Managing with Style:

The Effect of Managers on Firm Policies

Marianne Bertrand and Antoinette Schoar*

April 12, 2002

Abstract

This paper investigates the extent to which heterogeneity in firm policies can be explained by differences in managerial style. We use a firm-manager matched panel data set where we can track the same managers across different firms over time. We find that manager fixed effects matter for a wide range of corporate decisions. For example, differences in capital expenditures, financial structure, dividend policies, acquisition and diversification policies, and cost-cutting policies are to a significant extent explained by executive fixed effects. Moreover, we identify specific patterns in managerial decision making that seem to indicate general differences in "style." We also show that style affects performance and that this is reflected in part in managerial compensation levels.

In a final step, we tie back these findings to some observable managerial characteristics. The two characteristics we focus on are MBA graduation and birth cohort. We analyze whether and how corporate decisions are affected by these managerial characteristics. Executives from earlier birth cohorts appear overall more financially conservative. On the other hand, managers who hold an MBA degree seem on average to follow more financially aggressive strategies.

*University of Chicago Graduate School of Business, NBER and CEPR; MIT Sloan School of Management and NBER. Addresses: 1101 E. 58th Street, R0 229D, Chicago, IL 60637; 50 Memorial Drive, E52-447, Cambridge, MA 02142. E-mail: marianne.bertrand@gsb.uchicago.edu; aschoar@mit.edu. We thank Rebecca Henderson, Steve Kaplan, Sendhil Mullainathan, Canice Prendergast, David Scharfstein, Jerry Warner, Michael Weisbach, seminar participants at Harvard University, the Kellogg Graduate School of Management, MIT, 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 Murphy and Robert Parrino for generously providing us with their data. Jennifer Fiumara and Mike McDonald provided excellent research assistance.

1 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 person, a builder. Sandy [Weil] is an acquirer. Just totally different."

-John Reed, CEO Citicorp

A large amount of research in finance and economics has been dedicated to understanding the determinants of corporate financing and investment policies. A persistent result that emerges from this literature is the enormous (and largely unexplained) heterogeneity in practices across firms. For example, Titman and Wessels (1988) and Smith and Watts (1992) look at the cross-sectional determinants of firms' capital structure. These papers analyze the effect of firm characteristics such as market-to-book ratios, the type of assets a firm operates or non-debt tax shields on capital structure. They show that a lot of variation remains unaccounted for by firm level characteristics. Similarly, Bradley, Jarrell and Kim (1984) find that a significant amount of variation in capital structure remains unexplained even after controlling for industry fixed effects.[1] In a parallel vein, the ongoing debate about differences in the investment to cash flow and investment to Q sensitivities between firms (Fazzari, Hubbard and Petersen (1987), Kaplan and Zingales (1997)) highlights considerable disagreement as to how to explain the wide variation in investment behavior across firms.

The novel contribution of this paper is to explicitly introduce a *people*, or more specifically a *manager*, dimension to explain part of the unaccounted heterogeneity in these corporate practices. A prevailing view in the business press and among managers themselves (as the quote by John Reed in the beginning of the paper suggests) is that CEOs and other top executives have different "styles" when making investment, financing or strategic decisions in their firm. For example, some managers have a reputation for being financially conservative, others for favoring acquisition-driven growth.[2] However, economic theory, and more specifically corporate finance research, have given little consideration to such a "people dimension." In most theories of the firm, all decision variables

[1] For a recent study of intra-industry variation in leverage see MacKay and Phillips (2001).

[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.

are tied down at the firm level, largely ignoring the manager level. This is even true of most agency theory models of the firm. While agency models acknowledge that managers might have varying degrees of discretion to alter firm decisions in order to advance their own objectives, they explain heterogeneity between firms by relying on differences in the ability of boards to control managers, or more generally, differences in the strength of corporate governance (again a firm level variable).[3]

Our primary objective in this paper is to provide some measurement of the importance of manager effects for a wide range of corporate decisions. Intuitively, one would like to quantify how much of the variation in firm policies can be attributed to manager specific fixed effects. One obvious problem with this intuitive approach is that manager effects could be correlated with other *firm specific* characteristics. Consequently, we propose to estimate the importance of manager fixed effects in a framework where we can control for any time-invariant differences across firms as well as for important time-varying factors at the firm level. For this purpose, we construct a firm-manager matched panel data set where we can track the same top managers across different firms over time. This allows us to estimate how much of the unexplained variation in firm policies can be attributed to manager fixed effects, after controlling for firm fixed effects and firm characteristics. The specific corporate decisions we study relate to investment policy (capital expenditures, investment to Q sensitivity, investment to cash flow sensitivity and acquisition policy), financing policy (financial leverage, interest coverage, cash holdings, and dividend pay-outs), as well as organizational strategy (R&D expenditures, advertising expenditures, diversification policy and cost-cutting policy).[4]

Our results show that manager fixed effects are quantitatively important determinants of corporate decisions. On average, adjusted R^2's of corporate variables on firm fixed effects and firm time-varying characteristics increase by about four percentage points with the inclusion of manager fixed effects. Most interestingly, we find that manager effects matter much more for some decisions than others. For example, manager fixed effects explain an additional 11 percentage points of the variance in acquisition or diversification policy in our data set. Manager fixed effects also appear to

[3] A few recent exceptions in the theory literature are the papers by Saloner and Rotemberg (2000) and Van den Steen (2002). These papers explicitly model the vision of the CEO as an important determinant of firm policy, which in our terminology could be interpreted as a specific managerial style.

[4] Obviously 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 in choosing this set up, we do not want to rule out that managers learn over time or change their style. In fact, the question of where styles come from and how they are acquired is of central interest for future research. Yet if managerial decision making were subject to constant changes we would not be able to find any evidence for heterogeneity in "style" as defined here.

be especially important in the determination of dividend policy, interest coverage and cost-cutting policy. We also show that different managers matter for different decisions. CFOs have the biggest impact on financial structure variables. Maybe more surprisingly, segment-level CEOs appear to vary substantially in their acquisition and diversification policies.

In a second step, we analyze how these various manager specific decisions are related. For that purpose, we correlate the manager fixed effects across all corporate variables to identify over-arching patterns in managerial decision-making. Among other things, we find that managers seem to differ in their approach towards internal versus external growth. Those that engage in more external acquisitions and diversification also display lower levels of capital expenditures and R&D. Also, we find that managers who have high investment to Q fixed effects rank very low on their investment to cash flow sensitivity (and vice versa). This suggests that managers may either use Q or cash flows as their benchmark for making investment decisions.[5]

These managerial fixed effects in corporate strategies can be interpreted in two directions. On the one hand, managerial style could be the outcome of managers imposing their specific style on the firms where they are hired. Even though it seems highly implausible to assume that firms randomly hire managers, there is plenty of anecdotal evidence that firms are not always fully aware of the type of manager they hire. On the other hand, boards might be aware of differences in style and actively hire a manager for the specific style that he or she brings to the firm. For example, if a firm decides that it needs to change its strategy and adopt more aggressive growth, it might choose to hire a CEO who is known to have this management style.[6] Under this interpretation the manager does not impose his style on the firm, but is chosen exactly because of his style.[7] Under either interpretation, however, managers play a central role in bringing about the change.

We provide a number of distinct pieces of evidence in order to assess the relative importance of these two alternative interpretations. First, we show that the differences in managerial practices

[5]Note that managerial styles are not simply the result of random changes at the firm level, when a new manager is hired. Purely random changes of firm policies would not produce significant fixed effects. Only if the policies at the firm level change systematically each time a particular manager moves to a new firm can we estimate significant manager fixed effects.

[6]A well-known example of this matching view is Mike Armstrong at AT&T who, according to Business Week, was hired because the board of AT&T had decided that the firm needed to acquire companies in several areas.

[7]For this story to hold it is not enough that firms which generally have more aggressive policies hire managers with the same styles and vice versa, since these effects would be taken out by firm fixed effects. Instead, boards must decide about *changes* in corporate policies that are implemented when the new manager comes in. One possible story is that outside managers are better able to break implicit contracts inside the firm and therefore to credibly commit to a new strategy.

documented above are not performance neutral. We document that there are important manager fixed effects in corporate performance and that these fixed effects in performance are systematically related to some of the manager fixed effects in corporate practices. Manager fixed effects explain an additional five percentage points of the variance in accounting performance. Managers that are more investment-Q sensitive, hold less cash, cut cost more and are less active in the acquisitions and diversification also have higher performance fixed effects. These findings shed some doubt on the view that managers are *perfectly* matched to firms based on their style. Under the assumptions of an optimal matching model we would not expect to find such systematic differences in outcomes. However, we also provide some evidence that corporate boards are at least partly aware of this heterogeneity in managerial style. We do this by relating manager residual compensation *levels* to their fixed effects on performance and corporate decisions. We find that managers that display higher performance fixed effects also have higher compensation levels. Overall, therefore, our findings suggest that while boards are aware of some differences in managerial style or skills, the matching process is most likely imperfect and still allows for differences in firm performance. This might either be due to residual information asymmetry about a manager's style, or because of other frictions in the executive labor market.[8]

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

The rest of this paper is organized as follows. Section 2 provides a brief review of the related literature. Section 3 presents the different data sources, describes the construction of the data set and defines the main variables of interest. Section 4 quantifies the importance of manager fixed effects for various corporate practices. Section 5 relates the manager fixed effects in corporate

[8]We conduct a number of additional tests to analyze whether industry or firm characteristics at the time of management change are systematically related to the style of the managers that firms hire. For example, one could conjecture that industries with higher growth opportunities select managers with a more aggressive management style. But we do not observe any systematic evidence that managers sort by industry or type of firm according to their style. One more exogenous test of the matching interpretation would be to ask whether style effects are more important during voluntary versus involuntary turnover episodes. We are currently in the process of collecting this data.

practices to manager fixed effects in corporate performance and compensation. Section 6 studies birth cohort and MBA graduation as two specific determinants of managerial style. Section 7 summarizes and offers some concluding remarks.

2 Literature Review

While economists have rarely considered the issue of heterogeneity in managerial style, a large body of literature has analyzed stock market responses to episodes of CEO turnover. One of the earliest papers studying excess returns following CEO turnover announcements is Warner, Watts and Wruck (1988). Kesner and Sebora (1994) conduct a detailed survey of this literature, and show that results have been inconclusive so far, with papers finding conflicting evidence on the relationship between CEO turnover and excess returns during the event window. Other studies of CEO turnover focus on the reasons behind a top executive turnover. While firm performance can contribute to CEO turnover, it appears not to be a dominant factor (Vancil (1987)). Several later papers document that management turnover is more likely to be triggered by a proxy fight or an episode of financial distress (see Gilson (1989), Gilson (1990), DeAngelo and DeAngelo (1989)).

There are a only few papers in economics and finance analyzing the role of managers on specific firm decisions. For example, Barberis, Byocko, Shleifer and Tsukanova (1996) study the privatization process of Russian firms. They show the important role played by new managers and new owners in the restructuring decision and its eventual impact on productivity. Also, Weisbach (1995) focuses on U.S. firms following a leveraged buyout. Weisbach shows that the probability of asset divestitures increases after a CEO change. Both of these papers, however, focus on the average effects of managerial change. They do not systematically relate strategy changes to the specific management styles of the old and new managers. Much more directly related to our approach are two papers by Chevalier and Ellison (1999) and Graham and Harvey (2002) who look for systematic differences in the ways managers behave. Chevalier and Ellison (1999) study cross-sectional differences in the behavior and performance of mutual fund managers, focusing on observable managerial traits. More specifically, they show that younger managers and managers 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 (2002) find that CFOs who report holding an MBA degree, also report using more sophisticated valuation techniques, than those without an MBA.

Other fields of research, however, have given management style much more consideration. Specifically, there is a large body of management literature anlyzing the determinants of decision-making among CEOs. Yet, both the specific focus in 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 rather than the actual economic outcomes we care about in this study. For example, various papers have studied how managerial background characteristics affect their leadership style, communication process or charisma. See for example, Hambrick and Mason (1984) or Waldman, Ramirez, House and Puranam (2001). While establishing such differences in the process of leadership is a central micro-foundation to the concept of managerial style, it tells little as to whether and how much these differences eventually impact corporate decisions. Second, most of the existing work on CEO style in the field of management relies on case studies, laboratory experiments or subjective survey responses. While these research methods often offer a more controlled environment and a richer institutional setting, they lack the level of generality of our empirical approach. More specifically, these research methods do not generally permit the type of quantification exercise we propose to perform here. One paper in the management science literature that follows an empirical approach more closely related to ours is Lieberman (1990). Lieberman finds significant managerial fixed effects in productivity in the US and Japanese automobile industry.

3 Data

3.1 Sample Construction

Our first goal in this paper is to understand whether there are systematic differences in the ways top managers behave, and whether these differences can help explain part of the observed unexplained heterogeneity in a set of corporate practices. A simple way to proceed is 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 simple approach is that certain firms might have persistently higher investment or leverage levels due to some unobservable differences

which are not captured by observable characteristics, but which are independent of the managers in place. Practically, this means that we need to separately identify manager fixed effects from firm fixed effects. This separate identification is only possible for managers that can be observed in at least two different firms.

We therefore construct a firm-manager matched panel data set that allows us to track the same 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 US firms. Execucomp allows us to track the name of the top five highest paid executives in 1500 publicly traded US 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 top executive can be observed in at least two firms. We also impose that the managers have to be in each firm for at least three years. This three-year requirement insures that managers are given a chance to "leave their mark" in a given company.[10] The resulting sample contains about 600 firms and slightly over 500 individual managers that can be followed in at least two different firms in this sample.[11] The average length of a manager's stay within a given firm in our sample is a little over 5 years.

For this sample of firms, we construct a series of annual accounting variables. We concentrate our analysis on three different sets of corporate decisions: investment policy, financing policy and organizational strategy. The investment policy variables are: investment, investment to Tobin's Q sensitivity, investment to cash flow sensitivity, and number of acquisitions. We define investment as 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). The financing policy variables are: financial

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

[10] All of the results below were replicated ignoring this three-year constraint in the sample construction. The results we obtained were qualitatively similar but, not surprisingly, statistically weaker.

[11] For a subset of firms, we observe several management switches.

leverage, cash holdings, interest coverage and dividends over earnings. 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 (COMPUSTAT item 21) and preferred (COMPUSTAT item 19) dividends over earnings before depreciation, interest and tax (COMPUSTAT item 13). Finally, the organizational strategy variables we consider are: number of diversifying acquisitions, R&D expenditures over lagged total assets (COMPUSTAT item 46/COMPUSTAT item 6), advertising expenditures over lagged total assets (COMPUSTAT item 45/COMPUSTAT item 6), and selling, general and administrative expenses over sales (COMPUSTAT item 189/COMPUSTAT item 12). A diversifying acquisition is defined as an acquisition of an asset (or firm) in a different two-digit industry than those that the firm currently operates in. All of these variables are constructed from COMPUSTAT except for the information on acquisition and diversification, which we obtain from the SDC merger and acquisition files.

3.2 Sample Description

Table 1 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 1 report equivalent summary statistics for the entire COMPUSTAT sample over the period 1969 to 1999. As one might have 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.[12] The firms in our sample of movers also have slightly higher Q ratio and

[12]One could argue that this required focus on larger firms may in fact bias our test 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

rate of return on assets, and lower investment and financial leverage. They are very similar to the average COMPUSTAT firm with respect to cash holdings, dividend payouts, R&D and advertising expenditures to total assets, SG&A to total sales, and number of acquisitions and diversifying acquisitions.

Table 2 tabulates the type of executive transitions in our sample. As we mentioned earlier, our sample contains about 500 separate executives that we can track across firms. Of these, 117 are individuals that move from a CEO position in one firm to a CEO position in another firm; four are CEOs that move to becoming CFOs and 52 are CEOs that move to other top positions (i.e., neither a CEO or CFO position). We observe seven CFOs becoming CEOs while 58 move from CFO to CFO positions and 30 CFOs move to positions where they are neither CEO or CFO. Finally, Table 2 shows that 106 CEOs held positions other than CEO or CFO in their previous firm and 145 managers switch between other top positions. Among the latter category, we found that about 40% are moves from a subdivision CEO position in one firm to a subdivision CEO position in another firm. In the second row of each cell of Table 2, we report the fraction of moves in that cell that are between different 2-digit industries. It is interesting to note that a large fraction of the executive moves in our sample are between industry. For example, 63% of the CEO to CEO moves are across different 2-digit industries, as are 71% of the CFO to CFO moves. A relatively lower fraction of the moves from other top positions to other top positions (42%) are across industries. These results seem intuitive if ones believes that CEOs and CFOs need less industry and firm specific knowledge and more general management skills.[13]

In the analysis that follows, we will make it a convention to code a move between different top positions according to the *last* position held by the specific manager. For instance, Edward Liddy was the CFO of Sears Roebuck in 1992 and 1993. In 1994 he became the CEO of Allstate Corporation. We will code this event as a CEO move, since this person ends up as the CEO of the last company we can track him in.[14]

be that managers that have more distinct "styles" are more likely to be found in larger firms.

[13]See, for example, Fligstein (1989) for a discussion of this argument.

[14]We also repeat all of our analyses where we separately identify CEO to CEO moves, CEO to CFO moves, and so on. The results are qualitatively similar to the more aggregated results reported in the paper.

4 Is There Heterogeneity in Executive Practices?

4.1 Empirical Methodology

Our goal in this section is to investigate whether there are systematic differences in corporate practices across managers. In other words, we want to quantify the extent to which investment, financing and other organizational policies of a firm are influenced by the identity of the manager making the decision. For that purpose, we propose to estimate several fixed effects models for the array of dependent variables described in section 3.1. For each dependent variable, we first estimate a benchmark specification which includes both firm and time fixed effects as well as relevant time-varying firm controls.[15] We then ask how much the fit of that benchmark specification can be improved by adding manager fixed effects.

The nature of our identification strategy might 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 that net out any average differences in payouts across firms and time as well as the effect of any firm-year specific shocks (such as earnings shocks) that might affect the dividend payout of a firm. We then ask whether there are systematic differences across managers in this residual dividend payout ratio and how much of this remaining variance in dividend payouts can be attributed to the managers.

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

$$y_{it} = \alpha_t + \gamma_i + \beta X_{it} + \lambda_{CEO} + \lambda_{CFO} + \lambda_{Others} + \epsilon_{it} \quad (1)$$

where y_{it} stands for one of the corporate policy variables, α_t are year fixed effects, γ_i are firm fixed effects, and X_{it} represents a vector of time-varying firm level controls. 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: λ_{CEO} are fixed effects for the group of managers that are CEOs in the last position we observe them in, λ_{CFO} are fixed effects for the group of managers that are CFOs in the last position we observe them in, and λ_{Others} are fixed effects for the group of managers that are neither CEOs nor CFOs in the last position we observe them in.

The top managers we focus on for this analysis need to be those that we can observe in at

[15] The specific time-varying controls included for each dependent variable are discussed in section 4.2.

least two different companies. First, it should be clear that the estimation of the manager fixed effects is not possible for managers that 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 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 co-linear in this case.

It would statistically be possible to extend the analysis to top managers that we only observe in one firm but 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. This is especially problematic if an episode of managerial turnover corresponds to a time when the board wants to implement some changes within the company or correct some previous trends. Instead, for manager fixed effects to matter under our approach, we require a much more stringent test. Corporate practices have to be correlated across (at least) two firms when the same manager is present in these two firms.[16] For the sake of completeness, we replicate our results under this alternative approach, covering a much larger set of executives. As one might have expected, we find even stronger manager fixed effects.

While the discussion above clarifies why our identification relies solely on outside hires, it is also important to discuss possible implications of this sample selection for 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 we focus on in this analysis. We use the entire Execucomp sample to compute the fraction of CEOs that were hired from the outside rather than internally promoted. We find that only 48% are internally promoted.[17] Nevertheless, one could reasonably argue that managers who are recruited from the outside are different from internally promoted ones.[18] 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

[16]Note that this correlation will not be driven by the fact that managers might move between firms that are similar in their investment, financing or organizational policies. Indeed, our analysis nets out any firm fixed effects.

[17]In a more detailed study, Parrino (1997) shows that the prevalence of inside versus outside succession varies a lot by industry.

[18]Suggestive evidence for this seems to emerge from a set of papers that look 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.

managers.[19].

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 empirical analysis 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 those firms. This could be because top managers impose their style onto a firm. Alternatively, our results could be interpreted as boards hiring specific managers *because of* their style and the changes they could bring to the firm. Under either interpretation, the important point is that specific individuals are needed to bring about the changes inside companies. In section 5.1, we will try to address the question of awareness by corporate boards of differences in managerial style.

4.2 Results

Tables 3 to 5 report F-tests and adjusted R^2 from the estimation of equation 1 for the different sets of corporate policy variables. For each variable, the first row reports the fit of a benchmark specification that includes only firm fixed effects, year fixed effects and time-varying firm specific controls. The next three rows report the change in adjusted R^2 when we consecutively add CEO fixed effects (second row), CFO fixed effects (third row) and finally fixed effects for all other executives (fourth row). The second to fourth rows also report F-statistics from tests of the joint significance of the different sets of manager fixed effects.

Overall, the findings in Tables 3 to 5 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 for most cases the null hypothesis that all the manager fixed effects are zero. Moreover, we see that there are important differences as to which decision variables seem to be affected the most by managers and as to which type of managers matters the most for which decision. We now discuss these results in greater detail for each set of corporate policies.

Table 3 reports the results for the set of investment regressions. Overall, we see that the adjusted R^2 of the model increases by more than 3% on average when adding executive fixed effects.

[19]We will however show in Table 6 that the median of the manager fixed effects on corporate performance is not different from 0

Moreover, the F-tests on the constraints are overall large, indicating that the manager fixed effects are jointly significant. The first corporate variable studied in this table is capital expenditures as a fraction of lagged net property, plant and equipment, which we refer to as investment. The benchmark specification (row 1) includes controls for firm fixed effects, year fixed effects, cash flow, Tobin's Q, return on assets and the logarithm of total assets. The adjusted R^2 for that specification is 91%. Even though the fit of this benchmark model is already very high, the adjusted R^2 increases significantly when we include CEO, CFO and all other manager fixed effects, as reported in rows (2) to (4). When including the full set of manager fixed effects, the adjusted R^2 goes up to 96%. Also, the F-tests are large, allowing us to reject the null hypothesis of no joint effect for all three sets of managers.

The next two set of corporate variables we consider are investment to Q (rows 5 to 8) and investment to cash flow sensitivities (rows 9 to 12). For these two variables, the benchmark specifications are somewhat different from those reported in section 4.1. We allow here for each firm to have its own investment to Q and investment to cash flow sensitivity. In other words, in addition to firm fixed effects, year fixed effects and controls for cash flow, Q, return on assets and log(total assets), we also allow for interactions of the firm fixed effects with Q (rows 5 to 8) and cash flow (rows 9 to 12). We then test whether allowing the investment sensitivities to Q and to cash flow to depend on the identity of managers improves the fit of the model. We do this by interacting the manager fixed effects with Q and cash flow respectively. The results in rows (5) to (12) show increases in adjusted R^2 from the inclusion of the manager fixed effects, especially with regard to the investment to Q sensitivities. The adjusted R^2 goes up from 95% to 98% when we allow the investment to Q sensitivity to be manager specific.

The last four rows of Table 3 report our findings on the number of acquisitions. For this variable we observe an especially large increase in adjusted R^2 of about 11% following the inclusion of all the manager fixed effects. Moreover, we see that the CEO fixed effects lead to a bigger increase in adjusted R^2 than CFO fixed effects, which indicates that all managers do not affect acquisition policy equally. As we might have expected from prior intuition, CEOs matter more in the decision to acquire new firms than CFOs do. Including fixed effects for all other managers also has a very large effect on adjusted R^2. In regressions not reported here, we broke down that set of other managers into those that are segment level CEOs and those that are not. We found out that the

fixed effects for segment level CEOs were those that explained the increase in adjusted R^2 in the last row of Table 3. Interestingly, this seems to indicate that many acquisition decisions are made at the segment level within firms.

Table 4 focuses on financing decisions: financial leverage (rows 1 to 4), interest coverage (rows 5 to 8), cash holdings (rows 9 to 12) and dividends over earnings (rows 13 to 16). Included in all regressions in Table 4 are firm fixed effects, year fixed effects, the logarithm of total assets, cash flow, and the rate of return on assets.[20] The results, in terms of an increase in adjusted R^2, are comparable to those obtained for the investment regressions. The adjusted R^2 of the leverage regression increases from 39% to 41% when including manager fixed effects. The adjusted R^2 of the regression for interest coverage, an alternative measure of capital structure, increases by as much as 10% after we include fixed effects for all sets of managers. Interestingly, CFOs have the strongest effect on interest coverage, a key financial indicator. Adjusted R^2 increases by 7% when including CFO fixed effects, while CEO fixed effects only marginally improve the fit of the model. The adjusted R^2 of the cash holdings regression goes up by 3%, from 77% to 80%, when we compare the benchmark specification to the specification that includes all manager fixed effects. Finally, managers appear to be important determinants of dividend policy, with an overall increase in adjusted R^2 of 7%. Interestingly, we find that dividend policy seems to be more substantially affected by CEOs than by CFOs.

Table 5 looks at organizational strategy. The different variables we consider are: number of diversifying acquisitions (rows 1 to 4), R&D expenditures (rows 5 to 8), advertising expenditures (rows 9 to 12) and SG&A (rows 13 to 16).[21] Again, we find that top executives have large effects on the realization of these variables. The fit of the diversification regression improves by 11%. The adjusted R^2s of the R&D and advertising regressions both increase by 5%. 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. Moreover, and in line with a priori intuition, we find that CEOs and other top executives seem to have much larger effects on organizational strategy than CFOs.[22]

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

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

[22] In regressions not reported here, we again separated segment level CEOs from other top executives in the last category of fixed effects. We found that segment level CEOs were the ones driving the large effect of that group of managers on diversification strategy.

Finally, in the last four rows of Table 5, we ask whether the observed effects of managers on corporate policies translate into effects on corporate performance. More specifically, we estimate the effects of managers on an accounting measure of performance: return on assets. When estimating these return on assets regressions, we include the logarithm of total assets as a control. Our results show that accounting performance varies significantly based on the identity of the top executives. The F-tests are large for all groups of managers and the adjusted R^2 increases by more than 5%.

It is importnat to note that there are systematic differences across managers in their performance achievement. This might suggest that the systematic differences in corporate policies observed above are not performance neutral for the firms. If managers with specific styles were perfectly matched to firms based on the firms' specific needs at a given point in time., we might not expect a strong relation between performance and manager styles. In fact, in section 5, we will show that some of the manager fixed effects in corporate practices are systematically related to manager fixed effects in performance.

4.3 The Magnitude of 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. This can be done with a study of the distributions of the fixed effects we have estimated in the previous section. 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 6, we report the size distribution of the manager fixed effects from the regressions reported in Tables 3 to 5. We report the medians, 25th percentile and 75th percentile of the manager fixed effects distribution for all the corporate variables.

Overall, Table 6 shows that the variation in the size of the manager fixed effects is economically large. To discuss just a few examples, row (1) of Table 6 shows that the difference between a manager at the 25th percentile of the distribution of investment level and one at the 75th percentile is 0.20. To give a benchmark, the average ratio of investment to assets in our sample is about 0.30. The difference between the 25th and 75th 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 6 shows that a manager

in the bottom quartile reduces the number of acquisitions by -0.49, while a manager in the top quartile increases the number of acquisitions by 0.44 per year. Finally, in the last row of Table 6, we see that the variation in corporate performance fixed effects is also large. A manager in the top quartile of the distribution increases the rate of return on assets by about 3%. In contrast, a manager in the bottom quartile reduces the rate of return on assets by about 3%.

It is also interesting to note that the median manager fixed effects for most of the corporate variables are not different from 0. For example, as we discussed above, one might have expected that the nature of the sample construction and the focus on outside hires might have led us to select a group of managers with "better" styles. In fact, we find that the median manager fixed effect on corporate performance is about 0.

4.4 Robustness Checks

We have assessed the robustness of the findings in this section to a series of specification checks. A first possible source of concern is the serial correlation in firm policies over time. While we already account for serial correlation by clustering the error term in all the regressions above by firm, we also perform two alternative tests to deal with this concern. First, we replicate all the results above after collapsing the data at the manager/firm level. More specifically, using the micro data, we first regress all the firm variables of interest on firm fixed effects, year fixed effects and the time-varying firm controls. We then collapse the residuals from these regressions at the manager/firm level. Lastly, we re-estimate the managerial fixed effects in this collapsed data. The findings are robust to this alternative estimation technique. Additionally, we also use a more parametric specification to estimate managerial style. For each policy variable, we regress a manager's residual (as described above) on his second job to his residual on his first job. Our findings are also robust to this alternative specification.

A second possible source of concern relates to the timing of the managerial fixed effects. We posited that the managerial fixed effects capture the active influence of a manager on corporate decisions. One might potentially object that, while executives appear to be correlated with changes in corporate decisions, the change in corporate policy might have been implemented *before* the actual succession happened. To be more precise, suppose the board of a given firm decides that it should bolster its firm's acquisition policy. The firm might be able to go ahead and implement

this change without changing managers. But, say, to signal the credibility of this new policy, the firm might decide to hire a new top manager who has proven to be a big acquirer on his previous job. Under this scenario, we would empirically estimate significant managerial fixed effects but the interpretation of the results should be very different.

In order to address this concern, we perform the following test. We re-estimate all the regressions above but assumed that the managers joined their last firm two years prior to the date at which they actually joined and quit the year they actually joined, censuring the data at the date of actual arrival of the manager into their last firm. This gives us a simple way to assess whether the change in policy is tightly linked to the executive succession. Under the scenario above, we would expect that the change in corporate policy might have started prior to the hire and we would therefore find significant "managerial fixed effects" in this modified data. In fact, when we perform this exercise, the F-tests on the manager fixed effects are statistically insignificant at the 10 percent level in almost all cases.[23] This test suggests that the change in policy comes about precisely when the manager changes positions. This rules out the possibility that firms are already implementing policy changes prior to the arrival of the new manager.

5 Management Styles, Performance and Compensation

The previous section documents that there is 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. Do some managers favor internal growth strategies while others rely more on external growth, ceteris paribus? Are some managers overall more financially aggressive than others? Finally, we also want to ask whether certain patterns in decision making are systematically associated with higher performance fixed effects.

To answer these questions, we analyze the correlation structure between the manager specific fixed effects which we retrieve from the set of regressions above.[24] We form a data set that contains, for each manager, 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 the last rows

[23] The only two exceptions are advertising and R&D expenditures where the fixed effects on *Other Executives* are marginally significant.

[24] We will use term correlation somewhat loosely in the following, since de facto we are estimating the coefficient of a univariate regression of one set of fixed effects on another.

of Tables 3 to 5.

In practice, we propose to estimate regressions as follows:

$$F.E.(y)_j = a + \beta F.E.(z)_j + \epsilon_j \tag{2}$$

Here j varies with the identity of the managers, and y and z are any two of the corporate policy variables. Note that the right hand side variable in equation 2 is an estimated variable which is noisy by definition. This will lead to a downward bias in an OLS estimation of β. 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.[25]

Table 7 reports coefficients and standard errors from the estimations of equation 2 for all the possible correlations of corporate strategy variables. Each element in Table 7 corresponds to a different regression. The R^2s for all of these regressions are in the vicinity of 10%.

A few interesting patterns seem to emerge from this table. First, managers seem to differ in their approach towards external versus internal growth, holding everything else constant. We see from the last two rows of column (1) in Table 7 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 7 shows that the coefficients from a regression of R&D on either of these variables are -0.01 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 β in a regression of the investment to Q fixed effects on the investment to cash flow fixed effects (column (2)-row (3) of Table 7) is -0.23

[25] We also repeat this analysis using a different technique to account for measurement error in the estimated fixed effect. For each set of fixed effects we form averages of the observations by deciles (ranking observations by size), then we regress the transformed set of fixed effects on each other in the above-described manner. The estimated "correlations" between the fixed effects produce results that are qualitatively equivalent to the analysis shown in the paper. Finally, we also conduct a factor analysis for the full set of fixed effects. We can distinguish three different eigenvectors. The factor loadings seem to support our overall story, which differentiates managers based on their aggressiveness and willingness to grow their firms through internal investment or external acquisitions.

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 the context of the current debate on the investment cash flow sensitivity in firms. So far, most research has analyzed the difference in investment to Q and investment to cash flow sensitivities across firms along the financial constraint dimension. Our results suggest that we need to be aware of another important dimension: manager specific heterogeneity.

Moreover, 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.

From the last row of Table 7, we also see that managers with low levels of SG&A over sales favor more internal investment and R&D expenditures, while they engage in significantly less acquisitions. Managers with higher level of SG&A to sales are also less investment-Q sensitive and more investment-cash flow sensitive.

Finally, the last column of Table 7 displays some interesting relations between corporate practices and accounting performance. As we already showed in Table 5, there are systematic differences in corporate performance between the managers in our sample. The last row of Table 7 shows that these differences are systematically related to managerial differences in corporate decisions. Managers with higher investment-Q sensitivities have higher levels of return on assets. Those that keep more cash on the balance sheet or have higher levels of SG&A have lower returns on assets. And lastly, managers that engage in more acquisitions and more diversifying acquisitions also are statistically associated with lower performance level.

The presence of manager fixed effects in performance and the systematic relation between manager specific performance and some manager specific decisions is hard to reconcile with the view that managers are optimally matched to firms based on their decision-making style. Under the view that boards hire, for example, managers that are more internal growth-driven when this reflects the firm's current needs, or managers that are more acquisition-driven when this reflects the firm's needs, one would not expect a systematic relationship between those investment styles and

performance. Instead, our results suggest that not only are there systematic differences between managers in decision-making, but that these differences have implications for firm performance.

Note that we conduct a series of additional tests to evaluate how much matching there is between manager types and firms or industry types. We first examine whether there is any evidence that managers are sorted across industries based on their style, the idea being that some managerial styles might complement certain industries better than others. For example, financially aggressive CEOs may be more prevalent in high growth industries while, cost-cutting CEOs may be more prevalent in mature industries. For that purpose, we relate managerial fixed effects to industry Q ratios and sales growth. We find no robust systematic relationship between manager style and these industry characteristics. It therefore does not appear at first glance that managers are sorted into the industries where their style might be the most valuable. Second, we ask whether managerial fixed effects are related to firm-specific conditions at the time of hire. In order to investigate this question, we compute for each corporate policy the firm deviation from its (asset-weighted) industry mean in the year prior to a turnover. Again, we find no statistically significant patterns.

5.1 Management Style and Compensation

In this section, we investigate the relationship between the manager fixed effects in corporate practices and performance and manager compensation levels. While the evidence above suggests that managers are unlikely to be perfectly matched to firms based on their type, it might still be the case that boards are at least aware of these systematic differences across managers. More specifically, we would like to ask whether boards pay a premium for managers that show performance-enhancing style.

By analogy with our previous approach, we construct manager specific compensation fixed effects that are net of firm fixed effects and other time-varying firm characteristics. We estimate a compensation regression at the manager level where we control for firm fixed effects, year fixed effects, the logarithm of total assets, the logarithm of total sales and the rate of return on assets. We also include dummy variables for whether the manager is a CEO, a CFO or another type of top executive. More specifically, we estimate the following regression:

$$log(comp)_{ijt} = \beta X_{ijt} + \lambda(CEO) + \delta(CFO) + \alpha_t + \gamma_i + \epsilon_{ijt} \tag{3}$$

In a second step, we form residual compensation measures for each executive. We then regress these residuals on the fixed effects from the executive style regressions. We use the GLS type adjustment described above to account for the measurement error in the right hand side variables.

Table 8 shows some interesting patterns. Most importantly, in column (8) of this table, we find that managers with higher return on assets fixed effects receive higher residual compensation. This relationship is statistically significant. The point estimate is 0.72 with a standard error of 0.24. It is particularly interesting that we find such a strong positive correlation here given that we have already controlled for return on assets in equation 3 and have therefore netted out the well known pay-for-performance relationship. Firms pay a premium for managers that are associated with higher rates of return on assets. This result suggests that there is some awareness by corporate boards of these systematic managerial differences in performance.[26]

With respect to the specific corporate variable fixed effects, the picture is a little murkier as to whether boards compensate more managers for what appears as performance-enhancing style. Higher investment-Q sensitivities, which we showed above to be positively related to performance fixed effects, are also positively related to compensation levels. This is also true of advertising expenditures. Managers that choose to hold more cash also have lower performance fixed effects; this negative relation also appears in the compensation regression, but it is far from statistically significant. Interestingly, managers with high levels of acquisition and diversification activity earn a compensation premium. On the other hand, we saw in Table 7 that the acquisition and diversification fixed effects are in fact negatively related to the return on assets fixed effects.

6 Observable Managerial Characteristics

6.1 Motivation

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,

[26] In regressions not reported here, we also looked at the relation between manager fixed effect in performance and *change* in compensation. If firms were to learn over time that certain managers are particularly successful in creating value, one would expect that these managers would have a bigger increase in pay from their first to their second job. Interestingly we did not find any relationship between the return on assets fixed effects and changes in compensation. This could indicate that wages of managers with perceived better styles might have already been already bid up in his first job and that a lot of the learning about managers' type happens earlier in their career.

which specific managerial traits or characteristics might influence their decision-making. In this section, we analyze the possible role of two such managerial characteristics: education and birth cohort/age.

Educational background could be an important factor in managerial decision-making. We are able to collect information on an important element of top executives' education: whether or not the executives went to business school. It is likely that MBA graduation affects managerial decision-making either through human capital accumulation or because of a selection effect. During the two years of training in business school, MBA students accumulate knowledge about best practices in fields such as finance, strategy, marketing or operation management.[27] Also, individuals that decide to attend business school may have a different attitude towards risk and discounting that may influence, for example, their financial practices once at the top of the corporate ladder. It therefore seems relevant to ask whether business school training systematically influences the type of decisions managers make. In practice, we will consider both the effect of MBA graduation and the effect of the specific business school attended.[28]

The second managerial characteristic we consider is birth cohort. It has been anecdotally suggested that older generations of managers may behave differently from younger generations. One specific dimension where these differences have been highlighted is financial decisions. Some have argued that older generations are less trustful of external sources of finance and might prefer to rely on internal capital markets (Chew (1998)). More generally, older generations are often believed to be more financially conservative.

Obviously, the two specific managerial characteristics we propose to study 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

[27] Besides human capital accumulation, business schools are also believed to play an important role in the accumulation of social capital. MBA students develop social and professional networks that may also affect some of their decisions. For example, MBA graduates may have more contacts in other firms and industries, which may facilitate any acquisition or diversification attempt.

[28] Clearly, these two variables in no way capture the full richness of the MBA graduation effect. For example, one would also like to know the specific field an executive concentrated in (e.g. finance versus marketing) and obtain finer information about grades and overall performance while in business school. Unfortunately, there is no easy way for us to gather this information for a large number of executives. Similarly, we could not obtain detailed information on college majors for a wide range of top executives. While most top executives in the United States hold a college degree (92 percent of the CEOs in the sample we study below hold an undergraduate degree), the specific undergraduate major may matter for their decision-making.

exercise we can perform.

6.2 Sample Construction and Summary Statistics

For this section of the study, we limit ourselves to a sample of CEOs only.[29] 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 (year of birth, MBA graduation, business school attended) for these CEOs. The first data source we consult is the *S&P Directory of Corporate Executives.* We then turn to a second data source, *Who is Who of Corporate America,* to try to fill in as much of the missing information as possible. In the end, we are able to find MBA information (whether completed or not) for about 65 percent of the CEOs and birth cohort information for about 75 percent of the CEOs.[30] 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 section 3.1). Means and summary statistics for this sample are reported in columns (3) and (4) of Table 1.

Perhaps somewhat surprisingly, the fraction of CEOs that have completed an MBA is not very large. Only about 40 percent of the CEOs that we are able track completed business school. The distribution of business schools attended is consistent with prior intuition. The most highly represented business school is Harvard Business School (25 percent of the sample). More than 50 percent of the CEOs holding an MBA graduated from one of the following top 8 universities: Harvard University, Stanford University, University of Pennsylvania, Columbia University, MIT, University of Chicago, NYU, and University of Michigan. The average CEO in our sample is born in 1928. The earliest year of birth is 1884 and the latest is 1966. Also, this variation does not merely reflect a time trend. For example, in 1990, the oldest CEO is born in 1904 while the youngest CEO was born in 1952. Even within a year, there is a large amount of variation in the birth cohort of the CEOs. We also find that younger generations are more likely to have attended business school.

[29] We originally tried to collect background information for top executives other than CEOs. Unfortunately, these other top executives are much less likely to be represented in the two data sources we consulted.

[30] We are relatively more successful in finding birth cohort information as some of this information was readily available in Execucomp.

6.3 Empirical Methodology

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

$$y_{ijt} = \beta X_{it} + \delta MBA_j + \eta Cohort_j + \alpha_i + \lambda_t + \epsilon_{ijt} \tag{4}$$

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 CEOj, α_i are firm fixed effects, λ_t are year fixed effects and ϵ_{ijt} is an error term. We allow for clustering of the error term at the individual manager level.

There are two points worth emphasizing about equation (4). First, equation (4) includes firm fixed effects. Our identification is therefore not driven by average differences across firms in corporate decisions. Othewise we might worry that firms that tend to hire CEOs with MBA degrees or younger CEOs are systematically different from firms that do not. For example, one might imagine that firms in the high tech sector have younger CEOs than firms in the consumer goods sector. If this was the case, ignoring firm (and industry) effects would lead us to unduly attribute differences in ,say, financial leverage, to differences in managerial characteristics. By controlling for firm fixed effects, we instead ask, given the average level of financial leverage in a firm, whether that level is systematically higher when the CEO is an MBA graduate or when the CEO belongs to a younger cohort. In other words, our identification comes from within-firm variation in the MBA status or birth cohort of the CEO.

Second, the identification of equation (4), 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 empirical 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 here 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:

$$
\begin{aligned}
I_{ijt} \quad = \quad & \beta X_{it} + \delta_1 MBA_j + \delta_2 MBA_j * CF_{it}/K_{i(t-1)} + \delta_3 MBA_j * Q_{i(t-1)} \\
& + \eta_1 Cohort_j + \eta_2 Cohort_j * CF_{it}/K_{i(t-1)} + \eta_3 Cohort_j * Q_{i(t-1)} \\
& \alpha_i + \alpha_{i2} * CF_{it}/K_{i(t-1)} + \alpha_{i3} * Q_{i(t-1)} + \lambda_t + \epsilon_{ijt}
\end{aligned}
$$

where $\alpha_{i2} * CF_{it}/K_{i(t-1)}$ is a vector of interactions between firm fixed effects and cash flow availability, $\alpha_{i3} * Q_{i(t-1)}$ 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 (4), equation (5) allows for firm specific differences in investment to cash and investment to Q sensitivities. Given the average sensitivities of investment to cash or Tobin's Q in a firm, we ask whether these sensitivities systematically vary with the MBA status or birth cohort of the CEO.

6.4 Results

Table 9 investigates the effect of MBA graduation and birth cohort on the four investment policy variables: level of capital expenditures, investment to cash flow sensitivity, investment to Q sensitivity and number of acquisitions.[31]

We start in columns (1) to (4) with a study of capital expenditures. Column (1) shows that CEOs from earlier birth cohorts are associated with lower investment levels, everything else equal. Each 10-year increase in year of birth decreases capital expenditures (as a ratio to lagged property, plant and equipment) by about 1 percentage point. Column (2) shows that MBA graduation also matters for capital expenditures. MBA graduates appear to invest more, everything else equal. An MBA degree increases investment by nearly 2 percentage points. In regressions not reported here, we also investigate possible business school effects in the investment decisions. While most effects are imprecisely estimated, we find a significant negative relationship between a Chicago MBA and investment level.

Because birth cohort and MBA graduation are positively correlated, as discussed above, one might worry that the results in columns (1) and (2) suffer from an omitted variable bias. In column (3), we allow for birth cohort and MBA graduation to both affect capital expenditures at the same

[31] In results not reported here, we verify that all of the findings in Tables 9, 10 and 11 are robust if we allow for firm-specific trends in each of the dependent variables.

time. This reduces the size of our sample to about 10,000 observations. The qualitative effect of birth cohort and MBA graduation on investment are however unaffected, even though the effect of MBA graduation becomes noisier.

Finally, in column (4), we include an additional control for the number of years the CEO has been in office. This control should account for career concern or entrenchment effects. Indeed, if career concern and entrenchment also affect investment levels, one might worry that the effect of birth cohort on investment might be biased in the absence of this additional control. We find no statistically significant effect of CEO tenure on investment. In fact, the effect of birth cohort on investment appears even stronger in column (4). Each 10-year increase in the year of birth of a CEO increases investment by nearly 2 percentage points, everything else equal. In summary, our results in columns (1) to (4) are consistent with the idea that younger generations of CEOs and CEOs that have an MBA training invest more, everything else equal.

Columns (5) to (8) consider the effect of year of birth and MBA graduation on investment to cash and investment to Q sensitivities. These results come from the estimation of regression 5. Let's start with the effect of MBA graduation. We find that MBA graduates on average respond more to Tobin's Q and less to cash flow availability when deciding about capital expenditures. The effects are however only statistically significant in columns 7 (and 8), i.e. when we also control for birth cohort (and CEO tenure). 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.

With respect to the effects of birth cohort on investment sensitivities, we find that older generations of CEOs are less responsive to Tobin's Q when deciding about investment level. This finding is robust, even if we control for the effect of tenure on investment to Q sensitivity. 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, in columns (9) to (12), we consider the effect of birth cohort and MBA status on the number of acquisitions. None of these results are statistically significant. It is interesting that the large manager fixed effects we found earlier on acquisition policy cannot at all be explained by these

two individual characteristics. The decision or not to undertake an acquisition must therefore be driven by much more subtle individual traits than simply education or birth cohort. Interestingly, we find that CEOs with Columbia MBAs engage in significantly more acquisitions.

In Table 10, we study the effect of CEO characteristics on financial policy. The first variable we consider is financial leverage. Interestingly, we find that older generations of CEOs choose lower levels of financial leverage, everything else equal. This finding is robust to controlling for MBA graduation and CEO tenure. The effect of birth cohort on financial leverage is also economically large. Each 10-year increase in CEO year of birth increases financial leverage by about 2 percentage points.

To give a better perspective as to how big this effect is, we estimated a regression of financial leverage on firm fixed effects, all the time-varying firm controls included in Table 10 and a linear time trend. As it is already well known, financial leverage has been increasing within firms over time. In our sample, the average annual increase in leverage is about .4 percentage points. We then re-estimated the same regression adding a control for the birth cohort of the CEO. We found that the annual trend coefficient dropped to .2 percentage points per year. In other words, about 50 percent of the linear increase in leverage can be eliminated by controlling for CEOs' birth cohort.

The point estimate on the effect of MBA graduation on financial leverage is consistently positive. In other words, CEOs that are MBA graduates, like CEOs from later birth cohorts, are associated with higher levels of financial leverage. The size of the effect varies from 1 to 1.7 percentage points. When we look at specific school effects, we observe that only one school, MIT, is associated with an especially large positive effect on leverage.

Consistent with the leverage results above, we show in columns (5) to (8) that interest coverage is higher among CEOs from earlier birth cohorts. However, we find no systematic relationship between MBA graduation and birth cohort.

The third corporate decision we study are cash holdings (columns (9) to (12)). The stronger patterns appear here again for birth cohort effects. There is a stable and statistically 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 statistically insignificant. This is surprising

as one might have suspected that the financial training received in business school would have warned against the cost of holding too much liquid assets. Interestingly, when we consider school specific effects, we find that 2 schools, Harvard and Stanford, are associated with large levels of cash holdings. Once we control for Harvard and Stanford graduates cash holdings are statistically negatively related to holding and MBA degree.

Finally, columns (13) to (16) investigate the effect of managerial characteristics on dividend policy. While we find no robust relationship between dividends over earnings and birth cohort, there is a robust negative correlation between dividend payout and whether the CEO is an MBA graduate. Two schools, Wharton and NYU, are associated with especially low dividend payments.

Table 11 focuses on our organizational strategy variables and performance. We first study diversification policy, controlling for number of acquisitions. Our results suggest that CEOs with MBA degrees, and CEOs from earlier cohorts, have a stronger tendency to engage in diversification moves. Columns (5) to (8) look at research and development expenditures. Here again, it does appear that year of birth and MBA graduation of the CEO have a systematic effect on R&D expenditures. More specifically, younger generations and MBA graduates engage in less R&D. We find no consistent relationship between advertising expenditures (columns (9) to (12)) or SG&A (columns (13) to (16)) and the CEO characteristics. 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.

In summary, our results in Tables 9, 10 and 11 suggest that the important 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. Some general patterns emerge from our analysis in these last three tables. CEOs with MBAs appear to be on average more aggressive, choosing to engage in higher level of capital expenditures, hold more debt and pay less dividends. CEOs from older generations appear to be less aggressive on average, choosing lower level of capital expenditures, lower financial leverage and higher cash holdings.

For the sake of completeness, we also investigate in the last four columns of Table 11 the effect of MBA status and birth cohort on accounting performance. The most interesting finding is the positive relationship between MBA graduation and corporate performance. Rates of return on assets increase by more than 1 percentage point on average when firms move from a CEO without

an MBA to a CEO with an MBA. We find that three schools appear consistently associated with higher levels of corporate performance: Columbia, Stanford and Wharton.

7 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 the manager dimension in the observed unexplained variation in several corporate practices. 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.

We find considerable heterogeneity across managers. The realizations of all investment, financing and other organizational strategy variables we look at appear to systematically depend on the specific executives in charge. We also show within that empirical framework that some of the managerial differences in corporate practices are systematically related to differences in corporate performance and differences in managerial compensation. Another finding of this paper is that these differences in behavior across managers can in part be tied back to managers' observable characteristics. We concentrate on birth cohort and MBA graduation. We show that older generations of managers are on average financially more conservative, while managers who hold an MBA degree follow more aggressive strategies.

We plan to extend the results of this paper in several directions in the future. First, we want to further pursue our investigation of whether, and how much, investors and firms are aware of these managerial differences. For example, as noted earlier, the finance literature has found so far ambiguous stock price responses to CEO turnover. It would be interesting to ask whether more definite patterns emerge in such event studies once one accounts for the style and overall ability of the newly appointed CEOs, as measured in this paper. Second, we plan to study in further details the efficiency of the matching process between firms and managers. The evidence we put together so far is not unambiguously supportive of the idea that firms entirely account for style effects in their hiring decisions. We are currently collecting information on the nature of managerial turnover to help further illuminate this question, e.g. are there differences in style between voluntary and

involuntary turnovers and also can we find differences in styles for managers that are hired by more or less stringently governed firms. Finally, if managerial style effects are indeed large and have implications for corporate performance, one may need to invest further effort in figuring out which other specific managerial traits, besides education and birth cohort, help us better predict corporate policies. In particular, it would be important to understand at which point in a manager's career specific preferences and behaviors take form.

References

Barberis, Nicholas, Maxim Byocko, Andrei Shleifer and Natalia Tsukanova, 1996, How Does Russian Privatization Work? Evidence from the Russian Shops, *Journal of Political Economy.*

Berger, Philip, and Eli Ofek, 1995, Diversification's Effect on Firm Value, *Journal of Financial Economics.*

Bradley, Michael, Gregg A. Jarrell and E. Han Kim, 1984, On the Existence of an Optimal Capital Structure, *Journal of Finance.*

Campa, Jose Manuel, and Simi Kedia, 1999, Explaining the Diversification Discount, *Working Paper, Harvard Business School.*

Chevalier, Judith and Glenn Ellison, 1999, Are Some Mutual Fund Managers Better than Others? Cross-Sectional Patterns in Behavior and Performance, *Journal of Finance.*

Chew, Donald H. Jr., *The New Corporate Finance* (Irwin/McGraw-Hill, 1998).

DeAngelo and DeAngelo, 1989, Proxy Contests and the Governance of Publicly Held Corporations, *Journal of Financial Economics.*

Fazzari, Steven, R. Glenn Hubbard and Bruce Petersen, 1987, Financing Constraints and Corporate Investment, *Brookings Papers on Economic Activity.*

Gilson, Stuart C., 1989, Management Turnover and Financial Distress, *Journal of Financial Economics.*

Gilson, Stuart C., 1990, Bankruptcy, Boards, Banks and Blockholders: Evidence on Changes in Corporate Ownership and Control when Firms Default, *Journal of Financial Economics.*

Hambrick, D. and P. Mason, 1984, Upper Echelons: The Organization as a Reflection of its Top Managers, *Academy of Management Review.*

Graham, John and Campbell Harvey, 2001, The Theory and Practice of Corporate Finance: Evidence from the Field, *Journal of Financial Economics.*

Kaplan, Steve and Luigi Zingales, 1997, Do Investment-Cashflow Sensitivities Provide Useful Measures of Financing Constraints?, *Quarterly Journal of Economics.*

Kesner, Idalene F. and Terrence Sebora, 1994, Executive Succession: Past, Present and Future, *Journal of Management.*

Lieberman, Marvin, Lawrence Lau and Mark Williams, 1990, Firm-Level Productivity and Management Influence: A Comparison of U.S. and Japanese Automobile Producers, *Management Science.*

McKay, Peter and Gordon Phillips (2001), Is There an Optimal Industry Capital Structure?, *Unpublished Working Paper.*

Parrino, Robert, 1997, CEO Turnover and Outside Succession: A Cross-Section Analysis, *Journal of Financial Economics.*

Saloner, Garth and Julio J. Rotemberg, 2000, Visionaries, Managers and Strategic Direction, *RAND Journal of Economics.*

Smith, Clifford W. and Ross L. Watts, 1992, The Investment Opportunity Set and Corporate Financing, Dividend and Compensation Policies, *Journal of Financial Economics.*

Thomas, A., 1988, Does Leadership Make a Difference to Organizational Performance, *Administrative Science Quarterly.*

Titman, Sheridan and Roberto Wessels, 1988, The Determinants of Capital Structure, *Journal of Finance.*

Vancil, Richard F., 1987, *Passing the Baton: Managing the Process of CEO Succession* (Cambridge, MA: Harvard Business School Press).

Van Den Steen, Eric, 2001, Organizational Beliefs and Managerial Vision, *MIT Sloan Working Paper* No. 4224-01.

Waldam, David, Gabriel G. Ramirez, Robert J. House and Phanish Puranam, 2001, Does Leadership Matter? CEO Leadership Attributes and Profitability under Conditions of Perceived Environmental Uncertainty, *Academy of Management Journal.*

Warner, Jerold B., Ross L Watts and Karen Wruck, 1988, Stock Prices and Top Management Changes, *Journal of Financial Economics.*

Weiner, N. and T.A. Mahoney, 1981, A Model of Corporate Performance as a Function of Environmental, Organizational and Leadership Influences, *Academy of Management Journal.*

Weisbach, 1995, CEO Turnover and the Firm's Investment Decisions, *Journal of Financial Economics.*

Table 1: Descriptive Statistics[a]

Sample:	Manager-Firm Matched Sample		Manager Characteristics Sample		Compustat	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Total Sales	4,055	5,221	3,544	8,749	1,799	2,130
Investment (I_t/K_{t-1})	0.29	0.20	0.30	0.28	0.40	0.78
Average Tobin's (Q_t)	2.10	1.51	2.34	2.40	1.70	1.02
Cash Flow (CF_t/K_{t-1})	0.44	0.39	0.48	0.77	0.47	0.41
N of Acquisitions	0.65	0.97	0.65	1.37	0.65	1.42
Leverage	0.34	0.21	0.33	0.22	0.39	0.65
Interest Coverage	16.0	21.7	30.9	97.2	17.6	27.5
Cash Holdings ($Cash_t/K_{t-1}$)	0.10	0.12	0.10	0.13	0.09	0.08
Dividends/Earnings	0.10	0.09	0.12	0.11	0.11	0.10
N of Diversifying Acquis.	0.26	0.57	0.25	0.83	0.24	0.81
$R\&D_t/K_{t-1}$	0.05	0.05	0.04	0.08	0.04	0.04
$Advertising_t/K_{t-1}$	0.05	0.04	0.05	0.06	0.04	0.03
$SG\&A_t/S_t$	0.23	0.14	0.22	0.16	022	0.14
Return on Assets	0.16	0.07	0.19	0.12	0.13	0.07

[a]

1. "Manager-Firm Matched Sample" describes the sample of firms for which we can observe at least one manager in at least one other firm over the sample period (see text for detail). "Manager Characteristics Sample" describes the sample of firms for which we could obtain information either on year of birth or MBA education for the CEO (see text for detail). "Compustat" is a comparison sample of the largest 1500 listed firms over the period 1969 to 1999.

2. "Total sales" is COMPUSTAT item 12. I_t/K_{t-1} (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). CF_t/K_{t-1} (Cash Flow) is 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). 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). $Cash_t/K_{t-1}$ (Cash Holdings) is cash and short-term investments (COMPUSTAT item 1) over net property, plant and equipment at the beginning of the fiscal year (COMPUSTAT item 8). $R\&D_t/K_{t-1}$ is R&D expenditures (COMPUSTAT item 46) over lagged total assets (COMPUSTAT item 6). $Advertising_t/K_{t-1}$ is advertising expenditures (COMPUSTAT item 45) over lagged total assets (COMPUSTAT item 6). $SG\&A_t/S_t$ is selling, general and administrative expenses (COMPUSTAT item 189) over total sales (COMPUSTAT item 12). A diversifying acquisition is defined as an acquisition in an industry different from the 2-digit SIC industry the firm mainly operated in prior to the acquisition. All variables are from Compustat, except for number of acquisitions and diversifications, which are from SDC.

Table 2 : Executive Transitions Between Positions and Industries [a]

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

[a] This table reports the flow of executives between positions and industries in our sample of executive movers. All transitions are across different firms. The first entry in each cell reports the number of transitions in our sample from one executive position to another where the person also changes company. For example, the first entry in the far left column reports the number of CEOs that take a position as a CEO in a different company. The second line in each cell reports the fraction of the reported transitions that are between different 2-digit industries.

Table 3: Executive Effects on Investment Policy[a]

	F-Tests on Fixed Effects for			*Observations*	*Adjusted* R^2
Specification:	*CEOs*	*CFOs*	*Other Executives*		
Investment				6631	.91
Investment	16.74 (< .0001, 198)			6631	.94
Investment	16.43 (< .0001, 197)	39.08 (< .0001, 55)		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	19.39 (< .0001, 224)	6.23 (< .0001, 57)		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	1.60 (< .0001, 203)	2.82 (.0100, 55)		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	1.88 (< .0001, 203)	1.72 (.0008, 55)		6593	.29
N of Acquisitions	1.68 (< .0001, 199)	1.74 (.0006, 55)	4.08 (< .0001, 203)	6593	.36

[a]

1. Sample is the matched manager-firm panel data set as described in Section 3.1 and Table 1. The different dependent variables (as indicated in the leftmost column of the table) are described in Section 3.1 and Table 1. We exclude firms in the banking and energy sectors.
2. Reported are the results from fixed effects panel regressions. For each dependent variable, the fixed effects included, in addition to year fixed effects, are as follows. Raw 1: firm fixed effects; raw 2: firm fixed effects and CEO fixed effects; raw 3: firm fixed effects, CEO fixed effects and CFO fixed effects; raw 4: firm fixed effects, CEO fixed effects, CFO fixed effects and Other Executives fixed effects. For "Investment to Q" and "Investment to Cash Flow" sensitivities, these fixed effects are also interacted with lagged Tobin's Q and cash flow, respectively.
3. Also included in the "Investment," "Investment to Q" and "Investment to Cash Flow" regressions are lagged logarithm of total assets (interacted with year fixed effects), lagged Tobin's Q (interacted with year fixed effects) and cash flow (interacted with year fixed effects). Also included in the "Number of Acquisitions" regressions are lagged logarithm of total assets (interacted with year fixed effects) and return on assets (interacted with year fixed effects).
4. 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 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. Also reported in the table are the number of observations (column 5) and adjusted R^2s (column 6) for each regression.

Table 4: Executive Effects on Financial Policy[a]

	F-Tests on Fixed Effects for			*Observations*	*Adjusted* R^2
Specification:	*CEOs*	*CFOs*	*Other Executives*		
Leverage				6563	.39
Leverage	0.99 (.5294, 203)			6563	.39
Leverage	1.00 (.4893, 203)	1.25 (.1004, 54)		6563	.40
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	0.26 (.99, 193)	13.11 (< .0001, 51)		6278	.38
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	2.46 (< .0001, 204)	3.48 (< .0001, 54)		6592	.79
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	5.86 (< .0001, 202)	.94 (.6102, 55)		6580	.71
Dividends/Earnings	4.95 (< .0001, 199)	1.07(.3368, 54)	1.74 (< .0001, 203)	6580	.72

[a]

1. Sample is the matched manager-firm panel data set as described in Section 3.1 and Table 1. The different dependent variables (as indicated in the leftmost column of the table) are described in Section 3.1 and Table 1.
2. Reported are the results from fixed effects panel regressions. For each dependent variable, the fixed effects included, in addition to year fixed effects, are as follows. Raw 1: firm fixed effects; raw 2: firm fixed effects and CEO fixed effects; raw 3: firm fixed effects, CEO fixed effects and CFO fixed effects; raw 4: firm fixed effects, CEO fixed effects, CFO fixed effects and Other Executives fixed effects.
3. Also included in each regression are the logarithm of total assets (interacted with year fixed effects), return on assets (interacted with year fixed effects) and cash flow (interacted with year fixed effects).
4. 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 the number of constraints. Also reported in the table are the number of observations (column 5) and adjusted R^2s (column 6) for each regression.

Table 5: Executive Effects on Organizational Strategy and Performance[a]

	F-Tests on Fixed Effects for			*Observations*	*Adjusted* R^2
Specification:	*CEOs*	*CFOs*	*Other Executives*		
N of Diversifying Acquis				6593	.22
N of Diversifying Acquis	2.06 (< .0001, 204)			6593	.25
N of Diversifying Acquis	1.99 (< .0001, 204)	1.68 (.0014, 54)		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	2.11 (< .0001, 145)	4.04 (< .0001, 45)		4283	.80
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	2.84 (< .0001, 95)	0.75 (.7856, 21)		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	33.41 (< .0001, 121)	0.23 (.99, 43)		2397	.83
SG&A	13.80 (< .0001, 118)	0.82 (.7934, 42)	0.77 (.9777, 146)	2397	.83
Return on Assets				6593	.72
Return on Assets	2.04 (< .0001, 217)			6593	.74
Return on Assets	2.15 (< .0001, 203)	3.58 (< .0001, 55)		6593	.74
Return on Assets	2.46 (< .0001, 201)	3.39 (< .0001, 54)	4.46 (< .0001, 202)	6593	.77

[a]

1. Sample is the matched manager-firm panel data set as described in Section 3.1 and Table 1. The different dependent variables (as indicated in the leftmost column of the table) are described in Section 3.1 and Table 1.
2. Reported are the results from fixed effects panel regressions. For each dependent variable, the fixed effects included, in addition to year fixed effects, are as follows. Raw 1: firm fixed effects; raw 2: firm fixed effects and CEO fixed effects; raw 3: firm fixed effects, CEO fixed effects and CFO fixed effects; raw 4: firm fixed effects, CEO fixed effects, CFO fixed effects and Other Executives fixed effects.
3. Also included in the "N of Diversifying Acquisitions," "R&D," "Advertising" and "SG&A" regressions are the logarithm of total assets (interacted with year fixed effects), return on assets (interacted with year fixed effects) and cash flow (interacted with year fixed effects). The "N of Diversifying Acquisitions" regressions also include a dummy variable as a control for whether the firm undertook any acquisition in that year. Included in the "Rate of Return" regressions are the logarithm of total assets (interacted with year fixed effects).
4. 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 the number of constraints. Also reported in the table are the number of observations (column 5) and adjusted R^2s (column 6) for each regression.

Table 6: Size Distribution of Manager Fixed Effects[a]

	Median	25th Percentile	75th Percentile
Investment	0.00	-0.09	0.11
Inv to Q Sensitivity	0.00	0.18	0.20
Inv to CF Sensitivity	0.08	-0.59	0.30
N of Acquisitions	-0.03	-0.49	0.44
Leverage	0.02	-0.05	0.11
Interest Coverage	0.00	-72.8	66.0
Cash Holdings	0.00	-0.03	0.02
Dividends/Earnings	0.00	-0.69	0.53
N of Diversifying Acquis	-0.01	-0.28	0.27
R&D	0.00	-0.1	0.02
SG&A	0.00	-0.02	0.04
Advertising	0.00	-0.01	0.01
Return on Assets	0.00	-0.03	0.03

[a]The fixed effects are those retrieved from the regressions of Tables 3 to 5 (last row for each of the dependent variable). Column (1) reports the median fixed effect. Column (2) and (3) report the fixed effects at the 25th percentile and 75th percentile of the distribution.

Table 7: Relationship Between the Manager Fixed Effects[a]

Dependent Variable: Executive Fixed Effects for Different Dependent Variables

	Investment	Inv to Q	Inv to CF	Cash Holdings	Leverage	R&D	Return on Assets
Investment							0.00
							(0.00)
Inv to Q	**6.8**						**0.03**
	(0.92)						**(0.01)**
Inv to CF	0.02	**-0.23**					-0.01
	(0.6)	**(0.11)**					(0.01)
Cash Holdings	-1.10	-0.79	-0.46				**-0.12**
	(1.62)	(1.71)	(1.72)				**(0.05)**
Leverage	-0.39	-0.28	-0.63	**-0.40**			-0.02
	(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**	-0.01	-0.01	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**	-0.14	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. 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 are weighted by the inverse of the standard errors on the independent variable. Coefficients that are significant at the 10% level are highlighted in bold.

Table 8 : Compensation Levels and Manager Fixed Effects[a]

Dep. Var:	*Residual Compensation*								
	(1)	(1)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Investment	**0.02**								0.01
	(0.01)								(0.01)
Inv to Q Sensitivity		**0.08**							0.02
		(0.03)							(0.04)
Inv to Cash Sensitivity			-0.06						-0.02
			(0.04)						(0.10)
Leverage				0.04					0.02
				(0.26)					(0.30)
Cash Holdings					-0.02				-0.01
					(0.15)				(0.20)
N of Acquisitions						**0.10**			0.02
						(0.05)			(0.05)
SG&A							**-0.16**		**-0.14**
							(0.04)		**(0.05)**
Return on Assets								**0.72**	**0.66**
								(0.24)	**(0.32)**

[a] The dependent variable is the residual from a compensation regression at the manager level where we control for firm fixed effects, year fixed effects, the logarithm of total assets, the logarithm of total sales and return on assets, as well as tenure on the job. We also include dummy variables for whether the manager is a CEO, a CFO or another type of top executive. Each observation is weighted by the inverse of the standard errors of the independent variables to account for estimation error in the independent variables. We use heteroscedasticity robust standard errors and cluster at the firm level.

Table 9
CEOs' Birth Cohort and MBA Effects on Investment Policy [a]

Dep. Var:	**Investment**								**N of Acquisitions**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Year of Birth	**.009**	—	**.015**	**.017**	.004	—	-.011	-.006	-.013	—	.016	.001
*(*10)*	(.005)		(.005)	(.005)	(.005)		(.006)	(.008)	(.023)		(.038)	(.037)
MBA	—	**.016**	.015	.016	—	-.003	-.009	-.007	—	-.020	-.021	-.017
		(.009)	(.011)	(.010)		(.009)	(.014)	(.011)		(.046)	(.056)	(.056)
*Year of Birth**	—	—	—	—	**.029**	—	**.107**	**.118**	—	—	—	—
CF_t/K_{t-1}					(.007)		(.018)	(.014)				
*(*10)*												
*MBA**	—	—	—	—	—	-.026	**-.077**	**-.075**	—	—	—	—
CF_t/K_{t-1}						(.017)	(.035)	(.026)				
*Year of Birth**	—	—	—	—	-.003	—	**-.011**	**-.013**	—	—	—	—
Q_{t-1}					(.002)		(.004)	(.003)				
*(*10)*												
*MBA**	—	—	—	—	—	.004	**.017**	**.017**	—	—	—	—
Q_{t-1}						(.003)	(.008)	(.006)				
Tenure as CEO	—	—	—	.003	—	—	—	—	—	—	—	-.018
*(*10)*				(.004)								(.033)
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.* CF_t/K_{t-1}	No	No	No	No	Yes	Yes	Yes	Yes	No	No	No	No
Firm F.E.* Q_{t-1}	No	No	No	No	Yes	Yes	Yes	Yes	No	No	No	No
Tenure as CEO* CF_t/K_{t-1}	No	No	No	No	No	No	No	Yes	No	No	No	No
Tenure as CEO* Q_{t-1}	No	No	No	No	No	No	No	Yes	No	No	No	No
R^2	.514	.423	.428	.470	.821	.806	.759	.813	.429	.408	.404	.404
N obs	15482	12530	10446	10133	15481	12529	10445	10132	16049	12909	10724	10397

[a] Notes:

1. Sample is the sample of firm/year observations for which we could obtain information on the year of birth and MBA graduation of the CEO. The sample is described in Section 6.2. The different dependent variables are described in Section 3.1 and Table 1. We exclude firms in the banking and utility sectors.
2. Each column corresponds to a different regression. Also included in columns (1) to (8) are lagged Tobin's Q, cash flow and the lagged logarithm of total assets. Also included in columns (9) to (12) are return on assets and the lagged logarithm of total assets.
3. Standard errors are in parentheses. Standard errors are corrected for clustering of observations at the individual manager level.

Table 10
CEOs' Birth Cohort and MBA Effects on Financial Policy [a]

Dep. Var:	**Leverage**				**Interest Coverage**				**Cash Holdings**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Year of Birth	**.019**	—	**.023**	**.024**	**-4.54**	—	**-5.54**	**-6.50**	**-.004**	—	**-.004**	**-.005**
*(*10)*	(.004)		(.006)	(.007)	(1.77)		(2.41)	(2.67)	(.001)		(.002)	(.002)
MBA	—	**.017**	.011	.011	—	-.472	.927	.924	—	-.002	-.000	-.001
		(.007)	(.007)	(.008)		(2.68)	(3.39)	(3.41)		(.002)	(.002)	(.003)
Tenure as CEO	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	.719	.701	.683	.681	.589	.559	.543	.550	.772	.799	.778	.770
N obs	16000	12878	10699	10372	15220	12351	10354	10061	16042	12896	10717	10390

Dep. Var:	**Dividends/Earnings**			
	(13)	(14)	(15)	(16)
Year of Birth	-.003	—	.000	.000
*(*10)*	(.002)		(.003)	(.003)
MBA	—	**-.006**	**-.008**	**-.009**
		(.003)	(.003)	(.004)
Tenure as CEO	No	No	No	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
R^2	.660	.685	.662	.654
N obs	16021	12884	10706	10381

[a] Notes:

1. Sample is the sample of firm/year observations for which we could obtain information on the year of birth and MBA graduation of the CEO. The sample construction is described in Section 6.2. The different dependent variables are described in Section 3.1 and Table 1. We exclude firms in the banking and utility sectors.
2. Also included in each regression are the lagged logarithm of total assets, return on assets and cash flow.
3. Standard errors are in parentheses. Standard errors are corrected for clustering of observations at the individual manager level.

Table 11
CEOs' Birth Cohort and MBA Effects on Organizational Strategy and Performance [a]

Dep. Var:	**N of Diversifying Acquis.**				**R & D**				**Advertising**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Year of Birth	-.009	—	**-.021**	**-.036**	**-.003**	—	**-.004**	**-.003**	-.001	—	-.001	-.001
*(*10)*	(.009)		(.012)	(.015)	(.001)		(.002)	(.002)	(.001)		(.002)	(.002)
MBA	—	.023	**.038**	**.040**	—	**-.003**	-.002	-.002	—	.003	.003	.003
		(.015)	(.017)	(.017)		(.002)	(.002)	(.002)		(.003)	(.003)	(.003)
Tenure as CEO	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	.713	.733	.749	.750	.644	.645	.578	.574	.835	.857	.842	.838
N obs	16043	12897	10718	10391	9873	7930	6648	6475	6120	4812	4216	4161

Dep. Var:	**SG&A**				**Return on Assets**			
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Year of Birth	**.002**	—	.002	.002	-.000	-.000	-.003	
*(*10)*	(.001)		(.002)	(.003)	(.003)	(.003)	(.004)	
MBA	—	.000	-.006	-.004	—	**.010**	**.012**	**.012**
		(.003)	(.004)	(.003)		(.005)	(.005)	(.005)
Tenure as CEO	No	No	No	Yes	No	No	No	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	.938	.931	.935	.935	.696	.561	.536	.534
N obs	6025	4979	3496	3272	16049	12909	10724	10397

[a] Notes:

1. Sample is the sample of firm/year observations for which we could obtain information on the year of birth and MBA graduation of the CEO. The sample construction is described in Section 6.2. The different dependent variables are described in Section 3.1 and Table 1. We exclude firms in the banking and utility sectors.
2. Also included in regressions (1) to (16) are the logarithm of total assets, return on assets and cash flow. A dummy variable for whether the firm undertook any acquisition that year is also added as a control in columns (1) to (4). Also included in regressions (17) to (20) are the logarithm of total assets.
3. Standard error are in parentheses. Standard errors are corrected for clustering of observations at the individual manager level.