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RELATIVE PERFORMANCE EVALUATION FOR  
CHIEF EXECUTIVE OFFICERS

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ABSTRACT

Measured individual performance often depends on random factors which also affect the performances of other workers in the same firm, industry, or market. In these cases, relative performance evaluation (RPE) can provide incentives while partially insulating workers from the common uncertainty. Basing pay on relative performance, however, generates incentives to sabotage the measured performance of co-workers, to collude with co-workers and shirk, and to apply for jobs with inept co-workers. RPE contracts also are less desirable when the output of co-workers is expensive to measure or in the presence of production externalities, as in the case of team production.

The purpose of this paper is to review the benefits and costs of RPE and to test for the presence of RPE in one occupation where the benefits plausibly exceed the costs: chief executive officers (CEOs). In contrast to previous research, our empirical evidence strongly supports the RPE hypothesis—CEO pay revisions and retention probabilities are positively and significantly related to firm performance, but are negatively and significantly related to industry and market performance, *ceteris paribus*. Our results also suggest that CEO performance is more likely to be evaluated relative to aggregate market movements than relative to industry movements.

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# RELATIVE PERFORMANCE EVALUATION FOR CHIEF EXECUTIVE OFFICERS

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## 1. Introduction

It is common for workers to be rewarded not simply on the basis of their own performance but rather on their performance measured relative to the performances of co-workers. Examples include explicit contests and tournaments, the allocation of a fixed bonus pool at the corporate or divisional level, forced-distribution performance appraisals, and promotions awarded to one worker from a group of subordinates.

Economic theory provides a rationale for relative performance evaluation (RPE) based on risk-sharing. Compensation based solely on observed individual performance provides incentives for a worker to be productive, but measured performance (and therefore compensation) is almost always affected by random factors beyond the worker's control. The random factors which affect one worker's performance often affect the observed performances of other workers in the same division, firm, industry, or market. In these cases, RPE can provide incentives while partially insulating the worker from the common uncertainty.

While RPE offers benefits when there exists an important source of common risk or uncertainty, it also entails costs. Basing pay on relative performance generates incentives to sabotage the measured performance of co-workers (or any other reference group), to collude with co-workers and shirk, and to apply for jobs with inept co-workers. Relative-performance contracts also are less desirable when the output of co-workers is expensive to measure or in the presence of production externalities, as in the case of team production.

One occupation where the risk-sharing advantages of RPE likely exceed its counterproductive side-effects is top-level corporate management. Executive compensation

contracts based only on firm performance subject executives to vagaries of the stock and product markets that are clearly beyond management control. Measuring managerial performance relative to the performance of other firms in the same industry or market partially insulates executives from such industry and market shocks. Relative-performance contracts for chief executive officers (CEOs) are inexpensive to administer, since stock performance of rival firms is available on a daily basis at trivial cost. In addition, CEOs tend to have limited interaction with CEOs in rival firms, thus sabotage and collusive shirking seem unlikely.

The purpose of this paper is to investigate empirically the extent to which CEOs are rewarded on the basis of relative performance. We begin with a review of the benefits and costs of RPE in the workplace, and then apply the analysis to the case of top-level management. We test for the presence of RPE in executive compensation by analyzing a longitudinal sample of over 1,600 CEOs from more than one thousand publicly held corporations. Our results suggest that RPE plays a (possibly implicit) role in the determination of CEO compensation: we find that CEO pay revisions are positively and significantly related to the firm's stock-market return, but are *negatively* and significantly related to industry and market returns, *ceteris paribus*. In addition, our results suggest that RPE also plays a role in the determination of CEO turnover: we find that the probability that a CEO leaves his firm is negatively and significantly related to the firm's return, and positively and significantly related to industry and market returns, *ceteris paribus*. For both compensation and dismissals, our results suggest that CEO performance is more likely to be evaluated relative to aggregate market movements than relative to industry movements.

This paper builds on previous research in agency theory, especially Holmström (1982), who developed the economic theory of relative performance evaluation, and Antle and Smith (1986), who tested for the existence of RPE in executive compensation contracts. We extend Holmström's analysis by discussing the costs as well as the benefits of relative-performance contracts. Our conclusion that CEO compensation depends on relative performance contrasts with the results of Antle and Smith, whose careful examination of a smaller and earlier sample

did not yield significant evidence of RPE. In addition, we extend the Antle-Smith analysis by examining RPE in CEO turnover as well as in CEO compensation.<sup>1</sup>

## 2. The Theory of Relative Performance Evaluation

### *A Simple Model of the Benefits of RPE*

This section presents an informal review of the economic theory of relative performance evaluation. The analysis in this section applies to all employees in a workplace; we defer our application of the theory to top-level management until the next section.

Suppose that output,  $y$ , depends on the worker's effort,  $a$ , but is stochastic. For example,

$$y = a + \varepsilon,$$

where  $\varepsilon$  is a noise term with zero mean. Because the worker's effort is unobservable, the employer and worker cannot sign a contract specifying that the employee supply a particular level of effort. Instead, the employer offers the worker a compensation contract,  $w(y)$ , that determines the worker's wage as a function of the realized output of the production process. The worker then chooses whether to sign the contract or to take up alternative employment yielding utility  $U^0$ . After signing the contract, the worker chooses a level of effort and production occurs.

Suppose that the employer is risk-neutral: he cares only about output net of compensation,  $y-w$ . The worker, on the other hand, dislikes effort and is averse to compensation risk. Under these assumptions, full efficiency (the "first-best") is defined by two conditions: (i) the worker must supply the productively efficient level of effort (*i.e.*, the level of effort at which the expected value of the output resulting from an additional unit of effort equals

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<sup>1</sup> Our debt to other related research must also be acknowledged, and will be discussed more thoroughly below. Early theoretical research related to RPE includes Holmström (1979), Baiman and Demski (1980), and Lazear and Rosen (1981). Some of the costs associated with RPE are discussed by Dye (1984, 1988), Mookherjee (1984), Lazear (1987), and Wolfson (1985). Relative performance in executive compensation contracts is addressed briefly by Murphy (1985) and Jensen and Murphy (1989). The relation between CEO turnover and relative performance is directly or indirectly analyzed by Coughlan and Schmidt (1985), Morck, Shleifer, and Vishny (1988), Weisbach (1988), and Warner, Watts, and Wruck (1988).

the disutility to the worker from this additional unit of effort); and (ii) the worker must be perfectly insured against compensation risk.

If the worker were risk-neutral, insurance would be unnecessary and the first-best could be achieved via a contract  $w(y) = y - F$  that, in effect, sells the worker the firm: the worker receives the entire output,  $y$ , as compensation, but pays the employer an up-front fee,  $F$ , so that the worker's expected utility just equals his reservation utility.<sup>2</sup> When workers are risk-averse, however, the optimal contract is necessarily "second-best," both because the worker bears income risk and because the worker's effort choice is smaller than is productively efficient. The optimal contract for a risk-averse worker reflects a trade-off between the goals of insuring the worker and providing incentives for efficient effort choice.

The fact that the best possible contract of the form  $w(y)$  induces a risk-averse worker to supply less than the productively efficient amount of effort suggests that employers may be able to improve the contract by making pay contingent on a second variable,  $z$ , in addition to being contingent on output,  $y$ . Holmström (1979) shows that the optimal contract has the form  $w(y,z)$  whenever  $z$  contains incremental information valuable in assessing the worker's unobservable effort.<sup>3</sup> Intuitively, the role of  $z$  in the contract  $w(y,z)$  is either to provide more insurance without reducing incentives for effort or to provide greater incentives without increasing risk exposure.

As a concrete example, suppose that the worker's output depends not only on effort and idiosyncratic noise,  $\varepsilon$ , but also on an additional noise term,  $\theta$ , where  $\theta$  is a common shock experienced by some set of workers in the same division, firm, industry, or market:

$$y = a + \varepsilon + \theta.$$

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<sup>2</sup> That is, the entry fee  $F$  is set at  $F = E(y) - U^0$  so that the worker expects to receive his reservation utility. This contract is similar to typical franchise agreements in which the franchisee pays a fixed entry fee for purchase of the franchise and receives all profits (after payment of an annual fee to the franchisor that commonly amounts to between five and ten percent of revenues).

<sup>3</sup> More precisely, the optimal contract will depend on  $z$  if  $y$  is not a sufficient statistic for the pair  $\{y,z\}$  with respect to the worker's effort.

The shock  $\theta$  could reflect seasonal or random variations in industry demand, market-wide stock movements, or the fixed-effect of a single supervisor evaluating each member of a group of workers, among other possibilities. If  $\theta$  is perfectly observable, contracts of the form  $w(y-\theta)$  clearly improve on contracts of the form  $w(y)$ . Intuitively, the presence of  $\theta$  makes  $y$  a noisier signal about effort than it would be in the absence of  $\theta$ . Therefore, the employer can reduce the noise in his evaluation of the worker's effort by basing contracts on  $y-\theta$  rather than on  $y$  alone. If the common shock  $\theta$  is not perfectly observable, however, the employer still can improve on contracts of the form  $w(y)$  by incorporating an estimate of  $\theta$ , denoted by  $\hat{\theta}$ , into the contract. Since  $\hat{\theta}$  measures the common shock  $\theta$  with error, optimal contracts incorporating the estimate  $\hat{\theta}$  will generally be of the form  $w(y, \hat{\theta})$  rather than of the simpler form  $w(y-\hat{\theta})$ , and the common risk will be only *partially* filtered out of the worker's compensation.

To see how  $\hat{\theta}$  might be computed, consider the following elaboration of the example above.<sup>4</sup> Suppose that there are  $n$  identical workers whose outputs are given by  $y_i = a_i + \varepsilon_i + \theta$ , for  $i=1, \dots, n$ , where  $a_i$  and  $\varepsilon_i$  are the  $i^{\text{th}}$  worker's effort and idiosyncratic noise, respectively. Then, assuming that the  $\varepsilon_i$ 's are independently and identically distributed with zero mean, an estimate of the common shock is  $\hat{\theta} = \bar{y} - \bar{a}$ , where  $\bar{y}$  is the average output of all other workers subject to the common shock  $\theta$ , and  $\bar{a}$  is the average anticipated effort level of these  $n-1$  other workers conditional on their compensation packages.<sup>5</sup> Under the assumption that the  $\varepsilon_i$ 's are Normally distributed, Holmström (1982) shows that the optimal contract for the  $i^{\text{th}}$  worker is  $w_i(y_i, \bar{y})$ .<sup>6</sup> Typically, the wage paid to the  $i^{\text{th}}$  worker will be positively related to his own output but negatively related to the average output of all other workers subject to the same common

<sup>4</sup> Alternative methods of calculating  $\hat{\theta}$  applicable to special situations include seasonal adjustments and prorating output to adjust for unanticipated plant closings.

<sup>5</sup> The estimate in the text,  $\hat{\theta} = \bar{y} - \bar{a}$ , is the OLS estimate of  $\theta$  given the available information about the  $n-1$  other workers. A more efficient OLS estimate of  $\theta$  would also include the information available about the  $i^{\text{th}}$  worker. If the joint distribution of the idiosyncratic noise terms is known, one can also compute the maximum likelihood estimate of  $\theta$ . In all of this, one can relax the assumption that the idiosyncratic noise terms are independently and identically distributed.

<sup>6</sup> Note that  $y_i$  plays two roles in this contract. First, it provides incentives in the usual way. Second, it improves the estimate of  $\theta$  as described in the previous footnote.

shock. Holmström also shows that as the number of workers becomes very large (so that  $\hat{\theta}$  is a very precise estimate of  $\theta$ ), the optimal contract approaches the form  $w_i(y_i - \bar{y})$ , where  $y_i - \bar{y}$  is the performance of the  $i^{\text{th}}$  worker measured relative to the performance of others in the reference group.

Relative performance evaluation may be beneficial even in the absence of agency problems between employees and employers. Alternative models based on learning about ability rather than on incentives, for example, also predict RPE when workers in a group are exposed to a common shock. Formally, let individual output,  $y_i$ , depend on ability and not effort:

$$y_i = \eta_i + \varepsilon_i + \theta,$$

where  $\eta_i$  is the  $i^{\text{th}}$  worker's ability,  $\varepsilon_i$  is idiosyncratic noise, and  $\theta$  is the unobservable common shock. Suppose that ability is not known precisely; rather,  $\eta_i$  is believed to be Normally distributed with mean zero and variance  $\sigma_\eta^2$ . (Allowing for a non-zero mean is straightforward but uninformative.) Suppose further that  $\theta$  and  $\varepsilon_i$  are Normally distributed with mean zero and variances  $\sigma_\theta^2$  and  $\sigma_\varepsilon^2$ , respectively, and that the common shock and the  $n$  workers' individual abilities and noise terms are independent. Given these assumptions it is straightforward to show (see, for example, Theorem 9.11 of Mood and Graybill, 1963) that the expected ability of the  $i^{\text{th}}$  worker, given his output and the outputs of the  $n-1$  co-workers subject to the shock  $\theta$ , is:

$$E[\eta_i | y_1, \dots, y_n] = \frac{\sigma_\eta^2}{\sigma_\eta^2 + \sigma_\varepsilon^2} \left( y_i - \frac{\sigma_\theta^2}{\sigma_\eta^2 + \sigma_\varepsilon^2 + n\sigma_\theta^2} \sum_{j=1}^n y_j \right).$$

Suppose that individual outputs are observable and that wages are a function of expected ability. Then the wage offered to the  $i^{\text{th}}$  worker will typically be positively related to his own output but negatively related to the average output of all other workers subject to the same common shock.

In both the incentive model and the learning model, RPE provides a way to filter common risk from workers' compensation contracts. The qualitative predictions of (and underlying



economic rationales behind) both models are similar enough that we make no attempt to distinguish between these complementary theories of RPE.

### *Disadvantages of Rewarding Workers based on Relative Performance*

The benefits of RPE are straightforward—the risk exposure of risk-averse workers can be reduced by filtering out some or all of the common risk. This rationale for basing rewards on relative performance rather than on absolute performance accords with the general rule-of-thumb that workers should not be rewarded or punished for "things beyond their control." Before embracing the theory completely, however, we must also consider the costs of compensation systems based on relative performance.

An obvious cost of RPE is the cost of observing the average output of the workers subject to the same common shock. If the cost of measuring  $\bar{y}$  is a fixed cost,  $C$ , it affects whether or not a firm uses RPE at all but does not affect the importance of relative performance in compensation contracts that use RPE: firms will use relative performance if  $C$  is less than the increased value of the best contract  $w_i(y_i, \bar{y})$  relative to the best contract  $w_i(y_i)$ . Alternatively, the size of the reference group may be a choice variable for employers—employers will sample additional workers from the pool of workers subject to the common shock as long as the incremental gain from improved contracts (associated with more precise estimates of  $\theta$ ) exceeds the cost of measuring an additional worker's output.

There is another set of costs associated with RPE that may be much more important than the simple measurement costs described above. *Paying workers based on relative performance,  $y_i - \bar{y}$ , instead of on absolute performance,  $y_i$ , distorts the worker's incentives whenever the worker can take actions that affect the average output of the reference group,  $\bar{y}$ .* There are (at least) four ways that a worker's action can have this kind of effect: (1) *sabotage* (in which the worker takes unproductive actions to diminish the measured performance of another worker in the reference group), (2) *collusion* (in which workers in the same reference group collude and shirk), (3) *choice of reference group* (in which a worker chooses which of several groups of

other workers to which to be compared) and (4) *production externalities* (in which the productive actions of one worker affect the outputs of other workers).

Sabotage ranges from relatively subtle forms—such as concealing relevant information from, or spreading false gossip about, a fellow employee—to outright theft and/or destruction of co-worker output.<sup>7</sup> Lazear (1987) argues that the sabotage-related problems of RPE may be a driving force in the pay equality observed in many organizations. According to Lazear, reducing pay differences based on relative performance reduces uncooperative behavior that is detrimental to the firm. Withholding valuable comments on a colleague's paper or deleting data from a colleague's hard disk are examples of potential sabotage in academia. In addition, although implicit and explicit workplace sabotage is not a new phenomenon, the recent proliferation of computer networks and shared databases may have increased the it's scope.<sup>8</sup>

Rewards based on relative performance also generate incentives for workers to collude and supply less effort than would be supplied in the absence of collusion (see Mookherjee, 1984 and Dye, 1984). In academia, for example, colleagues could agree not to work at night, on weekends, or during summers. The general conditions for cartel stability in the product market (Stigler, 1964) apply to a workplace cartel as well—collusion is more likely when the number of potential colluders is small, for example, and collusion is more effective when workers can observe the output and compensation of co-workers. It seems plausible that collusion in the workplace is more likely to be stable than collusion in the product market will be, both because workplace collusion is not an antitrust violation and because it may be easier to monitor co-located co-workers than to monitor the costs and outputs of geographically dispersed rival firms.

Relative performance evaluation also distorts incentives when agents can influence the determination of which of several possible reference groups is used in the evaluation process.

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<sup>7</sup> Note that sabotage can be effective even if it diminishes one's own output provided that it has a larger negative effect on the average output of other workers.

<sup>8</sup> *PC Magazine* editor John Dvorak (9/27/88) explains how a friend received his promotion: "He managed to crack the network messaging system so that he could monitor all the memos. He also sabotaged the workgroup software and set back the careers of a few computer-naive souls who didn't realize that someone was manipulating their appointment calendars. They would miss important meetings and be sent on wild-goose chases, only to look like complete buffoons when they showed up for appointments that were never made." Dvorak argues that stealing passwords and destroying important data are a trivial task for computer-literate employees.

Workers are often involved in interviewing, selecting, and recruiting potential colleagues. Workers paid on the basis of relative performance clearly have an incentive *not* to hire rookies of superior ability and will instead desire incompetent co-workers.<sup>9</sup> The same rationale also applies to job search—RPE generates incentives for workers to self-select into firms with inferior co-workers. Dye (1988) argues that paying top executives based on industry-relative performance yields incentives to locate in industries with inept rivals, so that the manager appears to be both talented and hard-working relative to other managers in the (carefully selected) reference group. Similarly, RPE may adversely affect an executive's decision to leave a declining industry, and may cause a mediocre performer to leave a rapidly growing industry even if staying in the industry maximizes the value of the organization.

Finally, RPE will be less desirable when the effort levels of individual workers are complementary factors in the firm's production process, as in team production, for example. Workers paid on the basis of relative performance will not have incentives to develop innovations that uniformly increase the productivity of all other workers in the reference group. Optimal contracts in the presence of such production externalities typically involve rewarding individuals for group output,  $\partial w_i(y_i, \bar{y}) / \partial \bar{y} > 0$ , instead of rewarding individual performance measured relative to the performance of others,  $\partial w_i(y_i, \bar{y}) / \partial \bar{y} < 0$ .

The costs and benefits of RPE suggest that filtering out common risk will be more effective in some situations than in others. Incentives to sabotage or collude with other agents subject to the same common shock are more important when the number of co-workers is small, and RPE will therefore be less effective for small groups than for large groups, *ceteris paribus*. Also, RPE will be more effective when the worker is unable to influence the choice of reference group, and will be less effective when there are externalities in the production process (*e.g.*, team production), or when the output of co-workers is expensive to measure.

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<sup>9</sup> Indeed, a common justification for tenure in academia is that non-tenured faculty have incentives to recruit less-able colleagues (Carmichael, 1988).

### 3. Rewarding Executives on Relative Performance

The relationship between the shareholders of a publicly owned corporation and the corporation's top manager is an archetypal example of a principal-agent relationship. Widely diffuse shareholders, assumed to be well-diversified and therefore risk-neutral, want the manager to take actions that increase shareholder wealth. The imperfectly monitorable manager, however, takes actions that increase his expected utility. Managerial contracts tying pay and dismissal decisions to changes in shareholder wealth help align the interests of shareholders and the manager by giving the manager incentives to take actions that increase shareholder wealth.

While the change in shareholder wealth is an appropriate measure of managerial output or firm performance, it is an imperfect measure of managerial effort since many factors affect the value of a firm besides the actions taken by managers.<sup>10</sup> Executives facing contracts based only on changes in shareholder wealth are subject to vagaries of the stock and product markets which are clearly beyond management control. The theory of relative performance evaluation suggests that optimal managerial contracts may base compensation and dismissal decisions on performance relative to other firms in the industry or market in order to filter common risk from the compensation of risk-averse managers.

Top-executive compensation and dismissal policy seems a likely place to observe RPE. First, it seems plausible that industry or market shocks (such as the October 1987 stock-market crash) are important and that filtering out these shocks can benefit risk-averse executives. Second, although there are of course important problems related to defining an industry, measuring the stock-market performance of other firms in a defined industry is easily accomplished on a daily basis at trivial cost. Third, CEOs tend to have limited interaction with CEOs in rival firms, thus the potential for sabotage, collusive shirking, and production externalities is probably small. Another source of distorted incentives, the choice of a reference group, may be important, however (to the extent that the CEO plays a key role in choosing the

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<sup>10</sup> "Managerial effort" in this context refers not to how *hard* an executive works but rather to whether or not he takes actions and/or makes decisions that increase shareholder wealth.

industry in which his firm competes): a manager may be the best CEO in a declining industry, for example, but under RPE he will not have the correct incentives to leave the industry. Finally, there may be production externalities across firms: for example, RPE makes oligopolistic collusion more difficult to sustain.

Another way to filter risks associated with industry and market shocks is to pay (or dismiss) executives in a given firm on the basis of their performance measured relative to the performance of other executives *in the same firm*. Lazear and Rosen (1981), for example, argue that top-management promotions are a form of RPE in which the vice president with the highest measured output is promoted to CEO. It is precisely this type of situation, however, where the costs of RPE may be large. First, there are typically only a small number of vice presidents with a non-trivial chance of winning the promotion tournament—therefore the opportunities for sabotaging or colluding with rival vice presidents seems large. Also, CEOs typically play an important role in selecting the team of vice presidents as well as their ultimate successor (Vancil, 1987), so a CEO paid on the basis of his performance measured relative to the performance of subordinates may believe he can hold his position longer if he chooses incompetent vice presidents. Finally, firm output (in this case changes in shareholder wealth) is presumably jointly produced by the top management team—therefore RPE may interfere with the shareholder's objective of maximizing team production.

#### 4. Relative Performance and CEO Compensation

We test the importance of RPE in executive compensation and dismissal decisions by following all chief executive officers listed in the Executive Compensation Surveys published in *Forbes* from 1974-1986. These surveys include 2,214 executives serving in 1,295 corporations, or a total of 10,400 CEO-years of data. We focus on the CEO's *compensation* and *dismissal* rather than on his total incentives (which are created not only by compensation and threat of dismissal but also by changes in the value of his outstanding stock options and of his inside stockholdings) because changes in the value of previous holdings of stock and stock options are

determined entirely by firm performance, independent of relative performance. Thus, if relative performance is important, its importance must arise in compensation and dismissal policy. We examine RPE in executive compensation contracts in this section; dismissals based on relative performance are examined in the next section.

We measure firm performance as the change in the logarithm of shareholder wealth, which equals the continuously accrued rate of return received by the firm's shareholders, including price appreciation and dividends. Reference-group stock-market performance is measured using the population of 11,000 firms listed on the Compustat expanded industrial, full coverage, and historical research files. Several potential reference groups are analyzed—the entire Compustat population ("the market") and firms in the same one-, two-, three-, and four-digit SIC industries as the sample firm. Industry return is defined as the continuously accrued rate of return realized on a value-weighted portfolio of all Compustat firms (excluding the sample firm) in the same SIC industry; market return is the continuously accrued return on the value-weighted portfolio of all Compustat firms (excluding the sample firm).

Stock returns for New York Stock Exchange (NYSE) firms are calculated using fiscal-year stock-market data available from the Center for Research in Security Prices (CRSP). Fiscal-year industry classifications for NYSE and American Stock Exchange firms are also obtained from CRSP. Compustat industry classifications and stock data are used for firms included in Compustat files but not on CRSP files.<sup>11</sup> After matching these performance variables with the *Forbes* compensation sample and eliminating observations with missing data, our final sample contains 9,425 observations and includes 1,668 executives from 1,049 corporations.<sup>12</sup> All rates

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<sup>11</sup> Compustat industry designations are only appropriate for the current or final year (generally 1987), while CRSP data allow the firm's industry to change from year to year. Compustat stock returns are based on calendar-year and not fiscal-year data, but we replicated our analysis (including the construction of industry returns) for the subset of firms with December fiscal closings and our results are qualitatively unchanged.

<sup>12</sup> The regressions are based on approximately 1,668 fewer observations, since we lose the first observation for each CEO by taking first differences. *Forbes* compensation data published in year *t* are matched to Compustat data for fiscal year *t*-1 for firms with fiscal closings June through January, and are matched to fiscal-year *t*-2 Compustat data for firms with fiscal closings in April and May. February and March closings sometimes correspond to *t*-1 or *t*-2 Compustat data and it is generally necessary to check proxy statements to match these data.

of return and monetary variables are adjusted for inflation (using the consumer price index for the closing month of the fiscal year) to represent 1986-constant dollars.

Agency theory predicts that compensation will depend on absolute or relative performance but gives little guidance as to the form of the contract. We estimate the following relation between pay and firm, industry, and market performance:

$$(4.1) \quad \Delta \ln(\text{CEO Pay}) = \alpha + \beta(\text{Shareholder Return}) + \gamma(\text{Industry and/or Market Return}).$$

Rewarding CEOs for taking actions that increase shareholder wealth implies that  $\beta > 0$ . RPE implies that, holding the firm's rate of return constant, CEO pay revisions should be negatively related to the industry and market return,  $\gamma < 0$ .

Table 1 reports estimated coefficients from regressions relating the growth rate of CEO salary and bonus to the rates of return realized by shareholders in the firm, two-digit SIC industry, and market. Column (1) shows that CEO salaries and bonuses are positively and significantly related to firm performance as measured by the rate of return on common stock. The return coefficient of .1562 ( $t=19.5$ ) implies, for example, that CEO pay changes by about 1.6% for each 10% return on the common stock.<sup>13</sup>

The estimated coefficient for industry rate of return in column (2) of Table 1 is negative and statistically significant, suggesting that boards of directors make some adjustment for industry trends when determining executive compensation. The coefficients in column (2) suggest, for example, that a CEO receives an average pay increase of 5.5% in years when shareholder return is zero in both the firm and the industry. The same CEO receives an average pay increase of 9.1% in years when shareholder return is 20% and industry return is zero, but receives an increase of only 7.6% in years when *all* firms in the industry realize 20% rates of

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<sup>13</sup> The estimated return coefficient in column (1) is highly significant, but its economic magnitude is small. The sample medians for CEO pay and firm size (measured by market value) are \$465,000 and \$812,000,000, respectively. Thus, a 10% return in a median-size firm is \$81,200,000 and corresponds to a pay raise for the median-pay CEO of \$7,300, or less than nine cents for each \$1,000 of increased shareholder wealth. See Jensen and Murphy (1989) for further discussion on the magnitude of the empirical relation between pay and performance.

return.<sup>14</sup> This result is consistent with the hypothesis that uncertainties shared with other firms are partially filtered out of the CEO's compensation contract.<sup>15</sup>

Column (3) of Table 1 reports coefficients from regressions of CEO pay growth on shareholder return and the rate of return in the market. The negative and significant market-return coefficient suggests that market risks are also partially filtered out of executive compensation. The explanatory power of column (3) exceeds that of column (2), suggesting that CEO compensation contracts are more closely linked to market-relative returns than to two-digit industry-relative returns. The regression in column (4) includes both industry and market returns. The market-return coefficient is negative and statistically significant, indicating that CEOs are rewarded on the basis of market-relative performance and not only on absolute performance. The industry-return coefficient is no longer significant after controlling for market-wide stock-price movements, although the point estimate is still negative.<sup>16</sup>

The theory of RPE suggests that common uncertainties will be filtered out of executive compensation, but is of course silent on whether industry or market uncertainties are the most important source of common risk. The results in column (4) of Table 1 suggest that CEO performance is more likely to be evaluated relative to aggregate market movements than relative to industry movements, where the industry is defined as all other firms in the same two-digit SIC industry. We explore the robustness of this conclusion by re-estimating columns (2) and (4) of Table 1 for three alternative industry definitions: one-digit, three-digit, and four-digit SIC industries. Results from these regressions are presented in Table 2.

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<sup>14</sup> We re-estimate the regressions in Table 1 after including interaction variables that allow the coefficients on return and industry return to vary linearly with firm size. We find that the firm-return coefficient is not significantly related to firm size. The industry coefficient, however, is statistically significantly closer to zero for larger firms, but remains negative for all but the largest one percent of sample firms.

<sup>15</sup> In keeping with the discussion in the preceding footnote, the economic magnitude of the industry adjustment implied by column (2) is not large. In particular, a median-paid CEO in a median-sized firm receives ten cents for each \$1,000 of increased shareholder wealth when his firm's return is 20% and the industry return is zero, but receives only six cents per \$1,000 when both his firm and the industry realize returns of 20%.

<sup>16</sup> The market return in columns (3) and (4) is defined as the value-weighted annual return for all Compustat firms in the fiscal year; thus we have combined data for firms with different fiscal-closing months. We re-estimate columns (3) and (4) after defining market returns as the value-weighted return for all NYSE firms in the 12 months preceding each firm's fiscal closing. The two-digit industry return remains insignificant in the re-estimated version of column (4), although its coefficient and t-statistic increase to  $\gamma = -.0276$  and  $t = -1.5$ , respectively.



Columns (1), (2), and (3) of Table 2 show that CEO pay growth is positively related to shareholder return and negatively related to the value-weighted returns for other firms in the same one-digit, three-digit, and four-digit SIC industry. The results suggests that CEO pay is more closely linked to broad industry definitions than to narrow industry definitions. In fact, interpreting the market return as our broadest industry definition, the regressions in Tables 1 and 2 suggest that the magnitude and significance of the industry coefficient, and the explanatory power of the regression, decline monotonically as the industry becomes more narrowly defined.

The regressions in columns (4), (5), and (6) of Table 2 include market return as an additional explanatory variable and replicate the regression in column (4) of Table 1 for the three alternative industry definitions. The market-return coefficient is negative and significant in all three regressions, indicating that market risks are partially filtered out of executive compensation after controlling for industry returns. Holding market returns constant, however, CEO pay growth is negatively and marginally significantly related to one-digit industry returns, but is not significantly related to three-digit or four-digit industry returns. These results support the finding that CEO performance is more likely to be evaluated relative to aggregate market movements than relative to industry movements. An alternative interpretation of these results, however, is that firms are rarely engaged in a single industry, in which case our inability to detect an industry effect after controlling for market movements could simply reflect the fact that industry definitions based on a single SIC code for each firm are inappropriate for purposes of relative performance evaluation.

The regressions in Table 1 and Table 2 indicate that CEO salaries and bonuses are partially adjusted for industry- and market-wide stock price movements. This evidence is consistent with the RPE hypothesis.<sup>17</sup> These results are based only on salaries and bonuses,

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<sup>17</sup> The evidence also is consistent with a subtly different hypothesis: CEOs are rewarded for shareholder return in excess of the market return *because this is the true objective of the shareholders*. This latter hypothesis is incorrect, because it confuses the shareholders' *ex ante* and *ex post* objectives. To understand the distinction between the shareholders' *ex ante* and *ex post* objectives, consider the following two-stage model of investment and production. During the investment stage of the model there are two kinds of agents, entrepreneurs and investors. Entrepreneurs have ideas but no capital; investors have capital but no ideas. If an entrepreneur receives capital from (at least) one investor, then the entrepreneur becomes a CEO, the investor becomes a shareholder, and the production stage of the model begins. The production stage is identical to the model from Section 2 in which  $y_i =$

however, while CEOs in fact receive compensation in many additional forms—including stock options, profit-sharing arrangements, stock grants, savings plans, long-term performance plans, and other fringe benefits. These other forms of compensation may also be adjusted for industry and market movements, and it is therefore useful to analyze the CEO's *total* compensation and not only its cash component.

The annual *Forbes* surveys include data on total compensation as well as on salary and bonus. Unfortunately, although the salary and bonus data are easily verifiable and their definitions have remained constant throughout the sample period, the *Forbes* data for other components of compensation are neither easily verifiable nor reliable. The most severe problems relate to the largest non-cash component of compensation—stock options. *Forbes* surveys do not include any stock-option data prior to 1978, and after 1978 the surveys report *gains from exercising* stock options but do not report stock options *granted* during the year. The gains from exercising options are typically associated with options granted five to ten years previously, and therefore using these data imposes serious timing problems.

Columns (1) and (2) of Table 3 report regressions based on the *Forbes* total-compensation data, excluding both stock-option grants and the gains from exercising stock options. The definition of total compensation varies somewhat from year to year, but typically includes salary, bonus, value of restricted stock, savings and thrift plans, and other benefits. The one-digit industry coefficient in column (1) is negative and significant, indicating that the CEO's total compensation (exclusive of stock options) is based on industry-relative as well as absolute firm performance.<sup>18</sup> The regression in column (2) suggests that market returns are a

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$a_i + \varepsilon_i + \theta$ , where  $y_i$  is now the change in shareholder wealth (gross of wage payments) at the  $i^{\text{th}}$  firm,  $\varepsilon_i$  is idiosyncratic (i.e., completely firm-specific) noise, and  $\theta$  is a market-wide shock. (Note that this model does not allow the CEO's action to affect either the variance of the firm's returns or its covariance with the market.)

According to the capital-asset pricing model, *ex ante* (i.e., at the investment stage) investors care about the covariance between the firm's return and the market (among other things). But *ex post* (i.e., at the production stage), shareholders at the  $i^{\text{th}}$  firm face precisely the contracting problem described in Section 2: they have invested their capital and now want to make the most of it. Thus, in this model, the covariance between the firm's return and the market is relevant *ex ante* (because it helps determine which entrepreneurs will receive capital) but it is not relevant *ex post*.

<sup>18</sup> We also estimate the regressions in columns (1) and (2) of Table 3 using two-digit industry definitions and obtain qualitatively identical results.

more important determinant of total compensation than are industry returns, although the industry coefficient is negative and statistically significant. The results in columns (1) and (2) of Table 3 are qualitatively similar to the results in columns (1) and (4) of Table 2, suggesting that our earlier conclusions are not changed by analyzing broader measures of compensation.

The total-pay variable used in columns (1) and (2) excludes stock options, but a better measure would include the value of stock options granted during the year. Although these data are not available for the *Forbes* sample, they are available for a longitudinal sample of 73 manufacturing firms first analyzed by Murphy (1985). This sample, constructed from proxy statements, includes data on salaries, bonuses, deferred compensation, stock options, and fringe benefits, and covers 173 CEOs from 1966 through 1983. Total compensation is defined as the sum of salaries, bonuses, fringe benefits, stock options, the face value of deferred compensation, and restricted stock awarded during the year (valued at the year-end stock price). Stock options are valued on the date of grant using a Black-Scholes (1973) valuation formula which allows for continuously-paid dividends.<sup>19</sup>

Columns (3) through (6) in Table 3 report coefficients from relative-performance regressions based on the 73-firm sample of manufacturing firms. As a robustness check on our earlier results, we re-estimated the one-digit industry salary-and-bonus regressions reported in Table 2 for the manufacturing-firm sample. Column (3) in Table 3 shows that the growth rate of CEO cash compensation is positively related to firm performance and negatively related to industry performance, *ceteris paribus*. Neither the industry- nor market-return variables are individually statistically significant in the regression in column (4), although they are *jointly* significant at the .01% level. These results are qualitatively similar to those in columns (1) and

<sup>19</sup> In particular, the value of options granted during fiscal year  $\tau$  is calculated as,

$$V = N_t \{ S_\tau e^{-dT} \Phi(Z) - P_t e^{-rT} \Phi(Z - \sigma\sqrt{T}) \},$$

where  $N_t$  is the number of options granted in year  $t$  at exercise price  $P_t$ ,  $T$  is the number of months until expiration of these options,  $r$  is the average monthly market yield on five-year Government Securities in year  $\tau$ ,  $d$  is the dividend yield in year  $\tau-1$  defined as  $\ln(1+(\text{Dividends-per-share})/(\text{Closing Stock Price}))/12$ ,  $\sigma$  is the estimated standard deviation of monthly stock returns over the previous sixty-month period,  $S_\tau$  is the stock price on the day of grant (generally equal to  $P_t$ ),  $Z = (\ln(P_t/S_\tau) + (r-d+\sigma^2/2)T)/\sigma\sqrt{T}$ , and  $\Phi(Z)$  is the cumulative standard normal distribution function. See Noreen and Wolfson (1981) and Murphy (1985).

(4) of Table 2, indicating that our earlier results are not specific to the particular sample analyzed there.

The industry-return coefficient in column (5) of Table 3 is negative and significant, indicating that the CEO's total compensation (including stock option grants) is based on industry-relative as well as absolute firm performance. Column (6) includes market returns as an additional explanatory variable, and shows that CEO pay growth is negatively and significantly related to one-digit industry returns, but positively (and insignificantly) related to market returns. Thus, our earlier finding that CEO performance is more likely to be evaluated relative to aggregate market movements than relative to industry movements is not robust across samples for broader definitions of compensation and industry performance.<sup>20</sup>

As an additional robustness check, we use the *Forbes* sample of 1,668 CEOs to estimate the levels equation analogous to the first-difference specification reported in Table 2, namely:

$$(4.2) \quad \ln(\text{CEO Pay})_{it} = \lambda_i + \alpha(\text{Age})_{it} + \beta \sum_{\tau=1}^t (\text{Shareholder Return})_{i\tau} + \gamma \sum_{\tau=1}^t (1\text{-Digit Industry Return})_{i\tau},$$

where  $\lambda_i$  is a separate intercept or "fixed-effect" for the  $i^{\text{th}}$  CEO, and the first difference of (4.2) is (4.1). Our results are reported in Table 4. To ease comparison, column (1) of Table 4 repeats the regression reported in column (1) of Table 2. Column (2) of Table 4 reports estimated coefficients from the fixed-effects levels equation, (4.2).<sup>21</sup> If the levels regression is correctly specified, the time-trend coefficient in column (2) should equal the intercept in column (1), and the coefficients for the cumulative firm and industry stock returns in column (2) should equal the coefficients for shareholder return and industry return in column (1), respectively. In fact, the cumulative-stock-return coefficient in column (2) of Table 4 is very close to the stock-return coefficient in column (1), but the cumulative-industry-return coefficient in column (2) is insignificant while the corresponding coefficient in column (1) is significantly negative. This

<sup>20</sup> Our inability to replicate our earlier finding may reflect that market performance is highly correlated with one-digit industry performance for manufacturing firms. We re-estimate the regressions in columns (3) through (6) of Table 3 using two-digit industry definitions and find that market movements are a more important determinant of CEO pay changes than are two-digit industry movements.

<sup>21</sup> The coefficients are estimated using deviations from individual means, and the standard errors are adjusted to reflect the correct degrees of freedom.

difference in the industry-return coefficient in columns (1) and (2) suggests the possibility that the model is misspecified.

The first-difference and fixed-effects specifications in columns (1) and (2) of Table 4 control for observable and unobservable individual characteristics that are fixed for a CEO over time, but do not allow for economy-wide shocks that vary from year to year. We allow for these time-effects in columns (3) and (4) by including year dummy variables. Comparing the first-difference specifications in columns (1) and (3) shows that the rate-of-return coefficients are again similar while the industry-return coefficient is smaller in magnitude than the coefficient reported in column (1) but remains negative and significant. Comparing the fixed-effects specifications in columns (2) and (4), the cumulative-stock-return coefficient is again essentially unchanged while the cumulative-industry-return coefficient is negative and significant in column (4), compared to being insignificant in column (2). Thus, including year dummies appears to resolve the discrepancy between the first-difference and fixed-effects specifications.

#### *Comparison with Previous Research*

Antle and Smith (1986) conduct a careful examination of the relation between CEO salary and bonus and industry rates of return but find no evidence that two-digit industry risk is filtered out of executive cash compensation contracts.<sup>22</sup> In contrast, we find that industry risk is partially filtered out of contracts (although the effect diminishes when we include market movements as well). In an attempt to reconcile our conflicting findings, we investigated several differences between the Antle-Smith methodology and our own. First, while our regression specification relates changes in compensation to absolute and industry stock returns, the Antle-Smith specification relates pay changes to *changes* in absolute and industry stock and accounting returns. Also, while our sample pools data from over 1,000 firms in 58 two-digit SIC

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<sup>22</sup> Antle and Smith (1986), Table 6B. Although Antle and Smith find no relation between salary and bonus and industry returns, they do document a weak relation between industry returns and a broader definition of compensation which includes the *net* returns on inside stockholdings. They measure these net returns on inside stockholdings as the change in the value of stockholdings minus the change in the value of the S&P 500 portfolio. Thus, their results may be driven by correlations between industry returns and the S&P portfolio.

industries, the Antle-Smith results are based on separate regressions for 39 firms in three industries.

Column (1) of Table 5 reports results from relative-performance regressions using our data and the Antle-Smith specification,

$$(4.3) \quad \Delta \ln(\text{CEO Pay}) = \alpha + \beta \Delta(\text{Shareholder Return}) + \gamma \Delta(2\text{-Digit Industry Return}).$$

The coefficient on the change in shareholder return is positive and significant, indicating that CEOs are rewarded for taking actions that increase the value of their firm, *ceteris paribus*. Moreover, the coefficient on the change in industry return is negative and significant, indicating that boards of directors adjust for industry shocks when revising the CEO's compensation. This evidence is consistent with the RPE hypothesis, and indicates that specification differences do not explain the difference between our results and Antle and Smith's. Our specification does, however, have more explanatory power than the Antle-Smith specification, with  $R^2 = .0496$  in column (2) of Table 1, compared to  $R^2 = .0196$  in column (1) of Table 5.<sup>23</sup>

Our results in Tables 1 through 4 are based on pooled cross-sectional time-series data, while Antle and Smith report separate regressions for each firm. In a further attempt to replicate their results, we estimate the regression in column (1) of Table 5 separately for the 690 firms in our sample which had six or more usable observations. The average and median t-statistics associated with the coefficients on the change in industry return in these firm-specific regressions are -0.39 and -0.37, respectively. Two-thirds (450) of the 690 estimated coefficients are less than zero, and 89 (13%) of the associated t-statistics are less than -1.8. The estimated

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<sup>23</sup> Our specification in (4.1) restricts the coefficients on lagged shareholder and industry returns to be zero, while the Antle-Smith specification (4.3) implicitly restricts these coefficients to be equal in magnitude and opposite in sign to the analogous coefficients on the contemporaneous performance variables. Table 8 in the appendix presents results of both first-difference and fixed-effects (level) regressions of compensation on performance and lagged performance. The results in columns (1) and (3) suggest that changes in CEO pay are positively related to shareholder return and (to a much smaller degree) lagged shareholder return. In addition, the results—in column (3), for example—show that pay changes are negatively related to contemporaneous industry return, but are *positively* related to lagged industry return. The estimated coefficients in the associated pay-levels regression in column (4) suggest that mis-specification is not the cause of this perplexing result. Column (4) does provide an alternative statement of the result, however: while current and lagged returns are important determinants of the level of pay, we find support for the RPE hypothesis with respect to current but *not* lagged industry returns.

coefficients are based on varying degrees of freedom and it is therefore difficult to test the joint significance of the individual coefficients. As a whole, however, these firm-specific regressions provide fairly strong support for the RPE hypothesis.

It seems likely that the use of RPE varies across industries, and it is possible that the particular firms in the three industries analyzed by Antle and Smith (electronics, aerospace, and chemical companies) are for some reason characterized by a lack of relative-performance evaluation.<sup>24</sup> Column (3) of Table 5 reports coefficients from a regression of pay growth on changes in firm and industry rates of return for CEOs in the three industries examined by Antle and Smith: chemicals and pharmaceuticals (SIC code 28), electric machinery and equipment (SIC code 36), and transportation (SIC code 37).<sup>25</sup> The industry-return coefficient in column (3) is negative and significant ( $t=-5.5$ ), suggesting that the specific industries analyzed by Antle and Smith do not account for the difference in our results. Twenty-six of the 39 Antle-Smith sample firms are included in our data, and column (5) of Table 5 reports RPE regressions for this 26-firm subsample.<sup>26</sup> The industry-return coefficient is again negative and significant, thus the particular firms analyzed by Antle and Smith also appear not to account for the difference in our results.

In addition to stock performance, the Antle-Smith specification also includes changes in firm and industry return on assets. Columns (2), (4), and (6) of Table 5 replicate the Antle-Smith specification (including return on assets) for the *Forbes* sample, the three-industry sample, and 26-firm sample, respectively.<sup>27</sup> After controlling for accounting returns, the change in the industry-stock-return coefficient is negative and significant for the full sample in column (2) and the three-industry sample in column (4), but is insignificant for the 26-firm sample in column

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<sup>24</sup> The Antle-Smith data are based on a sample originally constructed and analyzed by Masson (1971), who selected the three industries because of their rapid and sporadic technologically intensive growth in the postwar expansion.

<sup>25</sup> The Antle-Smith sample are from the aircraft-manufacturing subset of the two-digit transportation industry (which also includes automotive firms), and the chemical subset of the two-digit chemical and pharmaceuticals industry, but their industry-relative returns are based on the more broadly-defined two-digit industries.

<sup>26</sup> Most of the 13 remaining firms were delisted or merged prior to the beginning of our sample.

<sup>27</sup> Return on assets is constructed from Compustat data and is defined as the sum of after-tax income (before extraordinary items), interest, and interest tax shields, divided by total assets. Industry return on assets is defined as the asset-weighted-average return on assets for all other firms in the same two-digit industry.







contracts by estimating the effect of firm, industry, and market performance on the probability that a CEO will soon leave office. In particular, we estimate the following logistic regression:

$$\ln\left(\frac{\text{Prob}[\text{Turnover}]}{1-\text{Prob}[\text{Turnover}]}\right) = \alpha + \beta(\text{Shareholder Return}) + \gamma(1\text{-Digit Industry and/or Market Return}).$$

The dependent variable equals one if the CEO is serving in his last full fiscal year, and equals zero otherwise.<sup>32</sup> The theory of relative performance evaluation predicts that  $\beta < 0$  and  $\gamma > 0$ : CEOs are *more likely* to be dismissed following bad years in the firm, but are *less likely* to leave following bad years in the industry and market, *ceteris paribus*.

Table 6 reports coefficients from logistic regressions predicting the probability of CEO turnover as a function of shareholder, industry, and market rates of return. Column (1) shows that, unconditional on industry and market performance, the probability that a CEO is serving in his last full fiscal year is negatively and significantly related to the rate of return realized by shareholders during that year. Converting the regression coefficients into estimated dismissal probabilities, the regression in column (1) implies that a CEO has a .109 dismissal probability in years when the firm earns a zero return, but a .131 dismissal probability in years when the firm realizes a -50% return.<sup>33</sup>

Columns (2) and (3) of Table 6 show that turnover probabilities are negatively and significantly related to shareholder return, but positively and significantly related to returns in the one-digit industry and market, *ceteris paribus*. The regression in column (3) implies, for example, that a CEO has a .098 dismissal probability in years when both the firm and the market earn a zero return. The same CEO has a .137 dismissal probability when the firm earns a -50% return while the market return is zero, but only a .083 dismissal probability in years when both the firm and market earn a -50% return.

<sup>32</sup> Coefficients and standard errors are computed using the LOGIST procedure in SAS. The 1988 *Forbes* survey was examined to identify CEOs whose last fiscal year was 1986. The final CEO-year for firms leaving the *Forbes* survey is excluded, since we cannot determine whether or not this is the last year for that CEO. A total of 582 firms were deleted from the *Forbes* surveys during the 1974-1986 sample period. Of these, 293 are still "going concerns" as of 1987, 214 were acquired by or merged with another firm, and 35 liquidated, went bankrupt, or went private. Current status data are unavailable for 40 of the 582 firms.

<sup>33</sup> Dismissal probabilities are computed as  $p = \frac{e^x}{1+e^x}$  where  $x = -2.10 - .4152(\text{Return})$ .

Column (4) in Table 6 includes both industry and market returns. As in column (3), the market-return coefficient in column (4) is positive and statistically significant, although it is much reduced in size. The industry-return coefficient in column (4) remains positive but is no longer significant after controlling for market-wide stock-price movements. We also re-estimated columns (2) and (4) using the two-digit, three-digit, and four-digit alternative industry definitions. Consistent with our earlier results on compensation, we find that the magnitude of the industry coefficient declines monotonically as the industry becomes more narrowly defined (although the results for one-digit and two-digit industries are virtually identical). Thus, our results suggest that CEO turnover policy is more likely to control for aggregate market movements than to control for industry movements.

Many factors can affect stock prices in addition to industry and market movements, and boards of directors may plausibly require more than a single bad year before forcing or encouraging a CEO to leave office. Table 7 reports coefficients from logit regressions predicting the probability of CEO turnover as a function of current *and lagged* shareholder, industry, and market rates of return. The results in Table 7 are very similar to the results in Table 6, and the effects of contemporaneous performance on turnover probability are uniformly greater than are the effects of lagged performance. We also re-estimated the regressions in Table 7 after incorporating an additional lagged performance variable (*i.e.*, performance measured two years before the CEO's final fiscal year) and obtained similar results. These results are consistent with the findings of Warner, Watts, and Wruck (1988) and Weisbach (1988), who conclude that decisions to replace CEOs are based on relatively recent performance data.

### *Comparison with Previous Research*

Warner, Watts, and Wruck (1988) estimate logit regressions for eighty-seven CEO changes and find that the probability of CEO turnover is negatively related to contemporaneous shareholder return and positively related to contemporaneous market return, while the relation between turnover probability and past performance is generally insignificant. Consistent with

our results, Warner, Watts, and Wruck find that turnover is unrelated to industry performance after controlling for market performance. Although their sample is much smaller than ours (which is based on 1,000 CEO changes), the Warner-Watts-Wruck data may be less noisy than ours since they measure performance from the date when the CEO change was announced rather than over the preceding fiscal year.<sup>34</sup> The similarity of our results and theirs suggests that our evidence supporting RPE in CEO turnover policy is robust to these potentially important timing issues.

While not directly testing the RPE hypothesis, Coughlan and Schmidt (1985) find an inverse relation between CEO turnover and abnormal stock-price performance (estimated as the cumulative annual residual from the market model) and Weisbach (1988) obtains similar results by measuring performance as shareholder return minus the return on a value-weighted market portfolio. In addition, Morck, Shleifer, and Vishny (1988) find that the probability that the entire top-management team leaves is negatively related to abnormal performance minus abnormal performance in the industry. Our results in Tables 6 and 7 are consistent with these previous studies.

CEOs leave office for a variety of reasons and are rarely openly fired from their positions. Weisbach, for example, analyzes 286 CEO changes over ten years and finds only nine cases in which boards mentioned performance as a reason why the CEO was replaced. Moreover, CEOs usually leave their position only after reaching normal retirement age—a third of the CEOs in our sample left when they were 64 or 65, and more than half left between the ages of 62 and 66. We attempt to exclude normal retirements by re-estimating the logit regressions in Tables 6 and 7 for the subset of CEOs younger than 64. Our results for the younger subsample are qualitatively similar to the results for the full sample, although the asymptotic t-statistics and significance levels tend to be higher for the subsample. Therefore, given the noisiness of the data (in terms of the timing issues discussed above) and the existence

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<sup>34</sup> In addition, Warner-Watts-Wruck also exclude cases where the CEO title changes hands but the top-management team remains intact (*e.g.*, the chairman-CEO remains chairman but passes the CEO title to the president). We have no way of identifying these cases in our data.

of important omitted factors (such as voluntary or mandatory retirement), we interpret our finding that relative performance affects the probability of CEO turnover as offering strong support for the RPE hypothesis.

## 6. Conclusion

Rewarding top-level executives based on performance measured relative to aggregate performance in the industry or market creates incentives to take actions increasing shareholder wealth while insuring executives against the vagaries of the stock and product markets that are beyond their control. We expect RPE to be a common feature of implicit CEO compensation and dismissal contracts because the potential benefit of filtering out common uncertainty is high, the cost of measuring the performance of other firms is small, and opportunities for sabotage and collusive shirking are limited.

The empirical evidence presented above supports the RPE hypothesis—CEO pay revisions and *retention* probabilities (*i.e.*, the complement of dismissal probabilities) are positively and significantly related to firm performance, but are negatively and significantly related to industry and market performance, *ceteris paribus*. Our results also suggest that CEO performance is more likely to be evaluated relative to aggregate market movements than relative to industry movements, although our inability to detect an industry effect after controlling for market movements may reflect that industry definitions based on SIC codes are inappropriate for purposes of relative performance evaluation.

The theoretical justification for rewarding top-level executives on the basis of relative performance is strong, and therefore the fact that previous research has failed to detect a significant relative-performance effect has been troublesome. The results in this paper begin to resolve the puzzle by documenting a significant negative relation between CEO compensation and industry and market performance, but more work remains to be done before one can conclude that the data provides completely convincing support for the RPE hypothesis. We cannot yet

explain, for example, the apparent absence of RPE in plausible regression specifications, such as equation (4.4). In addition, although our results support the RPE hypothesis with respect to current shareholder return, we do not find evidence for RPE with respect to lagged shareholder return (Appendix Table 8) or accounting returns (Table 5). Finally, we have made no attempt to distinguish between the incentive and learning versions of the RPE hypothesis.

Although the theory of relative performance evaluation predicts that optimal contracts will protect CEOs from industry and market shocks, there are many aspects of a CEO's wealth that are not protected from industry and market movements in stock prices. The gains and losses associated with stock options and inside stockholdings, for example, depend solely on the firm's absolute stock-market performance and not on performance measured relative to the industry or market. Therefore, although the CEO's compensation and probability of dismissal are partially insured against industry and market risk, other potentially more important components of his wealth are not.

It would be straightforward, however, to design a compensation plan that duplicates the rewards and losses from stock ownership or stock options *after* adjusting for industry- or market-wide stock movements, and it is puzzling that few firms have adopted such explicit RPE compensation systems. It may be that current stock-option and restricted-stock plans are easy to administer and offer tax advantages, and that the costs of switching to new systems outweigh the potential benefits. Alternatively, the distorted incentives induced by RPE—in terms of sabotage, collusion, choice of reference group, or production externalities—may in fact be important for top-level managers. Determining why RPE is not more comprehensive represents a productive opportunity for future research.

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TABLE 1

Coefficients of Ordinary Least Squares Regressions of  $\Delta \ln(\text{CEO Salary} + \text{Bonus})$  on Firm, Industry, and Market Rates of Return on Common Stock  
(t-statistics in parentheses)

Independent Variable	Dependent Variable: $\Delta \ln(\text{CEO Salary} + \text{Bonus})$			
	(1)	(2)	(3)	(4)
Intercept	.046	.055	.068	.068
Firm's Rate of Return on Common Stock	.1562 (19.5)	.1781 (19.4)	.1805 (21.0)	.1824 (19.9)
2-Digit SIC Industry Value-Weighted Return	—	-.0750 (-4.9)	—	-.0112 (-0.6)
Market Value-Weighted Return on Common Stock	—	—	-.1490 (-7.6)	-.1407 (-5.8)
R <sup>2</sup>	.0467	.0496	.0537	.0538

Note: Sample size is 7,755 for all observations. The sample is constructed from longitudinal data reported in *Forbes* on 1,668 CEOs serving in 1,049 firms from 1974-1986.

TABLE 2

Coefficients of Ordinary Least Squares Regressions of  $\Delta \ln(\text{CEO Salary} + \text{Bonus})$  on Shareholder Return, Industry Return, and Market Return for Three Alternative Industry Definitions (t-statistics in parentheses)

Independent Variable	Regression Coefficients					
	Dependent Variable is $\Delta \ln(\text{CEO Salary} + \text{Bonus})$					
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	.064	.051	.050	.069	.068	.068
Firm's Rate of Return	.1850 (20.6)	.1722 (18.7)	.1637 (18.4)	.1855 (20.7)	.1810 (19.5)	.1771 (19.5)
1-Digit SIC Industry Rate of Return	-.1266 (-7.1)	—	—	-.0554 (-2.0)	—	—
3-Digit SIC Industry Rate of Return	—	-.0452 (-3.5)	—	—	-.0018 (-0.1)	—
4-Digit SIC Industry Rate of Return	—	—	-.0303 (-2.8)	—	—	-.0019 (-0.2)
Value-Weighted Market Rate of Return	—	—	—	-.1027 (-3.4)	-.1462 (-6.6)	-.1444 (-6.9)
Sample Size	7,757	7,687	7,429	7,757	7,687	7,429
R <sup>2</sup>	.0528	.0483	.0479	.0542	.0536	.0539

Note: The sample is constructed from longitudinal data reported in *Forbes* on 1,668 CEOs serving in 1,049 firms from 1974-1986.

TABLE 3

Coefficients of Ordinary Least Squares Regressions of  $\Delta\ln(\text{Salary} + \text{Bonus})$  and  $\Delta\ln(\text{Total Pay})$  on Firm and Industry Return for *Forbes* Sample and for Murphy (1985) Sample of Manufacturing Firms (t-statistics in parentheses)

Independent Variable	Regression Coefficients					
	1,049-Firm <i>Forbes</i> Sample Dependent Variable: $\Delta\ln[\text{Total Pay}]$		73-Firm Murphy Sample Dependent Variable: $\Delta\ln[\text{Total Pay}]$			
	(1)	(2)	$\Delta\ln[\text{Salary} + \text{Bonus}]$ (3)	(4)	(5)	(6)
Intercept	.074	.082	.024	.025	.031	.025
Firm's Rate of Return on Common Stock	.2048 (17.9)	.2055 (18.0)	.1441 (7.1)	.1455 (7.2)	.1991 (5.1)	.1918 (4.9)
1-Digit SIC Industry Value-Weighted Return	-.1859 (-8.2)	-.0827 (-2.4)	-.1533 (-4.7)	-.1089 (-1.2)	-.3282 (-5.2)	-.5670 (-3.3)
Market Value-Weighted Return on Common Stock	—	-.1487 (-3.8)	—	-.0522 (-0.5)	—	.2807 (1.5)
Sample Size	7,757	7,757	997	997	997	997
R <sup>2</sup>	.0397	.0415	.0495	.0498	.0328	.0351

Note: *Forbes* sample constructed from longitudinal data on 1,668 CEOs serving in 1,049 firms from 1974-1986. The *Forbes* definition of total compensation typically includes salary, bonus, value of restricted stock, savings and thrift plans, and other benefits but does not include the value of stock options granted or the gains from exercising stock options. The Murphy (1985) data are based on 173 CEOs serving in 73 large manufacturing firms from 1966 to 1983. Murphy's definition of total compensation includes salary, bonus, value of restricted stock, savings and thrift plans, other benefits, and the estimated value of stock options granted.

TABLE 4

Coefficients of Ordinary Least Squares Regressions of  $\ln(\text{CEO Salary} + \text{Bonus})$  on Firm and One-Digit Industry Shareholder Return for Level and First-Difference Specifications  
(t-statistics in parentheses)

Independent Variable	Dependent Variable in 1000s of 1986-Dollars			
	Change in CEO Pay	Ln CEO Pay	Change in CEO Pay	Ln CEO Pay
	(1)	(2)	(3)	(4)
Intercept	.064	(CEO-specific)	.005	(CEO-specific)
Current Stock Return ( $R_{it}$ )	.1850 (20.6)	—	.1801 (20.1)	—
Current 1-Digit Industry Return ( $I_{it}$ )	-.1266 (-7.1)	—	-.0740 (-2.7)	—
Time Trend (t)	—	.0343 (15.5)	—	—
Cumulative Stock Return $\left(\sum_{\tau=1}^t R_{i\tau}\right)$	—	.1829 (25.0)	—	.1762 (24.3)
Cumulative 1-Digit Industry Return $\left(\sum_{\tau=1}^t I_{i\tau}\right)$	—	-.0011 (-0.1)	—	-.0449 (-2.2)
Year Dummies	(no)	(no)	(yes)	(yes)
Sample Size	7,757	9,361	7,757	9,361
$R^2$	.0528	.8926	.0758	.8964

Note: The sample is constructed from longitudinal data reported in *Forbes* on 1,668 CEOs serving in 1,049 firms from 1974-1986. Columns (2) and (4) include 1,668 individual intercepts.

Regression Specification: Level:  $\ln(w_{it}) = \lambda_i + \alpha + \beta \sum_{\tau=1}^t R_{i\tau} + \gamma \sum_{\tau=1}^t I_{i\tau} + \varepsilon_{it}$

First Difference:  $\Delta \ln(w_{it}) = \alpha + \beta R_{it} + \gamma I_{it} + v_{it}$

TABLE 5

Coefficients of Ordinary Least Squares Regressions of  $\Delta \ln(\text{CEO Salary} + \text{Bonus})$  on  $\Delta(\text{Shareholder Return})$ ,  $\Delta(\text{Industry Return})$ ,  $\Delta(\text{Return on Assets})$ , and  $\Delta(2\text{-Digit Industry Return on Assets})$  for Full 1,668-CEO Sample, CEOs in Three Industries, and CEOs in 26 Firms (t-statistics in parentheses)

Independent Variable	Dependent Variable is $\Delta \ln(\text{CEO Salary} + \text{Bonus})$					
	Full <i>Forbes</i> Sample		Three Antle-Smith Industries		26 Antle-Smith Firms	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	.065	.066	.061	.060	.051	.048
Change in Firm's Shareholder Return	.0731 (11.2)	.0583 (8.9)	.0646 (3.6)	.0436 (2.5)	.0431 (1.2)	-.0114 (-0.3)
Change in 2-Digit Industry Return	-.1071 (-11.0)	-.0828 (-8.4)	-.1494 (-5.5)	-.0738 (-2.6)	-.1370 (-2.5)	-.0026 (-0.0)
Change in Firm's Return on Assets	—	1.220 (15.2)	—	1.590 (8.0)	—	3.426 (7.1)
Change in 2-Digit Industry Return on Assets	—	.3730 (2.0)	—	1.804 (3.3)	—	1.682 (2.2)
Sample Size	7,693	7,468	1,079	1,059	247	247
R <sup>2</sup>	.0196	.0539	.0273	.1036	.0264	.2162

Note: Full sample and subsamples constructed from longitudinal data reported in *Forbes* on 1,668 CEOs serving in 1,049 firms from 1974-1986. The three-industry subsample includes 218 CEOs serving in 135 chemical (SIC=28), electronics (SIC=36) and transportation (SIC=37) firms. The 26-firm subsample includes 44 CEOs.

TABLE 6

Estimated Logistic Models Predicting CEO Turnover Using Shareholder, Industry,  
and Market Rates of Return  
(asymptotic t-statistics in parentheses)

Independent Variable	Coefficient Estimates			
	(1)	(2)	(3)	(4)
Intercept	-2.10	-2.19	-2.22	-2.22
Firm's Rate of Return on Common Stock	-.4152 (-4.1)	-.7130 (-5.7)	-.7542 (-6.2)	-.7341 (-5.8)
1-Digit Industry Value-Weighted Return on Common Stock	—	.8330 (3.9)	—	-.2613 (-0.6)
Market Value-Weighted Return on Common Stock	—	—	1.114 (4.9)	1.350 (3.1)

Note: Sample size is 9,372 for all regressions. The dependent variable is equal to 1 if the CEO is serving in his last full fiscal year (1,000 observations) and 0 otherwise (8,372 observations); rates of return are calculated for fiscal year. The sample is constructed from longitudinal data reported in *Forbes* on 1,915 CEOs serving in 1,104 firms from 1974-1986. All regressions are significant at the .01% level.

TABLE 7

Estimated Logistic Models Predicting CEO Turnover Using Current and Lagged Shareholder, 1-Digit SIC Industry, and Market Rates of Return  
(asymptotic t-statistics in parentheses)

Independent Variable	Coefficient Estimates			
	(1)	(2)	(3)	(4)
Intercept	-2.10	-2.21	-2.26	-2.25
Current Stock Return	-.3499 (-3.4)	-.6531 (-5.2)	-.6981 (-5.7)	-.6747 (-5.3)
Lagged Stock Return	-.2660 (-2.7)	-.4114 (-3.3)	-.4122 (-3.4)	-.4295 (-3.4)
Current 1-Digit Industry Return	—	.8253 (3.9)	—	-.2905 (-0.7)
Lagged 1-Digit Industry Return	—	.3263 (1.6)	—	.2377 (0.6)
Current Market Return	—	—	1.117 (4.8)	1.379 (3.1)
Lagged Market Return	—	—	.3835 (1.7)	.1924 (0.4)

Note: Sample size is 9,291 for all regressions. The dependent variable is equal to 1 if the CEO is serving in his last full fiscal year (992 observations) and 0 otherwise (8,299 observations); rates of return are calculated for fiscal year. The sample is constructed from longitudinal data reported in *Forbes* on 1,896 CEOs serving in 1,092 firms from 1974-1986. All regressions are significant at the .01% level.

APPENDIX  
TABLE 8

Coefficients of Ordinary Least Squares Regressions of  $\ln(\text{CEO Salary} + \text{Bonus})$  on Firm and Industry Shareholder Return for Level and First-Difference Specifications  
(t-statistics in parentheses)

Independent Variable	Dependent Variable in 1000s of 1986-Dollars			
	Change in ln CEO Pay	ln CEO Pay	Change in ln CEO Pay	ln CEO Pay
	(1)	(2)	(3)	(4)
Intercept	.046	(CEO-specific)	.043	(CEO-specific)
Current Stock Return ( $R_{it}$ )	.1792 (20.2)	.0166 (1.9)	.1746 (19.5)	.0222 (2.5)
Lagged Stock Return ( $R_{it-1}$ )	.0284 (3.3)	—	.0275 (3.2)	—
Current 1-Digit Industry Return ( $I_{it}$ )	-.0893 (-5.0)	-.1443 (-9.5)	-.0855 (-3.1)	-.0636 (-2.3)
Lagged 1-Digit Industry Return ( $I_{it-1}$ )	.0849 (6.1)	—	.0584 (2.1)	—
Time Trend (t)	—	.0256 (10.8)	—	—
Cumulative Stock Return $\left(\sum_{\tau=1}^t R_{i\tau}\right)$	—	.1779 (22.0)	—	.1676 (20.8)
Cumulative 1-Digit Industry Return $\left(\sum_{\tau=1}^t I_{i\tau}\right)$	—	.0830 (4.7)	—	-.0244 (-1.1)
Year Dummies	(no)	(no)	(yes)	(yes)
Sample Size	7,696	9,361	7,696	9,361
$R^2$	.0652	.8941	.0769	.8965

Note: The sample is constructed from longitudinal data reported in *Forbes* on 1,668 CEOs serving in 1,049 firms from 1974-1986. Columns (2) and (4) include 1,668 individual intercepts.

Regression Specification:

$$\text{Level:} \quad \ln(w_{it}) = a_i + \alpha t + (\beta_0 - \beta_1)R_{it} + \beta_1 \sum_{\tau=1}^t R_{i\tau} + (\gamma_0 - \gamma_1)I_{it} + \gamma_1 \sum_{\tau=1}^t I_{i\tau} + \varepsilon_{it}$$

$$\text{First Difference:} \quad \Delta \ln(w_{it}) = \alpha + \beta_0 R_{it} - (\beta_0 - \beta_1)R_{it-1} + \gamma_0 I_{it} - (\gamma_0 - \gamma_1)I_{it-1} + v_{it}$$