Asymmetric Benchmarking in Compensation: Executives are Rewarded for Good Luck But Not Penalized for Bad*

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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 we do not observe such indexation 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 that there is 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 suggest that the average executive loses 25-45% less pay from bad luck than she gains from good luck.

1 Introduction

Both the level and the stock-price sensitivity of executive compensation have increased dramatically since the 1980's (Hall and Liebman, 1998). The concern has been raised among academics (Bertrand and Mullainathan, 2001; Bebchuk and Fried, 2003) and practitioners (Crystal, 1991; Rappaport, 2000) 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) document 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.[1]

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 since the market provides no compensation for bearing such risks. Thus, Bertrand and Mullainathan's (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.

Observe that the above argument is based on the *estimated* sensitivity of compensation to various performance measures.[2] A typical regression coefficient will imply 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 was due to firm-specific performance or to 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

[1] A similar finding appears in the empirical literature on relative performance evaluation (see Antle and Smith, 1986; Aggarwal and Samwick, 1999a,1999b).

[2] Total compensation for the individual year is comprised of salary, bonus, other annual pay, the total value of restricted stock and stock options granted (using Black-Scholes), long-term incentive payouts, and all other compensation from the "Summary Compensation Table".

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.[3]

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-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. Rather, 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 removal of 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, we find that the effect is stronger when corporate governance is weaker according to the measure introduced by Gompers, Ishii and Metrick (2003). This makes sense since as argued above, executives strictly prefer asymmetric benchmarking to simple pay-for-luck. We are also able to attribute part of the phenomenon 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

[3]Betrand and Mullainathan (2001) note the appearance of such asymmetry but do not pursue the issue systematically. Our measures of luck include industry stock returns at the 2-digit SIC 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.

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 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.[4] Nonetheless, there are several viable alternative explanations. First, what we have in essence documented 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,[5] 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"). We find, however, that the asymmetries in pay for luck and skill are statistically different from one another.

Oyer (2003) and Himmelberg and Hubbard (2000) argue that we do not observe benchmarking 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 you tried to benchmark her. But we find that executives are in fact benchmarked when the market is down. The Oyer (2003) 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

[4]These alternative portfolios include the value-weighted CRSP index, the S&P 500 returns, and size-decile-based stock returns. We forgo these benchmarks in the results we report here since we are interested in contrasting the differential sensitivity of CEO pay to luck in both good and bad times. Using these broader market indices ultimately reduces to simply including year fixed effects, for which we can control directly in our regression analysis. We also employed a more refined measure of luck (oil prices) for a subsample of our firms operating in the Oil and Gas Extraction industry (SIC 13). Our results were unaffected owing to the fact that oil price movements are nearly perfectly collinear with our industry return benchmarks.

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

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 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 compensation she must receive, her pay will show the same asymmetric response to luck and to skill. As indicated above, 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, it is possible that the asymmetry between pay for good versus bad luck reflects 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 document. Concluding remarks are in Section 6.

2 A Simple Model of Pay for Luck

2.1 Basic results

To fix ideas, consider the following simple, one-period model of an all-equity firm that employs 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:

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

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 to either 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.

Since 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 simply 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

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

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 since 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 indeed 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 where either βr_m is large and positive (i.e., the market has gone up and the firm has a positive beta), or similarly that W increased because of good luck (δL). Naturally, if an executive has in fact captured the pay process, she will use it 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

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

Bertrand and Mullainathan (2001) document 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:[6]

$$\begin{aligned} \frac{\partial U}{\partial b_m} &= \beta E(r_m) - k b_m \beta^2 \sigma_m^2 \\ \frac{\partial U}{\partial b_L} &= -k b_L \delta^2 \sigma_L^2 . \end{aligned}$$

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. Since 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 $b_L = 0$.

[6] Note that 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' outside opportunity wages (unobservable), as well as their cash compensation (observable).

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-Mullainathan findings are not necessarily consistent with their conclusion that at least some managers have captured the pay process. We say "not necessarily" because our model actually implies that $b_L = 0$, but $b_m = \frac{E(r_m)}{k\beta\sigma_m^2}$. That is, the manager 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 in fact 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 above assumes that they can only invest in various markets 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). Since the risk-free rate is zero, we can now write her expected utility as:

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

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

$$\begin{aligned} \frac{\partial U}{\partial c_m} &= E(r_m) - k(b_m\beta + c_m)\sigma_m^2 = 0 \\ \frac{\partial U}{\partial c_L} &= -k(b_L\delta + c_L)\sigma_L^2 = 0. \end{aligned}$$

We see now that in light of her optimal private holdings, the effect of pay-for-luck on her expected utility is given by:

$$\begin{aligned} \frac{\partial U}{\partial b_m} &= \beta E(r_m) - \beta k(b_m\beta + c_m)\sigma_m^2 = 0 \\ \frac{\partial U}{\partial b_L} &= -k\delta(b_L\delta + c_L)\sigma_L^2 = 0. \end{aligned}$$

As long as the executive can make her optimal private investment choices c_m and c_L, she is now 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. Note that 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 in fact be able to freely choose their own desired investment positions. Legal restraints, transaction costs or limited personal wealth, for example, may prevent her from either taking large short positions in 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 due to 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, et al, 2000). There are also similar cases 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 – amended the compensation formula.[7] A glaring example is as follows:

> In 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.

Of course, affording a board of directors some discretion over bonus payouts need not result in such apparently perverse results. Discretion can be optimal in situations where 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 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. Rather, as stressed above, 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. Suppose, based on the evidence that there is little benchmarking on average, 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 where $b = a$, to a benchmarking regime where $b = 0$. 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,

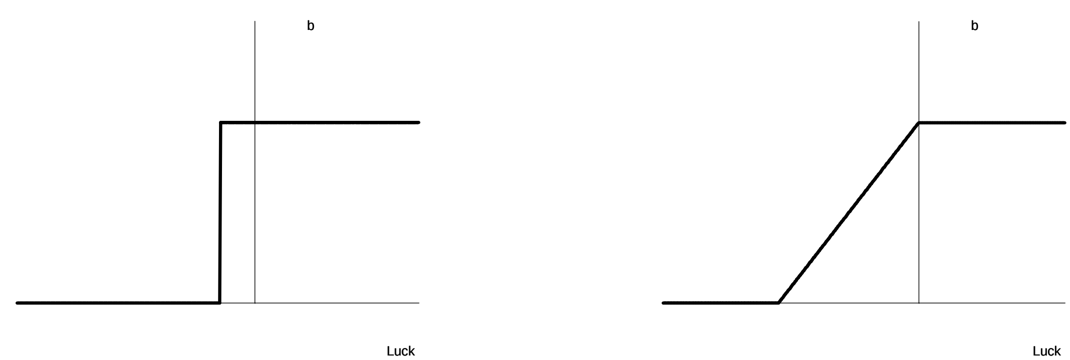
[7] See Jesse Drucker and Theo Francis, "Pensions Fall – Not CEO's Bonus", *The Wall Street Journal*, June 18, 2003, Section C1.

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 *Figure 1.*

The above argument helps lay out our empirical implications, but 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 also 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 *Figure 1* for the case where influence activities reduce b one-for-one at the strictly increasing cost of $I^2/2$. Lastly, observe that if the "default" is full benchmarking rather than 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 conclusion that there should be less pay-for-luck when $L < 0$ (benchmark is down) than when $L > 0$ (benchmark is up).

Figure 1: Pay-for-Luck Relationship

Panel A: Pay-For-Luck with Fixed Influence Costs Panel B: Pay-For-Luck with Marginal Influence Costs



3 Empirical Analysis

3.1 Data and Descriptive Statistics

Our data are drawn from two sources. Firm returns and estimates of their volatility come from CRSP, and the compensation data are drawn from Standard and Poors' ExecuComp. Our sample period covers the years 1992 through 2001, and *Table 1A* 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 other than December because we need to use peer group performance as a benchmark. Finally, since 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, *Table 1A* presents summary statistics from the full ExecuComp sample, whereas *Table 1B* contains the same summary statistics for the subsample we employ in our analyses. As seen in the table, 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.

Focussing on our usable subsample in *Table 1B*, Salary and Bonus represent the CEO's yearly salary and bonus values, respectively, and average to approximately $634,000 and $703,000, respectively. Option Grants represents the Black-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,[8] 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 that there is 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 also estimate robust standard errors.[9] We ignore changes in the value of the CEO's *existing* shares

[8] This is also the firm's fiscal year since we focus only on December fiscal year-ends.

[9] 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*.

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, where a firm's industry is given by the remaining ExecuComp firms in the same 2-digit 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 *% Positive.* Not surprisingly for our sample period, a large proportion of our sample contains positive benchmark returns.

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 focussed measures of luck such as oil price movements. Naturally, 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). It is inevitable that we will 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. Since 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 also typical of other compensation studies. Below, we turn to the empirical analysis.

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 Bertrand and Mullainathan's (2001) approach of breaking the test into two stages. First, firm performance (given 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).[10] This provides a natural and parsimonious way to deal with the large set of potential indices. In terms of the model, we estimate the regression:

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

where ε is the residual from the regression and X represents the indices plus year dummies.[11] Note that the model normalizes beginning of period value to 1. 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

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

where $\widehat{\beta}$, $\widehat{\delta}$, and $\widehat{\gamma}$ are the estimated coefficients from (1). Summary statistics on our empirical estimates of luck and skill are contained in *Table 4B*.

To test the effect of luck versus skill on compensation, we follow the approach of Aggarwal and Samwick's (1999a) work on the pay-for-performance relationship.[12] First, we use changes in

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

[11] 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.

[12] Bertrand and Mullainathan (2001), along with many other compensation studies, also use log-log specifications. As stressed by Baker and Hall (2002), 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 1 (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 1, 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.

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 importantly, 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. Rather, 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 heterogeneity systematically understates the strength of the pay-performance relationship.[13] 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 where 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

$$W = w + aV\varepsilon + bV\lambda + cV\varepsilon \times cdf(Var(V\varepsilon)) + dV\lambda \times cdf(Var(V\lambda)) + gY, \tag{2}$$

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

In the first regressions of *Table 4A*, 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-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 *Table 4A* 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 the 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 (i) total compensation by 96 cents ($= 1.823 - \frac{1}{2} * 1.725$), (ii)

[13] We use dollar variance since we are regressing dollar compensation on dollar stock returns. Thus, our variance measure captures both size and percentage risk effects.

bonus payouts by just under 31 cents, and (iii) new option grants by 42 cents. While this represents substantial pay-for-performance (and significantly understates actual incentives because we omit the ongoing incentives due to previously-granted shares and options), the same executive will gain (i) 74 cents in total compensation, (ii) 18 cents in bonus, and (iii) 35 cents in larger option grants for performance that is due entirely to 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 above 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. Note that the actual break point need not be at zero, and we take this point up in the next subsection.

As indicated in the Introduction, there is an optimal contracting alternative explanation for the asymmetry. The optimal incentive contract based on skill may have a non-linearity 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 may 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 simply involves allowing the sensitivity of pay to luck and skill to differ depending on whether luck, respectively skill, is up or down. Specifically, we add the following interaction terms to our specification in (2):

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

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.[14] 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 rather than 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.

As in the results of *Table 4A*, 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 where 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. It is, however, still possible that they receive their punishment for bad luck in the form of dismissal. We take up this alternative explanation in detail in the next section.

The findings summarized in *Table 4A* 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

[14]Not surprisingly, salary and the other components of compensation show negligible sensitivity to either luck or skill.

median risk receives approximately 79 cents (given by $1.413 - \frac{1}{2} \times 1.243$) in additional compensation for every additional \$1,000 increase in shareholder wealth due to 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, there is no such favorable asymmetry (from the executive's perspective) in the skill component. In some specifications, the manger is punished somewhat *more* for bad skill than she is rewarded for good skill.[15]

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 only valuable to the manager 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 *Figure 1*) sketched at the end of Section 2 suggests that the relationship between b and L is neither globally concave nor convex. Rather, there should be a significant increase in the estimated coefficient on luck (b) as we move 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* due to the unobservability of both the managerial influence costs (I) and the underlying relationship between these influence

[15] This is not to say that the average executive has incentives to reduce risk. We have ingored convexity and pay-performance incentives due to 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).

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 *Table 4B* 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.

We amend (2) by segmenting the contribution of exogenous factors (luck) to the firm's dollar returns ($bV\lambda$) into

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

where

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

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

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

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

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

in some specifications. Given the lack of theoretical basis for the exact breakpoint, we also report results for the less extreme case where 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 where the executive would strictly prefer *no* pay for luck, and 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 that minimize the sum of absolute rather than squared errors as in Aggarwal and Samwick (1999a). Results are qualitatively similar if we use 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 rather than 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 as well. 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 that 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 *Figure 1*, and indicates the empirical relevance of the constraints we have assumed on managerial skimming. First, executives do not seem able 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.[16] Second, executives are not able to exploit the gains of good luck to an increasing extent as luck improves. Rather, pay-for-luck is clearly bounded on the upside by the strength of the pay-for-skill relationship. That is, executives can only reap rewards for good luck to the extent that their pay is also tied to skill.

[16] 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.

4 Alternative Explanations

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

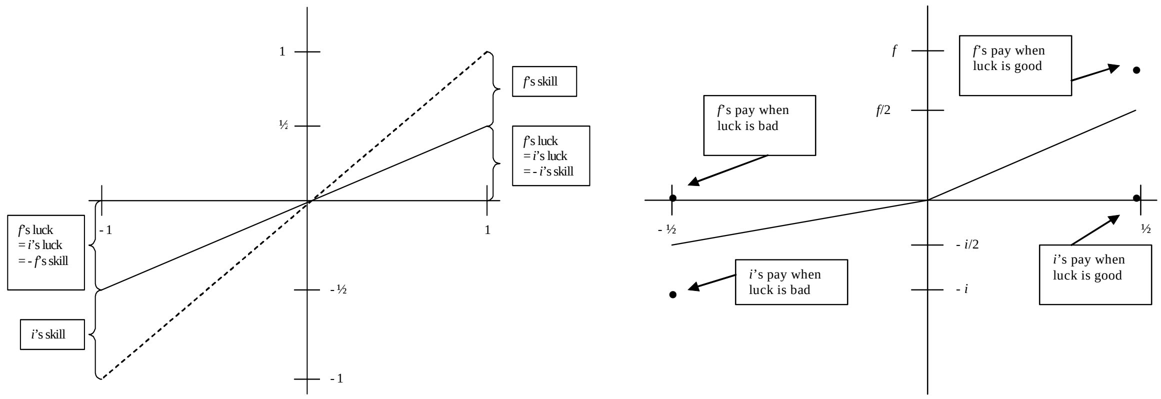
4.1 Performance Non-linearities

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 may be able to adjust quickly to exploit good luck and/or to avoid some of the consequences of bad luck. 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.[17] 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 *Figure 2* depicts the possible outcomes and the consequences of ignoring non-linearity in performance in estimating each firm's luck. When luck is good, the flexible firm has a dollar return of 1 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 $skill = 1/2$ (since it achieved a zero return when luck was bad) and the inflexible firm again having $skill = -1/2$.

[17] 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 which follow.

Figure 2: Asymmetric Performance

Panel A: Realizations of Luck and Skill — Panel B: Estimated Pay-For-Luck



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 1 rather than zero, and the inflexible firm penalizes its CEO an amount $-i$ if its returns are -1 rather than zero. *Panel B* of *Figure 2* depicts the resulting relationship that we will observe between pay and what we estimate as luck.[18] 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 non-linearities can explain our observation of asymmetric benchmarking *if* the flexible firm uses stronger incentives than the inflexible firm ($f > i$). 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 in order to test their contemporaneous pay-performance relationships.[19] Milbourn's (2003) evidence only implies that

[18] Since we control for executive fixed effects, it is realistic to normalize pay to zero.

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

$f > i$ 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 non-linear 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 *Figure 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.

Table 7 presents an additional test of the non-linearity 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 will have 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, where performance now makes no distinction between luck and skill.[20] 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 (and potentially more flexible) CEOs 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 finds 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 $f > i$.

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

4.2 External Labor Market Forces

The skimming approach views compensation as an arena where 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 (2003) 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 as argued earlier, they do not explain the asymmetry between good and bad luck. An up-market may strengthen an executive's outside opportunities and hence her bargaining power, but a down-market should *weaken* such opportunities.

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 non-profit 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" explains 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 reviewed above 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.[21] Suppose that executives are likely to be dismissed when the firm is experiencing bad luck. Since our sample contains only those executives who remain on the job, and compensation cannot be negative, we

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

systematically underestimate the true punishment for bad luck.[22]

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 in fact 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 the 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.

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. Indeed, extreme punishment effectively requires some kind of job change since pay cannot be negative. Since 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 *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.

[22] 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 since any asymmetry induced by the zero minimum would also imply asymmetric pay-for-skill, counter to our results.

5.1 Governance and skimming

Bertrand and Mullainathan (2001) document 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 director size, and the fraction of board members that 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.[23]

Absent readily-available data on the presence of a large shareholder and board composition and size, we turn to the Corporate Governance Index constructed by Gompers, Ishii, and Metrick (2003).[24] This index is based on the prevalence of various corporate governance provisions at each 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 as 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.[25]

To form our testable hypotheses we adapt the logic of Bertrand-Mullainathan's (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 columns I and II 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 rela-

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

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

[25] The governance index spans 2 through 18 in Gompers, Ishii, and Metrick (2003).

tionship in firms for which corporate governance can be characterized as strong ($G \leq 6$), whereas in the second column, we examine the same relationship in firms which corporate governance is weak ($G \geq 12$).[26] Similar to the results of *Table 4*, we see that 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, observe that 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 columns III and IV and *Table 9*, we replicate the analysis of *Table 5* on the strong and weak corporate governance subsamples by including the 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 in fact 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 in fact asymmetrically indexed (see *Table 5*), it appears to be marginally more prominent in situations where the CEO may 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 column V of *Table 9* where 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 first interact this with an indicator variables de-

[26] Observe that 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.

noting whether the firm had strong corporate governance ($G \leq 6$), and then interact this with an indicator of weaker corporate governance ($G \geq 12$). The asymmetric indexation for pay at firms with intermediate quality of corporate governance will be 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 may simply reflect that once firms move away from the "strong" corporate governance set, an executive's ability to influence her pay strategically may 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) documents 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, where 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 will 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, where the hypothesis is that the executive gets a distinct boost when luck is down. But this would merely recast our results in *Table 4A* where we show that the value of new option grants is unaffected by bad luck. Our volatility numbers do not change with good versus bad luck (the correlation between them in our sample is -.0002), luck and skill are by construction uncorrelated, and options are uniformly granted at-the-money. But both exercise price and stock price are on average lower when luck is down. Therefore it must be the case that they get more options granted.

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 *Table 4A* (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.[27] Instead, we control for two-digit SIC effects, but 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-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), but is also significantly different from one, implying that some firms do in fact 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

[27] The reason is that in this case the lagged grant measure would only capture 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 will be followed by a below-average grant.

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, there is if anything a tendency 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 positive 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, they should be applied when the benchmarks 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 may even inadvertently practice asymmetric benchmarking by increasing the number of options granted in down markets while not reducing them in up markets. Clearly, 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.

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Table 1: Descriptive Statistics of CEOs and Firms (1992-2001)

The following 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 represents the Black-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. The standard deviation of stock returns are 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, respectively. 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.

Panel A: Full ExecuComp sample

Variable	Obs	Mean	Min	Median	Max	SD
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 (Black-Scholes)	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
Age of CEO (years)	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 ($millions)	13,737	5,656.4	0.424	1,199.0	507,216.7	19,158.2
Standard deviation of % returns	13,737	.387	0.102	0.343	3.48	0.194

Panel B: Sample used in regression analyses

Firms must have data for all the above measures, plus at least two years of coverage so that we can compute lagged values

Variable	Obs	Mean	Min	Median	Max	SD
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 (Black-Scholes)	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
Age of CEO (years)	6,263	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 ($millions)	6,263	6,341.2	0.424	1,585.4	507,216	19,643
Standard deviation of returns	6,263	0.365	0.102	0.317	3.49	0.191

Table 2: Descriptive Statistics of Performance Benchmarks (1992-2001)

The following data are collected for every firm in which a 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 2-digit SIC code. Summary statistics for returns are in decimal form. The % Positive represents the proportion of the sample for which the relative benchmark return is positive.

Variable	**Obs**	**% Positive**	**Mean**	**Min**	**Median**	**Max**	**SD**
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 CEO and Firm Variables

Pairwise correlations are carried out for each of the following data items as collected for every CEO in the ExecuComp database as defined by the CEOANN field for each year 1992-2001. 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 are computed using the five years of monthly data preceding the data year. Levels of significance levels are given in parentheses below the correlations. * indicates different from zero at the 1% level, ** at the 5% level and *** at the 10% level.

	Value of Option Grant	Total Comp	CEO Age	CEO Tenure	Stock Return	Market Cap of Equity
Value of Option Grant	1					
Total Comp	0.750* (0.00)	1				
CEO Age	−0.440* (0.003)	0.0084 (0.57)	1			
CEO Tenure	−0.258** (0.140)	−0.0041 (0.70)	0.436* (0.00)	1		
Stock Return	0.051* (0.00)	0.0304* (0.003)	−0.035** (0.017)	0.015 (0.17)	1	
Market Cap of Equity	0.354* (0.00)	0.359* (0.00)	0.083* (0.00)	−0.002 (0.76)	−0.042* (0.00)	1
Std. Dev. of Returns	0.122* (0.00)	0.068* (0.00)	−0.267* (0.00)	−0.006 (0.61)	0.055* (0.00)	−0.113* (0.00)

Table 4A: 'Paying for Luck'

Column I of this table contains an OLS regression of changes in total direct CEO 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 II replaces total compensation with bonus and column III uses the Black-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 labelled $\Pr(b = a)$, we provide the p-value from the test that $b - a = 0$. 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. 6,263 observations in each case.

	Total Compensation	Bonus	Value of Option Grant
Luck (λ)	1.405* (0.463)	0.346* (0.0575)	0.703** (0.352)
Skill (ε)	1.823* (0.420)	0.598* (0.0471)	0.842* (0.253)
Luck×cdf Variance of Luck	−1.33* (0.438)	−0.334* (0.0562)	−0.701** (0.357)
Skill×cdf Variance of Skill	−1.725* (0.316)	−0.579* (0.0498)	−0.844* (0.278)
R^2	0.197	0.290	0.188
$\Pr(b = a)$[28]	0.47	0.018	0.67

Table 4B: Summary Statistics on Luck and Skill

This table 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 2-digit SIC code.

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

[28] Evaluated at the median for both variance of luck and skill.

Table 5: Testing for Asymmetry around zero

Column I of this table contains an OLS regression of changes in total direct CEO compensation on the contribution of exogenous factors (luck) on the performance of the firm's dollar returns, the contribution of firm-specific performance, 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 II estimates the same model, but drops the interaction of skill and the dummy variable indicating that skill is negative. Column III estimates the same model as column I, but relies on a median regression. Using the same specification as in column I, column IV replaces total compensation with bonus and column V uses the Black-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 labelled $\Pr(b_D = a_D)$, we provide the p-value from the test that $b_D - a_D = 0$. 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. 6,263 observations in each case.

	Total Comp	Total Comp No Skill Interaction	Total Comp Median Regression	Bonus	Value of Option Grant
Luck (λ)	1.413* (0.461)	1.314* (0.398)	0.598* (0.0753)	0.338* (0.0505)	0.742** (0.372)
Skill (ε)	1.705* (0.312)	1.597* (0.289)	0.804* (0.156)	0.595* (0.0385)	0.823* (0.265)
Luck×Luck is down (b_D)	−0.192* (0.0767)	−0.163* (0.0432)	−0.151* (0.0275)	−0.0249* (0.00533)	−0.121** (0.0601)
Skill×Skill is down (a_D)	0.169* (0.0563)		−0.0323 (0.0281)	0.0184* (0.00459)	0.0701 (0.0273)
Luck×cdf Variance of Luck	−1.243* (0.436)	−1.093* (0.402)	−0.554* (0.0671)	−0.321* (0.0514)	−0.721** (0.356)
Skill×cdf Variance of Skill	−1.442* (0.317)	−1.488* (0.294)	−0.912* (0.153)	−0.599* (0.0392)	−0.810* (0.248)
R^2	0.199	0.201	0.187	0.292	0.189
$\Pr(b_D = a_D)$	0.0004	NA	0.0001	0.0003	0.025
Bad luck removed for median firm[29]	24.6%	21.2%	47.0%	14.1%	31.7%

[29] 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.

Table 6: Piecewise linear regressions

This table contains median regressions of changes in total direct CEO 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 I and II, 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 III, 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 labelled $\Pr(LowLuck = MedLuck)$, 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. 6,263 observations in each case.

	$10^{th}/90^{th}$ Percentile	$10^{th}/90^{th}$ Percentile	$20^{th}/80^{th}$ Percentile
Low Luck ($\lambda_{\{Low\}}$)	0.221* (0.0601)	0.272* (0.065)	0.328* (0.0593)
Medium Luck ($\lambda_{\{Med\}}$)	0.376 * (0.0544)	0.403 * (0.0579)	0.423 * (0.0641)
High Luck ($\lambda_{\{High\}}$)	0.389 * (0.0612)	0.420 * (0.0643)	0.458 ** (0.0586)
Low Skill ($\varepsilon_{\{Low\}}$)		0.866 * (0.0413)	0.864 * (0.0528)
Medium Skill ($\varepsilon_{\{Med\}}$)		0.871 * (0.0518)	0.867 * (0.0598)
High Skill ($\varepsilon_{\{High\}}$)		0.870 * (0.0477)	0.0981* (0.0714)
Luck×cdf Variance of Luck	−0.290** (0.106)	−0.337* (0.0718)	−0.388* (0.0701)
Skill×cdf Variance of Skill	−0.612* (0.0913)	−0.804* (0.0511)	−0.807* (0.0456)
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 Breakpoint	-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 OLS regressions of changes in total compensation on dollar returns (luck plus skill), and measures of each firm-CEO pair's median skill measure. Robust standard erros 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, ** at the 5% level and *** at the 10% level.

	Full sample	High median skill	Low median skill
Dollar return	−2.158* (0.491)	1.607* (0.634)	2.342* (0.758)
Dollar return*cdf risk	−2.178* (0.514)	−1.554* (0.218)	−2.413* (0.788)
Dollar return*median skill	0.0986 (0.0734)		
adj R^2	0.202	0.201	0.224
Observations	6,263	3,132	3,131

Table 8: Luck versus Skill and CEO turnover

This table contains logit 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 I 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 II uses dummy variables taking on the value one if the relevant measure is negative. In the row labelled $\Pr(b = a)$, 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. The model is estimated on 6,809 observations.

	Continuous measures	Dummy for bad luck and skill
Luck	-0.0081 (0.0249)	
Skill	-0.0291^{*} (0.0113)	
Luck is down		-0.126 (0.135)
Skill is down		0.266^{**} (0.115)
Age	0.0210^{**} (0.00875)	0.202^{**} (0.00860)
tenure	-1.749^{*} (0.254)	-1.651^{*} (0.0774)
Log Likelihood	-836	-877
$\Pr(b = a)$	0.31	0.027

Table 9: Differential Asymmetry Based on Governance Index

Columns I-V of this table contain OLS regressions of changes in total direct CEO 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 I and III of this table contain coefficient estimates for the subsample of firms in the bottom quintile ($G \leq 6$) of the Gompers, et al (2003) Corporate Governance Index (Strong Governance). Columns II and IV contain coefficient estimates for the subsample of firms in the top quintile ($G \geq 12$) of the Corporate Governance Index (Weak Governance). Columns III and IV include the interaction of luck with a dummy variable indicating whether luck is negative. Column V contains estimates from the same model for the full sample, but include 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 ($G \geq 12$), respectively. 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.

	Strong Governance $G \leq 6$	Weak Governance $G \geq 12$	Strong Governance $G \leq 6$	Weak Governance $G \geq 12$	Full Sample
Luck (λ)	1.91*** (1.02)	2.45* (0.719)	1.56 (1.03)	2.81* (0.735)	1.28* (0.382)
Skill (ε)	2.77 * (0.765)	1.44* (0.493)	2.83* (0.765)	1.46* (0.492)	1.60* (0.264)
Luck×Luck is down (b_D)			0.312*** (0.165)	−0.364** (0.161)	−0.238* (0.076)
Luck×Luck is down× Strong Governance$_{\{G \leq 6\}}$					0.44* (0.149)
Luck×Luck is down× Weak Governance$_{\{G \geq 12\}}$					−0.021 (0.170)
Luck×cdf Var. of Luck	−1.90*** (1.038)	−2.34* (0.737)	−1.584 (1.051)	−2.67* (0.750)	−1.22* (0.388)
Skill×cdf Var. of Skill	−2.93* (0.795)	−1.30** (0.510)	−3.00* (0.795)	−1.32* (0.510)	−1.55* (0.272)
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-Scholes value of options granted. Lagged option grants is the value of options granted in the previous year. Robust standard erros 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 SIC dummies) are suppressed for convenience. The model is estimated on 6,263 observations.

	Value of options granted	Value of options granted	Value of options granted
Lagged option grant	0.349* (0.0130)	0.353* (0.0201)	0.337* (0.0150)
Laggged option grant $\times$Luck is down (b_D)		0.0411** (0.0208)	0.0439** (0.0201)
Lagged option grant $\times$Skill is down (a_D)		−0.0437*** (0.0225)	
Adjusted R^2	0.337	0.339	0.338
$\Pr(b_D = a_D)$	NA	0.0058	NA