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Firm Value and Managerial Incentives: A Stochastic Frontier Approach*

I. Introduction

Do managers acting as agents of shareholders maximize firm value? If not, how large are the costs of agency? How effective are compensation contracts and other incentive schemes in solving such agency problems? And, are these contracts and incentives structured optimally?

These empirical questions have been at the heart of corporate finance theory, starting with Berle and Means (1932) and continuing with Jensen and Meckling (1976) and Demsetz (1983). Yet, the empirical literature has not tested explicitly for value

* We thank Suddipto Bhattacharya, Michael Devereux, Julian Franks, Charles Goodhart, Cam Harvey, Albert Madansky (the editor), Jan Mahrt-Smith, Pierre Mella-Barral, Henri Servaes, and David Yermack for helpful comments; Tim Coelli and Bill Greene for kindly answering our many questions on stochastic frontier analysis; Eli Ofek for generous help in gathering board-size data; seminar participants at Duke University, the 2000 NYSE-CEPR Symposium in Financial Markets (Gerzensee), the London School of Economics, the 2000 TMR Conference at Universitat Pompeu Fabra, and the Universities of Bristol, Mannheim, Oxford, Vienna, Warwick, and Zurich for helpful comments; David Stolin and Deborah Lisburne for excellent research assistance; and *Forbes* and I/B/E/S for permission to use their data. We gratefully acknowledge funding from the European Union (TMR grant no. ERBFMRXCT960054) and NCCR-FINRISK (Habib). We are especially grateful to an anonymous referee. Contact the corresponding author, Alexander Ljungqvist, at aljungqv@stern.nyu.edu.

(Journal of Business, 2005, vol. 78, no. 6)

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0021-9398/2005/7806-0001\$10.00

We provide a direct estimate of the magnitude of agency costs in publicly held corporations. We compute an explicit performance benchmark that compares a firm's actual Tobin's Q to the Q^* of a hypothetical value-maximizing firm having the same inputs and characteristics as the original firm. The Q of the average sample firm is around 16% below its Q^* , equivalent to a \$1,432 million reduction in its potential market value. We relate the shortfall to the incentives provided CEOs. Boards appear to grant CEOs too few shares and too many options that are insufficiently sensitive to firm risk.

maximization and provides few direct estimates of the magnitude of agency costs. Ang, Cole, and Lin (2000) provide an estimate of such costs in small corporations, but there appears to be no counterpart to their study for large corporations. Perhaps this is because, in the absence of the 100% manager-owned firms that constitute the benchmark of Ang et al.'s study, there is no obvious benchmark against which a firm's actual value can be judged in the case of large firms.

Ideally, the benchmark would be each firm's maximum value. While this is not observable, it is possible to construct a benchmark that measures the hypothetical value a firm would obtain were it to match the performance of its best-performing peer. Clearly, to be useful, such a benchmark needs to hold constant the firm's opportunity set and characteristics: a utility company is unlikely to match the performance of, say, Microsoft. It also needs to be stochastic, to allow for errors in the estimation and so prevent the benchmark from being influenced by outliers.

In this paper, we show how such a benchmark can be estimated using data on a large sample of U.S. companies from the 1990s. The average firm in our sample attains a value that is around 16% below its benchmark value. Translated into dollars, this means that the average sample firm could increase its market value by \$1,432 million were it to match the performance of its best-performing peer. This shortfall from the value-maximization benchmark may be considered a measure of agency costs in U.S. corporations, insofar as the benchmark represents the performance that would result if there were no costs of solving the agency problem.¹

Agency costs differ across firms due to the differing extent to which costly monitoring and incentives can be used to reduce shirking by management (Demsetz 1995). For example, firms in more volatile industries likely encounter greater difficulty in incentivizing risk-averse managers using equity-based compensation. Agency costs may also differ across firms because some boards fail to provide managers with the appropriate, optimal set of incentives. If so, their agency costs would be inefficiently high.

We relate the shortfall from the value-maximization benchmark to measures of managerial incentives, controlling for firm differences in the costs of solving the agency problem. We find that the shortfall is smaller, the larger are the chief executive officer's stockholdings. This echoes earlier findings by Morck, Shleifer, and Vishny (1988) and McConnell and Servaes (1990) that the cross section of firm values, as measured by Tobin's Q , is positively related to managerial stockholdings. It contrasts with later findings of no such relation by Agrawal and Knoeber (1996), Loderer and Martin (1997), Cho (1998), Himmelberg, Hubbard, and

1. Alternatively, it may measure the consumption of amenities by controlling shareholders (Demsetz 1989).

Palia (1999), Demsetz and Villalonga (2001), and Palia (2001).² When we partition the sample, we find that the negative relation between the shortfall and managerial stockholdings is economically strongest among small firms. One possible explanation for the discrepancy between our results and the majority of studies that find no relation between Tobin's Q and managerial stockholdings is the endogenous nature of such holdings, for stockholdings are to a large extent set by boards. However, tests for endogeneity cannot reject the null hypothesis of no bias in our data. Alternative explanations are the larger size and more recent nature of our data set. There was much discussion and scrutiny of CEO compensation during the 1990s, perhaps making the task of boards more difficult.

Stock options have become increasingly important components of managerial incentive schemes. Our results show that the shortfall from the value-maximization benchmark is smaller, the fewer options the CEO holds. In other words, on average boards had awarded options beyond the point where the marginal cost equals the marginal benefit of doing so. This is consistent with Yermack (1995), who finds little evidence of a connection between CEO option awards and a reduction in agency costs, and with Meulbroek (2001), who provides evidence of deadweight costs that reduce the benefits of awarding options to CEOs. When we again distinguish among firms of different size, we find that it is medium-sized firms that have awarded too many options. Small and large firms appear to have awarded the optimal number of options.

Options not only provide effort incentives, the convexity of their payoff function also affects choice of project risk (Lambert, Larcker, and Verrecchia 1991). If risk-averse managers tend to choose lower-risk, lower-NPV projects over higher-risk, higher-NPV projects, boards may award options that make managers' wealth more sensitive to risk. As noted by Guay (1999), this implies awarding options whose value increases more rapidly with risk, which in turn can be measured using the option's *vega*.³ In our sample, the performance shortfall decreases in *vega*, which suggests that CEOs not only hold too many options but that their options provide insufficient risk-taking incentives.

Taken together, our findings on stock and option holdings and *vega* suggest that boards provided managers with inefficient incentives. This interpretation assumes that we have adequately controlled for firm differences in the costs of solving the agency problem. We attempt to do so by including measures of capital market pressure and product market pressure (both of which may substitute for internally provided incentives)

2. A related finding is that of Demsetz and Lehn (1985), who find no relation between accounting profit rates and ownership concentration.

3. Guay shows that *vega* is positively related to companies' investment opportunities, which is consistent with boards seeking to provide incentives to invest in risky projects. Rajgopal and Shevlin (2002) find that *vega* has a positive effect on future choice of project risk in the oil and gas industry.

and idiosyncratic risk (which raises the cost of providing incentives to risk-averse managers). We also include board size to control for the effectiveness of board monitoring. We find that capital market pressure in the form of takeovers or bankruptcy has no effect on firm performance (except among utilities), whereas greater product-market competition within an industry has a beneficial effect. Higher idiosyncratic risk significantly increases the shortfall from the value-maximization benchmark. Board size, on the other hand, has no significant effect on performance except among medium-sized firms, where the performance shortfall first decreases and then increases in board size.

A natural follow-on question is this: do boards respond to poor performance, relative to our benchmark, by strengthening incentives? Our evidence suggests they do: it is the companies whose boards adjust incentives appropriately over time that improve their performance the most.

We proceed as follows. Section II presents our empirical approach. We describe the data in Section III and present our empirical results in Section IV. Section V reports a number of robustness checks, notably for possible endogeneity bias. We examine boards' responses to poor performance in Section VI. Section VII concludes. The appendix details the construction of our dataset.

II. Empirical Approach

A. Constructing a Value Benchmark

A firm's value is the present value of the cash flows generated by the firm's assets, which consist of assets in place and growth opportunities. An estimate of the firm's value is provided by the market capitalization of its debt and equity. Tobin's Q is the ratio of the market value of debt and equity and the replacement cost of the firm's assets in place. If a firm operates and invests in assets that are expected to create value, then its Q will be greater than 1. (The marginal q of its least productive asset, however, should equal 1.) The more value created, the higher is the Q .

The question whether a firm's manager maximizes value can then be restated as follows: does the firm trade at a Q that is as high as it could be if all operating and investment decisions were made optimally? Call this benchmark Q^* . This benchmark Q^* should have two desirable characteristics. It should hold constant a firm's opportunity set and characteristics, to avoid an apples-and-oranges comparison of companies' performance. And it should be stochastic, to allow for errors in the estimation and so prevent the benchmark from being driven by outliers.

To see how an estimate of Q^* with these characteristics can be constructed in principle, consider a set of firms, each of which has access to the same opportunity set. We do not expect all firms to be equally

effective in pursuing these opportunities and so to trade at the same valuation. Monitoring costs differ across firms, as do the costs of providing incentives to managers. Individual managers make different production, investment, and strategic decisions, in response to the differing intensity of the monitoring to which they are subject and the incentives with which they have been provided. Some firms therefore trade at higher valuations than others. The firms with the highest valuations are the ones creating the most value per dollar of assets in place. Varying the opportunity set and firm characteristics, we can trace out a curve that gives the maximum Q observed in a sample for any combination of opportunity sets and firm characteristics, X . This curve, which we call the *frontier function*, is an estimate $Q^* = f(X)$ allowing for firm differences in X . Firms whose actual Q plots below the frontier fall short of the valuation they could achieve were they to perform as well as the frontier company whose X they share. The shortfall from the frontier, $Q^* - Q$, is a measure of agency costs.

Of course, the reason why a particular firm is on the frontier may merely be “good luck,” rather than superior management. Conversely, “bad luck” will push a firm below the frontier through no fault of its management. It is important, therefore, to view a firm’s actual performance as being the realization of a random variable. Thus, we should put less weight on extremely positive performance in estimating the frontier, since extreme observations are more likely to be generated by “good luck.”

B. Stochastic Frontier Analysis

How do we estimate Q^* ?⁴ Note that it is in the nature of a frontier that firms can lie only on the (true) frontier or below it but never above it. Stochastic frontier analysis (SFA) captures this asymmetry in the distribution of firms by supplementing the conventional two-sided error term used in ordinary least-squares methods (OLS) with a one-sided error term.⁵ This second term is zero for the value-maximizing firms that achieve the highest Q but strictly positive for those firms that do not maximize value and therefore fail to achieve as high a Q as can be achieved given their opportunity sets.

Formally, $Q_i = X_i\beta + \varepsilon_i$, where $\varepsilon_i = v_i - u_i$. The two-sided error term $v_i \sim N(0, \sigma_v^2)$ denotes the zero-mean, symmetric, independently and identically distributed error component found in conventional regression equations. It allows for estimation error in locating the frontier itself, thus preventing the frontier from being set by outliers. The one-sided error

4. The analysis in this section is based on Stevenson (1980), Battese and Coelli (1988, 1992, 1995), and Reifschneider and Stevenson (1991).

5. Stochastic frontier analysis was pioneered by Aigner, Lovell, and Schmidt (1977) and Meeusen and van den Broeck (1977) and is widely used in economic studies of productivity and technical efficiency. Two applications in finance are studies of banking efficiency and an article on pricing efficiency in the IPO market (Hunt-McCool, Koh, and Francis 1996).

term $u_i \geq 0$ permits the identification of the frontier, by making possible the distinction between firms that are on the frontier ($u_i = 0$) and firms that are strictly below the frontier ($u_i > 0$). The variable u therefore corresponds to the shortfall in a firm's actual valuation. Of course, if all firms were on the frontier, then $u_i = 0$ and $Q_i = Q_i^*$ for all firms i : all firms would achieve the highest feasible Q^* given their X and thus maximize value. In that case, the functions estimated by SFA and OLS would be identical.

If we have repeated observations on a set of firms over time, we can let the frontier move over time, capturing both changes in firms' opportunity sets and the extent to which their managers maximize firm value. Moreover, we can relate individual movements to changes in monitoring costs and the provision of incentives. Thus, if we have a panel data set, we can potentially capture the dynamics of the relationship between managers and shareholders. Using conventional panel-data notation, we can express Q as a function of a $(1 \times k)$ set of explanatory variables X , and the composite error term ε :

$$Q_{it} = X_{it}\beta + \varepsilon_{it}, \quad (1)$$

where β is a $(k \times 1)$ vector of unknown coefficients to be estimated, $i = 1, \dots, N$, and $t = 1, \dots, T_i$.⁶ The location of the frontier is allowed to shift by virtue of the time dependence of the X variables.

To actually estimate u , which is our primary variable of interest, we must make certain assumptions about its distribution from which we can derive a likelihood function. We assume u_{it} is obtained by truncation at zero of $N(\mu_{it}, \sigma_u^2)$. Truncation at zero captures the nonnegativity of u . We further assume $\text{cov}(u_{it}, v_{it}) = 0$. This restricts the stochastic error v around the frontier to be independent of the firm inefficiencies u . In other words, good or bad luck is assumed to be unrelated to systematic shortfalls from value maximization. With these restrictions, and with a further restriction on u_{it} introduced in Section II.D, we can estimate the parameters of the model using maximum likelihood.

Once these parameters have been estimated, we can measure a firm's performance shortfall using the predictions \hat{u}_{it} . We normalize these to lie between 0 and 1, by taking the ratio of a firm's actual Q to the corresponding $Q^* = Q + u$ if it were to maximize value: $\widehat{PE}_{it} = E(Q_{it}|\hat{u}_{it}, X_{it})/E(Q_{it}^*|\hat{u}_{it} = 0, X_{it})$. We follow the productivity and technical efficiency literature in referring to this ratio as firm i 's predicted efficiency at time t .⁷ If firm i 's

6. The number of observations per company T_i is allowed to vary across firms. The SFA model thus allows the panel to be unbalanced (see Greene 1993).

7. We use the term *predicted efficiency* because it is well-established but acknowledge that it is somewhat misleading: a departure from the frontier is suggestive of inefficiency only if the board has failed to set managerial incentives optimally.

predicted efficiency is 0.85, then this implies that it achieves 85% of the performance of a comparable but value-maximizing firm.

C. Testing $u = 0$

It is immediate from the structure of the error term $\varepsilon = v - u$ that $u = 0$ is a necessary and sufficient condition for value maximization:⁸ firm i maximizes its Q at time t if and only if it is on the frontier; that is, if and only if $u_{it} = 0$. We can test whether $u = 0$ on average in our sample by assessing the significance of the likelihood gain from imposing the additional one-sided error term on an OLS model. If $u_{it} = 0$ for all i, t then $\sigma_u^2 = 0$, so the likelihood function of the SFA specification will be identical to the OLS likelihood function. But, if $u_{it} > 0$ for sufficiently many i and t , then the SFA specification will lead to a likelihood gain because OLS wrongly restricts $\sigma_u^2 = 0$. The likelihood-ratio test corresponds to testing whether the OLS and the SFA functions are identical.

D. Explaining Shortfalls From Q^*

A rejection of the null hypothesis $u = 0$ naturally raises the question of what causes the shortfall from value maximization. As the shortfall is measured by the distance from the frontier u , relating u to monitoring costs and incentive variables can shed light on the reasons for the failure to maximize value and their relative importance. This amounts to decomposing the one-sided error term u introduced in Section II.B into two components, an explained component and an unexplained component:

$$u_{it} = Z_{it}\delta + w_{it}, \tag{2}$$

where Z_{it} is a $(1 \times m)$ set of variables which we refer to as *incentives*, δ is an $(m \times 1)$ vector of unknown coefficients to be estimated, and w_{it} denotes the unexplained component of u_{it} . The u_{it} and their determinants Z_{it} are allowed to vary over time, accommodating changes in a firm's position relative to the frontier over time and linking such changes to changes in the incentives given to CEOs. The value of w_{it} is obtained by the truncation of $N(0, \sigma_u^2)$ such that the point of truncation is $-Z_{it}\delta$, that is $w_{it} \geq -Z_{it}\delta$. This implies that $\mu_{it} = Z_{it}\delta$ and ensures that $u_{it} \geq 0$.⁹

It is possible to test how well our model explains shortfalls from the frontier and thus how appropriate and important our Z variables are. The better we are able to explain the cross section of u , the lower will be the unexplained variance σ_u^2 . A statistical test of the validity of our Z variables therefore can be based on $\gamma = \sigma_u^2/\sigma^2 \in [0, 1]$, where $\sigma^2 \equiv \sigma_v^2 + \sigma_u^2$. The term γ is the ratio of the unexplained error and the total error of the

8. Of course, Q^* is sample specific, so we cannot estimate a global maximum.

9. Note that equations (1) and (2) are estimated using joint maximum likelihood, which is more efficient than the alternative of a two-step approach.

regression (Aigner et al. 1977). This term γ will be zero if our Z variables fully account for departures from the frontier.

E. *The Empirical Model*

Model selection: partitioning the variable set. To estimate the model, we need to take a stand on what we consider to be an X variable that determines the location of the frontier and what we consider to be a Z variable that explains shortfalls from the frontier. In principle, there are two ways to partition the variable set: on the basis of an econometric criterion, such as maximizing the log-likelihood, or on the basis of economic theory. We choose the latter, although we note that our results are robust to letting the data determine the “best” specification. Specifically, we include among the Z variables anything that has to do with solving the agency problem between managers and shareholders. The following two subsections describe our choice of X and Z variables in detail.

The frontier. In constructing a firm’s benchmark Q , it is clearly important to control for differences in firms’ characteristics and opportunity sets. The determinants of Q have been modeled extensively, so we base our empirical specification on results established in prior literature. The precise definitions of our variable are given in the appendix and table 1. Here, we focus on their economic meaning and the predicted signs.

- Diminishing returns suggest that the average Q will fall as firms grow larger: each additional unit of capital employed will have a lower productivity than the previous one. We use log sales to capture the implied inverse relation between firm size and Q . We also include log sales squared to capture possible nonlinearities in the relation.
- “Soft” spending on research and development (R&D) and advertising (ADV) and “hard” spending on capital formation (CAPEX)—all of which we normalize by the capital stock K —proxy for growth opportunities. R&D and ADV also proxy for intangible assets. They thereby control for the upward bias in Q that results from the use of the book value of total assets, which rarely measures intangible assets precisely, as the denominator of Tobin’s Q .¹⁰ All three variables are expected to covary positively with Q .
- The operating margin Y /sales is a measure of profitability. It should be positively related to Q .
- The term K /sales and its square control for the relative importance of tangible capital in the firm’s production technology. A priori, there are two opposing effects. On the one hand, firms whose capital is relatively less tangible may be subject to greater agency problems, as capital providers cannot observe, monitor, and assess spending on intangibles as easily. They

10. For a discussion of the distortions in Tobin’s Q that result from the presence of intangible assets, see Section 2.1 of Demsetz and Villalonga (2001). As their discussion makes clear, “ Q ’s bag [of advantages and disadvantages] is far from empty.”

TABLE 1 Variable Definitions

	Definition
Firm Characteristics	
Tobin's Q	The ratio of the value of the firm divided by the replacement value of assets. Similar to Himmelberg et al. (1999), for firm value we use (market value of common equity+liquidation value of preferred equity+book value of total liabilities) and for replacement value of assets we use book value of total assets
Net sales	Net sales as reported in ExecuComp, Compustat, or a 10-K, expressed in \$ millions. Usually logged. Used to measure firm size
R&D/ K	The ratio of research and development expenditures to the stock of property, plant, and equipment (K), used to measure the role of R&D capital relative to other nonfixed assets
ADV/ K	The ratio of advertising expenditures to K , used to measure the role of advertising capital relative to other nonfixed assets
CAPEX/ K	The ratio of capital expenditures to K
Y /sales	Operating margin=ratio of operating income before depreciation to sales; proxies for market power and measures the gross cash flows available from operations
K /sales	The ratio of tangible long-term assets (property, plant, and equipment) to sales
Leverage	Book value of long-term debt/(market value of equity+book value of long-term debt), expressed in percent
Cost of capital	Estimated at the four-digit industry (not firm) level, using the sum of the Fama-French (1997) estimates of industry risk premia and the Fama-Bliss 3-month risk-free rates (from CRSP) prevailing at each company's fiscal year-end, expressed in percent
Industry growth forecasts	Analyst forecasts of long-term industry growth rates. Constructed bottom up as follows. For each firm covered in I/B/E/S, we collect the median long-term growth rate forecast for every month in our sample. We then use I/B/E/S's industry classification to compute an average growth rate for each industry in every month and assign our sample firms to I/B/E/S's industries. A sample firm's industry growth rate is the average of the I/B/E/S-industry median per-firm long-term growth forecasts in its fiscal-year end month, expressed in percent
Analyst following	The number of analysts following the stock in each fiscal year, computed as the maximum number of analysts reported in I/B/E/S as giving either a 1-year, 2-year, 3-year, or long-term forecast in or before its fiscal year-end month
Utility	A dummy equal to 1 if the company operates in two-digit SIC industries 40, 48, or 49
Dummy R&D/ K	A dummy variable equal to 1 if the data required to estimate R&D/ K is missing, and 0 otherwise
Dummy ADV/ K	A dummy variable equal to 1 if the data required to estimate ADV/ K is missing, and 0 otherwise

TABLE 1 (Continued)

Incentive Variables	
CEO stockholdings	CEO's common stockholdings as a fraction of common stock outstanding, in percent; includes beneficial ownership and restricted stock
CEO option holdings	CEO's optionholdings as a fraction of common stock outstanding, in percent.
Total delta	The partial derivative of Black-Scholes (1973) call option value, adjusted for dividends, with respect to the price of the underlying stock
Vega of options	The partial derivative of Black-Scholes (1973) call option value, adjusted for dividends, with respect to the volatility of the underlying stock. Volatility is measured as the annualized standard deviation of daily stock price returns, estimated over the 250 trading days preceding the fiscal year in question. In the regressions, we use <i>vega</i> times the dollar value of CEO wealth held in options.
Capital market pressure	Unconditional Pr(delisting) (in %), the probability of delisting in each firm's SIC-2 industry in a given panel-year. For each SIC two-digit industry and each panel year, we compute the fraction of all CRSP-listed companies delisted due to merger, bankruptcy, violation of exchange requirements, etc., capturing all involuntary and voluntary delistings. This measure is unconditional in the sense that we do not condition the probability of delisting on firm characteristics such as size or prior performance
Product market pressure	SIC four-digit Herfindahl index, computed as the sum of squared market shares (in %) of each company in an industry, here SIC four-digit, in a given year. Computed using net sales-market shares for the universe of Compustat firms in 1992–97
Board size	The number of directors voted onto the board, as per the proxy for that year
Sigma	The daily Fama-McBeth CAPM residual standard deviation, estimated over the previous year (in %, not annualized); used to measure firm-specific risk

therefore may have lower values of Q . On the other hand, and as noted previously, measures of Q tend to understate the replacement cost of intangibles. This induces a negative relation between Q and the firm's tangible capital intensity.

The preceding variables were suggested by Himmelberg et al. (1999).¹¹

To these, we add five variables: leverage, the cost of capital, industry growth forecasts, analyst following, and a dummy for regulation.

- In a Modigliani-Miller world, leverage should not affect firm value. However, if tax shields are valuable, Tobin's Q should increase in leverage. On the other hand, leverage could proxy for difficult-to-measure intangible assets, such as intellectual property, customer loyalty, or human capital. Firms

11. Himmelberg et al. suggest dealing with missing data by setting the missing values of the variable in question to 0 and including a dummy that equals 1 when data are missing, and 0 otherwise. This avoids having to drop firm-years where data are missing. In our sample, some values of R&D, ADV, and CAPEX are missing, so we include two (3 – 1) dummies. All results are robust to excluding missing observations instead.

that are more reliant on intangible assets are likely to have lower leverage and higher values of Q . The net effect therefore is ambiguous.

- The numerator of Q is the market value of the firm, which is obtained by discounting future cash flows at the firm’s cost of capital. Thus, the higher is the cost of capital R , the lower Q . To measure R , we use the industry risk premia estimated in Fama and French (1997).
- Declining industries have fewer growth opportunities and so lower Q values. As a proxy for growth opportunities, we use long-term industry growth rate forecasts obtained from securities analysts covered in I/B/E/S (International Brokers’ Estimate System).
- We control for the intensity of analyst following, measured as the number of analysts making growth forecasts in I/B/E/S. We expect analyst following to have a positive effect on Q (Trueman 1996).
- Regulation may constrain a utility firm’s ability to create value, by restricting the prices the firm can charge its customers, for example. Alternatively, by restricting entry into an industry, regulation may help maintain profitability in the industry at a level higher than would prevail if entry were free. The net effect of regulation therefore is ambiguous.

The following equation summarizes our empirical model for the determinants of Q :

$$\begin{aligned}
 Q_{it} = & \beta_0 + \beta_1 \ln(\text{sales}_{it}^-) + \beta_2 \ln(\text{sales}_{it}^+)^2 \\
 & + \beta_3 \frac{\text{R\&D}_{it}^+}{K_{it}} + \beta_4 \frac{\text{ADV}_{it}^+}{K_{it}} + \beta_5 \frac{\text{CAPEX}_{it}^+}{K_{it}} \\
 & + \beta_6 \frac{Y_{it}^+}{\text{sales}_{it}} + \beta_7 \frac{K_{it}^?}{\text{sales}_{it}} + \beta_8 \left(\frac{K_{it}^?}{\text{sales}_{it}} \right)^2 \\
 & + \beta_9 \text{leverage}_{it}^? + \beta_{10} \bar{R}_{it} + \beta_{11} \text{growth}_{it}^+ \\
 & + \beta_{12} \text{analysts}_{it}^+ + \beta_{13} \text{utility}_{it}^? \\
 & + \text{missing-value dummies} + \varepsilon_{it}, \tag{3}
 \end{aligned}$$

where we have indicated the signs we expect using +, −, and ? above the variables. We do not include industry fixed effects, because both equity-risk premia and long-term industry growth rate forecasts are defined at the industry level and so already filter industry effects.¹²

Shortfalls from the frontier. Since we have already accounted for random influences on value (such as bad luck or windfalls) via the v_{it} errors around the frontier, we assume shortfalls u are caused by conflicts of

12. We have repeated our empirical tests with industry fixed effects, with very similar results to those reported later.

interest, which can however be mitigated via incentive schemes. Specifically, if incentives matter, we expect firms to be closer to their potential, the better designed their incentive schemes. Our set of Z or incentive variables is

$$\begin{aligned}
 u_{it} &= \delta Z_{it} + w_{it} \\
 &= \delta_0 + \delta_1 \text{stockholdings}_{it} + \delta_2 \text{stockholdings}_{it}^2 \\
 &\quad + \delta_3 \text{option holdings}_{it} + \delta_4 \text{option holdings}_{it}^2 \\
 &\quad + \delta_5 \text{vega}_{it} \\
 &\quad + \delta_6 \text{capital market pressure}_{it} \\
 &\quad + \delta_7 \text{product market pressure}_{it} \\
 &\quad + \delta_8 \text{board size}_{it} + \delta_9 \text{board size}_{it}^2 \\
 &\quad + \delta_{10} \text{sigma}_{it} + w_{it}.
 \end{aligned} \tag{4}$$

The first five variables are designed to capture internal incentives that are at least in part under the board's control.

- CEO stockholdings is the fraction of the firm the CEO owns via vested or restricted stock.
- To make options comparable to stocks in their incentive effects, we measure managerial option holdings as the product of the option deltas and the fraction of firm equity managers would acquire were they to exercise their options.¹³

As in previous studies, we include squared terms for stock and option holdings to allow for nonlinearities in their relation with Tobin's Q .¹⁴

- In addition to providing effort incentives via equity and option awards, boards may also try to induce the manager to choose riskier projects by making his payoffs more convex. This would increase Q if the manager currently forgoes positive net present value (NPV) projects due to his personal risk aversion. To capture the extent to which options influence choice of project risk, we compute an option *vega* for each CEO-year, which measures the sensitivity of option value to a small change in volatility.¹⁵

13. See Yermack (1995) and Baker and Hall (1999) for a formal analysis. An alternative measure of the effort incentives of options multiplies our measure by the market value of the firm's equity. As noted by Baker and Hall (1999), ours is the proper incentive measure if managerial effort is additive, in the sense of being invariant to firm size. The second measure is appropriate if managerial effort is multiplicative and proportional to firm size. Murphy (1998) argues for the primacy of the additive measure. Our empirical results are wholly unaffected if we use the multiplicative measure instead.

14. We also investigate whether greater use of debt improves efficiency, as in Jensen's (1986) free cash flow hypothesis but find no significant effect.

15. Guay (1999) documents a positive relationship between *vega* and investment opportunities, which he interprets as "managers receiving incentives to invest in risky projects when the potential loss from underinvestment in valuable risk-increasing projects is greatest" (p. 43).

The next two variables measure the intensity of monitoring by the capital and product markets, respectively. By analogy to the internal incentives discussed earlier, we refer to these variables as *external incentives*, as they are not directly under the board's control.

- Capital market pressure is a combined measure of the within-industry risk of bankruptcy and takeover, both of which should act to discipline the CEO (Scharfstein 1988; Stulz 1990).
- Product market pressure, measured as the annual Herfindahl concentration index for every four-digit SIC industry, has an ambiguous effect on value a priori. On the one hand, Schmidt (1997) and others have argued there is more scope for managerial slack in less competitive markets, resulting in lower values of Tobin's Q . On the other hand, firms in less competitive markets might earn higher economic rents and thus have higher Q values.

The quality and effectiveness of board oversight likely affects managerial performance and thus Q , so our model also includes board size. Yermack (1996) shows that companies with smaller boards have higher values of Q , possibly because of increased free-riding (and thus reduced monitoring) as boards get larger. We include the square of board size to allow for nonlinearities. Specifically, it is possible that the relation between board size and Q is U-shaped: larger boards are prone to free-riding, but smaller boards may suffer from a lack of talent or diversity.

Finally, we include a measure of idiosyncratic risk. Although not itself an incentive variable, idiosyncratic risk affects the extent to which a risk-averse manager can be incentivized via stockholdings.¹⁶ To measure idiosyncratic risk, σ , we compute the daily residual standard deviation from Fama-McBeth CAPM regressions, estimated over the prior year.¹⁷

All Z variables potentially affect agency costs, as measured by the shortfall from the frontier. There is, however, a fundamental difference between the internal incentive variables, on the one hand, and their external counterparts and σ , on the other.¹⁸ A finding that the coefficients of the former set of variables are significant may be suggestive of inefficiency, as it may indicate that boards have failed to optimally set the internal incentives that are under their control. Similarly, a finding that the coefficient of board size is significant may indicate that shareholders have failed to set board size optimally. In contrast, a finding that the coefficients of the external incentive variables and σ are significant has

16. For a discussion of the relation between risk and incentives, see Garen (1994), Haubrich (1994), and Aggarwal and Samwick (1999).

17. Alternatively, we could use the "raw" standard deviation of returns. The correlation between the two exceeds 99%, and we obtain statistically indistinguishable results with either.

18. We thank the referee for pointing out this difference.

no implication for the efficiency of the task performed by boards, because these variables are outside their control: greater product market pressure or capital market pressure reduce the costs of solving the agency problem in as much they substitute for internal incentives, while idiosyncratic risk increases the cost of providing equity-based incentives.

III. The Data

A. Data and Sources

Our dataset is derived from the October 1998 version of Standard & Poor's ExecuComp. ExecuComp covers the 1,500 firms in the S&P Super Composite Index, consisting of the 500 S&P 500, the 400 MidCap, and the 600 SmallCap index firms, beginning in 1992.¹⁹ When Standard & Poor's changes the compositions of its indices, new firms are added to ExecuComp. The October 1998 version that we use covers 1,827 firms. Since being added to an index could be a sign of "success," using all ExecuComp firms would overrepresent "successful" firms. We therefore limit our analysis to the 1,500 original (1992 panel) firms. From these, we exclude 10 firms with dual CEOs and 1 firm for which no Compustat data were available. In common with the literature, we also exclude all financial-services companies (SIC codes 60–63), as accounting data for these are not directly comparable to those of other companies. This leaves 1,307 firms.

The panel runs from 1992 to 1997 and consists of 7,134 firm-years, 708 short of the theoretical maximum (1,307 firms \times 6 years). There are two reasons why the panel is unbalanced: attrition and missing data. Of the 1,307 companies, 176 were delisted prior to 1997, resulting in a loss of 359 firm-years (an attrition rate of 5%). Of these, 162 are taken over, 10 delisted due to violation of listing requirements, 2 ceased trading for unknown reasons, 1 was declared insolvent, and 1 liquidated. Given the low attrition rate, we do not expect attrition bias to be a serious problem.²⁰ Missing data affect 349 firm-years. In the main, missing data cause companies to "leave" our panel before 1997. For instance, the 10/1998 CD-ROM reports no 1997 data for 183 companies with non-December fiscal year-ends. Some of the missing firm-years, however, are at the beginning of the panel (1992 and 1993), due to systematic gaps in ExecuComp's coverage of option and ownership information. We discuss these issues in the appendix. A closer look at the companies affected suggests some nonrandomness: early firm-years are more likely to be

19. We verify that firms that drop out of the indices are retained in the data set unless they cease to be listed, thus minimizing survivorship bias.

20. A comparison of the Tobin's Q values of the 176 takeover targets and the surviving firms confirms that there are no systematic differences in performance.

TABLE 2 Descriptive Sample Statistics

	Mean	SD	Minimum	Q1	Median	Q3	Maximum
Firm Characteristics							
Tobin's <i>Q</i>	1.985	1.292	.229	1.237	1.569	2.216	16.340
Net sales (\$ millions)	3,137	8,158	0	328	890	2,721	153,627
R&D/ <i>K</i>	.206	.786	0	0	0	.132	33.516
ADV/ <i>K</i>	.081	.480	0	0	0	.018	19.490
CAPEX/ <i>K</i>	.236	.160	0	.129	.196	.302	1.204
<i>Y</i> /sales	-.005	4.262	-307.314	.090	.145	.222	0.823
<i>K</i> /sales	.602	1.140	0	.153	.285	.656	54.823
Leverage (%)	19.39	18.46	0	3.28	14.76	30.96	99.76
Cost of capital (%)	9.491	1.421	5.91	8.358	9.802	10.655	12.724
Industry growth forecasts (%)	16.637	5.828	2.792	13.473	16.370	19.997	35.665
Analyst following	11.9	8.4	1	5	10	17	47
Incentive Variables							
% of equity owned via stocks	3.42	7.30	0	.09	.43	2.61	80.06
% of equity "owned" via options	1.00	1.49	0	.14	.52	1.31	25.76
Total delta of options	.67	.30	0	.57	.77	.89	1.00
Total vega of options	11.58	11.53	0	4.00	9.49	16.44	356.34
SIC-2 Pr(delisting) (%)	5.85	3.11	0	3.95	5.57	7.28	31.25
SIC-4 Herfindahl index	1,444.0	1,306.3	224.9	594.3	1,067.0	1,812.1	10,000
Board size	9.64	2.97	3	7	9	12	22
Sigma (%)	2.212	1.064	0.440	1.430	1.960	2.78	13.990

NOTE.—For variable definitions, see table 1.

missing for the smallest tercile of firms, mainly because smaller firms (by number of shareholders) are not required to file proxies with the SEC. However, none of the results that follow are qualitatively changed if we exclude all 1992 and 1993 firm-years or if we exclude 1997.

We performed a wide range of data checks and manual data fills on both ExecuComp's and Compustat's data items (see the appendix). In general, we find the accuracy of ExecuComp's data to be extremely high, but we also find systematic lapses in ExecuComp's coverage. For instance, ExecuComp fails to flag who is CEO in 1,785 firm-years, reports no managerial stockholdings in 289 firm-years, and lacks information about option holdings in 317 firm-years. We handfilled gaps in the data where possible.

B. Descriptive Sample Statistics

A summary of our variable definitions can be found in table 1. The appendix provides additional detail. Table 2 reports means and distributional information for our variables. The average (median) firm has a Tobin's *Q*

of 1.985 (1.569). Sample firms are large, with average (nominal) sales of \$3.1 billion, although this is partly driven by the quartile of largest firms: the seventy-fifth percentile firm has sales of \$2.7 billion and the largest (Ford Motor Company) has sales of \$153.6 billion. Both R&D/K and ADV/K are right skewed and have some very large positive outliers, which spend more than their asset bases on research and development and advertising. The median company reports zero R&D and ADV expenditure. The average rate of capital formation CAPEX/K in the sample is 23.6%. The average firm has a negative operating margin, although this is heavily influenced by the 4% of firm-years in which operating income is negative. The median operating margin of 14.5% therefore is more informative. Our sample firms appear very capital intensive, given median K/sales of 0.29: they use 29 cents of tangible capital to generate a dollar of sales. The average firm has 19% leverage, with a range from 0% to 99.8% (Payless Cashways, Inc., which subsequently sought Chapter 11 protection from its creditors). Cost of capital estimates range from 5.9% to 12.7% nominal, with a mean and median just below 10%. Industry growth rate forecasts average 16.6% per annum, with a range from 2.8% to 35.7%. The average company is followed by 12 securities analysts.

The lower half of table 2 lists the incentive variables. The average CEO owns a mere 3.4% of his firm, with an even lower median of 0.4%. Not surprisingly, CEO ownership depends on firm size, averaging 6.8% in the smallest quartile and 1.1% in the largest (results not shown). Option ownership, which in the table is defined as the number of options held divided by shares outstanding, averages 1%. For the median firm, option ownership is 0.5%, higher than median CEO stock ownership. This is consistent with Murphy's (1998) finding that CEOs' option ownership has come to rival their direct equity ownership. However, these numbers are not directly comparable, for the incentive properties of an option are proportional to the option's delta, which has a median value of 0.67 in our sample. (All estimates reported hereafter use the delta adjustment.) The total *vegas* of the average CEO's option portfolio is 12, which means that a 1% change in volatility increases the value of the average option portfolio by a factor of 0.12. For comparison, Guay reports average and median *vegas* for 278 CEOs in 1993 of 16.7 and 15.6, about 40% higher than our estimates. The average firm faces a 5.9% probability of delisting in a given year, our measure of capital market pressure. Just under half the firms operate in unconcentrated industries (defined by the Federal Trade Commission as a Herfindahl index value below 1,000), a quarter in moderately concentrated industries (Herfindahl values between 1,000 and 1,800), and the remaining quarter in highly concentrated industries (Herfindahl values >1,800). The average (median) board has 9.6 (9) members, ranging from a low of 3 to a high of 22. Firm-specific risk sigma, measured as daily stock return volatility, averages 2.2%, or 34% on an annualized basis.

IV. Empirical Results

The discussion of our empirical results is structured as follows. In this section, we first estimate the benchmark function, Q^* , and show that firms do not maximize value in our sample. We then ask what determines the shortfall from Q^* in the cross section of firms. In Section V, we show that our results are robust to potential endogeneity concerns, sample partitions by size, outliers, and alternative variable definitions. Finally, in Section VI, we ask whether boards adjust internal incentives to improve performance over time.

A. Estimating the Benchmark Function

All the frontier variables, shown in the upper half of table 3, column (1), have the predicted signs. The maximum-attainable Tobin's Q decreases significantly with log sales and increases slowly with its square, with a turning point outside the range for sales in our data. It is similarly U-shaped in tangible capital-intensity K/sales with a turning point at 22.4%. The value of Q decreases significantly in leverage. We interpret this negative leverage effect as proxying for a positive relation between difficult-to-measure intangibles and Q and note that it points to debt tax shields being of second-order importance.²¹ The value of Q increases in "soft" and "hard" expenditures on research and development and capital formation, respectively, in operating margins Y/sales and in industry growth rate forecasts. It also increases in analyst following. Utility companies have significantly higher Q values, on average, than nonutility companies, consistent with the notion that regulation acts as a barrier to entry. The Q frontier appears to be invariant to advertising spending and to our measure of the cost of capital.

B. Do Sample Firms Maximize Value?

If firms maximize value, the one-sided error terms u will be zero. The diagnostics panel of table 3 reports a likelihood ratio test of this null hypothesis, which we comfortably reject ($p = 0.1\%$). Therefore, in our sample, firms do not maximize value, on average.²²

How large are the shortfalls from Q^* ? As explained earlier, this can be measured using the predicted values, \hat{u}_{it} , normalized to lie between 0 and 1 by taking the ratio of a firm's actual Q to the corresponding $Q^* = Q + u$ if it were to maximize value: $\widehat{PE}_{it} = E(Q_{it}|\hat{u}_{it}, X_{it})/E(Q_{it}^*|\hat{u}_{it} = 0, X_{it})$. The average predicted efficiency is 83.8%, meaning that the average firm underperforms the frontier by around 16%. Translated into dollars, this implies that the market value of the average firm would be \$1,432 million

21. Agrawal and Knoeber (1996) also find a negative relation between leverage and Q .

22. Since the u are not zero, we expect the residuals in an OLS version of the model to be significantly right skewed, implying that the median OLS error is negative. This is indeed the case, see col. (2) of table 3.

TABLE 3 Estimating the Valuation Benchmark and Testing for Value Maximization

	Whole Sample			Unregulated			Utilities					
	SFA (1)			OLS (2)			SFA (3)			SFA (4)		
	Coeff.	<i>t</i> -Stat.		Coeff.	<i>t</i> -Stat.		Coeff.	<i>t</i> -Stat.		Coeff.	<i>t</i> -Stat.	
Frontier Variables												
Constant	2.793	17.916		2.724	13.51		2.644	11.064		2.785	10.706	
ln(sales)	-.311	-10.652		-.316	-10.920		-.314	-9.527		-.209	-3.000	
ln(sales) ²	.009	4.434		.009	4.480		.009	3.957		.011	2.429	
R&D/K	.121	6.685		.123	6.670		.105	4.981		1.423	15.738	
ADV/K	-.002	-.059		-.003	-.130		-.013	-.401				
CAPEX/K	1.227	13.809		1.231	13.29		1.206	10.772		.713	5.694	
Y/Sales	.012	3.616		.012	3.720		.010	1.930		.535	6.244	
K/Sales	-.118	-5.343		-.119	-5.260		-.146	-4.425		-.105	-2.795	
(K/Sales) ²	.003	4.777		.003	4.740		.003	4.217		.010	1.508	
Leverage	-1.983	-23.906		-1.980	-23.320		-2.071	-19.603		-1.212	-14.187	
Cost of capital	-.003	-.227		-.003	-.230		-.007	-.455		-.033	-3.245	
Industry growth forecasts	.031	10.141		.032	10.340		.039	9.394		.008	3.265	
Analyst following	.365	15.126		.368	15.330		.373	10.284		.079	3.054	
Utility dummy	.129	2.707		.126	2.580							

Incentive Variables (SFA coefficients measure distance from frontier, so signs are reversed relative to OLS)									
Constant	.082	1.123			.104	.710	2.53	1.966	
CEO stockholdings	-.027	-11.897			-.033	-4.932	-.033	-3.734	
(CEO stockholdings) ²	.0005	10.762			.001	3.873	.002	4.716	
CEO option holdings	.065	4.346			.059	3.301	-.045	-1.693	
(CEO option holdings) ²	-.004	-2.114			-.003	-3.594	-.002	-.495	
Vega of options	-.012	-12.120			-.010	-10.459	-.006	-1.519	
Capital market pressure	.098	.375			.005	.035	-2.496	-4.989	
Product market pressure	.397	4.188			.449	4.524	.355	2.079	
Board size	.026	1.444			.012	.619	.022	1.04	
Board size ²	-.001	-1.441			-.001	-1.022	-.001	-.799	
Sigma	.005	7.395			.006	8.029	-.001	-.080	
Diagnostics									
Likelihood ratio test of $u = 0 (\chi^2)$	154***				147.7***			72.4***	
Mean predicted u (as % of Q^*)	83.8				87.0	71.998		.083	20.904
$\sigma^2 = \sigma_u^2 + \sigma_v^2$	1.070	56.084			1.209	1.655		.005	7.056
$\gamma = \sigma_u^2 / \sigma_v^2$.000	.385			.000003				
Adjusted R^2 (%)									
All coeff. = 0 (F -test)									
Skewness in residuals (p -value)									

35.7
118.3***
<.001

TABLE 3
(Continued)

	Whole Sample		Unregulated		Utilities	
	SFA (1)	OLS (2)	SFA (3)	SFA (4)		
	Coeff.	Coeff.	Coeff.	Coeff.	t-Stat.	t-Stat.
DWH Endogeneity Tests						
CEO stockholdings (<i>F</i> -test)		.00				
CEO option holdings (<i>F</i> -test)		.02				
<i>Vega</i> of option (<i>F</i> -test)		.29				
Board size (<i>F</i> -test)		.70				
Sample Size						
No. firm-years	7,134		6,188	946		
No. firm	1,307		1,135	172		
Max no. panel-years	6		6	6		

NOTE.—Columns (1), (3), and (4) present stochastic frontier models estimated using maximum likelihood. The model in column (2) is estimated using ordinary least squares. The dependent variable in all models is Tobin's Q . Analyst following is the natural log of 1 plus the number of analysts following the stock. The Herfindahl index is normalized to have a maximum of 1.0 = monopoly. The firm-level volatility sigma is expressed in percent. The models also include two dummies taking the value 1 if data are missing for R&D/K or ADV/K, respectively. The coefficients are generally not significant and are not reported. For all other variable definitions see table 1. The Durbin-Wu-Hausman endogeneity tests are estimated by including in regression (2) the residuals of auxiliary regressions of the four potentially endogenous variables on all the exogenous variables in the system. To ensure the auxiliary regressions are identified, we include four instruments: log CEO age, the dividend yield, a dummy identifying CEOs who founded the company, and a dummy for newly appointed CEOs. These correlate significantly with the potentially endogenous variables but not with Q (an F -test of their joint significance in the Q model gives 2.01, with a p -value of 0.091). In columns (3) and (4), companies are sorted into two groups: utilities (two-digit SIC codes 40, 48, 49) and unregulated industries. All SFA diagnostics are as defined in Section II. One, two, and three asterisks indicate significance at $p < 5\%$, $p < 1\%$, and $p < 0.1\%$, respectively.

TABLE 4 Predicted Efficiencies by Empirical Specification and Sample Characteristics

	No. Obs.	Mean	SD	Minimum	Q1	Median	Q3	Maximum
All firms	7,134	83.8	9.6	2.1	80.3	84.8	88.8	100.0
By year								
1992	1,170	83.8	10	19.2	79.8	84.7	89.2	100.0
1993	1,299	84.5	8.9	14.7	81.1	85.2	89.3	100.0
1994	1,280	83.7	9.4	7.1	80.4	84.8	88.5	100.0
1995	1,237	83.7	9.5	22.4	80.1	84.8	88.6	100.0
1996	1,198	83.4	10.3	2.1	80.1	84.8	88.5	100.0
1997	950	83.9	9.4	11	80.4	84.7	88.6	100.0
By size								
Small	2,286	85.6	8.6	2.1	81.9	86.1	90.6	100.0
Medium	2,390	82.4	10.2	14.4	79.3	83.9	87.5	100.0
Large	2,458	83.7	9.6	7.1	80.0	84.6	88.6	100.0

NOTE.—Predicted efficiencies by year and size are derived by partitioning the cross-section of predicted efficiencies for the sample as a whole from table 3, col. (1). Predicted efficiencies are expressed in percent. For the size partition, companies are sorted into terciles on the basis of their net sales in the first panel year.

higher were it to move to the frontier.²³ This can be viewed as a measure of agency costs, for there would be no systematic shortfall from the frontier if there were no agency problem and all managers were to maximize firm value. The median firm has a predicted efficiency of 84.8%, and the interquartile is 80.3–88.8%.

In table 4, we report distributional characteristics of the predicted efficiencies by year and size. For the size partition, companies are sorted into terciles on the basis of their net sales in the first panel year. A performance shortfall appears to be present in all years and among companies of all sizes.

The shortfall from the frontier we estimate for the 1,307 largest listed companies in the United States is in line with extant stochastic frontier results for individual industries. Berger and Mester (1997) report an average shortfall of 20% in the U.S. commercial banking industry; Altunbaş et al. (2001) report a shortfall of the same order in European banking; Anderson et al. (1999) report a shortfall of 12% in the U.S. hotel industry; and Trip et al. (2002) report a shortfall of 16% among Netherlands greenhouse chrysanthemum growers. Perhaps no less important, Hoffer and Payne (1997) report an average shortfall of 11% among the teams of the National Basketball Association.

The preceding studies use a wide variety of input and output measures. For example, the Hoffer and Payne (1997) study uses wins as an output and the ratios of field goals, free throws, offensive and defensive rebounds, assists, and steals as inputs. The Trip et al. (2002) study uses

23. The difference between a firm's actual Q and its frontier Q^* , multiplied by the replacement value of its assets, gives the increase in the firm's market value were it to move to the frontier.

turnover per square greenhouse meter as an output and the date of construction of the greenhouse, the area of supplemental lighting, and hours of work per square greenhouse meter as inputs. Anderson et al. (1999) and Altunbaş et al. (2001) are studies of cost minimization. The former uses total hotel costs as an output and total revenues and a variety of prices, such as average employee wage and average room rate, as inputs. The latter uses total bank costs as an output and a variety of production measures (aggregate loans, off-balance-sheet activities) and prices (personnel expenses, interest rates) as inputs. Last but not least, Berger and Mester (1997) conduct an exhaustive study of the different approaches to measuring cost minimization and profit maximization (data envelopment analysis, stochastic frontier analysis, distribution-free approach), using a variety of functional forms (translog, Fourier flexible). They share many variables with Altunbaş et al. (2001).²⁴

The widespread finding of a shortfall from the frontier, for a wide variety of input and output measures, in a wide range of industries, may indicate the shortfall is but a statistical artifact; that is, more a reflection of SFA's failure to identify the efficient frontier correctly than evidence of systematic departure from the frontier. To determine whether shortfalls from Q^* are indeed systematic, we investigate the time series behavior of the predicted efficiencies, $\hat{P}E_{it}$. If the cross section of firms' positions relative to the frontier were random rather than systematic, there would be no reason to expect it to remain stable over time; and we would expect no correlation from year to year in firms' predicted efficiencies. Under the alternative hypothesis of a systematic shortfall, we would expect persistence in the shortfall from year to year and possibly reversals over longer periods (as boards take action to reduce the shortfall in performance). Table 5 shows a correlogram of the predicted efficiencies. There is clear evidence of significant positive correlation across all lags, consistent with persistence in (the shortfall in) performance. We therefore are unlikely to be picking up random movements in the shortfall. The correlations tend to decline with longer lags. In Section VI, we investigate whether changes in the shortfall over time are related to board actions.

24. Unlike Altunbaş et al. (2001), Berger and Mester (1997) relate the shortfall from the frontier to various variables, including monitoring and incentive variables. They find no relation between the shortfall and managerial stockholdings, suggesting that these have been chosen optimally. This is in contrast to our results, shown later, and may be because of the reasons discussed in the sixth paragraph of the Introduction. None of the other studies relates the shortfall from the frontier to measures of monitoring and incentives, but the Trip et al. (2002) study documents a negative relation between the shortfall and a measure of the quality of the decision-making process used by growers. The study does not address the question of why some growers should use low-quality decision-making processes. This may be evidence of X-inefficiency (Leibenstein 1966) or amenities consumption by owner-growers (Demsetz 1989), where the amenity consists in the leisurely adoption of process innovations.

TABLE 5 Correlogram of Predicted Efficiencies

Predicted Efficiency	1996	1995	1994	1993	1992
1997	82.5***	61.0***	59.9***	20.9***	54.3***
1996		71.1***	66.0***	27.9***	55.2***
1995			84.8***	35.9***	65.7***
1994				41.5***	78.5***
1993					88.5***

NOTE.—Pairwise correlations are expressed in percent. One, two, and three asterisks indicate significance at $p < 5\%$, $p < 1\%$, and $p < 0.1\%$, respectively.

C. What Determines the Shortfall?

Does the extent of the shortfall depend on the strength of managerial incentives, as captured by our Z variables in equation (4)? The Z coefficients are shown in the middle part of table 3, listed under the heading Incentive Variables. In interpreting the coefficients, recall that $Z\delta$ enters the SFA equation negatively. A negative δ therefore indicates that the shortfall u_{it} can be decreased by increasing the value of the corresponding variable Z_{it} .

Overall, our Z variables are very successful at accounting for shortfalls Q^* : γ , which measures the relative importance of the unexplained part, w_{it} , of equation (4) and the overall error of the SFA regression, is very close to zero and not statistically significant (see col. [1]).

With the exception of capital market pressure and board size, all coefficients are statistically significant. The coefficient of CEO stockholdings is negative, indicating that CEOs own too little equity: the shortfall could be decreased by increasing their stockholdings. The coefficient of the square of CEO stockholdings is positive and highly significant, indicating concavity in the relation between stockholdings and the shortfall from the frontier. This inverse U-shaped relation between CEO ownership and Q mirrors the results of McConnell and Servaes (1990). It contrasts with Himmelberg et al. (1999), who find no relation between managerial stockholdings and Q in the 10 years prior to our sample period.

To illustrate the economic magnitude of the effect in our data, we compute the change in Tobin's Q for a 1 standard deviation increase from the mean of stockholdings, holding all other variables at their sample means. This increases Q from 1.985 to 2.164. Since Q gives the multiple at which a dollar of assets trades in the market, we can translate this into dollar changes in market value. The average firm has assets of \$3,613 million, so each 0.01 increase in Q increases its market value by \$36.1 million. Increasing CEO stockholdings by 1 standard deviation from the sample mean therefore increases market value by \$646.7 million, all else equal.²⁵

25. These point estimates are meant to be crude illustrations only. Clearly, they suffer from at least two shortcomings that likely cause the economic effect to be overstated. (1) The estimates do not adjust for the cost of changing incentives (such as dilution when awarding restricted stock). (2) All else presumably will not remain equal: as Ofek and Yermack (2000) show, changes in one incentive variable can trigger countervailing changes in another.

The coefficients estimated for option holdings and its square have the opposite signs to those estimated for stockholdings: CEOs appear to own too many options from the point of view of maximizing Q . A 1 standard deviation increase in CEO option holdings from the mean, for the average company, decreases Tobin's Q from 1.985 to 1.909, equivalent to a fall in market value of \$274.7 million. CEOs simultaneously own too few stocks and too many options.²⁶

Given our finding that CEOs hold too many options, do their options at least induce optimal risk taking? The negative and significant coefficient estimated for *vega* suggests they do not: the companies closest to the frontier are those that have awarded options with high *vegas*. A 1 standard deviation increase in *vega* from the sample mean raises Q from 1.985 to 2.069, corresponding to a \$303.2 million increase in market value for the average firm.

Capital market pressure, as measured by the probability of delisting, has a small but positive effect on the shortfall, contrary to our prediction, but it is statistically insignificant.

An increase in product market competition significantly reduces the shortfall, in line with Schmidt (1997). The effect is large: firms operating in "unconcentrated" industries, as defined by the Federal Trade Commission, have values of Tobin's Q that are on average 0.099 higher than firms operating in "highly concentrated" industries, corresponding to a \$356.5 million difference in market value. No doubt part of the difference is due to factors we have not controlled for. Still, all else equal, competition appears to have a considerable effect on performance.

The shortfall increases in board size and decreases in its square, but neither coefficient is statistically significant. Moreover, the effect is economically small, with a 1 standard deviation increase in board size having almost no effect on Q .²⁷ This finding is consistent with board size having been chosen optimally by shareholders.

Finally, the shortfall increases significantly in idiosyncratic risk, σ . This is consistent with the prediction that idiosyncratic risk adversely affects the extent to which a risk-averse manager can be incentivized via stockholdings.

D. SFA versus OLS

Table 3 also reports the results of estimating our empirical model using OLS (see col. [2]). The regression has high explanatory power (the

26. If we use the sum of stock and option holdings (adjusted for delta and thus comparable to equity) instead of the individual variables in the OLS or SFA regressions, we continue to find suboptimality: CEOs have too small a claim on their firms through the combination of stocks and options.

27. Our specification for board size differs from that of Yermack (1996), who uses the log of the number of board members rather than the level and square. Using his specification, we continue to find that inefficiency is unrelated to board size ($t = 0.039$). All our other results remain qualitatively (and, largely, quantitatively) unchanged.

adjusted R^2 is 35.7%). Except for the intercept, the OLS and SFA coefficient estimates are very close. This is not surprising, for asymptotically both will give the same coefficient estimates in case all departures from the frontier have been explained ($\gamma = 0$).²⁸ But, unlike OLS, SFA also gives an estimate of shortfalls from the value-maximization benchmark, Q^* .

E. Utilities versus Unregulated Firms

The sample contains 172 utility companies whose economic behavior may differ from that of other firms. The SFA model discussed so far controls for this by including a dummy variable for utility firms among the X variables. This may capture differences in the average Q of utilities and unregulated firms but does not allow for potential differences in the effects of the individual X and Z variables. We therefore partition the sample into utilities (two-digit SIC codes 40, 48, and 49) and unregulated firms and estimate individual stochastic frontiers for each subsample; see columns (3) and (4) in table 3, respectively. (We exclude ADV/K from the model for utilities as utilities report no advertising expenditure.)

We find no major differences in the frontier variables between the sample as a whole (col. [1]) and the subsample of unregulated firms (col. [3]). Comparing the subsamples of utilities (col. [4]) and unregulated firms (col. [3]), the signs of the frontier variables are the same, even though the magnitudes of some of the coefficients differ. For instance, operating margins and spending on R&D have larger effects on Q for utilities, while leverage, spending on CAPEX, and analyst following have smaller effects. The negative effect of the cost of capital on Q , not significant in the overall sample or among unregulated firms, is highly significant among utilities.

In both subsamples, firms fail to maximize Q on average, but the average utility has a slightly lower predicted efficiency (83.2%) than the average unregulated firm (87.0%) and the interquartile range is lower for utilities (78.6%–87.8%) than for unregulated firms (83.7%–92.0%). The lower predicted efficiency of utilities is perhaps not unexpected, given the restrictions on competition that often accompany regulation (note the smaller coefficient on product market pressure).

We note that the lower predicted efficiency of utilities is not inconsistent with the positive coefficient on the utility dummy reported in column (1) of table 3. This is because the coefficient on the utility dummy is obtained from the pooled sample of unregulated firms and utilities, whereas the predicted efficiencies just discussed are obtained from the separate subsamples of unregulated firms and utilities. Utilities on average may be more profitable than unregulated firms, yet the difference in

28. If $\gamma > 0$, it can be shown that δ_{OLS} will be biased, for the Z variables will then correlate with the error term w (which has distribution $N(0, \sigma_w^2)$ with upper truncation at $-Z\delta$). Since $\gamma = 0$ for most of our results, this potential bias of OLS is not evident in our data.

profitability between the most profitable utility and the average utility may be larger than that between their unregulated counterparts.

The coefficients estimated for the incentive variables in the subsample of unregulated firms (col. [3]) are virtually identical to those in the sample as a whole (col. [1]). In the subsample of utilities (col. [4]), on the other hand, there are four important differences. First, while we still find that managers own too little equity, the coefficients estimated for option holdings, its square, and *vega* are statistically insignificant. Second, the coefficient estimated for capital market pressure switches sign and becomes significant. In other words, an increase in the likelihood of delisting is associated with substantially better performance. To illustrate, a 1 standard deviation increase in this likelihood is associated with a 0.06 increase in Q , equivalent to an increase in market value of \$227 million for the average utility. Third, idiosyncratic risk (as measured by sigma) has a strongly positive effect on the shortfall in performance among unregulated firms, but for utilities, the effect is negative, small, and not significant. Finally, note that the estimate of γ , although small, is statistically significant, so our set of Z variables does not fully capture all the determinants of the shortfall among utility companies.

Perhaps, the preceding results can be explained as follows. First, regulation may constrain the incentives that can be offered to utility managers, especially as regards relatively new incentive schemes such as options. Second, the regulatory restrictions on product market competition among utilities may shift competition to the market for corporate control. Third, there may be little idiosyncratic risk in utilities, especially those regulated on a rate-of-return basis. Finally, one plausible omitted variable is the intensity of regulatory pressure, which could well differ from state to state.

F. Summary and Discussion

In locating the stochastic frontier, we find results that mirror those of earlier studies: Q first decreases then increases with firm size and tangible capital intensity; increases in soft (R&D) and hard (capital-formation) spending, operating margins, forecasts of industry growth, and analyst following; and decreases in leverage. We can comfortably reject the null that all firms maximize value ($u = 0$). The \$1,432 million shortfall from the average firm's potential market value appears first order economically. The time-series behavior of firms' predicted efficiencies is much more consistent with systematic rather than random shortfalls from Q^* . Utilities are somewhat more prone to depart from the frontier than are unregulated firms.

In relating the shortfall from Q^* to the internal and external incentives CEOs face, we find that CEOs own too few stocks. This mirrors the findings of Morck et al. (1988) and McConnell and Servaes (1990) but is in contrast to those of Agrawal and Knoeber (1996), Loderer and Martin (1997), Cho (1998), Himmelberg et al. (1999), Demsetz and Villalonga (2001), and Palia (2001). The latter series of papers differ from the former

in the adjustment they make for the endogeneity of managerial stockholdings. Himmelberg et al., for example, use firm fixed effects to mitigate potential biases caused by omitted variables. If we follow this approach (not shown), we still find that CEOs own too few stocks. In other words, we find no evidence in our sample for Himmelberg et al.'s argument that unobserved but time-invariant heterogeneity causes OLS to be biased.²⁹ (See Zhou, 2001, for a critique of the use of firm fixed effects in the present context.) We perform a direct test for the endogeneity of managerial stockholdings in Section V.B.

Idiosyncratic risk adversely affects the extent to which boards can incentivize managers using equity-based compensation. Consistent with this prediction, we find that the shortfall from Q^* increases in idiosyncratic risk.

In addition to stockholdings, we investigate the effects of CEO option holdings on performance. As far as we know, we are the first to do so. Our results indicate that the CEOs of unregulated firms own too many options and these options are insufficiently sensitive to risk. We also show that product market competition improves firm performance. A priori, its effect is ambiguous: greater competition may improve incentives but reduces supernormal profits. Our results indicate that the incentive effect dominates the rent effect. We show that the industry-adjusted probability of delisting has no discernible effect on performance for unregulated firms but a strongly performance-increasing effect for utilities. Finally, we find that board size does not affect performance. This could imply either that board size has been chosen optimally by shareholders or that it is irrelevant in explaining departures from the frontier. In Section VI, we investigate the reaction of boards to performance shortfalls to shed further light on the role of board monitoring.

V. Robustness Checks

Before we ask whether boards react to a shortfall in performance by restructuring CEOs' incentives, we provide a range of robustness checks. These investigate the classification of the variables as X or Z variables, possible endogeneity biases, and control for size, outliers, and alternative definitions of equity incentives.

29. The OLS and fixed-effects coefficient estimates for CEO stockholdings are very close and indeed not significantly different from each other in a Wald test. This is what we would expect if the covariance between CEO stockholdings and the fixed effects were zero, because the bias in OLS is proportional to that covariance: $plim_{N \rightarrow \infty} \hat{\delta}_{k,OLS} = \delta_k + \text{cov}(z_{it}, \alpha_i) / \sigma_z^2$, where δ_k is the true parameter to be estimated, z_{it} is the k th element of Z (here, CEO stockholdings), and α_i is firm i 's fixed effect. (The expression for $plim_{N \rightarrow \infty} \hat{\delta}_{k,OLS}$ assumes $\text{cov}(x_{it}, z_{it}) = 0$.) In our data, the second term in the $plim$ equals 0.009 with a p -value 0.43, so it is not surprising that it does not matter whether we include fixed effects for the purpose of investigating the provision of CEO stock incentives.

A. Classification

As discussed in Section II.E, the distinction between what is a frontier or an input variable (X) and what is an incentive variable (Z) is, to some extent, arbitrary. Experimenting with alternative specifications, we find that neither coefficient estimates nor their significance changes appreciably when we change the way we classify variables as X and Z . This is largely to be expected: we saw in Section IV.D that the SFA estimates are very close to the OLS estimates, and there is no distinction between X and Z variables in OLS.

The classification of variables does, however, affect predicted efficiencies, because the more variables are considered inputs, the fewer can be ascribed to a departure from value maximization. For instance, predicted efficiencies average 86.9% when we classify capital and product market pressure as X variables instead of Z variables. However, in this specification, we would reject the hypothesis that our set of remaining Z variables fully account for the shortfall in performance, indicating that this is a worse model.

B. Endogeneity

To test for possible bias caused by the endogeneity of the incentive variables with respect to Q , we use a Durbin-Wu-Hausman test (see Davidson and MacKinnon 1993). The test is formed by including the residuals of each potentially endogenous right-hand-side variable, as a function of all exogenous variables and a set of instruments, in a least-squares regression of the original model (here, of Q on all X and Z variables). We treat CEO stock and option holdings, *vega*, and board size as endogenous but take the external incentive variables, capital and product market pressure, to be exogenous in the sense of being outside the board's control. We thus require four auxiliary regressions for the Durbin-Wu-Hausman test.

Identification requires at least four exogenous variables that are not also included in the original Q model. We use CEO age (in logs), the dividend yield, a dummy identifying CEOs who founded the company, and a dummy for newly appointed CEOs. Econometrically, these are valid instruments in that they correlate with the endogenous variables but not with Q (an F -test of their joint significance in the Q model gives 2.01, with a p -value of 0.091). Economically, they can easily be motivated. For instance, new and younger CEOs reasonably own fewer stocks and options than older CEOs, founders keep their boards smaller, and higher dividend yields make owning options less attractive (unless the options are dividend protected).

The Durbin-Wu-Hausman test will reject the null of no endogeneity bias when the coefficients on the residuals from the auxiliary regressions are significantly different from zero in the Q model. If the tests do reject,

we ought to use instrumental variables, for otherwise our estimates would be inconsistent. The test statistics, reported in table 3, do not indicate that endogeneity bias is a concern in our data set.³⁰

C. Size Effects

In table 6, we report the results of estimating stochastic frontiers individually in size terciles, formed by sorting firms into terciles based on their net sales in the first panel year. This reveals some interesting patterns in the frontier variables. The U-shaped relation between size and Q is reversed among large firms: Q first increases then decreases in log sales. Among small firms, Q decreases monotonically in log sales. Spending on CAPEX increases Q only among small and medium-sized firms. Spending on advertising, which in the sample as a whole was insignificant, increases Q for the large firms and decreases Q for the medium-sized companies. Industry growth rate forecasts do not correlate with Q among medium-sized firms, and analyst following, although significant throughout, has the largest effect among small companies. Our measure of the cost of capital has the predicted negative effect on Q among large and medium-sized companies, significantly so for the latter.

As the likelihood ratio tests show, $u > 0$ in all terciles, indicating that firms fail to maximize Q in all size groups. The insignificant γ indicates that our set of incentive variables captures the main reasons for the shortfall in performance in all three terciles.

The signs for CEO ownership, option holdings, vega, and sigma are the same as in table 3, where we used the whole sample, even though there are differences in magnitude and significance. Specifically, the lack of effort incentives in the form of stockholdings is strongest among the smallest firms. Using 1 standard deviation increases in stockholdings from the mean to illustrate the economic magnitude of the coefficients, Tobin's Q increases by 0.467 among small companies, versus 0.149 among medium-sized, and 0.163 among large companies. The corresponding implied changes in market value are \$862 million, \$498 million, and \$1,360 million, respectively.

The result of excessive option holdings in table 3 appears to be concentrated among medium-sized companies, where we continue to find that the shortfall increases with option holdings and decreases with its square. Economically, a 1 standard deviation increase in option holdings from the mean would correspond to a decrease in market value of \$224 million among medium-sized companies. Among the smallest and

30. Of course, this finding does not mean that the incentive variables are exogenous, only that no statistically significant bias arises from their endogeneity. As Davidson and MacKinnon (1993, p. 239) write, "what is being tested is not the exogeneity or endogeneity of some components of X , but rather the effect on the estimates of β of any endogeneity that might be present. The null hypothesis is that the OLS estimates $\hat{\beta}$ are consistent, not that every column of X is asymptotically independent of u ."

TABLE 6 Stochastic Frontier Estimates by Size Tercile

	SFA					
	Smallest Tercile		Medium Tercile		Largest Tercile	
	Coeff.	<i>t</i> -Stat.	Coeff.	<i>t</i> -Stat.	Coeff.	<i>t</i> -Stat.
Frontier Variables						
Constant	2.683	7.966	3.038	3.808	-2.774	-2.851
ln(sales)	-.284	-6.236	-.570	-3.360	.865	3.871
ln(sales) ²	-.012	-2.487	.043	2.680	-.046	-3.648
R&D/K	.048	1.769	.421	6.221	.189	2.292
ADV/K	-.018	-.205	-.054	-2.561	.401	6.082
CAPEX/K	1.445	8.026	.633	6.370	-.001	-.009
Y/sales	.013	2.518	5.278	57.032	3.597	17.934
K/sales	-.126	-3.104	-1.020	-27.206	-.571	-6.363
(K/sales) ²	.003	3.660	.110	7.163	.095	3.358
Leverage	-2.659	-10.329	-1.231	-12.672	-1.495	-15.967
Cost of capital	.029	1.016	-.031	-2.389	-.005	-.428
Industry growth forecasts	.041	6.567	.001	.354	.017	4.774
Analyst following	.425	8.066	.207	7.460	.157	4.806
Utility dummy	.298	2.165	-.206	-3.493	-.259	-4.355
Incentive Variables (SFA coefficients measure distance from frontier, so signs are reversed relative to OLS)						
Constant	.166	.128	.152	1.536	-.147	-.772
CEO stockholdings	-.060	-2.220	-.025	-7.292	-.033	-6.754
(CEO stockholdings) ²	.001	2.031	.0005	4.817	.0003	1.911
CEO optionholdings	.061	.925	.056	3.997	.026	.602
(CEO optionholdings) ²	-.004	-.477	-.003	-2.060	.009	1.513
Vega of options	-.068	-2.747	-.028	-5.430	-.003	-1.985
Capital market pressure	-1.105	-1.102	1.114	1.144	-.465	-1.226
Product market pressure	-2.132	-.724	.299	5.596	.414	4.365
Board size	.154	.437	-.085	-3.595	.03	.989
Board size ²	-.012	-.529	.004	1.383	-.0003	-.261
Sigma	4.266	4.262	.056	.116	1.634	.730
Diagnostics						
Likelihood ratio	29.6**		62.6***		143.3***	
Test of $u = 0(\chi^2)$						
$\sigma^2 = \sigma_v^2 + \sigma_u^2$	1.987	20.688	.585	65.171	.374	33.636
$\gamma = \sigma_u^2/\sigma^2$.046	1.240	.000	.082	.000	.178
No. firm-years	2,286		2,390		2,458	
No. firms	436		436		435	
Max no. panel years	6		6		6	

NOTE.—The dependent variable is Tobin's Q . All explanatory variables are as defined in table 3. As in table 4, companies are sorted into terciles on the basis of their net sales in the first panel year. One, two, and three asterisks indicate significance at $p < 5\%$, $p < 1\%$, and $p < 0.1\%$, respectively.

largest companies, the signs still indicate that CEOs own too many options, but the coefficients are not significantly different from zero.

The shortfall is negatively and significantly related to *vega* in all size terciles. The economic magnitude is largest among the smallest companies, where a 1 standard deviation increase in *vega* would increase Q by 0.152 (equivalent to a \$281 million increase in market value). For medium-sized and large companies, the corresponding increase in Q would be 0.081 (\$271 million) and 0.028 (\$232 million), respectively.

Increases in firm-specific risk significantly increase the shortfall for all size classes, but the effect is much the strongest among small firms.

The signs on the remaining incentives variables vary across size groups. Capital market pressure has a negative (albeit insignificant) effect on the shortfall, except among medium-sized companies, although this is not significant. Product market competition significantly improves the performance of medium-sized and large companies. Among small companies, the effect is negative but not significant.

Finally, we obtain interesting results regarding board monitoring. Among medium-sized companies, the shortfall decreases in board size and increases in its square. It reaches a minimum at 11.1 board members. Economically, the effect is large: a 1 standard deviation increase in board size from the mean (from 9.5 to 11.9) would increase Q by 0.184 (equivalent to a \$615 million increase in market value). Among the smallest and largest firms, on the other hand, we cannot reject the hypothesis that board size is optimal.

D. Outliers and Alternative Variable Definitions

We investigated the robustness of all our results with respect to outliers and measurement errors. We addressed the skewness in the R&D and advertising variables by taking logs and found our results unchanged. We tested for sensitivity to outliers by winsorizing each explanatory variable at the 1% level in each panel year. Again, our results were unchanged. Using log board size rather than the level and square did not affect our findings: except among medium-sized companies, board size does not correlate significantly with the shortfall from Q^* . Finally, we replaced our additive CEO stock and option ownership measures with the multiplicative measures advocated by Baker and Hall (1999) and discussed in note 13. This also leaves our results unchanged.

VI. Board Actions to Reduce the Shortfall

The results in Section IV indicate that internal incentives have a strong impact on the performance of the firms in our panel: companies are closer to Q^* , the greater are CEO stockholdings, the lower CEO option holdings, and the higher the *vega* of CEO option portfolios. Following Core and Guay (1999a), we investigated whether boards adjust internal incentives

to improve performance over time. We exploited the time dimension of our panel, specifically the fact that the shortfall from the frontier can change over time. Relating such changes to changes in internal incentives, we asked whether the improvement over time in a firm's performance relative to Q^* —its rate of “catch-up”—is related to changes in its internal incentives. If it is not, we would have little cause to have faith in the economic interpretation of our frontier estimates. Put differently, our results so far suggest that the cross section of firm shortfalls from Q^* are highly related to the strength of internal incentive schemes, but it would be disconcerting if the time-series behavior of firm performance were not also related to changes over time in the strength of internal incentive schemes.

We denote by $\Delta_t^{\bar{t}}$ the operator that takes the difference in a variable between a company's first panel year (\underline{t}) and its last panel year (\bar{t}). We define catch-up = $\Delta_t^{\bar{t}}$ predicted efficiency as the change in each company's location relative to the frontier, based on the predicted efficiencies tabulated in table 4. The catch-up is bounded above by 1 (for a firm that moves from a position of 0 to the frontier) and below by -1 (for a firm that drops from the frontier to 0). Over its existence in our panel, the average (median) firm maintains its position relative to the frontier. A quarter of companies move down by 4 percentage points or more, and a quarter move up by 2.8 percentage points or more. To illustrate the economic magnitude of a 1 percentage point move, we compute the corresponding increase in market value given each firm's actual Q , its Q^* , and its asset base. For the average firm, a 1 percentage point move toward the frontier is “worth” \$68 million. The rates of catch-up at the twenty-fifth and seventy-fifth percentiles thus imply economically significant changes in Q and hence market value.

To see if the degree of catch-up is related to changes in CEOs' internal incentives, we regress catch-up on the total changes in CEO stock and option holdings and the vega of their options. We also control for the firm's idiosyncratic risk using its average sigma between \underline{t} and \bar{t} as strengthening a CEO's incentives may be constrained by risk aversion. (White t -statistics are reported in italics below the coefficient estimates; all variables are expressed in percentage terms.)

$$\begin{aligned} \text{catch up} &= 0.016 + 0.549 \Delta_t^{\bar{t}} \text{stockholdings} \\ &\quad \substack{2.63 & 6.21} \\ & - 1.116 \Delta_t^{\bar{t}} \text{option holdings} \\ &\quad \substack{-4.79} \\ & + 0.173 \Delta_t^{\bar{t}} \text{vega of options} \\ &\quad \substack{-2.45} \\ & - 0.885 \overline{\text{sigma}}; \\ &\quad \substack{-2.98} \end{aligned}$$

$$\text{adjusted } R^2 = 14.4\%, \quad F\text{-test} = 21.5^{***}, \quad N = 1,307.$$

As the adjusted R^2 indicates, the regression has reasonable explanatory power. The positive and significant coefficients estimated for stockholdings and *vega* strongly support the hypothesis that internal incentives matter: it is the companies that increase these internal incentives the most that move closer to their Q^* over time.³¹ The negative and significant coefficient estimated for option holdings suggests that companies can move closer to Q^* over time by slowing the growth in managerial option holdings. This is consistent with our result that CEOs appear to hold too many options. To illustrate the economic magnitude of the effects, consider increasing CEO stockholdings and *vega* by 1 standard deviation from the mean. This would move the average company 2.9 and 1.7 percentage points closer to the frontier, respectively. A similar increase in option holdings would result in a -1.4 percentage point movement.

Why do boards adjust incentives only gradually? One possible explanation is the cost of adjusting managerial incentives. For example, it is likely that a dramatic increase in stockholdings will be resisted by a risk-averse CEO, who would thereby be required to assume much additional risk. In support of this hypothesis, we note that the coefficient estimated for σ is negative and significant.

There is an alternative interpretation for our findings.³² It is possible that changes in CEOs' stock and option holdings are determined not so much by boards seeking to adjust incentives as by CEOs buying stock in anticipation of a rise in the stock price and thus in Q and exercising stock options following a rise in the stock price. To investigate this alternative hypothesis, we replaced the explanatory variables in the catch-up regression with measures that are more nearly under a board's control. Specifically, we used the sum of newly awarded options (normalized by shares outstanding) between a company's first (\underline{t}) and last panel year (\bar{t}), the average *vega* of new option awards, and new grants of restricted stock. Unfortunately, ExecuComp reports only the value (rather than number of shares) of stock grants. A noisy measure of how much of the outstanding equity such grants represent can be obtained by dividing the value of the grant by the market value of the firm's equity at year-end. This is a noisy measure because the two variables are valued on different dates.

We found that firms' rates of catch-up over the period decreased in the number of new options their CEOs were awarded and increased in the *vega* of new option grants. These results confirm those reported earlier and suggest that, as far as options are concerned, board actions are related to changes in managerial performance. Grants of restricted stock, on the other hand, do not significantly affect the rate of catch-up. This could be

31. The results are unaffected if utilities are excluded and continue to hold in each of the three size terciles. They are also unaffected if we regress catch-up between \underline{t} and \bar{t} on the changes in stock and option holdings and *vega* up until the *penultimate* panel year ($\bar{t} - 1$).

32. We thank the referee for suggesting this alternative interpretation.

because of the noisy way we measure stock grants or because, in the main, boards have not used stock to alter CEOs' incentives. The latter possibility is consistent with the alternative interpretation that the positive correlation between catch-up and the change in CEOs' stockholdings is driven by CEOs' trading decisions.

VII. Conclusion

In this paper, we provided a direct test of the hypothesis that managers who are agents of shareholders fail to maximize firm value. Our test is based on an explicit value-maximization benchmark estimated using a stochastic frontier approach. Our empirical results can be summarized as follows. We find evidence that publicly traded U.S. companies between 1992 and 1997 systematically fell short of maximizing value on average and that shortfall in market value is economically significant: \$1,432 million for the average company. The shortfall is related in part to the inadequate provision of internal incentives. The effectiveness of the incentives we consider depends on company size and, to a lesser degree, industry. Overall, CEOs own too little stock, too many options, and their options are insufficiently sensitive to risk. For utilities, the level of option incentives appears to be optimal while equity incentives are not.

Given these findings, we asked whether boards respond to a shortfall in performance by subsequently redesigning managerial incentives. The evidence suggests that they do: it is the companies whose incentives are strengthened the most that over time improve their performance the most.

The picture that emerges is one where a substantial fraction of companies operates under suboptimal incentives at any given point in time but boards also adjust incentives dynamically, perhaps as they update their beliefs about the CEO's risk tolerance, ability, or cost of effort. Whether this picture should be viewed as evidence of serious disequilibrium, however, depends on the adjustment costs of changing incentives. If a series of small adjustments dominates a drastic and rapid change in cost terms, boards in fact may be optimizing. We believe the question of costly adjustment warrants further research.

Appendix

Variable Definitions

A summary of our variable definitions can be found in table 1. With the exception of managerial ownership, our definitions follow those of Himmelberg et al. (1999) very closely. In what follows, we detail our measures of managerial ownership, Tobin's Q and the variables not used by Himmelberg et al.

Managerial ownership. Himmelberg et al. compute managerial ownership as the sum of the equity stakes of all officers whose holdings are disclosed in annual proxy statements. In contrast, we focus on the chief executive officer. We prefer the

narrower focus, because the number of officers listed in a proxy often changes from year to year,³³ resulting in possibly spurious changes in aggregate managerial stockholdings. For instance, Bear Sterns' aggregate managerial ownership dropped from 8.4% in 1994 to 4.9% in 1997 simply due to a fall in the number of officers listed in the proxy, from seven to five. Over the same time, Bear Sterns' CEO increased his ownership slightly, from 3% to 3.2%. We recognize nonetheless that our narrower focus may entail a cost, especially where corporate performance depends on team effort. Our results are robust to adopting Himmelberg et al.'s broader focus.

Tobin's Q. We measure Tobin's Q as the sum of the market value of equity, the liquidation value of preferred stock, and the book value of total liabilities, divided by the book value of assets. For 14 firm-years, Compustat does not report total liabilities, so we use the book values of short-term, long-term, and convertible debt instead. Our measure of Tobin's Q , which we borrow from Himmelberg et al. (1999), is an approximation to the textbook definition, which would use market values rather than book values of debt in the numerator and the replacement cost rather than historic cost value of the assets in the denominator. Chung and Pruitt (1994) show that our simple Q approximates a Q based on replacement costs extremely well, with a correlation coefficient between the two in excess of 97%.

R. Fama and French (1997) argue strongly against measuring the cost of capital at the firm level due to the high degree of statistical noise in β estimates and instead provide various estimates of industry risk premia $\beta_j[R_M - R_f]$ for $j = 1, \dots, 48$ industries defined at the four-digit SIC level. After assigning our firms to Fama and French's 48 industries, we compute time-varying industry costs of capital $R_{jt} = R_{f,t} + \beta_j[R_M - R_f]$, using Fama and French's one-factor model estimates over the 5 years ending December 1994 (taken from their table 7, pp. 172–73). $R_{f,t}$ is the annualized nominal Fama-Bliss 3-month return from the Center for Research in Security Prices (CRSP) tapes, estimated in each firm's fiscal year-end month. Note that, for each industry, the Fama-French risk premium is constant across panel-years but our cost-of-capital measure varies over time due to variation in the risk-free rate.

Growth forecasts. We use security analysts' long-term growth forecasts as reported in I/B/E/S, which we aggregated by industry. Specifically, for every month between June 1992 and August 1998 (the earliest and latest fiscal year-end months in our sample), we collected the median of all long-term growth forecasts made about a particular company that month. We then computed the average of the median forecasts across all firms in a particular industry, using I/B/E/S's industry classifications. (I/B/E/S assigns every firm to 1 of about 100 industries. Firms whose business focus changes are subsequently reassigned to a new industry, without changing their historic industry assignment.) For a sample firm whose Q we observed at the end of December 199X, the relevant industry growth forecast is the average of the median long-term forecasts in that month in its I/B/E/S industry group.

Analyst following. We measured the intensity of securities analyst following as the maximum of the number of analysts reported in I/B/E/S as giving either a 1–3 year or long-term growth forecast for a given sample firm in or before its fiscal year-end month.

CEO option holdings. To measure the effort and risk properties of a CEO's option holdings, we needed to estimate option *delta* and *vega*. Using the Black-Scholes

33. Only 123 of the 1,307 sample companies report a constant number of officers in every panel year.

(1973) model as modified by Merton (1973) to incorporate dividend payouts, the *delta* and *vega* of an option equal³⁴

$$\text{delta} = \frac{\partial \text{option value}}{\partial \text{stock price}} = e^{-dT} N(Z)$$

and

$$\text{vega} = \frac{\partial \text{option value}}{\partial \text{stock volatility}} = e^{-dT} N'(Z) S \sqrt{T},$$

where d is $\ln(1 + \text{expected dividend yield})$, S is the fiscal year-end share price, T is the remaining time to maturity, N and N' are the cumulative normal and the normal density functions, respectively, and Z equals $[\ln(S/X) + T(r - d + \frac{1}{2}\sigma^2)]/\sigma\sqrt{T}$, where X is the strike price, r is $\ln(1 + \text{risk-free rate})$, and σ^2 is the stock return volatility. We used as the expected dividend yield the previous year's actual dividend yield. The stock return volatility is estimated over the 250 trading days preceding the fiscal year in question, using daily CRSP returns. In 72 firm-years, we were forced to use the concurrent (as opposed to preceding) year's volatility estimate due to lack of prior trading history in CRSP. To compute *delta* and *vega* for individual CEOs, it is necessary to reconstruct their option portfolios. This is a labor-intensive task whose details are discussed in the next subsection. The *vega* defined previously needs to be adjusted for scale. To see why, consider a CEO holding one option with a high *vega* and another CEO holding a million options with an intermediate *vega*. Whose incentives are greater? Clearly those of the latter CEO. To capture this, we multiply *vega* by the dollar value of the CEO's options.

Capital market pressure. Following Agrawal and Knoeber (1998), we estimated this as the probability of delisting in each firm's two-digit SIC industry in a given panel-year. Specifically, for a sample company whose Q we observed at the end of December 199X, the probability of delisting equals the fraction of all CRSP-listed companies in its two-digit SIC industry that were delisted between January and December 199X due to merger, bankruptcy, violation of exchange requirements, and the like. We did not attempt to distinguish between involuntary and voluntary delistings, as we do not know the motivation behind the mergers and takeovers. The justification for estimating industry-specific measures of capital market pressure is the finding of Palepu (1986) and Mitchell and Mulherin (1996) that takeover activity has a strong industry component.

Product market pressure. To measure product market pressure, we computed Herfindahl concentration indices for each four-digit SIC industry and panel year. The Herfindahl index is defined as the sum of squared market shares of each company in an industry in a given year. We computed market shares using net-sales figures for the universe of Compustat firms in 1992–97.

Board size. The board size in year t is measured as the number of directors voted onto the board of directors at the annual general meeting at the beginning of year t , as reported in that year's proxy. We ignored subsequent (within-year) changes in board size due to death, resignation, or unscheduled appointments of new directors.

34. Like previous authors, we note that the Black-Scholes assumptions, especially concerning optimal exercise, are probably violated due to managerial risk aversion and non-transferability. For suitable modifications, see Carpenter (1998).

Managerial Option Portfolios

To compute option *deltas* and *vegas*, we needed to reconstruct each CEO's option portfolio for every panel year. For options awarded during our observation period 1992–97 (which we refer to as *newly awarded options*), we knew all the necessary information: the number of options awarded, the maturity, and the strike price.³⁵ For options already held at the beginning of our observation period (old options), we knew only the number of options held,³⁶ not their strike prices or maturities. One solution, employed by Guay (1999), is to create an option history using each company's 10 previous proxy statements—a little over 13,000, in our case! A less labor-intensive alternative is to impute the strike prices of old options from the information available in ExecuComp and make assumptions about maturities. Specifically, proxies since October 1992 are required to report each executive's total number of options held and their intrinsic value (fiscal year-end share price minus strike price, multiplied by the number of in-the-money options).³⁷ From this, we can infer the average strike price of old options as $\bar{X} = S - (\text{intrinsic value})/(\text{number of old options})$. This is exact as long as all old options are in the money. Since we did not know what fractions of options were in the money, we investigated all apparently deep in-the-money ($S/\bar{X} < 0.5$) or out-of-the-money options ($S/\bar{X} > 5$). Largely, our imputed strikes turned out to be correct, reflecting for instance options awarded before a company's IPO, which often turn out to be deep-in-the-money later on.³⁸ Missing or negative imputed strike values are replaced, as in Guay (1999), by the average of the previous fiscal year's first and last share price. Regarding maturities, we partly relied on definitive information from the proxies we looked up anyway and partly assumed old options have an average of 5 years to run. We followed the 5-year rule unless the CEO continues to hold the old options for more than 5 subsequent years in a panel, in which case we increased the assumed time to maturity by 1 or more years as necessary.

Armed with the imputed strikes and assumed maturities of the old options and the actual strikes and maturities of the newly awarded options, we computed total option *deltas* and total option *vegas* for every CEO-year as follows: for every year, we computed the *vega* and *delta* of all old options still held and each option award since

35. There were a few exceptions. (1) For 32 options awards, ExecuComp fails to report the time to maturity. Hall and Liebman (1998) report that most options expire after 10 years. Assuming that options are awarded halfway through the fiscal year gives a remaining time to maturity of 9.5 years at fiscal year-end. (2) For 10 options, ExecuComp reports negative remaining times to maturity, as of the fiscal year-end. We set these times to maturity to zero. (3) For eight option awards, ExecuComp fails to report a strike price. We handfilled the missing information from proxy statements.

36. There were a large number of exceptions. In about 300 firm-years, ExecuComp reports no option information at all. We reconstructed option holdings in these years using option holdings at the next year-end, adjusted for new awards, option exercises, and stock splits during the next year. This works only where the CEO is the same in both years. Where this is not the case, we went back to proxy statements. Note that our procedure will miss options that have expired out of the money. To assess the extent of this potential problem, we spot-checked one in five of the corrections we made, finding virtually no errors.

37. In 76 cases, CEOs did hold options but ExecuComp failed to report their intrinsic value. We were able to handfill 58 of these using proxy statements.

38. Core and Guay (1999b) propose a similar solution to the problem of unobserved option portfolios and find that it is near-100% accurate compared to the more laborious full-history approach.

the beginning of the panel.³⁹ We then computed the total *vega* and total *delta* as the weighted average of the *vegas* and *deltas* of the old option holdings and the new option awards, using the number of options in each as weights. The number of options changes over time as options are exercised, but proxies do not disclose which particular options were exercised. Therefore, we assume (as do Hall and Liebman 1998) that the oldest options are always exercised first.

Data Integrity

The following remarks refer to the complete set of 1,500 S&P companies, that is, before we excluded financial services companies from the sample.

Identifying CEOs. ExecuComp fails to flag who is CEO in 1,785 years, mostly in the earlier years (980 CEOs in 1992, 472 in 1993, 166 in 1994, 117 in 1995, and 4 in 1997). We used proxy statements, 10-Ks, the Forbes CEO database, and news reports to identify incumbent CEOs in all the missing years. We also compared ExecuComp's CEO flag against ExecuComp's information about the dates at which executives assumed (and left) their positions. In total, we checked 4,324 CEO-years. This identified 50 cases where ExecuComp flagged the wrong person as the CEO, and 756 cases of midyear CEO changes, where ExecuComp flagged the individual who was CEO at year-end, as opposed to the individual who was CEO for the greatest part of the fiscal year. We corrected all these cases. We also found that ExecuComp missed 44 instances where two individuals are co-CEOs.

CEO age. ExecuComp provided age information for only 1,123 of the 2,052 CEOs in the sample, so we hand-gathered missing information using proxies, the Forbes CEO database, various S&P directories, regulatory filings accessed via EDGAR, and other sources.

CEO stockholdings. ExecuComp failed to report managerial stockholdings for 289 firm-years. Typically, this affects a CEO's first panel-year, mostly in 1992. We tried to find the relevant proxies in Disclosure and were successful in 212 cases; the remaining 77 firm-years had to be dropped.

To guard against reporting errors, we investigated all 158 large (one order of magnitude) year-on-year changes in a CEO's percentage equity stake. The (rare) errors we found ExecuComp making tend to stem from inconsistent treatment of beneficial ownership. For example, the reported ownership of the CEO of Fedders Corp. dropped from circa 10% to 0.01% simply due to ExecuComp's failure to consistently count two additional classes of shares. We also investigated all "extreme" values for CEO stockholdings (>50% of equity) and corrected one data error.

CEO option holdings. Corresponding to the problem of missing CEO stockholding information, 317 firm-years lacked information on the CEO's option holdings. We handfilled the missing option-holding information for 252 of the 317 firm-years. We also found 79 option awards that ExecuComp missed and were able to resolve some other internal inconsistencies in ExecuComp's data (such as four reports of option exercises where a CEO allegedly held no options).

39. That is, we treated old options as one award, with one (average) strike price and one time to maturity, whereas for newly awarded options, we considered the individual strikes and maturities of each award. Given the nonlinear nature of the Black-Scholes formula, the *vega* of an "average" of options does not equal the average *vega* of the individual options. Therefore, our treatment of the old options is approximate, whereas our treatment of the newly awarded options is exact.

We investigated all “unusual” option information in ExecuComp. For instance, options are typically awarded at or near the current market share price, so we investigated the 15 options with unusually low reported strike prices, relative to the fiscal year-end share price. For 10 of these, ExecuComp’s information is correct. For the remaining five, the companies awarded options not on their own stock but on the stock of unlisted subsidiaries. Since we cannot compute option delta and vega in the absence of share price information, we set these five awards to missing.

Compustat data. With respect to the Compustat data with which we measure Tobin’s Q and other firm-specific variables, we checked all missing or zero values of sales, book value of assets, and total liabilities; all missing values for research and development, advertising, and capital expenditures; and all cases of unusually large (>3) or small (<0.5) values of Tobin’s Q . We were able to handfill a small number of missing or zero Compustat values and to resolve all extreme Tobin’s Q values, using 10-Ks and information gathered from Lexis-Nexis news sources.

Research and development (R&D), advertising (ADV), and capital expenditures (CAPEX) are normalized by “net property, plant and equipment” (K). Where this was missing or zero in Compustat, we used the difference between the book value of assets and intangibles. There are about 140 such cases.

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