Managers With and Without Style: Evidence using Exogenous Variation

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Abstract

We study managerial style effects in a large panel of Compustat firms from 1990 to 2007. We find that policy changes after exogenous CEO departures do not display abnormally high levels of variability, casting doubt on the hypothesis that unanticipated idiosyncratic managerial style effects have a substantive impact on corporate policies. For endogenous CEO departures, we detect abnormally large policy changes after forced CEO turnover. These changes are larger in firms that are likely to draw from a deeper pool of replacement CEO candidates based on the firm’s geographic location. This evidence is consistent with the presence of casual style effects that are fully anticipated by the board when choosing a new leader. In contrast to prior work, managers in our sample do not appear to adopt a common relative-to-firm style bias across multiple employers. We also offer cautionary evidence on the use of standard F-tests to detect style effects.

JEL Classification: G30; G31; G32

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1. Introduction

Recent empirical evidence suggests that manager-specific characteristics and preferences may play an important role in a variety of corporate decisions including investment and financing policies. If this is the case, we may be unable to fully understand corporate policies without uncovering the factors that govern the distribution and manifestation of various manager-specific traits or “styles.” The presence of manager-specific effects in observationally identical economic situations suggests the interesting possibility that major firm policy decisions may be governed by much more than traditional economic tradeoffs and incentives.

The prior literature reports several findings that are consistent with the presence of substantive managerial style effects. Much of this evidence identifies style from the presence of policy changes after a change in a firm’s top management.\(^1\) While this evidence is both interesting and informative, most top management changes are highly endogenous events. This endogeneity makes it difficult to determine whether managerial style plays a causal role in a firm’s choices and/or performance. It is certainly possible that firms/boards decide to simultaneously make a large set of major changes, including changing the firm's leadership along with its major investment and financing decisions. Variants of this logic could explain many of the prior findings on the role of managerial style.

In this paper we attempt to present a more complete picture of the causal role of managerial style in corporate policies and performance by exploiting certain exogenous aspects of turnover events. Our goal is to provide new insights into the role of style in corporate decisions and in a firm’s selection of CEO. We also consider the related issue of whether style is

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\(^1\) The study by Bertrand and Schoar (2003) directly attempts to identify style effects in corporate policies and has spawned a large set of related studies. Several prominent earlier papers offer related evidence, including the studies of Murphy and Zimmerman (1993), Denis and Denis (1995), and Weisbach (1995).
an innate/fixed managerial trait or alternatively a function of past leadership experiences. We refer to the possibility of no substantive causal style effects in firm policy choices as the lack of style hypothesis. We contrast this with the possibility that there are causal managerial style effects that are idiosyncratic and not fully anticipated/observed/controlled by the board (the idiosyncratic style hypothesis), and/or the possible presence of causal style effects that are deliberately selected by the board to induce the corporation to move in a certain direction (the selected style hypothesis). We conduct this analysis in a large and comprehensive sample of Compustat firms from 1990 to 2007.

We first focus our attention on a sample of exogenous CEO departures. These departures include events precipitated by a death or health problem, augmented in some cases by natural retirements. If managerial style plays a causal role in firm policy choices, and if there is some random unanticipated variation in the distribution of styles within the pool of potential replacements, we would expect to observe abnormally large changes in firm polices/performance after exogenous leadership transitions. Interestingly, we fail to find any convincing evidence of this behavior. After exogenous departures, new CEOs do not make changes that display any directional drift or abnormal variability compared to CEOs at matched firms. Further, the estimated upper confidence bound on any potential style effect that may exist is small. We reach similar conclusions when we examine subsets of firms for which we suspect that managerial style would be particularly important. We conclude that executives at firms with exogenous leadership transitions appear to display a profound lack of style, casting substantial doubt on the idiosyncratic style hypothesis.

We next turn to endogenous leadership departures in the form of forced turnover. In these cases, we find strong evidence of abnormally large policy and performance changes when a
new CEO takes the helm. This behavior may reflect a causal channel in which a new manager imparts his/her own style into a firm's decisions. However, it is also possible that this behavior reflects the board's decision to actively change the firm's direction, and/or the policies that any manager would choose given the special, and partially unobservable to the econometrician, economic circumstances associated with endogenous turnover events. Thus, this evidence alone, while suggestive and consistent with prior work, does not offer unambiguous support for or against any of the relevant hypotheses.

To learn more from the endogenous CEO departures, we consider exogenous features of the pool of potential CEO replacements. In particular, motivated by the work of Yonker (2011), we separate firms based on the likely depth of the pool of replacement CEOs using headquarters’ location population (dense versus sparse) and weather (sunny versus cloudy). If managers are deliberately selected because of an identified style that the board wants to harness, we would expect boards to be more successful in doing so when the pool of choices is relatively deep. Under many circumstances, this leads to the prediction that changes in firm polices after endogenous turnover will be more variable when the firm has a deeper labor pool. When we turn to the data, we find evidence supporting this prediction.

Our evidence on exogenous changes casts doubt on the idiosyncratic style hypothesis, while the evidence on endogenous changes is consistent with a selected style hypothesis in which styles are deliberately chosen by the board to move the firm in a certain direction. This raises the issue of how styles are identified by a board in the selection of a manager. Bertrand and Schoar’s (2003) evidence on job movers suggests that style is an innate/fixed characteristic that can be observed by examining the policy choice biases of an executive at his/her employers. Within our sample, we are able to replicate the Bertrand and Schoar (2003) main result that
moving managers display significant individual fixed-effects as indicated by F-tests on a joint set of manager-specific dummy variables.

While this evidence on job movers suggests that managerial styles are innate/fixed characteristics, we have some methodological concerns with the use of F-tests to detect individual styles. To investigate whether these concerns are warranted, we experiment by scrambling the actual data on the identities of moving managers and by simulating data in a manner that incorporates no actual style effects. The associated F-statistics on these samples are almost identical to the tests on the true sample. This evidence strongly suggests that F-tests on manager-specific dummy variables are not valid indicators of managerial style effects.

To investigate further, we consider a robust testing strategy advanced by Bertrand and Schoar (2003) in which the estimated manager effect (above and beyond any firm effect) at a new employer is regressed against the estimated manager effect at the prior employer. While Bertrand and Schoar (2003) report evidence of a strong relation, we do not uncover any parallel evidence in our substantially larger and more diverse sample. Thus, we are unable to find any convincing evidence that managerial style can be observed by examining manager-level biases in policy choices at prior employers, casting some doubt on the idea that style is a fixed or innate individual characteristic. However, if we modify this examination by excluding firm fixed effects and replacing these effects with coarser controls, we do find some evidence of correlated firm-level policy choice biases across multiple employers of the same individual manager. This evidence suggests that experiences matter in the selection of corporate leaders and that styles

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2 Our concerns about endogeneity also apply to any sample of job movers. Firms that intend to move in a certain direction, say high growth, may hire someone with past experience with high growth, even if any replacement manager would move the firm in the same direction given its economic fundamentals. We discuss this subtle issue of perceived versus actual style below.
developed by working in certain environments are relevant for working in similar new environments.

Putting this all together, we believe we offer a more nuanced and complete picture of managerial style effects than the prior literature would suggest. First, we find very little evidence of idiosyncratic style effects. This suggests that random managerial traits that are hard for a board to predict or observe do not explain a large portion of the variation of some of the major policy choices of interest to financial economists. Managerial style does not appear to be the key missing ingredient for more fully explaining the determinants of investment and financing decisions, at least at a coarse level. While we find little evidence of idiosyncratic style effects, our evidence is consistent with a causal relation between style and firm policies and with the board anticipating these effects in their choice of replacement managers. This behavior suggests that the underlying mechanisms governing firm policies should reflect the economic and governance factors at the board/firm level, rather than at the CEO level. This in turn indicates that our ultimate understanding of these policies should focus on traditional factors, with some attention to the role of leaders, and the pool of potential leaders, as a conduit to implement any desired change.

The rest of the paper is organized as follows. In section 2 we discuss the related literature, situate our study, and motivate the hypotheses and empirical strategy. In section 3 we discuss and describe our sample and sampling procedures. In section 4 we consider the role of style around exogenous management departures, while in section 5 we conduct our analysis of policy changes at the time of endogenous management departures. In section 6 we examine style portability within a set of managers who move across firms. Section 7 offers concluding thoughts.
2. Leadership Styles

2.1 Sources of variation in managerial styles

The notion that managerial traits or preferences can affect firms’ decisions has a long tradition in economics, finance, and management. The literature identifies many managerial characteristics that may generate heterogeneity in actions in observationally identical situations. Rotemberg and Saloner (1993, 1994, 2000) discuss leadership styles and illustrate how heterogeneity in managerial utility functions can lead to style effects in project choices. Similarly, Aggarwal and Samwick (2003, 2006) hypothesize that variations in managerial risk aversion can lead to variations in investment and acquisition choices.

In a different vein, the work of Shleifer and Vishny (1989) and Morck, Shleifer, and Vishny (1990) suggests that variations in specific or general managerial abilities may affect what types of investments managers undertake. On the behavioral front, several papers follow Roll (1986) and posit that managers may differ in their perceptions of the relative payoffs from different actions. Of particular note, the work of Malmendier and Tate (2005a, 2005b, 2008), Goel and Thakor (2008), and Itzakh, Graham, and Harvey (2010) suggests that managers may vary in their degree of overconfidence, and this variation can translate into observed differences in investment, financing, and acquisition decisions.

2.2 Empirical evidence on managerial style

Prior empirical studies can be loosely assigned to two groups. One group examines whether certain managers are associated with an increase or decrease in performance or a policy variable of interest. We will refer to these as directional studies. A second group considers
whether managers tend to display an idiosyncratic individual effect in policies or performance, even if the mean/median effect is zero. We will refer to these as non-directional studies.

In an important early directional study, Murphy and Zimmerman (1993) study changes in investment and performance subsequent to CEO turnover. They detect some changes, but conclude that they are largely driven by the poor performance underlying the endogenous decision to replace the CEO. Related findings are reported by Denis and Denis (1995), who report that management dismissals often result in decreased investment spending and increased accounting profitability. Weisbach (1995) finds that divestitures are particularly likely after a CEO turnover event, both for forced turnover and mandatory retirements. Finally, in a large sample of Danish firms, Bennedsen, Pérez-González, and Wolfenzon (2007) find that firms tend to cut investment and display decreases in profitability subsequent to a leader's death. Given the differing character of some of our findings, we will discuss our results further in the specific context of these last two studies when interpreting our results.

While directional evidence is informative and can detect some types of style effects, it may be poorly suited for identifying these effects in many settings. In particular, if replacement managers are drawn from approximately the same style distribution as departing managers, there will likely be no significant average directional change in a given variable of interest. Non-directional tests focusing on the absolute magnitude of changes have the potential to be more informative in these contexts.

In an important non-directional study, Bertrand and Schoar (2003) examine the role of manager-specific effects in several corporate policies. If new managers tend to shift corporate choices, this should manifest itself in a significant test statistic on a joint set of dummy variables.
indicating new management in a regression explaining a choice variable of interest. Bertrand and Schoar (2003) impose the additional constraint that manager-specific dummy variables are common across employers for managers serving at multiple firms. Using this approach, they find evidence of style effects in investment spending, acquisitions, leverage, and accounting profitability. Using related modeling techniques, others have found evidence of manager-level style effects in leverage choices (Frank and Goyal (2007)), compensation levels (Graham, Li, and Qiu (2009)), accounting/tax choices (Bamber, Jiang, and Wang (2010), Dyreng, Hanlon, and Maydew (2010)), and performance variability (Adams, Almeida, and Ferreira (2005)).

While this prior work is informative, there are some interpretation issues. In particular, since firms may select new leaders precisely when they have decided to make other changes, it is unclear whether any altered firm choices are a reflection of a new manager's specific style. It is possible that the board would insist that any new leader make the same set of prescribed changes and/or that the economic circumstances that lead to a turnover event would cause any leader to make the same set of changes. Our study is intended to circumvent some of these interpretation issues arising from endogeneity concerns. A second concern with many prior studies is methodological. In particular, the variables considered are generally highly serially correlated, leading to potentially serious inference issues (see Bertrand, Duflo, and Mullainathan (2004)). Moreover, the number of manager-specific dummy variables in these models explodes as the sample gets larger. Consequently, standard asymptotic theory does not apply to tests on these variables, and the properties of standard F-tests for joint significance of the coefficients on these variables are unknown (Wooldridge (2002)).

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3 Since fixed-effects tests have natural first-difference counterparts, these analyses can be viewed as tests of whether there are abnormally large changes in a variable after a management change. First-difference tests of this type have been used in the economics literature on political leadership styles (e.g., Jones and Olken (2005)).
2.3 The process of choosing managers

A large literature considers the process of firing and hiring managers. Firms tend to dismiss managers when performance is poor (Weisbach (1988), Warner, Watts and Wruck (1988), Fee and Hadlock (2004)). The replacement is more likely to be an outsider when firm performance is poor (Parrino (1997), Huson, Parrino, and Starks (2001)). Outside CEOs who have led other firms tend to come from smaller, high-performing firms (Fee and Hadlock (2003)), and it is generally bad news to lose a top executive to another firm (Hayes and Schaefer (1999)). Hiring firms often select executives with specific skills and characteristics (Kaplan, Klebanov, and Sorensen (2011)). Collectively, this literature establishes that management changes are often highly endogenous events and that executives who move across firms are a highly select group.

Given the endogeneity of CEO selection, it is difficult to separately identify the effect of a new leader on firm policies from the effect of the board's directive for the firm to pursue a new direction. To circumvent the endogeneity issue, several studies exploit the approach of Johnson, Magee, Nagarajan, and Newman (1985) by focusing on executive deaths. While the replacement manager choice remains endogenous, the exit of the departing executive and need for a replacement is clearly exogenous in this context. Other authors identify turnover events associated with age-based natural retirements as exogenous cases of management changes (e.g., Denis and Denis (1995), Weisbach (1995)). These events can offer insights on whether managerial style plays an independent role in firm policies independent of any major crisis.

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2.4 Hypothesis Development

To motivate our hypotheses, it is useful to consider a generic firm policy variable for firm $i$ at time $t$ denoted by $P_{it}$. This variable will typically depend on a variety of time-varying firm characteristics, and also on a firm fixed effect. The question of interest is whether and how a firm's policy choice depends on a manager specific effect, which for manager $j$ at firm $i$ can be thought of as an additive term $m_{ij}$ in the policy choice function.

Suppose we compare the change in the policy variable between some point before a management change and some point after the change, where we partial out other control variables using regression techniques. The resulting unexplained change in the policy variable of interest, $\Delta P$, will be equal to the difference in the manager effects, $\Delta m$, plus random noise $\Delta \varepsilon$. For firms without a management change, $\Delta m$ will be zero. Thus, if we compare policy variable changes for firms with turnover (driven by $\Delta m$ plus noise) and a set of suitable matches (driven only by noise), we can assess whether the data is consistent with the presence of managerial style effects.

One possibility is that manager specific effects are small or negligible (i.e., $m_{ij}$ are all small and thus $\Delta m$ is close to zero). This would occur if managerial idiosyncrasies do not play a substantive role in their policy preferences and/or if the board is able to use its power to subvert any particular managerial biases with respect to major policy choices. We will refer to this as the lack of style hypothesis. This hypothesis may apply in most situations, or it may be relevant only for a subset of firm types or firm conditions. This hypothesis may also describe situations in which there is a negligible level of style variability within the pool of managers that might be candidates to lead a given firm.
An alternative possibility is that managers do display non-negligible variation in styles, but boards can observe these styles and pick a manager with the desired style to take the firm in the direction that the board deems optimal (i.e., \( \Delta m \) is a deterministic function of firm characteristics). We will refer to this as the *selected style hypothesis*. Note that under this hypothesis, it is the firm's economic situation and the board's endogenous response to these circumstances that fundamentally determine the policy choice.⁵ The manager's style does influence his/her choices for the firm, so there is causality running from style to policies, but the board anticipates the style and ultimately gets the desired outcome as determined by more fundamental factors.

The lack of style and selected style hypotheses share some similarities. In one case the specific leader chosen from the replacement pool truly does not matter for policy decisions, and in the other case a different party, the board, effectively decides on firm policies and then hires an individual with preferences that will lead the individual to make the desired choice. In both cases, the cross-sectional variation in firm choices will be driven by more fundamental firm characteristics (e.g., financial, governance, etc.). Any unexplained variation in policy choices cannot be attributed to managerial idiosyncrasies, either because they are irrelevant, or because they are fully anticipated and thus effectively determined by more fundamental factors. However, while the two hypotheses can be observationally similar, it is important to separately evaluate the empirical relevance of the selected style hypothesis. This hypothesis posits that managerial selection is an important policy decision in its own right, and thus a firm’s objectives

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⁵ Graham, Harvey, and Puri (2009) present evidence that is broadly consistent with the matching of managerial traits and firm characteristics. For a recent finance model of endogenous optimal job matching, see Eisfeldt and Kuhnen (2011). Bertrand and Schoar (2003) discuss some of the subtleties associated with identifying style effects from endogenously selected managers and are careful not to attribute an entirely causal role to their findings.
can be inferred from its choice of leaders. Moreover, it suggests that frictions or constraints in the process of choosing a manager may have a substantive effect on a firm’s policy choices.

The third possibility we consider is that managers do display non-negligible style effects and the board either (a) cannot fully observe/anticipate/control styles, and/or (b) cannot find a manager within the feasible pool that has a style that simultaneously agrees with the board’s preferences on multiple policy dimensions, and/or (c) picks managers primarily based on general characteristics (e.g., perceived ability) rather than specific styles. Under these conditions, there will be residual variability in managerial styles within any pool of potential leaders, and the particular choice of a leader will play a causal role in the firm's subsequent policy choices that cannot be explained by firm characteristics (i.e., $\Delta m$ is non-zero and has a truly random and unexplainable stochastic component). We will refer to this as the idiosyncratic style hypothesis.\footnote{To use a famous example from the U.S. Supreme Court, Justice Souter emerged as a far more liberal justice than anticipated when chosen by conservative president Ronald Reagan. This idiosyncratic style hypothesis concerns the prevalence of this type of idiosyncratic unanticipated/random/unobservable individual effect}

An important goal of our analysis is to assess the empirical validity of the idiosyncratic style hypothesis and the associated magnitude of any underlying style effects. Evidence in favor of this hypothesis would suggest that some of the variation in the variables that financial economists seek to understand, for example, growth and leverage, is determined by idiosyncratic managerial factors rather than traditional economic calculations. It is quite possible that managerial style is partially selected, and partially idiosyncratic, in which case our analysis should identify each of these two distinct possibilities.

2.5 Empirical Strategy
Our testing strategy is to examine changes in investment, financing, and profitability following a change in a firm’s CEO. We briefly consider directional shifts in policy choices, but focus primarily on non-directional tests that consider only the magnitude of the size of any post-turnover changes in firm policies. This examination can be viewed as the first-difference analog of prior managerial fixed-effects studies. The first-difference approach naturally lends itself to a variety of nonparametric tests. We measure changes from a single point in time after a turnover event less the value preceding the event, using a single observation under each managerial spell to alleviate concerns about using multiple highly serially correlated observations.

To test the idiosyncratic style hypothesis, we focus on exogenous CEO departures in which the event reveals little information about the board or firm’s desire for a major change in policies or direction. Our cleanest set of exogenous CEO departures includes events that are precipitated by a death or health episode. Since these events should occur for a fairly random set of firm-years, they offer a window into changes in firm policies subsequent to an unanticipated fresh draw from the managerial style distribution across a broad section of firms. To augment this sample, we also consider in some cases natural retirements driven by a manager's age rather than the board's perception of the need for a change. While the board may have anticipated these events, they are similar to the health/death events in the sense that any substantive idiosyncratic differences in policy styles between the departing and replacement CEO should be reflected in altered policy choices.

Prior tests of the significance of manager-specific effects could be driven by directional or non-directional effects. While many theories are non-directional, it is certainly possible for some policies that style manifests itself in directional effects, for example the tendency of new CEOs to reverse past acquisition errors (e.g., Weisbach (1995)).

In a famous HBS case, American Home Products adopted a very conservative financial structure in the 1980s. The case suggests that this choice was driven by the style of the firm's (otherwise successful) CEO and that his retirement might lead to a change in financial policy. In our natural retirement sample, we are essentially investigating whether or not these types of changes occur on a systematic basis.
While the idiosyncratic style hypothesis predicts abnormal policy changes around exogenous CEO departures, the other hypotheses do not make this prediction. If style does not matter, or if the firm decides (and is able) to select a replacement manager with the exact same style as the incumbent on all dimensions, we would not expect to observe major changes in firm policies after these events. Thus, the lack of style hypothesis and (a strong version of) the selected style hypothesis both are consistent with no abnormal changes around an exogenous CEO departure. If the firm is unable or unwilling to select a replacement who is very similar to the predecessor on all dimensions, then we may observe abnormal variation in policy choices after exogenous CEO departures. This possibility suggests that it may be informative to look separately at exogenous departures at poorly performing firms or firms that choose outside replacements, as these firms may be more likely to select a manager with a new style.

Tests based on exogenous CEO departures can be viewed as tests for the presence of style effects resulting from a new draw from the style distribution (i.e., idiosyncratic style) in the absence of major organizational stress. It is possible that managerial style matters more for determining firm policies precisely when an organization is in crisis and deliberately decides to change its leader. Unfortunately, it is difficult to identify the causal role of managerial style in these settings, since the effect of the impetus for the managerial change, even after conditioning on all observables, cannot readily be separated from the effect of the managerial change itself. Given the unobservable omitted factors that may drive the management change decision, any of the three hypotheses we discuss above could predict abnormal changes in firm policies after a forced management change/endogenous turnover.

Holding constant the endogenous motivations underlying a forced CEO change, we propose a test of the selected style hypothesis using exogenous features of the pool of potential
replacements.\footnote{Using exogenous managerial supply characteristics to identify the causal role of managerial selection on firm performance has been applied in the literature on family firms (e.g., Bennedsen, Nielsen, Perez-Gonzalez, and Wolfenzon (2007) and Bertrand and Schoar (2006)). If firms with a shallower supply of potential replacements are more averse to removing an incumbent CEO, this will tend to bias downwards our tests for selected style effects.} Conditional on the board deciding that major changes are needed, the firm’s ability to find a manager with the desired style to implement such a change will be larger when there is a deeper pool of potential replacement choices. Consequently, under the selected style hypothesis, we may observe larger policy changes subsequent to endogenous turnover in firms that we expect to have a larger and more diverse pool of potential replacements. More generally, any exogenous feature of the managerial supply function that is systematically related to post-turnover policy changes would suggest a selected style effect. To implement a test, we require an exogenous measure of the supply of potential managerial talent. To conduct this test, we rely on a fairly fixed firm characteristic, the location of its headquarters. Motivated by the results of Yonker (2011) on the role of geography in the supply of executive talent, we assume that firms that are located near larger population centers and firms in more sunny environments are, all else equal, able to select from a deeper pool of managerial styles.

Summarizing, examining changes in policies around exogenous CEO departures allows us to test whether unanticipated idiosyncratic managerial factors affect policy choices in non-crisis environments. A lack of support for such factors would cast doubt on the idiosyncratic style hypothesis and indicate that managers either have no evident style, or alternatively, that they are deliberately selected to have the same style (on all dimensions) as their predecessors. While all of the hypotheses may predict sharp policy changes after endogenous turnover, the selected style hypothesis predicts relatively larger changes when the replacement pool is deeper. If boards do deliberately select styles, this raises the issue of how style is identified. The Bertrand and Schoar (2003) approach may be informative here. If managers who move across
firms tend to display the same style at their old and new employers, this would suggest that there is an observable dimension of style that could be anticipated by the board at the time of a hiring decision.

3. Sample selection and description

3.1 Identifying the initial sample

To identify a large set of turnover events, we exploit the fact that the Compustat Research Insight CDs (formerly Compustat PC Plus) include a listing of each firm's top four executives derived from SEC filings. We use discs from the summer of each year from 1990 to 2007 to identify each firm's fiscal-year-end CEO. We merge this data with the Compustat database and drop financial firms (SIC Codes 6000-6999), utilities (SIC Codes 4900-4949), non-U.S. firms, and firms with less than $10 million in book assets (in 1990 dollars). Once a firm crosses this size threshold, it is retained until one year after it last satisfies the size screen.

After selecting the initial sample, we use financial filings and Factiva searches to clean the data and to ascertain the timing of each CEO change. We also collect data on the employment history of every newly appointed CEO. Our resulting sample is quite large and comprehensive by the standards of this literature, containing 61,429 firm-years in which we know the identity of the CEO at the start and end of the fiscal year. We identify 7,179 turnover events in this sample, implying an overall turnover rate of 11.69%, a comparable rate to prior studies.

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10 Executive names are usually from the most recent 10K or 10Q filing. In our data cleaning we collect finer data on the timing of events. The CEO is any manager listed as CEO or holding the dual titles of Chairman and President. If a firm drops in and out of the files and the CEO is unchanged between exit and re-entry, we assume the CEO was unchanged during the intervening period.
3.2 Classifying turnover events

We identify health/death turnover events by conducting a Factiva search for articles including the name of each outgoing CEO and a set of keywords related to health and death (details in appendix). We assign an event to the health/death category if the event appears to be precipitated by the death or health status of the outgoing CEO or, in a few cases, an immediate family member. As we report in Table 1, 208 events are assigned to this category.

The prior literature establishes that CEOs often retire, either voluntarily or because of their employer’s retirement policies, once they reach certain age thresholds (see Jenter and Lewellen (2012)). We use this observation to identify a set of departures that should primarily include natural retirements. We initially select for this category all non-health/death related departures by CEOs who are between 63 and 71 years old as of the start of the year, an age band that includes the traditional retirement age of 65. Since some older managers may in fact be forced to depart, we also require that the firm's most recent level of accounting performance, measured by return-on-assets (ROA), exceed the sample annual median. We exclude from this group any departures that we later discover are overtly forced. The remaining set of 616 departures represents the natural retirement sample.

While natural retirement CEOs clearly have more discretion in their departure choice than the health/death group, these departures are highly unlikely to indicate a deliberate firm choice to alter operational or financial strategies. Thus, these events can be viewed as largely exogenous for our purposes. In what follows, we will consider the health/death group alone as a conservative categorization of exogenous departures, and the health/death/natural retirement groups together as a more liberal categorization of exogenous departures.
Turning to endogenous CEO departures, we identify an initial group of overtly forced events by conducting Factiva searches over a 3-year period centered on each turnover event and examining all articles that include the name of the outgoing CEO along with a keyword related to an overt firing such as "fired," "ousted," "under pressure," etc. (details in appendix). We assign the event to the overtly forced category if the articles suggest that the departure was forced. As we report in Table 1, there are 533 turnover events in this category.

Since press accounts of CEO departures are often uninformative, particularly for smaller firms with less press coverage, we create an alternative category for suspected forced departures. We place in this group all events that are not in any of the prior categories and in which: (a) the departing CEO was under the age of 60 at the start of his/her last year in office, and (b) the departing CEO does not immediately resurface as a CEO of another sample firm. While a few of these managers surely leave office voluntarily, prior evidence suggests that most of these events are forced dismissals. The performance data detailed below is consistent with this hypothesis. A large number of turnover events (4,087) are placed into the suspected forced group.

Table 2 reports sample medians and indicates whether the reported figures for each group differ significantly from the overall sample. As one would expect if health/death turnover is random and exogenous, the figures for these events (column 3) are fairly similar to the overall sample. In particular, the recent industry-adjusted stock performance of the health/death observations is close to 0. The statistics for the natural retirement group in column 4 indicate that these are superior performing firms, which is to be expected given the performance screen used in selecting these events. The figures in column 5 of Table 2 indicate that the overtly forced group underperforms relative to the overall sample based on both accounting and stock performance. Interestingly, the performance of the suspected forced group is quite similar to the
overtly forced group (see column 6), confirming our suspicion that suspected forced events are often in fact forced dismissals. Finally, consistent with prior evidence, the figures in column 7 indicate that outside CEO hiring is more prevalent in poorly performing firms.

3.3 Identifying matches and measuring abnormal changes

To measure abnormal policy changes, we create policy change benchmarks using the behavior of matching firms. To increase the power of our tests, we require that the turnover event firm and its match both had stable management that could be expected to "imprint" their decisions in the pre-turnover period. Thus, we restrict attention to sample firms and candidate matches with the same CEO in place for at least two years prior to the turnover event year.

As an initial matching choice, we select the firm in the same 2-digit industry that is closest in book assets to the turnover firm as of the fiscal-year end immediately preceding the event. We refer to this as the size/industry match. In the case of the natural retirements, we add the additional condition that matches must be drawn from the restricted set of firms with ROA exceeding the sample annual median, as this criterion is imposed in identifying these retirements.

Since we are interested in the variability of policy changes around management changes, we are concerned that firms in our turnover groups may tend to display abnormal levels of variability even in the absence of a management change. Given this concern, we attempt to choose alternative matches with a similar past variability in firm policy variables. In particular, for each variable of interest, we select as an alternative match the firm in the same 2-digit industry that has book assets between 50% and 200% of the turnover firm and that is closest to the turnover firm in the level of the most recent 3-year (preceding the turnover year) industry-adjusted change in the underlying variable. We refer to this as the size/recent change match.
A simple application of our testing strategy would be to compare policy changes measured around a turnover event, $\Delta P$, between the turnover firms and their matches. However, since our matching procedures may not fully control for predictable components in the evolution of the policy variables, in our baseline models we compare residuals, $\Delta P_{\text{resid}}$, from models predicting the change in the policy variable as a function of various lagged firm characteristics. The regression models used to partial out other factors are detailed in the appendix. For robustness we also report below results without the regression adjustment.

When we compare $\Delta P_{\text{resid}}$ or $\Delta P$ between turnover firms and matches, we implicitly assume that the noise process (i.e., $\Delta \epsilon$) is the same across groups. Our second matching procedure chooses matches based on lagged values of policy variability, helping to ensure that this assumption holds. We will also check this assumption by examining differences in policy variability during the pre-turnover period.

4. Exogenous CEO Departures and Changes in Corporate Policies

4.1 Choice of policy variables

We select for examination variables related to a firm’s growth policy, financing policy, and profitability. We try to be parsimonious in our selection and to focus on variables that have been widely discussed in the related literature. Some of our choices and variable definitions are deliberately intended to closely match the important analysis of Bertrand and Schoar (2003). We measure all changes between year 0, the end of the last fiscal year before the management change, and three years later (year +3).

One of the most fundamental choices made by a CEO is the growth trajectory of their firm. Many other firm characteristics, notably financing policies and profitability, may depend
directly on a firm's growth strategy. We consider two different measures of growth. The first is the percentage change in a firm's total book assets. This measure will capture many types of growth, including changes in capital spending, acquisitions, and divestitures. The second growth measure we consider is the change in a firm's capital spending intensity, measured as the ratio of capital expenditures to property, plant, and equipment. Capital spending is one of the chief growth levers under control of a CEO and is the focus of a large empirical literature.

As a simple and important indicator of a firm's financing policies, we consider changes in a firm’s leverage ratio. Following Bertrand and Schoar (2003), we measure leverage as total debt over total debt plus book equity. A firm’s overall leverage choice has the potential to be affected by many CEO characteristics, including risk aversion, confidence, optimism, etc.

Finally, following Bertrand and Schoar (2003), we consider ROA (defined as EBIT/average assets) as a general measure of firm profitability. Differences in managerial ability, and the ultimate success of chosen policies, may be reflected in ROA figures. As such, the threshold for finding ROA style effects may be larger than for the other variables. In particular, optimizing behavior by firms coupled with competitive managerial labor markets may minimize or eliminate any observable and less-than-fully-compensated causal managerial style effects on economic profitability. However, style effects for ROA may remain if accounting profitability is not a fully informative measure of economic profitability. New managers may adopt different accounting practices, write down asset values, sell or purchase assets, or change the firm's risk profile. Thus, a finding of a change in accounting profitability may indicate that something has changed under new management, even without an actual change in economic profits.
4.2 Directional tests

We first compare signed abnormal changes in each of the policy variables after an exogenous CEO departure. In our baseline models we calculate this abnormal change as the median $\Delta P_{\text{resid}}$ of the exogenous departure observations less the median of the size-industry matches. We focus on medians given the presence of outliers in variables constructed from accounting ratios. Results for mean comparisons, our alternative matching scheme, and other modeling choices are discussed in the robustness section below. The p-value for the directional abnormal change is calculated using a Wilcoxon rank sum test for differences in medians.

Comparisons for the four policy variables and the two sets of exogenous events (i.e., with and without the natural retirements) are reported in Table 3. As is evident from the figures, none of the policy variables exhibit directional abnormal changes after an exogenous departure. Half of the signs are positive, half are negative, and all p-values are greater than .5. As an indicator of the magnitude of each abnormal change, we report figures for the normalized abnormal change defined as the abnormal change divided by the median of the absolute value of the changes for the matching firms (somewhat analogous to a t-statistic). These figures are all small in magnitude, with most indicating a typical abnormal change of less than one tenth the magnitude of the typical variability in the associated policy variable. Clearly the data offer little evidence of significant directional movement, either upwards or downwards, in any of the policy variables of interest after an exogenous CEO departure.

These directional results may appear to contrast in character with some of the related findings reported by Weisbach (1995) and Bennedsen, Pérez-González, and Wolfenzon (2007) in their studies using very different samples from ours. Weisbach (1995) reports that new CEOs who rise to office in mandatory retirement events often sell previously acquired underperforming
assets. Our results suggest that the Weisbach (1995) finding does not reflect a general phenomenon of downsizing by new CEOs, but rather a specific context in which a micro-level strategy decision (or style) has a leader-specific component. This suggests that idiosyncratic style effects may generally be more important in explaining specific investment and financing choices rather than coarse aggregates. Bennedsen, Pérez-González, and Wolfenzon (2007) uncover systematic drops in profitability after deaths of both CEOs and their family members. Given the ownership structure of the firms they study, CEO deaths are likely to be associated with a directional shift in ownership, incentives, and ability that may explain profitability changes for non-style related reasons. Their results on family member deaths suggest the interesting possibility of significant time variation in within-manager behavior.

4.3 Nondirectional tests

The lack of evidence of style effects in directional changes reported above may not be surprising if style effects lead some new managers to shift a given policy variable upwards and others to shift the same variable downwards. We thus turn our focus to the absolute magnitude of each policy change. Our principal measure of this quantity is the median absolute value of the unexplained change in the policy variable of interest, again using regression residuals and changes between year 0 and year +3. To adjust for any directional movements in central tendency, we subtract the median residual for each population from each individual residual before taking absolute values.\footnote{The character of the tests we report are substantively unchanged if we do not make this adjustment.}

To create a measure that is easy to interpret, we construct a variability ratio which is defined to be the median of the absolute value of the adjusted residuals of the turnover
observations divided by the corresponding figure for the matches. Under a null hypothesis of no style effects, this ratio should assume a value of 1 (i.e., no inflation in policy variability for the turnover firms relative to the matches). Any number greater than (less than) 1 is indicative of more (less) policy variability in the turnover observations compared to their matches. For each variability ratio, we also report a p-value derived from a Wilcoxon rank sum test of differences in the median magnitude of the adjusted residuals across the two groups.

As we report in Table 4, the majority of the estimated variability ratios are actually less than one, and none of the p-values are significant. This indicates that policy changes for firms with exogenous CEO departures are not typically larger in magnitude than comparable matching firms. These figures provide little evidence of sharp shifts in policies after turnover that would be suggestive of an idiosyncratic managerial style effect.

One might be concerned about power issues with these tests. To provide additional information, we use bootstrap techniques to calculate empirical distributions for each of the estimated variability ratios in Table 4. We select 200,000 bootstrap samples of turnover observations and corresponding matches, with replacement and with sample sizes selected to equal the number of observations used in each cell of the table. Using the derived distribution, we calculate and report in Table 4 the derived 95th percentile point for the estimated variability ratio.

In the case of the more conservative and smaller sample (health/death only), the derived confidence band estimates admit the possibility of some moderate style effects, with possible increases in variability in the 16.7% (ROA variability ratio of 1.167) to 61.7% (asset growth variability ratio of 1.617) range. Turning to the larger sample that includes natural retirements, these upper bounds estimates become much smaller and in all cases are under 20%. This
evidence suggests that style effects, even at the upper limit, are unlikely to be large in magnitude relative to the usual noise in policy determination.

To provide a more precise test using pooled information across policy variables, we consider a pooled variability ratio calculated as .25 times each of the four individual variability ratios. The 95\textsuperscript{th} estimated percentile for this ratio is also calculated using the same bootstrap techniques. The associated estimates and upper bounds are reported in the final row of Table 4. For the smaller sample (health/death only), the point estimate on the pooled variability ratio is almost exactly 1 (1.004), again providing a point estimate indicating no increase in variability. The 95\textsuperscript{th} percentile upper bound on this estimate is now a moderate 24.1%. When we add the natural retirements, the point estimate falls below 1 (to .943), and now even the upper bound estimate of potential variability arising from a style effect is negligible in magnitude (a 1.043 ratio corresponding to a 4.3% increase in variability). Thus, while we cannot definitively conclude that there are no idiosyncratic style effects evident in exogenous turnover, the data do not even hint at such an effect and suggest that any such effect is, at the upper extreme, quite moderate in magnitude.\textsuperscript{12}

4.4 Robustness of exogenous turnover findings

The preceding results reveal little compelling evidence of abnormal changes in corporate policies after an exogenous CEO departure. Since the idiosyncratic style hypothesis predicts substantial changes, this evidence provides fairly strong evidence against this specific hypothesis. Given the importance of this finding, which contrasts in character with some of the prior literature, we report results for a fairly extensive set of robustness checks.

\textsuperscript{12} As a comparison, if we randomly select samples of observations from the suspected forced departure sample with sample sizes equal to the health/death/retirement group, the same tests we report in Table 4 are generally significant. This suggests again that our findings on idiosyncratic style are not just a reflection of low power.
First, we consider post-turnover policy changes measured on a simple industry-adjusted basis rather than using regression residuals. This has no substantive effect on the estimated abnormal changes, variability ratios, and associated p-values in Tables 4 and 5. Second, we consider variability ratios using the standard deviation of policy changes. The point estimates again suggest no significant increase in variability for the various policy variables and samples, although the point estimates are generally less precise, as one would expect given the extra weight placed on large residuals using this measure. We also experiment with a paired binomial test which calculates the proportion of exogenous CEO departure firms with policy changes (i.e., adjusted residuals) that are larger in absolute value magnitude than their matches. In no case is this figure significantly greater than .5, again suggesting no increased variability after exogenous turnover and no substantive idiosyncratic style effects.

The main results we report in Tables 4 and 5 are for the size-industry matches. As we discuss earlier, our alternative matching procedure, the size recent-change match, may control better for the underlying noise process in the policy variables. We have repeated the analysis in Tables 4 and 5 using these matches, and the results are substantively unchanged to what we report in the tables. We continue to find no evidence of significant directional or nondirectional changes in policy after exogenous turnover compared to the matching population.\textsuperscript{13}

All of the tabulated results are conditional on survival until year +3. It is possible that managerial style effects manifest themselves in a new CEO's willingness to be acquired and/or liquidated. To investigate, we calculate survival rates to year +3 of all of the exogenous turnover

\textsuperscript{13} To further address the possibility of differences in variability in policy noise unrelated to managerial style, we compare the exogenous turnover firms to their size-industry matches in the pre-turnover period (year -3 to year 0). Using the same variability ratio measures as in Table 4, we find no evidence of significant policy variability differences between the exogenous change firms and their matches in this earlier period.
firms and their size-industry matches. Since our turnover identification requires observing the firm in year +1, we impose the same survival condition on the matches. We then separately compute the percentage of firms that exit CRSP by year +3 for reasons related to mergers (as defined by CRSP delisting codes) and all other reasons (liquidations, delisting, exchanges, etc.).

When we compute these two exit rates for the two groupings of exogenous turnover events, in all four cases we find that the exit rate for the turnover group is less than or equal to the corresponding rate for the matches. However, the differences are small in magnitude and insignificant. Thus, we find no evidence suggesting that exogenous turnover events are natural opportunities to put a firm in play as an acquisition target or delisting candidate, again casting doubt on the presence of idiosyncratic managerial style effects.

4.5 *Exogenous turnover in special subsamples*

While we find no evidence of idiosyncratic style effects, these effects may be apparent for certain subgroups of sample firms. Some firms identify a replacement CEO several years before a leadership change takes place (see Naveen (2006)). Style effects may be muted for these firms, as the baton is slowly passed to the heir apparent. To investigate, we identify all of the exogenous turnover cases in which a firm has an heir apparent as indicated by the title of president and/or COO. We then examine the variability ratios in Table 4 with this set of events excluded. In all cases, the estimated variability ratios hover slightly above or below 1, and in no case are they significantly greater than 1. Thus, the case for idiosyncratic style effects remains weak even when we exclude orderly/planned leadership transitions.

One might suspect that outsiders who are hired as CEOs are more likely to make major changes than are internally promoted candidates. In addition, the choice of hiring an outside
CEO would tend to indicate that the board may want to change the firm's direction, which would suggest more variability in policy changes under the selected style hypothesis. Thus, we consider the subset of all exogenous CEO changes in which the replacement leader is an outsider. Interestingly, all variability ratios corresponding to Table 4 remain insignificantly different from 1 for this subsample. Thus, even when the replacement is an outsider, we find no convincing evidence of style effects after exogenous turnover.14

While the preceding evidence suggests that new managers in exogenous departure events do not typically sharply alter past policy choices, it is possible that they choose to make large changes when the firm has been making fairly extreme choices. For example, if a conservative leader implements a policy of abnormally low leverage, an exogenous CEO change may represent a natural opportunity for a new manager to raise leverage and bring the firm more in line with its peers.

To investigate, we identify all health/death/natural retirement events in which a firm's industry-adjusted investment intensity or leverage are located in the bottom 20% (top 20%) of the sample distribution. We then ask whether these firms are more likely after the event to raise (lower) the selected variable of interest compared to their matching firms using a simple binomial test. We find only a single case out of the 16 comparisons in which the new manager is associated with a significant (at the ten percent level) tendency to alter the selected variable more than the matches in the hypothesized direction.15 Given this evidence, the data appear to offer no

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14 We have also considered whether style effects are evident in the smaller firms in our sample (below median book assets) or the more research intensive firms (R&D/assets above median). Using the tests in Tables 3 and 4, we find no strong evidence of managerial style effects for these subsamples.

15 We identify these 16 comparisons from the 4 policy variables times the 2 groupings of exogenous events times the 2 potential locational extremes (high/low). It is worth noting that our regression framework controls for the year 0 value of the variable of interest in predicting future changes. Thus, we are asking whether these variables move towards the center of the distribution more than would be expected given the (fairly extreme) start of period values.
compelling case that new managers tend to alter a firm's past choices when the previous manager has located the firm at an extreme tail of the policy choice distribution.

5. Endogenous CEO Departures and Changes in Corporate Policies

5.1 A first look at endogenous departures

The preceding results from exogenous CEO departures offer no support for the idiosyncratic style hypothesis. This casts doubt on the idea that there are large random style factors that are less-than-fully anticipated by the board and that have a causal impact on firm policy decisions. However, it remains possible that style is anticipated by boards and affects their decision to dismiss an incumbent CEO and hire a replacement, a possibility that we refer to as the selected style hypothesis. In the case of exogenous departures, boards may deliberately choose CEOs with a similar style to the departing CEO. In the case of endogenous departures, the replacement decision is likely to indicate that the board believes a new direction is needed.

To provide some initial evidence using endogenous departures, we calculate our measures of directional and nondirectional abnormal policy changes for the overtly forced and suspected forced CEO departure events. As we report in Table 5, there is strong evidence of abnormal policy changes around these events. The main finding to emerge from Panel A on directional shifts is that firms tend to substantially downsize (on the order of 10% - 20%) relative to their matches after an endogenous CEO removal. The Panel B figures on nondirectional shifts indicate significant effects across all of the policy variables, with p-values below .05 in 2 of the 4 cases for the smaller overtly forced sample, and below .01 in all cases for the larger sample of suspected forced departures. The point estimates on the variability ratios in the suspected forced
sample range from approximately 15 to 50 percent, a meaningfully large increase in policy variability.\textsuperscript{16}

These initial results are consistent with a selected style effect in which boards choose certain managers with styles that will lead them to make the changes that the boards deems as necessary or optimal. However, it is possible that any new leader (and perhaps the old one as well) would choose to take the firm in the same new direction given the general economic crises that frequently precede forced CEO turnover. Thus, the correlation between leadership changes and policy changes can only be taken as suggestive of a causal selected style effect.

5.2 Evaluating the selected style hypothesis

In this subsection we more closely evaluate the selected style hypothesis. If firms deliberately select certain types of leaders in certain situations, this is consistent with a perception by the board of a causal relation between managerial characteristics and policy choices. We will refer to this as the perceived selected style hypothesis, since it is possible that boards believe CEOs have different styles, even when a causal relation does not exist. To provide evidence that board perceptions are in fact correct, a possibility we refer to as the causal selected style hypothesis, we will need to exploit some type of exogenous variation in the replacement choice.

A great deal of prior evidence supports the perceived selected style hypothesis in the sense that hiring patterns are consistent with certain managerial characteristics being desired in certain situations. For example, firms are more likely to hire outsiders when performance is

\textsuperscript{16} It is worth noting that our regression residual approach controls for firm performance, so these large policy changes are above and beyond what might be expected given the generally poor performance of endogenous turnover firms. We have also chosen matches based on recent performance and the results are quite similar.
particularly poor (see Parrino (1997)). To investigate in the context of our sample, we estimate logit models predicting when firms hire: (a) outside CEOs, (b) CEOs from outside the industry, (c) CEOs with prior top executive experience, and (d) older CEOs. As we report in Table 6, outside CEOs are more likely to come from outside the firm when the firm is performing poorly. Conditional on going outside the firm, we find that larger firms are more likely to hire older CEOs, CEOs with prior top executive experience, and CEOs from other industries. The choice to go outside the industry is also weakly negatively related to sales growth performance. All of these results are consistent with systematic variation in the demand for certain types of managers, and associated styles, as would be predicted under the perceived style hypothesis.

While the preceding evidence is consistent with the perception of style effects by boards, it remains possible that these perceptions are incorrect and/or that their choices cater somewhat to other parties who mistakenly believe in causal style effects (investors, employees, etc.). To identify a causal link, we consider exogenous features of the replacement choice. While the precise choice of a CEO replacement will always be endogenous, the pool of possible replacements has some exogenous features that may affect the actual replacement choice and can thus be useful in identification.

The recent work of Yonker (2011) on the geography of managerial supply is particularly relevant here. His findings suggest that executives are more willing to join firms that are located in a relatively nearby geographic vicinity. In addition, his findings suggest that locations that are relatively sunny are more likely to attract more distant executives. These findings suggest that the size of the population close to corporate headquarters and the climate of the headquarters location will impact the depth of the pool of managerial candidates.
If there is a causal relation between managerial styles and firm choices, and if styles are deliberately selected by the board from the pool of available executive talent, we would expect differences in the supply pool to be related to the distribution of policy changes. Any such distributional differences would be suggestive of a causal relation. However, under some reasonable assumptions, we can make more specific predictions. In particular, if firms dismiss their CEO, it would appear likely that the board has a preference for a particularly large change in the firm’s policies and direction. If styles that lead executives to make large changes are relatively scarce, we would expect firms with a deeper pool of choices to be more likely to locate an executive with a style that will guide them towards making changes of the desired magnitude.

To investigate, we collect data from Compustat on the corporate headquarters location of all sample firms. We assign each firm to a sunny or cloudy group depending on whether the headquarters location experiences 100 or more clear days per year on average (99 is the sample median) using NOAA National Weather Data. Similarly, we assign a firm to a dense/non-dense population group depending on whether there are 7.5 million or more inhabitants living within 100 mile radius (this is approximately the sample 25th percentile) using U.S. census data.

For each of the four policy variables, we calculate the median magnitude of policy changes (absolute value of adjusted residuals) for all forced departures (overt and suspected together), grouped by these proxies for a relatively deep or shallow supply of possible replacement managers.

In Table 7 we report these figures, along with an indication on whether each figure differs significantly from its matching firms. More importantly, we report p-values for a test of a difference between the deep and shallow labor pool groups. As the table illustrates, in all 8 cases (4 policy variables x 2 ways of characterizing supply depth) the post-turnover policy variability
is greater in the firms with a suspected deeper pool of styles to select from. Of these 8 comparisons, 6 are significant at the 5% level or better. Finally, the asterisks indicate that in most cases the firms with a deep (shallow) replacement pool exhibit significant (insignificant) excess policy variability relative to their matches.

To check the robustness of the Table 7 findings, we have repeated the analysis with the following modifications: (1) using the standard deviation of policy changes rather than the median absolute value as a measure of dispersion, (2) using simple industry-adjusted policy changes in place of policy change regression residuals, and (3) subtracting off the median of the absolute values of policy changes for each group’s set of matches before calculating differences between groups. With all of these modifications, the basic conclusion is the same. Policy variability after endogenous turnover is usually greater for the group with the assumed deeper supply of replacement managers, and the differences are significant at the 10% level or better in the majority of these comparisons.

These findings provide interesting support for a causal version of the selected style hypothesis. Firms who are likely to want to make large changes and able to locate a suitable executive with the desired style are in fact quite likely to make large changes. While our earlier results suggest little idiosyncratic noise in style effects, these results suggest that style effects are real and thus constraints or frictions in the hiring or firing process could have a meaningful and causal effect on the policy choices that firms make. Our results thus suggest that firms select leaders to implement desired policies, presumably policies that are anticipated to maximize value, but they may be constrained in this optimization problem by the available pool of managers.
6. Executives Moving Across Firms

Our findings above indicate no evidence of idiosyncratic style effects but some evidence consistent with the presence of (causal) selected style effects. The natural question that arises is the issue of how boards are able to identify an individual manager’s style when selecting a leader. The important work of Bertrand and Schoar (2003) suggests that style can be identified by examining an individual’s policy choice bias at a prior employer. If style is a fixed innate characteristic, an individual should exhibit a similar style across employers.17

To examine this possibility in our sample, we identify all CEOs who were previously employed as a CEO at another sample firm during the sample period. We eliminate cases in which the old employer was acquired by the new employer or where there was some ownership linkage between the two employers. Similar to Bertrand and Schoar (2003), we impose an imprinting condition that the individual serves in a top executive position at each firm for at least 3 annual Compustat data releases. This yields a final sample of 131 executives who move across firms and serve in the CEO role at both firms (the CEO to CEO sample).

Following Bertrand and Schoar (2003), we estimate regressions for each of the four policy variables over a sample derived entirely from firms that are the source or destination of a CEO to CEO job mover. In the case of the asset growth variable, we use the 1-year percentage change in book assets. We include all available firm-years in the regressions and include the same independent variables as Bertrand and Schoar (2003), including firm fixed effects. Most importantly, we add a separate dummy variable for each CEO to CEO mover. The estimate on

17 A famous example is Al Dunlap (a.k.a. Chainsaw Al), a CEO who exhibited a style of massive cost cutting across multiple employers. The empirical question is whether this style portability reflects a more general phenomenon. The evidence of Cronqvist, Makhija, and Yonker (2011) indicates portability of individual borrowing styles across both personal and corporate borrowing choices.
this dummy variable is intended to detect whether the individual displays a common style effect across both of her employers that is distinct from any firm fixed effect.

Results for these regressions are reported in Panel A of Table 8. For each policy variable we report the F-statistic and corresponding p-value for a test of joint significance on the set of CEO dummy variables. As is clear from the table, the individual CEO dummy variables are highly significant for all of the variables of interest. This is consistent with Bertrand and Schoar (2003) and suggests that CEOs display distinct growth, investment, financing, and profitability styles that they port across employers

While these results are intriguing, Wooldridge (2002) cautions against using F-tests for testing significance of a large set of individual effects absent very strong assumptions about the error term. These assumptions surely do not hold in the current context. A large sample does not help matters, as the number of parameters to be estimated grows proportionally with the sample. To investigate, we experiment with scrambling the job movement data. In particular, we randomly assign each CEO to CEO mover to a different hiring firm than the one they actually join. We scramble the data 1000 times, run the same regression for each scramble, and calculate the median F-statistic and p-value for the CEO fixed effects. As we report in the second row of Table 8, the results using this sample continue to indicate very significant CEO style effects, even though we have assigned executives to firms that they did not actually join. In some cases median F-statistics are larger for the scrambled sample than for the actual sample.

These findings are concerning and suggest serious underlying methodological difficulties in using dummy variables to identify style effects. We suspect that part of the problem may arise from the high serial correlation in the policy variables we study. To investigate, we generate a simulated sample of 400 firms for 6 years by drawing year 1 observations from a standard
normal distribution and selecting subsequent observations from an AR(1) process with standard normal innovations and an autocorrelation coefficient matching the sample data. We assume that CEOs at the first 200 firms each randomly move in year 4 to a distinct firm within the second set of 200 and then regress the simulated policy variable against firm and CEO fixed effects. We repeat this process 1000 times and calculate the median F-statistic and p-value for the CEO fixed effects. As we report in the third row of Table 8, we continue to find significant CEO effects using a traditional F-test, even though the data is generated randomly.\textsuperscript{18}

Clearly our findings indicate that the popular F-test approach for identifying individual managerial effects is problematic. Bertrand and Schoar (2003) suggest a more robust alternative test by using average regression residuals from models that include firm-fixed effects but no CEO fixed effects as an estimate of a CEO’s style at a given employer. They then regress this estimated style at the new employer against the estimated style at the old employer and report positive and significant coefficient estimates for almost all of their selected policy variables. This approach offers direct evidence of individual style persistence across employers.

We undertake this more robust test in our CEO to CEO sample. The associated regression coefficients are reported in the first row of Panel B of Table 8. As the figures illustrate, our coefficient estimates are small in magnitude, and none are significant. This evidence suggests little style portability in our sample. To increase the power of this test, we estimate a corresponding regression for individuals who move from any executive officer

\textsuperscript{18} As Bertrand, Duflo, and Mullainathan (2004) illustrate, evaluating significance for a small number of dummy variables can be problematic when serial correlation is high. The problem in the current setting is exponentially higher. We do not cluster our Table 8 estimates by firm, as clustering only guarantees the robustness of the non-dummy variable estimates to serial correlation. If we do cluster, the F-statistics on the CEO fixed effects for both the real and simulated data actually grow in magnitude, suggesting the problem actually gets worse.
position in a sample firm to a CEO position.\textsuperscript{19} The results in this case (Panel B, row 2), are even less supportive of the presence of style effects. All of the coefficients are of the wrong sign (negative), and all are insignificant.

The preceding findings on job movers provide no evidence that individual managers display an innate style that they port across employers and suggest that prior evidence on this issue may be overstated and/or sample specific. However, it remains possible that managers are selected not for an innate or intrinsic style, but rather for a style that was developed as a function of the type of firm that they manage. For example, managers at a downsizing firm may develop a set of skills or experiences that are particularly useful to another firm that is in the midst of a long-run downsizing campaign.

To investigate, we run analogous regressions but with the firm-fixed effects dropped. Thus, the estimated individual style effect at each employer reflects the firm’s average policy bias relative to other firms during a manager’s tenure rather than the manager’s bias relative to other managers at the same firm. These estimates are reported in Panel B of Table 8 (rows 3 and 4). Interestingly, the coefficient estimates on two of the four policy variables (asset growth, ROA) are reliably positive and significant in these models and suggest that there is significant correlation in firm-level policy biases across a given manager’s multiple employers. To check whether this simply reflects an industry effect, we report in the last two rows of Table 8 analogous models that again omit firm fixed effects but include industry times year effects. The results here change little and again suggest a correlation in policy biases for asset growth and profitability across multiple employers, even if measured relative to an industry-year cohort.

\textsuperscript{19} This augmented set of job changes is identified from corporate biographies of incoming CEOs. We impose the 3-year imprinting requirement in this augmented sample. We do not examine executives who move to non-CEO positions, as it seems unlikely that they would play a significant role in the variables of interest.
Putting this evidence on job movers together, it does not appear that managers display innate styles that are distinct from their employer and that can be reliably detected across firms that they manage. This casts some doubt on the idea that style is an innate and fixed individual characteristic, subject to the caveat that endogeneity concerns about firms that hire and lose job movers may cloud the ability to detect an individual’s style bias within a given employer. The fact that managers seem to be employed by firms with similar types of policy bias is consistent with the notion that a style (or set of skills) can be developed in one environment that may be particularly useful or in demand in similar future environments. This more generally suggests that style may be a nuanced concept that could be difficult to detect and measure by the econometrician, even if it is observable with little noise to market participants.

7. Conclusion

In this study, we offer evidence that helps clarify the picture on the nature and role of managerial style effects in firms’ investment and financing decisions by exploiting exogenous features of the managerial turnover process. In a large and comprehensive sample of Compustat firms, we find no evidence of abnormal policy changes after exogenous CEO departures arising from health events, deaths, and natural retirements. Under certain assumptions, even our estimated upper bound on the magnitude of any style effects around these events is quite small in magnitude. This evidence casts substantial doubt on the hypothesis that managers have unpredictable and unanticipated idiosyncrasies in style that govern their decisions regarding a firm’s overall investment or financing policy. Thus, random or unanticipated managerial traits or styles do not appear to be the key missing factor in our understanding of some key policy choices that have been widely studied by financial economists.
In the case of endogenous leadership departures arising from forced turnover, we confirm prior findings of large abnormal policy changes when a new CEO assumes office. To overcome interpretation issues arising from the endogenous choice to remove a CEO, we group firms by the suspected depth of their replacement supply pool of managers using the population and climate near a firm’s headquarters location. We find that firms with a suspected deeper supply pool tend to display significantly larger shifts in corporate policies after forced turnover. This evidence is consistent with behavior in which managers have certain styles that play a causal role in the firm’s policy choices. Moreover, this evidence suggests that these causal style effects are anticipated by the board and used in the selection decision, behavior we refer to as the causal selected style hypothesis.

Our negative evidence on idiosyncratic style effects and supportive evidence on selected style effects leads us to ask how managerial styles are observed by boards. To investigate whether styles are fixed innate characteristics that can be inferred from prior managerial experiences, we follow Bertrand and Schoar (2003) and examine style portability for managers who serve at multiple firms. While we are able to replicate their F-test results on a joint set of manager-specific dummy variables, we find that these tests are highly flawed as a method for detecting style effects. More robust tests do not indicate the presence of correlated manager-level policy biases in our sample that can be reliably separated from firm fixed effects. However, we do find some evidence of a correlation in firm-level policy biases for managers serving at multiple firms. This evidence suggests the possibility that, rather than being fixed innate characteristics, styles that are useful in a one environment may developed by working in a similar prior environment.
Taken as a whole, we believe our evidence helps to complement and clarify previous findings regarding managerial style with respect to aggregate variables describing overall investment and financing choices. Since there is little evidence of idiosyncratic style effects, this suggests that noise at the individual manager level is unlikely to explain much of the residual variance in existing empirical models of investment and capital structure. Our evidence on selected style helps confirm that not only are managers chosen partially based on a perceived style, but that these style effects do in fact play a role in firm decisions. This suggests that the general direction taken by the firm should primarily reflect economic and governance considerations at the firm or board level. However, the choice of a specific manager should provide a lens into the firm’s objectives, and any errors or constraints in the process of picking a leader could have substantive effects on a firm’s policy trajectory.

An important open remaining question is the role of perceived general ability versus specific policy styles in the choice of a firm’s managers. The evidence of Kaplan, Klebanov, and Sorensen (2011) suggests that some different dimensions of a manager’s style are an input into an assessment of perceived ability. However, we know little about the tradeoffs that boards make when undertaking the constrained optimization problem of picking a manager characterized by a bundle of skills and style/policy bias characteristics, only some of which may be desired by the board. Our study also leads to the question of whether managerial style effects, possibly even random idiosyncratic effects, manifest themselves in other important but more subtle areas, for example the type of debt a firm issues or the communication style of the CEO. Finally, as suggested by the work of Bennedsen, Pérez-González, and Wolfenzon (2007), it remains possible that managers’ styles change over time, and thus the within-manager variation
in style could vary as much as the across-manager variation. Future research will need to investigate these types of possibilities.

A secondary contribution of our paper is a methodological point. Our analysis indicates that inferences from traditional F-tests on the joint significance of fixed-effects dummy variables with multiple observations for each unit is highly suspect in many settings typically encountered in corporate finance. In many cases there will be no information content in these tests, even when reported p-values are well below .0001. Many panel-data estimation approaches, such as those discussed by Petersen (2009), do not solve this problem. These approaches are intended to generate asymptotically valid inferences on other explanatory variables, not the fixed-effects themselves. As is often the case when using panel data in a corporate finance setting, attention to deriving appropriate test statistics is certainly in order.
**Appendix**

**News Searches**

For the health/death searches, we identify every outgoing CEO and search Factiva for all articles in any year that include the CEO's last name, the firm's name at the time of the turnover event, and any of the following keywords (with indicated wildcards): death*, die*, illness, medical, sick, or health. If the articles reveal that the turnover was precipitated by a health issue or death of the CEO or an immediate family member we assign the turnover to the health/death category.

For the forced turnover searches, we identify every outgoing CEO and search Factiva for all articles in a four year period centered on the turnover year that include the CEO's last name, the firm's name at the time of the turnover event, and any of the following keywords (with indicated wildcards): fire*, oust*, force*, remove*, pressure*, terminat*, dismiss*, or shake up (shake adj1 up). If the articles suggest that the turnover was involuntary, we assign the turnover to the overtly forced category.

**Regression Modeling**

In section 4 of the paper we consider four distinct policy variables: the percent change in book assets between year 0 and year +3, the change in capex/PPE between year 0 and year +3, the change in financial leverage between year 0 and year +3, and the change in ROA between year 0 and year +3. The dependent variable in each regression is this actual variable less the median value of the same variable calculated over all Compustat firms in the same 4-digit industry over the same time period. The regressions are estimated over the entire subset of firm-years in our initial sample with the requisite data.

The independent variables in each regression include the industry-adjusted policy variable change calculated over the most recent historical 3 year period (i.e., change in industry-adjusted assets/capex intensity/leverage/ROA between year -3 and year 0), the change in industry-adjusted cash flow/assets between year -3 and year 0, the policy variable level calculated as of year 0, the log of inflation-adjusted book assets, the percent change in book assets between year -3 and year 0, Tobin's q as of year 0, cash flow/assets as of year 0, and the total return on the firm's stock between year -3 and year 0. Tobin's q is defined as (book assets + market equity - book equity - deferred taxes)/book assets or [(data 6 + data199 x data25 – data 60 – data 74)/data6]. Cash flow is defined as income before extraordinary items plus depreciation (data 14 + data 18). This cash flow variable is normalized by start of year book assets. All other variables are defined in the text or the tables.

In performing the panel data regressions, we winsorize all variables except log assets at the 1% and 99% tails over the domestic Compustat universe. The underlying variables that are expressed as ratios (e.g., cash flow/assets or capex/PPE) are winsorized before calculating any changes while the percent change variables (including the industry-adjusted change variables) are winsorized after calculating the change.
References


Malmendier, Ulrike, and Geoffrey A. Tate, 2005a, CEO Confidence and Corporate Investment, *Journal of Finance* 60-6, 2661-2700.


<table>
<thead>
<tr>
<th>Category</th>
<th>Number</th>
<th>% of turnover events</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Firm-Years</td>
<td>61,429</td>
<td></td>
</tr>
<tr>
<td>Turnover Events</td>
<td>7,179</td>
<td>100.00%</td>
</tr>
<tr>
<td>Health/death events</td>
<td>208</td>
<td>2.90%</td>
</tr>
<tr>
<td>Natural retirements</td>
<td>616</td>
<td>8.58%</td>
</tr>
<tr>
<td>Overtly forced</td>
<td>533</td>
<td>7.42%</td>
</tr>
<tr>
<td>Suspected forced</td>
<td>4,087</td>
<td>56.93%</td>
</tr>
<tr>
<td>Outside hire – previously CEO at public firm</td>
<td>325</td>
<td>4.53%</td>
</tr>
<tr>
<td>Outside hire – previously executive officer at public firm</td>
<td>844</td>
<td>11.76%</td>
</tr>
</tbody>
</table>

Note. The sample contains all CEO turnover events identified from the listing of executive officers in the Compustat officer name files from 1990 to 2007 and confirmed by direct inspection of financial filings. Financial firms, regulated utilities, foreign firms, and firms with below $10 million in 1990 inflation-adjusted assets are excluded. The health/death group includes all cases in which news searches revealed that the CEO departure was related to a health condition or death. The natural retirement group includes non-health/death departures for CEOs in the age 63 to 71 category with accounting performance (ROA) exceeding the sample annual median. Age data was collected from Compact Disclosure CDs and supplemented with information from biographies in 10K statements and annual reports. The overtly forced group includes cases not in the prior categories for which news searches indicated that the CEO was forced to leave or left under pressure. The suspected forced group is all turnover events not in the prior categories for CEOs who were under the age of 60 and did not immediately resurface as an executive officer at another sample firm. Outside hires who were former CEOs (executive officers) are individuals for whom we can identify from biographies in financial filings that their most recent executive position was with a public firm and can confirm from Compustat and/or Compact Disclosure that they served as a CEO (executive officer) at that firm.
Table 2: Sample Summary Statistics

<table>
<thead>
<tr>
<th></th>
<th>All Firm-Years</th>
<th>All turnover events</th>
<th>Health/death events</th>
<th>Natural retirements</th>
<th>Overtly forced</th>
<th>Suspected forced</th>
<th>Outside hire from public</th>
</tr>
</thead>
<tbody>
<tr>
<td>ROA</td>
<td>.0720</td>
<td>.0475***</td>
<td>.0660</td>
<td>.1227***</td>
<td>.0417***</td>
<td>.0204***</td>
<td>.0402***</td>
</tr>
<tr>
<td>Industry-Adjusted ROA</td>
<td>.01384</td>
<td>.0000***</td>
<td>.0015**</td>
<td>.0476***</td>
<td>-.0004***</td>
<td>-.0073***</td>
<td>.0000***</td>
</tr>
<tr>
<td>Leverage</td>
<td>.2705</td>
<td>.2762</td>
<td>.2577</td>
<td>.3043***</td>
<td>.3599***</td>
<td>.2313***</td>
<td>.2878</td>
</tr>
<tr>
<td>Capex/PPE</td>
<td>.2423</td>
<td>.2401</td>
<td>.2192*</td>
<td>.2139***</td>
<td>.2300</td>
<td>.2813***</td>
<td>.2495</td>
</tr>
<tr>
<td>Book Assets</td>
<td>156.86</td>
<td>159.10</td>
<td>132.96</td>
<td>707.15***</td>
<td>538.55***</td>
<td>104.90***</td>
<td>260.24***</td>
</tr>
<tr>
<td>1-year ind.-adj. excess stock return</td>
<td>.0000</td>
<td>-.0413***</td>
<td>-.0086</td>
<td>.0515***</td>
<td>-.1137***</td>
<td>-.0845***</td>
<td>-.1101***</td>
</tr>
</tbody>
</table>

Note. - The reported statistics are sample medians for the indicated set of observations. The set of observations in each category is defined in the prior table. The outside hire category includes all individuals who were identified as executive officers at a prior public-firm employer. All figures are calculated as of the end of the fiscal year immediately prior to the turnover event (year 0/date 0). ROA is defined as EBIT/average assets or \([\text{data 178}/(\text{average of start and end of year data 6})]\). Industry-adjusted ROA is the firm's ROA less the median in the same year for all firms in the same 4-digit SIC industry. Leverage is \([\text{book debt}/(\text{book debt plus book equity})]\) or \([\text{[(data 9 + data34)/(data9 + data34 + data60)]}]\). Capex/PPE is \([\text{data 128}/(\text{start of period data 8})]\). Book assets is equal to data item 6 (inflation adjusted to 2007 dollars). Industry-adjusted stock return is the firm's stock return in the most recent fiscal year prior to the turnover event less the industry median stock return over this same period for all firms in the same 4-digit industry. Asterisks refer to whether the indicated median differs from the all firm-years group using the non-parametric continuity corrected median test implemented by Stata 11. We exclude the indicated group from the all firm-years group when conducting this test. ***Different from the all firm-years group at the 1% level, **Different from the all firm-years group at the 5% level, *Different from the all firm-years at the 10% level.
<table>
<thead>
<tr>
<th>Variable</th>
<th>Abnormal Change</th>
<th>P-value</th>
<th>Normalized Abnormal Change</th>
<th>Abnormal Change</th>
<th>P-value</th>
<th>Normalized Abnormal Change</th>
</tr>
</thead>
<tbody>
<tr>
<td>Asset Growth</td>
<td>-0.025</td>
<td>0.972</td>
<td>-0.108</td>
<td>-0.004</td>
<td>0.338</td>
<td>-0.016</td>
</tr>
<tr>
<td>Capital Exp. Intensity</td>
<td>0.001</td>
<td>0.721</td>
<td>0.005</td>
<td>-0.009</td>
<td>0.919</td>
<td>-0.097</td>
</tr>
<tr>
<td>Leverage</td>
<td>0.002</td>
<td>0.814</td>
<td>0.019</td>
<td>0.006</td>
<td>0.575</td>
<td>0.064</td>
</tr>
<tr>
<td>Return on Assets</td>
<td>0.001</td>
<td>0.549</td>
<td>0.029</td>
<td>-0.007</td>
<td>0.743</td>
<td>-0.187</td>
</tr>
</tbody>
</table>

Which Events

<table>
<thead>
<tr>
<th>Health/Death (N=109)</th>
<th>Health/Death/Retirement (N=448)</th>
</tr>
</thead>
</table>

Note. This table reports figures on the change in each policy variable between year 0 and year +3 where year 0 is the end of the fiscal year immediately preceding a turnover event. The abnormal change is calculated as the median of all unexplained policy changes for firms with an exogenous CEO departure calculated as the residual from a regression model that adjusts for industry changes and a variety of year 0 characteristics (details in text and appendix) less the corresponding median for the set of all size-industry matches. P-values are from a Wilcoxon rank sum test for differences in medians between the turnover firms and the matches. The normalized abnormal change is the abnormal change divided by the median absolute value policy change for the set of matches. Asset growth is defined as (book assets in year +3 – book assets in year 0)/(book assets in year 0). Capital spending intensity is measured as (capital expenditures/start of year PPE). Leverage is measured as (long term debt plus short term debt)/(long term debt plus short term debt plus book equity). ROA is measured as (earnings before interest and taxes)/(average book assets during the year).
Table 4 – Abnormal Variability in Policy Changes Following Exogenous Turnover

<table>
<thead>
<tr>
<th>Variable</th>
<th>Variability Ratio</th>
<th>P-Value</th>
<th>Estimated 95th Pctile</th>
<th>Variability Ratio</th>
<th>P-Value</th>
<th>Estimated 95th Pctile</th>
</tr>
</thead>
<tbody>
<tr>
<td>Asset Growth</td>
<td>1.164</td>
<td>0.506</td>
<td>1.617</td>
<td>0.939</td>
<td>0.362</td>
<td>1.083</td>
</tr>
<tr>
<td>Capital Exp. Intensity</td>
<td>0.888</td>
<td>0.396</td>
<td>1.312</td>
<td>0.933</td>
<td>0.204</td>
<td>1.091</td>
</tr>
<tr>
<td>Leverage</td>
<td>1.078</td>
<td>0.850</td>
<td>1.452</td>
<td>0.940</td>
<td>0.342</td>
<td>1.069</td>
</tr>
<tr>
<td>Return on Assets</td>
<td>0.887</td>
<td>0.273</td>
<td>1.167</td>
<td>0.959</td>
<td>0.919</td>
<td>1.180</td>
</tr>
<tr>
<td>Pooled Policies</td>
<td>1.004</td>
<td></td>
<td>1.241</td>
<td>0.943</td>
<td></td>
<td>1.043</td>
</tr>
</tbody>
</table>

Which Observations: Health/Death (N=109) | Health/Death/Retirement (N=448)

Note.- This table reports figures on the magnitude (absolute value) of policy changes for each policy variable between year 0 and year +3. The variability ratio is defined to be the median absolute value unexplained policy change for the turnover firms divided by the corresponding ratio for the matches. Unexplained policy changes are calculated as the residual from a regression model that adjusts for industry changes and a variety of year 0 characteristics (details in text and appendix). P-values are from a Wilcoxon rank sum test for the differences in the median absolute value policy change between the turnover firms and the matches. The policy variables are defined as in Table 4. The variability ratio for the pooled policy variable is calculated as the sum of .25 times the variability ratio for each of the four individual policy variables. The estimated 95th percentile point for the variability ratio is calculated by constructing 200,000 bootstrap samples of turnover observations and corresponding matches, with replacement, and with sample sizes selected to equal the number of observations used in each cell of the table.
Table 5 – Policy Changes Following Endogenous Turnover

<table>
<thead>
<tr>
<th>Panel A: Directional Shifts</th>
<th>Abnormal Chg..</th>
<th>P-value</th>
<th>Abnormal Chg..</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(Normalized)</td>
<td></td>
<td>(Normalized)</td>
<td></td>
</tr>
<tr>
<td>Asset Growth</td>
<td>-0.187</td>
<td>0.000</td>
<td>-0.098</td>
<td>0.000</td>
</tr>
<tr>
<td>(0.721)</td>
<td></td>
<td></td>
<td>(-0.329)</td>
<td></td>
</tr>
<tr>
<td>Capital Exp. Intensity</td>
<td>-0.005</td>
<td>0.453</td>
<td>0.007</td>
<td>0.057</td>
</tr>
<tr>
<td>(0.426)</td>
<td></td>
<td></td>
<td>(0.063)</td>
<td></td>
</tr>
<tr>
<td>Leverage</td>
<td>0.013</td>
<td>0.463</td>
<td>-0.006</td>
<td>0.876</td>
</tr>
<tr>
<td>(0.125)</td>
<td></td>
<td></td>
<td>(-0.060)</td>
<td></td>
</tr>
<tr>
<td>Return on Assets</td>
<td>-0.001</td>
<td>0.202</td>
<td>-0.005</td>
<td>0.326</td>
</tr>
<tr>
<td>(0.309)</td>
<td></td>
<td></td>
<td>(-0.113)</td>
<td></td>
</tr>
<tr>
<td>Which observations</td>
<td>Overtly Forced (N=246)</td>
<td>Suspected Forced (N=1,307)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Abnormal Variability</th>
<th>Variab. Ratio</th>
<th>P-value</th>
<th>Variab. Ratio</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Asset Growth</td>
<td>1.185</td>
<td>0.302</td>
<td>1.151</td>
<td>0.002</td>
</tr>
<tr>
<td>Capital Exp. Intensity</td>
<td>0.989</td>
<td>0.873</td>
<td>1.150</td>
<td>0.003</td>
</tr>
<tr>
<td>Leverage</td>
<td>1.120</td>
<td>0.006</td>
<td>1.205</td>
<td>0.000</td>
</tr>
<tr>
<td>Return on Assets</td>
<td>1.211</td>
<td>0.031</td>
<td>1.519</td>
<td>0.000</td>
</tr>
<tr>
<td>Which observations</td>
<td>Overtly Forced (N=246)</td>
<td>Suspected Forced (N=1,307)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note. - All figures in this table are for the indicated set of endogenous CEO departures representing either overtly or suspected forced departures. The statistics and tests in Panel A are directional in nature and are defined as in Table 3. The normalized abnormal change figures are in parentheses underneath each corresponding abnormal change statistic. The statistics and tests in Panel B are nondirectional in nature and are defined as in Table 4. P-values in both panels are for tests in differences between the turnover firms and their corresponding size-industry matches.
Table 6 – Predicting Replacement CEO Characteristics

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Outside firm replacement</td>
<td>Outside industry replacement</td>
<td>Replacement has CEO experience</td>
<td>Replacement above median age</td>
</tr>
<tr>
<td>Industry-adjusted stock return</td>
<td>-0.329*** (0.049)</td>
<td>0.132 (0.114)</td>
<td>0.022 (0.075)</td>
<td>-0.101 (0.069)</td>
</tr>
<tr>
<td>Sales growth</td>
<td>0.032 (0.037)</td>
<td>-0.132* (0.078)</td>
<td>0.056 (0.054)</td>
<td>-0.068 (0.051)</td>
</tr>
<tr>
<td>Log(inflation adjusted assets)</td>
<td>-0.156*** (0.015)</td>
<td>0.084** (0.034)</td>
<td>0.109*** (0.025)</td>
<td>0.074*** (0.024)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.712 (0.508)</td>
<td>0.265 (0.887)</td>
<td>-1.666** (0.829)</td>
<td>0.692 (0.826)</td>
</tr>
<tr>
<td>Log likelihood</td>
<td>-38945.243</td>
<td>-750.677</td>
<td>-1537.629</td>
<td>-1735.000</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>5,920</td>
<td>1,143</td>
<td>2,440</td>
<td>2,532</td>
</tr>
<tr>
<td>Which observations</td>
<td>All turnover events</td>
<td>Events with an outside public hire</td>
<td>Events with an outside hire</td>
<td>Events with an outside hire</td>
</tr>
</tbody>
</table>

Note: All estimates are from logit model predicting whether the indicated event occurs. Asymptotic standard errors are reported in parentheses under the coefficient estimates. The estimates in model 1 are for whether the firm hires an outsider as a CEO conditional on a turnover event occurring. The estimates in model 2 are for whether the incoming CEO is from the same Fama-French industry as the hiring firm, conditional on the firm hiring an outside CEO from a Compustat firm with an industry identifier. The estimates in model 3 are for whether the incoming CEOs most recent previous executive job was a CEO, conditional on hiring a firm outsider and having nonmissing information on the most recent position. The estimates in model 4 are for whether the incoming CEO is above the median age of all new CEO hires in the given year conditional on the firm hiring an outsider with nonmissing age data. The industry-adjusted stock return is the firm's stock return in the most recent fiscal year prior to the turnover event less the industry median stock return over this same period for all firms in the same 4-digit industry. Sales growth is the most recent percentage change in sales for the same fiscal year period. Inflation adjusted assets are total book assets inflation adjusted to 2007 dollars.

*Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level
### Table 7 – Labor Market Supply and Policy Changes after Forced Turnover

<table>
<thead>
<tr>
<th>Area</th>
<th>Asset Growth Variability</th>
<th>Capital Spending Variability</th>
<th>Leverage Variability</th>
<th>ROA Variability</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sunny weather firms (N=786)</td>
<td>0.372***</td>
<td>0.148**</td>
<td>0.122***</td>
<td>0.069***</td>
</tr>
<tr>
<td>Cloudy weather firms (N=732)</td>
<td>0.308</td>
<td>0.119</td>
<td>0.108</td>
<td>0.052***</td>
</tr>
<tr>
<td>P-value for difference</td>
<td>0.013</td>
<td>0.002</td>
<td>0.044</td>
<td>0.000</td>
</tr>
<tr>
<td>Densely populated location (N=1119)</td>
<td>0.351***</td>
<td>0.131**</td>
<td>0.122***</td>
<td>0.061***</td>
</tr>
<tr>
<td>Non-densely populated location (N=384)</td>
<td>0.307</td>
<td>0.129</td>
<td>0.096</td>
<td>0.055**</td>
</tr>
<tr>
<td>P-value for difference</td>
<td>0.046</td>
<td>0.913</td>
<td>0.007</td>
<td>0.506</td>
</tr>
</tbody>
</table>

Note.- The statistics in each cell represent the median magnitude (absolute value) of unexpected policy changes for each policy variable for the indicated set of firms between year 0 and year +3 calculated as an adjusted regression residual as in the prior tables. Firms are classified into sunny versus cloudy based on whether the corporate headquarters location experiences more than 100 clear days per year. Firms are classified into densely versus non-densely populated depending on whether the population within a 100 mile radius exceeds 7.5 million. P-values are for differences in the indicated variability statistic between the two sets of firms. Asterisks indicate whether the reported statistics differs from the corresponding median for the set of size-industry matches from the indicated set of observations. *Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level
Table 8: CEO Style Effects for Job Movers

<table>
<thead>
<tr>
<th>Panel A: F-tests on manager fixed effects</th>
<th>Asset Growth</th>
<th>Capital Expend.</th>
<th>Leverage</th>
<th>ROA</th>
</tr>
</thead>
<tbody>
<tr>
<td>F-statistic: Actual Data (N=2,990)</td>
<td>1.27 (.027)</td>
<td>1.58 (.000)</td>
<td>2.67 (.000)</td>
<td>3.70 (.000)</td>
</tr>
<tr>
<td>(p-value)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>F-statistic: Scrambled Data (N=2,990)</td>
<td>1.28 (.022)</td>
<td>1.56 (.000)</td>
<td>2.42 (.000)</td>
<td>3.89 (.000)</td>
</tr>
<tr>
<td>(p-value)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>F-statistic: Generated Data (N=2,400)</td>
<td>1.17 (.057)</td>
<td>1.51 (.000)</td>
<td>2.31 (.000)</td>
<td>2.57 (.000)</td>
</tr>
<tr>
<td>(p-value)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Policy transmission across employers</th>
</tr>
</thead>
<tbody>
<tr>
<td>CEO to CEO moves with firm fixed effects</td>
</tr>
<tr>
<td>Top exec to CEO moves with firm fixed effects</td>
</tr>
<tr>
<td>CEO to CEO moves, no firm fixed effects</td>
</tr>
<tr>
<td>Top exec to CEO moves, no firm fixed effects</td>
</tr>
<tr>
<td>CEO to CEO moves, industry x year effects</td>
</tr>
<tr>
<td>Top exec to CEO moves, industry x year effects</td>
</tr>
</tbody>
</table>

Note.- The figures in Panel A pertain to F-statistics on a joint set of dummy variables for individual managers who move across firms in models that include firm fixed effects, CEO effects for job movers, and explanatory variables drawn from parallel models in Bertrand and Schoar (2003) and a dependent variable indicated in the column heading. The asset growth variable is the annual percentage change in book assets and all other dependent variables are defined as in the prior tables. The independent variables in the asset growth and investment models are start of period Q, log of start of period inflation-adjusted book assets, cash flow/(start of year PPE), and year effects. The leverage regression has the same independent variables except that ROA is substituted for Q. The ROA regression includes only the log assets variable and year effects. All dependent and independent variables in the Panel A models are winsorized at the 1% and 99% tails. The F-statistic for the actual data models is for a traditional test of whether all of the CEO effects are equal to 0 and the p-value is reported in parentheses under the statistic. The F-statistics and associated p-values in the scrambled and generated data models are medians for an analogous test run on 1,000 created samples using the procedures described in the text. In Panel B we collect average residuals for each executive's spell at an employer from a panel model with all of the same explanatory variables except the CEO effects. In the first two models of Panel B we include firm fixed effects, in the second two models we exclude firm fixed effects, and the final two models we replace the firm fixed effects with 2-digit industry times year effects. In all models we run a simple OLS regression of average residuals at the new employer against the average residuals at the old employer and report the associated coefficient, with the associated p-value in parentheses. The CEO to CEO (Top Exec to CEO) models include information for all individuals who are identified as moving from a CEO position (any top executive position) at one firm to a CEO position at a different sample employer.