance 12, 97-106.

Hall, B., Liebman, J., 1998. Are CEOs really paid like bureaucrats? Quarterly Journal of Economics 113, 654-691.

Hart, O., Holmstrom, B., 1987. The theory of contracts. In: Advances in Economic Theory: Fifth World Congress. Econometric Society Monographs series, no. 12. Cambridge University Press, New York, New York and Melbourne, Australia, pp. 71-155.

Hermalin, B., Weisbach, M., 1997. Endogenously-chosen boards of directors and their monitoring of the CEO. American Economic Review 88, 96-118.

Himmelberg, C., Hubbard, R.G., 2000. Incentive pay and the market for CEOs: an analysis of pay-for-performance sensitivity. Unpublished working paper. Columbia University, New York, New York.

Jegadeesh, N., Titman, S., 1993. Returns from buying winners and selling losers: implications for market efficiency. Journal of Finance 48, 65-91.

Jin, L., 2002. CEO compensation, diversification and incentives. Journal of Financial Economics 66, 29-63.

Milbourn, T. T., 2003. CEO reputation and stock-based compensation. Journal of Financial Economics 68, 233-262.

Murphy, K. J., Oyer, P., 2003. Discretion in executive incentive contracts. Unpublished working paper. University of Southern California, Los Angeles, California.

O'Byrne, S., 1995. Total compensation strategy. Journal of Applied Corporate Finance 8, 185-197.

Oyer, P., 2004. Why do firms use incentives that have no incentive effects? Journal of Finance 59, 1619-1649.

Rappaport, A., 1999. New thinking on how to link pay to performance. Harvard Business Review 77-2, 91-105.

Scharfstein, D., 1988. The disciplinary role of takeovers. Review of Economic Studies 45, 185-199.

Wall Street Journal, 2003, Pensions fall – not CEO's bonus. Jesse Drucker, Theo Francis, June 18.

Yermack, D., 1995. Do corporations award CEO stock options effectively? Journal of Financial Economics 39, 237-269.

Table 1

Descriptive statistics of chief executive officers (CEOs) and firms. The data are collected for every CEO in the ExecuComp database as defined by the CEOANN field for each year 1992-2001. Salary and bonus represent the CEO's yearly salary and bonus values. Cash compensation is the sum of salary, bonus, long-term incentive payouts and all other cash compensation paid. Option grants represent the Black and Scholes value of the options granted to the CEO in the year. Total direct compensation is the sum of salary, bonus, other annual compensation, long-term incentive payouts, other cash payouts, and the value of restricted stock and stock option awards. CEO age is the CEO's age in the data year, and CEO tenure is calculated as the difference between the fiscal year-end of the current year and the date at which the CEO became CEO as given by Became_CEO. Stock return is the one-year percentage return for the firm over its fiscal year. Market cap of equity is the firm's market capitalization at the end of the firm's fiscal year in millions of dollars. The standard deviation of stock returns (stock volatility) is computed using the five years of monthly data preceding the data year. Compensation data are in thousands, and market values are in millions of yearly dollars. Panel A contains the full ExecuComp sample, and Panel B, which contains only CEOs with at least two consecutive years of coverage, contains the subsample upon which we perform our analysis.

Variable	Number of observations	Mean	Minimum	Median	Maximum	Standard deviation
Panel A. Full ExecuComp sample						
Salary	13,737	577.6	0	521.9	4,000	316.3
Bonus	13,737	605.8	0	300	102,015.2	1,552.8
Option grants	13,737	2,371.1	0	521.2	60,347.4	6,638.6
Total compensation	13,737	3,703.6	0	1,817.0	293,097	11,363.2
CEO age	6,809	57.6	30	58	88	7.8
CEO tenure	12,223	7.7	0	5.6	35.8	7.1
Stock return	13,737	0.266	-0.991	0.111	617.8	5.40
Market cap of equity	13,737	5,656.4	0.424	1,199.0	507,216.7	19,158.2
Stock volatility	13,737	0.387	0.102	0.343	3.48	0.194

Panel B. Sample used in regression analyses						
Salary	6,263	634.1	0	584.4	4,000	337.1
Bonus	6,263	703.6	0	361.5	102,015	1834
Option grants	6,263	1,984.6	0	569.1	60,347	4,221.2
Total compensation	6,263	4,424.9	0	2,063.2	293,097	10,043.1
CEO age	3,849	58.1	33	58	87	7.29
CEO tenure	6,263	8.31	0	6.25	35	6.95
Stock return	6,263	0.252	-0.991	0.0977	617.8	7.26
Market cap of equity	6,263	6,341.2	0.424	1,585.4	507,216	19,643
Stock volatility	6,263	0.365	0.102	0.317	3.49	0.191

Table 2

Descriptive statistics of performance benchmarks. The data are collected for every firm in which a chief executive officer (CEO) in the ExecuComp database is identified as defined by the CEOANN field for each year 1992-2001. The equal-weighted and value-weighted industry returns on are based on the firm's two-digit standard industrial classification (SIC) code. Summary statistics for returns are in decimal form. The percent positive represents the proportion of the sample for which the relative benchmark return is positive.

Variable	Number of observations	Percent positive	Mean	Minimum	Median	Maximum	Standard deviation
Equal-weighted industry returns	13,737	82.6%	0.256	-0.831	0.167	19.97	0.934
Value-weighted industry returns	13,737	73.6%	0.161	-.831	0.146	1.211	0.255

Table 3

Simple correlations among chief executive officers (CEO) and firm variables. Pairwise correlations are carried out for each data item as collected for every CEO in the ExecuComp database as defined by the CEOANN field for each year 1992-2001. Option grants represent the Black and Scholes value of the options granted to the CEO in the year. Total direct compensation (total comp) is the sum of salary, bonus, other annual compensation, long-term incentive payouts, other cash payouts, and the value of restricted stock and stock option awards. CEO age is the CEO's age in the data year, and CEO tenure is calculated as the difference between the fiscal year-end of the current year and the date at which the CEO became CEO as given by Became_CEO. Stock return is the percentage return for the firm over its fiscal year. Market cap of equity is the firm's market capitalization at the end of the firm's fiscal year. The standard deviation of stock returns (stock volatility) is computed using the five years of monthly data preceding the data year. Significance levels are given in parentheses below the correlations. * indicates different from zero at the 1% level and ** at the 5% level .

	Option grants	Total comp	CEO age	CEO tenure	Stock return	Market cap of equity
Option grants	1					
Total comp	0.750*	1				
	(0.00)					
CEO age	−0.440*	0.008	1			
	(0.003)	0.57				
CEO tenure	−0.258**	−0.0041	0.436*	1		
	(0.140)	0.70	(0.00)			
Stock return	0.051*	0.030*	−0.035**	0.015	1	
	(0.00)	(0.003)	(0.017)	(0.17)		
Market cap of equity	0.354*	0.359*	0.083*	−0.002	−0.042*	1
	(0.00)	(0.00)	(0.00)	(76)	(0.00)	
Stock volatility	0.122*	0.068*	−0.267*	−0.006	0.055*	−0.113*
	(0.00)	(0.00)	(0.00)	(0.61)	(0.00)	(0.00)

Table 4

Column 1 of Panel A contains an ordinary least squares regression of changes in total direct chief executive officer compensation on the contribution of exogenous factors (luck) on the performance of the firm's dollar returns, the contribution of firm-specific performance, the cdf of the dollar variance of firm returns, tenure, and an interaction of luck with the cdf of the variance of luck and skill with the variance of skill, plus executive fixed effects and year effects. Column 2 replaces total compensation with bonus, and Column 3 uses the Black and Scholes value of options granted. Total direct compensation is the sum of salary, bonus, other annual compensation, long-term incentive payouts, other cash payouts, and the value of restricted stock and stock option awards. In the row labeled $\Pr(b = a)$, we provide the p-value from the test that $b - a = 0$. $\Pr(b = a)$ is evaluated at the median for both variance of luck and skill. Robust standard errors are reported in parentheses, and the coefficients on the intercept, the cdf of the dollar variance tenure, and the year and executive fixed effects are suppressed for convenience. * indicates different from zero at the 1% level, ** at the 5% level, and *** at the 10% level. In each case, there are 6,263 observations. Panel B contains summary statistics on the predicted dollar values of luck (λ) and the residual skill (ε) from our first-stage regression of firm returns on equally-weighted and value-weighted average industry returns, where industry is given by the firm's two-digit standard industrial classification code.

Panel A. Paying for luck

Independent variables	Total compensation	Bonus	Option grants
	(1)	(2)	(3)
Luck (λ)	1.405*	0.346*	0.703**
	(0.463)	(0.058)	(0.352)
Skill (ε)	1.823*	0.598*	0.842*
	(0.420)	(0.047)	(0.253)
Luck × cdf variance of luck	−1.33*	−0.334*	−0.701**
	(0.438)	(0.056)	(0.357)
Skill × cdf variance of skill	−1.725*	−0.579*	−0.844*
	(0.316)	(0.050)	(0.278)
R^2	0.197	0.290	0.188
$\Pr(b = a)$	0.47	0.018	0.67

Panel B. Summary statistics on luck and skill

Variable	Mean	1%	Median	99%	Standard deviation
Luck (λ)	945.8	-2,944.4	154.7	17,124.1	3,884.0
Skill (ε)	-159.9	-14,014.4	-35.4	11,030.5	4,329.2

Table 5

Testing for asymmetry around zero. Column 1 contains an ordinary least squares regression of changes in total direct chief executive officer compensation on the contribution of exogenous factors (luck) on the performance of the firm's dollar returns, the contribution of firm-specific performance, and interactions of both luck and skill with dummy variables indicating that luck or skill are negative. We also control for the cdf of the dollar variance of firm returns, tenure, and an interaction of luck with the cdf of the variance of luck and skill with the variance of skill, plus executive fixed effects and year effects. Column 2 estimates the same model, but drops the interaction of skill and the dummy variable indicating that skill is negative. Column 3 estimates the same model as Column 1, but relies on a median regression. Using the same specification as in Column 1, Column 4 replaces total compensation with bonus and Column 5 uses the Black and Scholes value of options granted. Total direct compensation is the sum of salary, bonus, other annual compensation, long-term incentive payouts, other cash payouts, and the value of restricted stock and stock option awards. In the row labeled $\Pr(b_D = a_D)$, we provide the p-value from the test that $b_D - a_D = 0$. Median bad luck removed for median firm is equal to the opposite of the coefficient on the luck×luck is down (b_D), divided by the sensitivity of pay to luck (coefficient b on λ) for a firm with the median riskiness of luck. Robust standard errors are reported in parentheses, and the coefficients on the intercept, the cdf of the dollar variance, tenure, and the year and executive fixed effects are suppressed for convenience. * indicates different from zero at the 1% level and ** at the 5% level. N/A stands for not applicable. In each case, there are 6,263 observations.

Independent variables	Total comp	Total comp (No skill interaction)	Total comp (Median regression)	Bonus	Option grants
	(1)	(2)	(3)	(4)	(5)
Luck (λ)	1.413*	1.314*	0.598*	0.338*	0.742**
	(0.461)	(0.398)	(0.075)	(0.051)	(0.372)
Skill (ε)	1.705*	1.597*	0.804*	0.595*	0.823*
	(0.312)	(0.289)	(0.156)	(0.039)	(0.265)
Luck $\times$ luck is down (b_D)	−0.192*	−0.163*	−0.151*	−0.0249*	−0.121**
	(0.077)	(0.043)	(0.028)	(0.005)	(0.060)
Skill $\times$ skill is down (a_D)	0.169*		−0.0323	0.0184*	0.0701
	(0.056)		(0.028)	(0.005)	(0.027)
Luck $\times$ cdf variance of luck	−1.243*	−1.093*	−0.554*	−0.321*	−0.721**
	(0.436)	(0.402)	(0.067)	(0.051)	(0.356)
Skill $\times$ cdf variance of skill	−1.442*	−1.488*	−0.912*	−0.599*	−0.810*
	(0.317)	(0.294)	(0.153)	(0.039)	(0.248)
R^2	0.199	0.201	0.187	0.292	0.189
$\Pr(b_D = a_D)$	0.0004	N/A	0.0001	0.0003	0.025
Median bad luck removed	24.6%	21.2%	47.0%	14.1%	31.7%

Table 6

Piecewise linear regressions. This table contains median regressions of changes in total direct chief executive officer compensation on the contributions of exogenous factors (luck) and firm-specific performance (skill) on the performance of the firm's dollar returns over various regions (based on percentiles) of these two variables. Specifically, in Columns 1 and 2, we define low luck as the region over which luck took a value less than the 10^{th} percentile, high luck as the region over which luck took a value greater than the 90^{th} percentile, and medium luck as the region in between. We do this similarly for the values of skill in the second column. In Column 3, we alter the breakpoints to the $20^{th}/80^{th}$ percentiles, respectively. The regressions also include as control variables the cdf of the dollar variance of firm returns, an interaction of luck with the cdf of the variance of luck and skill with the variance of skill, plus firm fixed effects and year effects. Total direct compensation is the sum of salary, bonus, other annual compensation, long-term incentive payouts, other cash payouts, and the value of restricted stock and stock option awards. In the row labeled Pr(LowLuck = MedLuck), we provide the p-value from the test that LowLuck - MedLuck = 0. Bootstrapped standard errors with 100 repetitions are reported in parentheses, and the coefficients on the intercept, the cdf of the dollar variance, tenure, and the year and executive fixed effects are suppressed for convenience. * indicates different from zero at the 1% level, ** at the 5% level, and *** at the 10% level. In each case, there are 6,263 observations.

Independent variables	$10^{th}/90^{th}$ Percentile (1)	$10^{th}/90^{th}$ Percentile (2)	$20^{th}/80^{th}$ Percentile (3)
Low luck ($\lambda_{\{Low\}}$)	0.221*	0.272*	0.328*
	(0.060)	(0.065)	(0.059)
Medium luck ($\lambda_{\{Med\}}$)	0.376*	0.403*	0.423*
	(0.054)	(0.058)	(0.064)
High luck ($\lambda_{\{High\}}$)	0.389*	0.420*	0.458**
	(0.061)	(0.064)	(0.059)
Low skill ($\varepsilon_{\{Low\}}$)		0.866*	0.864*
		(0.041)	(0.053)
Medium skill ($\varepsilon_{\{Med\}}$)		0.871*	0.867*
		(0.052)	(0.060)
High skill ($\varepsilon_{\{High\}}$)		0.870*	0.981*
		(0.048)	(0.071)
Luck×cdf variance of luck	−0.290**	−0.337*	−0.388*
	(0.106)	(0.072)	(0.070)
Skill×cdf variance of skill	−0.612*	−0.804*	−0.807*
	(0.091)	(0.051)	(0.046)
Pseudo R^2	0.172	0.191	0.188
Pr(Low Luck = Med Luck)	0.000	0.000	0.0016
Pr(Med Luck = High Luck)	0.532	0.429	0.358
Pr(Low Luck = High Luck)	0.000	0.000	0.000
Percentile breakpoints			
Low/high luck breakpoints	-158.7/2,243.4	-158.7/2,243.4	-6.70/822.9
Low/high skill breakpoints	-1,349.0/1,013.5	-1,349.0/1,013.5	-470.9/282.7

Table 7

Pay-for-performance and executive skill. This table contains ordinary least squares regressions of changes in total compensation on dollar returns (luck plus skill) and measures of each firm-CEO (chief executive officer) pair's median skill measure. Robust standard errors are in parentheses, and the coefficients on the intercept, the cdf of the dollar variance tenure, and the year and executive fixed effects are suppressed for convenience. * indicates different from zero at the 1% level and ** at the 5% level.

Independent variables	Full sample	High median skill	Low median skill
Dollar return	−2.158*	1.607*	2.342*
	(0.491)	(0.634)	(0.758)
Dollar return × cdf risk	−2.178*	−1.554*	−2.413*
	(0.514)	(0.218)	(0.788)
Dollar return × median skill	0.099		
	(0.073)		
Adjusted R^2	0.202	0.201	0.224
Number of observations	6,263	3,132	3,131

Table 8

Luck versus skill and CEO turnover. This table contains probit regressions of the probability the CEO leaves her firm in a given year. Age is a dummy variable that takes on the value one if the CEO is 65 years or older. Column 1 uses continuous measures of the contribution of exogenous factors (luck) on the performance of the firm's dollar returns and the contribution of firm-specific performance (skill). Column 2 uses dummy variables taking on the value one if the relevant measure is negative. In the row labeled Pr (b = a), we provide the p-value from the test that b - a = 0. The estimated intercept term and coefficients on controls for market capitalization, asset value, and year dummies are suppressed for convenience. * indicates different from zero at the 1% level and ** at the 5% level. In each case, there are 6,809 observations.

	Continuous measures	Dummy for bad luck and skill
	(1)	(2)
Luck	−0.008	
	(0.025)	
Skill	−0.0291*	
	(0.011)	
Luck is down		−0.126
		(0.135)
Skill is down		0.266**
		(0.115)
Age	0.0210**	0.202**
	(0.009)	(0.009)
Tenure	−1.749*	−1.651*
	(0.254)	(0.077)
Log likelihood	-836	-877
$\Pr(b = a)$	0.31	0.027

Table 9

Differential asymmetry based on governance index. Columns 1-5 of this table contain ordinary least squares regressions of changes in total direct chief executive officer compensation on the contribution of exogenous factors (luck) on the performance of the firm's dollar returns, the contribution of firm-specific performance, the cdf of the dollar variance of firm returns, tenure, and interactions of luck with the cdf of the variance of luck and skill with the variance of skill, plus industry fixed effects and year effects. Columns 1 and 3 contain coefficient estimates for the subsample of firms in the bottom quintile ($G \leq 6$) of the Gompers, Ishii, and Metrick (2003) Corporate Governance Index (strong governance). Columns 2 and 4 contain coefficient estimates for the subsample of firms in the top quintile ($G \geq 12$) of the Corporate Governance Index (weak governance). Columns 3 and 4 include the interaction of luck with a dummy variable indicating whether luck is negative. Column 5 contains estimates from the same model for the full sample but includes two additional interactions of luck with a dummy variable indicating whether luck is negative with another dummy variable indicating whether corporate governance is good ($G \leq 6$) or poor ($G \geq 12$). Here, we can interpret the coefficient on luck with an indicator of whether luck is negative as capturing the firms of intermediate governance quality. Total direct compensation is the sum of salary, bonus, other annual compensation, long-term incentive payouts, other cash payouts, and the value of restricted stock and stock option awards. Robust standard errors are reported in parentheses, and the coefficients on the intercept, the cdf of the dollar variance, tenure, and the year and industry fixed effects are suppressed for convenience. * indicates different from zero at the 1% level, ** at the 5% level, and *** at the 10% level.

Independent variables	Strong governance G ≤ 6 (1)	Weak governance G ≥ 12 (2)	Strong governance G ≤ 6 (3)	Weak governance G ≥ 12 (4)	Full sample (5)
Luck (λ)	1.91***	2.45*	1.56	2.81*	1.28*
	(1.02)	(0.719)	(1.03)	(0.735)	(0.382)
Skill (ε)	2.77*	1.44*	2.83*	1.46*	1.60*
	(0.765)	(0.493)	(0.765)	(0.492)	(0.264)
Luck × luck is down (b_D)			0.312***	−0.364**	−0.238*
			(0.165)	(0.161)	(0.076)
Luck × luck is down ×					0.44*
strong governance$_{\{G\leq6\}}$					(0.149)
Luck × luck is down ×					−0.021
weak governance$_{\{G\geq12\}}$					(0.170)
Luck × cdf variance of luck	−1.90***	−2.34*	−1.584	−2.67*	−1.22*
	(1.038)	(0.737)	(1.051)	(0.750)	(0.388)
Skill × cdf variance of skill	−2.93*	−1.30**	−3.00*	−1.32*	−1.55*
	(0.795)	(0.510)	(0.795)	(0.510)	(0.272)
Number of observations	944	1,146	944	1,146	5,377
R^2	0.061	0.106	0.065	0.110	0.026
Pr(StrongGov = IntermGov)					0.0009
Pr(StrongGov = WeakGov)					0.4681
Pr(IntermGov = WeakGov)					0.0254

Table 10

Option granting policies and luck. The dependent variable in all cases is the Black and Scholes value of options granted. Lagged option grants is the value of options granted in the previous year. Robust standard errors are in parentheses, and the coefficients on the control variables (risk, luck, skill, luck and skill interacted with the cdf of risk, asset value, market capitalization, debt/assets, tenure, and year and two-digit standard industrial classification dummies) are suppressed for convenience. * indicates different from zero at the 1% level, ** at the 5% level, and *** at the 10% level. N/A stands for not applicable. In each case, there are 6,263 observations.

Independent variables	Value of options granted		
Lagged option grant	0.349*	0.353*	0.337*
	(0.013)	(0.020)	(0.015)
Lagged option grant × luck is down (b_D)		0.0411**	0.0439**
		(0.021)	(0.0201)
Lagged option grant × skill is down (a_D)		−0.0437***	
		(0.023)	
Adjusted R^2	0.337	0.339	0.338
$\Pr(b_D = a_D)$	NA	0.0058	NA

Fig. 1

Pay for luck relationship. Panel A depicts pay for luck with fixed influence costs; Panel B depicts pay for luck with marginal influence costs.

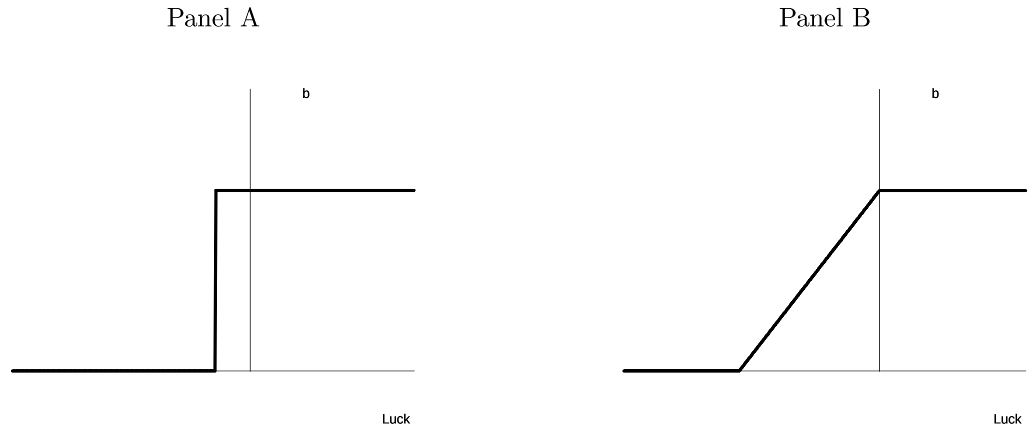

Fig. 2

Asymmetric performance: Panel A depicts the realizations of luck and skill, while Panel B depicts the implied pay for luck relationship that would be estimated.

Panel A

Panel B