

# Firm Value and Managerial Incentives<sup>1</sup>

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## Abstract

We examine the relation between firm value and managerial incentives in a sample of 1,307 publicly-held U.S. firms in 1992-1997. As predicted by Berle and Means (1932), we find that CEOs do not maximize firm value when they are not the residual claimant: our firms have higher Tobin's  $Q$ , the higher are CEO stockholdings. We also investigate the incentive properties of options and find that CEOs appear to hold too *many* options and that these options are insufficiently sensitive to firm risk. Our results do not appear to be driven by endogeneity biases. To assess the economic significance of the suboptimal provision of incentives, we compute an explicit performance benchmark 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. The  $Q$  of the average sample firm is around 12% lower than its  $Q^*$ , equivalent to a \$751 million reduction in its potential market value.

# 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. This spawned a rich literature which has analyzed different mechanisms that serve to align the interests of managers with those of shareholders, including the threat of hostile takeovers, career concerns, and the structure of managerial compensation contracts.

In principle, there are two ways to test what has come to be called the Berle-Means hypothesis. A direct test would examine whether firm value is maximized when managers are not the sole residual claimant. The corresponding test statistic would be based on the difference between a firm's actual value and the value it would attain at the maximum. As we will argue, this test statistic being zero is a necessary and sufficient condition for firm value to be maximized. Alternatively, an indirect test would examine whether firm value can be increased by increasing managerial stockholdings. The prior literature has followed this second route. In this paper, we provide both a direct and an indirect test of the Berle-Means hypothesis.

The prior empirical evidence is mixed. Using data from the early 1930s, the period in which Berle and Means put forward their hypothesis, Stigler and Friedland (1983) find no evidence that manager-controlled firms were less profitable than their shareholder-controlled counterparts. Using more recent data, Demsetz and Lehn (1985) find 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) find a significant relation between firm value, as measured by Tobin's  $Q$ , and managerial stockholdings.

Agrawal and Knoeber (1996) and Himmelberg, Hubbard, and Palia (1999) have recently questioned whether the findings of Mørck, Shleifer, and Vishny (1988) and McConnell and Servaes (1990) are robust given the potential endogeneity of managerial stockholdings. Using simultaneous equations, Agrawal and Knoeber find no relation between firm value and managerial stockholdings and conclude that managerial stockholdings are chosen optimally. Himmelberg, Hubbard, and Palia (1999), henceforth HHP, argue that unobserved heterogeneity across firms can cause biased estimates to the extent that such heterogeneity correlates both with performance and with managerial ownership. They give the example of 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. Assuming that the underlying factors that give rise to the unobserved heterogeneity are constant over time, HHP argue that the inclusion of firm fixed effects can mitigate bias when we have access to repeated observations on the same set of firms (that is, panel data). They find no relation between firm value and managerial stockholdings once fixed effects are included and conclude with Agrawal and Knoeber that managerial stockholdings are chosen optimally.

We contribute to this debate in three ways. First, we reconfirm the finding of Mørck,

Shleifer, and Vishny (1988) and McConnell and Servaes (1990) that CEOs own too little equity *even when we treat managerial stockholdings as potentially endogenous*. Specifically, we begin by re-estimating HHP’s specification in a new panel of 1,307 non-financial publicly-traded U.S. companies in 1992-1997. We show that we cannot replicate HHP’s finding of optimal managerial ownership in our sample: Tobin’s  $Q$  increases significantly in CEO stockholdings. This result does not depend on the choice between OLS and fixed effects. Indeed, the inclusion of firm fixed effects has no effect on our inference: the coefficient estimates for CEO stockholdings are virtually identical. This indicates that CEO stockholdings do not correlate with the fixed effects, or equivalently, that unobserved time-invariant heterogeneity does not, in our data, give rise to bias in OLS regressions as HHP predict.

However, this does not rule out that OLS and fixed-effects are *both* biased, for the underlying factors that give rise to unobserved heterogeneity may in fact vary over time. A formal omitted-variable test indicates that OLS is subject to such bias whereas fixed-effects is not. In practice, however, the bias is small and leaves our result that  $Q$  increases significantly with CEO stockholdings unaffected. Furthermore, we show that we can make the OLS estimates robust by including additional time-varying regressors (leverage, the cost of capital, industry growth forecasts, and analyst following). This reconfirms the earlier findings of Mørck, Shleifer, and Vishny (1988) and McConnell and Servaes (1990). Whilst we cannot say whether those earlier findings were or were not driven by endogeneity bias, our own results suggest that OLS can provide unbiased estimates if the empirical model is sufficiently comprehensive.

Our second contribution is to widen the set of incentive instruments to include CEO options. We do so 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. Indeed, given our finding that CEOs own too little equity, it is natural to ask whether options are used as substitutes for stock. Second, a small but growing literature documents the importance of options as managerial incentives in specific cases.<sup>1</sup> 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 incentives to invest in risky projects.

Like CEO stockholdings, optionholdings and *vega* are potentially endogenous to  $Q$ . We

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<sup>1</sup>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 whether the use of options affects firm value.

test formally whether they are subject to bias in both an OLS and a fixed-effects framework. We cannot reject that *vega* is exogenous with respect to  $Q$  in either framework. We also find that the fixed effects — but *not* the OLS — estimators of optionholdings are biased, even when we include our expanded set of variables. The effect of the bias — which we reconcile with the data — is so large as to reverse the signs compared to the OLS estimates. Using the more robust OLS estimates, we find that the CEOs in our sample own too *many* options. In other words, on average boards have awarded options beyond the point where the marginal cost equals the marginal benefit of doing so. This is consistent with Yermack (1995) who finds little evidence of a connection between CEO option awards and a reduction in agency costs, and with Meulbroek (2000) who provides evidence of deadweight costs which reduce the benefits of awarding options to CEOs. At the same time, we find that CEOs' options are insufficiently sensitive to risk, in the sense that higher *vega* is associated with better performance.

In sum, we find that CEOs' internal incentives — equity, options, and *vega* — are suboptimal. The fact that CEOs face suboptimal incentives implies that firm value is not being maximized, consistent with the Berle-Means hypothesis. How severe a problem is this? Is it first-order or second-order in magnitude? OLS is unable to answer this fundamental empirical question because it lacks a benchmark of expected performance against which to compare a firm's actual performance. Our third contribution is to provide such a benchmark which allows us to put a number on the extent of the performance shortfall. In doing so, we test the Berle-Means hypothesis directly, for we explicitly measure how far companies fall short of value maximization.

The benchmark is derived from an econometric technique called stochastic frontier analysis.<sup>2</sup> Consider a set of firms, each of which has access to the same production inputs. Clearly we would not expect all firms to be equally efficient, even given the same inputs, for 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  $Q$ s than others. The firms with the highest  $Q$ s 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.

To see what difference having an explicit benchmark makes, recall that OLS computes the *average* relationship between (say) CEO stockholdings and  $Q$ , other things equal. But being below-average does not imply underperformance, nor does being above-average imply efficient performance. For example, WorldCom, Inc. had a below-average  $Q$  in 1993 according to OLS but was in fact a frontier-company. ADC Telecommunications, Inc., on the other hand, had an above-average  $Q$  in 1993 and 1994 according to OLS but underperformed its potential

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<sup>2</sup>Stochastic frontier analysis was pioneered by Aigner, Lovell, and Schmidt (1977) and Meeusen and van den Broeck (1977) and is widely used in economic studies of productivity and technical efficiency. Two applications in finance are studies of banking efficiency and a recent article on pricing efficiency in the IPO market (Hunt-McCool, Koh, and Francis, 1996).

$Q$  by more than 10%. The need for an explicit benchmark is analogous to the need for a risk-pricing model in asset pricing: without a measure of a firm’s required return, we cannot compute its excess return from data on actual returns.

The stochastic frontier approach (SFA) is a natural tool for answering the question that is at the heart of the Berle-Means hypothesis and much of corporate finance since Jensen and Meckling: do managers of widely-held companies maximize firm value? A necessary and sufficient condition for the maximization of firm value is that all firms are (stochastically) on the frontier. As we will argue, SFA allows us to test whether this necessary and sufficient condition holds in the data, whereas OLS-based methodologies have only limited power because they focus on a different necessary (but not sufficient) condition: that a particular incentive instrument be chosen optimally.

We find that the  $Q$  of the average sample firm is around 12% lower than the  $Q$  of a hypothetical fully-efficient firm having the same inputs and characteristics. Translated into dollars, this means that the average firm could increase its market value by \$751 million were it to become fully efficient. This suggests not only that our firms are systematically inefficient, but also that the inefficiency is economically significant.

The paper proceeds as follows. We outline our empirical approach in Section 2, where we explain the stochastic frontier approach, discuss its advantages over OLS, and specify our empirical model. Section 3 describes the data and Section 4 presents our empirical results. Section 5 concludes.

## 2 Empirical approach

### 2.1 Stochastic frontier analysis

An OLS (or fixed-effects) 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 and investment. 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.

Using conventional panel-data notation, we can express  $Q$  as a function of a  $(1 \times k)$  set of explanatory variables  $X$ , and an error term  $\varepsilon$ :

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

where  $\beta$  is a  $(k \times 1)$  vector of unknown coefficients to be estimated,  $i = 1, \dots, N$ , and  $t = 1, \dots, T_i$ . Note that the number of observations per company  $T_i$  is allowed to vary across firms. The SFA model thus allows the panel to be unbalanced (see Greene, 1993). The location of the frontier is allowed to shift by virtue of the time-dependence of the  $X$  variables.

It is the special form of the error term  $\varepsilon_{it}$  that enables us to detect possible departures from efficiency. Specifically,  $\varepsilon_{it}$  is composed of two terms:  $\varepsilon_{it} = v_{it} - u_{it}$ . The two-sided error term  $v_{it} \sim N(0, \sigma_v^2)$  denotes the zero-mean, symmetric, iid 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  $u_{it} \geq 0$  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 achieve the highest feasible  $Q^*$  given their inputs and thus be efficient. In that case, SFA would reduce to a conventional regression, for the average function and the frontier function would then be identical.

Following Stevenson (1980),  $u_{it}$  is obtained by truncation at zero of  $N(\mu_{it}, \sigma_u^2)$ . This is a generalization of the half-normal distribution (which has  $\mu_{it} = 0$ ) originally used by Aigner, Lovell, and Schmidt (1977). The half-normal is open to the criticism that it assumes most firms are on the frontier because its mode is zero. The Stevenson log-likelihood is  $\ell(Q; \beta, \mu, \sigma^2, \gamma)$  where  $\sigma^2 \equiv \sigma_u^2 + \sigma_v^2$  and  $\gamma \equiv \frac{\sigma_u^2}{\sigma^2} \in [0, 1]$ . The likelihood function assumes  $cov(u_{it}, v_{it}) = 0$  and  $cov(u_{is}, u_{it}) = 0$ . The former requires the stochastic error  $v$  around the frontier to be independent of the firm efficiencies  $u$ . The latter requires  $u$  to be independently distributed. Serial correlation in  $u$  would violate this assumption. However, this is more of a problem for OLS and fixed-effects regressions than for SFA, because maximum-likelihood estimators are consistent and asymptotically efficient (see Davidson and McKinnon, 1993, pp. 255-260 for the general result and Gourieroux, Monfort, and Trognon, 1985, for frontier models).

## 2.2 Testing for and explaining departures from efficiency

It is immediate from the structure of the error term  $\varepsilon = v - u$  that  $u = 0$  is a necessary and sufficient condition for efficiency and value maximization: firm  $i$  maximizes its  $Q$  at time  $t$  if and only if it is on the frontier given its inputs and characteristics, that is, if and only if  $u_{it} = 0$ .  $u = 0$  is therefore a direct test of the Berle-Means hypothesis. We can test  $u = 0$  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  $\sigma_u^2 = 0$  so the likelihood function of the SFA specification will be identical to the least-squares likelihood function. But if  $u_{it} > 0$  for sufficiently many  $i$  and  $t$ , then the SFA specification will lead to a likelihood gain because OLS wrongly restricts  $\sigma_u^2 = 0$ . The likelihood-ratio test corresponds to testing whether the average and the frontier functions are identical.

We can measure the degree of a firm's inefficiency using the estimated  $\hat{u}_{it}$ . We will usually report normalized predicted efficiencies, which are defined as the ratio of a firm's actual  $Q$  to the corresponding  $Q^* \equiv Q + u$  if it were fully efficient:  $\widehat{PE}_{it} = \frac{E(Q_{it}|\hat{u}_{it}, X_{it})}{E(Q_{it}^*|\hat{u}_{it}=0, X_{it})}$ ; 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.

A rejection of the null hypothesis of efficiency  $u = 0$  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 on 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, because the two-stage procedure violates the assumption that  $u_{it}$  is independent of  $v_{it}$ . To implement 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:

$$u_{it} = Z_{it}\delta + w_{it} \quad (2)$$

where  $Z_{it}$  is a  $(1 \times m)$  set of variables which we will refer to as ‘incentives’,  $\delta$  is a  $(m \times 1)$  vector of unknown coefficients to be estimated, and  $w_{it}$  denotes the unexplained component of  $u_{it}$ . In terms of the MLE, (2) models the mean of  $u \sim N(\mu_{it}, \sigma_u^2)$  as  $\mu_{it} = Z_{it}\delta$ . The dependence on both  $i$  and  $t$  explains why SFA does not assume  $u$  to be identically (homoskedastically) distributed.  $w$  is obtained by the truncation of  $N(0, \sigma_w^2)$  such that the point of truncation is  $-Z_{it}\delta$ , that is  $w_{it} \geq -Z_{it}\delta$ . The log-likelihood corresponding to the model specified by (1) and (2) is  $\ell(Q; \beta, \delta, \sigma^2, \gamma)$ .

A measure of our ability to explain the determinants of inefficiency — the appropriateness of our choice of  $Z$  variables — is the variance of the residual error term  $w_{it}$ . 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 therefore be based on  $\gamma = \frac{\sigma_w^2}{\sigma^2}$ , the ratio of the unexplained error and the total error of the regression (Aigner, Lovell, and Schmidt, 1977).  $\gamma$  will be zero if our  $Z$  variables fully account for departures from the frontier.

Note that the inefficiencies  $u_{it}$  and their determinants  $Z_{it}$  are allowed to vary over time. The SFA specification can therefore accommodate changes in a firm’s position relative to the frontier over time, and link such changes to changes in the incentives given to CEOs. Because the  $X_{it}$  variables are also time-varying, the location of the frontier itself can also change over time.  $u_{it}$  measures inefficiency net of such changes in the location of the frontier.

## 2.3 SFA versus least-squares regressions

Asymptotically, SFA will in general give the same coefficient estimates as OLS (or as fixed-effects, if we add an intercept for each firm).<sup>3</sup> SFA coefficient estimates will therefore be subject to the same biases as their OLS or fixed-effects counterparts. In other words, if any of the regressors are endogenous or the specification suffers from omitted variable bias, SFA will be no more robust than OLS. Of course, robustness is testable, and we will show in Section 4 that we cannot reject the null hypothesis that the regressors included in our empirical model are exogenous. In that regard, our SFA model is robust, but this does not provide a justification for preferring it to OLS. In this section, we outline three advantages of SFA over OLS and similar techniques.

<sup>3</sup>This is true of the coefficients  $\beta$  for the frontier variables  $X$ . It is true of the coefficients  $\delta$  for the incentive variables  $Z$  in case all inefficiency has been explained ( $\gamma = 0$ ). If  $\gamma > 0$ , it can be shown that  $\delta_{OLS}$  will be biased, for the  $Z$ s will then correlate with the error term  $w$  (which has distribution  $N(0, \sigma_w^2)$  with upper truncation at  $-Z\delta$ ).  $\delta_{SFA}$ , on the other hand, is consistent by the properties of maximum likelihood. Since  $\gamma = 0$  for most of our results, this potential bias of OLS is not evident in our data.



## A direct test for efficiency

SFA allows us to separate two related questions: *i*) do firms perform efficiently and *ii*) if not, does the degree of inefficiency depend on suboptimal incentives? The distinction between *efficiency* in overall performance and the *optimality* of a particular incentive instrument is not purely semantic: efficiency implies that all incentive instruments have been chosen optimally, but the optimality of any one incentive instrument does not imply efficiency. The following finding may illustrate the difference. In Section 4.4, we show that the tercile of the largest companies in our sample award their CEOs stocks and options optimally. This, however, does not warrant the inference that these firms are maximizing firm value. Indeed, such inference would be wrong: the SFA estimates tell us that the average large firm underperforms the frontier by 12%. More generally, the optimal provision of a particular (say equity) incentive is only a necessary — not a sufficient — condition for value maximization: there may be substitutes and complements to equity incentives which boards may *not* have chosen optimally. Since we can never know for sure if we have included *all* relevant incentive variables in OLS, the apparent optimality of even a large number of potential incentive variables does not allow us to infer that a sample of firms has maximized firm value. Therefore, in focusing on a particular (set of) incentive variable(s), OLS can at best provide an indirect test of the Berle-Means hypothesis.

SFA allows us to test separately for efficiency and optimality. The test for efficiency,  $u = Q^* - Q = 0$ , is based on estimating directly whether a given company performs as well as its inputs and characteristics suggest it is capable of, given ‘best practice’ as embodied in the frontier. Estimating  $u$  does not require knowledge of the relevant incentive variables. It is in this sense that  $u = 0$  is both a necessary and a sufficient condition for the maximization of firm value and thus a direct test of Berle-Means. (Of course, whether we can estimate  $u$  with the required precision is an empirical question. It is, for instance, possible that our sample does not include the true frontier companies. Because of this, our test is a conditional one.) The test for optimality in the provision of incentives is based on  $\delta = 0$ . As in the OLS-based approaches of the earlier literature, we view the  $\delta$  coefficients as estimates of the partial derivatives of  $Q$  with respect to the incentive variables, which at the optimum must be zero.

## Benchmarking

The second advantage that SFA provides is based on the first. Suppose we find that the OLS coefficient of CEO stockholdings is strictly positive. We can infer that firms are not maximizing firm value —  $Q$  could be increased by increasing CEO stockholdings — but we cannot infer anything about the extent of the problem: is it first-order or second-order?<sup>4</sup> In order to assess the extent of the problem we require a benchmark against which to judge actual performance, and against which to compute the performance shortfall. OLS does not provide such a benchmark.<sup>5</sup> SFA, on the other hand, does provide a benchmark: the frontier.

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<sup>4</sup>Extrapolation based on the magnitude of the estimated coefficient is suspect since we do not know the functional form of the partial derivative beyond the immediate vicinity of the estimate itself.

<sup>5</sup>If fixed effects are added to OLS, a benchmark of sorts can be derived. If the  $N$  estimated fixed effects are  $\hat{\alpha}_1 \dots \hat{\alpha}_N$ , then  $\hat{u}_i \equiv \max(\hat{\alpha}_j) - \hat{\alpha}_i$  is an estimate of the distance from the frontier (see Schmidt and Sickles, 1984). This approach is followed in Maksimovic and Phillips (2000). There are, however, four shortcomings

To appreciate the difference this makes, consider the following analogy with asset pricing. Suppose we draw a sample of stocks whose average return is 10%, with some firms returning more and others less. If we do not know the required return for each stock, we can obviously say very little about the ‘excess’ returns achieved by any one company: being above-average does not mean the stock outperforms its true required return, nor does being below-average imply underperformance. In the absence of a performance benchmark, we cannot compute abnormal returns.

So, much like we need to benchmark observed returns against required returns (given risk) in asset pricing, we need to benchmark observed company performance against potential performance (given inputs and characteristics) in testing the Berle-Means hypothesis.

In providing the required benchmark, SFA allows us to put a number on the economic magnitude of the problem. In our sample, we find that the average firm underperforms the frontier by close to 12%, or \$751 million in ‘lost’ market value. This to us appears to be first-order.

## Power

Under certain conditions, SFA has greater power to find the causes of inefficiency than does OLS. Formally, a test for efficiency by means of a zero slope coefficient on an incentive variable is biased against rejecting the null hypothesis of efficiency precisely when the null is false. To see the economic intuition for this result, 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 statistically inefficient estimates when least-squares or similar techniques are used and reduces the power of the zero-coefficient test for efficiency.<sup>6</sup> 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 efficiency 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, leading to greater power.

## 2.4 The empirical model

To implement the stochastic frontier approach, we need to specify the relevant  $X$  (input and firm characteristics) and  $Z$  (incentive) variables.<sup>7</sup> Our preferred specification includes many

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relative to SFA. *i*) The  $\hat{\alpha}_i$  are consistent only as  $T_i \rightarrow \infty$ . *ii*) The company with the largest  $\hat{\alpha}_j$  is taken to be 100% efficient, which does not allow for random errors in the position of the frontier. *iii*) The inefficient and the efficient firms have equal influence on the location of the frontier, while SFA allows the most efficient firms to have greater influence on the location of the frontier if estimated using maximum likelihood. And *iv*) the  $u_i$  are assumed to be constant over time, which economically is unduly restrictive (Maksimovic and Phillips overcome this restriction by estimating  $\hat{u}_i$  recursively).

<sup>6</sup>See Greene (1997), pp. 309-310.

<sup>7</sup>We frame the discussion in terms of input ( $X$ ) and incentive ( $Z$ ) variables for expositional convenience. A priori, the distinction between what is an  $X$  or  $Z$  variable is not always clear. Leverage, for instance, could

$X$  variables that have been used extensively in previous analyses of Tobin’s  $Q$ :

$$\begin{aligned}
Q_{it} = & \beta_0 + \beta_1 \ln(\overline{sales}_{it}) + \beta_2 \ln(\overline{sales}_{it})^2 + \beta_3 \overline{SIGMA1}_{it} \\
& + \beta_4 \frac{R\&D_{it}^+}{K_{it}} + \beta_5 \frac{ADV_{it}^+}{K_{it}} + \beta_6 \frac{CAPEX_{it}^+}{K_{it}} \\
& + \beta_7 \frac{Y_{it}^+}{sales_{it}} + \beta_8 \frac{K_{it}^?}{sales_{it}} + \beta_9 \left( \frac{K_{it}^?}{sales_{it}} \right)^2 \\
& + \beta_{10} \overline{leverage}_{it}^? + \beta_{11} \overline{R}_{it} + \beta_{12} \overline{growth}_{it}^+ + \beta_{13} \overline{analysts}_{it}^+ \\
& + \text{missing-value and year dummies} + \varepsilon_{it}
\end{aligned} \tag{3}$$

where we have indicated the signs we expect using +, – and ? above the variables. The precise variable definitions are given in Section 3.2. Here, we focus on their economic meaning and the predicted signs.

The first nine regressors are those used in HHP. Log  $sales$  and its square control for firm size.  $SIGMA1$  is a measure of firm-specific risk. HHP suggest riskier firms have lower  $Q$ s, ceteris paribus, because risk affects the extent to which a risk-averse manager can be incentivized via stock- and optionholdings. ‘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 of profitability and therefore, possibly, market power and should thus be positively related to  $Q$ .  $\frac{K}{sales}$  and its square control for the relative importance of tangible capital in the firm’s production technology. A priori, there are two opposing effects. On the one hand, firms whose capital is relatively less tangible may be subject to greater agency problems as capital providers cannot observe, monitor, and assess spending on intangibles as easily. They may therefore have lower  $Q$ s. On the other hand, measures of  $Q$  tend to understate the replacement cost of intangibles — which are hard to value — which could induce a negative relationship between  $Q$  and the firm’s tangible capital intensity.

Regressors 10 through 13 are not part of the original HHP specification. We will argue in Section 4 that these additional variables help mitigate endogeneity and omitted variable biases. In a Modigliani-Miller world,  $leverage$  should not affect firm value. 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  $Q$ s. 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

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well be a  $Z$  variable: debt might have incentive properties. We investigate all possible specifications and compare them using Akaike’s Information Criterion. The specification we report is the one that maximizes the log-likelihood.

including industry *growth* rate forecasts obtained from securities analysts. We also control for the intensity of *analyst* following which we expect to have a positive effect on  $Q$ .

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. All results are robust to excluding missing observations instead.

Since we have already accounted for random influences on value (such as bad luck or windfalls) via the  $v_{it}$  errors around the frontier, we assume inefficiency  $u$  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. Our set of  $Z$  or incentive variables is:

$$\begin{aligned}
 u_{it} &= \delta Z_{it} + w_{it} \\
 &= \delta_0 + \delta_1 \text{stockholdings}_{it} + \delta_2 \text{stockholdings}_{it}^2 \\
 &\quad + \delta_3 \text{optionholdings}_{it} + \delta_4 \text{optionholdings}_{it}^2 \\
 &\quad + \delta_5 \text{vega}_{it} \\
 &\quad + \delta_6 \text{capital market pressure}_{it} \\
 &\quad + \delta_7 \text{product market pressure}_{it} + w_{it}
 \end{aligned} \tag{4}$$

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 Yermack (1995) and 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.<sup>8,9</sup> As in previous studies, we include squared terms for stock (and option-) holdings to allow for non-linearities in their relationship with Tobin’s  $Q$ . In addition to providing effort incentives via equity and option awards, boards may also try to induce the manager to choose higher risk projects by making their payoffs more convex. This would increase  $Q$  if the manager currently foregoes positive NPV projects on account of his personal risk aversion. 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.<sup>10</sup>

<sup>8</sup>See Yermack (1995) and Baker and Hall (1999) for a formal analysis.

<sup>9</sup>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.

<sup>10</sup>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 under-investment in valuable risk-increasing projects is greatest” (p. 43).

The final 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; Scharfstein, 1988). *Product market pressure*, a measure of the degree of product market competition, 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  $Q$ s. On the other, firms in less competitive markets might earn higher economic rents and thus have higher Tobin's  $Q$ s.<sup>11</sup>

Our empirical model does not (for lack of data) include every conceivable incentive variable in our attempt to find the determinants of inefficiency. For instance, we have no data on three additional incentive instruments that Agrawal and Knoeber (1996) make use of: institutional shareholdings, the use of outside directors, and the managerial labor market. 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 utilities and small 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.<sup>12</sup> 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 ten firms with dual CEOs and one firm for which no Compustat data was available. In common with the literature, we also exclude all financial-services companies (SIC codes 60 to 63), as accounting data for these is not directly comparable to that of other companies. This leaves a total sample of 1,307 firms.

The panel runs from 1992 to 1997 and consists of a total of 7,134 firm-years. This is 708 short of the theoretical maximum of 1,307 firms  $\times$  6 years. There are two reasons why the panel is unbalanced: attrition and missing data. 176 of the 1,307 companies delist prior to 1997, resulting in a loss of 359 firm-years (an attrition rate of 5%). Of these, 162 are taken over, ten are delisted due to violation of listing requirements, two cease trading for unknown

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<sup>11</sup>We also investigate whether greater use of debt improves efficiency, as in Jensen's (1986) free cash flow hypothesis, but find no significant effect.

<sup>12</sup>We investigate what happens to companies which ExecuComp drops completely and find that all but two of these delist.

reasons, one is declared insolvent, and one is liquidated. Given the low attrition rate, we do not expect attrition bias to be a serious problem. A comparison of the Tobin's  $Q$ s of the 176 takeover targets and the surviving firms confirms that there are no systematic differences in performance. The second cause of the unbalanced nature of the panel is missing data, affecting 349 firm-years. In the main, missing data causes companies to 'leave' our panel before 1997. For instance, the October 1998 ExecuComp CD-ROM reports no 1997 data for 183 companies, 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

A summary of our variable definitions can be found in Table 1. In general, our 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,<sup>13</sup> 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. Our results are robust to adopting HHP's broader focus.

In what follows, we detail the derivation of our additional  $X$  (leverage, cost of capital  $R$ , industry *growth* forecasts, analyst following) and  $Z$  (optionholdings, and capital and product market pressure) variables, as well as Tobin's  $Q$ .

*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  $\beta$  estimates. Instead, Fama and

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<sup>13</sup>Only 123 of the 1,307 sample companies report a constant number of officers in every panel year.

French (1997) provide various estimates of industry risk premia  $\beta_j[R_M - R_f]$  for  $j = 1, \dots, 48$  industries defined at the four-digit SIC level. After assigning our firms to Fama and French's 48 industries, we compute time-varying industry costs of capital  $R_{jt} = R_{f,t} + \beta_j[R_M - R_f]$ , using Fama and French's one-factor model estimates over the five years ending December 1994 (taken from their Table 7, pp. 172-173).  $R_{f,t}$  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.

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

*Analyst following.* We measure the intensity of security analyst following as the maximum of the number of analysts reported in I/B/E/S as giving either a one-year, two-year, three-year or long-term growth forecast for a given sample firm in or before its fiscal year-end month.

*CEO optionholdings.* To measure the effort and risk properties of a CEO's optionholdings, we need to estimate 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<sup>14</sup>

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

and

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

where  $d$  is  $\ln(1+\text{expected dividend yield})$ ,  $S$  is the fiscal year-end share price,  $T$  is the remaining time to maturity,  $N$  and  $N'$  are the cumulative normal and the normal density functions, respectively, and  $Z$  equals  $\frac{\ln(S/X) + T(r - d + \frac{1}{2}\sigma^2)}{\sigma\sqrt{T}}$ , where  $X$  is the strike price,  $r$  is  $\ln(1+\text{riskfree rate})$ , and  $\sigma^2$  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 72 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 *delta* and *vega* for

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<sup>14</sup>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)

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 a sample company whose  $Q$  we observe at the end of December 199X, the probability of delisting equals the fraction of all CRSP-listed companies in its two-digit SIC industry which were delisted between January and December 199X due to merger, bankruptcy, violation of exchange requirements etc. We do not attempt to distinguish between ‘involuntary’ and ‘voluntary’ delistings as we do not know the motivation behind the mergers and takeovers. The justification for estimating industry-specific measures of capital market pressure is the finding of Palepu (1986) and Mitchell and Mulherin (1996) that takeover activity has a strong industry component.

*Product market pressure.* To measure product market pressure, we 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. 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 1.985 (1.569).  $Q$  is significantly greater than 1, but since we compute average rather than marginal  $Q$ , there is no reason to expect  $Q = 1$ . Sample firms are large, with average (nominal) sales of \$3.1 billion, though this is partly driven by the quartile of largest firms: the 75th percentile firm has sales of \$2.7 billion and the largest (Ford Motor Company) has 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 zero  $R\&D$  and  $ADV$  expenditure. The average rate of capital formation  $\frac{CAPEX}{K}$  in the sample is 23.6%. The average firm has a negative operating margin, though this is heavily influenced by the four percent of firm-years in which operating income is negative. The median operating margin of 14.5% is thus more informative. Our sample firms appear very capital-intensive, given median  $\frac{K}{sales}$  of 0.29: they use 29 cents of tangible capital to generate



a dollar of sales. The average firm has 19% leverage, with a range from 0% to 99.8% (Payless Cashways, Inc., which subsequently sought Chapter 11 protection from its creditors). Cost of capital estimates vary between 5.9% and 12.7% nominal, with a mean and median just below 10%. Industry growth rate forecasts average 16.6% per annum, with a range from 2.8% to 35.7%. The average company is followed by 12 securities analysts.

The lower half of Table 2 lists the incentive variables. The average CEO owns a mere 3.4% of his firm, with an even lower median of 0.4%. Not surprisingly, CEO ownership depends on firm size, averaging 6.8% in the smallest quartile and 1.1% in the largest (results not shown). Option ownership, which in the table is defined as the number of options held divided by shares outstanding, averages 1%. For the median firm, option ownership is 0.5%, higher than median CEO stock ownership. This is consistent with Murphy’s (1998) finding that CEOs’ option ownership has come to rival their direct equity ownership. However, these numbers are not directly comparable, for the incentive properties of an option are proportional to *delta*, which has a median value of 0.67 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 *vegas* for 278 CEOs in 1993 of 16.7 and 15.6, about 40% higher than our estimates. The average firm faces a 5.9% probability of delisting in a given year, our measure of capital market pressure. Just under half the firms operate in unconcentrated industries (defined by the Federal Trade Commission as a Herfindahl index value below 1,000), a quarter in moderately concentrated industries (Herfindahl values between 1,000 and 1,800), and the remaining quarter in highly concentrated industries (Herfindahl values >1,800).

## 4 Empirical results

The discussion of our empirical results is structured as follows. In Section 4.1, we investigate how well different econometric estimation techniques deal with the problem of endogeneity bias and provide OLS and fixed-effects estimates of the optimality of CEO stock- and optionholdings and of *vega*. In Section 4.2, we estimate a stochastic frontier model. We first estimate the location of the frontier (equation (3)) and discuss its determinants. We then test whether firms maximize  $Q$  and find evidence of systematic inefficiency. In Section 4.3, we attempt to identify the causes of inefficiency (equation (4)) by relating the degree of inefficiency to the internal and external incentives. In Section 4.4, we show that our results are robust to sample partitions by size, to outliers, and to alternative variable definitions. Finally, in Section 4.5, we ask whether boards adjust internal incentives to improve performance over time.

### 4.1 Estimation technique and endogeneity bias

We first re-estimate HHP’s specification in our data, using both OLS and fixed effects regressions. Specifically, we regress Tobin’s  $Q$  on the subset of regressors in equations (3) and (4) which were previously used by HHP. The OLS and fixed effects results are reported in the first two columns in Table 3. We defer a discussion of the coefficients estimated for what we call the  $X$  variables until the next section and focus for now on the one incentive

variable, CEO stockholdings, included in the HHP specification. Both the OLS and the fixed-effects coefficients indicate that  $Q$  increases in CEO stockholdings and decreases in its square, and that it does so significantly. This implies the same sort of inverse U-shaped relationship previously found by McConnell and Servaes and Mørck, Shleifer, and Vishny. We cannot replicate HHP’s finding of optimal managerial ownership in our data, whether we use OLS or include fixed effects. CEOs appear to own too little equity. Since optimality in the provision of equity incentives is a necessary condition for value maximization, we can infer that our sample firms will not be efficient, so we expect to find  $u > 0$  when we turn to the SFA specification in the next section.

The OLS and fixed-effects coefficient estimates for CEO stockholdings are very close and indeed not significantly different from each other in a Wald test. This is what we would expect if the covariance between CEO stockholdings and the fixed effects was zero, because the bias in OLS is proportional to that covariance:  $plim_{N \rightarrow \infty} \hat{\delta}_{k,OLS} = \delta_k + \frac{cov(z_{it}, \alpha_i)}{\sigma_z^2}$  where  $\delta_k$  is the true parameter to be estimated,  $z_{it}$  is the  $k$ th element of  $Z$  (here: CEO stockholdings), and  $\alpha_i$  is firm  $i$ ’s fixed effect.<sup>15</sup> In our data, the second term in the  $plim$  equals 0.0006 with  $p$ -value 0.70, so it is not surprising that it does not matter whether we include fixed effects for the purpose of investigating the provision of CEO stock incentives. In other words, we find no evidence in our data for HHP’s argument that unobserved but time-invariant heterogeneity causes OLS to be biased.

This does not, however, imply that the estimators are consistent, for there could be unobserved time-*varying* variables that correlate both with  $Q$  and with the incentive variables. If so, their omission would cause both OLS and fixed-effects estimators to be biased. To test for such bias, we use a Durbin-Wu-Hausman omitted variable test (see Davidson and MacKinnon, 1993, pp. 237-242). The test is formed by including the residuals of each potentially endogenous right-hand-side variable (here: CEO stockholdings), as a function of all exogenous variables, in a regression of the original model (here: of  $Q$  on  $X$  and  $Z$ ). To ensure that the auxiliary regression is identified, it must include at least one exogenous variable that is not also included in the original  $Q$  model. We use CEO age and verify that it correlates with CEO stockholdings but not with  $Q$ .

The DWH test will reject the null of exogeneity when the coefficient on the residual is significantly different from zero. If the test does reject, we ought to use an instrumental variable, for otherwise our set of estimates obtained by least squares — with or without fixed effects — would be inconsistent. The test statistics, reported in Table 3, indicate that OLS is subject to endogeneity bias, while fixed effects is not. However, as we have already shown, the consequence of this bias appears to be small given that the OLS and fixed-effects coefficient estimates are virtually identical. It thus does not affect our inference that CEOs own too little equity.

The same may not be true when we include the other internal incentive variables of interest, CEO optionholdings and *vega*. If we add these to the HHP model (not shown), the DWH tests indicate that the coefficients estimated for CEO optionholdings in *both* the OLS and fixed-effects regressions are significantly biased at the 0.1% level.<sup>16</sup> (We do not

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<sup>15</sup>The expression for  $plim_{N \rightarrow \infty} \hat{\delta}_{k,OLS}$  assumes  $cov(x_{it}, z_{it}) = 0$ .

<sup>16</sup>To identify the auxiliary regressions, we use the dividend yield in the case of optionholdings and the variance of the per-industry delisting probability in the case of *vega*. As required, these variables correlate

reject the null that *vega* is exogenous in either specification.) In other words, controlling for omitted variables by including fixed effects does *not* mitigate the bias in estimating the effect of options. This suggests that the cause of the bias must lie elsewhere, most plausibly in the omission of time-varying variables that correlate both with  $Q$  and with the incentive variables. We therefore expand the set of regressors beyond that used by HHP, to include four further  $X$  variables: leverage, the cost of capital, industry growth forecasts, and analyst following. The OLS and fixed-effects results are reported in columns 3 and 4 of Table 3, respectively. We also include the two additional external incentive variables, capital and product market pressure, so we are estimating the complete model defined by equations (3) and (4). We take these to be exogenous in the sense of being outside the board’s control. The results we report next are not driven by their inclusion.

The first key result is that after including the additional  $X$  variables, OLS provides unbiased estimates for all potentially endogenous variables, according to the DWH tests. This includes CEO stockholdings, whose exogeneity we previously rejected in the HHP specification. Our additional regressors thus appear to correlate sufficiently well with the true omitted variables to reduce the bias substantially. Comparing the coefficient estimates in the first and third column, we see that the bias in practice is small to begin with.

The second key result is that fixed effects does *not* provide unbiased estimates for the potentially endogenous variables: the DWH tests reject the exogeneity of CEO optionholdings individually (at the 1% level) as well as the exogeneity of all three potentially endogenous variables jointly (at the 5% level). The effect of this bias in the fixed effects regression is so large as to reverse the signs of the options coefficients: while  $Q$  in the OLS specification is negatively and concavely related to CEO optionholdings, the relationship is positive and concave in the fixed-effects specification. In either case the coefficients are significantly different from zero and from each other. For inference purposes, it clearly matters whether or not we include fixed effects, and the DWH tests tell us not to. We will therefore proceed without fixed effects, though due to the often weak power of endogeneity tests our results concerning optionholdings must be interpreted with caution.

The OLS results give rise to the following, perhaps surprising inference: the CEOs in our sample hold too many options. In other words, on average boards have awarded options beyond the point where the marginal cost equals the marginal benefit of doing so. The fixed-effects coefficients tell the opposite story: CEOs appear to hold too few options. The DWH test results notwithstanding, we might question whether the OLS estimates really paint a truthful picture. To investigate this further, we focus on the functional relationship between  $Q$  and optionholdings. Specifically, we generate OLS and fixed-effects predictions of  $Q$  for different levels of optionholdings, holding the other covariates at their sample means. This reveals that  $Q$  is highest if a CEO holds no options at all, unless optionholdings are very high. CEOs who hold no options often include founder-entrepreneurs with very large equity ownership (such as Bill Gates) whose companies trade at high market values. To test if our results are driven by such ‘outliers’, we dropped *i*) all firms whose CEOs hold no options and *ii*) all firms whose CEOs own more than 10% of equity. In either case, our results are unaffected. Moreover, the DWH tests continue to reject in the fixed-effects specification but

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with the respective endogenous variables but not with  $Q$ .

not in the OLS specification.<sup>17</sup>

## 4.2 Frontier estimates and tests for inefficiency

### Locating the frontier

In Section 2.3 we argued that OLS (or fixed-effects) estimates will have lower power than SFA if the errors are skewed. Are they? Based on the residuals in all four specifications reported in Table 3, we reject zero skewness at  $p = 0.1\%$  (see the Diagnostics Section). 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. This could affect the two  $Z$  variables whose OLS coefficients in Table 3, column 3 are not significant (capital market pressure) or just barely so (*vega*).

We now estimate the model defined by (3) and (4) using a stochastic-frontier maximum likelihood model with time-varying inefficiencies  $u_{it}$ , based on Reifschneider and Stevenson (1991) and Battese and Coelli (1995) and defined in Sections 2.1 and 2.2. The upper part of Table 4 lists the coefficient estimates for the frontier variables alongside standard  $t$ -statistics. As expected, the coefficients are virtually identical to those estimated via OLS shown in Table 3, column 3.

The frontier variables all have the predicted sign. The maximum-attainable Tobin's  $Q$  decreases significantly with  $\log sales$  and increases slowly with its square, with a turning point outside the range for sales in our data. It is similarly U-shaped in tangible capital-intensity  $\frac{K}{sales}$  with a turning point at 22%.  $Q$  decreases significantly in firm-specific risk  $SIGMA1$ , and in *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.<sup>18</sup>  $Q$  increases in 'soft' and 'hard' expenditures on research and development and capital formation, respectively, in operating margins  $\frac{Y}{sales}$ , and in forecast industry growth rates. It also increases in analyst following. The  $Q$  frontier appears to be invariant to advertising spending, as was also the case in HHP, and to our measure of the cost of capital.

The sample contains 172 utility companies whose economic behavior may differ from that of other firms. We therefore partition the sample into utilities (two-digit SIC codes 40, 48, and 49) and unregulated firms and estimate individual stochastic frontiers for each subsample. (We exclude  $\frac{ADV}{K}$  from the regression for utilities as utilities report no advertising expenditure.) As Table 4 shows, there are no major differences in the frontier variables between the sample as a whole and the subsample of unregulated firms. Comparing the two subsamples, the signs are the same with two exceptions: firm-specific risk  $SIGMA1$  lowers  $Q$  amongst unregulated firms but has no effect on utilities, and higher costs of capital have a negative effect on  $Q$  amongst utilities but not unregulated firms. The magnitudes of some of the other coefficients differ. For instance, operating margins and spending on R&D have

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<sup>17</sup>We are grateful to David Yermack for suggesting this robustness check.

<sup>18</sup>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.

larger effects on  $Q$  for utilities, while *leverage*, spending on *CAPEX*, and analyst following have smaller effects.

### Testing for inefficiency

We have already shown that the OLS 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 4 reports likelihood ratio tests of the null hypothesis that the one-sided SFA error terms  $u$  are zero. Recall that this is a necessary and sufficient condition for firm value maximization. We comfortably reject the null in the sample as a whole and both subsamples ( $p = 0.1\%$ ).

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,  $\widehat{PE}_{it} = \frac{E(Q_{it}|u_{it}, X_{it})}{E(Q_{it}^*|u_{it}=0, X_{it})}$ . 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 inefficiency from year to year and possibly reversals over longer periods (as boards take action to reduce inefficiency). Table 5, Panel A shows a correlogram of the predicted efficiencies, estimated using the results for the sample as a whole in Table 4. There is clear evidence of significant positive correlation across all lags, consistent with persistence in (in-)efficiency.<sup>19</sup> We are thus not picking up random movements in inefficiency. The correlations tend to decline with longer lags. In Section 4.5, we will investigate whether changes in inefficiency over time are related to board actions.

Table 5, Panel B reports distributional characteristics of the predicted efficiencies calculated using the estimates for the sample as a whole in Table 4. The average predicted efficiency is 88.4%, meaning that the average firm underperforms the frontier by around 12%. Translated into dollars, this implies that the market value of the average firm would be \$751 million higher were it to move to the frontier.<sup>20</sup> These results do not change appreciably when we change the way we classify variables as  $X$  and  $Z$ . For instance, predicted efficiencies average 90.6% when we classify *capital* and *product market pressure* as  $X$  variables instead.

Panel B also compares predicted efficiencies by year, size and industry, derived by partitioning the cross-section of predicted efficiencies. For the size partition, companies are sorted into terciles on the basis of their net sales in the first panel year. Inefficiency appears to be present in all years and amongst companies of all sizes. However, this does not preclude the possibility that the *causes* of inefficiency differ across size terciles. We investigate this pos-

<sup>19</sup>Note that the serial correlation in the predicted efficiencies cannot be attributed to possible serial correlation in the two-sided error terms, for the former do not depend on the latter in a case such as ours where all departure from the frontier has been explained (see Section 4.3). We thank the referee for suggesting this alternative explanation.

<sup>20</sup>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.

sibility in Section 4.4. The average utility has a lower predicted efficiency than the average unregulated firm.

## Summary

In locating the stochastic frontier, we find results which mirror those of HHP’s earlier study:  $Q$  first decreases and then increases with firm size and tangible capital intensity; increases in soft (R&D) and hard (capital-formation) spending, operating margins, forecasts of industry growth, and analyst following; and decreases in firm-specific risk and leverage. We can comfortably reject the null that all firms are efficient ( $u = 0$ ) using a likelihood ratio test. The extent of inefficiency, which implies a \$751 million shortfall from the average firm’s potential market value, appears first-order economically. The time series behavior of firms’ predicted efficiencies is much more consistent with systematic rather than random departures from the frontier and thus in inefficiency. Partitioning the predicted efficiencies by year and firm size reveals no particular clustering in inefficiency. Utilities are more prone to inefficiency than are unregulated firms.

### 4.3 Identifying the causes of inefficiency

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

We first discuss the results for the sample as a whole.

Overall, our  $Z$  variables are quite successful at accounting for departures from the frontier:  $\gamma$ , which measures the relative importance of the unexplained part,  $w_{it}$ , of equation (4) and the overall error of the SFA regression, is very close to zero and not statistically significant.

All but one of the  $\delta$  coefficients are significant.<sup>21</sup> 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 the distance from the frontier. Based on the parameter estimates, greater stockholdings increase efficiency up to 30.8% 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 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 1.985 to 2.175. 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

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<sup>21</sup>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.

value. The average firm has assets of \$3,613 million, so each 0.01 increase in Tobin's  $Q$  increases its market value by \$36.1 million. Increasing CEO stockholdings by one standard deviation from the sample mean therefore increases market value by \$687 million, all else equal.<sup>22</sup>

As discussed in Section 4.1, the coefficients estimated for optionholdings and its square have the opposite signs compared to those estimated for stockholdings: CEOs appear to own too many options from the point of view of maximizing  $Q$ . A one standard deviation increase in CEO optionholdings from the mean, for the average company, decreases Tobin's  $Q$  from 1.985 to 1.918, equivalent to a fall in market value of \$242 million. CEOs simultaneously own too few stocks.<sup>23</sup>

Given our finding that CEOs hold too many 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 *vegas*. A one standard deviation increase in *vega* from the sample mean raises Tobin's  $Q$  from 1.985 to 2.054, corresponding to a \$249 million increase in market value for the average firm. 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 or risk-aversion (Hall and Murphy, 2000) considerations which make such options more valuable to managers, 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.

Capital market pressure, as measured by the probability of delisting, has a negative effect on inefficiency, as predicted, but is statistically insignificant. 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  $Q$ s that are on average 0.106 higher than firms operating in 'highly concentrated' industries, corresponding to a \$383 million difference in market value. No doubt part of the difference is due to factors we have not controlled for. Still, 'all else equal', competition appears to have a considerable effect on performance.

The coefficient estimates in the subsample of unregulated firms are virtually identical to those in the sample as a whole. In the subsample of utilities, on the other hand, there are three important differences. First, while we still find that managers own too little equity, the coefficients estimated for optionholdings, its square, and *vega* are statistically zero, indicating that these have been chosen optimally and so cannot explain the extent to which utilities in our sample fall short of the frontier. Second, the negative coefficient estimated for capital market pressure is substantially larger in magnitude and becomes significant. In other words, an increase in the likelihood of delisting is associated with substantially better

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<sup>22</sup>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.

<sup>23</sup>If we use the sum of stock- and optionholdings (adjusted for *delta* and thus comparable to equity) instead of the individual variables in the OLS or SFA regressions, we continue to find suboptimality: CEOs have too small a claim on their firms through the combination of stocks and options.

performance. To illustrate, a one standard deviation increase in this likelihood is associated with a 0.06 increase in  $Q$ , equivalent to an increase in market value of \$227 million for the average utility. Finally, note that the estimate of  $\gamma$ , though small, is statistically significant, so our set of  $Z$  variables does not fully capture all the determinants of inefficiency amongst utility companies. One plausible omitted variable is the intensity of regulatory pressure, which could well differ from state to state.

## 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: when we replicate HHP's specification using OLS or fixed-effects, we also find suboptimal 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 the CEOs of unregulated firms own too many options, and that these options 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 that the industry-adjusted probability of delisting has only a marginal effect on performance for unregulated firms, but a strongly performance-increasing effect for utilities.

## 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, outliers, and alternative definitions of equity incentives.

### Size effects

In Table 5, 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 reported in Table 4. In Table 6, we report the results of estimating individual stochastic frontiers for each of the terciles. This reveals some interesting patterns in the frontier variables. The U-shaped relationship between size and  $Q$  is reversed amongst the large firms, such that  $Q$  first increases and then decreases in  $\log sales$ . Firm-specific risk significantly depresses  $Q$  only amongst small firms, perhaps because larger firms benefit from internal diversification across business lines. Spending on R&D and on CAPEX increases  $Q$  only amongst small and medium-sized firms. Spending on advertising, which in the sample as a whole was insignificant, increases  $Q$  for the large firms and decreases  $Q$  for the



medium-sized companies. Industry growth rate forecasts do not correlate with  $Q$  amongst medium-sized firms, and analyst following, though significant throughout, has the largest effect amongst small companies.

As the likelihood ratio tests (and the predicted efficiencies in Table 5) show, all three groups are prone to systematic departures from the frontier. The  $Z$  variables mostly have the same signs as in Table 4, 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 and medium-sized firms, and absent for the largest firms. This indicates that large-company CEOs have optimal stockholdings. HHP's finding of optimal managerial ownership thus re-emerges amongst our largest companies. However, as we argued earlier this is not a sufficient condition for large firms to be maximizing firm value, and the SFA model confirms that they do not. Using one standard deviation increases in stockholdings from the mean to illustrate the economic magnitude of the coefficients, Tobin's  $Q$  increases by 0.195 amongst small companies and 0.179 amongst medium-sized companies, corresponding to increases in market value of \$66 million and \$200 million, respectively.

The pattern of effort incentives in the form of optionholdings is similar. The coefficients are significant for the smallest and medium-sized companies and insignificant for the largest ones. The signs are as before. In other words, small and medium-sized companies award too many options, while option awards in large companies appear optimal. Economically, a one standard deviation increase in optionholdings from the mean would correspond to a decrease in market value of \$52 million amongst small companies and \$83 million amongst medium-sized companies.

Inefficiency is negatively and significantly related to *vega* for both small and medium-sized firms: higher *vega* moves companies closer to the efficient frontier. For large companies, the effect is insignificant. The economic magnitude of this effect is greater amongst medium-sized companies: a one standard deviation increase in *vega* raises their market value by \$101 million, compared to \$72 million for the smallest companies.

Capital market pressure does not correlate with efficiency for any size group. Product market competition, on the other hand, raises the efficiency of the smallest and the largest companies. Indeed, product market competition is the *only*  $Z$  variable which can explain departures from the frontier amongst our largest companies, all other variables indicating optimal provision of incentives. As the insignificant  $\gamma$  parameter shows, product market competition is sufficient to *fully* explain departures from the large-company frontier.

The same is not true for the tercile of small companies. There,  $\gamma$  is significant so our  $Z$  variables do not fully capture all the determinants of inefficiency. Plausible omitted incentive variables include the presence of venture capitalists on the board of directors and the greater frequency with which smaller companies raise outside financing, both of which may increase the level of monitoring.

## **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 9. This also leaves our results unchanged.

## 4.5 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 CEO stockholdings, the lower CEO optionholdings, and the higher the *vega* of CEO option portfolios. We now investigate whether boards adjust internal incentives to improve performance over time.<sup>24</sup> 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 whether the improvement over time in a firm’s performance relative to the frontier — its rate of ‘catch-up’ — is related to changes in its internal incentives. If it were not, we would have little cause to have faith in the economic interpretation of our frontier estimates. Put differently, our results so far suggest that the cross-section of firm inefficiencies (distances from the frontier) are highly related to the strength of internal incentive schemes, but it would be disconcerting if the time series behavior of firm inefficiencies were not also related to changes over time in the strength of internal incentive schemes.

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 tabulated in Table 5, 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 maintains its position relative to the frontier. A quarter of companies move down by 3 percentage points or more, and a quarter move up by 2.2 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’ \$68 million. The rates of *catchup* at the 25th and 75th percentiles thus imply 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  $t$ -statistics are reported in italics below the coefficient estimates; all variables are expressed in percentage terms):

$$\begin{aligned} \textit{catchup} = & \underset{-0.053}{-0.015} + \underset{6.609}{0.530} \Delta_{\underline{t}}^{\bar{t}} \textit{stockholdings} \\ & - \underset{-3.153}{0.693} \Delta_{\underline{t}}^{\bar{t}} \textit{optionholdings} \end{aligned}$$

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<sup>24</sup>See Core and Guay (1998) for a related analysis.

$$+ 0.143 \Delta_t^{\bar{t}} \text{vega of options}$$

$$\text{adjusted } R^2 = 11.9\% \quad F\text{-test} = 18.7^{***} \quad N = 1,307$$

As the adjusted  $R^2$  indicates, the regression has good explanatory power. The positive and significant coefficients estimated for stockholdings and *vega* strongly support the hypothesis that internal incentives matter: it is the companies that increase these internal incentives the most that move closer to the frontier over time. The negative and significant coefficient estimated for optionholdings suggests that companies can move closer to the frontier over time by slowing the growth in managerial optionholdings. This is consistent with our result that CEOs appear to hold too many options.<sup>25</sup> To illustrate the economic magnitude of the effects, consider increasing CEO stockholdings and *vega* by one standard deviation from the mean. This would move the average company 3.9 and 1 percentage points closer to the frontier, respectively. A similar increase in optionholdings would result in a -0.9 percentage point movement.

## 5 Conclusion

In this paper, we have provided a direct test of the Berle-Means hypothesis that managers who are not the sole residual claimant fail to maximize firm value. Unlike earlier tests, ours is based on a necessary *and* sufficient condition for value maximization which we identify using a stochastic frontier approach. Our empirical results can be summarized as follows. We find evidence that publicly traded U.S. companies between 1992 and 1997 are systematically inefficient on average, and that the shortfall in market value is economically significant: \$751 million for the average company. 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. Overall, CEOs own too little stock, too many options, and their options are insufficiently sensitive to risk. Greater product market competition tends to improve performance, especially amongst large companies where this is the only significant determinant of inefficiency. For utilities, the level of option incentives appears to be optimal while equity incentives are not. Greater capital and product market pressure improves utility performance.

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

The picture that emerges is one where a substantial fraction of companies operates under suboptimal incentives at any given point in time, but 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 adjust-

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<sup>25</sup>The results are unaffected if utilities are excluded, and continue to hold in each of the three size terciles. They are also unaffected if we regress *catchup* between  $\underline{t}$  and  $\bar{t}$  on the changes in stock- and optionholdings and *vega* up until the *penultimate* panel year ( $\bar{t} - 1$ ).

ments 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.

## 6 Data Appendix

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

### 6.1 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 identifies 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.

### 6.2 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 optionholdings. 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.

### 6.3 CEO age

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

### 6.4 Computing option deltas and vegas

To compute option deltas and *vegas*, 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.<sup>26</sup> For options *already* held at the beginning of our observation period (‘old options’), we only know the number of options held,<sup>27</sup> 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).<sup>28</sup> From this, we can infer the average strike price of old options as  $\bar{X} = S - \frac{\text{intrinsic value}}{\text{number of old options}}$ . 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 ( $\frac{S}{X} < .5$ ) or out-of-the-money options ( $\frac{S}{X} > 5$ ). 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.<sup>29</sup> 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 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.

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<sup>26</sup>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.

<sup>27</sup>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.

<sup>28</sup>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.

<sup>29</sup>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.

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 *vegas* 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.<sup>30</sup> We then compute the total *vega* and total delta as the weighted average of the *vegas* and deltas of the old optionholdings 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.

## 6.5 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  $Q$ s. We are able to handfill a small number of missing/zero Compustat values and to resolve all extreme Tobin’s  $Q$ s, 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.

## 7 References

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<sup>30</sup>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.	yes
net sales	Net sales as reported in ExecuComp, Compustat or a 10-K, expressed in \$m. Usually logged. Used to measure firm size.	yes
SIGMA1	The daily Fama-McBeth CAPM residual standard deviation, estimated over the previous year (in %, not annualized). Used to measure firm-specific risk.	yes
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.	yes
ADV / K	The ratio of advertising expenditures to K, used to measure the role of 'advertising capital' relative to other non-fixed assets.	yes
CAPEX / K	The ratio of capital expenditures to K.	yes
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.	yes
K / Sales	The ratio of tangible long-term assets (property, plant and equipment) to sales.	yes
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.	yes
Dummy R&D / K	A dummy variable equal to one if the data required to estimate R&D / K is missing, and zero otherwise.	yes
Dummy ADV / K	A dummy variable equal to one if the data required to estimate ADV / K is missing, and zero otherwise.	yes
leverage	Book value of long-term debt / (market value of equity + book value of long-term debt). Expressed in per cent.	no
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.	no
industry growth forecasts	Analyst forecasts of long-term industry growth rates. Constructed bottom-up as follows. For each firm covered in I/B/E/S, we collect the median long-term growth rate forecast for every month in our sample. We then use I/B/E/S's industry classification to compute an average growth rate for each industry in every month and assign our sample firms to I/B/E/S's industries. A sample firm's industry growth rate is the average of the I/B/E/S-industry median per-firm long-term growth forecasts in its fiscal-year end month. Expressed in per cent.	no
analyst following	= number of analysts following the stock in each fiscal year. Computed as the maximum of the number of analysts reported in I/B/E/S as giving either a one-year, two-year, three-year or long-term forecast in or before its fiscal year-end month.	no

**Table 1. Cont'd.**  
**Variable definitions.**

<b>Incentive variables</b>		<b>Included in HHP?</b>
CEO stockholdings	CEO's common stockholdings as a fraction of common stock outstanding, in per cent. Includes beneficial ownership and restricted stock.	yes
CEO optionholdings	CEO's optionholdings as a fraction of common stock outstanding, in per cent.	no
total delta	The partial derivative of Black-Scholes call option value, adjusted for dividends, with respect to the price of the underlying stock.	no
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.	no
capital market pressure	= unconditional $Pr(\text{delisting})$ , 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.	no
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.	no

**Table 2.****Descriptive sample statistics.**

For variable definitions see Table 1.

	mean	stdev	min	Q1	median	Q3	max
<b>Firm characteristics</b>							
Tobin's $Q$	1.985	1.292	0.229	1.237	1.569	2.216	16.340
net sales (\$m)	3,137	8,158	0	328	890	2,721	153,627
SIGMA1 (%)	2.212	1.064	0.440	1.430	1.960	2.780	13.990
R&D / K	0.206	0.786	0	0	0	0.132	33.516
ADV / K	0.081	0.480	0	0	0	0.018	19.490
CAPEX / K	0.236	0.160	0	0.129	0.196	0.302	1.204
Y / Sales	-0.005	4.262	-307.314	0.090	0.145	0.222	0.823
K / Sales	0.602	1.140	0	0.153	0.285	0.656	54.823
leverage (%)	19.39	18.46	0	3.28	14.76	30.96	99.76
cost of capital (%)	9.491	1.421	5.910	8.358	9.802	10.655	12.724
industry growth forecasts (%)	16.637	5.828	2.792	13.473	16.370	19.997	35.665
analyst following	11.9	8.4	1	5	10	17	47
<b>Incentive variables</b>							
% of equity owned via stocks	3.42	7.30	0	0.09	0.43	2.61	80.06
% of equity 'owned' via options	1.00	1.49	0	0.14	0.52	1.31	25.76
total delta of options	0.67	0.30	0	0.57	0.77	0.89	1.00
total vega of options	11.58	11.53	0	4.00	9.49	16.44	356.34
SIC-2 $Pr(\text{delisting})$ (%)	5.85	3.11	0	3.95	5.57	7.28	31.25
SIC-4 Herfindahl index	1,444.0	1,306.3	224.9	594.3	1,067.0	1,812.1	10,000

**Table 3.****OLS and fixed-effects specifications.**

For variable definitions see Table 1. The dependent variable is Tobin's  $Q$ . Analyst following is the natural log of one plus the number of analysts following the stock. The Herfindahl index is normalized to have a maximum of 1.0 = monopoly. Year dummies and firm-specific dummies (in columns 2 and 4) are included but not reported. The Durbin-Wu-Hausman (DWH) endogeneity tests are estimated by including in regressions (1)-(4) the residuals of auxiliary regressions of the potentially endogenous variable on all the exogenous variables in the system. To ensure the auxiliary regressions are identified, we include the following variables: CEO age (in the auxiliary regression for stockholdings), dividend yield (for optionholdings), and the variance of the per-industry delisting probability (for *vega*). Each of these correlate with the potentially endogenous variable but not with  $Q$ .  $t$ -statistics are White. One, two and three asterisks indicate significance at  $p < 5\%$ ,  $p < 1\%$ , and  $p < 0.1\%$ , respectively.

	HHP specification					Expanded specification				
	OLS (1)		Fixed effects (2)		$F$ -test: $\beta_{OLS} = \beta_{FE}$	OLS (3)		Fixed effects (4)		$F$ -test: $\beta_{OLS} = \beta_{FE}$
	coeff.	$t$ -stat.	coeff.	$t$ -stat.		coeff.	$t$ -stat.	coeff.	$t$ -stat.	
Constant	3.208	4.744	1.932	10.621		2.643	5.931	2.146	11.148	
$\ln(\text{sales})$	-0.371	-2.062	0.042	1.586	5.3*	-0.311	-2.560	0.082	3.037	10.5**
$\ln(\text{sales})^2$	0.019	1.553	-0.010	-4.498	5.6*	0.009	1.125	-0.008	-3.739	4.6*
SIGMA1	-0.092	-3.613	-0.061	-3.631	1.5	-0.055	-2.489	-0.051	-3.079	0.3
R&D / K	0.226	1.960	0.276	7.362	0.2	0.127	1.707	0.278	7.714	4.1*
ADV / K	0.016	0.595	0.061	1.539	3.1	-0.004	-0.156	0.042	1.095	3.6
CAPEX / K	2.406	14.380	1.363	15.417	38.9***	1.215	8.386	1.035	11.948	1.5
Y / Sales	0.012	1.458	-0.005	-1.687	4.3*	0.013	2.041	-0.003	-1.039	6.2*
K / Sales	-0.175	-6.005	-0.102	-3.661	6.3*	-0.099	-2.964	-0.042	-1.534	3.0
(K / Sales) <sup>2</sup>	0.004	7.887	0.001	1.506	38.3***	0.002	3.999	-0.0003	-0.582	20.7***
Leverage						-1.973	-22.811	-2.085	-19.046	1.7
Cost of capital						-0.008	-0.749	-0.055	-5.274	20.8***
Industry growth forecasts						0.029	6.730	0.018	3.510	7.2**
Analyst following						0.372	13.291	0.022	0.674	156.6***
Dummies: SIGMA1	-0.148	-0.944	0.132	1.398	3.2	0.196	1.378	0.073	0.770	0.8
Missing R&D / K	-0.041	-0.559	-0.029	-0.310	0.0	0.041	0.669	-0.045	-0.501	2.0
data ADV / K	-0.149	-2.481	0.000	0.005	6.2*	-0.040	-0.723	0.026	0.331	1.4
<b>Incentive variables</b>										
CEO stockholdings	0.025	4.078	0.023	3.721	0.1	0.029	5.608	0.023	3.983	1.1
(CEO stockholdings) <sup>2</sup>	-0.0004	-2.886	-0.0003	-1.912	1.2	-0.0005	-3.809	-0.0003	-2.369	1.8
CEO optionholdings						-0.063	-3.123	0.112	5.071	74.6***
(CEO optionholdings) <sup>2</sup>						0.003	2.323	-0.006	-3.415	40.0***
Vega of options						0.011	1.944	0.010	5.634	0.0
Capital market pressure						0.023	0.048	0.466	1.566	0.8
Product market pressure						-0.427	-4.782	-0.551	-2.571	1.9
<b>Diagnostics</b>										
$R^2$ (%)	21.8		7.4			35.9		14.9		
all $\beta = 0$ ( $F$ )	64.8***		33.1***			121.7***		44.3***		
all $\beta_{OLS} = \beta_{FE}$ ( $F$ )					14.0***					17.7***
fixed effects = 0 ( $F$ )			11.8***					10.1***		
Correlation (FE, $X\beta$ ) (%)			10.9					4.9		
Skewness in residuals ( $p$ )	<0.001		<0.001			<0.001		<0.001		
<b>DWH endogeneity tests</b>										
CEO stockholdings ( $F$ )	7.9**		1.5			2.8		0.5		
CEO optionholdings ( $F$ )						1.2		9.0**		
vega of options ( $F$ )						2.7		0.0		
joint ( $F$ )						2.2		3.2*		
No. firm-years	7,134		7,134			7,134		7,134		
No. firms			1,307					1,307		
Max no. panel years			6					6		

**Table 4.****Estimating the frontier and testing for inefficiency.**

The dependent variable is Tobin's  $Q$ . All regressors are as defined in Table 3. In the second and third columns, companies are sorted into two groups: utilities (two-digit SIC codes 40, 48, 49), and unregulated industries. All test statistics are as defined in Section 2. One, two and three asterisks indicate significance at  $p < 5\%$ ,  $p < 1\%$ , and  $p < 0.1\%$ , respectively.

	Whole sample (1)		Unregulated (2)		Utilities (3)	
	coeff.	<i>t</i> -stat.	coeff.	<i>t</i> -stat.	coeff.	<i>t</i> -stat.
<b>Frontier variables</b>						
Constant	2.839	18.250	2.811	16.664	2.649	10.421
$\ln(\text{sales})$	-0.312	-10.971	-0.322	-10.719	-0.259	-4.014
$\ln(\text{sales})^2$	0.009	4.772	0.011	5.271	0.014	3.240
SIGMA1	-0.056	-5.503	-0.045	-4.498	0.016	1.079
R&D / K	0.128	7.120	0.109	5.613	1.427	16.287
ADV / K	-0.006	-0.233	-0.016	-0.546		
CAPEX / K	1.214	14.066	1.196	11.888	0.691	5.491
Y / Sales	0.013	4.337	0.011	3.359	0.524	6.074
K / Sales	-0.098	-4.578	-0.136	-5.117	-0.112	-3.061
$(K / \text{Sales})^2$	0.002	4.243	0.003	4.776	0.010	1.650
Leverage	-1.976	-24.963	-2.098	-22.579	-1.176	-13.682
Cost of capital	-0.008	-0.764	-0.009	-0.686	-0.019	-2.083
Industry growth forecasts	0.029	10.363	0.039	11.342	0.013	5.895
Analyst following	0.374	16.418	0.385	14.308	0.090	3.680
<i>Dummies:</i> SIGMA1	0.191	1.685	0.218	1.784	0.413	3.280
<i>Missing data</i> R&D / K	0.039	0.462	0.000	-0.001	0.020	0.704
ADV / K	-0.039	-0.540	-0.031	-0.384		
<b>Incentive variables</b>						
<b>(SFA coefficients measure distance from frontier, so signs are reversed relative to OLS)</b>						
Constant	0.198	5.696	0.384	7.475	0.196	6.560
CEO stockholdings	-0.029	-10.481	-0.031	-5.371	-0.027	-3.401
$(\text{CEO stockholdings})^2$	0.0005	6.898	0.0004	3.165	0.001	4.057
CEO optionholdings	0.057	7.935	0.054	2.967	0.002	0.085
$(\text{CEO optionholdings})^2$	-0.003	-3.457	-0.004	-3.251	-0.005	-1.070
Vega of options	-0.010	-8.297	-0.004	-5.134	0.000	0.061
Capital market pressure	-0.049	-0.190	0.059	0.115	-2.285	-12.031
Product market pressure	0.426	5.509	0.253	2.157	0.499	2.873
<b>Diagnostics</b>						
$\sigma^2 = \sigma_v^2 + \sigma_u^2$	1.072	64.629	1.205	53.729	0.084	21.213
$\gamma = \sigma_w^2 / \sigma^2$	0.000	0.020	0.003	0.651	0.004	3.455
LR test ( $\chi^2$ )	138.4	***	119.8	***	52.7	***
No. firm-years	7,134		6,188		946	
No. firms	1,307		1,135		172	
Max no. panel years	6		6		6	

**Table 5. Panel A.****Correlogram of predicted efficiencies.**

Predicted efficiencies are calculated following Battese and Coelli (1988) using the estimates for the sample as a whole from Table 4. 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	<b>55.0</b> ***	37.2***	56.2***	55.8***	55.7***
1996		<b>42.8</b> ***	65.8***	60.3***	50.9***
1995			<b>80.6</b> ***	69.5***	52.5***
1994				<b>87.0</b> ***	68.2***
1993					<b>83.6</b> ***

**Table 5. Panel B.****Predicted efficiencies by empirical specification and sample characteristics.**

Predicted efficiencies by year, size, and industry are derived by partitioning the cross-section of predicted efficiencies for the sample as a whole from Table 4. Predicted efficiencies are expressed in %. 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 two groups: utilities (two-digit SIC codes 40, 48, 49), and unregulated industries.

	nobs	mean	stdev	min	Q1	median	Q3	max
<b>All firms</b>	7,134	88.4	8.3	9.2	85.1	89.2	92.7	100.0
<b>By year</b>								
1992	1,170	88.5	8.5	28.9	84.9	89.2	93.0	100.0
1993	1,299	89.0	7.7	17.9	85.6	89.5	93.1	100.0
1994	1,280	88.3	8.5	9.2	85.0	89.0	92.7	100.0
1995	1,237	88.3	8.2	33.1	85.1	89.2	92.5	100.0
1996	1,198	87.9	8.9	9.4	87.8	89.0	92.5	100.0
1997	950	88.4	8.1	19.9	85.3	89.1	92.6	100.0
<b>By size</b>								
Small	2,280	90.9	7.3	9.4	87.6	91.1	95.5	100.0
Medium	2,390	87.3	8.9	9.2	84.1	88.3	91.6	100.0
Large	2,464	87.2	8.2	13.0	84.2	88.2	91.2	100.0
<b>By industry</b>								
Unregulated	6,188	89.1	8.0	9.4	86.2	89.7	93.1	100.0
Utilities	946	83.9	8.8	9.2	81.1	84.4	87.8	100.0

**Table 6.****Stochastic frontier estimates by size tercile.**

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

		SFA					
		smallest size tercile		medium size tercile		largest size tercile	
		coeff.	<i>t</i> -stat.	coeff.	<i>t</i> -stat.	coeff.	<i>t</i> -stat.
<b>Frontier variables</b>							
	Constant	2.216	2.827	2.625	2.655	-2.230	-1.494
	$\ln(\text{sales})$	-0.296	-10.337	-0.415	-1.437	0.730	2.608
	$\ln(\text{sales})^2$	-0.015	-4.778	0.032	1.509	-0.039	-1.771
	SIGMA1	-0.040	-3.996	0.011	1.137	0.0004	0.001
	R&D / K	0.038	2.164	0.411	5.881	0.246	0.770
	ADV / K	0.030	0.537	-0.060	-2.508	0.397	3.395
	CAPEX / K	1.444	12.518	0.552	3.911	0.039	0.049
	Y / Sales	0.010	3.290	5.370	26.077	3.287	6.370
	K / Sales	-0.172	-6.392	-1.079	-13.376	-0.589	-3.736
	$(K / \text{Sales})^2$	0.004	5.819	0.117	5.209	0.073	0.883
	Leverage	-2.335	-15.260	-1.249	-11.815	-1.529	-3.526
	Cost of capital	0.002	0.146	-0.018	-1.235	0.012	0.340
	Industry growth forecasts	0.031	6.864	0.004	0.979	0.017	4.435
	Analyst following	0.381	10.597	0.216	6.821	0.163	20.316
<i>Dummies:</i>	SIGMA1	0.028	0.210	0.271	1.833	0.102	0.096
<i>Missing data</i>	R&D / K	-0.254	-1.789	0.413	3.044	0.062	0.493
	ADV / K	0.109	0.978	-0.216	-1.708	-0.066	-0.242
<b>Incentive variables</b>							
<b>(SFA coefficients measure distance from frontier, so signs are reversed relative to OLS)</b>							
	Constant	-0.845	-1.104	0.058	1.514	0.178	0.310
	CEO stockholdings	-0.026	-4.675	-0.030	-6.385	-0.033	-1.464
	$(\text{CEO stockholdings})^2$	0.0004	2.759	0.001	5.516	0.000	0.303
	CEO optionholdings	0.100	4.062	0.062	2.876	0.003	0.032
	$(\text{CEO optionholdings})^2$	-0.003	-0.969	-0.002	-1.177	0.011	1.933
	Vega of options	-0.096	-9.161	-0.030	-10.103	-0.003	-1.355
	Capital market pressure	-0.961	-1.325	0.887	1.779	-0.231	-0.083
	Product market pressure	0.363	1.965	0.195	1.582	0.392	8.900
<b>Diagnostics</b>							
	$\sigma^2 = \sigma_v^2 + \sigma_u^2$	1.873	60.254	0.607	41.174	0.380	5.526
	$\gamma = \sigma_w^2 / \sigma^2$	0.0001	4.162	0.000	1.079	0.000	0.007
	LR test ( $\chi^2$ )	62.1	***	63.0	***	127.0	***
	No. firm-years	2,280		2,390		2,464	
	No. firms	435		436		436	
	Max no. panel years	6		6		6	