THE
QUARTERLY JOURNAL
OF ECONOMICS

Vol. CXVIII November 2003 Issue 4

MANAGING WITH STYLE: THE EFFECT OF MANAGERS ON FIRM POLICIES*

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This paper investigates whether and how individual managers affect corporate behavior and performance. We construct a manager-firm matched panel data set which enables us to track the top managers across different firms over time. We find that manager fixed effects matter for a wide range of corporate decisions. A significant extent of the heterogeneity in investment, financial, and organizational practices of firms can be explained by the presence of manager fixed effects. We identify specific patterns in managerial decision-making that appear to indicate general differences in "style" across managers. Moreover, we show that management style is significantly related to manager fixed effects in performance and that managers with higher performance fixed effects receive higher compensation and are more likely to be found in better governed firms. In a final step, we tie back these findings to observable managerial characteristics. We find that executives from earlier birth cohorts appear on average to be more conservative; on the other hand, managers who hold an MBA degree seem to follow on average more aggressive strategies.

I. INTRODUCTION

"In the old days I would have said it was capital, history, the name of the bank. Garbage—it's about the guy at the top. I am very much a process

* We thank the editors (Lawrence Katz and Edward Glaeser), three anonymous referees, Kent Daniel, Rebecca Henderson, Steven Kaplan, Kevin J. Murphy, Sendhil Mullainathan, Canice Prendergast, David Scharfstein, Jerry Warner, Michael Weisbach, seminar participants at Harvard University, the Kellogg Graduate School of Management at Northwestern University, the Massachusetts Institute of Technology, the University of Chicago Graduate School of Business, the University of Illinois at Urbana-Champaign, Rochester University, and the Stockholm School of Economics for many helpful comments. We thank Kevin J. Murphy and Robert Parrino for generously providing us with their data. Jennifer Fiumara and Michael McDonald provided excellent research assistance.
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The Quarterly Journal of Economics, November 2003

person, a builder. Sandy [Weil] is an acquirer. Just totally different." —John Reed, CEO Citicorp

How much do individual managers matter for firm behavior and economic performance? Research in finance and economics so far has given little consideration to this question.[1] Existing empirical studies typically rely on firm-, industry-, or market-level characteristics to explain corporate behavior and performance but largely ignore the possible role that individual managers may play in shaping these outcomes. Yet, a prevailing view in the business press and among managers themselves (as the quote by John Reed at the beginning of the paper suggests) is that CEOs and other top executives are key factors in the determination of corporate practices. Managers are often perceived as having their own "styles" when making investment, financing, and other strategic decisions, thereby imprinting their personal marks on the companies they manage.[2] The novel contribution of this paper is to explicitly introduce such a people, or manager, dimension in an empirical study of corporate practices.[3]

The relevance of this approach is further underlined when we consider the large heterogeneity in corporate practices that is left unexplained by more standard models that rely only on firm- and industry-level factors. For example, research on the cross-sectional determinants of capital structure (e.g., Titman and Wessels [1988], Smith and Watts [1992], and Bradley, Jarrell, and Kim [1984]) shows that a large amount of variation remains unexplained after controlling for firm-level characteristics (such as market-to-book ratios, the type of assets a firm operates or

1. A few recent exceptions in the theory literature are papers by Rotemberg and Saloner [2000] and Van den Steen [2002]. These papers explicitly model the vision of the CEO as an important determinant of firm policy.

2. To mention just one example, an article in a May 2001 issue of *Business Week,* titled "The Koszlowski Method," discusses the aggressive acquisition style of Dennis Koszlowski, the CEO of Tyco.

3. While the role of managers in shaping corporate practices has been virtually ignored in the economics and finance literature, there is a large body of work in the management science literature analyzing the determinants of decision-making among CEOs (see, for example, Hambrick and Mason [1984] or Waldman, Ramirez, House, and Puranam [2001]). Yet, both the specific focus of this literature and the methodological approach it follows differ substantially from the study we propose to undertake here. First, the outcome variables considered in the management literature are mostly process-related variables (e.g., communication process or charisma) rather than the actual economic outcomes we care about here. Second, most of the existing work in management science relies on case studies, laboratory experiments, or subjective survey responses, therefore lacking the level of generality of our approach. A paper that follows an empirical approach more closely related to ours is Lieberman, Lau, and Williams [1990], who find significant manager fixed effects in productivity in the U. S. and Japanese automobile industry.

nondebt tax shields) or industry fixed effects.[4] In a similar vein, the ongoing debate about differences in investment to cash flow and investment to Q sensitivities [Fazzari, Hubbard, and Petersen 1988; Kaplan and Zingales 1997] highlights the considerable disagreement as to the roots of the wide variation in investment behavior across firms. One primary objective of this paper is to ask whether managers' personalities, as opposed to firm, industry, or market factors, can in part account for these unexplained differences.

Intuitively, we want to quantify how much of the observed variation in firm policies can be attributed to manager fixed effects. Since manager effects might be correlated with other *firm-specific* characteristics, we estimate the role of managers in a framework where we can control for observable and unobservable differences across firms. For this purpose, we construct a manager-firm matched panel data set, where we track individual top managers across different firms over time. This allows us to estimate how much of the unexplained variation in firm practices can be attributed to manager fixed effects, after controlling for firm fixed effects and time-varying firm characteristics.[5]

The specific corporate variables we study relate to investment policy (capital expenditures, investment to Q sensitivity, investment to cash flow sensitivity, and acquisition policy), financial policy (financial leverage, interest coverage, cash holdings, and dividend payouts), organizational strategy (R&D expenditures, advertising expenditures, diversification policy, and cost-cutting policy), and performance.[6]

Our results show that manager fixed effects are empirically important determinants of a wide range of corporate variables. On average, adding the fixed effects to models of corporate practices that already account for observable and unobservable firm characteristics results in increases in adjusted R^2's of more than four percentage points. More interestingly, we find that manager

4. For a recent study of intraindustry variation in leverage, see MacKay and Phillips [2002].

5. A few recent papers relate managerial characteristics to firm performance and investment. See, for example, Malmendier and Tate [2002] and Wasserman, Nohria, and Anand [2002]. However, these papers do not control for firm fixed effects and therefore cannot separate manager effects from firm effects. In a more recent paper Malmendier and Tate [2003] use a methodology more similar to ours. They track switchers across firms to study the effect of managerial overconfidence on acquisition behavior.

6. The fixed effects approach used in this analysis intends to measure whether there is persistence of managerial style over time and across different jobs. This is the very definition of "style" used in this paper. But we do not want to rule out that managers may learn or develop their style over time.

effects matter much more for some decisions than others. Manager fixed effects appear to be especially important in acquisition or diversification decisions, dividend policy, interest coverage, and cost-cutting policy.

By correlating these estimated manager fixed effects across different corporate variables, we are also able to identify some overarching patterns in managerial decision-making. Among other things, we find that managers seem to differ in their approach toward company growth and in their financial aggressiveness. Managers who engage in more external acquisitions and diversification also display lower levels of capital expenditures and R&D. We also find that managers who have high investment to Q fixed effects rank lower in their investment to cash flow sensitivity (and vice versa), suggesting that managers may differ, all else equal, in the benchmark that they use when making investment decisions.

These results provide evidence that top executives vary considerably in their management "styles" and thereby suggest a rather novel approach for corporate finance research. Yet, they also raise questions as to why managers may behave so differently in apparently similar economic environments. Do these findings reflect differences in preferences, absolute or relative skills, or opinions? More importantly, what are the efficiency implications of these findings? While these questions outline clear directions for future work, we provide some preliminary evidence on some of these issues. First, we show that the differences in managerial practices documented above are systematically related to differences in performance. More precisely, we show that there are significant managerial fixed effects in performance and these effects are statistically related to some of the fixed effects in corporate practices. For example, managers who are more investment-Q sensitive, and have higher administrative expenses, and are less active in the acquisition and diversification markets also have lower performance fixed effects. In addition, we show that managers with higher performance fixed effects also receive higher salary and total compensation and that these managers are more likely to be found in better governed firms. These results are suggestive of possibly important efficiency implications of our findings.

In a final step, we tie back differences in style to observable managerial characteristics. The two characteristics we consider are birth cohort and MBA graduation. We analyze the extent to

which corporate decisions are affected by these two characteristics, after controlling for any fixed differences across firms and other time-varying firm factors. We find that older generations of CEOs appear overall more conservative in their decision-making. On the other hand, managers who hold an MBA degree appear overall to follow more aggressive strategies.

The rest of this paper is organized as follows. Section II provides a brief discussion of alternative hypotheses as to why individual managers may matter for corporate decisions. Section III presents the different data sources, describes the construction of the data set, and defines the main variables of interest. Section IV quantifies the importance of manager fixed effects for various corporate practices, and Section V discusses possible efficiency implications of these findings. Section VI studies birth cohort and MBA graduation as two specific determinants of managerial style. Section VII summarizes and offers some concluding remarks.

II. Why Should Individual Managers Matter?

Many empirical studies of corporate decisions implicitly assume a neoclassical view of the firm in which top managers are homogeneous and selfless inputs into the production process. Under this quite narrow view, different managers are regarded as perfect substitutes for one another. An even more extreme assumption is that top managers simply do not matter for what is going on within a firm. While executives might differ in their preferences, risk-aversion or skill levels, none of this translates into actual corporate policies, if a single person cannot easily affect these policies. Under either of these scenarios, we would not expect individual managers to matter for corporate decisions. Two firms sharing similar technologies, factor, and product market conditions will make similar choices, whether or not they also share the same management team.

In contrast, standard agency models acknowledge that managers may have discretion inside their firm, which they can use to alter corporate decisions and advance their own objectives. However, these models do not generally imply that corporate behavior will vary with individual managers, as they typically do not focus on idiosyncratic differences across managers. Rather, agency models attribute variations in corporate behavior to heterogene-

ity in the strength of governance mechanisms across firms, i.e., heterogeneity in firms' ability to control managers.[7]

Heterogeneity in corporate practices across managers will arise in models that explicitly allow managers to differ in their preferences, risk aversion, skill levels, or opinions. But there are two distinct interpretations as to how these managerial differences translate into corporate choices. The first are extensions of the standard agency models in which a manager can impose his or her own idiosyncratic style on a company, if corporate control is poor or limited. Under this view, one might expect that the impact of managers to increase as the sources of internal and external controls weaken. Alternatively, if some management styles are more performance-enhancing than others, better governed firms may be more likely to select managers with such styles.

A second set of models that imply manager-specific effects in corporate practices are extensions of the neoclassical model in which managers vary in their match quality with firms. In this case, managers do not impose their idiosyncratic style on the firm they lead, but are purposefully chosen by firms *because of* these specific attributes. For example, a board may determine the need to go through an external growth phase and therefore hire a new manager who is more aggressive or more prone to engage in expansion strategies.[8] Under this interpretation and given the empirical framework we develop below, we would only find significant manager effects in corporate practices if firms' optimal strategies change over time. Indeed, if a given company's optimal strategy were invariant over time, an incoming manager's style would only be the continuation of the prior manager's style.

These two main variants of the "managers matter" view of corporate decisions have very different efficiency implications. Under the first interpretation, some managerial traits or preferences may cause corporations to adopt suboptimal strategies. The extent to which this occurs will be limited by boards' ability to

7. One exception is Hermalin and Weisbach [1998], who model a process by which good managers can gain more discretion, which in turn allows them to change the governance relationship within their firm. Also, career concern models show that the intensity of the conflict of interest between managers and owners may vary over the life cycle of managers.

8. Alternatively, one could argue that boards systematically get fooled and *mistakenly* infer a manager's style based on the manager's prior job experience. A manager may by chance be involved in a wave of acquisitions in her or his prior firm, which may be wrongly perceived as an "acquisition style" and influence future hiring by other firms. We discuss this alternative view in more detail in subsection IV.C.

screen or monitor managers. Under the second interpretation, managerial differences in style will not lead to inefficiencies as long as boards optimally select the right manager for the right job. However, under either interpretation, individual managers are central in bringing about the changes in corporate policies.

While our primary goal in this paper is not to distinguish between these different interpretations but rather to first establish that individual managers do matter in the determination of firm policies, we will provide some preliminary evidence about possible efficiency implications of our findings in Section V.

III. Data

III.A. Sample Construction

A straightforward way to proceed when trying to determine whether there are systematic differences in the way top managers behave would be to ask whether there are important manager fixed effects in corporate practices, controlling for all relevant observable firm-level characteristics. One obvious problem with this approach is that there might be persistent differences in practices across firms due to some unobservable third factors and that these factors might be correlated with the manager fixed effects. Practically, this implies that one needs to separate manager fixed effects from firm fixed effects.

We therefore construct a manager-firm matched panel data set that allows us to track the same top managers across different firms over time. The data we use are the Forbes 800 files, from 1969 to 1999, and Execucomp data, from 1992 to 1999. The Forbes data provide information on the CEOs of the 800 largest U. S. firms. Execucomp allows us to track the names of the top five highest paid executives in 1500 publicly traded U. S. firms. These include the CEO, but also other top executives, most often the CFO, COO, and subdivision CEOs.[9] We then restrict our attention to the subset of firms for which at least one specific top executive can be observed in at least one other firm.[10] In doing so, we also impose that the managers have to be in each firm for at least three years.[11] For each firm satisfying these requirements,

9. We use the variable *titlean* in Execucomp to code the specific position of a manager in a given firm.

10. We discuss below (subsection IV.A) the possible selection issues associated with this sample construction.

11. This three-year requirement ensures that managers are given a chance to "imprint their mark" in a given company. All of the results below were replicated

we keep all observations, i.e., including years where the firm has managers that we do not observe in multiple firms. The resulting sample contains about 600 firms and slightly over 500 individual managers who can be followed in at least two different firms.[12] The average length of stay of a manager within a given firm is a little over five years in our data. As is customary in the study of investment regressions, we exclude firms in the banking and insurance industries as well as utilities from our sample. To preserve consistency across results, we also exclude these firms in the analysis of noninvestment variables.[13]

For this sample of firms, we use COMPUSTAT and SDC data to construct a series of annual accounting variables. We concentrate our analysis on three different sets of corporate decisions (investment policy, financial policy, and organizational strategy) as well as on corporate performance. The definition and construction of the specific variables used in the analysis are reported in the Data Appendix.

III.B. Sample Description

Table I presents means and standard deviations for all the corporate variables of interest. The first two columns report summary statistics for the manager-firm matched sample. For comparison, the last two columns of Table I report equivalent summary statistics for the entire COMPUSTAT sample between 1969 and 1999. As expected, constraining our sample to firms where we can observe at least one executive switch leads us to select larger firms. Indeed, executives from larger firms are more likely to move between COMPUSTAT firms. Executives from smaller firms, on the other hand, might have a higher probability to move to private firms or positions within large firms that are below the top five level. Such executives cannot be tracked in our data sources.[14] The average firm in our sample also has a somewhat higher Tobin's Q ratio, higher rate of return on assets, and higher

ignoring this three-year requirement in the sample construction. The results we obtained were qualitatively similar but, not surprisingly, statistically weaker.

12. A very small subset of managers are observed in strictly more than two different firms.

13. When we include these observations in the noninvestment regressions, our results are virtually unchanged.

14. One could argue that this required focus on larger firms may in fact bias our results against finding systematic effects of managers on firm policies. Indeed, a specific individual might be more influential in a smaller organization that requires more personal involvement of the top managers in day-to-day activities. An alternative argument would be that managers who have more distinct "styles" are more likely to be found in larger firms.

TABLE I
DESCRIPTIVE STATISTICS

	Manager-firm matched sample		Manager characteristics sample		Compustat	
	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.
Total sales	5606.5	11545.6	5333.3	10777.4	2649.6	5878.2
Investment	0.39	2.94	0.28	0.50	0.34	2.67
Average Tobin's *Q*	2.40	3.85	2.03	2.05	1.70	1.43
Cash flow	0.44	1.91	0.45	2.10	0.43	2.47
N of acquisitions	0.77	1.48	0.65	1.40	0.36	1.45
Leverage	0.35	0.39	0.34	0.28	0.45	1.21
Interest coverage	35.0	875.1	40.5	663.1	27.6	166.2
Cash holdings	0.11	0.16	0.08	0.11	0.17	0.80
Dividends/earnings	0.11	0.79	0.14	1.05	0.16	0.25
N of diversifying acquisitions	0.32	1.09	0.28	0.91	0.12	0.63
R&D	0.05	0.07	0.04	0.14	0.03	0.06
Advertising	0.05	0.06	0.05	0.06	0.04	0.06
SG&A	0.26	0.98	0.21	0.19	0.18	0.64
Return on assets	0.16	0.11	0.19	0.15	0.10	0.09
Operating return on assets	0.09	0.12	0.11	0.22	0.08	0.13
Sample size	6766		10472		38489	

a. "Manager-firm matched sample" refers to the set of firm-year observations for firms that have at least one manager observed in multiple firms with at least a three-year stay at each firm. This sample includes observations for these firms in the years in which they have other managers that we do not observe in multiple firms (see subsection III.A for details). "Manager characteristics sample" refers to the set of firm-year observations for which we can obtain information on the year of birth and educational background of the CEO (see subsection VI.A for details). "Compustat" is a comparison sample of the 1500 largest listed firms over the period 1969 to 1999. All samples exclude firms in the banking and insurance industry, as well as regulated industries.

b. Details on the definition and construction of the variables reported in the table are available in the Data Appendix.

c. Total sales are expressed in 1990 dollars.

d. Sample size refers to the maximum number of observations; not all variables are available for each year and firm.

number of acquisitions, but slightly lower cash holdings and leverage levels. It is, however, very similar to the average COMPUSTAT firm with respect to cash flow, investment levels, dividend payouts, R&D, and SG&A.

Table II tabulates the nature of the executive transitions in our sample. We separate three major executive categories: CEOs, CFOs, and "Others." The majority of the job titles in this "Others" category correspond to operationally important positions: 44 percent are subdivision CEOs or Presidents, 16 percent are Executive Vice-Presidents, and 12 percent are COOs; the rest are Vice-Presidents and other more generic titles.

TABLE II
EXECUTIVE TRANSITIONS BETWEEN POSITIONS AND INDUSTRIES

to:	CEO	CFO	Other
from:			
CEO	117	4	52
	63%	75%	69%
CFO	7	58	30
	71%	71%	57%
Other	106	0	145
	60%		42%

a. This table summarizes executives' transitions across positions and industries in the manager-firm matched panel data set (as described in subsection III.A and Table I). All transitions are across firms. The first entry in each cell reports the number of transitions from the row position to the column position. The second line in each cell reports the fraction of the transitions in that cell that are between different two-digit industries.

b. "Other" refers to any job title other than CEO or CFO.

Of the set of about 500 managers identified in our sample, 117 are individuals who move from a CEO position in one firm to a CEO position in another firm; 4 are CEOs who move to CFO positions; and 52 are CEOs who move to other top positions. Among the set of executives starting as CFOs, we observe 7 becoming CEOs, 58 moving to another CFO position, and 30 moving to other top positions. Finally, among the 251 managers who start in another top position, 106 become CEOs, and 145 move to another non-CEO, non-CFO position. Within this latter group we found that more than 40 percent of the transitions are moves from a position of subdivision CEO or subdivision president in one firm to a similar position in another firm.

In the second row of each cell in Table II, we report the fraction of moves that are between firms in different two-digit industries.[15] It is interesting to note that a large fraction of the executive moves in our sample are between industries. For example, 63 percent of the CEO to CEO moves are across different two-digit industries, as are 71 percent of the CFO to CFO moves. A relatively lower fraction of the moves from other top positions to other top positions (42 percent) are across industries. These patterns seem intuitive if ones believes that CEOs and CFOs need relatively less industry and firm-specific knowledge and instead rely more on general management skills.[16]

15. The industry classification is based on the primary SIC code of each firm, as reported in COMPUSTAT.

16. See, for example, Fligstein [1990] for a discussion of this argument.

IV. Is There Heterogeneity in Executive Practices?

IV.A. Empirical Methodology

The nature of our identification strategy can be most easily explained with an example. Consider the dividend payout ratio as the corporate policy of interest. From a benchmark specification we derive residual dividend payouts at the firm-year level after controlling for any average differences across firms and years as well as for any firm-year specific shock, such as an earnings shock, that might affect the dividend payout of a firm. We then ask how much of the variance in these residual dividend payouts can be attributed to manager-specific effects.

More specifically, for each dependent variable of interest, we propose to estimate the following regression:

$$(1) \qquad y_{it} = \alpha_t + \gamma_i + \beta X_{it} + \theta_{CEO} + \theta_{CFO} + \theta_{Others} + \varepsilon_{it},$$

where y_{it} stands for one of the corporate policy variables, α_t are year fixed effects, γ_i are firm fixed effects, X_{it} represents a vector of time-varying firm level controls, and ε_{it} is an error term. The remaining variables in equation (1) are fixed effects for the managers that we observe in multiple firms. Because we want to separately study the effect of CEOs, CFOs, and other top executives on corporate policies, we create three different groups of manager fixed effects: θ_{CEO} are fixed effects for the group of managers who are CEOs in the last position we observe them in, θ_{CFO} are fixed effects for the group of managers who are CFOs in the last position we observe them in, and θ_{Others} are fixed effects for the group of managers who are neither CEOs nor CFOs in the last position we observe them in.[17] Finally, when estimating equation (1), we account for serial correlation by allowing for clustering of the error term at the firm level.[18]

It is evident from equation (1) that the estimation of the manager fixed effects is not possible for managers who never leave a given company during our sample period. Consider, for example, a specific manager who never switches companies and advances only through internal promotions, maybe moving from

17. We also repeated all of the analyses below after separating CEO to CEO moves, CEO to CFO moves, etc. The results were qualitatively similar to the more aggregated results reported in the paper.

18. In subsection IV.C we propose two alternative estimation methods to deal with serial correlation issues and better address possible issues regarding the persistence of the manager fixed effects.

a CFO to a CEO position in his/her firm. The effect of this manager on corporate practices cannot be estimated separately from his firm fixed effect. The manager fixed effect and the firm fixed effect are perfectly collinear in this case. It would be statistically possible to extend our analysis to top managers whom we observe in one firm but who stay in that firm for only a subset of the entire sample period. To be conservative in our estimation, however, we decided to stay away from this approach. Indeed, the fixed effects for such managers correspond to period-firm-specific effects, which could be more easily attributed to other unobservable time-varying factors. Instead, for manager fixed effects to matter under our more stringent approach, we require that corporate practices have to be correlated across (at least) two firms when the same manager is present.[19]

While the discussion above clarifies why our identification relies solely on outside hires, let us highlight possible implications of this sample selection for more general inferences based on our results. First, it is useful to note that the outside hire of top executives, and especially of CEOs, is far from exceptional among the large U. S. public firms that we focus on in this analysis.[20] Nevertheless, one could reasonably argue that managers who are recruited from the outside are different from internally promoted ones.[21] For example, one might argue that outside managers have "stronger" or "better" styles on average, as firms are willing to look outside their organization to find these managers.

Finally, and most importantly, there is no such thing as a random allocation of top executives to firms. Therefore, we are not hoping in this section to estimate the *causal* effect of managers on firm practices. Instead, our objective is more modest. We want to assess whether there is any evidence that firm policies systematically change with the identity of the top managers in these firms.

19. For the sake of completeness, we replicate our results under this alternative approach, thereby covering a much larger set of executives. As one might have expected, we find even stronger manager fixed effects.

20. We use the entire Execucomp sample to compute the fraction of CEOs who were hired from the outside rather than internally promoted. We find that only 48 percent are internally promoted. In a more detailed study, Parrino [1997] shows that the prevalence of inside versus outside succession varies a lot by industry.

21. Suggestive evidence for this seems to emerge from a set of papers looking at stock market responses to the announcement of management turnover. For example, Warner, Watts, and Wruck [1988] document abnormally high returns around outsider succession events, but no significant overall effect.

IV.B. Results

Tables III and IV report F-tests and adjusted R^2 from the estimation of equation (1) for the different sets of corporate policy variables. For each variable we report in the first row the fit of a benchmark specification that includes only firm fixed effects, year fixed effects, and time-varying firm controls. The next two rows, respectively, report the change in adjusted R^2 when we consecutively add the CEO fixed effects and the fixed effects for all three groups of executives (CEOs, CFOs, and other top positions). The second and third rows also report F-statistics from tests of the joint significance of the different sets of manager fixed effects.

Overall, the findings in Tables III and IV suggest that manager-specific effects matter both economically and statistically for the policy decisions of firms. Including CEOs as well as other managers' fixed effects increases the adjusted R^2 of the estimated models significantly. Similarly, we find that the F-tests are large and allow us to reject in most cases the null hypothesis that all the manager fixed effects are zero. We also see that there are important differences as to which decision variables seem to be most affected by manager decisions. Moreover, different types of manager matters for different decisions; e.g., CFOs matter more for financial decisions. We now discuss these results in greater details.

Table III reports our results for investment policy (Panel A) and financial policy (Panel B). We start with a discussion of the investment results. The first variable in this table is capital expenditures (as a fraction of lagged net property, plant, and equipment). The benchmark specification includes controls for firm fixed effects, year fixed effects, cash flow, lagged Tobin's Q, and the lagged logarithm of total assets. The adjusted R^2 for this specification is 91 percent. Even though the fit of this benchmark model is already very high, the adjusted R^2 increases by 3 percent when we include the CEO fixed effects and by more than 5 percent when we include all sets of manager fixed effects. Also, the F-tests are large, leading us to reject the null hypothesis of no joint effect in all cases.

The next two variables are investment to Tobin's Q and investment to cash flow sensitivities, respectively. The estimation method for these two variables differs slightly from the one described in subsection IV.A. Indeed, the fixed effects of interest here do not relate to the level of a given variable (in this case, investment), but rather to the sensitivity of that variable to

TABLE III
EXECUTIVE EFFECTS ON INVESTMENT AND FINANCIAL POLICIES

Panel A: Investment policy

	F-tests on fixed effects for				
	CEOs	*CFOs*	*Other executives*	*N*	*Adjusted* R^2
Investment				6631	.91
Investment	16.74 (, .0001, 198)			6631	.94
Investment	19.39 (, .0001, 192)	53.48 (, .0001, 55)	8.45 (, .0001, 200)	6631	.96
Inv to Q sensitivity				6631	.95
Inv to Q sensitivity	17.87 (, .0001, 223)			6631	.97
Inv to Q sensitivity	5.33 (, .0001, 221)	9.40 (, .0001, 58)	20.29 (, .0001, 208)	6631	.98
Inv to CF sensitivity				6631	.97
Inv to CF sensitivity	2.00 (, .0001, 205)			6631	.98
Inv to CF sensitivity	0.94 (.7276, 194)	1.29 (.0760, 55)	1.28 (.0058, 199)	6631	.98
N of acquisitions				6593	.25
N of acquisitions	2.01 (, .0001, 204)			6593	.28
N of acquisitions	1.68 (, .0001, 199)	1.74 (.0006, 55)	4.08 (, .0001, 203)	6593	.36

Panel B: Financial policy

	F-tests on fixed effects for				
	CEOs	*CFOs*	*Other executives*	*N*	*Adjusted* R^2
Leverage				6563	.39
Leverage	0.99 (.5294, 203)			6563	.39
Leverage	0.86 (.9190, 199)	1.43 (.0225, 54)	1.21 (.0230, 203)	6563	.41
Interest coverage				6278	.31
Interest coverage	0.56 (.99, 193)			6278	.31
Interest coverage	0.35 (.99, 192)	13.85 (, .0001, 50)	2.61 (, .0001, 192)	6278	.41
Cash holdings				6592	.77
Cash holdings	2.52 (, .0001, 204)			6592	.78
Cash holdings	2.48 (, .0001, 201)	3.68 (, .0001, 54)	2.53 (, .0001, 202)	6592	.80
Dividends/earnings				6580	.65
Dividends/earnings	5.78 (, .0001, 203)			6580	.71
Dividends/earnings	4.95 (, .0001, 199)	1.07 (.3368, 54)	1.74 (, .0001, 203)	6580	.72

a. Sample is the manager-firm matched panel data set as described in subsection III.A and Table I. Details on the definition and construction of the variables reported in the table are available in the Data Appendix.

b. Reported in the table are the results from fixed effects panel regressions, where standard errors are clustered at the firm level. For each dependent variable (as reported in column 1), the fixed effects included are row 1: firm and year fixed effects; row 2: firm, year, and CEO fixed effects; row 3: firm, year, CEO, CFO, and other executives fixed effects. Included in the "Investment to Q" and "Investment to cash flow" regressions are interactions of these fixed effects with lagged Tobin's Q and cash flow, respectively. Also the "Investment," "Investment to Q," and "Investment to cash flow" regressions include lagged logarithm of total assets, lagged Tobin's Q, and cash flow. The "Number of Acquisitions" regressions include lagged logarithm of total assets and return on assets. Each regression in Panel B contains return on assets, cash flow, and the lagged logarithm of total assets.

c. Reported are the F-tests for the joint significance of the CEO fixed effects (column 2), CFO fixed effects (column 3), and other executives fixed effects (column 4). For each F-test we report the value of the F-statistic, the p-value, and the number of constraints. For the "Investment to Q" and "Investment to Cash Flow" regressions, the F-tests are for the joint significance of the interactions between the manager fixed effects and Tobin's Q and cash flow, respectively. Column 5 reports the number of observations, and column 6 the adjusted R^2s for each regression.

Tobin's Q and cash flow. In practice, for investment to Q sensitivity, we start by regressing investment on year fixed effects, cash flow, lagged Tobin' Q, the lagged logarithm of total assets, firm fixed effects, and firm fixed effects interacted with lagged Tobin's Q. We then add to this benchmark specification manager fixed effects as well as manager fixed effects interacted with lagged Tobin's Q. The estimated coefficients of interest are those on the interaction terms. We proceed in a similar fashion in our study of investment to cash flow sensitivity. The results indicate increases in adjusted R^2 when including the interaction terms of manager fixed effects with cash flow and lagged Tobin's Q, especially for investment to Q sensitivity. The adjusted R^2 goes up from 95 percent to 98 percent when we allow investment to Q to be manager specific.

The last variable in Panel A is number of acquisitions. For this variable we observe an increase in adjusted R^2 of about 11 percent following the inclusion of the manager fixed effects. Maybe surprisingly, we find that the fixed effects for the "Other" managers are very significant and that their inclusion has an especially large impact on the adjusted R^2. In regressions not reported here we broke down the set of other managers into more specific job title categories. We found that the subdivision CEOs and COOs explain most of the increase in adjusted R^2 within this "Other" category.

Panel B of Table III focuses on financial policy. Included in all regressions are firm fixed effects, year fixed effects, the lagged logarithm of total assets, and the rate of return on assets.[22] Overall, the increases in adjusted R^2 in this Panel are of a similar order of magnitude as those found for the investment variables. The adjusted R^2 of the leverage regression increases from 39 percent to 41 percent when we include the manager fixed effects. The adjusted R^2 of the interest coverage regression, an alternative measure of capital structure, increases by as much as 10 percent (from 31 percent to 41 percent). Interestingly, CFOs have the strongest effect on interest coverage, a key financial indicator. The adjusted R^2 of the cash holdings regression goes up by 3 percent, from 77 percent to 80 percent, when we compare the benchmark specification with the specification that includes all manager fixed effects. Finally, managers appear to be important determinants of dividend policy, with an overall increase in ad-

22. We also experimented with adding controls for assets uniqueness and tax advantage from debt in the leverage regressions. The results were unaffected.

TABLE IV
EXECUTIVE EFFECTS ON ORGANIZATIONAL STRATEGY AND PERFORMANCE

	Panel A: Organizational strategy *F-tests on fixed effects for*				
	CEOs	*CFOs*	*Other executives*	*N*	*Adjusted* R^2
N of diversifying acquis.				6593	.22
N of diversifying acquis.	2.06 (, .0001, 204)			6593	.25
N of diversifying acquis.	1.23 (.0163, 202)	1.74 (.0007, 53)	3.97 (, .0001, 202)	6593	.33
R&D				4283	.78
R&D	1.86 (, .0001, 145)			4283	.79
R&D	2.27 (, .0001, 143)	3.60 (, .0001, 45)	4.46 (, .0001, 143)	4283	.83
Advertising				2584	.79
Advertising	2.88 (, .0001, 95)			2584	.81
Advertising	4.03 (, .0001, 95)	0.84 (.6665, 21)	6.10 (, .0001, 80)	2584	.84
SG&A				2397	.46
SG&A	33.55 (, .0001, 123)			2397	.83
SG&A	13.80 (, .0001, 118)	0.82 (.7934, 42)	0.77 (.9777, 146)	2397	.83

TABLE IV
(CONTINUED)

	Panel B: Performance F-tests on fixed effects for				
	CEOs	*CFOs*	*Other executives*	*N*	*Adjusted* R^2
Return on assets				6593	.72
Return on assets	2.04 (, .0001, 217)			6593	.74
Return on assets	2.46 (, .0001, 201)	3.39 (, .0001, 54)	4.46 (, .0001, 202)	6593	.77
Operating return on assets				5135	.34
Operating return on assets	2.61 (, .0001, 217)			5135	.39
Operating return on assets	1.60 (, .0001, 216)	0.66 (.9788, 58)	1.01 (.4536, 217)	5135	.39

a. Sample is the manager-firm matched panel data set as described in subsection III.A and Table I. Details on the definition and construction of the variables reported in the table are available in the Data Appendix.

b. Reported in the table are the results from fixed effects panel regressions, where standard errors are clustered at the firm level. For each dependent variable (as reported in column 1) the fixed effects included are row 1: firm and year fixed effects; row 2: firm, year, and CEO fixed effects; row 3: firm, year, CEO, CFO, and other executives fixed effects.

c. Also included in the "N of diversifying acquisitions," "R&D," "advertising," and "SG&A" regressions are the logarithm of total assets, return on assets, and cash flow. The "N of diversifying acquisitions" regressions also include a dummy variable for whether the firm undertook any acquisition in that year. Also included in the "Return on assets" and "Operating return on assets" regressions is the logarithm of total assets.

d. Reported in the table are F-tests for the joint significance of the CEO fixed effects (column 2), CFO fixed effects (column 3), and other executives fixed effects (column 4). For each F-test we report the value of the F-statistic and, in parentheses, the p-value and number of constraints. Also reported are the number of observations (column 5) and adjusted R^2s (column 6) for each regression.

justed R^2 of 7 percent. Moreover, we find that dividend policy seems to be more substantially affected by the CEOs than by the CFOs or other top executives.

Table IV reports our results for the organizational policy variables (Panel A) and for corporate performance (Panel B). Again, we find that top executives have large effects on the realization of these variables. The fit of the diversification regression improves by 11 percent.[23] The adjusted R^2s of the R&D and advertising regressions both increase by 5 percent. Finally, cost-cutting policy, as proxied by the ratio of SG&A to total sales, appears to systematically depend on the identity of the CEOs.[24] In line with a priori intuition we find that CEOs and other top managers seem to have larger effects on organizational strategy than CFOs do.

Finally, Panel B of Table IV focuses on two different measures of corporate performance. The first measure we consider is a standard rate of return on assets. Included in the benchmark specification here are firm fixed effects, year fixed effects, and the logarithm of total assets. Our results show that accounting performance varies significantly across top executives. The F-tests are large for all groups of managers, and the adjusted R^2 increases by more than 5 percent.

One possible concern with this latter finding is that the systematic differences in rate of return on assets across managers may not reflect actual differences in performance but rather differences in aggressiveness of accounting practices or willingness to "cook the books."[25] In order to address this concern, we use an alternative accounting measure of performance that is less subject to accounting manipulations and better captures true operating performance: operating cash flow (as a ratio of total assets). We find that this measure of operating performance also varies systematically across top managers. The F-tests on the CEO fixed effects are jointly significant and the increase in adjusted R^2 is nearly 6 percent. Interestingly, for this measure of performance, we cannot reject the null hypothesis that the fixed

23. In regressions not reported here we again broke down the set of other managers into more specific job title categories. We found that the subdivision CEOs and COOs explain most of the increase in adjusted R^2.

24. The regressions for advertising expenditures, R&D expenditures, and SG&A were estimated on a smaller sample due to the inconsistent availability of these variables in COMPUSTAT.

25. In an ongoing project, we are more systematically investigating the importance of manager fixed effects in accounting practices and how they relate to the results on real variables reported in this paper.

effects on the group of the CFOs and "Other" executives are all zeros.

IV.C. Robustness of Results

We conduct a series of specification checks to verify the robustness of the findings reported above. First, we replicate the analysis above after collapsing the data at the manager/firm level. This provides an alternative way to address possible serial correlation concerns. More specifically, starting with the firm-year data, we estimate firm-year residuals by regressing the policy variables of interest on firm fixed effects, year fixed effects, and the time-varying firm controls. We then collapse these annual residuals by manager-firm spell. Last, we reestimate the manager fixed effects in this collapsed data set. We find, in regressions not reported here, that our results are robust to this alternative estimation technique.

Second, one might worry that the manager fixed effects identified above do not imply persistence of managerial style across jobs and firms. For example, consider a manager who happens to be part of a firm during a period of intense acquisition activity; we might estimate a positive acquisition fixed effect for that manager even though that effect does not persist in his future firm. This concern is especially warranted for some of the lumpier policy variables covered in our analysis.

We address this concern in the first column of Table V. Here we use a more parametric specification to analyze the persistence in managerial styles. More specifically, for each policy variable, we construct manager-firm residuals as described above. We then regress a manager's average residual in his second firm on his average residual in the first firm we observe her/him in.[26] Reported in the first column of Table V are the estimated coefficients on the first firm residual for each of the corporate variables.

We find a positive and statistically significant relationship between a manager's residual in his last job and his residual in his first job for all the policy variables, with t-statistics varying between 4 and 16 and R^2 between 5 and 35 percent. Moreover, the estimated coefficients in these regressions are also economically very significant for most of the variables. For example, a top manager associated with 1 percent extra leverage in his first job is associated with about 0.5 percent extra leverage in his second

26. Note that we cannot directly perform this more parametric exercise for the investment to Q and investment to cash flow sensitivities.

TABLE V
PERSISTENCE OF MANAGER EFFECTS: REAL DATA AND PLACEBO DATA

	Real data	Placebo data
Investment	0.05	0.01
	(0.02)	(0.02)
	[0.01]	[0.00]
N of acquisitions	0.49	20.02
	(0.05)	(0.05)
	[0.13]	[0.00]
Leverage	0.40	0.02
	(0.03)	(0.05)
	[0.21]	[0.01]
Cash holdings	0.74	0.05
	(0.05)	(0.07)
	[0.35]	[0.01]
Dividends/earnings	0.80	0.06
	(0.04)	(0.12)
	[0.51]	[0.02]
N of diversifying acquis.	0.25	0.04
	(0.06)	(0.05)
	[0.07]	[0.00]
R&D	0.65	0.09
	(0.05)	(0.05)
	[0.33]	[0.02]
Advertising	0.62	0.11
	(0.08)	(0.06)
	[0.02]	[0.01]
SG&A	0.14	0.08
	(0.01)	(0.08)
	[0.03]	[0.02]
Return on assets	0.31	0.02
	(0.07)	(0.06)
	[0.40]	[0.01]
Operating return on assets	0.18	0.03
	(0.03)	(0.11)
	[0.07]	[0.00]

a. Sample is the manager-firm matched panel data set as described in subsection III.A and Table I. Details on the definition and construction of the variables reported in the table are available in the Data Appendix.

b. Each entry in this table corresponds to a different regression.

c. In column 1 we regress for each of the policy variables a manager's average residual in his second firm on his average residual in his first firm. In column 2 we regress for each of the policy variables a "manager's average residual" in his second firm *three years prior to the manager joining that firm* on his true average residual in his first firm. See subsection IV.C for details.

d. The first number in each cell is the estimated coefficient on the first job residual, the second number is the estimated standard error (in parentheses) and the third number is the estimated R^2 (in square brackets).

job. Moreover, corporate policies for which we find particularly strong manager fixed effects in Tables III and IV (such as acquisitions, diversification, dividend policy, or R&D) also prove to generate higher R^2 and larger coefficients in this more parametric setup. These results are consistent with a persistence of the manager fixed effects across firms.

Third, we want to argue that the manager fixed effects capture the *active* influence of managers on corporate decisions. There is, however, an alternative interpretation that is potentially consistent with our findings, but does not imply an active influence of managers on their companies. Suppose a model of the world where managers have no specific skills or styles but boards mistakenly believe otherwise. A manager may, by coincidence, be involved in a wave of acquisitions in her or his prior firm, and this may be wrongly perceived as an "acquisition style" by other boards. If this leads to the hiring of that manager by a firm that would have gone on an external expansion phase even without the manager, we might estimate a positive acquisition fixed effect for that manager.

To investigate this alternative interpretation, we analyze the precise timing of the observed changes in corporate policies. Under the story outlined above, we would not expect to find a precise overlap between the arrival of the new manager and the change in corporate practices. In fact, one might expect that some of the changes in policies actually *precede* the arrival of the new manager, as the board has already decided to undertake the changes. On the other hand, if managers do play the active role in shaping corporate policies, the changes in policy will only happen *after* the manager is hired.[27]

Practically, we construct average residuals in corporate policies as described above but now assume that each manager in our data set joins his second firm three years prior to the actual turnover date and leaves that firm at the time of the actual turnover date. In doing this, we are careful to censure the data for the second firms at the actual date of arrival of the new managers in these firms. We then regress these average pre-turnover residuals in the second firm on the true average residuals in the first firm.

Column 2 of Table V presents the results of this exercise. We

27. This test relies on a specific timing assumption. One could still argue that boards, although they do not need the manager to bring about any changes in corporate strategy, nevertheless only go ahead with the changes once the new manager arrives.

find that the estimated coefficients in these placebo regressions are economically very small compared with those in column 1. Most of the estimated coefficients are very close to zero, and all but two are statistically insignificant.[28] These results confirm that the bulk of the changes in corporate policy happen once the new manager has joined the firm and not prior to his arrival, suggesting an active role of the managers in implementing these changes.

IV.D. Magnitude of the Manager Fixed Effects

So far, we have seen that manager-specific effects explain a significant fraction of the variation in firm policies and outcomes. Additionally, we would like to assess how big the observed differences between managers are. Therefore, we look at the distributions of the fixed effects estimated above. For example, we can see how much extra leverage a manager in the upper tail of the leverage fixed effects distribution contributes, relative to a manager who is in the lower tail of that distribution. In Table VI we report the size distribution of the manager fixed effects for each of the regressions in Tables III and IV. We show median, standard deviation, twenty-fifth percentile, and seventy-fifth percentile. When computing these statistics, we weigh each fixed effect by the inverse of its standard error to account for estimation error.

Overall, Table VI shows that the variation in the size of the manager fixed effects is economically large. To highlight just a few examples, row 1 of Table VI shows that the difference between a manager at the twenty-fifth percentile of the distribution of investment level and one at the seventy-fifth percentile is 0.20. To give a benchmark, the average ratio of capital expenditures to assets in our sample is about 0.30. The difference between the twenty-fifth and seventy-fifth percentile in the leverage distribution is 0.16 (row 5), compared with an average leverage level of .34 in our sample. For acquisitions, we observe about 0.7 acquisitions per year for the firms in our sample. Row 4 of Table VI shows that a manager in the bottom quartile reduces the number of acquisitions by 20.49, while a manager in the top quartile increases the number of acquisitions by 0.44 per year. Finally, in the last row of Table VI we see that the variation in corporate performance fixed effects is also large. A manager in the top

28. We also repeated a similar exercise in the less parametric framework followed in Tables III and IV and obtained similar findings.

TABLE VI
SIZE DISTRIBUTION OF MANAGER FIXED EFFECTS

	Median	Standard deviation	25th percentile	75th percentile
Investment	0.00	2.80	20.09	0.11
Inv to *Q* sensitivity	20.02	0.66	20.16	0.12
Inv to CF sensitivity	0.04	1.01	20.17	0.28
N of acquisitions	20.04	1.50	20.54	0.41
Leverage	0.01	0.22	20.05	0.09
Interest coverage	0.00	860.0	256.0	51.7
Cash holdings	0.00	0.06	20.03	0.02
Dividends/earnings	20.01	0.59	20.13	0.11
N of diversifying acquis.	20.04	1.05	20.28	0.21
R&D	0.00	0.04	20.10	0.02
SG&A	0.00	0.66	20.09	0.09
Advertising	0.00	0.04	20.01	0.01
Return on assets	0.00	0.07	20.03	0.03
Operating return on assets	0.00	0.08	20.02	0.03

a. The fixed effects used in this table are retrieved from the regressions reported in Tables III and IV (row 3).

b. Column 1 reports the median fixed effect for each policy variable. Column 2 reports the standard deviation of the fixed effects. Columns 3 and 4 report the fixed effects at the twenty-fifth percentile and seventy-fifth percentile of the distribution, respectively.

c. Each fixed effect is weighted by the inverse of its standard error to account for estimation error.

quartile of the distribution increases the rate of return on assets by about 3 percent. In contrast, a manager in the bottom quartile reduces the rate of return on assets by about 3 percent.

Also, the median manager fixed effects for most of the corporate variables are not different from zero. This is interesting as one might have expected that the nature of the sample construction and the focus on outside hires might have led us to select a different type of managers. This seems to indicate that this possible selection issue is not an important factor in our analysis.

IV.E. Management Styles

The previous section documents a wide degree of heterogeneity in the way managers conduct their businesses. We now want to go a step further and investigate whether there are overarching patterns in managerial decision-making. For example, do some managers favor internal growth strategies while others rely more on external growth, ceteris paribus? Or can we observe that some managers overall are financially more aggressive than others?

To answer these questions, we analyze the correlation structure between the manager specific fixed effects which we retrieve from the set of regressions above. We form a data set that, for each manager, contains the estimated fixed effects for the various corporate variables. More precisely, the different variables in this new data set are the manager fixed effects estimated in Tables III and IV for the specification that includes all groups of managers (row 3).

In practice, we propose to estimate regressions as follows:

$$F.E.\text{~}y!_j = a + \mathrm{b}F.E.\text{~}z!_j + \mathrm{e}_j, \tag{2}$$

where j indexes managers, and y and z are any two corporate policy variables. Note that the right-hand-side variable in equation (2) is an estimated coefficient which is noisy by definition. This will lead to a downward bias in an OLS estimation of b. Since we know the precision with which the fixed effects are measured, we use a GLS estimation technique to account for the measurement error in the right-hand-side variable. We weigh each observation by the inverse of the standard error on the independent variable, which we obtain from the first step regressions.[29]

The results of this exercise are reported in Table VII. Each element in this table corresponds to a different regression. The average R^2 for these regressions is about 10 percent; the maximum R^2 is about 33 percent, while the minimum R^2 is about 0.03 percent. A few interesting patterns seem to emerge from this table. First, managers seem to differ in their approach toward external versus internal growth. We see from the last two rows of column 1 that there is a strong negative correlation between capital expenditures, which can be interpreted as internal investments, and external growth through acquisitions and diversification. In a similar vein, managers who follow expansion strategies through external acquisitions and diversification engage in less R&D expenditures. Row 7 of Table VII shows that the coefficients

29. We also repeated this analysis using a different technique to account for measurement error in the estimated fixed effect. For each set of fixed effects we formed averages of the observations by deciles (ranking observations by size), and then regressed the transformed set of fixed effects on each other in the above-described manner. This produces qualitatively similar results. Finally, we also conducted a factor analysis for the full set of fixed effects. We were able to distinguish three different eigenvectors. The factor loadings seem to support the view that financial aggressiveness and internal versus external growth are two important dimensions of style.

TABLE VII
RELATIONSHIP BETWEEN THE MANAGER FIXED EFFECTS

	Investment	Inv to *Q*	Inv to CF	Cash holdings	Leverage	R&D	Return on assets
Investment							0.00
							(0.00)
Inv to *Q* sensitivity	**6.8**						**0.03**
	(0.92)						**(0.01)**
Inv to CF sensitivity	0.02	**−0.23**					20.01
	(0.6)	**(0.11)**					(0.01)
Cash holdings	21.10	20.79	20.46				**−0.12**
	(1.62)	(1.71)	(1.72)				**(0.05)**
Leverage	20.39	20.28	20.63	**−0.40**			20.02
	(0.55)	(0.59)	(0.60)	**(0.17)**			(0.02)
R&D	**0.07**	**0.08**	**−0.03**	**−0.23**	**−0.02**		0.11
	(0.00)	**(0.02)**	**(0.01)**	**(0.04)**	**(0.01)**		(0.11)
Advertising	0.01	**0.02**	20.01	20.01	0.00	0.25	**0.31**
	(0.01)	**(0.01)**	(0.01)	(0.04)	(0.01)	(0.15)	(0.15)
N of acquisitions	**−0.27**	0.08	**0.23**	**0.01**	**0.02**	**−0.01**	**−0.01**
	(0.11)	(0.10)	**(0.10)**	**(0.00)**	**(0.01)**	**(0.00)**	**(0.00)**
N of divers. acquis.	**−0.30**	20.14	0.14	0.01	0.01	**−0.01**	**−0.01**
	(0.13)	(0.15)	(0.14)	(0.01)	(0.02)	**(0.00)**	**(0.00)**
SG&A	**−0.22**	**−0.30**	**0.10**	0.54	0.06	**−4.32**	**−3.36**
	(0.01)	**(0.04)**	**(0.03)**	(0.56)	(0.21)	**(0.90)**	**(0.62)**

a. Each entry in this table corresponds to a different regression.
b. Each entry reports the coefficient from a weighted regression of the fixed effects from the row variable on the fixed effects from the column variable. Observations in these regressions are weighted by the inverse of the standard error on the independent variable.
c. Coefficients that are significant at the 10 percent level are highlighted in bold.

from a regression of R&D on either of these variables are 20.01 with standard errors of 0.002. Moreover, capital expenditures and R&D expenditures are significantly positively correlated.

Another interesting finding is that managers who are more investment-*Q* sensitive also appear to be less investment-cash sensitive. The coefficient on b in a regression of the investment to *Q* fixed effects on the investment to cash flow fixed effects (column 2 and row 3 of Table VII) is 20.23 with a standard error of 0.11. This suggests that managers may follow one of two strategies: either use the firm's market valuation or use the cash flow generated by operations as a benchmark for their investment decisions. This result is interesting in light of the current debate on the investment to cash flow sensitivity in firms. So far, most research has analyzed differences in investment behavior across firms along a financial constraint dimension. Our findings sug-

gest that one might need to be aware of another important dimension: manager-specific heterogeneity.

On the financing side, we observe a negative correlation between the leverage fixed effects and the cash holding fixed effects. If cash holding is a proxy for financial slack, this result supports the idea that managers may differ in the conservatism or aggressiveness of their financing choices. Everything else equal, some managers prefer holding relatively less debt and more cash than other managers do.

Also, from the last row of Table VII, we see that managers with low levels of SG&A over sales favor more internal investment and R&D expenditures, while they engage in significantly fewer acquisitions. Managers with higher levels of SG&A to sales are also less investment-Q sensitive and more investment-cash flow sensitive.

Finally, as we already showed in Table IV, there are systematic differences in corporate performances across the managers in our sample, whether we measure performance as rate of return on asset or use an operating income measure. In the last column of Table VII, we see that the fixed effects in return on assets are systematically related to some managerial fixed effects in corporate decisions.[30] We find that managers with higher investment to Q sensitivities have higher return on assets fixed effects. Also, managers who keep more cash on the balance sheet or have higher levels of SG&A have lower rates of return on assets. Last, managers who engage in more acquisitions and more diversifying acquisitions are also associated with lower performance levels. These latter findings suggest that not only are there systematic differences in decision-making between managers but that these differences are systematically correlated with the performance fixed effects.

V. Possible Interpretations of the Manager Fixed Effects

As we discussed in Section II, there are at least two different interpretations of the observed manager fixed effects. One view is

30. Note that there is a strong positive relationship between the fixed effects in return on assets and operating return on assets. In a regression not reported here we found that the estimated coefficient in a regression of the return on assets fixed effects on operating return on assets fixed effects is 0.41, with a standard error of 0.05. The R^2 of that regression is 24 percent.

that managers impose their own idiosyncratic style onto the firm they head. If some styles are less performance-enhancing than others, some of these managers may cause corporations to adopt value-reducing policies. An alternative view is that managers differ in their comparative advantage or relative skills and firms optimally choose managers who are the best match for their current strategic needs. In this case, there is no such thing as a "better" or "worse" style but rather different styles that are best suited to different environments. While it is beyond the scope of this paper to fully sort out these two alternative interpretations, in the following we provide some evidence suggesting that the observed managerial fixed effects cannot be reasonably explained by a pure optimal matching story alone.

V.A. Sorting Based on Firm and Industry-Level Characteristics

A first interesting piece of evidence stems from our results in the last column of Table VII. Certain manager-specific styles seem to correlate systematically with manager fixed effects in performance: managers who hold more cash, are less investment-Q sensitive, engage in more M&A activity or spend more on SG&A, also have lower performance fixed effects. While these findings are consistent with the view that some styles are better for performance than others, we need to be cautious about possible alternative interpretations. More specifically, these results could also be consistent with the view that certain styles are better suited than others to periods of economic distress. Firms might be hiring "turnaround managers" whenever performance is particularly poor in order to implement specific policies that are beneficial in those times. However, this interpretation seems less convincing when we consider the correlation patterns in the last column of Table VII. Policies like high SG&A spending, or more intense M&A and diversification activities, which are negatively correlated with the fixed effects in performance, do not at first sight coincide with our intuition of turnaround practices. Moreover, this interpretation relies on the assumption that managers with particular styles are hired *in response* to changes in the economic environment of the firm (e.g., poor performance). This interpretation, however, seems inconsistent with our finding that most of the changes in firm policy happen *after* the manager joins a firm (Table V).

Yet another way to assess the relevance of the matching interpretation is to ask whether there is any apparent sorting of

different management styles across industries. For example, one might expect financially aggressive CEOs to be more prevalent in high growth industries while cost-cutting behavior may be more of a norm in more mature industries. For that purpose, we relate the manager fixed effects to industry measures of Tobin's Q and sales growth. We fail to find any robust systematic relationship.[31]

Another piece of evidence against a pure optimal matching interpretation comes from replicating our findings in Tables III, IV, and V after dropping the time-varying firm level controls. Indeed, under a pure matching model, we will identify manager fixed effects only if the firms' strategic needs at a given point in time are not fully captured by the time-varying firm controls that we have included in all of the regressions above. A natural implication of this argument is that the manager fixed effects we have identified should become economically and statistically stronger if we drop the available time-varying controls. When we perform this exercise, we find only statistically insignificant and economically small changes in our estimates.[32]

V.B. Governance, Compensation, and Style

In addition, we propose to investigate whether the estimated manager fixed effects, and especially the manager fixed effects in performance, are systematically related to differences in corporate governance across firms. If some management styles are "better" than others, one might expect that better governed firms will be more likely to select managers with these "good" styles.

To address this issue, we briefly turn to an alternative data source, CDA Spectrum, which provides some governance information for most of the firms in our sample. From CDA Spectrum we compute, for the second firm a manager is in, the fraction of shares held by large block holders.[33] We then regress the manager fixed effects in corporate practices and performance on this governance variable. Unfortunately, this database does not provide other governance measures, such as board composition variables.

The results of this analysis are reported in the first column of

31. We repeat this analysis for *firm-specific* conditions relative to the rest of their industry. For each corporate policy we compute the firm's deviation from its (asset-weighted) industry mean in the year prior to a turnover. Then we check whether the direction of the deviation from the industry mean helps to predict the type of manager hired by the firm. Again, we find no statistically robust patterns.

32. These results as well as other nonreported results below are available from the authors upon request.

33. See the Data Appendix for details.

Table VIII. Each cell in this column corresponds to a different regression where the dependent variables are the fixed effects on the corporate variables and the independent variable is the governance measure. Most interestingly, we find a positive and statistically significant relationship between the manager fixed effects in return on assets and the fraction of shares held by large block holders.[34] Moreover, most of the policy fixed effects that we found to be significantly related to the manager fixed effects in performance are also significantly related to governance with the same sign, although only the relationships for investment to Q sensitivity and M&A activity are statistically significant at traditional levels.

While we do not want to push these results too far due to the crudeness of the governance variable, they appear to suggest that better governed firms select managers with performance-enhancing styles and as such might point toward efficiency implications of the managerial heterogeneity.

Finally, we relate the manager fixed effects to manager compensation levels. If the correlation of manager styles with performance is symptomatic of some managerial styles being better than others, we might expect boards to pay a premium on average for managers with these styles. To perform this analysis, we first construct manager-specific compensation residuals that are net of firm fixed effects, year fixed effects, and other time-varying firm characteristics. More precisely, we estimate compensation regressions where we regress the logarithm of compensation on firm fixed effects, year fixed effects, the logarithm of total assets, the logarithm of total sales, the rate of return on assets, and the manager's tenure on the job; we also include dummy variables for whether the manager is a CEO, a CFO, or another top executive. The two compensation measures we consider are the logarithm of total compensation (defined as cash salary plus bonus plus the value of stock option granted in a year) and the logarithm of salary compensation. From these regressions we compute residual compensation measures for each executive in our sample. We then regress these residuals on the fixed effects derived in Tables III and IV. We use the GLS estimation described above to account for the measurement error in the right-hand-side variables.

The results of this exercise are reported in columns 2 and 3 of Table VIII. Most importantly, we see that managers with higher

34. Similarly, we find a positive relationship between the governance variable and the manager fixed effects on operating return on assets.

TABLE VIII
GOVERNANCE, COMPENSATION, AND MANAGER FIXED EFFECTS

	Percent shares held by large block holders	*Residual compensation*	
		Total compensation	*Salary compensation*
Return on assets	0.012	0.72	2.86
	(0.006)	(0.24)	(0.57)
Investment	0.278	0.02	20.08
	(0.252)	(0.01)	(0.06)
Inv to *Q* sensitivity	0.246	0.08	0.19
	(0.053)	(0.03)	(0.13)
Inv to CF sensitivity	20.004	20.06	20.06
	(0.088)	(0.04)	(0.07)
Cash holdings	20.001	20.02	20.26
	(0.007)	(0.15)	(0.29)
Leverage	20.018	0.04	20.01
	(0.021)	(0.26)	(0.18)
R&D	0.009	20.94	20.33
	0.009	20.94	20.33
	(0.006)	(0.08)	(0.90)
Advertising	0.008	2.18	1.36
	(0.007)	(0.93)	(0.54)
N of acquisitions	20.568	0.10	0.00
	(0.131)	(0.05)	(0.03)
N of diversifying acquisitions	20.617	0.09	0.03
	(0.092)	(0.04)	(0.05)
SG&A	20.027	20.16	20.09
	(0.093)	(0.04)	(0.25)

a. Each entry in column 1 corresponds to a different regression. The *dependent* variable in each of these regressions is the manager fixed effect on the row variable, as retrieved from Tables III and IV). The *independent* variable is the fraction of shares held by 10 percent or more block holders in the second firm we observe the manager in (from CDA Spectrum). The first number in each cell is the estimated coefficient; the second number is the estimated standard error. Each observation is weighted by the inverse of the standard error of the dependent variable.

b. Each entry in columns 2 and 3 corresponds to a different regression. The *independent* variable in each of these regressions is the manager fixed effect on the row variable, as retrieved from Tables III and IV). The *dependent* variable is a manager-level residual from a compensation regression where we control for firm fixed effects, year fixed effects, the logarithm of total assets, the logarithm of total sales, return on assets, tenure on the job, and dummies for whether the manager is a CEO, a CFO, or another top executive (see subsection IV.E for details). The two different compensation measures are the logarithm of total compensation (column 2), defined as salary plus bonus plus the Black and Scholes value of stock options grants, and the logarithm of salary compensation (column 3). In the reported regressions, each observation is weighted by the inverse of the standard error of the independent variable to account for estimation error.

return on assets fixed effects receive higher residual total compensation as well as higher salary compensation. This relationship is statistically significant for both compensation variables. It is interesting that we find such a strong positive correlation given that we have already controlled for return on assets when computing residual compensation to net out the pay-for-performance

relationship.[35] Firms thus appear to pay a premium for managers who are associated with higher rates of return on assets.[36]

With regard to the fixed effects on the specific policy variables, the picture is a little murkier. Two of the policy effects that are significantly related to the performance fixed effects (investment to Q sensitivity and advertising) are significantly related, with the same sign, to compensation. The correlations of compensation with cash holdings and SG&A, while of the same sign as those obtained for performance, are statistically weak. Finally, we find that managers with high levels of acquisition and diversification activity earn a premium on total compensation. This is surprising, since we saw in Table VII that the acquisition and diversification fixed effects are negatively related to the fixed effects on return on assets.[37] However, this relationship is statistically insignificant if we look at cash compensation only.

VI. Observable Managerial Characteristics

The previous sections have provided suggestive evidence of systematic differences in corporate decisions among top managers. However, the presence of managerial fixed effects does not tell us much about which specific managerial traits or characteristics might influence their decision-making. In this section we analyze the possible role of two such managerial characteristics: MBA graduation and birth cohort/age.[38] One expects MBA education to affect managerial decision-making either through human and social capital accumulation or because of a selection effect. Similarly, birth cohort might also be a relevant managerial

35. While we might still be concerned about a mechanical relationship between stock option grants (or bonus) and performance, we see that the positive relation holds even for cash salary alone.

36. In regressions not reported here we also investigated the relationship between the manager fixed effects in performance and compensation *change.* If firms were to learn over time that certain managers are particularly successful at creating value, one would expect that these managers would experience a bigger increase in pay from their first to their second job. Interestingly, we did not find any such relationship. This could indicate that the compensation of managers with perceived better styles were already bid up in their first job and that the learning about managers' type happens earlier in the managers' career.

37. Similarly, in a study of the determinants of CEO pay, Rose and Shepard [1997] find that managers of larger and more diversified firms are paid more.

38. In a related paper Chevalier and Ellison [1999] study cross-sectional differences in the behavior and performance of mutual fund managers. They show that younger managers and those who attended better schools earn higher rates of returns. They also show that managers from schools with higher SAT scores are more risk-taking in their investment behavior. In a survey of CFOs Graham and Harvey [2001] find that CFOs who report holding an MBA degree also use more sophisticated valuation techniques than those without an MBA.

trait as it is often suggested that older generations of managers are relatively more conservative in their decision-making.[39]

VI.A. Sample Construction

For this section of the study, we limit ourselves to a sample of CEOs.[40] As above, we use the Forbes 800 data from 1969 to 1999 and Execucomp data from 1992 to 1999 to create a list of CEO names. We then complement this information with two different data sources that provide background information for these CEOs: the *S&P Directory of Corporate Executives* and the *Who is Who of Corporate America.* We then merge this data set of observable managerial characteristics to COMPUSTAT and SDC data and construct all the relevant corporate variables (as described in the Data Appendix). Means and summary statistics for this sample are reported in columns 3 and 4 of Table I.

Perhaps somewhat surprisingly, the fraction of CEOs who have completed an MBA is only about 40 percent. The average CEO in our sample was born in 1928. The earliest year of birth is 1884 and the latest is 1966. Not surprisingly, we find that younger generations are more likely to have attended business school.

VI.B. Empirical Methodology

For all of the corporate variables y_{ijt} considered above, except investment to cash flow sensitivities and investment to Q sensitivities, we estimate the following regression:

$$(3) \qquad y_{ijt} = \beta X_{it} + \delta MBA_j + \eta Cohort_j + \gamma Tenure_j + \alpha_i + \lambda_t + \varepsilon_{ijt},$$

where i indexes firms, j indexes CEOs, t indexes time, X_{it} is a vector of firm characteristics, MBA_j is a dummy variable that equals 1 if CEO j completed an MBA and 0 otherwise, $Cohort_j$ is the birth cohort of CEO j, α_i are firm fixed effects, λ_t are year fixed effects, and ε_{ijt} is an error term. Also included in equation (3) is a control for the number of years the CEO has been in office, $Tenure_j$. This control should account for possible entrenchment

39. Obviously, the two specific managerial characteristics we propose to study here constitute only a small subset of the individual characteristics that we believe might be relevant to decision-making. For example, one would like to know more about family background, past professional experience, or even personal psychology. Unfortunately, obvious data constraints limit the richness of the exercise we can perform.

40. It is much more difficult to find background information on other top executives in the data sources that we consulted.

or career concern effects. Finally, we allow for clustering of the error term at the individual manager level.

There are two points worth emphasizing about equation (3). First, equation (3) includes firm fixed effects. Our identification is therefore not driven by average differences across firms in the type of CEOs they hire. Instead, our identification comes from within-firm variation in the MBA status or birth cohort of the CEO. Second, the estimation of equation (3), in contrast to equation (1), no longer relies on our ability to track the same manager into different firms over time. While managerial turnover still drives our test, the only requirement for identification is changes in CEO characteristics within firms over time. One implication of this feature is that, in contrast to our prior analysis, we rely on both internal and external hires to isolate the effect of MBA graduation and birth cohorts.

A study of the effect of managerial characteristics on investment to cash and investment to Q sensitivities requires a somewhat different empirical specification. We estimate the effect of MBA and birth cohort on investment to cash flow and investment to Q sensitivities by estimating the following regression:

(4)

$$
\begin{aligned}
I_{ijt} = {} & \beta X_{it} + \delta_1 MBA_j + \delta_2 MBA_j * CF_{it}\, K_{i,t-1} + \delta_3 MBA_j * Q_{i,t-1} \\
& + \eta_1 Cohort_j + \eta_2 Cohort_j * CF_{it}\, K_{i,t-1} + \eta_3 Cohort_j * Q_{i,t-1} \\
& + \gamma_1 Tenure_j + \gamma_2 Tenure_j * CF_{it}\, K_{i,t-1} + \gamma_3 Tenure_j \\
& * Q_{i,t-1} \alpha_i + \alpha_{i2} * CF_{it}\, K_{i,t-1} + \alpha_{i3} * Q_{i,t-1} + \lambda_t + \varepsilon_{ijt},
\end{aligned}
$$

where $\alpha_{i2} * CF_{it}/K_{i(t-1)}$ is a vector of interactions between firm fixed effects and cash flow, $\alpha_{i3} * Q_{i(t-1)}$ is a vector of interactions between firm fixed effects and lagged Tobin's Q and all the other variables are defined as above. By analogy with equation (3), equation (4) allows for firm-specific differences in investment to cash flow and investment to Q sensitivities.

VI.C. Results

The results are presented in Table IX. Each row corresponds to a different regression. Reported in all rows except rows (2) and (3) are the estimated coefficients on the birth cohort and MBA dummy from equation (3). In rows (2) and (3) we report the estimated coefficients on the interactions of these managerial traits with cash flow and lagged Tobin's Q, respectively, from equation (4).

TABLE IX
CEOs' BIRTH COHORT AND MBA EFFECTS ON FIRM POLICIES

Dependent variable:	Year of birth (*10)	MBA
(1) Investment	.017	.016
	(.005)	(.010)
(2) Inv to Q sensitivity	2.013	.017
	(.003)	(.006)
(3) Inv to CF sensitivity	.118	2.075
	(.014)	(.026)
(4) N of acquisitions	.001	2.017
	(.037)	(.056)
(5) Leverage	.024	.011
	(.007)	(.008)
(6) Interest coverage	26.50	.924
	(2.67)	(3.41)
(7) Cash holdings	2.005	2.001
	(.002)	(.003)
(8) Dividends/earnings	.000	2.009
	(.003)	(.004)
(9) N of diversifying acquis.	2.036	.040
	(.015)	(.017)
(10) R&D	2.003	2.002
	(.002)	(.002)
(11) Advertising	2.001	.003
	(.002)	(.003)
(12) SG&A	.002	2.004
	(.003)	(.003)
(13) Return on assets	2.003	.012
	(.004)	(.005)
(14) Operating return on assets	2.002	.008
	(.003)	(.003)

a. Sample is the set of firm-year observations for which we could obtain information on the year of birth and MBA graduation of the CEO, as described in subsection VI.A and Table I. Details on the definition and construction of the variables reported in the table are available in the Data Appendix.

b. Each row, except rows (2) and (3), corresponds to a different regression. Reported are the estimated coefficients on year of birth and MBA dummy. Also included in each regression are year fixed effects, firm fixed effects, and a control for CEO tenure. Other included controls are as follows: row (1): lagged Tobin's Q, cash flow, and lagged logarithm of total assets; row (4): return on assets and lagged logarithm of total assets; rows (5) to (8): return on assets, cash flow, and lagged logarithm of total assets; row (9): return on assets, cash flow, logarithm of total assets, and a dummy for whether the firm undertook any acquisition that year; rows (10) to (12): return on assets, cash flow, and logarithm of total assets; rows (13) and (14): logarithm of total assets.

c. The reported coefficients in rows (2) and (3) are from a unique regression of investment on year fixed effects, lagged Tobin's Q, cash flow, lagged logarithm of total assets, firm fixed effects, firm fixed effects interacted with lagged Tobin's Q and cash flow, CEO tenure, CEO tenure interacted with lagged Tobin's Q and cash flow, year of birth, year of birth interacted with lagged Tobin's Q and cash flow, an MBA dummy, an MBA dummy interacted with lagged Tobin's Q and cash flow. Reported in rows (2) and (3) are the estimated coefficients on the interactions between year of birth and the MBA dummy with lagged Tobin's Q and cash flow, respectively.

d. Standard errors are in parentheses. Standard errors are corrected for clustering of observations at the individual manager level.

We start with the investment variables. We find that CEOs from earlier birth cohorts are associated with lower investment levels, everything else equal. Each ten-year increase in year of birth decreases capital expenditures (as a ratio to lagged property, plant, and equipment) by about 1.7 percentage point. MBA graduates appear to invest 1.6 percentage point more on average, but this effect is more noisily estimated. Rows (2) and (3) consider investment to cash flow and investment to Q sensitivities. We find that MBA graduates on average respond more to Tobin's Q and less to cash flow availability when deciding about capital expenditures. This pattern is interesting. CEOs with MBA education appear to follow more closely the "textbook guidelines" when making investment decisions. They are less responsive to the availability of internal sources of funds but more responsive to the presence of growth opportunities as embodied in Tobin's Q. We find that older generations of CEOs are less responsive to Tobin's Q when setting investment level. However, somewhat more surprisingly, we do not find that younger generations weigh less internal sources of financing when making investment decisions. To the contrary, we find that investment to cash sensitivities are larger among younger cohorts. Finally, we find no significant relationship between the two managerial attributes and acquisition behavior (row (4)).

We next consider the financial policy variables. We find that older generations of CEOs choose lower levels of financial leverage, everything else equal (row (5)). Each ten-year increase in CEO year of birth increases financial leverage by about 2.5 percentage points. The point estimate on the effect of MBA graduation on financial leverage is positive but statistically insignificant. Consistent with the leverage results, interest coverage (row (6)) appears higher among CEOs from earlier birth cohorts and lower (but not significantly so) among MBA graduates. In row (7) we find a significant negative relationship between cash holdings and year of birth. If one regards lower levels of cash holdings as the sign of a more sophisticated or more aggressive financial policy, these results indicate that older generations might lack that kind of sophistication or aggressiveness. The effect of MBA graduation on cash holdings is economically and statistically insignificant. While we find no robust relationship between dividends over earnings and birth cohort (row (8)), there is a robust negative correlation between dividend payout and MBA graduation.

Next, we study the organizational strategy variables. CEOs with MBA degrees, and CEOs from earlier cohorts, have a stronger tendency to engage in diversification moves (row (9)). Younger generations and, to a lesser degree, MBA graduates engage in less R&D (row (10)). We find no consistent relationship between advertising expenditures (row (11)) or SG&A (row (12)) and the CEO characteristics.[41]

In summary, the results in Table IX suggest that the manager fixed effects we identify in the first part of the paper can, in part, be attributed to observable individual characteristics such as education and year of birth. CEOs with MBAs appear to be on average more aggressive, choosing to engage in a higher level of capital expenditures, hold more debt, and pay less dividends. CEOs from older generations appear to be less aggressive on average, choosing a lower level of capital expenditures, lower financial leverage, and higher cash holdings.

Finally, we also investigate the effect of MBA status and birth cohort on accounting performance (rows (13) and (14)). The most interesting finding is the positive relationship between MBA graduation and corporate performance. Rates of return on assets are more than 1 percentage point higher for MBA graduates. Similarly, CEOs who hold an MBA degree are associated with higher operating returns on assets.

VII. Conclusion

The primary objective of this paper is to document systematic behavioral differences in corporate decision-making across managers. We develop an empirical framework to analyze the importance of a manager dimension in the observed unexplained variation in several corporate practices. We find considerable heterogeneity across managers. The realizations of all investment, financing, and other organizational strategy variables appear to systematically depend on the specific executives in charge. While the framework we follow does not allow us to estimate the causal effect of managers on firm policies or performance, it provides a simple and intuitive approach to deal with many of the first-order selection problems that such a study might face.

41. This lack of statistical significance may in part reflect the fact that our sample becomes much smaller in these regressions due to the many missing values in COMPUSTAT.

We also find that some of the managerial differences in corporate practices are systematically related to differences in corporate performance. Moreover, managers with higher performance fixed effects receive higher levels of compensation and are more likely to be found in firms where ownership is more concentrated. Finally, we can tie back the differences in behavior across managers in part to some observable managerial characteristics. Older generations of managers, on average, are financially more conservative, while managers who hold an MBA degree follow more aggressive strategies.

Data Appendix

The corporate variables used in this paper are extracted from four major data sources: COMPUSTAT, the SDC Platinum Merger and Acquisition database, Execucomp, and CDA Spectrum.

COMPUSTAT is a data source that reports financial variables for more than 7500 individual corporations established in the United States (and territories) since 1976. The data are drawn from annual reports, 10-K filings and 10-Q filings, and sample large companies with substantial public ownership.

SDC Platinum Merger and Acquisition database is a financial data set collected by Thompson Financial. It contains information on M&A transactions by private and public firms in the United States from 1979 to the present. Reported for each transaction are the name and industry of the acquiring and target firms, as well as other variables about the specifics of the sale.

Standard and Poor's Execucomp data contain information on the name and total compensation of up to the five highest paid executives for all firms in the S&P 500, S&P MidCap 400, and S&P SmallCap 600 since 1992. The reported compensation data cover base salary, bonus, and the value of granted stock options in the current year.

CDA Spectrum collects information on institutional shareholdings from the SEC's 13f filings. The 1978 amendment to the Security and Exchange Act of 1934 requires all institutional investors with more than $100 million under management to report their shareholdings to the SEC. Holdings are reported quarterly on 13f filings. Institutions fall into five distinct categories: banks, insurance companies, mutual funds, independent investment advisors (such as brokerage firms), and others (which mainly include pension funds and endowments).

The specific variables used in the analysis are defined as follows:

- *Investment* is capital expenditures (COMPUSTAT item 128) over net property, plant, and equipment at the beginning of the fiscal year (COMPUSTAT item 8).
- *Average Tobin's Q* is defined as the market value of assets divided by the book value of assets (COMPUSTAT item 6), where the market value of assets equals the book value of assets plus the market value of common equity less the sum of the book value of common equity (COMPUSTAT item 60) and balance sheet deferred taxes (COMPUSTAT item 74).
- *Cash flow* is defined as the sum of earnings before extraordinary items (COMPUSTAT item 18) and depreciation (COMPUSTAT item 14) over net property, plant, and equipment at the beginning of the fiscal year (COMPUSTAT item 8).
- *Leverage* is defined as long-term debt (COMPUSTAT item 9) plus debt in current liabilities (COMPUSTAT item 34) over long-term debt plus debt in current liabilities plus the book value of common equity (COMPUSTAT item 60).
- *Cash holdings* is defined as cash and short-term investments (COMPUSTAT item 1) over net property, plant, and equipment at the beginning of the fiscal year (COMPUSTAT item 8).
- *Interest coverage* is earnings before depreciation, interest, and tax (COMPUSTAT item 13) over interest expenses (COMPUSTAT item 15).
- *Dividends over earnings* is the ratio of the sum of common dividends (COMPUSTAT item 21) and preferred dividends (COMPUSTAT item 19) over earnings before depreciation, interest, and tax (COMPUSTAT item 13).
- *R&D* is the ratio of R&D expenditures (COMPUSTAT item 46) over lagged total assets (COMPUSTAT item 6).
- *Advertising* is the ratio of advertising expenditures (COMPUSTAT item 45) over lagged total assets (COMPUSTAT item 6).
- *SG&A* is the ratio of selling, general, and administrative expenses (COMPUSTAT item 189) over sales (COMPUSTAT item 12).
- *N of acquisitions* is the total number of acquisitions in the fiscal year.
- *N of diversifying acquisitions* is the number of acquisitions

during the fiscal year in two-digit industries different from those the acquirer currently operates in.

- *Return on assets* is the ratio of EBITDA (COMPUSTAT item 18) over lagged total assets (COMPUSTAT item 6).
- *Operating return on assets* is the ratio of operating cash flow (COMPUSTAT item 308) over lagged total assets (COMPUSTAT item 6).
- *Total compensation* is the total value of a manager's compensation package for the fiscal year. It is defined as the sum of cash salary, cash bonus, and the Black and Scholes value of options granted in that year.
- *Percent shares held by large block holders* is the fraction of shares that are owned by block holders with 10 percent or more of the firm's outstanding shares (based on the last quarter of each year).

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