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Understanding the determinants of managerial ownership and the link between ownership and performance☆

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Abstract

Both managerial ownership and performance are endogenously determined by exogenous (and only partly observed) changes in the firm's contracting environment. We extend the cross-sectional results of Demsetz and Lehn (1985) (Journal of Political Economy, 93, 1155–1177) and use panel data to show that managerial ownership is explained by key variables in the contracting environment in ways consistent with the predictions of principal-agent models. A large fraction of the cross-sectional variation in managerial ownership is explained by unobserved firm heterogeneity. Moreover, after

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controlling both for observed firm characteristics and firm fixed effects, we cannot conclude (econometrically) that changes in managerial ownership affect firm performance.

JEL classification: G14; G32; D23; L14; L22

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1. Introduction

Since Berle and Means (1932), the conflict between managers and shareholders has been studied extensively by researchers seeking to understand the nature of the firm. When shareholders are too diffuse to monitor managers, corporate assets can be used for the benefit of managers rather than for maximizing shareholder wealth. It is well known that a solution to this problem is to give managers an equity stake in the firm. Doing so helps to resolve the moral hazard problem by aligning managerial interests with shareholders' interests. Therefore, Jensen and Meckling (1976) suggest that managers with small levels of ownership fail to maximize shareholder wealth because they have an incentive to consume perquisites. In a similar fashion, some commentators have decried low levels of managerial ownership in U.S. corporations, and the theme has even appeared in discussions by compensation specialists and boards of directors.

In this paper, we propose an equilibrium interpretation of the observed differences in ownership structures across firms. Rather than interpret low ownership levels as per se evidence of suboptimal compensation design, we argue that the compensation contracts observed in the data are endogenously determined by the contracting environment, which differs across firms in both observable and unobservable ways. In particular, low levels of managerial ownership might well be the optimal incentive arrangement for the firm if the scope for perquisite consumption (or more generally, the severity of the moral hazard problem for managers) happens to be low for that firm. We do not deny the importance of agency problems between stockholders and managers, but rather emphasize the importance of *unobserved heterogeneity* in the contracting environment across firms.

We begin by examining the observable determinants of managerial ownership. This investigation builds upon Demsetz and Lehn (1985), who use cross-sectional data to show that the level of managerial ownership is determined by the riskiness of the firm, measured by the volatility of the stock price. They argue that the scope for moral hazard is greater for managers of riskier firms, which therefore means that those managers must have greater ownership stakes to align incentives. They also point out that riskiness makes it costlier for managers to hold nondiversified portfolios (assuming that equity holdings in the firm are not easily hedged), so the relation between managerial ownership and nondiversifiable stock price risk is not necessarily monotonic.

To document the extent to which managerial ownership is endogenously determined by the contracting environment, we extend the empirical specification used by Demsetz and Lehn by including a number of additional explanatory variables other than stock price variability (see also Kole, 1996). Most importantly, we include variables (such as firm size, capital intensity, R&D intensity, advertising intensity, cash flow, and investment rate) designed to control for the scope for moral hazard. To the extent that our additional explanatory variables proxy for moral hazard, our specification clarifies the role of stock price variance as an explanatory variable for managerial ownership. We also use panel data that allow us to estimate the importance of unobserved (time-invariant) firm effects. These results show that a large fraction of the cross-sectional variation in managerial ownership is 'explained' by unobserved firm heterogeneity. In our subsequent analysis of the determinants of firm value, we argue that this unobserved heterogeneity generates a spurious correlation between ownership and performance.

The second goal of this paper is to reexamine theoretical explanations of the empirical link between managerial ownership and firm performance. Mørck et al. (1988) estimate a piecewise-linear relation between board ownership and Tobin's Q and find that Tobin's Q increases and then decreases with managerial ownership. McConnell and Servaes (1990) examine a larger data set than the Fortune 500 firms examined by Mørck et al. and find an inverted U-shaped relation between Q and managerial ownership, with an inflection point between 40% and 50% ownership. Hermalin and Weisbach (1991) analyze 142 NYSE firms and find that Q rises with ownership up to a stake of 1%; the relation is negative in the ownership range of 1–5%, becomes positive again in the ownership range of 5–20%, and turns negative for ownership levels exceeding 20%. The pattern identified by Mørck et al. has been corroborated for a cross-section of U.S. firms from 1935 by Holderness et al. (1999). Kole (1995) examines the differences in data sources used in several recent studies and concludes that differences in firm size can account for the reported differences between those studies. These studies generally interpret the positive relation at low levels of managerial ownership as evidence of incentive alignment, and the negative relation at high levels of managerial ownership as evidence that managers become 'entrenched' and can indulge in non-value-maximizing activities without being disciplined by shareholders. However, these studies do not address the endogeneity problem that confronts the use of managerial ownership as an explanatory variable, a problem noted early by Jensen and Warner (1988, p. 13).

We investigate the degree to which this heterogeneity makes managerial ownership an endogenous variable in models of firm performance. Following in the tradition of Demsetz and Lehn, we describe the contracting problem faced by the firm and develop a simple empirical model to illustrate the econometric issues that are encountered when estimating the relation among managerial ownership, its determinants, and its effect on firm performance. Distinct from Demsetz and Lehn, and in contrast to previous papers that attempt to measure the impact of managerial ownership on firm performance, we use panel data to test for the endogeneity of managerial ownership in models linking ownership to performance (measured by Tobin's Q). In particular, we use panel data to investigate the hypothesis that managerial ownership is related to observable and unobservable (to the econometrician) firm characteristics influencing contracts. If the unobserved sources of firm heterogeneity are relatively constant over time, we can treat these unobserved variables as fixed effects, and use panel data techniques to obtain consistent estimates of the parameter coefficients. This approach provides consistent estimates of the residuals in the Q regression, which we use to construct a test for correlation between managerial ownership and unobserved firm heterogeneity.

Our principal findings are threefold. First, proxies for the contracting environment faced by the firm (i.e., observable firm characteristics) strongly predict the structure of managerial ownership. We substantially extend the set of explanatory variables examined by Demsetz and Lehn, and we show that many of our results are robust to the inclusion of observed determinants of managerial ownership, industry fixed effects, or firm fixed effects. Second, we show that the coefficient on managerial ownership is not robust to the inclusion of fixed effects in the regression for Tobin's Q. Our formal statistical test rejects the null hypothesis of a zero correlation between managerial ownership and the unobserved determinants of Tobin's Q, thus supporting our conjecture that managerial ownership is endogenous in Q regressions. That is, managerial ownership and firm performance are determined by common characteristics, some of which are unobservable to the econometrician. Third, we explore the use of instrumental variables as an alternative to fixed effects to control for the endogeneity of managerial ownership in the Q regression. We find some evidence to support a causal link from ownership to performance, but this evidence is tentative because of the weakness of our instruments. We argue that future progress will require a more structural approach to the model.

Kole (1996) also argues that managerial ownership is endogenous; she further argues that causality operates in the opposite direction, from performance to ownership. Using a panel-data vector autoregression, we corroborate Kole's reverse causality evidence (results available upon request). Our research, however, supports the idea that both ownership and performance are determined by similar (observed and unobserved) variables in the firm's contracting environment. Thus, our interpretation is different from Kole's interpretation. That is, we find evidence endogeneity caused by unobserved heterogeneity as opposed to reverse causality.

The paper is organized as follows. In Section 2, we outline a simple model of managerial ownership and explain why it is difficult to estimate the relation between managerial ownership levels and firm performance, particularly in the context of cross-sectional data. Section 3 describes the sample selection criteria and the data we use in our empirical analysis of managerial ownership and firm performance. In Sections 4 and 5, respectively, we present empirical evidence on the determinants of managerial ownership and on the relation between managerial ownership and firm performance. Section 6 concludes.

2. An empirical framework for analyzing executive contracts

A common approach for estimating the impact of managerial ownership on firm value is to regress Tobin's Q on such variables as the percentage of equity held by managers. In this section, we argue that this regression is potentially misspecified because of the presence of unobserved heterogeneity. Specifically, if some of the unobserved determinants of Tobin's Q are also determinants of managerial ownership, then managerial ownership might spuriously appear to be a determinant of firm performance. To motivate our focus on the endogeneity of managerial ownership, we provide three examples of likely sources of unobservable heterogeneity, and in each case, we discuss their econometric consequences for cross-sectional regressions. We follow this discussion with a more formal exposition, in which we assume that the unobserved heterogeneity is a 'firm fixed effect', and we show how, under this assumption, panel data can be used to mitigate the endogeneity problem. In Section 5, we return to this model to describe a test for the endogeneity of ownership in regressions for Tobin's Q.

For our first example of unobserved heterogeneity, consider two firms that are identical except that the owner of one of the firms has access to a superior monitoring technology. Under the optimal contracting regime, the owners with access to the superior monitoring technology will choose a lower level of managerial ownership to align incentives, and this firm will have a higher valuation because fewer resources will be diverted to managerial perquisites. If measures of the quality of the monitoring technology are omitted from the specification, a regression of firm value on managerial ownership will spuriously (and falsely) indicate a negative relation, because ownership is a negative proxy for the quality of monitoring technology.

Intangible assets provide a second example of unobserved firm heterogeneity. Suppose two firms are identical except that one of the firms operates with a higher fraction of its assets in the form of intangibles. Under the optimal contracting regime, the owners of this firm will require a higher level of managerial ownership to align incentives because the intangible assets are harder to monitor and therefore subject to managerial discretion. This firm will also have a higher Q value because the market will value intangibles in the numerator (market value), but the book value of assets in the denominator will understate the value of intangibles (because Tobin's Q is measured as the ratio of the market value of the firm's outstanding debt and equity divided by the book value of assets). In this example, the unobserved level of intangibles induces a positive correlation between managerial ownership and Tobin's Q, but this relation is spurious, not causal.

A third example of unobserved heterogeneity is variation in the degree of market power. Suppose there are two firms competing in a market with differentiated products and that one firm enjoys a competitive advantage because (for some historical reason) it has been able to locate its products in such a way that confers more market power. If this market power insulates managerial decision-making from the discipline of competitive product markets, then the optimal contract for managers will call for higher levels of managerial ownership. Hence, unobserved heterogeneity in the form of unobserved differences in market power will (spuriously) induce a positive relation between ownership and performance. Alternatively, causation could run the other way; stockholders might design the manager's compensation to implicitly encourage collusive outcomes in the product market (Fershtman and Judd, 1987). Attempting to test this proposition using regressions of Tobin's Q on managerial ownership suffers from the same econometric problems we study here. The ownership decision is endogenous because of unobserved firm heterogeneity.

It is possible to generalize these examples in a simple analytical framework. We assume that within the general set of contracts agreed to by the firm, the owners of the firm choose a simple management compensation contract that includes a share of the firm's equity. This equity share (or 'managerial stake') is chosen to maximize the owners' equity return subject to incentive compatibility and participation constraints. For this purpose, we assume that gains from other means for reducing agency costs have been maximized, so that we examine the residual agency cost to be addressed by managerial ownership (we revisit this assumption in Section 5 below). Let x_{it} and u_{it}, respectively, denote observable and unobservable characteristics for firm i at time t related to the firm's contracting environment (including, e.g., proxies for the potential for moral hazard). In addition to unobserved firm characteristics, we implicitly assume a profitability shock that is observable to the manager, but not to outside shareholders. This shock cannot be contracted upon, giving rise to moral hazard.

The firm's owners must decide how much equity to give to managers in order to align incentives for value maximization. This equity share m_{it} depends on such factors as the potential for moral hazard and managers' exposure to risk, which we assume are partly measured by x_{it}, but are otherwise unobserved and included in u_{it}. We assume that the functional relation is linear, and that $u_{it} = u_i$ is time-invariant for the firm, so that

$$m_{it} = \beta_1 x_{it} + \gamma_1 u_i + e_{it}, \tag{1}$$

where e_{it} represents independent measurement error.

Faced with this contract, managers choose an optimal 'effort level', y_{it}, which could include a range of participation in non-value-maximizing activities. This effort choice depends on the managerial ownership stake, m_{it}, and, like the optimal contract itself, depends on both observed and unobserved characteristics of the firm, x_{it} and u_i. Assuming a linear functional form, we can represent the manager's effort choice by the following relation:

$$y_{it} = \theta m_{it} + \beta_2 x_{it} + \gamma_2 u_i + v_{it}. \tag{2}$$

Using firm value as a summary measure of expected firm performance, we assume that firm value depends on managerial effort plus the vector of observed and unobserved firm characteristics. Denoting the value of firm i at time t by Q_{it}, we assume that

$$Q_{it} = \delta y_{it} + \beta_3 x_{it} + \gamma_3 u_i + w_{it}. \tag{3}$$

We can now combine Eqs. (2) and (3) to derive the following relation among firm managerial ownership, firm characteristics, and firm performance:

$$Q_{it} = \delta\theta m_{it} + (\delta\beta_2 + \beta_3)x_{it} + (\delta\gamma_2 + \gamma_3)u_i + \delta v_{it} + w_{it}. \tag{4}$$

Simplifying the notation reveals the regression specification commonly used in the empirical literature:

$$Q_{it} = a_0 + a_1 m_{it} + a_2 x_{it} + \varepsilon_{it}. \tag{5}$$

In a cross-section of firms, as long as the error term, $\varepsilon_{it} = (\delta\gamma_2 + \gamma_3)u_i + \delta v_{it} + w_{it}$ – is uncorrelated with both m_{it} and x_{it}, one can consistently estimate the reduced-form coefficient on managerial ownership in the regression for firm value. However, because the choice of managerial ownership depends on unobserved firm characteristics, m_{it} depends on u_i, and is therefore correlated with ε_i. Specifically,

$$E(m_{it}\varepsilon_{it}) = E((\beta_1 x_{it} + \gamma_1 u_i)\,(\delta\gamma_2 + \gamma_3)u_i) = \gamma_1(\delta\gamma_2 + \gamma_3)\sigma_v^2. \tag{6}$$

In general, the expectation in Eq. (6) will be zero only in the unlikely event that the optimal contract does not depend on observed firm characteristics ($\gamma_1 = 0$), or in the event that neither effort nor Q_{it} do ($\gamma_2 = \gamma_3 = 0$). Hence one cannot estimate Eq. (5) using ordinary least squares. A natural solution to this problem would be to use instrumental variables for ownership, but this approach is difficult in practice because the natural instruments – the observed firm characteristics x_{it} – are already included on the right-hand side of the equation for firm valuation in Eq. (5). Hence it is difficult to identify instrumental variables that would permit identification of a_1. With panel data, however, one can use a fixed-effects estimator, assuming that the unobserved heterogeneity is constant over time.

In contrast to the model for Tobin's Q, the model for the optimal choice of managerial ownership levels in Eq. (1) is more easily identified because it requires only the much weaker assumption that the unobserved firm characteristics are uncorrelated with observed characteristics. Hence the focus of our results in Section 4 is on Eq. (1).

The above discussion suggests four lines of empirical inquiry. First, we explore whether the observed firm characteristics (proxies for the potential for moral hazard and risk) influence managerial ownership in ways that are consistent with theoretical predictions. Second, we investigate the importance of unobserved characteristics as determinants of managerial ownership. Third, we investigate the extent to which the empirical relation between managerial ownership and firm performance (measured by Tobin's Q) can be explained by the omission of observed and unobserved firm characteristics (i.e., by uncontrolled-for or unobserved heterogeneity). Fourth, we explore the possibility of using instrumental variables to recover the parameter values in Eq. (5). We describe these results in Sections 4 and 5 after describing our sample and data in Section 3.

3. The data

Our sample consists of firms from the Compustat universe. We restrict ourselves to firms that have no missing data (on sales, the book value of capital, and the stock price) over the three-year period 1982–1984. (We cannot avoid this conditioning because we cannot use firms with missing data or fewer than three years of data for the variables of interest.) We then select 600 firms by random sampling, and we collect data for all subsequent periods. Our panel is therefore balanced at 600 firms from 1982 through 1984, but the number of firms declines to 551 by 1985, and falls to a low of 330 by 1992, the last year in the sample. Because of this attrition from Compustat (principally due to mergers and acquisitions), our panel is systematically less random over time. However, we avoid exacerbating the scope for sampling bias by not requiring a balanced panel.

For this unbalanced panel of firms, we attempt to collect the following additional data for each firm-year observation: the number of top managers and directors (as reported in the proxy statement), the percentage of the firm's shares owned by those managers and directors, and the date of the proxy statement from which these two numbers are collected. For those observations for which we can locate proxy statements, we collect the managerial ownership variables and merge this information with the Compustat data. Because smaller firms (in

Table 1
Sample of Compustat firms by year

We start out with 600 firms randomly sampled from the universe of Compustat firms with data available over the period 1982–1984 on sales, book value of capital, and stock price. The number of firms declines after 1984, principally due to mergers and acquisitions. The number of available ownership observations represents firms for which we are able to obtain proxy statements with the number of top managers and directors and their collective percentage share ownership.

Year	Number of available Compustat observations	Number of available ownership observations
1982	600	398
1983	600	425
1984	600	427
1985	549	408
1986	518	385
1987	482	359
1988	442	330
1989	422	329
1990	396	300
1991	382	296
1992	330	293

terms of the number of shareholders) are not required to file proxies with the Securities and Exchange Commission, we are unable to obtain proxy information for all firms. We end up with managerial ownership information for about 70% of the Compustat firms. Table 1 summarizes the number of firms in our sample as a result of the sample selection process.

Despite the problems of attrition and proxy availability (which are not unique to our study), our sample provides several distinct advantages over datasets used in previous studies. First, in contrast to studies that focus on the Fortune 1000, our sample includes a much larger number of small firms and is more representative of the typical firm in Compustat. Second, we have a panel of firms rather than a single cross-section. This allows us to control for firm-level fixed effects. Third, we deliberately construct our panel in such a way that we can control for sample selection bias because of lack of data (for ownership) and attrition. In fact, it is possible to describe the significance of the bias imposed on the level of managerial ownership by a requirement that the panel be balanced; looking over the 1982–1992 period, the average ownership share varies from 16.2% to 19.4%, and for the balanced panel, for the firms removed by the balancing criterion, the ownership share varies between 22.4% and 25.3%. The availability of data on managerial ownership is well predicted by variables such as firm size and fixed

Table 2
Managerial ownership stakes by firm size, 1982

For the 398 Compustat firms for which we have data on sales, book value of capital, stock price, number of top managers and directors, and collective equity ownership of top managers and directors, we report the average number of managers and their average collective ownership stake by firm size.

Firm size class	Number of firms	Average number of managers per firm	Average total managerial ownership stake
Sales < \$22 million	111	7.2	32.0%
\$22 million ⩽ Sales ⩽ \$188 million	147	12.4	25.4%
Sales > \$188 million	140	22.3	13.4%

capital intensity, but as we explain in our discussion of empirical results, controlling for this 'missing data bias' does not qualitatively affect our results. While our sample design allows us to estimate and control for the effects of attrition bias, exit from Compustat due to mergers and acquisitions or bankruptcies is, in practice, difficult to predict using observable firm characteristics. A simple probit model for exit reveals that size is the principal explanatory variable; many more firms exit because of mergers than because of failure. When we include the inverse Mills ratio in our Q regressions, we find no statistically significant effect of selection bias. We therefore decide not to correct formally for attrition bias.

To illustrate differences between small and large firms, Table 2 shows, by size class, the average number of managers per firm and the percentage of shares outstanding owned collectively by those managers in 1982. The frequency distributions of managerial ownership and the number of managers are reported in Fig. 1. Note that the percentage of shares owned by insiders is much higher for small firms, measuring 32% on average for firms in (roughly) the bottom third of the size distribution of firms. By contrast, existing studies typically oversample large firms, and report average ownership shares of approximately 10%. This figure is consistent with the ownership stakes in firms in the top third of our size distribution (for comparison, the sales cutoff for Fortune 1000 firms is approximately \$1 billion).

4. Determinants of managerial ownership: empirical evidence

4.1. Firm characteristics

The simple model outlined in Section 2 indicates the need to identify observable variables that relate to potential moral hazard and influence optimal

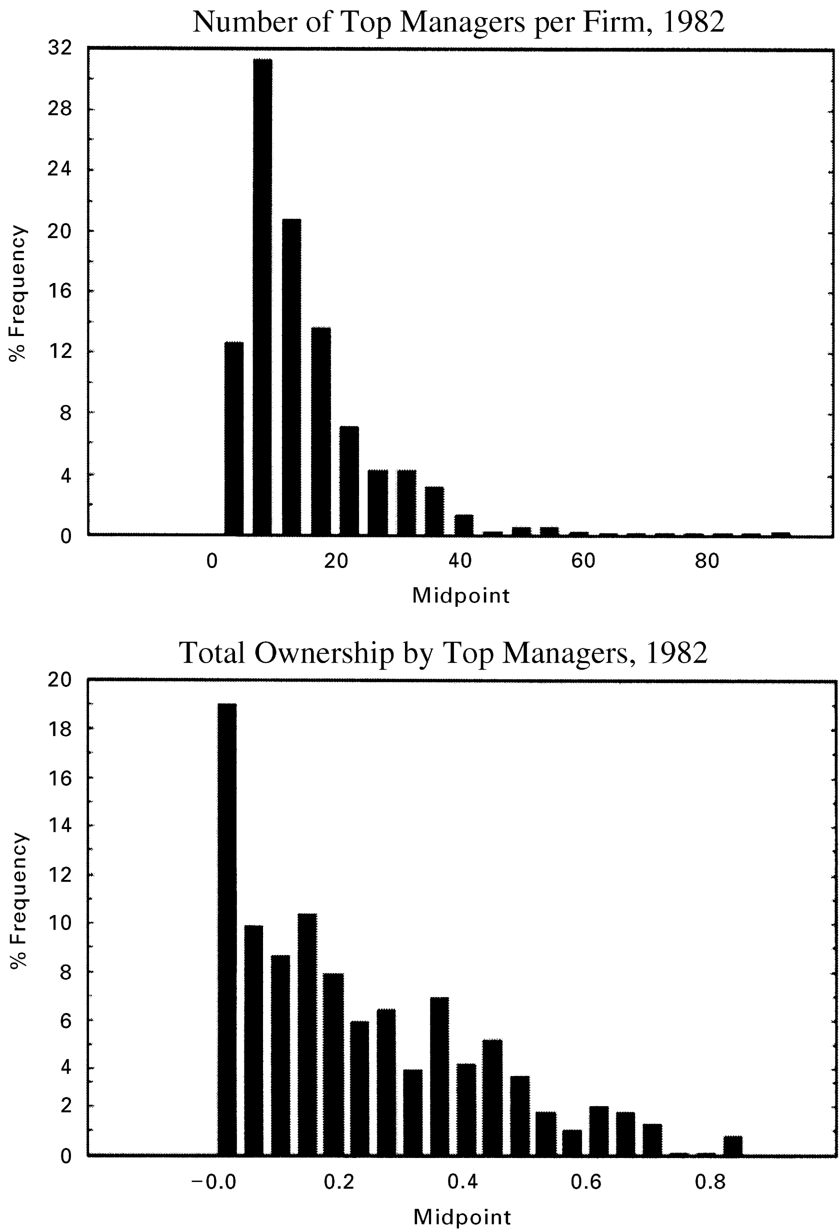

Fig. 1. Frequency distribution of managerial ownership and number of managers, 1982.

managerial stakes. If the scope for managerial discretion differs across firms according to observable differences in the composition of assets, then a prediction of the theory is that firms with assets that are difficult to monitor will have higher levels of managerial ownership. The specification used by Demsetz and Lehn (1985) to explain ownership concentration includes stock price volatility and industry dummies, but does not include proxies for the scope for managerial discretion (though managerial discretion is one interpretation offered for stock price volatility). We extend their specification by adding a large number of explanatory variables designed to proxy for the scope for managerial discretion, namely, size, capital intensity, cash flow, R&D intensity, advertising intensity, and gross investment rates. As we show below, this expanded variable set dramatically improves the R^2 statistic, and the coefficient estimates are all statistically different from zero with the predicted signs.

Size. Firm size has an ambiguous effect a priori on the scope for moral hazard. On the one hand, monitoring and agency costs can be greater in large firms, increasing desired managerial ownership. In addition, large firms are likely to employ more skilled managers, who are consequently wealthier, suggesting a higher level of managerial ownership. On the other hand, large firms might enjoy economies of scale in monitoring by top management and by rating agencies, leading to a lower optimal level of managerial ownership. We use the log of firm sales, $LN(S)$, and its square, $(LN(S))^2$, to measure size.

Scope for discretionary spending. To the extent that investments in fixed capital are observable and more easily monitored, firms with a greater concentration of fixed or 'hard' capital in their inputs will generally have a lower optimal level of managerial ownership (Gertler and Hubbard, 1988). We use the firm's capital-to-sales ratio, K/S, and its square, $(K/S)^2$, as measures of the relative importance of hard capital in the firm's technology.

Beyond hard capital, other firm spending is more discretionary and less easily monitored. The greater the role of these 'soft capital' inputs in the firm's technology, all else being equal, the higher is the desired level of managerial ownership. By including the capital-to-sales ratio, we have controlled (inversely) for soft capital, but some soft capital is 'softer' than others and hence more vulnerable to managerial discretion. To refine our proxies for the scope for discretionary spending, we use the ratio of R&D spending to capital, $(R\&D)/K$, the ratio of advertising spending to capital, A/K, and dummy variables for whether the firm reports R&D spending ($RDUM$) and advertising spending ($ADUM$) in that year. We include dummy variables when R&D and advertising are missing to control for the possibility that nonreporting firms are discretely different from reporting firms. By far the most common reason for not complying with the disclosure requirement is that the level of R&D or advertising expenditure is negligible. Simply eliminating observations with missing values for these variables is undesirable because it significantly reduces the sample size and biases the sample in favor of R&D-intensive and advertising firms.

As a proxy for the link between high growth and opportunities for discretionary projects, we use the firm's investment rate measured by the ratio of capital expenditures to the capital stock, I/K. Finally, we use the ratio of operating income to sales Y/S to measure market power or a firm's 'free cash flow' (the difference between cash flow and spending on value-enhancing investment projects). As suggested by Jensen (1986), the higher is a firm's free cash flow, all else being equal, the higher is the desired level of managerial ownership. While free cash flow is itself unobservable, it is presumably correlated with operating income.

Managerial risk aversion. Because higher managerial ownership levels, all else being equal, imply less portfolio diversification for managers, the optimal contract involves a tradeoff between diversification and incentives for performance. The higher is the firm's idiosyncratic risk, the lower is optimal managerial ownership. Demsetz and Lehn (1985) offer a second interpretation of this relation, suggesting that higher volatility indicates more scope for managerial discretion and thereby increases equilibrium managerial ownership levels. Unlike their specification, ours includes measures of intangible capital to control for managerial discretion. We therefore focus on the first interpretation of risk. As an empirical proxy for volatility, we use the standard deviation of the idiosyncratic component of daily stock prices (constructed from residuals from a standard CAPM regression), denoted by *SIGMA*, although our results are not qualitatively changed by the substitution of total stock return variance for our definition of *SIGMA*. Analogous to our treatment of missing values of $(R\&D)/K$ and A/K, we set missing values of *SIGMA* equal to zero, and then also include in the regression a dummy variable *SIGDUM* equal to unity when *SIGMA* is not missing, and zero otherwise.

To deal with zero-volume trading days in the daily data, we construct n-day returns by summing the Center for Research in Security Prices (CRSP) daily returns over the days in the period to create an approximate n-day return. We then divide this return (as well as n-day returns created by weekends and holidays) by the number of days in the period to obtain an average daily return. The variance of this average return will equal the variance of the daily return, and the same will be true for the idiosyncratic variance in a CAPM regression. Converting n-day returns to average daily returns thus removes the heteroskedasticity introduced by combining n-day-return observations with daily returns. Out of our initial universe of 600 firms for the 1984 period, the CRSP NYSE/AMEX and NASDAQ daily files contains 525 firms reporting returns in 1984. Of these, there are 502 firms with enough data to construct at least 20 observations on daily returns using only days for which trading volume is positive.

In addition to the problem of days with zero trading volume, there are days for which closing prices are not available, or days for which CRSP uses the average of the closing bid–ask spread instead of the closing price. This introduces nonclassical measurement error into the return calculation, which could bias our ordinary least squares (OLS) estimates of beta, return variance, and residual variance. To check the robustness of our results, we experiment with smaller samples that include only firms for which we can construct at least 20 observations based on (i) positive trading volume and (ii) transactions prices rather than the average of bid–ask prices. This substantially reduces the sample in 1984 to 328 firms (from 502). In practice, however, the results do not differ qualitatively from the larger sample. Hence, we report results in the paper using the larger sample, which, for many firms, relies on the closing average of the bid–ask spread rather than an actual closing price.

While we have addressed the most obvious examples of nonsynchronous trading (namely, days on which no trading occurs or on which CRSP cannot obtain a valid transaction price), there remain days on which CRSP calculates returns using the last price transacted rather than the closing price. As Scholes and Williams (1976), among others, point out, the inclusion of nonsynchronous trading days produces biased OLS estimates of beta. To check the robustness of our results against the possibility of biased beta estimates due to nonsynchronous trading days, we follow the approach recommended by Dimson (1979) by including leads and lags of the market return in the beta regression. These additional regressors are occasionally significant for some firms in some years, but using the alternative estimates of the idiosyncratic variance does not materially affect our results.

Summary. Combining these observable variables associated with moral hazard yields the following reduced-form expression for managerial ownership:

$$m_{it} = f(LN(S)_{it}, (K/S)_{it}, (R\&D/K)_{it}, RDUM_{it}, (A/K)_{it}, ADUM_{it}, (I/K)_{it}, (Y/S)_{it}, SIGMA_{it}, SIGDUM_{it}) + u_i + \eta_{it}, \quad (8)$$

where i and t represent the firm and time, respectively, u_i is a firm-specific effect, and η_{it} is a white-noise error term. Our list of variables is summarized in Table 3.

4.2. Evidence

Table 4A reports our estimates of the determinants of managerial stakes. The dependent variable in each case is $LN(m/(1-m))$. Each of the specifications includes year dummies (not reported). In specifications including fixed firm effects, we control for the unobserved firm heterogeneity represented by u_i in Eq. (6).

The first column reports results from a baseline specification using pooled data for all firm-years. Increases in firm size, all else being equal, are associated with a reduction in managerial stakes. Increases in fixed capital intensity (which

Table 3
Variable descriptions

Q	Tobin's *Q*, that is, the ratio of the value of the firm divided by the replacement value of assets. For firm value, we use the market value of common equity plus the estimated market value of preferred stock (roughly estimated as ten times the preferred dividend) plus the book value of total liabilities, and for replacement value of assets we use the book value of total assets. This definition is closely related to the market-to-book ratio, which is easily seen by subtracting total liabilities from both the numerator and denominator
m	The total common equity holdings of top-level managers as a fraction of common equity outstanding
m^2	The square of *m*, included to allow for nonlinearities
*m*1	Equals m if $0.00 < m < 0.05$; 0.05 if $m \geqslant 0.05$
*m*2	Equals $m - 0.05$ if $0.05 < m < 0.25$; 0.00 if $m \leqslant 0.05$; 0.20 if $m \geqslant 0.25$
*m*3	Equals $m - 0.25$ if $0.25 < m < 1.00$; 0.00 if $m \leqslant 0.25$
e	The average common equity holdings per manager. This number is calculated as the market value of common equity times the fraction held by top managers divided by the number of top managers
LN(*S*)	The natural log of sales, used to measure firm size
$(Ln(S))^2$	The square of *LN*(*S*), included to allow for nonlinearities in *LN*(*S*)
K/*S*	The ratio of tangible, long-term assets (property, plant, and equipment) to sales, used to measure the alleviation of agency problems due to the fact that such assets are easily monitored and provide good collateral
$(K/S)^2$	The square of *K*/*S*, included to allow for nonlinearities in *K*/*S*
Y/*S*	The ratio of operating income to sales, used to proxy for market power and measure the gross cash flows available from operations
SIGMA	The standard deviation of idiosyncratic stock price risk, calculated as the standard error of the residuals from a CAPM model estimated using daily data for the period covered by the annual sample
SIGDUM	A dummy variable equal to unity if the data required to estimated *SIGMA* is available, and otherwise equal to zero (if *SIGMA* is missing). To maintain sample size and reduce the risk of sample selection bias, we set missing observations of *SIGMA* equal to zero, and then include this dummy variable to allow the intercept term to capture the mean of the *SIGMA* for missing values
R&D/*K*	The ratio of research and development expenditures to the stock property, plant, and equipment, used to measure the role of 'R&D capital' relative to other non-fixed assets
RDUM	A dummy variable equal to unity if R&D data were available, and otherwise equal to zero (see the definition of *SIGDUM*)
A/*K*	The ratio of advertising expenditures to the stock of property, plant, and equipment, used to measure the role of 'advertising capital' relative to other non-fixed assets
ADUM	A dummy variable equal to unity if R&D data were available, and otherwise equal to zero. For usage details, see the definition of *SIGDUM*
I/*K*	The ratio of capital expenditures to the stock of property, plant, and equipment

we associate with lower monitoring costs) also lead to a decline in managerial stakes. Among our proxies for discretionary spending (R&D, advertising, investment rates, and operating income relative to capital), R&D intensity appears to have a negative effect on ownership stakes, while advertising intensity, operating income, and the investment rate appear to have positive effects on ownership stakes. Increases in idiosyncratic risk, as measured by *SIGMA*, raise the cost of managerial ownership in terms of reduced portfolio diversification and also reduce managerial ownership.

Table 4
(A) Determinants of total equity ownership by top managers

The specifications reported in this table all model the fraction of common *equity held by top managers*, *m*, by regressing the transformed dependent variable $LN(m/(1-m))$ on the explanatory variables indicated below. Intercept terms and year dummies are included for all regressions, but not reported. Fixed effects at the industry or firm level are included where indicated, but not reported. Variable definitions for the acronyms are given in Table 3.

Variable	All firms (Pooled)	All firms (SIC3 effects)	All firms (Firm effects)	Fortune 500 (Firm effects)	Non-500 (Firm effects)
$LN(S)$	− 0.195	− 0.182	0.058	− 1.288	0.252
	(0.050)	(0.053)	(0.095)	(0.697)	(0.121)
$(LN(S))^2$	− 0.027	− 0.027	− 0.038	0.040	− 0.067
	(0.005)	(0.005)	(0.010)	(0.045)	(0.016)
K/S	− 1.131	− 0.826	− 0.826	− 1.05	− 0.448
	(0.250)	(0.274)	(0.259)	(0.543)	(0.296)
$(K/S)^2$	− 0.023	− 0.011	0.301	0.440	0.143
	(0.157)	(0.145)	(0.122)	(0.228)	(0.141)
SIGMA	− 5.20	− 3.84	− 5.13	− 0.707	− 4.84
	(1.96)	(1.86)	(1.43)	(13.3)	(1.38)
SIGDUM	0.098	0.142	0.083	1.49	− 0.092
	(0.098)	(0.092)	(0.111)	(0.568)	(0.090)
Y/S	0.143	− 0.020	0.219	0.683	0.191
	(0.240)	(0.232)	(0.178)	(0.678)	(0.175)
$(R\&D)/K$	− 1.084	− 0.239	0.502	3.08	0.546
	(0.197)	(0.206)	(0.284)	(1.21)	(0.289)
RDUM	− 0.191	− 0.056	0.332	0.665	0.242
	(0.061)	(0.090)	(0.105)	(0.322)	(0.105)
A/K	0.227	0.953	0.184	3.60	− 0.067
	(0.217)	(0.332)	(0.438)	(1.19)	(0.413)
ADUM	0.143	− 0.082	0.042	0.033	− 0.037
	(0.061)	(0.067)	(0.072)	(0.215)	(0.077)
I/K	0.440	0.114	0.157	0.280	0.144
	(0.156)	(0.152)	(0.099)	(0.191)	(0.106)
# Obs.	2630	2630	2630	764	1866
Adj. R^2	0.407	0.584	0.884	0.884	0.831

Table 4. Continued.

(B) Determinants of average equity ownership per manager

The specifications reported in this table all model the *average equity owned by top managers*, *e*, by regressing the dependent variable *LN*(*e*) on the explanatory variables indicated below. Intercept terms and year dummies are included for all regressions, but not reported. Fixed effects at the industry or firm level are included where indicated, but not reported. Variable definitions for the acronyms are given in Table 3.

Variable	All firms (Pooled)	All firm (SIC3 effects)	All firm (Firm effects)	Fortune 500 (Firm effects)	Non-500 (Firm effects)
LN(*S*)	0.334	0.387	0.066	– 0.328	0.053
	(0.056)	(0.067)	(0.112)	(0.742)	(0.145)
$(LN(S))^2$	– 0.008	– 0.012	0.030	0.032	0.041
	(0.005)	(0.006)	(0.011)	(0.049)	(0.018)
K/S	1.044	1.629	0.830	0.510	0.888
	(0.255)	(0.302)	(0.300)	(0.600)	(0.355)
$(K/S)^2$	– 0.783	– 0.892	– 0.188	– 0.095	– 0.253
	(0.154)	(0.160)	(0.137)	(0.256)	(0.161)
SIGMA	– 18.5	– 18.3	– 14.5	– 24.6	– 12.6
	(2.14)	(2.12)	(1.68)	(15.3)	(1.67)
SIGDUM	0.089	0.915	0.598	1.63	0.412
	(0.101)	(0.100)	(0.115)	(0.526)	(0.107)
Y/S	1.58	1.14	1.80	4.63	1.56
	(0.326)	(0.335)	(0.242)	(0.830)	(0.235)
$(R\&D)/K$	– 0.174	0.154	0.380	3.79	0.282
	(0.201)	(0.223)	(0.348)	(1.48)	(0.353)
RDUM	0.212	0.003	0.430	0.597	0.379
	(0.065)	(0.100)	(0.116)	(0.315)	(0.125)
A/K	– 0.139	0.834	0.235	3.34	0.121
	(0.225)	(0.418)	(0.658)	(1.08)	(0.662)
ADUM	0.314	– 0.080	0.030	0.276	0.012
	(0.067)	(0.076)	(0.082)	(0.261)	(0.090)
I/K	1.429	1.06	0.575	1.04	0.525
	(0.174)	(0.173)	(0.117)	(0.251)	(0.128)
# Obs.	2628	2628	2628	763	1865
Adj. R^2	0.300	0.446	0.818	0.838	0.770

Notes: Estimated standard errors (reported in parentheses) are consistent in the presence of heteroskedasticity. The adjusted R^2 statistics reflect the inclusion of fixed effects (where included).

The specifications reported in the second and third columns of Table 4A control for unobserved heterogeneity at the industry level and firm level, respectively. The second column includes fixed three-digit SIC effects; the third column includes fixed firm effects. Demsetz and Lehn (1985) included controls for certain (regulated) industries. By including fixed industry effects (and, in some cases, fixed firm effects), we control for industry influences generally. The inclusion of fixed effects changes the estimated coefficients significantly in some cases. For example, if we do not control for unobserved industry-level or firm-level heterogeneity, the estimated coefficients on size and investment rate are significantly larger in absolute value, and the estimated coefficient of the ratio of R&D spending to capital changes sign. These differences suggest that the unobserved firm characteristics are correlated with the observed characteristics, and therefore bias the estimated coefficients in a cross-sectional or pooled regression. For example, in a univariate regression, if there were a strong positive equilibrium relation between R&D intensity and managerial ownership, excluding firm fixed effects would bias downward the estimated coefficient on *R&D/K* in a pooled regression.

The fourth and fifth columns of Table 4A report results from splitting the sample according to whether the firm is in the Fortune 500 in the given year (including firm-level fixed effects). Some subsample differences emerge. The negative effect of idiosyncratic risk (measured by *SIGMA*) on ownership is traced to non-Fortune 500 firms, consistent with our earlier interpretation. Effects of capital intensity, operating income, R&D intensity, advertising intensity, and the investment rate are larger in absolute value for larger firms.

Because theoretical models generally emphasize managerial ownership levels relative to the managers' wealth and not simply the fraction of firm equity held by managers, we present in Table 4B results from the same models presented in Table 4A, but with the dependent variable being the log of managerial equity per manager. (We do not observe managerial wealth, so we focus only on managerial equity.) Broadly speaking, the patterns we identified in Table 4A carry over to the estimates in Table 4B. One difference is that the estimated coefficient on the capital-to-sales ratio is everywhere positive and statistically significantly different from zero. This could reflect the fact that capital-intensive firms employ relatively fewer workers and managers, but have higher levels of value added per worker, and hence derive larger incentive benefits from higher levels of managerial ownership.

Taken together, the results presented in Table 4A and B suggest strongly that observable firm characteristics in the contracting environment influence managerial ownership. In addition, unobserved firm characteristics are correlated with observed characteristics, making coefficients estimated using panel data more reliable than those estimated using cross-sectional data. The beneficial ownership data include options exercisable within 60 days, but omit recent awards that are not yet vested. Because we lack data on all of the stock options granted to all top managers, we do not investigate the substitutability of direct ownership stakes and stock options as mechanisms to align incentives for value maximization. In the ExecuComp data over the 1992–1996 period, however, the correlation between the pay-performance sensitivity for managers using the 'stock' definition and the pay-performance sensitivity using the stock plus options definition exceeds 0.95.[1] Thus our focus on the beneficial ownership data appears warranted.

5. Managerial ownership and firm performance

5.1. Evidence on the exogeneity of managerial ownership

Thus far, we have emphasized that managerial stakes are part of a larger set of equilibrium contracts undertaken by the firm to align incentives for value maximization, and we have shown that managerial ownership can be explained by observable characteristics of the firm's contracting environment, such as stock price volatility and the composition of assets, as predicted by the contracting view. These results also show, however, that even when industry dummies are included, many important features of the firm's contracting environment remain unobserved. Specifically, including firm-level fixed dummy variables raises the adjusted R^2 from 0.584 to 0.884. These results cast doubt on the assumption that managerial ownership is exogenous in regressions that attempt to measure the impact of ownership on performance by regressing variables like Tobin's Q on managerial ownership without controlling for fixed effects.

In this section, we use panel data techniques to investigate more directly the question of whether managerial ownership can be treated as exogenous in the performance regressions. We use Tobin's Q as our measure of firm performance, but our results are robust to using return on assets as the dependent variable (tables are available upon request). To investigate the impact of managerial ownership on Q, we use variants of the reduced-form model in Eq. (3), in which Q depends upon managerial ownership, m, observable firm characteristics, x, and unobserved firm characteristics, u. We use two specifications of managerial ownership in the Q regression. The first includes m and m^2 (see McConnell and Servaes, 1990). The second includes three piecewise-linear terms in m (as in Mørck et al., 1988). Specifically,

$$m1 = \begin{cases} \text{managerial ownership level} & \text{if managerial ownership level} < 0.05, \\ 0.05 & \text{if managerial ownership level} \geqslant 0.05; \end{cases}$$

[1] We are grateful to Andrew Samwick for this calculation.

$$m2 = \begin{cases} \text{zero} & \text{if managerial ownership level} < 0.05, \\ \text{managerial ownership level minus } 0.05 & \text{if } 0.05 \leqslant \text{managerial ownership level} < 0.25, \\ 0.20 & \text{if managerial ownership level} \geqslant 0.25; \end{cases}$$

$$m3 = \begin{cases} \text{zero} & \text{if managerial ownership level} < 0.25, \\ \text{managerial ownership level minus } 0.25 & \text{if managerial ownership} \geqslant 0.25. \end{cases}$$

For observable characteristics, we use the same vector of x variables used in the model for managerial ownership. We report results including and excluding arguably endogenous 'investment' variables (R&D, advertising, and fixed capital).

Our empirical analysis of the effects of managerial ownership and firm characteristics on Q is summarized in Table 5A and B. Table 5A reports estimated coefficients for cases in which managerial ownership is represented by m and m^2. Table 5B reports estimated coefficients for cases in which managerial ownership is represented by the piecewise-linear terms, $m1$, $m2$, and $m3$. For both of the above specifications, we report estimated coefficients for (1) regressions with managerial ownership alone (pooled, SIC3 industry effects, and firm effects), (2) the regressions including the full set of x variables (pooled, SIC3 industry effects, and firm effects), and (3) the regressions including the non-investment set of x variables. All specifications include year effects (not reported).

Turning first to the quadratic specifications of managerial ownership in Table 5A, we note that the managerial ownership variables are statistically significant only in the pooled model with no other variables and in the model with only industry effects. In other specifications, the managerial ownership coefficients are virtually never statistically significantly different from zero. (The Wald test for the joint significance of m and m^2 is reported at the bottom of the table.) Once we control for observed firm characteristics (x), or for unobserved firm characteristics (in the firm-fixed-effect version of u), there is no effect of changes in managerial ownership on Q. Though not reported in Table 5A, these results hold for both the Fortune 500 and non-Fortune 500 subsamples considered earlier.

Turning to the spline specifications for managerial ownership in Table 5B, the pooled results are consistent with those of Mørck et al. (1988), who find that the impact of m on Q increases at a decreasing rate, and thereafter declines. In contrast to the quadratic specification for managerial ownership, the Mørck–Shleifer–Vishny specification is robust to the inclusion of observable contracting determinants and industry dummies. Once we control for x variables and for u (via firm fixed effects), however, changes in managerial ownership levels have no statistically significant effect on Q. These results hold both for the Fortune 500 and non-Fortune 500 subsamples investigated earlier.

The results reported in Table 5A and B confirm the intuition of the contracting example sketched in Section 2. First, the results obtained when observed characteristics (x) are included suggest that previously asserted relations between Q and m in part reflect equilibrium relations among Q and firm characteristics in the firm's contracting problem. Second, to the extent that firm characteristics unobserved by the econometrician influence the firm's contracts and the equilibrium level of managerial ownership, the coefficient on m in a Q regression (when no attempt is made to incorporate the unobserved heterogeneity) is biased. Third, in keeping with our emphasis on contracting, the relations we estimate suggest that no inference can be made about the effect of 'exogenous' local increases in managerial ownership on firm performance.

One can formalize this evidence against the exogeneity of managerial ownership by testing for a correlation between the fixed effect and managerial ownership. We could use a Hausman (1978) test, but this test would tend to over-reject the null hypothesis of zero correlation because it would tend to reject if *any* of the explanatory variables were correlated with the fixed effect. To reduce this Type I error, we construct a more precise 'conditional moment' test, which is in the spirit of a Hausman test, but tends to reject only if managerial ownership is the source of the specification error (Greene, 1997, p. 534; Newey, 1985).

The test is constructed as follows. Let the performance model be

$$Q_{it} = \beta_0 + \beta z_{it} + u_i + \varepsilon_{it}, \tag{9}$$

where z_{it} includes the managerial ownership variables and the x variables described earlier, and u_i is the firm fixed effect. The formal hypothesis we want to test is whether the unobserved fixed effect, u_i, is correlated with managerial ownership, an element of z_{it}. That is, H_0: $\mathrm{E}(m_{it} \cdot u_i) = 0$, where m_{it} is an $r \times 1$ vector of variables measuring the effect of managerial ownership. The idea of the test is to construct the simple analogue to the population moment, $s = \mathrm{E}(m_{it} w_{it})$, and then to test whether it is statistically significantly different from zero.

Using a consistent 'within' estimator of β, we can construct consistent estimates of the residual $w_{it} = u_i + \varepsilon_{it}$. Our test statistic is $\hat{s} = \sum_{i=1}^{N} \sum_{t=1}^{T_i} m_{it} \hat{w}_{it} / N T_i$, where T_i is the number of observations for firm i. Under standard regularity conditions and under the null hypothesis that $\mathrm{E}(m_{it} \cdot u_i) = 0$, $\sqrt{N}\hat{s}$ will be asymptotically distributed $\mathrm{N}(0, \Sigma)$. Therefore the statistic $k = N \hat{s} \hat{\Sigma}^{-1} \hat{s}$ is asymptotically chi-squared with r degrees of freedom, where $\hat{\Sigma}$ is a consistent estimate of Σ (for more details, see Greene, 1997).

Table 5
(A) Determinants of firm value (Tobin's Q), quadratic specification

The specifications reported in this table all model firm value, Q, as a linear function of the explanatory variables indicated below. In this table, the influence of m enters as a quadratic function. Intercept terms and year dummies are included for all regressions, but not reported. Fixed effects at the industry or firm level are included where indicated, but not reported. Variable definitions for the acronyms are given in Table 3.

Variable	Pooled	SIC3 effects	Firm effects	Pooled	SIC3 effects	Firm effects	Pooled	SIC3 effects	Firm effects
m	0.539	1.25	0.573	− 0.460	− 0.031	0.125	− 0.395	− 0.061	0.293
	(0.219)	(0.338)	(0.402)	(0.218)	(0.277)	(0.395)	(0.234)	(0.281)	(0.392)
m^2	− 1.123	− 1.649	− 0.582	− 0.062	− 0.579	− 0.438	− 0.235	− 0.571	− 0.577
	(0.317)	(0.457)	(0.559)	(0.304)	(0.393)	(0.507)	(0.317)	(0.401)	(0.522)
$LN(S)$	–	–	–	− 0.251	− 0.239	− 0.890	− 0.329	− 0.260	− 0.896
				(0.052)	(0.062)	(0.147)	(0.053)	(0.063)	(0.152)
$(LN(S))^2$	–	–	–	0.015	0.010	0.073	0.021	0.012	0.075
				(0.004)	(0.005)	(0.012)	(0.004)	(0.005)	(0.012)
K/S	–	–	–	0.621	0.277	− 0.482	0.469	0.342	− 0.504
				(0.152)	(0.192)	(0.289)	(0.155)	(0.208)	(0.303)
$(K/S)^2$	–	–	–	− 0.391	− 0.420	0.040	− 0.403	− 0.454	0.048
				(0.084)	(0.120)	(0.123)	(0.087)	(0.131)	(0.124)
$SIGMA$	–	–	–	− 5.06	− 4.82	− 4.26	− 4.47	− 5.40	− 4.62
				(1.25)	(1.26)	(1.28)	(1.25)	(1.27)	(1.29)

SIGDUM	–	–	–	0.260	0.241	− 0.044	0.258	0.275	− 0.036
				(0.051)	(0.056)	(0.066)	(0.054)	(0.057)	(0.067)
Y/S	–	–	–	0.652	0.713	1.44	0.782	0.664	1.43
				(0.279)	(0.305)	(0.269)	(0.292)	(0.317)	(0.264)
(*R&D*)/*K*	–	–	–	0.543	0.497	0.391	–	–	–
				(0.227)	(0.271)	(0.410)			
RDUM	–	–	–	0.156	0.264	0.191	–	–	–
				(0.046)	(0.071)	(0.125)			
A/K	–	–	–	− 0.066	− 0.423	0.082	–	–	–
				(0.125)	(0.209)	(0.448)			
ADUM	–	–	–	0.166	0.144	0.148	–	–	–
				(0.041)	(0.056)	(0.095)			
I/K	–	–	–	0.799	0.723	0.340	–	–	–
				(0.121)	(0.115)	(0.103)			
# Obs.	2630	2630	2630	2630	2630	2630	2630	2630	2630
Adj. R^2	0.012	0.127	0.584	0.130	0.213	0.630	0.073	0.178	0.626
p-value	–	0.038	0.194	–	0.008	0.156	–	0.008	0.084
Wald	20.125	13.142	2.501	23.254	15.599	1.897	36.348	15.034	1.588
pwald	–	0.001	0.286	–	–	0.387	–	0.001	0.452

Table 5. Continued.

(B) Determinants of firm value (Tobin's Q), spline specifications

The specifications reported in this table all model firm value, Q, a linear function of the explanatory variables indicated below. The influence of m enters as a spline function. Intercept terms and year dummies are included for all regressions, but not reported. Fixed effects at the industry or firm level are included where indicated, but not reported. Variable definitions for the acronyms are given in Table 3.

Variable	Pooled	SIC3 effects	Firm effects	Pooled	SIC3 effects	Firm effects	Pooled	SIC3 effects	Firm effects
*m*1	4.678	7.379	0.772	2.88	3.75	1.62	3.691	3.724	2.097
	(1.127)	(1.727)	(1.820)	(1.34)	(1.53)	(1.73)	(1.334)	(1.555)	(1.736)
*m*2	− 0.070	0.428	0.122	− 0.587	− 0.150	− 0.214	− 0.689	− 0.201	− 0.167
	(0.312)	(0.365)	(0.395)	(0.295)	(0.338)	(0.387)	(0.314)	(0.342)	(0.386)
*m*3	− 0.567	− 0.546	0.171	− 0.446	− 0.703	− 0.225	− 0.636	− 0.712	− 0.152
	(0.167)	(0.201)	(0.257)	(0.168)	(0.218)	(0.247)	(0.173)	(0.228)	(0.250)
LN(*S*)	–	–	–	− 0.263	− 0.247	− 0.896	− 0.345	− 0.268	− 0.903
				(0.054)	(0.062)	(0.147)	(0.055)	(0.064)	(0.152)
$(LN(S))^2$	–	–	–	0.017	0.012	0.074	0.023	0.013	0.075
				(0.005)	(0.005)	(0.012)	(0.005)	(0.005)	(0.012)
K/*S*	–	–	–	0.648	0.297	− 0.475	0.492	0.359	− 0.493
				(0.154)	(0.193)	(0.289)	(0.156)	(0.208)	(0.304)
$(K/S)^2$	–	–	–	− 0.395	− 0.427	0.036	− 0.404	− 0.459	0.042
				(0.084)	(0.120)	(0.123)	(0.087)	(0.130)	(0.125)
SIGMA	–	–	–	− 4.83	− 4.66	− 4.24	− 4.23	− 5.24	− 4.61
				(1.25)	(1.24)	(1.27)	(1.25)	(1.25)	(1.28)

SIGDUM	–	–	–	0.243	0.219	– 0.044	0.242	0.254	– 0.035
				(0.051)	(0.054)	(0.066)	(0.054)	(0.056)	(0.067)
Y/S	–	–	–	0.661	0.725	1.45	0.790	0.675	1.44
				(0.279)	(0.303)	(0.269)	(0.292)	(0.315)	(0.264)
(*R&D*)/*K*	–	–	–	0.551	0.499	0.384	–	–	–
				(0.225)	(0.268)	(0.409)			
RDUM	–	–	–	0.158	0.273	0.202	–	–	–
				(0.046)	(0.072)	(0.125)			
A/K	–	–	–	– 0.046	– 0.402	0.083	–	–	–
				(0.125)	(0.205)	(0.451)			
ADUM	–	–	–	0.159	0.142	0.144	–	–	–
				(0.041)	(0.055)	(0.095)			
I/K	–	–	–	0.776	0.713	0.339	–	–	–
				(0.123)	(0.115)	(0.103)			
# Obs.	2630	2630	2630	2630	2630	2630	2630	2630	2630
Adj. R^2	0.016	0.135	0.584	0.131	0.215	0.630	0.075	0.181	0.626
p-value	–	0.004	0.126	–	0.001	0.037	–	0.002	0.018
Wald	34.641	23.729	1.379	25.396	20.928	1.984	41.12	20.133	1.937
pwald	–	–	0.71	–	–	0.576	–	–	0.586

Note: Estimated standard errors (reported in parentheses) are consistent in the presence of heteroskedasticity. The adjusted R^2 statistics reflect the inclusion of fixed effects (where included). (The '*p*-value' is the probability of observing the test statistic for endogeneity described in the text. Low *p*-values suggest that ownership is endogenous.) *Wald* and *pwald*, report, respectively, the Wald statistic and associated *p*-value for a test that the managerial ownership variables are jointly zero.

The p-values for this test statistic are reported in Table 5A and B. In both tables, the p-values tend to be lower for the tests based on industry-level fixed-effects estimator. This presumably reflects the higher test power generally implied by the greater efficiency of the slope estimates. The rejection of the null hypothesis of exogeneity of managerial ownership is particularly strong for the spline specification reported in Table 5B. These results strongly suggest that reported results using such a specification are subject to endogeneity bias.

An important caveat to all empirical work using fixed-effect estimators on panel data is that the 'within' estimator can, under a range of certain circumstances identified by Griliches and Hausman (1986), exacerbate the bias toward zero caused by measurement error. If our ownership variable were measured with classical error, then this would reduce the power of our Wald test for the joint significance of the ownership variables, and would invalidate the distributional assumptions for our test statistic due to the inconsistency of the residual estimates. While it is always possible to make an a priori case for measurement error, there is little empirical evidence that measurement error is a serious problem in our data. Table 4A and B show, for example, that the within variation in managerial ownership is significantly correlated with the explanatory variables, a result that does not square with serious measurement error. In addition, the within-firm point estimates of the ownership coefficients in Table 5A and B are not obviously biased toward zero, as measurement error would suggest. Finally, the conditions identified by Griliches and Hausman might not hold. If the variance of the measurement error were primarily cross-sectional rather than within, then the within estimator would actually tend to reduce the bias effects of measurement error. We nevertheless recognize the limitations of the within estimator, and in the next section, we report instrumental variables estimated as an alternative approach to deal with the endogeneity of ownership variables.

5.2. *Toward a more structural interpretation of contracting relations*

The strength of the empirical evidence against the exogeneity of managerial ownership suggests that more model structure is required to identify the impact of managerial ownership on firm value. A standard remedy would be to use an instrumental variable in the regression for firm value. In a related paper, Hermalin and Weisbach (1991) recognize a similar endogeneity problem and use lagged explanatory variables as instruments for managerial ownership. They find that the instrumental variable estimator increases the magnitude of the ownership effect on firm value. Hermalin and Weisbach report that a Hausman specification test rejects the exogeneity assumption. While this rejection provides evidence of endogenous ownership, it does not validate their choice of instruments. If omitted firm characteristics are the source of the endogeneity (as we have argued above), and if these unobserved firm characteristics change slowly over time (as we have also argued above), then lagged explanatory variables will suffer as much from the endogeneity problem as do contemporaneous ones.

Instrumental variables for managerial ownership are difficult to find. The basic problem is that for any variable that plausibly determines the optimal level of managerial ownership, it is also possible to argue that the same variable might plausibly affect Tobin's Q. For example, our results in Table 4A and B showed that market power (as measured by operating margins) is a candidate instrument. However, even though it is correlated with managerial ownership, it also determines the equilibrium value of Tobin's Q, and therefore must appear independently in this regression. Additional candidates suggested by these results, such as the capital-to-sales ratio, advertising, R&D, and fixed investment, are also invalid because of links between investment and Q and because intangible assets are conservatively valued on the balance sheet, therefore influencing the level of Tobin's Q.

A more plausible case can be made for using firm size and stock price volatility as instruments. It is possible to construct arguments under which either variable could be correlated with Tobin's Q. For example, suppose that high Q values reflect future growth opportunities. Such firms might generally be smaller (or larger), and might also have more volatile stock prices due to the greater uncertainty about future growth prospects. However, these arguments seem weaker than the arguments against operating margins, the capital-to-sales ratio, advertising, R&D, and investment. Moreover, in studies of fixed investment, it is generally argued that deviations of Tobin's Q from its equilibrium value are explained by the costs of adjusting the capital stock, and that these adjustment costs are proportional to the rate of investment. Therefore, the inclusion of advertising and R&D intensity and the investment rate should control for future growth opportunities. This argument eliminates the a priori case for including the size and volatility variables in the Q equation, and thus provides an argument for omitting these variables from the Q equation and using them as instruments for managerial ownership instead.

The results using $LN(S)$, $(LN(S))^2$, *SIGMA*, and *SIGDUM* as instruments are reported in Table 6. We use the more parsimonious quadratic specification for managerial ownership to reduce the number of instruments required for identification. The first column of Table 6 reports the results of pooling without controlling for industry or firm effects. In contrast to Table 5A, these results confirm a large and statistically significant inverse-U relation between ownership and firm value. The coefficients of 6.29 and -10.8 on m and m^2, respectively, imply an inflection point of about 0.58. Given the distribution of managerial ownership shown in Fig. 1, Tobin's Q is generally an increasing, concave function of m.

The second and third columns of Table 6 show that these results are robust to the inclusion of three-digit industry effects, but not to firm effects. In both

Table 6
Ownership-performance model with instrumental variables

The specifications reported in this table all model firm value, Q, as a linear function of the explanatory variables indicated below. Intercept terms and year dummies are included. Instruments are $LN(S)$, $(LN(S))^2$, $SIGMA$, and $SIGDUM$. Fixed effects at the industry or firm level are included where indicated, but not reported. Variable definitions for the acronyms are given in Table 3.

Variable	Pooled	SIC3 effects	Firm effects
m	6.29	8.38	− 10.7
	(1.27)	(1.78)	(10.6)
m^2	− 10.8	− 12.3	− 4.88
	(2.61)	(3.48)	(10.8)
K/S	1.45	0.736	1.25
	(0.220)	(0.376)	(1.01)
$(K/S)^2$	− 0.738	− 0.587	− 0.611
	(0.119)	(0.171)	(0.396)
Y/S	0.414	0.603	1.003
	(0.273)	(0.300)	(0.637)
$(R\&D)/K$	0.739	0.718	0.687
	(0.242)	(0.339)	(1.11)
RDUM	0.178	0.256	− 0.230
	(0.049)	(0.115)	(0.805)
A/K	0.169	− 0.735	0.007
	(0.164)	(0.290)	(1.262)
ADUM	0.140	0.251	0.301
	(0.043)	(0.076)	(0.234)
I/K	0.990	0.937	0.478
	(0.140)	(0.151)	(0.202)
# Obs.	2630	2630	2630
Adj. R^2	− 0.197	− 0.057	0.192
Wald	67.576	55.659	1.310
pwald	–	–	0.519

Notes: Standard errors are in parentheses. *Wald* and *pwald* report, respectively, the Wald statistic and associated p-value for a test that the ownership variables are jointly zero.

specifications, the standard errors rise substantially, rendering the coefficients statistically indistinguishable from zero. However, this need not be interpreted as bad news for the results reported in the first column. One cannot reject the hypothesis that the firm effects are jointly zero, though one can reject that the industry effects are jointly zero (the p-values on the associated Hausman tests are 0.208 for the test of pooled versus the inclusion of firm effects and 0.00002 for the test of pooled versus the inclusion of industry effects). This conclusion has intuitive appeal because, by using instrumental variables, we have presumably controlled for the endogeneity that was the motivation for including firm fixed effects. However, it is more likely that the combined effect of using instrumental variables and controlling for fixed effects has reduced the precision of estimates to the point at which such a test would have little power. We believe that the results in Table 6 represent a promising step toward the construction of more complete models of the relation between managerial ownership and firm performance.

6. Conclusions

Firms are governed by a network of relations representing contracts for financing, capital structure, and managerial ownership and compensation, among others. For any of these contractual arrangements, it is difficult to identify the correspondence between the contractual choice and firm performance (e.g., measured by accounting rates of return or Tobin's Q) because contractual choices and performance outcomes are endogenously determined by exogenous and only partly observed features of the firm's contracting environment.

We confront this endogeneity problem in the context of the firm's compensation contract with managers. Because managerial equity stakes are an important and well-known mechanism used to align the incentives of managers and owners, we examine the determinants of managerial ownership as a function of the contracting environment. We extend the cross-sectional results of Demsetz and Lehn (1985) and use panel data to show that managerial ownership is explained by variables describing the contracting environment in ways consistent with the predictions of principal-agent models.

We find that a large fraction of the cross-sectional variation in managerial ownership is explained by unobserved firm heterogeneity. This unobserved heterogeneity in the contracting environment has important implications for econometric models designed to estimate the effect of managerial ownership on firm performance. Our empirical analysis shows that existing results are not robust to controls for endogeneity induced by time-invariant unobserved heterogeneity. Moreover, once we control both for observed firm characteristics and firm fixed effects, it becomes difficult to conclude that changes in firm managerial ownership affect performance. Our instrumental-variables results, however, suggest a promising step toward the construction of more complete models of the relation between managerial ownership and firm performance.

To take these observations one step further, we believe that the Q model results reported in Table 5A and B can be interpreted as supporting more generally the notion that the firm chooses among alternative mechanisms for minimizing agency costs. This is, of course, the concept articulated in Alchian (1969); Fama (1980); Fama and Jensen (1983) and Demsetz and Lehn (1985); more recently, see Crutchley and Hansen (1989) and Agrawal and Knoeber (1996).

Suppose, for example, that Q capitalizes the market's expectation of the effect of agency costs on firm value. The loss in value reflects residual agency costs, or agency costs remaining after corporate control mechanisms are chosen. In addition to managerial ownership choices emphasized here, alternative means of reducing agency costs include leverage (Jensen, 1986; Gertler and Hubbard, 1993), increased reliance on outside directors (American Law Institute, 1982; Baysinger and Butler, 1985; Millstein and MacAvoy, 1998), large shareholders (Shleifer and Vishny, 1986; Zeckhauser and Pound, 1990), institutional investors, dividend policy (Easterbrook, 1984), and radical changes in corporate control (Kaplan, 1989).

One can interpret the results in Table 5A, B, and 6 as reduced-form exercises in which the x variables and the firm fixed effects are determinants of the use of these mechanisms. For example, Gertler and Hubbard (1993) relate leverage in this context to the relative importance of firm-specific and aggregate risk and proxies for the scope for moral hazard (variables captured by R&D and advertising intensity, year dummies, and firm effects). Benefits from large shareholders likely depend on size or the relative importance of R&D (Zeckhauser and Pound, 1990); these channels are proxied through size, R&D intensity, and firm effects. Institutional shareholdings likely depend on firm size and whether the firm is listed on the New York Stock Exchange (variables accounted for in part by firm effects). The degree to which dividend policy can reduce agency costs depends on the importance of the scope for moral hazard (perhaps measured by idiosyncratic risk, R&D, or advertising) and the tax costs of paying dividends (measured in part by year and firm effects). Net benefits of a major restructuring also are reflected in proxies for moral hazard and firm effects.

Two other possible strategies are tasks for future research. The first involves identifying large, arguably exogenous changes in ownership levels arising from shifts in tax policy, regulation, or fixed costs in the market for corporate control (Kaplan, 1989; Hubbard and Palia, 1995; Cole and Mehran, 1997), though care must be taken because even certain 'natural experiments' are endogenous in that they affect performance directly. The second involves designing a dynamic structural model of firm contracting decisions, possibly permitting identification from economically reasonable assumptions about functional form (Margiotta and Miller, 1991). This strategy is particularly desirable given the lack of easily identified instrumental variables.

While our findings are consistent with the proposition that firms choose strategies to reduce agency costs optimally over the long run, at least two issues remain. First, the simultaneous choice of individual mechanisms or some subset needs to be modeled; subsets of these choices have been considered in a reduced-form setting, as in Hermalin and Weisbach (1991); Jensen et al. (1992); Moyer et al. (1992); Holthausen and Larcker (1993) and Agrawal and Knoeber (1996). Second, the choice of mechanisms likely involves some fixed costs or 'costs of adjustment' so that firms are not always at their long-run contractual optimum. Exploring these costs and how they might have changed over time for different agency-cost-reducing mechanisms is a particularly interesting task for future research.

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