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The investment opportunity set and corporate financing, dividend, and compensation policies*

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We examine explanations for corporate financing-, dividend-, and compensation-policy choices. We document robust empirical relations among corporate policy decisions and various firm characteristics. Our evidence suggests contracting theories are more important in explaining cross-sectional variation in observed financial, dividend, and compensation policies than either tax-based or signaling theories.

1. Introduction

To date, there has been little empirical analysis of the cross-sectional structure of corporate financing, dividend, and compensation policies. Although much effort has been devoted to developing the theory of these basic corporate policies, empirical support for the models is largely anecdotal. Our primary objective in this paper is to examine whether there are robust empirical relations among corporate policy decisions and various firm characteristics. We believe a more balanced interaction between theory and testing in corporate finance will produce richer models and more powerful econometric methods of data analysis.

A model of the cross-sectional variation in corporate policies requires specification of the exogenous variables that drive policy selection. Many potential variables vary over time, but not across firms. For example, all firms have access to the same contracting technology (e.g., sinking funds, dividend covenants,

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executive stock options, cancelable leases). And, given well-functioning labor, capital, and product markets, all firms have access to any potential stockholder, bondholder, manager, lessor, or customer; they all have access to individuals with different risk preferences or personal tax rates. Thus, neither personal tax provisions, risk preferences, nor the contracting technology appear able to explain observed cross-sectional variation in corporate policies.

In making investment and employment decisions, however, firms invest in specialized physical and human capital. These firm-specific investments result in variation in firms' investment opportunity sets (i.e., their prospective investment opportunities and associated payoff distributions). Corporate taxes and regulation also vary across firms. Thus, the investment opportunity set, regulation, and corporate tax provisions offer the potential to explain cross-sectional policy variation. These are the explanatory variables we use in our analysis.

Of course, aspects of these variables are endogenous. For example, regulation and tax policy are determined within the political process, and we observe innovation in both the real investment activities of firms as well as the contracts they employ. Our statistical analysis, however, requires only that these factors be predetermined, not that they be completely exogenous.

Using industry-level data from 1965 to 1985 we find that measures of the firm's investment opportunity set (such as the availability of growth options and firm size) are related to its financing, dividend, and executive-compensation policies. In particular, we document that firms with more growth options (i.e., greater access to positive net present value projects) have lower leverage,[1] lower dividend yields [see also Rozeff (1982)], higher executive compensation, and greater use of stock-option plans. We also find that regulated firms have higher leverage, higher dividend yields, lower executive compensation, and less frequent use of both stock-option and bonus plans. Finally, we find that larger firms have higher dividend yields and higher levels of executive compensation [see also Fox (1986) and Murphy (1985)].

These relations imply associations among the corporate policies themselves. Our evidence indicates positive associations between leverage and dividend yield and between compensation and the use of both bonus and stock-option plans. Negative associations are documented between leverage and compensation, bonus and stock-option plans, as well as between dividend yield and both bonus and stock-option plans.

Our empirical analysis includes a broader range of investment-policy characteristics than previous studies,[2] and focuses on the partial effects of each exogenous variable (i.e., holding the other variables constant). We also relate firm characteristics not just to a single corporate policy choice but to financing,

[1]See also Ferri and Jones (1979), Castanias (1983), Bradley, Jarrell, and Kim (1984), Long and Malitz (1985), and Titman and Wessels (1988).

[2]See footnote 1; also Rozeff (1982).

dividend, and compensation policies. In this way, we help control for potential sources of spurious correlation that can be troublesome if a single corporate policy is examined in isolation.

Other research confirms the empirical relations documented in this paper. Gaver and Gaver (1993) use firm-level data and measure growth options by the frequency of a stock's inclusion in growth-stock mutual funds. Holthausen and Larcker (1991) use firm-level data supplemented by confidential firm-level compensation data. Kole (1991) uses firm-level data on compensation plans to investigate the variation in the board of directors' authority to award stock or stock options to management.

In section 2 we describe our data and the instrumental variables used to measure corporate financing, dividend, and compensation policies as well as our independent variables. We also discuss our empirical methods. In section 3 we predict the empirical relations between these policies and investment-opportunity-set, size, regulation, and tax variables, and present evidence on the estimated relations. In section 4 we examine the implications of our analysis for the relations among financing, dividend, and compensation policies. We present our conclusions in section 5. The appendix contains sensitivity analysis and provides evidence on the robustness of our results.

2. Data and empirical methods

Investigating the empirical relations among the investment opportunity set, regulation, and firm size on the one hand, and firms' financing, dividend, and executive-compensation policies on the other, requires a wide range of data, some of which (especially compensation data) can be difficult to obtain. Data on executive compensation and the use of formal incentive plans are available by industry in the Conference Board surveys of executive compensation. We use the Conference Board survey data for every fourth year from 1965 through 1985 as reported in Fox (1966, 1970, 1974, 1978, 1982, 1986). Because this compensation data is available at an industry level only, we estimate investment-opportunity-set, financing-policy, and dividend-policy variables for each of Fox's industry definitions for each year in the study using annual firm data for a sample of *Compustat* firms chosen to match the firm-size attribute Fox reports (which is typically industry sales). We then generate industry-level data by averaging data on individual firms sorted by industry. The use of industry-level data should reduce measurement error in the variables if Fox's classification of industries using SIC codes effectively groups firms by the nature of their investment opportunity set. It should also maintain dispersion among the variables. We describe how we assemble our data and match the compensation data with other data in section A.1 of the appendix.

In this section, we describe measures used in the empirical analysis. Relatively accurate financing, dividend, compensation, regulation, and firm-size measures are available, but measures of the investment opportunity set involve substantial measurement error. We attempt to address this problem by using several alternate measures, as well as by using an instrumental-variables approach and testing the specification of the relations among the measures.

2.1. Endogenous variables

Financing policy. A firm's financing policy is represented by its *equity-to-value ratio* (E/V). The equity-to-value ratio for industry i in year T is calculated using four years of data:

$$E/V_{iT} = \left\{ \sum_{t=T-3}^{T} \left[\sum_{j=1}^{N_{it}} E_{jt}/V_{jt} \right] \Big/ N_{it} \right\} \Big/ 4, \tag{1}$$

$$T = 1965, 1969, 1973, 1977, 1981, 1985,$$

where N_{it} is the number of sample firms in industry i with data available in year t, and V_{jt} is the proxy for the market value of firm j at the end of year t. V_{jt} is equal to the market value of firm j's equity at the end of year t (E_{jt}) plus the book value of its assets at the end of year t (A_{jt}) minus the book value of its equity at the end of year t.

Dividend policy. A firm's dividend policy is represented by its *dividend yield* or dividend-to-price ratio (D/P). The dividend yield for industry i in year T is calculated as

$$D/P_{iT} = \left\{ \sum_{t=T-3}^{T} \left[\sum_{j=1}^{N_{it}} D_{jt}/P_{jt} \right] \Big/ N_{it} \right\} \Big/ 4, \tag{2}$$

$$T = 1965, 1969, 1973, 1977, 1981, 1985,$$

where N_{it} is the number of sample firms in industry i with data available in year t, D_{jt} is dividends per share for firm j in year t, and P_{jt} is firm j's share price at the end of year t.

Compensation. We use the *CEO's salary* as a surrogate for management compensation. Since this surrogate ignores compensation under incentive plans, it measures compensation with error. Ignoring incentive compensation probably reduces the likelihood of observing any relation between the investment opportunity set and compensation, however, since it reduces the variation in the measured level of compensation. We adjust the median CEO salary for each industry-year using the GNP deflator. The log of the resulting median CEO real salary is used to measure compensation.

Use of incentive plans. The variables for the use of incentive plans are the percentage of firms in each industry with bonus plans and the percentage with stock-option plans. (Fox also reports the use of stock appreciation rights, but only for years after 1977.) Data on the combination of plans (for example, the percentage of firms in each industry with at least one incentive plan) are not available in Fox.

2.2. Exogenous variables

Investment opportunity set. The primary variable used in this study as a proxy for the investment opportunity set is the ratio of book value of assets to firm value (A/V). The book value of assets (A_{jt}) is used as a surrogate for assets in place. We predict that the higher A/V, the higher the ratio of assets in place to firm value, and the lower the ratio of the value of investment opportunities to firm value. Since book value is historical cost less depreciation, it contains potentially significant measurement error for firms with long-lived assets.

The ratio is calculated for each industry i in year T using four years of data:

$$A/V_{iT} = \left\{ \sum_{t=T-3}^{T} \left[\sum_{j=1}^{N_{it}} A_{jt}/V_{jt} \right] \Big/ N_{it} \right\} \Big/ 4, \tag{3}$$

$$T = 1965, 1969, 1973, 1977, 1981, 1985,$$

where N_{it} is the number of sample firms in industry i with data available in year t. Sensitivity analysis using other investment-opportunity-set measures is reported in section A.2 of the appendix.

Regulation. We use dummy variables for regulation. We consider the insurance, gas and electric utility, and banking industries as regulated and the other thirteen industries as unregulated. In our base-case regressions, we use a single intercept dummy variable for regulation, though the effect of regulation on policy choices probably varies across the three industries. (See the section A.2 of appendix for an examination of the sensitivity of the results to different dummy variables for each industry.)

Firm size. Strictly speaking, firm size is an endogenous variable that depends on economies of scale in both production and organization of the firm. Size is thus a function of the investment opportunity set. Yet given our limited knowledge of the determinants of size, we include size itself as an exogenous variable. We measure firm size by the log of the *Compustat* sample's median real sales for each industry year (i.e., 1965, 1969, 1973, 1977, 1981, or 1985) for the unregulated industries. We use the GNP deflator to restate nominal sales from *Compustat* so that we measure size in constant dollars. For the regulated industries, we measure size by median real premium income in the insurance industry, median real operating income in the utilities industry, and median real worldwide deposits in the banking industry. The use of a different size measure in the regulated industries introduces noise in the size measure, but it does not appear to introduce any bias (although we investigate this possibility in section A.2 of the appendix).

Accounting return. We add the accounting return as an additional independent variable in the compensation regression. The mean accounting return for industry i in year t is calculated using annual accounting data:

$$R_{jt} = (OI_{jt} + INT_{jt})/V_{jt-1}\,, \tag{4}$$

where for sample firm j in year t R_{jt} is the accounting return, OI_{jt} is the operating income, and INT_{jt} is interest expense. We obtain the mean return by averaging R_{jt} over the sample firms in the industry and over four years (the contemporaneous and three previous years). We include this variable because CEO compensation varies with performance [see Murphy (1985)].

2.3. Empirical methods

We pool cross-section and time-series observations and regress the various policy variables (financing, dividend, compensation, and incentive plans) on measures representing all three exogenous variables (investment opportunity set, regulation, and size). We also regress the policy variables on exogenous variables separately each year. Since a simultaneous system of equations underlies the data, our estimated parameters are thus reduced forms, not structural parameters.

The regressions are over industry-years. The two regressions with dependent variables obtained from *Compustat* (E/V and D/P) are estimated over 94 observations (insurance and banking are unavailable for 1965). The three compensation regressions are estimated over 91 observations (construction is unavailable for 1965, 1969, and 1973).

Specification tests. Two diagnostic tests are used in all regressions: the White (1980) specification test and a test for nonlinearities. The White test indicates whether the regression errors are heteroskedastic or if the errors and explanatory variables are (nonlinearly) dependent. Although White's specification test is valid asymptotically, its accuracy in small samples is more conjectural. For our regressions, the White chi-square test of first- and second-moment specification generally shows that the null hypothesis of no misspecification can be rejected at the 0.0001 level. The White procedure also generates a variance–covariance matrix of coefficient estimators that converges to the true variance–covariance matrix in large samples. This gives the opportunity to produce test statistics that have the right size. The White asymptotic standard error for our estimated coefficients is typically lower than that from an ordinary-least-squares regression, so the significance of the coefficients generally increases if the White standard error is used to calculate the t-statistic.

To test for nonlinearities, we sort the residuals by the values of each continuous explanatory variable and calculate a Durbin–Watson statistic. Nonlinearities for an explanatory variable show up as correlated errors. (We assume that, except for the influence of nonlinearities, regression errors are cross-sectionally independent.) Firms tend to follow the same financing, dividend, and compensation policies over time. When we run cross-sectional regressions separately for each year, however, none of the Durbin–Watson statistics is significant. Hence, there is no evidence of significant cross-sectional dependence.

3. Theory and evidence

We discuss each policy variable in turn, first developing predictions about the relation between the policy and the exogenous variables from contracting, tax-based, and signaling theories. We then examine evidence from our regression results. Table 1 summarizes the contracting-hypothesis predictions and reports the estimated regressions for the various policy variables based on observations pooled over time and across industries.

If there are multiple partial effects in the estimated coefficients, we are unable to separate them without additional structure. For example, contracting arguments imply that firms with more growth options should have lower debt in their capital structure, whereas signaling and tax effects imply higher debt. If the estimated relation between growth options and leverage is significantly negative, we conclude that the contracting effect is significant, whereas if the estimated relation is positive, we conclude that the combination of signaling and tax effects is significant. Since we estimate only the net effect, we cannot separately identify the significance of less important partial effects. In most cases in which two explanations lead to predictions for the sign of the relation between a variable and a policy (e.g., the proportion of assets in place and dividend yield), the predictions are for opposite signs, so we can reject one of them.

Also, we do not specify any interdependencies among policies. For example, we predict that firms with more growth options (fewer assets in place) use stock options more frequently because management is more difficult to monitor in such firms. We do not allow for the possibility that the management of firms using stock options increases leverage to increase the value of the options by raising equity volatility. To sort out these partial effects we would have to develop and estimate a structural model specifying the nature of the interdependence. Titman and Wessels (1988) follow such an approach and impose a complex structure on the estimated relations among variables. If the structure they use is correct, the power of their estimates is increased, but if their structure is incorrect, they impose bias. Given our current knowledge of these empirical

Table 1

Base-case results for financing-policy, dividend-policy, and compensation-policy regressions on investment-opportunity-set, regulation, size and return measures for 16 industries, 1965–1985.

The first row in the coefficients on independent variables columns gives the predicted sign for the coefficient under the contracting hypothesis, the second row is the estimated coefficient, and the third row is the t-statistic. The Durbin–Watson statistics next to A/V, Ln sales, and Acc return are the statistics when the residuals are sorted on those independent variables. These statistics test whether the relation is linear for the variable on which the residuals are sorted. The regression is significant at least at the probability level shown below the F-statistic.

Dependent variables	Number of observations	Intercept	Assets/value	Regulation dummy	Log of real sales	Accounting return	Adjusted R-square	F-statistic	Durbin–Watson	
			Coefficients on independent variables							
Equity value			−	−	−					
	94	1.34	− 0.62	− 0.25	− 0.03		0.79	114.53	A/V	1.82
		17.03	− 12.47	− 12.33	− 3.43			0.0001	Ln sales	1.88
Dividend price			+	+	?					
	94	− 0.05	0.05	0.01	0.01		0.61	50.20	A/V	1.86
		− 5.03	9.19	6.14	4.20			0.0001	Ln sales	1.69
Log of real salary			−	+	+	+				
	91	4.77	− 0.30	− 0.47	0.17	1.50	0.62	38.30	A/V	2.09
		23.41	− 2.51	− 9.08	6.47	2.89		0.0001	Ln sales	1.93
									Acc return	1.73
Existence of bonus plan			?	−	+					
	91	0.10	0.32	− 0.43	0.06		0.48	28.58	A/V	2.39
		0.51	2.69	− 8.79	2.23			0.0001	Ln sales	1.87
Existence of stock-option plan			−	−	+					
	91	0.78	− 0.19	0.46	0.03		0.78	105.93	A/V	2.04
		7.47	− 3.03	− 17.34	2.14			0.0001	Ln sales	2.23

relations, we believe progress is better served by documenting robust empirical relations between policy parameters and exogenous variables before attempting to subdivide the relations into component effects.

3.1. Financing policy

Contracting hypotheses. There are several contracting arguments relating to financing policy. Myers (1977) describes the firm's potential investment opportunities as call options whose values depend on the likelihood that management will exercise them. If the firm has risky debt outstanding, situations arise in which exercising the option to undertake a positive net present value project potentially reduces share value because debtholders have a senior claim on the project's cash flows. Unless this conflict between the shareholders and debtholders is controlled, the probability that these real investment options will be exercised is reduced, thereby reducing firm value. One way to control this underinvestment problem and its associated value loss is to finance growth options with equity rather than debt. Hence Myers predicts that the larger the proportion of firm value represented by growth options (i.e., the lower the assets in place), the lower the firm's leverage, and the higher its equity-to-value ratio. Regulation also controls incentive problems between stockholders and fixed claimholders by reducing discretion over the firm's projects. Hence regulated firms are predicted to have lower equity-to-value ratios. Jensen (1986) suggests that firms with more free cash flow choose higher levels of debt in their capital structure as a credible precommitment to pay out the excess cash. If firms with more growth options have less free cash flow, this analysis also predicts a negative relation between assets in place and equity-to-value.

Tax hypotheses. Progressivity in the tax structure implies that greater volatility in taxable income raises the firm's expected tax liabilities [Smith and Stulz (1985)]. If firms with more growth options have more volatile cash flows, they have incentives to reduce the amount of debt in their capital structure over the range of progressivity. Hence this tax effect implies a negative relation between the proportion of assets in place and the equity-to-value ratio.

Other tax-code provisions, however, potentially affect financing policy differently. DeAngelo and Masulis (1980) argue that firms that generate substantial noninterest tax shields, such as investment tax credits, have a comparative disadvantage in using interest tax shields and thus should have less debt in their capital structures. If capital-intensive firms are more likely to generate investment tax credits than firms whose value derives largely from growth options, such firms should have lower equity-to-value ratios.

Signaling hypotheses. A substantial literature examines the impact of information asymmetries on financing policy, but most of it does not attempt to develop implications for cross-sectional variation in leverage. Analyses such as that of Myers and Majluf (1984) focus on explaining stock-price reactions to announcements of security offers. Cross-sectional implications for financing policy are not apparent.

The information asymmetry models that do have potential implications for cross-sectional variation in firms' policy choices are signaling models. For example, Ross (1977) develops a signaling model that examines the relation between leverage and firm quality, holding any information disparity fixed. Issuing debt in his model is a signal of high quality because the firm exposes itself to the costs of financial distress. Therefore, high-quality firms choose higher leverage.

Yet in this signaling literature, quality is not defined in terms of observable variables. To derive testable implications, we assume that with no information disparity there is no incentive to signal, and that the greater the information disparity, the greater the derived demand for signaling. We also assume that if the costs of signaling vary, they are less sensitive than the benefits of signaling to variation in the size of the information disparity. With these assumptions, the signaling analysis implies that if firms with more growth options face greater information disparities, they should be high-debt (i.e., low equity-to-value) firms. Also, if regulated firms have lower information disparities, they should be low-debt firms.

Size hypotheses. If costs of financial distress limit leverage, the greater diversification (and consequent lower return variance) of larger firms enables them to have higher leverage than smaller firms. We therefore predict a negative relations between the equity-to-value ratio and firm size.

Regression results. In table 1, the coefficients of asset/value, regulation, and firm size from the regression are all reliably negative and the regression itself is significant at the 0.001 level. The Durbin–Watson statistics when the observations are ranked on the basis of A/V and log of real sales suggest there is no significant departure from linearity for those independent variables. Hence, the evidence from the regression is consistent with E/V being a reliably negative function of assets in place in the investment opportunity set and a reliably negative function of regulation. Both negative functions are consistent with our contracting arguments and inconsistent with the signaling hypothesis. The negative relation between E/V and assets in place is also consistent with the progressive-tax effect, but inconsistent with the effects of investment tax credits. That E/V is a negative function of firm size is consistent with costs of financial distress limiting leverage.

3.2. Dividend policy

Contracting hypotheses. The firm's cash-flow identity links investment and dividend policy: the greater the amount of investment during the period, the smaller the dividend or the more the new equity issued. Jensen (1986) argues that firms with more growth opportunities have lower free cash flow and pay lower dividends. Hence, there should be a positive relation between the proportion of assets in place and dividend yield. Two contracting arguments reinforce this predicted relation. First, Rozeff (1982) and Easterbrook (1984) argue that the new-issue market lowers agency costs by providing effective monitoring. Firms with fewer growth options would go to the new-issue market less frequently and forego this benefit if they pay fewer dividends. Second, dividend covenants that specify a maximum on payouts effectively impose a minimum investment requirement [see Smith and Warner (1979) and Kalay (1982)], thereby reducing the underinvestment problem. The more binding the dividend constraint, however, the more likely it is that managers will be forced to undertake negative net present value investments (although this cost is eliminated if firms can invest in financial assets that offer normal returns). Firms with more profitable investment options can tolerate more restrictions on dividends before the expected benefits of controlling payout are offset by the expected cost of forced negative net present value investments. Hence firms with more growth options (i.e., lower assets in place) are expected to pay lower dividends.

Smith (1986) argues that the regulatory process gives managers of regulated utilities an incentive to pay higher dividends in order to force the utility to raise funds more frequently in the capital market. New issues provide evidence on the firm's cost of capital that is useful in the regulatory process. Without such evidence, the utility commission faces fewer constraints in reducing the firm's rate of return. Higher dividends thus discipline the regulators as well as the firm's managers. In addition, some regulatory authorities still set required returns by using a form of the dividend-growth model, whereby higher dividend payments raise allowed rates of return. Therefore, we expect that regulated firms pay higher dividends.

Tax hypotheses. In the dividend literature we find no tax analysis that can explain cross-section variation in dividends. An important reason for this lack of explanatory power is the endogeneity of personal shareholder tax rates. For example, Litzenberger and Ramaswamy (1982) argue that since dividends are taxed at higher effective personal rates than capital gains, higher-dividend firms generate higher expected personal tax liabilities and thus require higher expected before-tax returns. But since all firms have access to potential shareholders of the various tax brackets, the shareholder tax rate is endogenous. Thus, the Litzenberger and Ramaswamy analysis has no implications for cross-sectional corporate dividend policy choice.

Signaling hypotheses. Bhattacharya (1979) develops a signaling model in which he argues that high-quality firms pay high dividends. Again, if the signal increases with the information disparity between managers and investors, firms with greater information disparities (typically unregulated firms with more growth options) should pay higher dividends.

Size hypothesis. To make the regression comparable to the other base policy regressions, the log of real sales is included in this regression although we have no reason to expect firm size to affect dividend policy.

Regression results. The regression evidence in table 1 indicates that the estimated coefficients of regulation and assets/value are both reliably positive. This evidence is consistent with the contracting predictions and inconsistent with the signaling predictions. The size coefficient also is reliably positive in the regression. The overall regression is significant and again there is no evidence of nonlinearity for A/V or the log of real sales.

3.3. Compensation

Contracting hypotheses. We hypothesize that the marginal product of investment decision makers is greater than the marginal product of supervisors and good decision makers are less numerous than good supervisors.[3] Therefore, the larger the proportion of firm value represented by growth options, the greater the manager's compensation.

Regulation restricts the manager's investment discretion and reduces the marginal product of the decision maker, so regulation should reduce the level of compensation. Some regulatory authorities appear to regulate compensation policy directly, placing limits on payments to executives. Such limit should increase perquisite consumption. This presents a potential problem in our empirical analysis, since consumption of perquisites is difficult to measure. If managers of regulated firms consume more perquisites than managers of unregulated firms, our evidence overstates the difference in compensation between the two groups. We also include the accounting return in this regression because we expect that CEO compensation varies with performance [see Murphy (1985)].

Size hypotheses. In general, the larger the firm, the larger the stock of real resources that can be affected by a given managerial decision. Managers of larger firms thus have a higher value added, so we expect higher compensation for executives of larger firms.

Regression results. Coefficients on all four independent variables are significant in the compensation regression in table 1. The coefficients of A/V and regulation are negative and the coefficients of the log of real sales and the accounting return are positive. The regression is significant at the 0.0001 level, and there is no evidence of nonlinearity for A/V, the log of real sales, or accounting return. The growth options, regulation, and firm-size results are

[3] We expect that this effect will be reinforced by managers' compensating differentials for risk. Given risk-averse managers with firm-specific human capital who cannot completely diversify their compensation risk, the higher the firm's risk, the higher the risk of the manager's compensation, and the higher the managers' equilibrium compensation. We expect that, as an empirical proposition, the larger the proportion of firm value represented by growth options, the greater the firm's risk. Also, we argue below that the larger the proportion of firm value represented by growth options, the more likely it is that the manager's compensation is tied to firm value and the greater the variance of the manager's compensation. We believe, however, that these compensating differentials for risk are secondary. See section A.2 of the appendix.

consistent with our contracting predictions. The accounting-return result is consistent with previous evidence that CEO compensation varies with firm performance. Although increased perquisite consumption by CEOs of regulated firms potentially explains the difference in compensation between regulated and unregulated firms, perquisite consumption cannot explain our reported financing-policy or dividend-policy coefficients for regulated firms.

3.4. Incentive compensation

Contracting hypotheses. The typical problem analyzed in the principal–agent literature is that of a risk-neutral principal attempting to induce a risk-averse agent to take the action the principal would take.[4] If the principal can observe the agent's actions, the optimal contract pays the agent a fixed wage and penalizes him for taking suboptimal actions; that contract imposes all the risk on the risk-neutral principal. If the principal cannot observe the agent's actions, the optimal contract gives the agent a share in the outcome of his actions. That contract provides an incentive to expend effort to achieve the principal's objective, thus justifying the increased compensation of the agent for bearing the additional risk.

When we apply this principal–agent analysis to large firms, shareholders are considered risk-neutral because they can diversify firm-specific risk. If managers cannot effectively diversify the risk of their compensation payments, they are risk-averse in their actions. We suggest that managers' actions are less readily observable if the firm has more investment opportunities. It is difficult for shareholders or outside board members who do not have the manager's specific knowledge to observe all the investments from which the manager chooses. In general, the larger the proportion of firm value represented by growth options, the more likely that the firm ties compensation to the effect of the manager's actions on firm value.

This linkage does not by itself imply the use of formal incentive plans. The manager's salary could be informally renegotiated periodically on the basis of previous performance. But the effectiveness of future salary renegotiation depends on expected future employment (e.g., a 64-year-old manager facing retirement at 65 would be little motivated by an annual salary-renegotiation scheme). It also depends on the degree to which the renegotiation promise is bonded. Informal salary renegotiation is less effective if there is higher management turnover and thus less reason to expect future managers to honor unwritten, informal contracts. These problems encourage the use of explicit incentive plans that tie the manager's compensation to a performance measure that reflects the effects of the manager's actions on firm value (e.g., stock price or accounting earnings). Hence the larger the proportion of firm value represented

[4]For a survey of this literature see MacDonald (1984).

by intangible investment opportunities, the more likely the firm is to have a formal incentive compensation plan. We thus expect a negative relation between the proportion of assets in place and the use of stock-option plans.

More growth options are likely to make accounting numbers poorer measures of performance. For example, Rao (1989) provides evidence that most of a start-up firm's value is represented by investment opportunities. The impact of managers' actions on those opportunities is not accurately measured by accounting numbers. This effect should reduce the use of accounting-based incentive plans and offset the incentive of firms with more growth options to use incentive compensation plans. Thus, the relation between the proportion of assets in place and the use of formal accounting-based incentive plans is ambiguous.

If regulation restricts the investment opportunity set and makes observation of the manager's actions easier, regulated firms are less likely to use formal incentive plans.

Tax hypotheses. In addition to these contracting arguments, taxes are potentially important in determining the use of incentive compensation plans. Miller and Scholes (1982) show that incentive compensation plans frequently contain a deferral aspect that is attractive only if the executive's effective tax rate is higher than that of the corporation (as happened during the period 1965–1985). This hypothesis also has implications for the compensation policy of banks and insurance companies. Firms in these industries are allowed to receive tax-exempt income from municipal bonds while deducting interest paid on CDs or indemnity payments to policyholders. Thus, banks and insurance companies face lower effective tax rates and should use incentive compensation provisions more frequently. Yet these industries use incentive compensation less frequently.

Size hypotheses. Given fixed costs and scale economies in the administration of incentive compensation plans, such plans should be observed more often in large firms. Eaton and Rosen (1983) argue that the problems of monitoring management increase with firm size. Also, Christie, Joye, and Watts (1989) offer span-of-control arguments and present evidence that larger firms are more likely to decentralize, implying that large firms employ incentive contracts more frequently [see Smith and Watts (1982) and Sloan (1993)]. We thus expect a positive association between firm size and the use of incentive compensation plans.

Regression results. Consistent with contracting predictions, the t-statistic in table 1 from the bonus-plan regression shows that the coefficient on the regulation dummy is reliably negative. The coefficient of A/V is positive and significant in the regression, which is consistent with the hypothesis that accounting numbers are less useful as performance measures for firms with growth opportunities. The coefficient of the log of real sales also is positive and significant.

All three coefficients (A/V, regulation, and the log of real sales) have the signs predicted by contracting arguments and are significant in the stock-option regression. The regression is significant at the 0.0001 level, and there is no evidence of nonlinearities for A/V or the log of real sales. Generally, the results suggest that the existence of a stock-option plan is a reliably negative function of both regulation and A/V.

3.5. Sensitivity analysis

In general, the regression results for financing policy, dividend policy, compensation, use of bonus plan, and use of stock-option plan are consistent with the contracting predictions. In contrast, the results for the two policies for which the signaling hypothesis has predictions (financial and dividend policy) are inconsistent with those predictions. Taxes could explain the A/V coefficient in the financial-policy regression via an association between A/V and cash flow variance, but contrary to tax implications, banks and insurance firms have higher equity-to-value ratios and less frequently use incentive compensation plans. Overall, the evidence is more consistent with the contracting hypothesis than with the signaling or tax hypotheses.

In the appendix we examine the robustness of our results. In particular, we investigate alternate investment-opportunity-set variables, sensitivity to problems with the regulatory subsample, the time-series stationarity of the relations, and positive dependence in corporate policies. The evidence in the appendix indicates that the results in table 1 are robust to alternate measures of investment opportunities. When we estimate the policy regressions without the regulated industries, the general tenor of the results is unchanged. When we allow the effect of regulation to vary across regulated industries, our basic results for the asset/value, size, and accounting-return variables do not change. The estimated coefficients are relatively stationary over time, especially the financing policy, dividend policy, and stock-option-plan relations. Finally, positive dependence in corporate policies is a problem for the regulation variable only. Even then, the regulation coefficients are still significant when cross-sectional variation and not time-series variation is used to estimate the corporate policy relations.

Gaver and Gaver (1993) provide an independent test of the robustness of the results. Using individual-firm data rather than industry data and different investment-opportunity-set variables, they replicate the results presented in this paper.

4. Relations among policies

If contracting theories are more important than signaling or taxes in explaining cross-sectional variation in corporate policy choices, we should observe predictable relations among policies. For example, we expect a negative relation between E/V and D/P because the larger the proportion of firm value represented by growth options, the higher the firm's equity-to-value ratio and the lower its dividend yield. Table 2 contains predictions on these policy relations.

4.1. Correlations with leverage

Under the contracting argument, firms with more growth options have less debt because of the more severe incentive problems associated with debt [Myers (1977)] and because they have less use for debt as a creditable commitment to distribute excess cash flow. These firms have less incentive to use dividends to subject themselves to the discipline of the new-issue market when their investments create demand for new capital. Signaling models reinforce the prediction of a positive association between leverage and dividend yield, since high-quality firms should choose both high leverage and high dividends. Regulatory restrictions on investment reduce incentive problems associated with debt and so encourage regulated firms to have higher leverage. Regulated firms also have incentives to pay higher dividends and thus discipline regulators through their more intensive use of capital markets.

For the full sample, the estimated correlation between the ratio of equity to value and dividend yield is negative, as predicted, and is significant. (The table 2 results are generally unaffected by using the Spearman rank-order correlation.) When the regulated industries are excluded, the absolute value of the estimated correlation between E/V and dividend yield falls from 0.49 to 0.33, suggesting that regulation reinforces the negative relation between E/V and D/P. When we estimate the E/V and D/P regressions excluding the regulated industries, the t-statistics for assets/value are still highly significant, but the t-statistic for firm size is insignificant in the leverage regression. This evidence suggests growth options are responsible for much of the correlation between leverage and dividend yield in the unregulated sample.

We predict a positive relation between E/V and compensation. Contracting arguments suggest managers of firms with more growth options are paid more because of their greater marginal product. In table 2 the relation between E/V and compensation is reliably positive for both the full and unregulated samples. Regulation should reinforce this positive relation because it reduces compensation by reducing the manager's marginal product. In fact, the absolute correlation between E/V and compensation in table 2 is less for the unregulated sample than for the full sample (0.50 versus 0.70). The table 1 regressions for financing and compensation both indicate significant effects of size. Since the estimated size coefficients show the opposite signs, however, size effects would imply a negative correlation between E/V and compensation, not the observed positive correlation.

We predict E/V to be positively related to the use of stock-option plans. Firms with more growth options are more likely to use stock-option plans because the

Table 2

Unconditional relations among financing-policy, dividend-policy, and compensation-policy variables for 16 industries and 13 unregulated industries, 1965–1985.

	Dividend policy	Compensation policy		
			Use of incentive plans	
	Dividend yield	Log of real salary	Bonus	Stock-option
Financing policy				
Equity value ratio				
Predicted sign	−	+	?	+
All industries	− 0.49[b]	0.70[a]	0.62[c]	0.73[a]
Unregulated industries	− 0.33	0.50[b]	− 0.05	0.55[b]
Dividend policy				
Dividend yield				
Predicted sign		−	?	−
All industries		− 0.19	− 0.68[c]	− 0.64[a]
Unregulated industries		0.32	0.03	0.11
Compensation policy				
Log of real salary				
Predicted sign			?	+
All industries			0.55[d]	0.70[a]
Unregulated industries			0.01	0.56[b]

[a]Significant at the 1% level (one-tailed test).
[b]Significant at the 10% level (one-tailed test).
[c]Significant at the 1% level (two-tailed test).
[d]Significant at the 10% level (two-tailed test).

manager's actions are less likely to be observable. Regulation is expected to reduce the use of incentive plans and so reinforce the expected positive relation between E/V and stock options. In table 2 the correlations between E/V and the use of stock-option plans are reliably positive for both the full and regulated samples. Consistent with the reinforcement effect of regulation, the association is stronger for the full sample than for the unregulated sample. The estimated correlation between E/V and bonus plans is close to zero for the unregulated sample, but for the full sample it is reliably positive, consistent with the implications of the effect of regulation.

4.2. *Correlations with dividends*

On the basis of contracting arguments, we predict dividend yield to be negatively associated with both compensation and the use of stock-option plans, since we expect firms with more growth options to have lower dividend yields and higher compensation, and to use stock-option plans more often. Regulation should reinforce these predictions, since we expect it to increase dividend yield, reduce compensation, and reduce the use of incentive plans. For the full sample, the estimated correlations in table 2 have the expected sign. The correlation between dividend yield and stock-option plans is reliably negative, but the correlation between dividend yield and compensation is insignificant. When we exclude regulated firms the estimated correlations are insignificant and have the wrong sign, suggesting that the estimated associations for the full sample are due to the regulatory effect. Similarly, the use of bonus plans is significantly negatively correlated with dividend yield for the full sample but not for the unregulated sample, which is again consistent with the effect of regulation on the use of incentive plans.

4.3. Correlations among compensation variables

Finally, we predict that compensation is positively correlated with the use of stock-option plans. We expect firms with more growth options to have higher compensation and to use stock-based incentive plans more often. Regulation should reinforce this expected relation. As predicted, the estimated correlation in table 2 is reliably positive in both the full and unregulated samples, but less so for the unregulated sample. The estimated correlation between compensation and the use of bonus plans is also reliably positive for the full sample, but not for the unregulated sample, again suggesting regulation is important in reducing the use of bonus plans.

4.4. Summary

The predicted and observed correlations among the policies in table 2 are generally consistent. When the regulated industries are included, each correlation has the predicted sign and regulation has the predicted directional effect on the correlation. We believe these unconditional correlation results are important in interpreting the results of many empirical studies. For example, Ang and Peterson (1984) examine tradeoffs between leasing and debt. They find that firms that issue more debt tend to engage in more leasing. Smith and Wakeman (1985) argue that this result should not be surprising; although leasing and debt are substitutes for a given firm, when investment opportunity sets provide high debt capacity they also tend to provide more profitable leasing opportunities. To measure the extent of substitutability between leasing and debt, differences in investment opportunities must be controlled. Lambert, Lanen, and Larcker (1989) find evidence that the initial adoption of executive stock option plans is associated with dividend reductions. Kole (1991) notes that this association could reflect changes in firms' investment opportunity sets rather than simply a compensation-induced change in dividend policy. Finally, Nance, Smith, and Smithson (1992) find no significant relation between leverage and hedging, which they interpret as an inability to separate two effects that work through leverage: (1) given investment opportunities, more leverage should produce stronger incentives to hedge; and (2) firms with more leverage have fewer growth options and lower incentives to hedge.

5. Conclusions

Although evidence on the relations between growth options and leverage and dividend policies had been provided previously, this paper is the first to present evidence on the relations between growth options and compensation policy, between regulation and leverage, dividend, and compensation policies, and among the policies themselves. Documentation of these empirical relations is an important step in focusing the profession's attention on the explanation of empirically important phenomena. Refinement of the relations examined here and examination of additional relations should provide guideposts for the development of richer theory. Our evidence suggests contracting theories are more important in explaining cross-sectional variation in observed financial, compensation, and dividend policies than either tax-based or signaling theories.

Although we believe our results, as well as those of Gaver and Gaver (1993), Holthausen and Larcker (1992), and Kole (1992) are suggestive, much work remains. There are potentially important limitations of this initial analysis and thus several ways in which the power of our tests might be increased. First, our exogenous variables are at least partially endogenous. A model that more effectively separates exogenous from endogenous components of the investment opportunity set would increase the power of our tests. Second, we do not have measures of the specific tax status of companies in our industries. More detailed data would enable more powerful tests of tax-based hypotheses. Third, other corporate policies can be examined: for example, leasing, hedging, and accounting policies also should be driven by the firm's investment opportunity set.

Appendix

A.1. Matching the Fox and Compustat data

Table A.1 gives Fox's industry groupings by SIC code for each year of the analysis. Some of the definitions are not constant across years. For the unregulated industries in table A.1 (all except insurance, utilities, and banking), we begin with all *Compustat* firms that fall into one of Fox's industries in a survey year. For each industry in each year, we sort the firms by sales. We find the firm with sales closest to the median industry sales reported by Fox for that year. If, for example, that firm is number 53 in our sorted list of firms in that year, we keep twice 53 or 106 firms in the *Compustat* sample for that industry-year; the 107th firm and firms further down the list are dropped. (The median size of

Table A.1
Industry definitions (SIC codes) for 16 industries from Fox by year for 1965, 1969, 1973, 1977, 1981, and 1985.

Industry	Years[a]	Definition (SIC codes)
1. Insurance	69–85	6312–6332
2. Gas & electric utilities	65–85	4911–4932
3. Banking	69–85	6022–6026
4. Manufacturing machinery	65	3500–3599
	69–85	3510–3580
5. Electrical machinery	65–81	3600–3699
	85	3600–3699 & 3800–3899
6. Paper	65–81	2600–2699
	85	2600–2799
7. Stone, clay & glass	65–85	3200–3299
8. Food	65–85	2000–2099
9. Textile mill products & apparel	65–73 & 85	2200–2399
	77–81	2200–2299
10. Primary metals	65–85	3300–3399
11. Construction	77–85	1600–1799
12. Retail trade	65–85	5211–5999
13. Consumer chemicals	65–77	2800–2899
	81–85	2830–2848
14. Fabricated metals	65	3400–3499
	69–85	3410–3499
15. Transportation equipment	65	3700–3799
	69–85	3711–3792
16. Industrial chemicals (petroleum)	65–77	2900–2999
	81–85	2810–2820 & 2850–2890

[a]If a year is not included in the ranges specified for an industry, data are not available for firms in that industry in that year and the industry-year is not included in the empirical work. Insurance and banking are not included in 1965 and construction is not included before 1977.

Compustat firms in a given unregulated industry is always smaller than the median Fox firm, so we always drop smaller rather than larger *Compustat* firms in forming the *Compustat* samples for those industries.) In calculating each variable for Fox's industries we use every firm in the *Compustat* industry-year subsample that has available data. Hence, the number of firms used for a given industry can vary across years and for different variables, although for most variables (research and development being the major exception), the number is the same.

For the regulated industries (insurance, gas and electrical utilities, and commercial banking), Fox reports size attributes other than sales (premium income, total current operating revenue, and deposits, respectively). We use those attributes instead of sales in forming the *Compustat* sample for the regulated industries. Insurance firms' financial data for 1965 are not available on *Compustat* at Rochester, and the insurance industry is included in the empirical work only from 1969 on (see table A.1). Premium income is not available on *Compustat* and has to be collected from *Moodys' Bank and Finance Manual*, which reduces the *Compustat* sample substantially and results in almost all available *Compustat* insurance firms being larger than the insurance firms in Fox's samples. We nonetheless include every *Compustat* insurance firm with data available in a given year in order to have an adequate sample.

The procedures used to obtain the *Compustat* sample for unregulated industries are also applied to obtain the *Compustat* sample for utility firms in 1969, 1973, 1977, 1981, and 1985. In 1965, however, only 20 gas and electric utility firms are available on *Compustat* at Rochester. We include all 20 in the 1965 *Compustat* utility sample, although because Fox's utility sample contains large NYSE-listed utilities, our 1965 *Compustat* utility sample tends to be smaller than Fox's 1965 sample.

Fox uses deposits as the size statistic for the banking industry. The *Compustat* file includes two deposit numbers: worldwide and domestic deposits. Since Fox's deposit definition is unclear, we use worldwide deposits because it yields the larger number of observations. This time the *Compustat* banks are larger than Fox's banks (possibly because Fox uses domestic deposits), but to give us a sample of acceptable size, we include all *Compustat* banks with financial data available for at least one of the five years beginning with 1969. (Bank data are not available on our *Compustat* files for 1965 – see table A.1.)

Besides the median, Fox also reports the distribution of the size measures across five to seven size intervals. We calculate a chi-square statistic to compare the Fox sample and *Compustat* sample size distributions. The two samples are significantly different at the 0.10 level in 10 of the 11 industry-years for which data are available for the insurance and banking industries and for the utilities industry for 1965. We expect these results, however, since in those industry-years we are unable to select the *Compustat* samples to match the Fox samples by firm size. For the 13 unregulated industries and the utilities industry for years other than 1965 (when we could select the *Compustat* sample by firm size), the two sample distributions are significantly different at the 0.10 level in only 25 out of the 80 industry-year observations, and at the 0.01 level in only 10 of the 80 industry-years. (There are 80 industry-years available instead of 83 – 13 industries for six years and one industry for five years – because Fox does not report compensation data for the construction industry until 1977.)

Although the limitations in matching the two samples introduce noise into the estimated relations that involve compensation-policy variables, estimated relations involving only financial variables are not affected. When the compensation variables are used as dependent variables in regressions that have *Compustat* variables as independent variables, the effect of the noise on the other variables'

Table A.2

Distributions of number of firms in industry-years for Fox and Compustat samples across 16 industries and six years.[a]

Quantiles	Number of firms in industry-year	
	Fox	Compustat
Maximum	243	208
0.75	84	94.5
0.50	56	55.5
0.25	34	36
Minimum	21	10

[a]Total number of industry-years is 91 rather than 96 because data are not available in *Compustat* for the insurance and banking industries in 1965 and Fox does not report compensation data for the construction industry in 1965, 1969, and 1973.

coefficients should be to bias their t-statistics toward zero. (In two of the three regulated industries, however, the mean firm size for the *Compustat* sample is larger, and this could bias the estimated coefficient of the regulation variable toward its predicted sign–see section A.2 of the appendix.)

Table A.2 gives the distributions of the number of firms available for calculating mean variables in industry-years for the Fox and *Compustat* samples. The total number of industry-years in both samples is 91 (75 unregulated and 16 regulated). The median industry-year in the Fox sample includes 56 firms and the median *Compustat* industry-year includes 55.5 firms. A Wilcoxon–Mann–Whitney test [see Siegel and Castellan (1988, p. 128)] does not reject the hypothesis that the two distributions are the same at conventional critical probability levels ($z = 0.003$).

A.2. Sensitivity analysis

Alternate investment opportunity set measures. As a specification check, we use other investment-opportunity-set measures: the ratio of depreciation to firm value (DEP/V), the ratio of research and development to firm value ($R\&D/V$), the variance of the rate of return on the firm[5] (VAR), the earnings/price ratio (X/P), and the ratio of capital expenditures to firm value (CAP/V). We average the ratios across firms and years as in eq. (3) and calculate the variance for each firm over four years and then average across firms. There is considerable correlation among these alternate measures, however. To deal with this multicollinearity, we focus on the base case that includes only the ratio of book-to-market values,

[5]The rate of return on the firm (r_t) is defined as

$$r_t = (r_{e,t}(E_{t-1}) + INT_t)/V_{t-1}\,.$$

A/V, reported in section 3. Here, we reestimate the base-case regression substituting each of these variables in turn for A/V.

When VAR is substituted for A/V, the results are very similar to those in table 1. All regressions are significant, though the dividend-policy and financial-policy regressions are less significant than those using A/V. All estimated coefficients have the same sign and significance as the equivalent coefficients in table 1.

The regressions when CAP/V or DEP/V is substituted for A/V are very similar to each other and to those in table 1. All regressions are significant at the 0.0001 level, though the financial-policy regressions are less significant than those using A/V. The one change in estimated coefficient sign for both CAP/V and DEP/V regressions is for the coefficient of log of real sales in the financial-policy regression. That coefficient is positive in the CAP/V and DEP/V financial-policy regressions and is significantly positive in the CAP/V regression. The only other different result for the CAP/V and DEP/V regressions is that the coefficient of log of real sales in the bonus regression is insignificant.

When the earnings/price ratio (X/P) is substituted for A/V, the results are similar to those in table 1. All regressions are significant, though the dividend- and financial-policy regressions are less significant than those using A/V. The difference in results from those in table 1 is that the coefficient of the investment-opportunity-set variable in the compensation, bonus, and stock-option regressions becomes insignificant. The two of those coefficients with the signs predicted by the contracting hypotheses (compensation and stock-option) have significance levels of 0.28 and 0.11, respectively. Overall, X/P is a less effective measure of the effect of the investment opportunity set on compensation policy than A/V.

When $R\&D/V$ is used instead of A/V, the coefficient in the bonus regression changes sign and is significant. There is one other change in sign. The coefficient of log of real sales in the financial-policy regression is insignificantly different from zero, but positive, contrary to prediction. Four coefficients that are significant in table 1 are insignificant in the $R\&D/V$ regressions: the coefficients of the investment opportunity set in the dividend and financial-policy regressions and the coefficients of log of real sales in the bonus and stock-option regressions. Overall these results suggest that $R\&D/V$ is more associated with compensation policy than with financial and dividend policy. All the regressions using $R\&D/V$ are significant at the 0.0001 level except the financial-policy regression, which is significant at the 0.03 level.

These results show that the results in table 1 are generally robust to alternate specification of the investment-opportunity-set variable. The strongest results in table 1 are for the financial-policy, dividend-policy, compensation, and stock-option regressions. In those regressions, all estimated coefficients have the sign predicted by the contracting hypotheses and are significant. When the alternate investment-opportunity-set variables are substituted in those four regressions, the estimated coefficients of the investment-opportunity-set variable have the predicted signs in all five alternate specifications for each of the four regressions and are significant in four of the five specifications for each of the four regressions. The X/P (compensation and stock-option regressions) and $R\&D/V$ (financial and dividend-policy regressions) specifications each provide two insignificant coefficients.

Instrumental variables. To examine further the robustness of the results to alternate specifications of the investment-opportunity-set variable, we use an instrumental-variable approach. A/V is regressed on the other investment-opportunity-set variables and the predicted values from that regression are substituted for A/V in the base-case regressions. As might be expected from the lack of sensitivity to the substitution of the individual alternatives to A/V, the results in table 1 do not change in any substantive way under the instrumental-variable approach. None of the coefficients change sign or significance.

Problems with the regulatory subsample. There are several potential problems with the use of the regulated industries in our empirical analysis. One is that while sales are used as the firm size measure for the unregulated industries, other measures are used for regulated firms. A stock measure (deposits) is used in banking although a flow measure (sales) is used in other industries. The effect of this use is unclear, since it is like using accounts receivable for sales and the effect will depend on how frequently deposits turn over. Bank deposits are higher than any other industry's size measure. Total current operating revenue (utilities' size measure) and premium income (insurance-industry size measure) are analogous to sales and are less likely to involve bias or noise than the measure for banking. To assess the effects of the different size measures we reestimate the five policy regressions excluding all three regulated industries and dropping the regulation dummy variable.

Table A.3 reports the results. All the regressions remain significant, though the significance level drops. Excluding the regulated industries causes size to become insignificant in the financing-policy and stock-option regressions and reduces the significance of A/V in the compensation regression. The A/V coefficient retains the sign predicted by the hypotheses and remains significant in both regressions involving *Compustat*-based dependent variables (financial and dividend policies) as well as in the compensation and stock-option regressions. The firm-size coefficient retains its predicted sign and remains significant in the compensation and bonus regressions. Thus the general tenor of our results remains.

Another problem is identified earlier in the paper. The banking, gas and electric utilities, and insurance industries are subject to different regulations, so the effect of regulation on firms' policies is likely to differ across these industries. To assess the effect of this variation, we substitute separate intercept dummy variables for each regulated industry for the single regulatory dummy variable in the policy regressions.

Table A.3

Base-case results for financing-policy, dividend-policy, and compensation-policy regressions on investment-opportunity-set, regulation, size and return measures for 16 industries, 1965–1985.

The first row in the coefficients on independent variables columns gives the predicted sign for the coefficient under the contracting hypothesis, the second row is the estimated coefficient, and the third row is the t-statistic. The Durbin–Watson statistics next to A/V, Ln sales, and Acc return are the statistics when the residuals are sorted on those independent variables. These statistics test whether the relation is linear for the variable on which the residuals are sorted. The regression is significant at least at the probability level shown below the F-statistic.

		Coefficients on independent variables							
Dependent variables	Number of observations	Intercept	Assets/value	Log of real sales	Accounting return	Adjusted R-square	F-statistic	Durbin–Watson	
Equity/value			−	−					
	78	1.11	−0.57	−0.00		0.71	94.32	A/V	2.19
		14.57	−13.64	−0.53			0.0001	Ln sales	1.68
Dividend/price			+	?					
	78	−0.03	0.04	0.00		0.68	83.84	A/V	1.89
		−5.20	12.49	5.33			0.0001	Ln sales	2.02
Log of real salary			−	+	+				
	75	4.02	−0.16	0.27	1.20	0.59	36.30	A/V	2.07
		18.47	−1.40	9.98	2.59		0.0001	Ln sales	2.09
								Acc return	2.39
Existence of bonus plan			?	+					
	75	0.19	0.30	0.04		0.07	3.73	A/V	1.82
		0.79	2.49	1.51			0.03	Ln sales	1.68
Existence of stock-option plan			−	+					
	75	0.89	−0.20	0.01		0.12	5.91	A/V	2.21
		7.01	−3.12	0.92			0.005	Ln sales	1.92

When we make this substitution in the base case, the coefficients of all 15 dummy variables have the sign predicted by the contracting hypotheses and all but the coefficient of the insurance dummy variable in the dividend/price regression are significant. But, the individual-industry dummy coefficients are significantly different across the three industries. All else being equal, the banks have a significantly lower equity-to-value ratio than the insurance companies, which in turn have a significantly lower ratio than the utilities. Utilities' dividend yields are significantly higher than those of the insurance companies or banks, and banks pay a significantly lower salary than the insurance companies or utilities. Finally, utilities are significantly less likely to have a bonus plan and a stock-option plan than either banks or insurance firms. Despite these differing industry effects, the substitution of separate industry dummy variables does not change the tenor of the results for the coefficients of assets/value, firm size, and accounting return.

It is possible that regulation affects the slope coefficients as well as the intercept. Although we expect this effect to work against confirming the contracting predictions, we introduce both regulated-industry intercept dummies and multiplicative dummies for the explanatory variables to check that the significant results for the investment-opportunity-set and firm-size variables in the base cases are not due to misspecification of the regulatory effect. This procedure also allows us to check for the problem (mentioned in section A.1) that the insurance and banking firms in the *Compustat* sample are much larger than those in Fox's sample. This difference affects the compensation and incentive-plan regressions. Since it appears smaller firms pay less compensation and are less likely to have incentive plans, the insurance and banking industries will show lower compensation and use of incentive plans (which come from Fox's sample) than their firm sizes (which come from *Compustat*) suggest. A possible result is a negative sign for the regulation coefficient and a lower slope coefficient for firm size.

Specific regulated-industry intercept and slope dummy variables do not change the tenor of the results for the assets/value, firm-size, and accounting-return variables. Few of the 33 slope dummy coefficients are significant. None of the predictions for the firm-size coefficients based on the sample matching problems are confirmed. The primary effect is to reduce the significance of the intercept dummy coefficients.

Time-series stationarity. The regressions assume that relations between the exogenous variables and the endogenous policy variables are stationary over time. These relations may have changed over time. For example, regulated firms have increased their use of bonus plans significantly in more recent years. This could be due to more relaxed regulation. The effect of nonstationarity is to increase the estimated standard errors of the coefficients and reduce the estimated coefficients' significance. But the changes in the relations can provide insight into the relations themselves, and for that reason we examine their stationarity.

The first hypothesis we test is that all coefficients (including the intercept) are constant across the full period (1965–1985). The sums of squared errors from separate cross-sectional regressions for each year are compared with sums of squared errors from the regressions that require all coefficients to be constant over time. The hypothesis is not rejected at standard levels, except for the bonus and compensation regressions, where it is rejected at the 0.01 and 0.05 levels, respectively.

To provide more information on nonstationarities, we test the stationarity of each of the five coefficients (coefficients of regulation, A/V, log of real sales, accounting return and intercept) separately. Regressions are run restricting the particular coefficient whose stationarity is being tested to be constant and allowing all other coefficients and the intercept to vary over time (by use of year dummies). The sums of squared errors from those regressions are compared with the sums of squared errors from separate regressions for each year. The hypothesis that the coefficient is stationary over time cannot be rejected at any reasonable probability level for any coefficient in any of the five regressions.

The rejection of stationarity of all coefficients for some regressions and the failure to reject stationarity for any single coefficient suggest that the rejections in the first test are due to joint effects. To test that hypothesis, we run regressions restricting the intercept and all but one coefficient to be constant over time and compare the regressions' sums of squared errors with the sums of squared errors of the individual-year regressions. The hypothesis that other coefficients are stationary can be rejected only for the bonus regression and the compensation regression when the regulation coefficient is allowed to vary (i.e., A/V and log-of-sales coefficients are constant). Thus it appears the rejection of the stationarity of all coefficients for the bonus and compensation regressions is due to the joint nonstationarity of the A/V and log-of-sales coefficients.

Generally, the estimated relations are stationary over time. The financial-policy, dividend-policy, and stock-option regressions show no significant non-stationarity. These equations also have the most explanatory power (see table 1) and are among the most robust to other specification checks. Also, one of the two regressions showing some evidence of nonstationarity is the bonus regression, where the sign of the predicted relation with A/V is ambiguous.

Dependence in corporate policies. Positive dependence in corporate policies would cause the standard errors of the coefficients in the pooled cross-section and time-series regressions to be understated and the t-statistics overstated. To assess the effect of this dependence on the significance of the estimated coefficients in table 1 we estimate the variance–covariance matrices for given policies within each industry and use those estimated matrices in a generalized-least-squares estimation [see Froot (1989)].

The results for the generalized-least-squares estimation are similar to those in table 1. As in table 1, each slope coefficient is significant. The significance levels for the coefficients of A/V, log of real sales, and accounting return are of a similar magnitude, but the levels for the regulation coefficients are less significant; for example, the t-statistic for the regulation coefficient in the equity/value regression is -4.04 versus -12.33 in table 1. This latter result is to be expected since industries are classified as regulated or unregulated for the entire estimation period.

Further insight into the significance of the coefficients in table 1 is obtained by regressing the mean policy variable on the mean independent variables for the entire 1965–1985 period. This regression examines cross-sectional variation and ignores time-series variation. The regulation coefficients are less significant in these regressions just as in the generalized-least-squares estimation. However, the other slope coefficients are also less significant in this specification than in the generalized-least-squares and pooled regressions (table 1); for example, the t-statistic for the A/V coefficient in the equity/value regression is -3.49 versus -10.29 and -12.47 in the generalized-least-squares and pooled regressions, respectively. This suggests that a substantial part of the table 1 explanatory power of A/V, log of real sales, and accounting return comes from their (nondependent) times-series variation.

The previous inference is confirmed by a fixed-effects analysis [see Hsiao (1986, ch. 3)]. The time-series mean for the industry is deducted from each variable and the policy regressions estimated with the transformed variables. The effect of this transformation is to eliminate the individual industry effect that is constant across time and estimate the independent variable coefficients solely on the basis of the within-industry (time-series) variation. Because the regulatory variables are constant across time, their estimated coefficients in the fixed-effect analysis are zero. The significance of the coefficients of log of real sales and accounting return is similar to that in the pooled regression (table 1) and in the generalized-least-squares estimation. However, the significance of the A/V coefficient increases in all policy regressions; for example, the t-statistic for the A/V coefficient in the equity/value regression in table 1 is -12.47 and in the fixed-effects analysis it is -16.13.

Our analysis therefore indicates positive dependence in corporate policies in the pooled cross-section and time-series regressions reported in table 1 is a problem for the estimation of the regulation coefficients only. Even so, the regulation coefficients remain significant when they are estimated using cross-sectional and not time-series variation.

On compensation differentials for risk. An alternate interpretation of our evidence is that risk considerations, rather than investment-opportunity-set characteristics, drive our results. Managerial risk aversion would imply that managers of high-risk projects receive higher compensation. The predicted impact on the use of incentive compensation plans is less clear; it ultimately should depend on the proportion of controllable and uncontrollable risk. But if more total risk tends to be associated with more controllable risk, the use of incentive compensation plans should increase. The simple bankruptcy-cost theory of capital structure suggests that more volatile firms should use less debt. We can find no analysis, however, to link volatility and dividend policy.

To attempt to distinguish between the investment-opportunity-set and volatility hypotheses, we add VAR to our benchmark regression. In all five regressions, the size and significance of the benchmark regression coefficients remain, but VAR is insignificant. Although insignificant, the signs of the VAR coefficients are the same as those reported in table A.2, where A/V was omitted. From this evidence, we conclude that our results are not driven simply by volatility. However, we recognize that this test faces potential problems: (1) A/V and VAR are correlated. Using them in the same regression makes it difficult to separately identify their effects because of multicollinearity. (2) We know that A/V is a noisy instrumental variable for the investment opportunity set. We also know that VAR is a noisy instrumental variable for risk. Thus the regression results reflect the correlations among the true variables, but also correlations among the error components.

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