Firm Value and Managerial Incentives: A Stochastic Frontier Approach[1]

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Abstract

We examine the relation between firm value and managerial incentives in a sample of 1,487 U.S. firms in 1992-1997, for which the separation of ownership and control is complete. Unlike previous studies, we employ a measure of *relative* performance which compares a firm's actual Tobin's Q to the Q^* of a hypothetical fully-efficient firm having the same inputs and characteristics as the original firm. We find that the Q of the average firm in our sample is around 10% lower than its Q^*, equivalent to a $1,340 million reduction in its potential market value. We investigate what causes firms to fail to reach their Q^* and find that our firms are more efficient, the higher are CEO stockholdings and optionholdings and the more sensitive are CEO options to firm risk. We also show that boards respond to inefficiency by subsequently strengthening incentives or replacing inefficient CEOs.

1 Introduction

The separation of ownership and control has been a long-standing concern in finance. In 1932, Berle and Means predicted that the increasing professionalization of management would lead to firms being run for the benefit of their managers rather than that of their owners. In 1976, Jensen and Meckling used a principal-agent framework to analyze the conflict of interest between managers and shareholders. The subsequent literature has analyzed different mechanisms that serve to align the interests of managers with those of shareholders. Examples include the threat of hostile takeovers, career concerns, and the structure of managerial compensation contracts. A rich empirical literature has investigated the efficacy of these mechanisms. For example, Jensen and Murphy (1990) examined the relation between managerial pay and firm performance. They argued that this relation was too small to be effectual.[1]

A rather smaller literature has attempted to test directly what has come to be called the Berle and Means hypothesis: managers fail to maximize firm value where they are not themselves significant shareholders. The empirical evidence on this point is mixed. Using data from the early 1930s, the period in which Berle and Means put forward their hypothesis, Stigler and Friedland (1983) found no evidence that manager-controlled firms were less profitable than their shareholder-controlled counterparts. Using more recent data, Demsetz and Lehn (1985) found no relation between firm performance, as measured by return on assets, and ownership concentration. In contrast, both Mørck, Shleifer, and Vishny (1988) and McConnell and Servaes (1990) found a significant relation between firm value, as measured by Tobin's Q, and managerial stockholdings.

The findings of Mørck, Shleifer, and Vishny (1988) and McConnell and Servaes (1990) have recently been questioned by Agrawal and Knoeber (1996) and Himmelberg, Hubbard, and Palia (1999), who explicitly adjust their test designs for the endogeneity of managerial stockholdings to firm value. Using simultaneous equations (Agrawal and Knoeber) and firm fixed effects (Himmelberg, Hubbard, and Palia), they find no relation between firm value and managerial stockholdings and conclude that managerial stockholdings are chosen optimally.

Our paper continues this line of enquiry in that it explores the link between firm value and managerial incentives in a panel of 1,487 publicly traded U.S. companies over the years 1992 to 1997. Its contribution is three-fold. First, we employ an econometric framework that specifically tests whether companies are run efficiently. Unlike previous studies, which look at a firm's actual Q, we compute the Q^* of a hypothetical fully-efficient firm having the same inputs and characteristics as the original firm. We find that the Q of the average firm in our sample is around 10% lower than its Q^*. Translated into dollars, this means that the average firm could increase its market value by $1,340 million were it to become fully efficient. This suggests the presence of systematic inefficiency across our firms, consistent with the Berle and Means hypothesis.

Our estimates of Q^* are based on an econometric technique called stochastic frontier analysis.[2] Consider a set of firms, each of which has access to the same production inputs.

[1]For an alternative view, see Aggarwal and Samwick (1998), Garen (1994), Hall and Liebman (1998), Haubrich (1993), John and John (1993) and others. See Murphy (1998) for a review.

[2]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

Clearly we would not expect all firms to be equally efficient, for even given the same inputs, the different managers may make different production, investment and strategic decisions, in response to the financial and other incentives they face, and on the basis of their ability, disutility of effort and risk aversion. Some firms will therefore have higher Tobin's Qs than others. The firms with the highest Qs are the most efficient and thus define points on a frontier, analogous to the microeconomic concept of a production possibility frontier. It is in the nature of a frontier that firms can only lie on the frontier or below it, but never above it. Efficiency then corresponds to all firms being on the frontier, given their inputs, whereas inefficiency corresponds to a significant fraction of firms lying below the frontier. There are no firms above the (true) frontier, though the technique allows for random noise in locating the frontier empirically. Estimating the frontier requires a comprehensive set of 'input' variables to control for firms' characteristics. We draw on the literature, and especially Himmelberg, Hubbard, and Palia (1999), in our search for relevant input variables.

Our second contribution is to investigate what causes firms to fail to reach their Q^*. Since we have already accounted for random influences on value (such as bad luck or windfalls), we assume inefficiency is caused by conflicts of 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. We distinguish between internal incentives (which are at least partly controllable by the board of directors) and external incentives (which are largely determined by the market). In investigating the efficacy of internal incentives empirically, we not only look at managerial stockholdings but also at managerial option plans.[3] Including options is appropriate for three reasons. First, Murphy (1998) documents that stock options have become increasingly widespread since the 1980s, yet their effect on firm value has not hitherto been explored. Second, a small but growing literature documents the importance of options as managerial incentives in specific cases.[4] Berger and Ofek (1999), for example, show that options, but not stocks, induce managers to refocus diversified companies voluntarily, thus reversing value-destroying diversification. Third, stockholdings and optionholdings are interdependent: Ofek and Yermack (2000) show that managers tend to reduce their direct stockholdings following option awards. Controlling for one without controlling for the other may thus bias empirical results. Lambert, Larcker, and Verrecchia (1991) argue that the value of an option alone is unlikely to capture all its incentive effects, due to the convexity of its payoff function. Following this argument, we distinguish between the effort-inducing effect of managerial optionholdings and their effect on managers' choice of project risk. As noted by Guay (1999), the former can be measured by the number of options the manager holds, whereas the latter can be measured by the sensitivity of option value to risk, or *vega*. Guay shows that *vega* is positively related to companies' investment opportunities which is consistent with boards seeking to provide

applications in finance are studies of banking efficiency (Berger and Humphrey, 1997) and a recent article on pricing efficiency in the IPO market (Hunt-McCool, Koh, and Francis, 1996).

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

[4] There is a larger literature that investigates the contribution of options to pay-performance sensitivities (as in Jensen and Murphy, 1990) and the relative mix of options, stock, and cash compensation as a function of companies' investment opportunities set. See, for instance, Hall and Liebman (1998) and Bryan, Hwang and Lilien (2000). However, this literature does not address how the use of options affects firm value.

incentives to invest in risky projects.

In addition to these internal incentives, we investigate the efficiency effects of two possible external incentives: capital market pressure (the probability of delisting in each industry) and product market pressure (the degree of competition in the each firm's primary line of business). Product market pressure 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 Tobin's Qs. On the other, firms in less competitive markets might earn higher economic rents and thus have higher Tobin's Qs.

We find that firms in our sample are more efficient, the better their internal and external incentive structures. Consistent with the Berle and Means hypothesis and the results of Mørck, Shleifer, and Vishny (1988) and McConnell and Servaes (1990), we find that inefficiency is greater, the lower are CEO stockholdings. Interestingly, the same is true for optionholdings: contrary to popular belief,[5] CEOs (in our sample) are not given enough options. This is particularly true for large companies, where suboptimal option awards appear to be the only determinant of managerial inefficiency. This may qualify the findings of Himmelberg, Hubbard, and Palia (1999), who do not include options in their tests. We also find that inefficiency decreases in option *vega*, so CEOs' options are not sufficiently sensitive to risk. These results confirm that internal incentive schemes do work in the intended way, but that not every firm provides optimal incentives at every point in time. Economically, the effects are large: all else equal, a one standard deviation increase in stockholdings from the mean would increase market value by \$1,198 million on average, while similar increases in optionholdings and *vega* would increase market value by \$775 million and \$352 million, respectively.

To illustrate the SFA approach, consider two sample companies which are comparable in their input variables and firm characteristics: Mueller Industries Inc (SIC code 335 - Metal fabricators) and Novacare Inc (SIC code 809 - Long-term health care). In 1994, the two companies had similar levels of sales, firm-specific risk, soft and hard capital spending, and leverage. But whereas Novacare's Q equalled its Q^*, Mueller Industries' was 21% below. This difference in efficiency is largely due to differences in incentives: Mueller's CEO had negligible stockholdings (0.008% of outstanding equity vs. 5.1%), modest optionholdings (1.5% vs. 4.7%) and much lower *vega*.

With regards to external incentives, we show that companies in more competitive industries are more efficient, which suggests that the incentive effect of competition dominates the rent effect of concentration. We also find evidence that capital market pressure has an ambiguous effect on efficiency. Firms appear to be more inefficient, the greater the probability of delisting, though the economic magnitude of this surprising result is small for all firms except utilities.

Our findings regarding inefficiency and its causes are robust to partitioning the sample by size and industry, as well as to standard outlier tests and alternative measures of the effort incentives provided by stocks and options (Baker and Hall, 1999).

Our third contribution is to answer a natural question raised by our empirical findings: given that there appears to be systematic inefficiency in our sample and that better incentives are linked to lower inefficiency, do boards respond to inefficiency by subsequently

[5] See for instance "The trouble with share options", *The Economist*, August 7th, 1999.

adjusting internal incentives? The answer is yes. We show that boards are more likely to award restricted stock and options, and that such awards are larger, following periods of low efficiency. We find that boards make CEOs' options more sensitive to risk the greater past inefficiency. Boards also appear to cut cash salaries in response to poor efficiency, changing the cash compensation mix away from fixed salaries towards bonuses. Finally, boards are more likely to replace a CEO aged 60 or under, the worse past efficiency. In other words, boards react to managerial inefficiency by strengthening incentives or replacing CEOs. We also show that it is the firms whose incentives are strengthened the most that increase efficiency the most over time.

Do boards react to relative efficiency or to absolute firm value? To check, we replace our measure of efficiency, the distance between Q and Q^*, by Q alone. Interestingly, Q alone is a far less effective predictor of subsequent board actions than is the distance between Q and Q^*. This suggests that boards react to firms not achieving their full-potential Q^* rather than to absolute Q.

One potential problem in testing the Berle and Means hypothesis is that incentives may not only affect efficiency, but efficiency may also affect incentives. Himmelberg, Hubbard, and Palia (1999), henceforth HHP, argue persuasively that the inclusion of fixed firm effects will help control for this endogeneity of managerial incentives, insofar as endogeneity gives rise to unobserved but time-invariant heterogeneity across firms. To borrow one of their illustrations, consider two identical firms, one of which has access to more effective monitoring technology which reduces its optimal level of managerial ownership. If the combination of managerial ownership incentives and effective monitoring achieves a higher Tobin's Q but we are unable to control for differences in monitoring technology, we would spuriously conclude that companies are more efficient, the lower managerial ownership. As HHP show, the use of repeated observations on the same set of firms (that is, panel data) allows us to remove unobserved factors such as differences in monitoring technology via fixed effects, as long as these unobserved factors do not change over time. We therefore include fixed effects in our stochastic frontier regression, as well as in our model of how boards react to past inefficiency. Like HHP, we find that unobserved firm effects are significant and therefore need to be controlled for.

As an alternative to mitigating endogeneity bias by way of fixed effects, we also investigate an instrumental-variables approach and find that our results are little changed. However, since good instruments are notoriously hard to find, we view the IV results as indicative only.

The paper proceeds as follows. Section 2 outlines our empirical approach, including a brief explanation of stochastic frontier analysis, and specifies our empirical model. Section 3 describes the data and Sections 4 and 5 present the empirical results regarding inefficiency and board reaction, respectively. Finally, Section 6 concludes.

2 Empirical approach

2.1 Stochastic frontier analysis

A conventional regression of Q on managerial stockholdings and the appropriate control variables results in the estimation of an 'average' function for Q. But a study of efficiency requires the estimation not of the average function for Q, but of the 'frontier' function for Q; that is the function that specifies the highest Q that can be achieved for a given set of inputs such as R&D, investment, managerial ability, etc. Stochastic frontier analysis allows such a frontier function to be estimated, by supplementing the conventional, two-sided, zero-mean regression error term with a one-sided error term. This second term is zero for the efficient firms that achieve the highest Q, but strictly positive for those firms that are inefficient and therefore fail to achieve as high a Q as can be achieved given their inputs. In analogy with conventional panel-data notation, we can express Q as a function of a set of explanatory variables X, firm-specific but unobservable factors η assumed to be time-invariant, and an error term ε:

$$Q_{it} = \beta X_{it} + \eta_i + \varepsilon_{it} \tag{1}$$

where $i = 1, ..., N$ and $t = 1, ..., T_i$.

It is the special form of the error term ε_{it} that enables us to detect possible departures from efficiency. Specifically, ε_{it} is composed of two terms: $\varepsilon_{it} = v_{it} - u_{it}$. The two-sided error term v_{it} denotes the zero-mean, symmetric error component that is 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 term $u_{it} \geqslant 0$ permits the identification of the frontier, by making possible the distinction between firms that are on the frontier ($u_{it} = 0$) and firms that are strictly below the frontier ($u_{it} > 0$). Of course, if all firms were on the frontier, then $u_{it} = 0$ and $Q_{it} = Q^*_{it}$ $\forall i, t$: all firms would be efficient and would achieve the highest feasible Q^* given their inputs. In that case, SFA would reduce to a conventional regression, for the average function and the frontier function would then be identical.

A measure of a firm's inefficiency or relative performance is $\widehat{u}_{it}$. Using this, we can compute firm i's predicted efficiency in year t as the ratio of its realized Q to the corresponding $Q^* \equiv Q + u$ if it was fully efficient: $PE_{it} = \frac{E(Q_{it}|u_{it}, X_{it})}{E(Q^*_{it}|u_{it}=0, X_{it})}$; for further details, see Battese and Coelli (1988). Predicted efficiencies lie between 0 and 1, with 1 being the frontier. If firm i's predicted efficiency is 0.85, then this implies that it achieves 85% of the performance of a fully efficient firm having comparable inputs.

2.2 Including fixed effects

Least-squares or standard SFA estimation of equation (1) will lead to biased coefficient estimates due to the presence of the unobserved η_i factors. If we have repeated observations of companies across time, we can estimate equation (1) consistently by either *i)* including N firm dummies or *ii)* transforming the regression into one where the variables are expressed as deviations from their within-group means. The first method is impracticable where N is large, as it is in our case, due to the size of the matrix that must be inverted. The second gives:

$$Q_{it} - \overline{Q}_i = \beta \left(X_{it} - \overline{X}_i \right) + v_{it} - \overline{v}_i - (u_{it} - \overline{u}_i) \tag{2}$$

where $\overline{Q}_i \equiv \frac{1}{T_i} \sum_{t=1}^{T_i} Q_{it}$ and $\overline{X}_i$, $\overline{v}_i$, and $\overline{u}_i$ are similarly defined. This transformation cannot be estimated by SFA, however, as $u_{it} - \overline{u}_i$ may be negative for some i and t. In order to make the use of SFA possible, we add back the 'grand mean' of each variable to (2):[6]

$$Q_{it} - \overline{Q}_i + \overline{\overline{Q}} = \beta \left(X_{it} - \overline{X}_i + \overline{\overline{X}} \right) + \overline{\eta} + v_{it} - \overline{v}_i + \overline{\overline{v}} - (u_{it} - \overline{u}_i + \overline{\overline{u}}) \tag{3}$$

where $\overline{\overline{Q}} \equiv \frac{1}{N} \sum_{i=1}^{N} \overline{Q}_i = \frac{1}{N} \sum_{i=1}^{N} \left(\frac{1}{T_i} \sum_{t=1}^{T_i} Q_{it} \right)$ denotes the grand mean of Q_{it} and $\overline{\overline{X}}$, $\overline{\eta}$, $\overline{\overline{v}}$, and $\overline{\overline{u}}$ are similarly defined. The advantage of regression (3) over regression (2) is that $u_{it} - \overline{u}_i + \overline{\overline{u}} \geqslant 0$ under the assumption that $|u_{it} - \overline{u}_i| \ll \overline{\overline{u}}$. Regression (3) can therefore be estimated by SFA.

Note that the transformations (2) and (3) leave the parameters of interest β unaffected.

2.3 Testing for and explaining departures from efficiency

We test for efficiency by assessing the significance of the likelihood gain from imposing the additional one-sided error term (Stevenson, 1980; Battese and Coelli, 1992). If $u_{it} = 0 \ \forall i, t$ then the likelihood of the SFA specification will be the same as the least-squares likelihood. But if $u_{it} > 0$ for sufficiently many i and t, then the SFA specification will lead to a likelihood gain. This likelihood-ratio test corresponds to testing whether the average and the frontier functions are identical.

A rejection of the null hypothesis of efficiency naturally raises the question of what causes inefficiency. As inefficiency is measured by the distance from the frontier u, a second regression of u on suspected causes of inefficiency can shed light on the reasons for the failure to perform efficiently and their relative importance. However, as noted by Reifschneider and Stevenson (1991), this two-stage procedure is less statistically efficient than joint maximum likelihood estimation of the frontier function and the distance from the frontier. In implementing the joint MLE, Reifschneider and Stevenson (1991) and Battese and Coelli (1995) decompose the one-sided error term u into two components, an explained component and an unexplained component:

$$u_{it} = \delta Z_{it} + \zeta_i + w_{it} \tag{4}$$

where Z_{it} is a set of incentive variables and $w_{it} \geqslant -\delta Z_{it} - \zeta_i$ denotes the unexplained component of u_{it}. We allow for the presence of unobserved but time-invariant factors ζ_i, such as managerial ability or risk aversion. We can remove the ζ_i by applying the same transformation to Z_{it} and w_{it} as we previously did to equation (1). We thus obtain:

$$\begin{aligned} Q_{it} - \overline{Q}_i + \overline{\overline{Q}} &= \beta \left(X_{it} - \overline{X}_i + \overline{\overline{X}} \right) + \overline{\eta} + v_{it} - \overline{v}_i + \overline{\overline{v}} \\ &\quad - \delta \left(Z_{it} - \overline{Z}_i + \overline{\overline{Z}} \right) - \overline{\zeta} - (w_{it} - \overline{w}_i + \overline{\overline{w}}) \end{aligned} \tag{5}$$

[6] We use the term 'grand mean' to refer to the time series and cross section mean of a variable.

For the purposes of estimating the MLE, we assume $v_{it}-\overline{v}_i+\overline{\overline{v}}\sim N\left(0,\sigma_v^2\right)$ and $w_{it}-\overline{w}_i+\overline{\overline{w}}$ is the truncation of the normal distribution $N\left(0,\sigma^2\right)$ at $-\delta\left(Z_{it}-\overline{Z}_i+\overline{\overline{Z}}\right)-\overline{\zeta}$: $w_{it}-\overline{w}_i+\overline{\overline{w}}\geqslant -\delta\left(Z_{it}-\overline{Z}_i+\overline{\overline{Z}}\right)-\overline{\zeta}$.

We refer to equation (5) as the firm fixed effects SFA specification. Note that the inefficiencies u_{it} and their determinants Z_{it} are allowed to vary over time. The specification can therefore accommodate changes in the incentives given to CEOs.

A measure of our ability to explain the determinants of inefficiency — the appropriateness of our choice of Z variables — is the variance of the transformed residual error term $w_{it}-\overline{w}_i+\overline{\overline{w}}$. The better we are able to explain departures from the frontier u, the lower will be the unexplained variance. A statistical test of the validity of our Z variables can then be based on $\gamma\equiv\frac{\sigma_w^2}{\sigma_v^2+\sigma_w^2}\in[0,1]$, that is the ratio of the unexplained error and the total error of the regression (Aigner, Lovell, and Schmidt, 1977). γ will be zero if our Z variables fully account for departures from the frontier.

2.4 SFA versus least-squares regressions

SFA allows us to ask two related questions: *i)* do firms perform efficiently and *ii)* if not, does the degree of inefficiency depend on suboptimal incentives? Previous papers dealing with the Berle-Means hypothesis have asked a somewhat different question, namely whether performance (usually Tobin's Q) depends on managerial ownership, m. To test whether managerial ownership is chosen optimally, these papers view the coefficient of m as an estimate of the partial derivative of Q with respect to m, which at the optimum must be zero. A significantly positive coefficient of m is interpreted as consistent with inefficiency: firm performance could be further increased if managerial ownership were increased. A significantly negative coefficient is interpreted as managers owning 'too much' of the firm, possibly indicating entrenchment. And finally, a zero coefficient is taken to indicate optimal managerial ownership.

We argue on economic and econometric grounds that this is a weak test of the Berle-Means hypothesis. Economically, it suffers from its narrow focus: even if the coefficient on m is zero, as in HHP and Agrawal and Knoeber (1996), there is no guarantee that CEOs are maximizing performance. Essentially, a zero slope coefficient on m is not a sufficient statistic for efficient performance when there are substitutes and complements to equity incentives that boards could use to incentivize CEOs. For instance, HHP and Agrawal and Knoeber readily admit that their research design leaves open the possibility that companies may have chosen (say) m optimally, but are inefficient in their use of (say) options. (This, incidentally, is precisely what we find amongst the largest firms in our dataset.) This implies that tests based on regression coefficients being zero will only amount to tests of efficient performance if we as econometricians start with the complete and comprehensive set of incentive tools.

Stochastic frontier analysis does not suffer from the need to be comprehensive, because it proceeds by first establishing *whether* a significant fraction of firms are inefficient and then testing how the *degree* of inefficiency is related to firm-by-firm differences in the incentive variables for which data is available: managerial ownership, use of options, board monitoring, pressure from capital, labor or product markets, and so on. This allows us to separate the test of efficiency from the test of the determinants of inefficiency: if certain firms are below the frontier, they are inefficient *for whatever reason(s)*, some or all of which we may or may not be able to capture with our choice of incentive variables.

Econometrically, a test for optimality by means of a zero slope coefficient on m is biased against rejecting the null hypothesis of optimality precisely when the null is false. To see the economic intuition for our claim, recall that economic inefficiency implies asymmetry: efficient firms achieve the frontier Q^*, inefficient firms perform below the frontier, and no firm performs above the frontier. This asymmetry has consequences for the error structure in empirical tests. Conditional on a set of control variables, the residuals of a regression with Tobin's Q as its dependent variable have a skewed distribution. The skewness in the residuals results in inefficient estimates when least-squares or similar techniques are used and reduces the power of the zero-coefficient test for optimality.[7] It is only when all firms are on the frontier and therefore efficient that the residuals will be well-behaved, allowing the true null of optimality to be correctly accepted. SFA adjusts for skewness by introducing the one-sided error term to capture potential departures from the frontier. In the presence of economic inefficiency, SFA will therefore yield more (statistically) efficient standard errors.

2.5 The empirical model

To implement the stochastic frontier approach, we need to specify the relevant X (input and firm characteristics) and Z (incentive) variables.[8] Our preferred specification includes many X variables that have been used extensively in previous analyses of Tobin's Q:

$$\begin{aligned}
Q_{it} \;=\; & \beta_1 \ln(\overset{-}{sales_{it}}) + \beta_2 \ln(\overset{+}{sales_{it}})^2 + \beta_3\, \overset{-}{SIGMA1_{it}} \\
& + \beta_4 \overset{+}{\frac{R\&D_{it}}{K_{it}}} + \beta_5 \overset{+}{\frac{ADV_{it}}{K_{it}}} + \beta_6 \overset{+}{\frac{CAPEX_{it}}{K_{it}}} + \beta_7 \overset{+}{\frac{Y_{it}}{sales_{it}}} \\
& + \beta_8 \overset{?}{\frac{K_{it}}{sales_{it}}} + \beta_9\, \overset{?}{leverage_{it}} + \beta_{10}\, \overset{-}{R_{it}} + \beta_{11}\, \overset{+}{growth_{it}} \\
& + \text{year controls} + \text{missing-value dummies} + \eta_i + \varepsilon_{it} \qquad (6)
\end{aligned}$$

where we have indicated the signs we expect using +, − and ? above the variables. We include log *sales* and its square as controls for firm size, partly because smaller firms typically have larger Qs, and partly because some of the Z variables (for instance, managerial ownership) are size-dependent. $SIGMA1$ is a measure of firm-specific risk. *Ceteris paribus*, we expect riskier firms to have lower Qs since the trade-off between the CEO's portfolio diversification and incentives sustains lower optimal managerial stock- and optionholdings than in less risky firms. 'Soft' spending on research and development (*R&D*) and advertising (ADV), and 'hard' spending on capital formation ($CAPEX$), normalized by the capital stock K, are expected to covary positively with Q. The operating margin $\frac{Y}{sales}$ is a measure

[7] See Greene (1997), pp. 309-310.

[8] Alternatively, one could leave the choice of X and Z variables unrestricted *a priori* (i.e. allow any variable to be included both amongst the X and the Z), and choose whichever specification has the highest log-likelihood. We prefer to frame the exposition in terms of input (X) and incentive (Z) variables, though we did perform unrestricted specification searches.

of profitability and therefore, possibly, market power and should thus be positively related to Q. $\frac{K}{sales}$ controls for the relative importance of tangible capital in the firm's production technology. In a Modigliani-Miller world, *leverage* should not affect capital structure. However, if tax shields are valuable or debt reduces agency problems as in Jensen's (1986) free cash flow hypothesis, 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 which are more reliant on intangible assets are likely to have lower leverage and possibly higher Qs. The net effect is therefore ambiguous. We include the cost of capital R to account for the lower market value accorded a riskier stream of cash flows: the numerator of Tobin's Q is the market value of the firm, which is obtained by discounting future cash flows at the firm's cost of capital. Declining industries have few growth opportunities and therefore low Tobin's Q, which we attempt to control for by including contemporaneous industry *growth* rates. We allow the frontier to shift over time by including year dummies. Finally, HHP suggest to deal with missing data by setting the missing values of the variable in question to zero and including a dummy which equals 1 when data is missing, and zero otherwise. This avoids having to drop firm-years where data is missing. In our sample, some values of $R\&D$, ADV, $CAPEX$, and $SIGMA1$ are missing, so we include three (4-1) dummies.

With the exception of *leverage*, R, and *growth*, our set of X variables is identical to HHP's. HHP also include managerial stock ownership, but in our notation, this is a Z variable. Our complete set of Z variables is:

$$\begin{aligned} u_{it} &= \delta Z_{it} + \zeta_i + w_{it} \\ &= \delta_1 stockholdings_{it} + \delta_2 stockholdings_{it}^2 \\ &\quad + \delta_3 optionholdings_{it} + \delta_4 optionholdings_{it}^2 \\ &\quad + \delta_5 vega_{it} + \delta_6 capital\ market\ pressure_{it} \\ &\quad + \delta_7 product\ market\ pressure_{it} + \zeta_i + w_{it} \end{aligned} \tag{7}$$

The first five variables are designed to capture 'internal incentives' which 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 including beneficial holdings. Following Baker and Hall (1999), we measure the effort-incentives of managerial optionholdings as the product of the delta of the options and the fraction of firm equity which managers would acquire if they were to exercise the options. This serves to make what are in effect potential managerial stockholdings comparable to actual managerial stockholdings in their incentive effects.[9] The total incentive effect of managerial ownership is the sum of the slope of managerial stockholdings and the slope of managerial optionholdings.[10] As in previous studies, we include squared terms for stock (and option-) holdings to allow for non-linearities in their relationship with Tobin's Q.

[9] See Baker and Hall (1999) for a formal analysis.

[10] 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.

To capture the extent to which managerial options influence choice of project risk we compute option *vegas*, which measure the sensitivity of option value to a small change in volatility. 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.'

The last two variables are based on external incentives. *Capital market pressure* is a combined measure of the *risk* of bankruptcy and takeover, both of which should act to discipline the CEO (Stulz, 1990 and Scharfstein, 1988). *Product market pressure*, a measure of the degree of product market competition, should discipline the CEO in similar ways to *capital market pressure* (Schmidt, 1997).

Our empirical model does not (for lack of data) include every conceivable incentive variable in our attempt to find the determinants of inefficiency. Fortunately, as we indicated earlier, stochastic frontier analysis does not require a complete empirical model to test for efficiency. So to the extent that our incentive variable set is incomplete, we only reduce our ability to explain departures from the frontier, not our ability to test for efficiency per se. In practice, our choice of incentive variables seems to account for most of the inefficiency we find, except amongst financial firms.

3 The data

3.1 Data and sources

Our data set 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. We verify that firms which drop out of the indices are retained in the data set unless they cease to be listed, thus minimizing survivorship bias.[11] As Standard & Poor's change the compositions of their indices, new firms are added to ExecuComp. In the October 1998 version that we use, there are a total of 1,827 firms. Since being included in an index could be a sign of 'success,' using the whole universe of ExecuComp firms available in the October 1998 version would over-represent 'successful' firms. We therefore limit our analysis to the 1,500 original 1992 panel firms. Of these, we exclude twelve firms with dual CEOs and one firm for which no Compustat data was available. This leaves a total sample of 1,487 firms.

The panel runs from 1992 to 1997 and consists of a total of 8,087 firm-years. This is 835 short of the theoretical maximum of 1,487 firms × 6 years. There are two reasons why the panel is unbalanced: attrition and missing data. 216 of the 1,487 companies delist prior to 1997, resulting in a loss of 464 firm-years (an attrition rate of 5%). Of these, 203 are taken over, eight are delisted due to violation of listing requirements, two cease trading for unknown reasons, one is liquidated, one taken private, and one demerged into three new entities. Given the low attrition rate, we do not expect attrition bias to be a problem. A comparison of the Tobin's Qs of the 203 takeover targets and the surviving firms confirms that there are no systematic differences in performance (except in 1996, when takeover targets have lower

[11] We investigate what happens to companies which ExecuComp drops completely and find that all but two of these delist.

average Q at p =1.4%). The second cause of the unbalanced nature of the panel is missing data, affecting 371 firm-years. Most of these result in companies 'leaving' our panel before 1997. For instance, 200 companies have no 1997 data on the October 1998 ExecuComp CD-ROM purely due to the timing of their fiscal year-ends. Some of the missing firm-years, however, are at the beginning of the panel (1992 and 1993) as a result of systematic gaps in ExecuComp's coverage of option and ownership information. We discuss these issues in the Data Appendix. A closer look at the companies affected suggests some nonrandomness: early firm-years are more likely to be 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.

3.2 Variable definitions

In general, our variable definitions follow those of HHP very closely. The exception is managerial ownership. HHP 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,[12] 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 7 to 5. 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.

A summary of our variable definitions can be found in Table 1. Most of these are straightforward, so we will here only discuss the definitions of our derived variables: Tobin's Q, cost of capital R, option delta and $vega$, and capital and product market pressure,

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 HHP, 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. Instead, Fama and French (1997) provide estimates of industry risk premia, based on assigning firms to 48 industries using their four-digit SIC codes and estimated in a variety of ways. After assigning our firms to Fama and French's 48 industries, we compute industry costs of capital as the sum of the riskfree rate and the Fama-French risk premium for that industry estimated in a one-factor model over the five years ending December 1994 (taken from Fama and French, Table 7, pp.

[12]Only 147 of the 1,487 sample companies report a constant number of officers in every panel year.

172-173). Our riskfree rate is the annualized nominal Fama-Bliss three-month return from the 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 that the cost of capital measure we compute varies over time due to variation in the riskfree rate.

CEO option delta and vega. Using the Black-Scholes (1973) model as modified by Merton (1973) to incorporate dividend payouts, the delta and *vega* of an option equal[13]

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

and

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

where d is $ln(1+$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 $\frac{ln(S/X)+T(r-d+\frac{1}{2}\sigma^2)}{\sigma\sqrt{T}}$, where X is the strike price, r is $ln(1+$riskfree rate$)$, and σ^2 is the stock return volatility. We use 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 82 firm-years, we are forced to use the concurrent (as opposed to preceding) year's volatility estimate due to lack of prior trading history in CRSP. To compute Δ 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 Data Appendix. The *vega* defined above 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 estimate this as the probability of delisting in each firm's two-digit SIC industry in a given panel-year. Specifically, for each two-digit SIC industry and for each panel year, we compute the fraction of all CRSP-listed companies that are delisted due to merger, bankruptcy, violation of exchange requirements etc, capturing all involuntary and voluntary delistings. 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 compute 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 compute market shares using net-sales figures for the universe of Compustat firms in 1992-1997.

We perform a number of data checks and manual data fills on both ExecuComp's and Compustat's data items. The Data Appendix provides a comprehensive summary of these.

[13]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)

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 optionholdings in 317 firm-years. We handfill these missing data points wherever possible.

3.3 Descriptive sample statistics

Table 2 reports means and distributional information for our firm characteristics (X) and incentive (Z) variables. The average (median) firm has a Tobin's Q of 1.889 (1.484) which is significantly greater than 1, possibly reflecting the long bull market of the 1990s. Sample firms are large, with average (nominal) sales of \$3.2 billion, though this is partly driven by the quartile of largest firms: the 75th percentile firm has sales of \$2.9 billion and the largest sales of \$153.6 billion. Daily stock return volatility averages 2.2%, or 34% on an annualized basis. Both $\frac{R\&D}{K}$ and $\frac{ADV}{K}$ are right-skewed and have some very large positive outliers which spend more than their asset base on research and development and advertising. The median company reports no $R\&D$ expenditure, and the 75th percentile company reports no ADV expenditure. This could be an artefact of accounting: instead of expensing $R\&D$ and ADV through the income statement, they could be capitalized on the balance sheet. This will likely weaken the empirical relationship between these variables and Q. The average rate of capital formation $\frac{CAPEX}{K}$ in the sample is 21.9%. The average firm has an operating margin of only 2.4%, though this is heavily influenced by the four percent of firm-years in which operating income is negative. The median operating margin of 15.3% is thus more informative. Our sample firms appear very capital-intensive, given median $\frac{K}{sales}$ of 0.25: they use 25¢ of tangible capital to generate a dollar of sales. The average firm has 20% leverage, with a range from 0% to 78%. Cost of capital estimates vary between 5.9% and 12.8% nominal, with a mean and median just below 10%. Industry growth rates average 4.5%, with some industries declining by as much as 16% and others growing by 38% a year.

The lower half of Table 2 lists the incentive variables. The average CEO owns a mere 3.3% of his firm, with an even lower median of 0.4%. Not surprisingly, CEO ownership depends on firm size, averaging 5.8% in the smallest quartile and 1.4% 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% but is actually higher for the median firm, at 0.5%, than is 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 *delta*, which has a median value of 0.77 in our sample. The total *vega* 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 *vega*s for 278 CEOs in 1993 of 16.7 and 15.6, about 40% higher than our estimates. The average firm faces a 6% probability of delisting in a given year, our measure of capital market pressure. Just under half of our 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).

4 Empirical results

The discussion of our empirical results is structured as follows. First, we investigate the importance of econometric estimation technique. We compare HHP-like within-groups panel regressions to SFA estimates and establish that panel regressions suffer from inefficient standard errors and thus low power. We also show that industry fixed effects are insufficient to remove unobserved heterogeneity compared to firm fixed effects. Second, we estimate the location of the frontier (equation (6)). Given the frontier, we test for the presence of inefficiency and find systematic underperformance. We generate predicted efficiencies based on our maximum likelihood estimates and analyze their distributions over time, by size, and by industry. This reveals that inefficiency is not concentrated in any particular period, size group, or industry. Third, we attempt to identify the causes of inefficiency (equation (7)) by relating the degree of inefficiency to the internal and external incentives. We find that CEOs systematically hold too few stocks and options and that the options are insufficiently sensitive to risk. We then investigate the robustness of these findings to sample partitions by size and by industry, to alternative treatments of endogeneity, to outliers, and to alternative variable definitions. Each of these leaves our main conclusions qualitatively unchanged.

4.1 Estimation technique

We estimate the model defined by (6) and (7) using three alternative estimation techniques. The first is a firm fixed effects panel regression, as in HHP. We will refer to this specification as within-groups. The other two are stochastic-frontier maximum likelihood regressions with time-varying inefficiencies u_{it}, based on Reifschneider and Stevenson (1991) and Battese and Coelli (1995) and defined in sections 2.1 and 2.3. Our two SFA specifications differ in the way they deal with the unobserved effects η and ζ. One includes industry fixed effects in a first attempt to make the estimators consistent. This will only be successful if η and ζ vary much more across industries than within industries. To illustrate, managerial ability or risk aversion would have to be similar within industries but different across industries. The other includes firm fixed effects as derived in section 2.2. This will yield consistent estimates regardless of industry distribution, as long as the unobserved effects η and ζ are constant over time. This specification is directly comparable to the within-groups regression.

The results of the three estimation techniques are reported in Table 3. We argued in Section 2.4 that within-groups estimates will be less efficient than SFA if the errors are skewed. Are they? Based on the within-groups residuals, we reject zero skewness at $p = 0.1\%$ (see the Diagnostics Section of Table 3). The residuals are right-skewed. This is consistent with systematic inefficiency as it implies that the median error is negative. As a consequence of skewness, we expect the SFA standard errors to be smaller than the within-groups standard errors. Comparing the first and last columns in Table 3 confirms that this is indeed the case.

One of HHP's contributions is to argue that at least some of the simultaneity of choice of managerial incentives and firm performance can be removed by including fixed effects. Using the within-groups specification, we reject the hypothesis that the fixed effects are jointly zero at $p = 0.1\%$. This suggests that unobserved heterogeneity is indeed present in our panel. Would industry effects suffice to make estimates consistent? A quick comparison of the coefficient estimates in Table 3 suggests not. Comparing SFA with industry effects to SFA with firm effects, we find that the 95% confidence intervals for ten of the X variables and four of the Z variables do not overlap, and eight of the X variables and two of the Z variables even change sign. This suggests the presence of omitted variable bias in the industry-effects specification. To illustrate, consider the coefficient for optionholdings which changes from $+0.047$ with industry effects to -0.099 with firm effects. The difference between the two — the omitted variable bias — equals the covariance between the omitted variable and optionholdings divided by the variance of optionholdings. The positive bias evident in the industry-effects estimate might be explained as follows: say there are unobserved differences, across firms within an industry, in CEOs' ability to hedge their option portfolios, perhaps because boards afford managers different leeway to sell stock in response to option awards (as in Ofek and Yermack, 2000). The better a CEO's hedging ability, the more options he is willing to hold. Industry effects cannot control for firm-level differences in hedging ability, and so the industry-effects estimate is positively biased compared to the firm-effects estimate.

Because of the evidence of unobserved firm-level heterogeneity, our discussion will concentrate on firm fixed effects estimates.

4.2 Frontier estimates and tests for inefficiency

Locating the frontier

The upper part of Table 3 lists the parameter estimates for the frontier variables alongside standard t-statistics. The frontier variables in the firm fixed effects specifications have the predicted sign, with the exception of log *sales* and operating margin $\frac{Y}{sales}$. The maximum-attainable Tobin's Q significantly decreases in firm-specific risk, increases in research and development and capital expenditures, and decreases in tangible capital-intensity $\frac{K}{sales}$ and *leverage*. We interpret the negative *leverage* effect as proxying for a positive relationship between difficult-to-measure intangibles and Q and note that it points to debt tax shields being of second-order importance.[14] The Q frontier appears to be invariant to advertising and to the cost of capital and general industry growth rates, though the signs in each case are as predicted.

Q increases with log *sales* and decreases with its square, with a turning point at sales of \$14.3 million. This inverse U-shaped relationship is the opposite of HHP's finding in their 1982-1992 panel, but is driven by some highly-rated drugs and bio-tech companies with very low sales. Excluding observations below \$14.3 million in sales re-establishes HHP's U-shaped relation, with a turning point at sales of \$1.47 billion, but does not alter any of our other results. The fact that operating margins do not influence Q is unexpected. Further investigation reveals that this result is due to very large negative operating incomes in four per cent of firm-years. Excluding these turns the coefficient estimated for $\frac{Y}{sales}$ positive and significant.

[14] Agrawal and Knoeber (1996) also find a negative relationship between *leverage* and Q. McConnell and Servaes (1995) distinguish between low- and high-growth firms and find a negative relationship between *leverage* and Q for the latter firms.

Testing for inefficiency

We have already shown that the within-groups residuals are significantly and positively skewed, which we have argued is indicative of systematic inefficiency. SFA models the skewness explicitly, resulting in a likelihood ratio gain compared to standard least-squares if the extent of skewness or inefficiency is sufficiently severe. The Diagnostics section of Table 3 reports likelihood ratio tests of the null hypothesis that the one-sided SFA error terms are zero. Irrespective of whether we control for industry or firm effects, we comfortably reject the null ($p = 0.1\%$).

Does skewness necessarily imply systematic economic inefficiency? Or could it arise randomly, without reflecting underlying economic performance? To shed light on this, we investigate the time series behavior of the predicted efficiencies, $PE_{it} = \frac{E(Q_{it} - \overline{Q}_i + \overline{\overline{Q}} | u_{it}, X_{it})}{E(Q^*_{it} - \overline{Q}^*_i + \overline{\overline{Q}}^* | u_{it}=0, X_{it})}$.[15] Under the null of randomness, we would expect no correlation from year to year in firms' predicted efficiencies: if the cross-section of firms' positions relative to the frontier truly was random, there would be no reason to expect it to remain stationary over time. Under the alternative hypothesis of systematic inefficiency, we would expect persistence in relative performance from year to year and possibly reversals over longer periods (as firms/boards combat their inefficiency). Table 4, Panel A shows a correlogram of the predicted efficiencies, estimated using the firm fixed effects specification in Table 3. There is clear evidence of significant positive first-order auto-correlation, consistent with persistence in (in-)efficiency. We are thus not picking up random movements in relative performance. At longer lags the correlation tends to become negative, suggesting reversals in firms' relative performance. In Section 5, we will investigate whether changes in relative performance over time are related to board actions.

Table 4, Panel B reports distributional characteristics of the predicted efficiencies calculated using the industry fixed effects and firm fixed effects specifications in Table 3. In addition, the table includes predicted efficiencies estimated without fixed effects to see if the fixed effects may have introduced spurious inefficiency. The average predicted efficiency is 87.1% without fixed effects, 91.8% with industry effects, and 90.7% with firm effects, meaning that the average firm performs around 10% below the frontier. Translated into dollars, the predicted efficiencies imply that the market value of the average firm would be $1,340 million higher were it to move to the frontier.[16] The distributions of predicted efficiencies look similar irrespective of empirical specification, though the minima (and in fact first percentiles) of the no and industry fixed effects specifications are very much lower than in the firm fixed effects specification.

Panel B also compares predicted efficiencies by year, size, and industry, derived by partitioning the cross-section of predicted efficiencies from the firm fixed effects specification in Table 3. The by-year distributions are graphed in Figure 1. For the size partition, companies are sorted into terciles on the basis of their net sales in the first panel year. For the industry partition, companies are sorted into three groups: utilities (two-digit SIC codes 40, 48, 49),

[15] We use $\frac{E(Q_{it} - \overline{Q}_i + \overline{\overline{Q}} | u_{it}, X_{it})}{E(Q^*_{it} - \overline{Q}^*_i + \overline{\overline{Q}}^* | u_{it}=0, X_{it})}$ as a proxy for $\frac{E(Q_{it} | u_{it}, X_{it})}{E(Q^*_{it} | u_{it}=0, X_{it})}$ introduced in Section 2.1 because η_i and ζ_i cannot be estimated from equation (5).

[16] 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.

financials (two-digit SIC codes 60 to 63), and unregulated industries. Inefficiency appears to be present in all years, amongst companies of all sizes, and in all industry groups. However, this does not preclude the possibility that the *causes* of inefficiency differ across size terciles or industry groups. We investigate this possibility in Section 4.4.

Summary

We interpret our stochastic frontier estimates as consistent with the within-groups results of HHP's earlier sample: Q first increases and then declines with firm size; decreases in firm-specific risk; increases in soft (R&D and advertising) and hard (capital-formation) spending; increases in operating margins; and decreases in asset intensity, leverage, and the cost of capital. Unlike HHP we base our test for efficiency on the distribution of the residuals in the Q regression, and not on the coefficient of managerial ownership. We find significant skewness in the within-groups residuals, which suggests systematic inefficiency. A formal likelihood ratio test rejects the null of symmetric errors and therefore lends support to our use of SFA. We show that the time series behavior of firms' predicted efficiencies is much more consistent with systematic departures from the frontier and thus inefficiency than with random skewness. Partitioning the predicted efficiencies by year, firm size, and industry reveals no particular clustering in inefficiency.

4.3 Identifying the causes of inefficiency

Does the degree of inefficiency in the sample as a whole depend on the strength of managerial incentives, as captured by our Z variables in equation (7)? The Z coefficients are shown in the lower part of Table 3, listed under the heading 'efficiency variables'. In interpreting the coefficients, recall that δZ enters the SFA equation negatively. A negative δ coefficient therefore indicates that inefficiency u_{it} can be decreased by increasing the value of the corresponding variable Z_{it}.

All but one of the δ coefficients in the firm fixed effects SFA specification are significant, reflecting their ability to capture the cross-sectional variation in inefficiency.[17] Overall, our Z variables are quite successful at accounting for departures from the frontier: γ, which measures the relative importance of the unexplained part, w_{it}, of equation (7) and the overall error of the SFA regression, is very close to zero.

The coefficient of CEO stockholdings is significantly negative, indicating that CEOs own too little equity: inefficiency could be decreased by increasing their stockholdings. The coefficient of the square of CEO stockholdings is positive, indicating concavity in the relationship between stockholdings and distance from the frontier. Based on the parameter estimates, greater stockholdings increase efficiency up to 36.9% CEO ownership and thereafter reduce it. These findings mirror the results of McConnell and Servaes (1990). They contrast with HHP, who find no effect of managerial stockholdings on Tobin's Q in 1982-1992. To illustrate

[17] In unreported regressions, we included interest cover to capture Jensen's (1986) free cash flow argument that the presence of debt increases efficiency by reducing managerial moral hazard. However, the effect was always negative: the more efficient firms are those that rely less heavily on debt. This is precisely the same effect we capture using *leverage* amongst the X variables. For that reason, the results we report do not include interest cover.

the economic magnitude of the effect in our data, we compute the change in Tobin's Q for a one standard deviation increase from the mean of stockholdings, holding all other variables at their sample means. This has the effect of raising Tobin's Q from 1.89 to 2.06. Since Tobin's Q gives the multiple at which each dollar of assets trades in the market, we can translate this into dollar changes in market value. The average firm has assets of $7,048 million, so each 0.01 increase in Tobin's Q increases its market value by $70.5 million. Increasing CEO stockholdings by one standard deviation from the sample mean therefore increases market value by $1,198 million, all else equal.[18]

The coefficients estimated for optionholdings and its square mirror those for stockholdings: CEOs do not have enough of an ownership interest in the company to maximize Q. The relationship is again concave and attains a maximum at 10.3% option ownership. A one standard deviation increase in CEO optionholdings from the mean, for the average company, increases Tobin's Q from 1.89 to 2, equivalent to an increase in market value of $775 million.

Given our finding that CEOs do not hold enough options, do their options at least induce optimal risk-taking? The negative coefficient estimated for *vega* suggests they do not: the companies closest to the frontier are those which have awarded options with high *vega*s. A one standard deviation increase in *vega* from the sample mean raises Tobin's Q from 1.89 to 1.94, corresponding to a $352 million increase in market value for the average firm.

An increase in product market competition significantly reduces inefficiency, in line with Schmidt (1997). The effect is large: firms operating in 'unconcentrated' industries, as defined by the Federal Trade Commission, have Tobin's Qs that are on average 0.15 higher than firms operating in 'highly concentrated' industries, corresponding to a $1,078 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.

Capital market pressure, as measured by the probability of delisting, has a positive effect on inefficiency. Literally, this means that capital market pressure may lead to worse performance. The effect here is not statistically significant, but we will show in the next section that it becomes significant once we exclude regulated firms from the sample. We conjecture four possible explanations for this curious finding. First, our measure of capital market pressure may be imprecise at the individual firm level since it is not conditioned on firm characteristics. Second, since the majority of delistings in the 1990s are due to mergers, we may be picking up changes in the effectiveness of relative performance evaluation. Specifically, the more firms delist in an industry the fewer comparators remain to condition a CEO's compensation on, possibly leading to less efficient incentive contracts (Holmström, 1982). By the same token, a higher merger-driven delisting rate may change strategic interactions in imperfectly competitive product markets to the detriment of (some of) the non-merging firms.[19] Finally, if the effect we are picking up is genuine, it may be supportive of Stein's (1988) argument that too much capital market pressure may induce managerial myopia. We

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

[19]For instance, in a Cournot model with asymmetric costs, mergers between differentially efficient firms can lead to more aggressive output choices and thus lower profits amongst non-merging firms.

do not take a stand on which of these alternatives best explains our result.

Summary

In the previous section, we provided evidence of systematic inefficiency. This section relates the degree of inefficiency to the internal and external incentives CEOs face. Unlike HHP and Agrawal and Knoeber, but like McConnell and Servaes (1990) and Mørck, Shleifer, and Vishny (1988), we find that CEOs own too few stocks. However, we do not claim to refute HHP's or Agrawal and Knoeber's results, given differences in sample compositions and sample periods that we cannot control for. It is clear, however, that our opposite findings are not due to using a stochastic frontier approach: our within-groups specification, which mirrors HHP's, also finds sub-optimal managerial stockholdings.

In addition to stockholdings, we investigate the effects of CEO optionholdings on firm performance. As far as we know, we are the first to do so. Our results indicate that CEOs own too few options, and that the options they do own are insufficiently sensitive to risk.

We 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 finally show, somewhat surprisingly, that the industry-adjusted probability of delisting has a negative effect on performance.

4.4 Robustness checks

Before we ask whether boards react to inefficiency by restructuring CEOs' incentives, we provide a range of robustness checks. These control for size and industry effects, endogeneity, outliers, and alternative definitions of equity incentives.

Size effects

In Table 4, we sorted companies into terciles based on their net sales in the first panel year to investigate patterns in the predicted efficiencies derived from the stochastic frontier regression for the whole sample. Table 5 estimates individual stochastic frontiers for each of the terciles. This reveals some interesting patterns in the frontier variables. Firm-specific risk significantly depresses Q only amongst small and medium-sized firms, perhaps because large firms benefit from internal diversification across business lines. Profit margins $\frac{Y}{sales}$ do significantly increase Q amongst medium-sized and large firms, as in HHP, but not amongst small firms. Perhaps not surprisingly: the smallest firms are the most prone to operating losses.

As the likelihood ratio tests (and the predicted efficiencies in Table 4) show, all three groups are prone to systematic departures from the frontier. The Z variables have the same signs as in Table 3, where we used the whole sample, though there are changes in significance. Specifically, the lack of effort incentives in the form of stockholdings is strongest amongst the smallest firms, smaller but still significant amongst medium-sized firms, and absent for the largest firms. This indicates that large-company CEOs have optimal stockholdings, consistent with scale-dependence in providing equity incentives: a given increase in CEO ownership has more of an effect on Q, the smaller the company. HHP's finding of optimal managerial ownership thus re-emerges amongst our largest companies. Using one standard deviation increases in stockholdings from the mean to illustrate the economic magnitude of the coefficients, Tobin's Q increases by 0.3 amongst small companies and 0.08 amongst medium-sized companies, corresponding to increases in market value of \$188 million and \$159 million, respectively.

The pattern of effort incentives in the form of optionholdings is subtly different. The coefficients are significant for the smallest and largest companies and insignificant for the medium-sized ones. In other words, small and large companies award too few options, whereas option awards in medium-sized companies appear optimal. Economically, a one standard deviation increase in optionholdings from the mean would correspond to an increase in market value of \$106 million amongst small companies and \$1,076 million amongst large companies, but only \$79 million amongst medium-sized ones.

Inefficiency amongst all size groups is negatively and significantly related to *vega*: higher *vega* invariably moves companies closer to the efficient frontier. The economic magnitude of this effect is greatest amongst large companies and smallest amongst medium-sized ones: a one standard deviation increase in *vega* raises Tobin's Q by 0.54 for small companies, 0.04 for medium-sized ones, and 0.03 for large ones, corresponding to increases in market value of \$338 million, \$79 million and \$538 million, respectively. As *vega* depends on the moneyness of the CEO's options, our finding suggests that it may be counterproductive to grant options that are at-the-money on the day of the grant. This widespread practice may be justified by tax considerations which make such options free of tax to the manager, but it appears to entail a real cost to the firm in the sense of precluding the provision of optimal incentives for the choice of project risk.

Finally, greater product market competition raises the efficiency of the smallest and medium-sized companies, but has no effect on efficiency amongst large companies. Greater capital market pressure increases *in*efficiency in each size group, as it did in the sample as a whole, but this effect remains insignificant.

To summarize, ownership incentives matter across all company sizes, all companies provide insufficient risk incentives, the CEOs of small and medium-sized companies are disciplined by product market competition, and capital market pressure (as we measure it) has no beneficial effect on efficiency. The only significant determinants of managerial inefficiency amongst the largest companies are optionholdings and *vega* — precisely the variables not included in HHP's study.

Industry effects

Like in Table 4, we partition sample firms into three groups on the basis of their industry affiliations (unregulated industries, utilities, and financials). Table 6 estimates individual stochastic frontiers for each of the partitions. We exclude $\frac{ADV}{K}$ and $\frac{R\&D}{K}$ from the regressions of utilities and financials, respectively, as our utilities report no advertising and our financials report no R&D expenditure. In all other respects, the specifications are as before. The first two columns of Table 6 report the coefficient estimates and t-statistics for the 1,135 unregulated sample firms. This group behaves almost exactly like the whole sample in Table 3: there is systematic inefficiency (the likelihood ratio test being significant) which is significantly linked to insufficient stock- and optionholdings and insufficient *vega* and which decreases in product market pressure. The only difference to Table 3 is that the positive coefficient on capital market pressure becomes significant at $p = 0.4\%$ when we concentrate on unregulated industries. Subject to the imperfections in the way we measure capital market pressure, this result is consistent with managerial myopia as in Stein (1988) or our other three explanations mentioned earlier. Though statistically significant, the effect is economically small: a one-standard deviation increase in capital market pressure from the mean reduces Tobin's Q only by 0.01, corresponding to a decrease in market value of $40.5 million, all else equal.

The frontier estimates for utilities differ from those of unregulated companies. Specifically, unlike unregulated companies utilities have significantly higher Qs the higher the operating margins $\frac{Y}{sales}$ and the higher are industry growth rates, both of which are intuitive. The size effect is reversed and mirrors the one found by HHP. Overall, the likelihood ratio test indicates that utilities, like unregulated firms, suffer from systematic inefficiency, but the causes are different. CEOs of utilities appear to have optimal stockholdings but too *many* options, though these are still insufficiently risk-sensitive, as the negative coefficient for *vega* shows. The excess option result is driven by ten firms with unusually large optionholdings (by utility standards) and disappears when these are excluded. Product market competition switches sign, such that utilities are closer to the frontier, the greater is industry concentration. This mirrors the positive effect of operating margins and suggests that the rent-effect of product market concentration dominates whatever incentive effect competition might have on utilities. Capital market pressure has the same curious effect on efficiency amongst utilities as amongst unregulated firms. Here, it is economically significant: a one standard deviation increase in capital market pressure from the mean decreases Tobin's Q by 0.04, corresponding to a fall in market value of $268 million. Note that the estimate of γ, though small, is statistically significant, so our set of Z variables does not fully capture all the determinants of inefficiency. One plausible omitted variable is the intensity of regulatory pressure, which could well differ from state to state.

The frontier estimates for financials are much the least precise. The only significant X variables are the negative square of $ln(sales)$, the positive $\frac{CAPEX}{K}$ effect, the negative *leverage* effect, and (at the 10% level) the positive industry growth effect. Operating margins, idiosyncratic risk, advertising and asset intensity are insignificant. The likelihood ratio test suggests the presence of systematic inefficiency, related to insufficient stockholdings. No other Z variables are significant.

Causality and endogeneity

Up to this point, we have treated ownership and other internal incentives as exogenous with respect to firm performance. HHP argue persuasively that firm fixed effects help *reduce* endogeneity problems when estimating the effect of managerial incentives on firm performance. It is unlikely, however, that fixed effects can fully remove endogeneity biases. HHP therefore also explore an instrumental variables approach using log sales and firm-specific risk to construct instruments for managerial ownership. While this leaves their main results unaffected, the power of their instruments appears to be low. This is not surprising, given the difficulty in finding instruments which correlate with managerial ownership but not with Q. In our context, we face an additional difficulty. Whereas HHP have only one potentially endogenous variable (managerial ownership), we have three: CEO stockholdings, optionholdings, and *vega*. Given the paucity of natural instruments, we cannot follow HHP's approach.

Instead, we follow the approach of Hermalin and Weisbach (1991) who use lagged explanatory variables as instruments for managerial ownership. Specifically, we use lagged values of the incentive variables Z_{it-1} to instrument for the potentially endogenous contemporaneous variables Z_{it}. Economically, we thus relate current departures from the frontier to lagged incentives. For T large, the resulting coefficients will be consistent if the Z_{it-1} are uncorrelated with v_{it}.

Although the resulting SFA estimates are unsurprisingly much noisier, our main results remain unaffected. We continue to find that CEOs own too few stocks and options, and that option *vega*s are too low. Product market pressure, though no longer significant, continues to exert a beneficial effect on performance. Interestingly, the capital market pressure variable switches sign and now indicates that a greater delisting probability improves performance. Taken together, the IV results show no sign of spurious causality or endogeneity bias in our estimations.

Outliers and alternative measures of equity incentives

Next, we investigate the robustness of all our results with respect to outliers and measurement errors. We address the skewness in the *R&D* and advertising variables by taking logs and find our results unchanged. We test for sensitivity to outliers by setting the upper- and lower-most percentiles for each explanatory variable equal to the values at the 1st and 99th percentile in each panel year, respectively. Again, our results are unchanged. Finally, we replace our 'additive' CEO stock- and option ownership measures with the 'multiplicative' measures advocated by Baker and Hall (1999) and discussed in footnote 10. This also leaves our results unchanged.

5 Board actions to reduce inefficiency

The results in Section 4.3 indicate that internal incentives have a strong impact on the economic performance of the firms in our panel: companies are closer to the frontier, the greater the stock- and optionholdings of their CEOs, and the higher the *vega* of CEO option portfolios. In this section, we investigate whether boards adjust internal incentives to improve performance. We exploit the time dimension of our panel, specifically the fact that inefficiency can change over time. Relating such changes to changes in internal incentives, we ask two questions. First, is the improvement over time in a firm's performance relative to the frontier — its rate of 'catch-up' — related to changes in its internal incentives? Second, do boards react to past inefficiency by subsequently altering CEOs' internal incentives? In addressing the second question, we distinguish between changes in CEO incentives that are under the board's control and those that are influenced by CEO hedging behavior.

5.1 Catch-up

Denote by $\Delta_{\underline{t}}^{\bar{t}}$ the operator that takes the difference in a variable between a company's first panel year ($\underline{t}$) and its last panel year ($\bar{t}$). Define $catchup \equiv \Delta_{\underline{t}}^{\bar{t}}\ predicted\ efficiency$ as the change in each company's location relative to the frontier, based on the predicted efficiencies shown in Figure 1 and tabulated in Table 4, Panel B. *catchup* is bounded above by 1 (for a firm which moves from a position of 0 to the frontier) and below by -1 (for a firm which drops from the frontier to 0). Over its existence in our panel, the average (median) firm moves up by 0.3 (0.5) percentage points. A quarter of companies move down by 2.5 percentage points or more, and a quarter move up by 3.6 percentage points or more. To illustrate the economic magnitude of a one percentage point move, we compute the corresponding increase in market value given each firm's actual and frontier Q and its asset base. For the average firm, a one percentage point move towards the frontier is 'worth' \$104 million. The rate of *catchup* for the average or median company is hence quite small economically. However, the top and bottom quartiles experience economically significant changes in Q and hence market value.

To see if the degree of *catchup* is related to changes in the internal incentives CEOs face, we regress *catchup* on the total changes in CEO stock- and optionholdings and the *vega* of their options (White standard errors in italics below the coefficient estimates):

$$\begin{aligned} catchup &= \underset{\mathit{0.00145}}{0.00129} + \underset{\mathit{0.052}}{0.570}\ \Delta_{\underline{t}}^{\bar{t}} stockholdings \\ &\quad + \underset{\mathit{0.394}}{2.279}\ \Delta_{\underline{t}}^{\bar{t}} optionholdings \\ &\quad + \underset{\mathit{0.0003}}{0.00199}\ \Delta_{\underline{t}}^{\bar{t}} vega\ of\ options \\ adjusted\ R^2 &= 53.2\% \qquad F-test = 59.5^{***} \qquad N = 1487 \end{aligned}$$

As the adjusted R^2 of 53.2% indicates, the regression has very high explanatory power. The positive and significant coefficients estimated for the three explanatory variables strongly support the hypothesis that internal incentives matter: it is the companies that increase internal incentives the most that move closer to the frontier over time. To illustrate, increasing CEO stock- or optionholdings by one standard deviation from the mean would move the company up 3 percentage points relative to the frontier, whilst a similar increase in *vega* would result in a 2 percentage point movement.[20]

5.2 Board actions

Do boards react to past inefficiency by altering CEOs' internal incentive structure? Table 7 investigates the effect of lagged predicted efficiency on subsequent board actions regarding CEO stockholdings (Panel A), optionholdings (Panel B), compensation (Panel C), and CEO replacement (Panel D). If boards attempt to adjust incentive structures, we would expect

[20] The results are unaffected if utilities and/or financials are excluded, and continue to hold in each of the three size terciles.

a negative relationship between past relative performance and subsequent *changes* in stock- and optionholdings and in *vega*. More speculatively, good past performance may be rewarded with increases in base salary — and bad past performance may be 'punished' with cuts in base salary. In the same spirit, we expect boards to change the mix of salary and bonus away from cash and towards performance-related bonuses following poor performance. Finally, bad relative performance should also increase the likelihood of CEO replacement.

The regressions in Panels A and B include $SIGMA1$ to control for the effect of firm-specific risk on a board's ability to increase equity incentives for a risk-averse manager. We also control for scale effects in the provision of incentives: for instance, we expect the flow of new equity incentives to be smaller the higher the stock of such incentives. Beyond that, our regressions are highly parsimonious. They are not intended to provide in-depth empirical analyses of all the determinants of board behavior.

Note that the temporal structure of the regressions in Table 7 mitigates endogeneity and simultaneity biases by focusing on the effect of *past* performance on *subsequent* changes to CEO incentives. We continue to include firm fixed effects to control for omitted time-invariant variables, such as managerial ability, risk aversion, or heterogeneity in monitoring technology.

CEO stockholdings

The first regression R1 in Panel A relates changes in CEO stock ownership between $t-1$ and t to the efficiency ranking at $t-1$. The coefficient estimated for lagged efficiency is negative as expected: the worse was last period's efficiency, the larger is the subsequent increase in CEO ownership. However, the coefficient is not significant. To control for scale effects in managerial ownership, the regression includes the level of CEO ownership at $t-1$. Its coefficient is negative and significant, indicating that the higher was CEO stock ownership, the smaller is the subsequent increase in CEO ownership. These results do not suggest that CEO stock ownership is adjusted in the light of poor past economic performance. However, note that CEO stock ownership *per se* is not under the board's control: CEOs are free to sell stock as soon as it has vested.

While CEO ownership is not under the board's control, the award of restricted stock is. In our sample, boards make restricted stock awards in 17.7% of firm-years. R2 is a fixed-effects logistic regression that relates the probability of such awards at time t to the efficiency ranking at $t-1$, controlling for the level of lagged CEO ownership and firm-specific risk. The effect of past efficiency is negative and significant at $p = 2.7\%$, consistent with boards reacting to inefficiency: the worse a firm's performance last period, the greater the probability of the board subsequently granting restricted stock. Regression R3 investigates the value of such restricted stock awards, as reported by ExecuComp, using a random-effects Tobit regression (R3).[21] The influence of lagged efficiency remains statistically significant at $p = 3\%$. So, not only are boards more likely to make restricted stock awards the worse the previous year's economic performance, the size of such awards is also larger.

[21] Fixed-effects Tobits are inconsistent.

CEO optionholdings

Panel B presents similar evidence for changes in CEO optionholdings (regression R4), the probability of the board making an option award (R5), the value of new option awards as reported by ExecuComp (R6), the change in the *vega* of the CEO's option portfolio (R7), and the *vega* of the new option award in year t (R8). As before, we control for the lagged *level* of the dependent variable and for firm-specific risk. The results are similar. Poor past relative performance does not affect subsequent changes in CEO optionholdings, but then like CEO stockholdings, these are not entirely under the board's control. Boards make new option awards in 66.4% of sample years. The probability of the board making an option award and the size of new option awards are both significantly negatively related to past performance, again suggesting boards react to past inefficiency. The change in option *vega* is likewise negatively related to lagged efficiency. However, because managers can affect *vega* via option exercises, a more appropriate measure of a board's reaction may be the *vega* of a new award (R8). This confirms the previous result: boards award options with higher *vega*s, the worse was the previous period's economic performance.

CEO compensation

The incentive properties of cash salaries should be substantially lower than those of stocks or options, due to their fixed nature. Still, managers who have turned in good economic performance may benefit from discretionary increases in base salaries. Conversely, salaries might be cut in response to poor relative performance. We might therefore expect a positive relationship between past economic performance and changes in cash salaries. Regression R9 in Panel C finds some evidence of this, though the coefficient estimated for lagged efficiency is only significant at $p = 8.2\%$. Its size indicates that each percentage point improvement in efficiency raises subsequent cash salaries by \$11,000, all else equal. Panel C also estimates the effect of past performance on subsequent changes in bonus payments (R10). The estimated effect is negative, indicating that bonus payments increase by more, the worse was the previous year's relative performance. This is consistent with an incentive role for bonuses: following poor performance, boards change the mix of salaries away from fixed payments and towards bonuses.

CEO replacement

Do boards replace CEOs following poor relative performance? To avoid counting normal retirements as forced resignations, we exclude CEOs leaving their companies aged 61 or above.[22] This leaves 149 firms which replace their CEO once and another 6 which replace their CEO twice between 1992 and 1997. Regression R11 in Panel D estimates the likelihood of a CEO being replaced in year t as a function of relative performance at $t - 1$ and the

[22]Our age criterion is a crude attempt to distinguish between forced and voluntary resignations. We obtain somewhat weaker results if we raise the age threshold from 60 to 65, presumably because we are then more likely to falsely classify normal retirements as forced resignations. Murphy and Zimmerman (1993) use a different procedure to deal with classification problems. They use all CEO changes, irrespective of CEO age, and include age as an explanatory variable. This approach leaves our parameter estimate unchanged but greatly increases explanatory power, with a Pseudo-R^2 of 29%.

CEO's stockholdings at $t-1$ as a control for entrenchment effects. The coefficient estimated for lagged efficiency is reliably negative, consistent with the hypothesis that boards are more likely to replace a CEO the worse past performance relative to the frontier.

Absolute versus relative performance

Do boards react to relative or absolute performance? To check, we replace our measure of relative performance in the board reaction regressions R1-R11 with each firm's actual Tobin's Q. This changes many of our results: CEOs significantly *in*crease their stockholdings following good economic performance (R1), the likelihood of boards awarding restricted stock (R2) or options (R5) or replacing the CEO (R11) is not significantly related to absolute performance, while the value of option awards *in*creases in the level of lagged Q (R6) suggesting poor absolute performance leads to smaller option awards. Finally, the change in *vega* (R7), cash salary (R9) and bonus payments (R10) are unrelated to absolute performance. The only results that remain unchanged are that the value of restricted stock awards (R3) and the *vega* of new option awards (R8) decrease in past performance.

These results strongly suggest that boards react to firms not achieving their full-potential Q^* rather than to performance per se. It also suggests that our measure of relative performance is more informative than standard measures of absolute performance.

5.3 Summary

Whilst highly parsimonious, the regressions in Table 7 provide strong support for the hypothesis that boards react to past inefficiency by altering CEOs' internal incentive structure. In particular, we have found that boards are more likely to award restricted stock and options, and that such awards are larger, following poor economic performance. We also find that boards make CEOs' options more sensitive to volatility and that they are more likely to replace the CEO, the worse past performance. Finally, we find that bonuses are increased and cash salaries are cut following poor relative performance.

6 Conclusion

In this paper, we have provided a direct test of the Berle-Means hypothesis, based on 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 are systematically inefficient on average and that the extent of inefficiency is related to the inadequate provision of internal incentives. The effectiveness of the incentives we consider depends on company size and, to a lesser degree, industry. Amongst smaller companies, greater stock and option ownership and greater product market competition are associated with better performance. Amongst medium-sized companies, stockholdings and product market competition are the key significant influences on efficiency. And amongst large companies, CEOs appear to hold too few options. Across all size classes, companies are prone to awarding options which are too far in-the-money and thus not sufficiently sensitive to risk. Given these findings, we asked whether boards respond to inefficiency by subsequently redesigning managerial incentives.

The evidence firmly suggests that they do: boards rebalance the mix of incentives away from cash towards performance-related rewards via greater stock and option awards and larger bonus elements following poor performance. They are also more likely to replace the CEO the worse the firm's performance relative to its potential.

The picture that emerges is one where a substantial fraction of companies operates under suboptimal incentives at any given point in time, but where 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 adjustments costs of changing incentives. If a series of small adjustments dominates a drastic and rapid change in cost terms, boards may in fact be optimizing. We believe the question of costly adjustment warrants further research.

7 Data Appendix

7.1 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 use proxy statements, 10-Ks, the Forbes CEO database, and news reports to identify incumbent CEOs in all the missing years. We also compare ExecuComp's CEO flag against ExecuComp's information about the dates at which executives assumed (and left) their positions. In total, we check 4,324 CEO-years. This throws up 50 cases where ExecuComp flags the wrong person as the CEO, and 756 cases of mid-year CEO changes, where ExecuComp flags the individual who is CEO at year-end, as opposed to the individual who was CEO for the greatest part of the fiscal year. We correct all these cases. We also find that ExecuComp misses 44 instances where two individuals are co-CEOs.

7.2 Managerial stock- and optionholdings

ExecuComp fails to report managerial stockholdings for 289 firm-years. Typically, this affects a CEO's first panel year, mostly in 1992. We try to find the relevant proxies in Disclosure and are successful in 212 cases; the remaining 77 firm-years have to be dropped. To guard against reporting errors, we investigate all 158 large (one order of magnitude) year-on-year changes in a CEO's percentage equity stake. The (rare) errors we find 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 investigate all 'extreme' values for CEO stockholdings (>50% of equity) and correct one data error.

Corresponding to the problem of missing CEO stockholding information, 317 firm-years lack information on the CEO's option holdings. We handfill the missing optionholding information for 252 of the 317 firm-years. We also find 79 option awards that ExecuComp misses, and are 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).

Finally, we investigate all 'unusual' option information in ExecuComp. For instance, options are typically awarded at or near the current market share price, so we investigate the fifteen options with unusually low reported strike prices, relative to the fiscal year-end share price. For ten 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.

7.3 Computing option deltas and vegas

To compute option deltas and *vega*s, we need to reconstruct each CEO's option portfolio for every panel year. For options awarded during our observation period 1992-1997 (which we will refer to as 'newly-awarded options'), we know all necessary information: the number of options awarded, the maturity, and the strike price.[23] For options *already* held at the beginning of our observation period ('old options'), we only know the number of options held,[24] but not their strike prices or maturities. One solution, employed by Guay (1999), is to create an option history using each company's ten previous proxy statements — just under 15,000, in our case! A less labor-intensive alternative is to impute the strike prices of old options from the information available in ExecuComp, and to 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).[25] From this, we can infer the average strike price of old options as $\bar{X} = S - \frac{intrinsic\ value}{number\ of\ old\ options}$. This will be exact as long as *all* old options are in-the-money. Since we do not know what fractions of options were in-the-money, we investigate all apparently deep in-the-money ($\frac{S}{\bar{X}} < .5$) or out-of-the-money options ($\frac{S}{\bar{X}} > 5$). Largely, our imputed strikes turn 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.[26] 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 rely on definitive information from the proxies we look up anyway, and partly assume old options have an average of five years to run. We follow the five-year rule unless the CEO

[23] With a few exceptions: *i*) For 32 option awards, ExecuComp fails to report time to maturity. Hall and Liebman (1998) report that most options expire after ten years. Assuming that options are awarded half-way through the fiscal year gives a remaining time to maturity of 9.5 years at fiscal year-end. *ii*) For ten options, ExecuComp reports negative remaining times to maturity, as of the fiscal year-end. We set these times to maturity to zero. *iii*) For eight option awards, ExecuComp fails to report a strike price. We handfill the missing information from proxy statements.

[24] With a large number of exceptions: in about 300 firm-years, ExecuComp reports no option information at all. We reconstruct 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 only works where the CEO is the same in both years. Where this is not the case, we go back to proxy statements. Note that our procedure will miss options which have expired out-of-the-money. To assess the extent of this potential problem, we spot-check one in five of the corrections we make, finding virtually no errors.

[25] In 76 cases, CEOs do hold options but ExecuComp fails to report their intrinsic value. We are able to handfill 58 of these using proxy statements.

[26] Core and Guay (1999) 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.

continues to hold the old options for more than five subsequent years in a panel, in which case we increase the assumed time to maturity by one 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 compute total option deltas and total option *vega*s for every CEO-year as follows: for every year, we compute the *vega* and delta of all old options still held, and of each individual option award since the beginning of the panel.[27] We then compute the total *vega* and total delta as the weighted average of the *vega*s 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.

7.4 Compustat data

With respect to the Compustat data with which we measure Tobin's Q and other firm-specific variables, we check 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) Tobin's Qs. We are able to handfill a small number of missing/zero Compustat values and to resolve all extreme Tobin's Qs, using 10-Ks and information gathered from 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 is missing or zero in Compustat, we use the difference between the book value of assets and intangibles. There are about 140 such cases.

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[27] That is, we treat old options as one award, with one (average) strike price and one time to maturity, whereas for newly awarded options, we consider the individual strikes and maturities of each award. Given the non-linear 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.

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Table 1.
Variable definitions.

Firm characteristics		Included in HHP?
Tobin's *Q*	The ratio of the value of the firm divided by the replacement value of assets. Similar to HHP, 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 $m. Usually logged. Used to measure firm size.	✓
SIGMA1	The daily Fama-McBeth CAPM residual standard deviation, estimated over the previous year (in %, not annualized). Used to measure firm-specific risk.	✓
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 non-fixed assets.	✓
ADV / K	The ratio of advertising expenditures to K, used to measure the role of 'advertising capital' relative to other non-fixed 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 per cent.	
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 three-month riskfree rates (from CRSP) prevailing at each company's fiscal year-end. Expressed in per cent.	
SIC-2 industry growth	Estimated using the U.S. Bureau of Economic Analysis's annual GDP-by-industry figures, which cover both listed and unlisted firms. Expressed in per cent.	
Dummy SIGMA1	A dummy variable equal to one if the data required to estimate SIGMA1 is missing, and zero otherwise. Missing values of SIGMA1 are set to zero and dummied out using this dummy variable.	
Dummy R&D / K	A dummy variable equal to one if the data required to estimate R&D / K is missing, and zero otherwise.	✓
Dummy ADV / K	A dummy variable equal to one if the data required to estimate ADV / K is missing, and zero otherwise.	✓

Table 1. Cont'd.
Variable definitions.

Incentive variables		Included in HHP?
CEO stockholdings	CEO's common stockholdings as a fraction of common stock outstanding, in per cent. Includes beneficial ownership and restricted stock.	✓
CEO optionholdings	CEO's optionholdings as a fraction of common stock outstanding, in per cent.	
total delta	The partial derivative of Black-Scholes call option value, adjusted for dividends, with respect to the price of the underlying stock.	
vega of options	The partial derivative of Black-Scholes 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 vega times the dollar value of CEO wealth held in options.	
capital market pressure	= unconditional *Pr*(delisting), the probability of delisting in each firm's SIC-2 industry in a given panel-year. For each SIC-2 industry and for each panel year, we compute the fraction of all CRSP-listed companies that are 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. Expressed in per cent.	
product market pressure	= SIC-4 Herfindahl index, computed as the sum of squared market shares (in %) of each company in an industry, here SIC-4, in a given year. Computed using net sales-market shares for the universe of Compustat firms in 1992-1997.	

Table 2.
Descriptive sample statistics.
For variable definitions see Table 1.

	mean	stdev	min	Q1	median	Q3	max
Firm characteristics							
Tobin's *Q*	1.889	1.250	0.229	1.158	1.484	2.107	16.340
net sales ($m)	3,187	7,890	0	347	922	2,895	153,627
SIGMA1	2.156	1.041	0.440	1.400	1.900	2.690	13.990
R&D / K	0.183	0.745	0	0	0	0.100	33.516
ADV / K	0.074	0.455	0	0	0	0	19.490
CAPEX / K	0.219	0.168	0	0.110	0.186	0.292	1.204
Y / Sales	0.024	4.040	–307.314	0.095	0.153	0.244	0.964
K / Sales	0.551	1.085	0	0.139	0.253	0.563	54.823
leverage (%)	20.21	19.11	0	3.66	15.51	31.82	78.23
cost of capital (%)	9.613	1.416	5.910	8.414	9.914	10.794	12.764
SIC-2 industry growth (%)	4.480	7.780	–15.960	0.209	4.004	7.152	38.211
Incentive variables							
% of equity owned via stocks	3.33	7.21	0	0.09	0.42	2.45	80.06
% of equity 'owned' via options	0.97	1.44	0	0.15	0.50	1.24	25.76
total delta of options	0.67	0.30	0	0.59	0.77	0.88	1.00
total vega of options	11.93	11.66	0	4.22	9.84	16.77	356.34
SIC-2 *Pr*(delisting) (%)	6.11	3.11	0	3.97	5.74	7.61	31.25
SIC-4 Herfindahl index	1,483.4	2161.0	224.9	600.2	1,092.1	1,817.5	10,000

Table 3.
Estimating the frontier and testing for inefficiency.
For variable definitions see Table 1. The dependent variable is Tobin's *Q*. The Herfindahl index is normalized to have a maximum of 1.0 = monopoly. The first specification does not impose a one-sided error term and is estimated using standard within-groups (firm fixed-effects) estimators. The second specification is estimated using SFA and includes 60 industry dummies, based on two-digit SIC codes. The coefficients for the industry dummies are not shown. The third specification is using SFA and firm fixed effects. One, two and three asterisks indicate significance at $p<5\%$, $p<1\%$, and $p<0.1\%$, respectively.

		Within-groups panel estimator		SFA with **industry** fixed effects		SFA with **firm** fixed effects	
		coeff.	*t*–stat.	coeff.	*t*–stat.	coeff.	*t*–stat.
Frontier variables							
	constant	2.553	*7.311*	2.845	*6.296*	2.765	*9.059*
	ln(sales)	0.054	*2.161*	–0.202	*–7.918*	0.054	*2.553*
	ln(sales)^2	–0.010	*–4.986*	0.012	*6.478*	–0.010	*–5.999*
	SIGMA1	–5.513	*–3.744*	1.584	*1.578*	–5.560	*–5.536*
	R&D / K	0.273	*8.094*	0.144	*8.166*	0.271	*8.836*
	ADV / K	0.047	*1.293*	0.011	*0.426*	0.049	*1.509*
	CAPEX / K	1.011	*13.063*	1.620	*19.374*	1.014	*14.631*
	Y / Sales	–0.001	*–0.554*	0.015	*4.603*	–0.002	*–0.663*
	K / Sales	–0.045	*–4.455*	0.013	*1.634*	–0.045	*–5.288*
	Leverage	–1.810	*–18.885*	–2.000	*–27.204*	–1.807	*–20.888*
	Cost of capital	–0.021	*–0.749*	0.021	*0.562*	–0.021	*–0.851*
	SIC-2 growth rates	0.000	*0.138*	–0.001	*–0.386*	0.000	*0.188*
Year controls	1992	–0.099	*–1.437*	–0.049	*–0.547*	–0.096	*–1.561*
	1993	–0.103	*–1.518*	–0.055	*–0.599*	–0.102	*–1.699*
	1994	–0.226	*–7.276*	–0.233	*–5.446*	–0.225	*–8.111*
	1995	–0.095	*–3.445*	–0.091	*–2.285*	–0.094	*–3.828*
	1996	–0.129	*–4.849*	–0.125	*–3.104*	–0.126	*–5.346*
Dummies:	SIGMA1	0.001	*0.015*	0.196	*1.941*	0.004	*0.064*
Missing data	R&D / K	–0.010	*–0.117*	0.057	*0.726*	–0.007	*–0.096*
	ADV / K	0.051	*0.709*	–0.103	*–1.515*	0.049	*0.788*
Efficiency variables							
(SFA coefficients measure distance from frontier, so signs are reversed relative to within-groups)							
	Constant			0.063	*2.856*	0.210	*5.858*
	CEO stockholdings	0.025	*4.643*	–0.028	*–9.401*	–0.024	*–8.025*
	(CEO stockholdings)^2	–0.0003	*–2.710*	0.001	*8.524*	0.0003	*4.912*
	CEO optionholdings	0.097	*4.822*	0.047	*7.263*	–0.099	*–7.146*
	(CEO optionholdings)^2	–0.005	*–3.219*	–0.002	*–2.733*	0.005	*4.268*
	vega of options	0.008	*4.901*	–0.009	*–7.206*	–0.007	*–5.473*
	Capital market pressure	–0.352	*–1.180*	0.416	*3.298*	0.397	*1.490*
	Product market pressure	–0.633	*–3.199*	0.437	*6.634*	0.673	*4.467*
Diagnostics							
	σ^2			0.965	*64.594*	0.293	*63.796*
	γ			0.000	*0.521*	0.000	*0.010*
	LR test (χ^2)			122.4 ***		114.8 ***	
	Skewness in residuals test (*p*)	<0.001					
	Significance of fixed effects (*p*)	<0.001					
	Within-groups R^2 (%)	14.71					
	F–test: all coefficients = 0	43.59 ***					
	No. firm-years	8,087		8,087		8,087	
	No. firms	1,487		1,487		1,487	
	Max no. panel years	6		6		6	

Table 4. Panel A.
Correlogram of predicted efficiencies.
Predicted efficiencies are calculated following Battese and Coelli (1988). The predicted efficiencies are derived from the firm fixed effects specification in Table 3. Pairwise correlations are expressed in per cent. One, two and three asterisks indicate significance at p<5%, p<1%, and p<0.1%, respectively.

Predicted efficiency	1996	1995	1994	1993	1992
1997	**49.7*****	0.8	–49.2***	–66.7***	–64.8***
1996		**29.6*****	–42.4***	–62.3***	–58.2***
1995			**5.7***	–30.3***	–42.7***
1994				**38.1*****	7.7**
1993					**52.5*****

Table 4. Panel B.
Predicted efficiencies by empirical specification and sample characteristics.
The predicted efficiencies for the industry and firm fixed effects specifications are calculated using the estimates from Table 3; those for the no fixed effect specification are based on an unreported SFA regression without either industry or firm fixed effects. Predicted efficiencies are expressed in %. Predicted efficiencies by year, size, and industry are derived by partitioning the cross-section of predicted efficiencies from the firm fixed effects specification. The by-year distributions are graphed in Figure 1. For the size partition, companies are sorted into terciles on the basis of their net sales in the first panel year. For the industry partition, companies are sorted into three groups: utilities (two-digit SIC codes 40, 48, 49), financials (two-digit SIC codes 60 to 63), and unregulated industries.

	nobs	mean	stdev	min	Q1	median	Q3	max
All firms								
No fixed effects	8,087	87.1	8.5	1.2	84.0	88.0	91.4	100.0
Industry fixed effects	8,087	91.8	7.5	0.8	90.2	92.4	96.3	100.0
Firm fixed effects	8,087	90.7	3.2	56.1	89.6	90.9	92.1	100.0
By year								
1992	1,338	90.6	3.3	68.5	89.1	90.7	92.0	100.0
1993	1,478	91.2	2.9	64.0	90.1	91.2	92.2	100.0
1994	1,456	90.2	2.8	56.1	89.3	90.3	91.4	100.0
1995	1,397	90.6	2.9	57.9	89.8	90.9	91.8	100.0
1996	1,352	90.6	3.4	62.0	89.6	91.0	92.2	100.0
1997	1,066	91.3	3.7	70.6	89.7	91.4	93.2	100.0
By size								
Small	2,586	90.7	3.7	57.9	89.3	90.9	92.5	100.0
Medium	2,714	90.8	3.0	66.2	89.7	90.9	92.0	100.0
Large	2,787	90.7	2.8	56.1	89.8	90.9	91.8	100.0
By industry								
Unregulated	6,188	90.7	3.4	56.1	89.4	90.9	92.2	100.0
Utilities	946	90.8	2.1	76.9	90.1	90.9	91.5	100.0
Financials	953	90.8	2.5	76.8	89.9	90.8	91.7	100.0

Table 5.
Stochastic frontier estimates by size tercile.
For variable definitions see Table 1. As in Table 4, companies are sorted into terciles on the basis of their net sales in the first panel year. The dependent variable is Tobin's *Q*. One, two and three asterisks indicate significance at p<5%, p<1%, and p<0.1%, respectively.

		SFA with firm fixed effects					
		smallest size tercile		medium size tercile		largest size tercile	
		coeff.	*t*–stat.	coeff.	*t*–stat.	coeff.	*t*–stat.
Frontier variables							
	constant	2.739	*2.891*	3.689	*3.817*	5.352	*5.514*
	ln(sales)	0.080	*2.089*	–0.388	*–1.437*	–0.862	*–3.788*
	ln(sales)^2	–0.013	*–3.324*	0.020	*1.049*	0.055	*4.135*
	SIGMA1	–8.922	*–8.067*	–2.611	*–2.609*	–0.748	*–0.748*
	R&D / K	0.260	*5.463*	0.127	*1.205*	0.809	*7.753*
	ADV / K	–0.014	*–0.124*	0.057	*2.034*	0.077	*1.335*
	CAPEX / K	1.009	*6.960*	1.100	*9.396*	0.173	*1.682*
	Y / Sales	–0.002	*–0.447*	2.707	*13.019*	1.236	*6.919*
	K / Sales	–0.048	*–3.457*	–0.129	*–3.779*	–0.028	*–1.800*
	Leverage	–2.306	*–10.504*	–1.345	*–11.900*	–1.107	*–13.280*
	Cost of capital	–0.023	*–0.285*	–0.033	*–1.136*	–0.019	*–0.727*
	SIC-2 growth rates	0.000	*0.138*	0.001	*0.758*	0.000	*–0.082*
Year controls	1992	0.127	*0.601*	–0.116	*–1.655*	–0.242	*–3.703*
	1993	0.128	*0.639*	–0.143	*–2.024*	–0.260	*–4.475*
	1994	–0.141	*–1.754*	–0.235	*–6.962*	–0.321	*–15.149*
	1995	0.101	*1.477*	–0.144	*–4.810*	–0.251	*–10.423*
	1996	–0.071	*–1.087*	–0.122	*–4.216*	–0.185	*–9.582*
Dummies:	SIGMA1	–0.078	*–0.588*	–0.261	*–3.031*	0.016	*0.152*
Missing data	R&D / K	–0.046	*–0.213*	–0.086	*–0.820*	0.108	*1.842*
	ADV / K	0.046	*0.302*	0.175	*1.748*	0.029	*0.524*
Efficiency variables							
(SFA coefficients measure distance from frontier, so signs are reversed relative to within-groups)							
	Constant	0.157	*4.376*	–0.002	*–0.026*	0.029	*0.748*
	CEO stockholdings	–0.038	*–5.758*	–0.013	*–2.020*	0.001	*0.098*
	(CEO stockholdings)^2	0.0004	*5.043*	0.0003	*2.121*	0.0003	*1.118*
	CEO optionholdings	–0.136	*–4.578*	–0.036	*–1.242*	–0.111	*–3.663*
	(CEO optionholdings)^2	0.010	*2.975*	0.002	*0.593*	0.020	*2.147*
	vega of options	–0.044	*–4.272*	–0.012	*–1.930*	–0.003	*–3.401*
	Capital market pressure	0.326	*0.923*	0.397	*0.905*	0.273	*0.616*
	Product market pressure	1.541	*6.410*	0.742	*2.442*	0.252	*1.293*
Diagnostics							
	σ^2	0.632	*34.051*	0.140	*27.832*	0.088	*20.293*
	γ	0.000	*0.445*	0.017	*0.633*	0.000	*0.334*
	LR test (χ^2)	62.5	***	32.2	***	36.5	***
	No. firm-years	2,586		2,714		2,787	
	No. firms	495		497		495	
	Max no. panel years	6		6		6	

Table 6.
Stochastic frontier estimates by industry: regulated vs. unregulated.

For variable definitions see Table 1. As in Table 4, companies are sorted into three groups: utilities (two-digit SIC codes 40, 48, 49), financials (two-digit SIC codes 60 to 63), and unregulated industries. The dependent variable is Tobin's *Q*. One, two and three asterisks indicate significance at p<5%, p<1%, and p<0.1%, respectively.

		SFA with firm fixed effects					
		unregulated industries		utilities		financials	
		coeff.	*t*–stat.	coeff.	*t*–stat.	coeff.	*t*–stat.
Frontier variables							
	Constant	2.723	*6.462*	3.833	*8.200*	2.712	*0.076*
	ln(sales)	0.067	*2.344*	–0.441	*–4.593*	0.139	*1.310*
	ln(sales)^2	–0.010	*–4.793*	0.029	*3.712*	–0.024	*–3.371*
	SIGMA1	–6.335	*–6.345*	1.268	*1.228*	1.465	*1.444*
	R&D / K	0.253	*7.782*	0.895	*11.099*		
	ADV / K	0.027	*0.735*			–0.221	*–1.377*
	CAPEX / K	1.081	*12.490*	0.293	*2.645*	0.222	*3.474*
	Y / Sales	–0.003	*–1.081*	1.042	*9.496*	0.038	*0.353*
	K / Sales	–0.055	*–4.905*	–0.173	*–7.233*	–0.001	*–0.136*
	Leverage	–2.077	*–19.201*	–1.456	*–14.475*	–0.360	*–5.096*
	Cost of capital	–0.023	*–0.716*	–0.040	*–0.948*	–0.032	*–0.955*
	SIC-2 growth rates	0.000	*0.144*	0.012	*4.262*	0.002	*1.866*
Year controls	1992	–0.084	*–1.071*	–0.157	*–1.662*	–0.276	*–3.482*
	1993	–0.095	*–1.289*	–0.165	*–1.789*	–0.264	*–3.364*
	1994	–0.264	*–7.826*	–0.208	*–5.907*	–0.196	*–6.326*
	1995	–0.095	*–3.090*	–0.107	*–3.288*	–0.147	*–5.116*
	1996	–0.141	*–4.679*	–0.152	*–5.445*	–0.113	*–5.037*
Dummies:	SIGMA1	–0.015	*–0.197*	0.202	*1.994*	0.053	*0.905*
Missing data	R&D / K	–0.057	*–0.587*	0.090	*1.773*		
	ADV / K	0.044	*0.611*			–0.055	*–1.390*
Efficiency variables							
(SFA coefficients measure distance from frontier, so signs are reversed relative to within-groups)							
	Constant	0.107	*6.316*	–0.134	*–6.146*	0.820	*0.023*
	CEO stockholdings	–0.024	*–7.725*	–0.002	*–0.372*	–0.037	*–4.522*
	(CEO stockholdings)^2	0.0003	*3.556*	0.000	*0.096*	0.0004	*2.307*
	CEO optionholdings	–0.107	*–5.479*	0.198	*7.284*	–0.0005	*–0.018*
	(CEO optionholdings)^2	0.006	*4.464*	–0.007	*–3.525*	0.003	*0.620*
	vega of options	–0.009	*–6.047*	–0.009	*–8.011*	0.001	*0.758*
	Capital market pressure	0.413	*2.658*	0.972	*3.864*	–0.522	*–1.310*
	Product market pressure	0.677	*4.734*	–0.633	*–7.290*	–0.030	*–0.113*
Diagnostics							
	σ^2	0.367	*56.808*	0.033	*25.252*	0.022	*21.724*
	γ	0.000	*0.508*	0.019	*6.354*	0.221	*0.802*
	LR test (χ^2)	97.0 ***		44.4 ***		38.2 ***	
	No. firm-years	6,188		946		953	
	No. firms	1,135		172		180	
	Max no. panel years	6		6		6	

Table 7.

Board reactions to managerial inefficiency.

Predicted efficiencies are calculated following Battese and Coelli (1988) using the SFA firm fixed effects estimates from Table 3. They are lagged one year, resulting in a smaller 5-year panel with 6,600 firm-years. The value of restricted stock awards and of option awards are in $'000s. ExecuComp values option awards using a modified form of Black-Scholes. Cash salary is base salary plus all other monetary compensation, but excludes bonus and long-term incentive payments. Bonus is annual bonus payments and excludes long-term incentive payments. Cash salary and bonus are in $'000s. The logit model of CEO replacement uses a dummy equal to one if a CEO aged 60 or under is replaced in year t, and zero else. The dummy does not distinguish between forced and voluntary CEO replacement, though the age condition is intended to control for retirements. For other variable definitions see Table 1. Within-groups are fixed-effects panel data regressions. Logits are estimated with fixed effects (a.k.a. conditional). Tobits are estimated with random effects (given that fixed effects Tobits are inconsistent) with censoring at 0. Standard errors are given in italics below the coefficient estimates. One, two and three asterisks indicate significance at $p<5\%$, $p<1\%$, and $p<0.1\%$, respectively.

Table 7. Cont'd.
Board reactions to managerial inefficiency.

		Dependent variable	constant	predicted efficiency$_{t-1}$	stock holdings$_{t-1}$	option holdings$_{t-1}$	vega of options$_{t-1}$	cash salary$_{t-1}$	bonus$_{t-1}$	SIGMA1	Pseudo /within R^2	Wald or χ^2 test: coeff=0
Panel A: CEO stockholdings												
R1	*within-groups*	Δ_t CEO stockholdings	2.805†	–0.126	–0.599***					5.283	30.9 %	761.8***
			0.995	*1.093*	*0.014*					*6.176*		
R2	*logit*	D=1 if restr. stock award at *t*		–5.005*	–0.022					–15.985	0.6 %	8.9*
				2.112	*0.049*					*12.318*		
R3	*Tobit*	Value(restr. stock grant)$_t$	2,048.3	–3,987.8**	–108.4***					–47,114***	76.8 %	111.8***
			1,369.6	*1,494.7*	*15.9*					*6,981*		
Panel B: CEO optionholdings												
R4	*within-groups*	Δ_t optionholdings	0.469†	0.032		–0.654***				2.049	32.5 %	822.0***
			0.258	*0.285*		*0.015*				*1.566*		
R5	*logit*	D=1 if option award at *t*		–5.248***		–0.243***				–22.818**	2.3 %	59.2***
				1.357		*0.078*				*7.494*		
R6	*Tobit*	Value(option award)$_t$	12,734.2***	–13,618***		339.6***				–32,753***	28.5 %	91.6***
			1,578.9	*1,731.4*		*52.5*				*6,316*		
R7	*within-groups*	Δ_t vega of options	6.835***	–6.605**			–0.149***			–4.936	1.7 %	30.1***
			2.034	*2.214*			*0.020*			*12.943*		
R8	*Tobit*	vega of option award at *t*	68.196***	–52.826***			0.206***			–475.3***	20.5 %	283.6***
			6.038	*6.589*			*0.048*			*32.609*		
Panel C: CEO compensation												
R9	*within-groups*	Δ_t cash salary	–136.7	1,100.2†				–1.031***			19.5 %	619.2***
			573.7	*631.5*				*0.029*				
R10	*within-groups*	Δ_t bonus	2,504.3***	–2,220.2***					–0.922***		44.7 %	2,067***
			503.1	*554.5*					*0.014*			
Panel D: CEO turnover												
R11	*logit*	D=1 if CEO replaced		–9.098**	0.098*						1.6 %	7.5*
				3.521	*0.050*							

Figure 1.
Predicted efficiencies.
The figure plots the predicted efficiencies, normalized on a scale from 0 to 1.0 (the frontier), for the 8,087 observations on the 1,487 firms. Predicted efficiencies are calculated following Battese and Coelli (1988) using the SFA firm fixed effects estimates from Table 3. Observations are ranked by panel year, from highest (left) to lowest (right) estimated efficiency. The mean efficiency is 0.907, and minimum efficiency 0.561. See also Table 4.