

TOWARD MORE ACCURATE CONTEXTUALIZATION OF THE CEO EFFECT ON FIRM PERFORMANCE

DONALD C. HAMBRICK^{1*} and TIMOTHY J. QUIGLEY²

¹ The Pennsylvania State University, Management and Organization Department, Smeal College of Business, University Park, Pennsylvania, U.S.A.

² University of Georgia, Department of Management, Terry College of Business, Athens, Georgia, U.S.A.

We introduce multiple refinements to the standard method for assessing CEO effects on performance, variance partitioning methodology, more accurately contextualizing CEOs' contributions. Based on a large 20-year sample, our new 'CEO in Context' technique points to a much larger aggregate CEO effect than is obtained from typical approaches. As a validation test, we show that our technique yields estimates of CEO effects more in line with what would be expected from accepted theory about CEO influence on performance. We do this by examining the CEO effects in subsamples of low-, medium-, and high-discretion industries. Finally, we show that our technique generates substantially different—and we argue more logical—estimates of the effects of many individual CEOs than are obtained through customary analyses. Copyright © 2013 John Wiley & Sons, Ltd.

INTRODUCTION

Scholars have shown a sustained interest in comprehending how much influence top executives have over organizational performance (Bertrand and Schoar, 2003; Lieberman and O'Connor, 1972; Mackey, 2008). Some theorists, especially those who study strategy and leadership, have argued that executive actions substantially shape the fates of enterprises (Child, 1972; Hambrick and Mason, 1984; Rumelt, 2011). Others have argued that executives are greatly constrained—by organizational inertia, path-dependence, rigid resource configurations, and pressures to adopt institutionalized norms—such that, on average, leaders do not hold much sway over what happens

to their companies (DiMaggio and Powell, 1983; Hannan and Freeman, 1977; Haveman, 1993). Yet other researchers, attempting to bridge this debate, have invoked the concept of managerial discretion (e.g., Hambrick and Finkelstein, 1987), identifying the conditions under which executives might have considerable versus minimal influence over organizational outcomes (Finkelstein and Boyd, 1998; Shen and Cho, 2005).

Beyond the substantial group of theorists who are directly interested in the topic of executive effects, scholars in a wide array of domains hold a stake in this issue. Having an accurate grasp of whether—or how much, when, and where—top executives matter is centrally important for advancing theory and research on executive compensation, board-management relations, executive selection and succession, top management teams, executive symbolism and celebrity, and other topical areas. Indeed, an understanding of executive effects can be thought of as fundamentally important for much of organizational science.

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*Correspondence to: Donald C. Hambrick, The Pennsylvania State University, Smeal College of Business, 414 Business Building, University Park, PA 16802, U.S.A. E-mail: dch14@psu.edu

The accepted analysis for gauging the influence of chief executive officers (CEOs) on organizational performance is variance partition methodology (VPM). Commencing with a seminal study by Lieberman and O'Connor (1972), and continuing to recent years (e.g., Bertrand and Schoar, 2003; Crossland and Hambrick, 2011), researchers have used various forms of VPM (which is actually a family of methods) to isolate how much variance in company performance is due to CEOs as opposed to other factors.¹ As we discuss below, researchers have used VPM to address three broad questions about whether CEOs 'make a difference': (1) how much influence do CEOs, in general, have on organizational performance? (Lieberman and O'Connor, 1972; Mackey, 2008); (2) in what settings do CEOs have particularly great versus minimal influence on performance? (Crossland and Hambrick, 2007, 2011; Wasserman, Nohria, and Anand, 2001); and (3) what are the performance outcomes from individual CEOs? (Bertrand and Schoar, 2003).

Using large data panels, typically spanning 15–20 years, researchers have attempted to partition the distinct effects (on company performance) of several categorical factors, operationalized essentially as large sets of dummy variables (or as entity grand averages): for calendar year (to gauge macroeconomic influences), industry membership (some industries are inherently more munificent than others), firm (some companies have stronger health and position than others), and—ultimately—the CEO (summarized in Bowman and Helfat, 2001; Crossland and Hambrick, 2007, 2011). In this analytic set-up, CEOs 'matter' or 'make a difference' to the extent that they exhibit performance tendencies that deviate from what can be explained by macro-economic conditions during their tenures, by their industry's grand average performance *over the entire data panel*, and by their company's grand average performance *over the entire data panel*.

The scholars who have conducted these prior studies deserve great credit for moving us beyond anecdotal and speculative assessments of executive potency. Moreover, these investigators have

employed progressively more sophisticated VPM techniques, tracking analytic advances over the last 40 years. But researchers of CEO effects have stopped short of what may be the most promising and important refinements in such inquiries: rethinking how to conceptualize and operationalize CEOs' contextual conditions.

The use of strictly dummy (or full-panel grand average) indicators of CEOs' contexts—as in all prior studies—particularly warrants reconsideration. Nominal indicators of context do not specify the pertinent, proximal conditions in which individual CEOs are located, which (as we discuss below) causes substantial blurring of contextual effects and CEO effects. The use of nominal predictors is especially problematic because it treats some of the CEO's *own* impact as part of the context in which he or she is operating, thus systematically underestimating overall CEO influence. Moreover, estimates of the effects of many individual CEOs diverge greatly from what their actual records would seem to warrant. For instance, as we shall show, when nominal indicators of context are employed, the legendary CEO Louis Gerstner—who is widely credited with saving IBM—is deemed to have been a poor CEO.

This blurring of contextual and executive influences is noteworthy, because the entire point of this research tradition is to distinguish accurately—insofar as possible—between the two. As Lieberman and O'Connor (1972:122) said, 'In describing leadership influence on organizational performance, clearly one must consider the influence of other forces.' If we think of these 'other forces' as comprising the contexts in which CEOs operate and bear in mind that 'context' refers to one's 'surroundings' (Cappelli and Sherer, 1991:56) or phenomena that are 'external to the individual' (Mowday and Sutton, 1993:198), the guiding question should be this: To what extent are individual CEOs associated with performance that differs from what would be predicted by their contexts, *particularly the performance and vitality of their firms when they start their jobs, as well as the performance of peer firms during their tenures?* Although researchers cannot conduct controlled experiments to confirm the true contributions of CEOs, they can use archival data to provide better estimates than customary analyses have allowed.

We introduce multiple refinements that more accurately contextualize each CEO's contribution to firm performance. Like other researchers in this

¹ We use the term 'variance partitioning methodology' (VPM) rather than 'variance components analysis,' because the latter is sometimes used to refer to a specific statistical technique (e.g., Crossland & Hambrick, 2007), rather than to the full family of techniques.

research stream, we rely on variance partitioning methodology; but we introduce new measures for gauging the relevant contexts of CEOs, particularly moving away from the use of full-panel grand averages for portraying industries and firms. For ease of exposition, we refer to our estimation model as the ‘CEO in Context,’ or ‘CiC,’ technique for gauging CEO influence on organizational performance. Based on data for 830 distinct CEOs over a 20-year period (for a total of 4,866 CEO-years), our new CiC approach points to a much larger aggregate CEO effect than is obtained from established methods. As a validation test, we show that our technique, relative to customary approaches, yields estimates of CEO effects that are much more in line with what would be expected from accepted theory about CEO influence on performance. We do this by examining the CEO effects in subsamples of low-, medium-, and high-discretion industries (Hambrick and Abrahamson, 1995; Hambrick and Finkelstein, 1987). Finally, we show that our method generates substantially different—and we argue more logical—estimates of the effects of many individual CEOs than are obtained through customary analyses.

Our project follows in the tradition of other *SMJ* authors who have encouraged reconsideration of prevailing analytic methods (e.g., Bowman and Helfat, 2001; McGahan and Porter, 1997). Indeed, two such papers have addressed analytic issues associated with research on CEO effects, in ways that are complementary to our own. In an effort to address the challenge of disentangling CEO effects from firm effects, arising because CEOs are nested within their firms, Mackey (2008) examined a unique sample of firms with CEOs who led two distinct companies. Although such an analysis is not generalizable (since there few multitime CEOs and, as Mackey noted, they are not representative of the overall population), Mackey showed that the CEO effect for this distinctive group of leaders was appreciably greater than for general samples. More recently, Blettner, Chaddad, and Bettis (2012) commented on some of the analytic challenges in detecting CEO influences on performance; they especially introduced the intriguing idea that CEO effects hinge on complex interdependencies (extending beyond additive effects) among various environmental, organizational, and executive attributes. They proposed that simulation might be appropriate for addressing such complex

relationships—a technique that would be complementary to empirical evidence on CEO effects, such as we hope to advance further here.

Before proceeding, we should emphasize that scholarly interest in the magnitudes of overall CEO effects is complementary to research examining the influence of *specific* executive attributes (e.g., functional experiences, personalities, or values) on organizational outcomes (summarized in Finkelstein, Hambrick, and Cannella, 2009). The latter style of research, often conducted under the rubric of upper echelons theory (Hambrick and Mason, 1984), has the benefit of identifying highly specific relationships, whereas the former style has the benefit of gauging the overall impact of CEOs—either in general or in one type of setting compared to another. To some degree, research on overall CEO effects helps scholars who are interested in more specific relationships decide where to target their inquiries. For instance, based on Crossland and Hambrick’s (2007) finding that Japanese CEOs have very little overall effect on their companies’ performance, researchers with an interest in the influence of, say, CEO personality might be well advised not to conduct their studies in Japan. Specific CEO attributes can only matter to the extent that CEOs, *per se*, matter. As we shall discuss, scholars are increasingly showing that the two styles of research can be fruitfully integrated (e.g., Bertrand and Schoar, 2003). Indeed, the two research agendas—attention to overall CEO effects and attention to specific CEO attributes—can be thought of as highly symbiotic.

THE ROLES AND INFLUENCE OF CEOs

The academic field of management resides, in great part, on the premise that managers vary in their effectiveness in ways that have consequences for their organizations—that managers matter. This premise is especially pronounced, and perhaps most plausible, when considering chief executive officers (CEOs). Following axiomatically from the concept of hierarchy, CEOs have more leeway than subordinate managers; and their actions can affect entire enterprises, not just subunits. Some theorists have emphasized the role of CEOs in setting strategy or making decisions about which businesses to invest in and how to compete and create value in those businesses (Porter, 1980; Rumelt, 2011).

Some have highlighted the role of CEOs in shaping organizational architecture, through their decisions about structure, executive staffing, incentives, and metrics (Bower, 1970; Burgelman, 1983). Yet others, particularly leadership scholars, have focused on the role of CEOs in energizing organizational constituencies (Fanelli, Misangyi, and Tosi, 2009; Flynn and Staw, 2004). Considering their combined roles in strategy formulation, strategy implementation, and leadership, there would seem to be ample scope for CEOs to place their marks on their organizations—for good and for ill.

At the same time, however, it is widely accepted that executives, including CEOs, face considerable limits on their actions. They are constrained by their organizations' preexisting asset configurations, entrenched cultures, and various other path dependencies (Fondas and Wiersema, 1997; Hannan and Freeman, 1977). They are constrained by institutional pressures to adopt mainstream policies or 'best practices,' or simply to appear 'normal' (DiMaggio and Powell, 1983; Stuart, Hoang, and Hybels, 1999). Many CEOs face localized constraints, including those whose predecessors remain as board chairs and those who work for dominant founding families (Morris *et al.*, 1997; Quigley and Hambrick, 2012). And some executives are bound up by their own psychology of inertia—including a commitment to the status quo, lack of imagination or boldness, and contentment with satisficing (Bertrand and Mullainathan, 2003; Carpenter and Golden, 1997; Hambrick, Geletkanycz, and Fredrickson, 1993).

Given this tension between the seemingly large potential for CEO influence on the one hand, but the presence of considerable constraint on the other, it is understandable that researchers have pointedly explored the question: Just how much impact do CEOs have on firm outcomes? Commencing with a landmark study by Lieberman and O'Connor (1972), researchers have relied on variance partitioning methodology (VPM) to isolate that portion of company performance that is attributable to CEOs (or the 'CEO effect'), as opposed to contextual factors. Table 1 provides a summary of such studies (Crossland and Hambrick, 2007, 2011; Lieberman and O'Connor, 1972; Mackey, 2008; Wasserman *et al.*, 2001; Weiner, 1978). For comparability, the table only reports results for U.S. samples and for profitability outcomes; the limited results available for non-U.S. samples and for other outcomes, such

as sales growth and market-based performance, are qualitatively quite different (e.g., Crossland and Hambrick (2007)). Using varied samples and varied VPM techniques (discussed below), prior studies have found CEO effects ranging from 8.7 to 31.6 percent, but generally in the range of 10–20 percent.

Beyond attempting to identify the overall size of CEO effects, some researchers have used VPM to assess how situational conditions shape whether CEOs will have great versus minimal effects on firm outcomes. Lieberman and O'Connor (1972) initiated this line of inquiry by reporting the CEO effect for each industry they studied; for instance, they found that the CEO effect was much greater in the soaps and toiletries industry than in the shipbuilding industry. Moreover, they examined how underlying industry attributes were associated with CEO effects; for example, the greater the advertising intensity in an industry (as in soaps and toiletries), the greater was the CEO effect. Recently, Crossland and Hambrick (2011) used a VPM approach (multilevel modeling, or MLM) to examine differences in CEO effects across countries. Arguing that formal and informal institutions vary greatly between countries, in ways that determine how much discretion is available to corporate CEOs, the authors found, for instance, that the CEO effect in U.S. firms was much greater than in Japanese firms.

Researchers have begun using VPM estimates of CEO effects in yet a third way: to gauge the caliber of individual CEOs. Exploiting the fact that VPM yields coefficients for individual CEOs, Bertrand and Schoar (2003) examined a sample of executives who had served as CEOs in two distinct firms, to assess whether there was an association between their personal coefficients for their two tenures. The authors found, for instance, that CEOs' fixed-effect coefficients for explaining ROA were correlated from one tenure to the next, thus revealing hallmark tendencies for individual executives. In a similar vein, accounting scholars Bamber, Jiang, and Wang (2010) extracted fixed-effects coefficients of individual CEOs for a performance metric of importance to accounting: earnings forecasting accuracy. They found that individual CEOs' coefficients were significantly related to personal traits—for instance, CEOs with accounting/finance backgrounds had more positive coefficients (demonstrating greater accuracy) than did other CEOs. This latter study particularly

Table 1. Summary of prior studies employing variance partitioning methodology to isolate ‘the CEO effect’ on firm profitability^a

	Firms	Time period	Years	Analysis	DV	CEO effect (%) ^b
Lieberson and O’Connor (1972)	167	1946–1965	20	Sequential ANOVA	ROS	14.5
Weiner (1978) (L&O replication)	193	1956–1974	19	Sequential ANOVA	ROS	8.7
Wasserman <i>et al.</i> (2001)	531	1979–1997	19	Hierarchical OLS	ROA	14.7
Mackey (2008) (L&O replication)	520	1992–2002	11	Sequential ANOVA	ROA	12.9
Mackey (2008) (expanded sample)	520	1992–2002	11	Sequential ANOVA	ROA	23.8
Crossland and Hambrick (2007) US sample	108	1988–2002	15	Simultaneous ANOVA	ROA	13.4
Crossland and Hambrick (2007) US sample	108	1988–2002	15	Simultaneous ANOVA	ROS	14.0
Crossland and Hambrick (2007) US sample	108	1988–2002	15	Maximum likelihood estimation	ROA	30.4
Crossland and Hambrick (2007) US sample	108	1988–2002	15	Maximum likelihood estimation	ROS	31.6
Crossland and Hambrick (2011) US sample	100	1996–2005	10	Multilevel modeling	ROA	15.5
Crossland and Hambrick (2011) US sample	100	1996–2005	10	Multilevel modeling	ROS	10.4

^a We only include results from samples of U.S. corporations and only results for profitability (i.e., ROS or ROA). We omit results from two unique analyses: (1) Weiner and Mahoney (1981) (an extension of Weiner (1978)) showed that the CEO effect increases greatly when the CEO term is entered first, rather than last, in sequential ANOVA. However, given that contextual factors are more exogenous, they should be entered either prior to or concurrently with CEOs, as all other researchers have done; (2) Mackey (2008), in an effort to avoid the problem of nested firm and CEO effects, conducted an intriguing analysis of 51 firms that had CEOs who led two or more companies. Finding a sizeable CEO effect of 29.2%, Mackey concluded that multitime CEOs are somewhat distinctive and tend to be appointed (at least their second time) by distinctive firms, yielding a relatively large CEO effect.

^b ‘CEO effect’ refers to the percentage of overall variance explained by CEOs.

illustrates the potential complementarities between research on CEO effects and on specific CEO attributes.

In sum, variance partitioning methodologies have been used to examine an array of centrally important questions about CEOs: To what extent do CEOs, in general, influence company performance? Under what conditions do CEOs have the most and least influence? And which individual CEOs have the most and least influence and/or deliver the best and worst performance?

MEASURING THE CEO EFFECT: CRITIQUES AND PROPOSED REMEDIES

In their efforts to identify the portion of firm performance that might reasonably be attributed to CEOs, researchers have employed an array of variance partitioning methodologies, which

differ primarily in how they treat shared variance among predictors (as well as in their assumptions about fixed vs. random effects [summarized in Bowman and Helfat, 2001; Crossland and Hambrick, 2007, 2011]). As shown in Table 1, Lieberson and O’Connor (1972) initiated the use of sequential ANOVA, conservatively adding CEOs to the model only after the variance explained by contextual factors (years, industry, and firm) had been fully accounted for. Wasserman *et al.* (2001), though describing their analysis as ‘hierarchical ordinary least-squares’ (OLS), used essentially the same sequential analysis as Lieberson and O’Connor. Crossland and Hambrick (2007) employed simultaneous ANOVA, entering all explanatory factors concurrently; this technique attributes only uniquely explained variance to each category, assigning any shared variance to a separate ‘shared’ category. Crossland and Hambrick (2007) also used maximum likelihood estimation (MLE), which is a

random-effect model that assumes a random draw from the population as well as independence of effects across predictors. The relatively large CEO effect obtained by Crossland and Hambrick with MLE (as shown in Table 1) might be because their sample was distinctly nonrandom (drawn to match a German sample), which tends to bring about unstable results from MLE (Brush and Bromiley, 1997); moreover, their predictors were nested and nonindependent, which the MLE algorithm ignores in estimating the best fitting model (probably inflating the CEO effect as a result). Most recently, Crossland and Hambrick (2011) used multilevel modeling (MLM), which addresses the nonindependence of effects by explicitly acknowledging the nested nature of the predictors.

Despite their differences, these methods have several features in common—at least as applied in this research stream so far. All prior studies of CEO effects have entailed large data panels (typically 15–20 years), with the aim of explaining firm performance (e.g., ROA) in a given year as a function of several distinct categories of factors, all operationalized essentially as large sets of dummy variables.² The standard predictor categories are calendar year (to control for macroeconomic conditions), industry (to control for persistent differences in industry munificence), firm (to control for stable differences in company resources and health), and the CEO (to identify distinctive tendencies under individual leaders); any remaining variance is deemed unexplained. As such, these prior studies have all explored this multipart question: To what extent can a firm's performance in a given year be explained by (1) the average performance of all firms in the overall economy (or, more specifically, those in the sample) that year; (2) the average performance of all firms in the focal industry (or those in the sample) over the entire data panel; (3) the average performance of the focal firm over the entire data panel; and (4) the average performance of the incumbent CEO over his or her (observed) tenure?

Again, these prior studies have been instrumental in moving the field of management from speculative and romanticized portrayals of executive

effects (e.g., Meindl, Ehrlich, and Dukerich, 1985) to far more rigorous accounts, by disentangling the influence of business leaders and contextual factors on company performance. Unfortunately, however, all these prior studies have greatly blurred contextual and CEO influences. These problems can be broadly described as (1) misspecification of industry influences and (2) misspecification of firm influences, both of which—but especially the latter—introduce serious distortions in identifying overall CEO effects as well as in gauging the influence of individual CEOs.

Misspecification of industry effects

Prior studies of CEO influence have controlled for industry factors by including dummy variables for the industries sampled. Such an approach indeed controls for any stable differences in industry profitability but has two problems that limit its effectiveness. First, it ignores the reality that industry health can change dramatically over time—especially over the 15–20 year periods that are typically examined. Thus, the contributions of some, perhaps many, CEOs are not accurately assessed against contemporaneous industry conditions. For example, some CEOs might serve in industries that were once booming but no longer are; others might serve in industries that were once depressed but are now vibrant. The use of industry grand averages distorts CEOs' impacts in such cases.

Second, and more serious, the industry average is derived only from firms in the sample, and each focal company's own performance, *including each CEO's own performance*, contributes to generating that average. If a sample were to contain dozens of firms per industry, this would be of little concern. However, most studies contain relatively few firms in each industry; for example, Crossland and Hambrick (2007) and Lieberman and O'Connor (1972) each had as few as six firms in some of their sampled industries. Since each CEO contributes to the grand industry average, the variance explained by industry is inflated, and the impact of the CEO is muted (Boyd, Dess, and Rasheed, 1993, made a similar point). For instance, a CEO who delivers exceedingly poor performance arithmetically pulls down the industry average, masking evidence of that CEO's true distinctiveness. Indeed, this misspecification becomes especially severe in those cases where long-tenure CEOs deliver outcomes

² We use the term 'dummy variables' for ease of exposition. In the case of MLM analysis, entity averages from within the sample (rather than dummy variables) are employed. All prior studies in this stream have examined deviations from nominal central tendencies, either through use of dummy variables or entity grand averages.

that diverge widely from the rest of the industry, as such outliers can greatly alter full-panel industry averages.

Given that the aim is to distinguish fully between contextual influences and CEO influences, it is imperative to develop an industry control that pertains to the specific period in which each individual CEO serves *and* that is not conflated with his or her own performance. *Our proposed solution is to replace industry dummies (or grand means derived only from sampled firms) with annual industry performance indicators that are based on the performance of all firms in each industry (not just sampled firms), excluding the focal firm.* This solves both problems noted above, controlling for industry ups and downs, while also excluding the focal firm from this important peer-group control.

Misspecification of firm effects

Prior studies of CEO influence have controlled for firm-level factors by including dummy variables for every firm, in an effort to isolate that portion of variance in performance that is due to stable differences in the health and capabilities of companies. But, as with industry dummies, the use of firm dummies, or full-panel grand averages, greatly distorts CEO effects.

Just as each CEO's performance contributes to an industry average (as discussed above), it contributes even more to a *firm's* average across a data panel. An example helps. Let us assume a 20-year data panel for a hypothetical firm, in which year and industry tendencies are held constant (to simplify). CEO A is observed for the first 5 years of the panel and delivers ROA of four percent each year; then CEO B is on the scene for 10 years and delivers ROA of eight percent each year; and finally CEO C is represented in the final 5 years and delivers ROA of four percent each year. Thus, the overall 20-year average ROA for the company—against which each individual CEO's contribution is gauged—is six percent ($[5 \text{ years} \times 4\% + 10 \text{ years} \times 8\% + 5 \text{ years} \times 4\%]/20 = 6\%$). Logic would suggest that CEO B should be seen as having improved the company by four ROA points; but when viewed under the firm-dummy (or full-panel average) approach, she exceeded the firm average by only two points, even though she greatly contributed to generating that firm average. Under the grand

average, or firm-dummy approach, any distinctive effects of long-tenured CEOs are especially obscured, because their performance largely comprises their firms' grand averages.

Returning to our example, one could fairly conclude that CEO C oversaw a four-point deterioration in ROA (recall that we are assuming constant year and industry conditions); but the firm-dummy approach would treat it as only a two-point deficit (relative to the firm average of 6). Thus, instead of having at least two distinctively performing CEOs—one who exhibited breakout performance relative to what she inherited and another who oversaw a four-point decline in performance, customary methods would view this as three middling CEOs who all performed within two points of the firm's grand average. As this example shows, the inclusion of focal CEOs in calculating firm-level controls systematically suppresses evidence of distinctive CEO contributions, thus reducing the overall CEO effect.

More troubling, however, is that using an entire data panel to calculate a firm-level contextual control means that even the years *following* a CEO's tenure enter into such calculations. This is at odds with the aim of controlling for the pertinent context against which individual CEOs' contributions should be judged. It is logical to assess the impact of CEOs relative to what they encountered upon the start of their tenures; but it is not reasonable to treat performance after CEOs' tenures as part of the contextual standard against which they should be judged. Indeed, to the extent that CEOs take actions that have enduring effects *beyond* their tenures—say, by enhancing or impairing their companies' brands, technology pipelines, or cultures—their 'CEO effects' under the firm-dummy approach will be especially muted. Returning to our example above, if the performance improvement of CEO B had persisted through the tenure of CEO C (for example, such that CEO C delivered 5 years at the same 8% level), the firm's 20-year average performance would be even higher (7%), and CEO B's apparent contribution would be all the more diminished—precisely the opposite of what might fairly be attributed to CEO B.

It makes little sense to treat a CEO's own performance as part of his or her context; and it makes even less sense to treat the performance of subsequent leaders as part of a focal CEO's context. It would be far better to control for

the performance of the firm *upon the start* of each CEO's tenure—or the CEO's inherited performance. With such an approach, researchers can more reasonably assess whether (and which) CEOs 'make a difference.'

A nominal indicator of firm context is suitable, even ideal, if the researcher's aim is to detect sources of deviations from long-term averages. However, if the researcher's aim is to assess the degree to which an intervention, such as a new CEO, alters the trajectory of the firm (either briefly or enduringly), it is more appropriate to gauge firm context at the point that the intervention starts. Such a change in measurement will inevitably reduce the amount of variance explained by firm context, as the focal CEO's performance will no longer be included in the contextual predictor, but it will yield a far more accurate portrayal of CEO 'impact.'

But even the inclusion of inherited performance would not fully acknowledge the overall health and vitality of the companies that CEOs encounter. For instance, a CEO might enter a situation of poor profitability but with numerous preexisting ingredients for improvement—say, a strong brand, a promising technology pipeline, or a resilient culture. If this CEO delivers great profits, he should not get as much credit as if he inherited a situation of low profits *and* poor overall health. This lack of recognition of a company's preexisting vitality is one of the greatest limitations of existing methods for detecting CEO effects. From the resource-based view, it is known that some CEOs have abundant resources to work with, while others are faced with limited organizational strengths (e.g., Barney, 1991; Peteraf, 1993). These differing capabilities are not necessarily reflected in the profit levels encountered by new CEOs, and therefore need to be gauged distinctly by researchers. The actual performance CEOs deliver, then, must be assessed, insofar as possible, against the context of their organizations' overall resource endowments.

Our proposed solution is to replace firm dummies (or panel grand means of firm profitability) with controls both for inherited profitability and inherited company health. This would solve the problem of including performance of focal CEOs (and subsequent CEOs) in calculating contextual benchmarks, and it adds acknowledgement of pre-existing firm health.

EMPIRICAL DEMONSTRATION OF 'CEO IN CONTEXT' (CiC) MODEL

To demonstrate the differences between our new CiC technique and prior approaches for gauging CEO effects, we now present an empirical study with three parts. First, we examine how the aggregate CEO effect in a large sample of U.S. public corporations differs under our method compared to prior techniques. Second, as a validity test, we examine the CEO effect in subsamples of low-, medium-, and high-discretion industries, showing that our CiC estimation technique (relative to prior techniques) yields results more in line with accepted theoretical expectations. Third, through a case study and simulation, we show how our model leads to very different insights about the effects of some individual CEOs, compared to those obtained with typical techniques.

Sample

Our sampling frame was all CEOs listed in the Execucomp database (roughly the 1,500 largest U.S. corporations) for the years 1992–2011, but with some necessary filters applied. We excluded financial services firms, public sector and conglomerate firms, and firms otherwise 'unclassifiable' or 'miscellaneous' in their broader industries (McGahan and Porter, 1997). We also excluded any (four-digit SIC) industry that had fewer than four firms across the sample.

To be able to conduct traditional analyses (for comparison), we excluded CEOs who served only 1 year (or less); and we excluded the few CEOs who served for the entire 20 years of our panel, as their effects would be inseparable from their firm effects. We used multiple data sources (SEC filings, company news releases, and media accounts) to confirm every CEO transition for every firm. After applying these filters, the final sample included 44 (four-digit) industries, 315 firms, 830 CEOs, and 4,866 firm-years of data.

Measures

To demonstrate our new CiC estimation technique, we examined annual return on assets (ROA), calculated as net income divided by assets for each firm-year. As a robustness test, we also examined return on sales (ROS) and market-to-book value of common stock (MTB); although we

do not report these latter results in tables, we will briefly describe their corroborative patterns below. These measures are common indicators of firm performance and have been used in other studies of CEO effects (e.g., Crossland and Hambrick, 2007; Lieberman and O'Connor, 1972; Mackey, 2008).

In contrast to customary techniques, which have used industry and firm dummies (or full-panel grand averages), we developed more refined indicators of contextual conditions, in line with the proposals we set forth above. For our measure of industry conditions, we sought to develop an indicator that would reflect industry performance in each focal year but that excluded the arithmetic influence of the focal firm and CEO. We started by identifying, for each year, all firms in the Compustat database (which is far more comprehensive than Execucomp) with the same primary four-digit SIC as the focal firm. Then, excluding the focal firm, we calculated the size-weighted mean ROA for each industry for each year, calculated as the sum of all companies' profits divided by the sum of all companies' assets (again, excluding the focal firm). (Weighting by size provides a more reliable indication of industry performance, by limiting the influence of relatively small firms and appropriately capturing the greater impact of larger firms.)

To overcome the problems in using firm dummies (discussed above), we used two indicators of a company's condition upon each CEO's start. The first, inherited profitability, was the company's mean ROA for the two years prior to the CEO's start. The second, inherited company health, was a measure of investors' judgments about the vitality and prospects of the firm prior to each CEO's start. This was calculated as the ratio of the company's MTB divided by the industry median MTB (excluding the focal firm) at the close of the fiscal year prior to each CEO's start. For instance, if a company's presuccession MTB was 3.0, and the industry median MTB was 2.0, the inherited company health indicator for the focal CEO would be 1.5 (i.e., 3.0/2.0). For each of these firm-level controls, we conducted various robustness tests, using other calculations and industry adjustments, with results greatly consistent with what we report here. (For instance, among the alternative presuccession controls examined were a single year and the average of three years of profitability, recent changes in the firm's stock price, and the simple difference between a firm's MTB and industry MTB.) Because our firm-level indicators required

data from prior to each CEO's start, we gathered earlier requisite data on every CEO who was present at the start of our data panel (1992), so as to have exactly equivalent samples of CEOs under all techniques.

As with prior studies, we included dummy variables for the calendar years of our panel, thus capturing macroeconomic effects. And, we included dummy variables for all the individual CEOs in the sample, allowing examination of their aggregate effects as well as of individual CEOs' personal coefficients.

Model and estimation

Because our method incorporated continuous measures (rather than only dummies), we used generalized estimating equations (GEE) (Liang and Zeger, 1986). GEE has been used in prior studies using panel data to explain annual organizational performance (e.g., Henderson, Miller, and Hambrick, 2006; Phelps, 2010). Since our dependent variable was measured within each firm on an annual basis, we clustered our models at the firm level and specified an autoregressive structure, which accounts for the correlation among repeated measures in a panel. As the dependent variable was normally distributed, we specified a Gaussian (or normal) distribution and link function. Models were estimated using the *xtgee* function in Stata 12.1. See Hardin and Hilbe (2003) and Ballinger (2004) for detailed discussions of GEE.

Comparative results: aggregate CEO effects

As an initial demonstration, we sought to show how our CiC method yields substantially different insights about overall CEO effects, or the proportion of variance in performance attributable to CEOs, compared to prior methods. For the comparison analyses, we used sequential ANOVA (which has the longest legacy in this research stream, dating from Lieberman and O'Connor (1972)) and multilevel modeling (MLM) (representing the latest advances in this stream) (Crossland and Hambrick (2011)), operationalizing all predictors as sets of dummy or categorical variables for year, industry, firm, and CEO. When using sequential ANOVA, we introduced the variables in the following order: year, industry, firm, and CEO, in keeping with prior studies (e.g., Lieberman and O'Connor, 1972). For the MLM

Table 2. Partitioning of variance in ROA by model type: (% of variance in explained by category)

	Year	Industry	Firm	CEO	Unexplained
Sequential ANOVA	2.5	9.2	29.8	16.3	42.2
Multilevel modeling	2.1	3.2	24.2	20.4	50.2
CEO in context (GEE)	2.5	6.9	12.1	38.5	40.0

n = 4,866 firm-years; 44 four-digit SIC industries, 315 firms, and 830 distinct CEOs.

analysis, we used a four-level nested model: years (level 1) within CEOs (level 2) with firms (level 3) within industries (level 4) (e.g., Crossland and Hambrick, 2011). We used Stata commands *anova* and *xtmixed*, respectively, for the ANOVA and MLM analyses. (For detailed discussions of these methods, see Crossland and Hambrick, 2007, 2011; Mackey, 2008; McGahan and Porter, 1997; Misangyi *et al.*, 2006).

Results for the comparative analyses are shown in Table 2. Sequential ANOVA indicates the following partitioning of variance in ROA (in percents) for our sample: year = 2.5; industry = 9.2; firm = 29.8; CEO = 16.3; with 42.2 left unexplained. MLM generates slightly different results, including smaller industry and firm effects of 3.2 and 24.2, respectively, and a somewhat larger CEO effect of 20.4.

Because we have asserted that our new CiC method will yield a greater CEO effect than is obtained with customary analyses, in our GEE analysis we chose to mirror the conservative extraction of the CEO effect as is obtained when CEO is entered last in sequential ANOVA. As Rumelt (1991: 176) noted, 'strict tests for the presence of effects are possible only for the last [class of] effects fitted.' Thus, we ran a series of four GEE models, cumulatively adding year, industry performance, firm controls, and finally CEOs. As shown in the Appendix, the first model included calendar year (explaining 2.5% of variance in ROA); the second model added the refined industry control (6.9% more); the third model added the two inherited firm condition indicators (12.1% more). For our fourth model, in which CEO dummies were examined, the dependent variable was the residual ROA from Model 3; this approach assures that the fixed-effect coefficients for individual CEOs (to which we turn below) can be meaningfully interpreted as each CEO's net effect after completely controlling for contextual factors (adding the CEO dummies to the full model generates the same amount of

Table 3. Proportion of variance in ROA explained by CEOs in low-, medium-, and high-discretion industries

Model	Industry discretion subsamples		
	Low	Med	High
Sequential ANOVA	17.9	15.6	21.5
Multilevel modeling	21.0	16.2	30.3
CEO in context (GEE)	28.3	35.0	42.4
Firm-years	1,646	855	1,021
CEOs	273	151	181

variance explained, or R-squared, but yields less stable estimates for the individual CEOs). In this fourth model, the CEO dummies accounted for 49.1 percent of the residual ROA variance, or 38.5 percent of total variance in ROA.

Returning to Table 2, the final line summarizes results from our new method (as shown in the Appendix). The calendar-year dummies explain 2.5 percent of variance in ROA, exactly in line with the other methods. Our new indicator of industry performance (industry ROA in the focal year, excluding the focal firm), explains 6.9 percent of variance in ROA. It is not surprising that this is lower than seen with other methods, as our new industry indicator excludes the influence of the focal firm. Our refined measures of firm conditions—*inherited* profitability and *inherited* company health—together explain 12.1 percent of variance in ROA. Here too, it is understandable that this is an appreciably lower firm effect than obtained from the other methods, as our new firm indicators are completely *ex ante*, measured prior to the start of each CEO's tenure, without any recursive inclusion of the focal year or subsequent periods.

Finally, our CiC analysis points to a much greater CEO effect, 38.5 percent of variance, than is obtained with the other approaches. With our analytic refinements—which include a

more complete disentangling of CEOs' contributions from both industry and firm contextual conditions—the influence of CEOs on company profitability is roughly *double* the apparent influence seen when we apply prior methods to our sample.³ This refined approach provides the lowest unexplained variance as well.

Although not shown in a table, results for ROS and MTB were highly similar to those shown for ROA. With sequential ANOVA, the CEO effect on ROS was 13.6; with MLM, it was 14.2; and with the new CiC technique, it was 35.5. Similarly, sequential ANOVA generated a CEO effect on MTB of 15.6; using MLM, it was 22.3; and from the CiC technique, the CEO effect on MTB was 46.4. Thus, across multiple performance measures, our new estimation models yielded appreciably greater CEO effects than were obtained from customary approaches.

Validation test: industry discretion and CEO effects

As a way to demonstrate the enhanced validity of our new technique, relative to prior approaches, we sought to demonstrate that it yields estimates of CEO effects more in line with what would be expected from accepted theory about executive influence on performance. Specifically, we examined CEO effects in industry subsamples that differed widely in their degree of managerial discretion (Hambrick and Finkelstein, 1987).

Building on Hambrick and Finkelstein's (1987) initial theorizing about industry-level determinants of discretion, Hambrick and Abrahamson (H&A)

(1995) systematically generated discretion scores for an array of industries. They first asked a panel of academic experts to rate the amount of managerial discretion in 17 industries, finding a high degree of consistency among the raters, as well as strong agreement between the academics and securities analysts who specialized in each of the industries. They then examined the associations between panelists' ratings and those objective industry attributes that Hambrick and Finkelstein had proposed as conferring (or restricting) managerial discretion, e.g., R&D intensity, advertising intensity, market growth, and capital intensity. Using regression analysis, they were able to estimate the implicit weights the panelists assigned to specific attributes when rating overall industry discretion. H&A then applied those weightings of industry attributes to compute discretion scores for 53 additional industries; the scores for all 70 industries were eventually reported in Finkelstein and Hambrick (1996). At the top of the list were such industries as computers (6.62 on seven-point scale) and perfume and cosmetics (6.60); in the middle were such industries as drugstores (4.78) and hotels/motels (4.67); and at the bottom, with the lowest discretion scores, were such industries as steel production (2.08) and natural gas transmission (2.01).

The Hambrick and Abrahamson (1995) industry discretion scores have been used in various scholarly projects (Adams, Almeida, and Ferreira, 2005), yielding results that generally support the overall accuracy of the ratings. These subsequent studies, coupled with the original systematic development of the scores, suggest that H&A's ratings are largely valid indicators of industry-level managerial discretion.

Our own test, then, followed from a two-part logic. First, to the extent that managerial discretion confers managerial leeway, allowing executives to 'matter,' discretion should show up in the accepted metric for how much CEOs matter: the 'CEO effect,' as extracted from large-sample variance partitioning methodology (e.g., Crossland and Hambrick, 2011). *Industries that differ in their discretion scores should differ in their CEO effect scores. Specifically, the greater the discretion, the greater the CEO effect.* Second, assuming that H&A's discretion scores are generally valid, then whichever CEO effect estimation technique yields the strongest positive association with those scores can be deemed, itself, the most valid—in a test of

³ To verify that our use of GEE—as well as our use of continuous variable predictors—do not, in and of themselves, generate noncomparable effects, we examined results from three sets of analyses. First, we conducted sequential ANOVA with dummy indicators, with results as shown in the first row of Table 2. Second, we ran sequential GEE analyses with all dummy predictors; the results (except for minor right-of-decimal rounding differences) were identical. Third, we ran GEE analysis but replaced all the dummies with entity means—for each calendar year, each industry, each firm, and each CEO. Again, the results were the same. In short, GEE allows us to move away from dummy indicators of contextual conditions, but neither the GEE model nor the use of continuous variable predictors, per se, affect the results obtained. As a further aside, if we were to use each CEO's personal mean instead of a dummy indicator, the aggregate variance explained by CEOs would be exactly the same as shown in the final row of Table 2, but we would not be able to extract a coefficient for each CEO, which (as we discuss throughout) is one of the potentially most fruitful uses of CEO VPM analyses (especially as illustrated by Bertrand and Schoar, 2003).

convergent validity (Campbell and Fiske, 1959). Thus, *we anticipate that the association between H&A industry discretion scores and CEO effect scores derived from our new CiC technique will be stronger than for those CEO effect scores derived from prior techniques.*

To perform this validation test, we first identified all the industries in our sample that were also among those with discretion ratings (in Finkelstein and Hambrick, 1996), for a total of 32 such industries. We then divided the 32 industries into low-, medium-, and high-discretion subsamples, using the 33rd and 66th percentiles of the Finkelstein and Hambrick listing as cutpoints, yielding subsamples of 12, 10, and 10 industries, respectively.⁴ Then, repeating the analyses presented in Table 2, we extracted and compared the CEO effects across techniques. Results are shown in Table 3.

The new CiC technique generated results most in line with theoretical expectations, with the CEO effect increasing monotonically across the low- (28.3), medium- (35.0), and high-discretion (42.4) industry subsamples. Although all three techniques generated a larger CEO effect for the high-discretion than for the low-discretion subsample, the difference (by Fisher Z-test) was significant only for the MLM ($p < 0.02$) and CiC ($p < 0.01$) results. However, at odds with expectations, both ANOVA and MLM generated the *lowest* CEO effect for the medium-discretion subsample. Only the CiC method generated results completely in line with theoretical expectations, providing increased evidence of the new technique's enhanced validity relative to prior techniques.

Comparative results: individual CEO effects

Our new CEO in context method not only points to a much greater aggregate CEO effect than obtained from prior methods, but it also generates appreciably different indicators of the

⁴ Although the industry discretion scores reported by Hambrick and Finkelstein (1996) are based on industry attributes in 1985–1989, as measured by Hambrick and Abrahamson (1995), their relative rankings are highly stable over time. For instance, when we recalculated industry discretion scores using various five-year frames from our own sampling period of 1992–2011, they were consistently correlated at about 0.80 with the original scores. Three of the component indicators of industry discretion—R&D intensity, advertising intensity, and capital intensity (negatively)—are especially stable, thus contributing greatly to persistent rank-orderings of industry discretion over time. We opted to use the original scores because they have been subject to the most validation.

effects—and efficacy—of many individual CEOs. In this section, we assess these differences.

Our method allows for the extraction of a fixed-effect coefficient for every CEO, which can be interpreted as an indicator of that CEO's distinctive mark on the firm (after controlling for all other factors) (e.g., Bertrand and Schoar, 2003). In using CEO dummies (as we do), one CEO must be excluded; in turn, all other CEOs' coefficients refer to differences from the excluded CEO. To make these coefficients more interpretable, we went to lengths (in all our analyses) to exclude expressly a CEO whose performance was exactly what would be predicted from the contextual factors, i.e., whose residual ROA from a contextual model was zero. As such, individual CEOs' coefficients can be interpreted as differences from zero (or an absence of impact on the firm's performance). Thus, a coefficient of 2.0 (–2.0) for a given CEO would indicate that, during the CEO's tenure, the company's annual performance was 2.0 ROA points higher (or lower), on average, than contextual factors would have predicted.

When we examined the individual coefficients for the 830 CEOs in our sample for three techniques—ANOVA,⁵ MLM, and our new CiC—they covaried but were far from identical. Specifically, the correlation between the CEO coefficients generated by our new technique and those obtained from ANOVA was 0.61; and the correlation between coefficients from the new CiC analysis and MLM was 0.87. Although these correlations are large, they convert to coefficients of determination (r^2) of 0.37 and 0.76, respectively, suggesting (as we highlight below) that our method yields substantially different coefficients for some CEOs.

Case study

A case study helps to illustrate such differences. In Table 4 we show the performance and contextual control variables for two legendary CEOs of IBM, Lou Gerstner and Sam Palmisano, whose tenures fully comprise our data panel for that

⁵ Individual CEO fixed-effect coefficients are not obtainable directly from an ANOVA model. Thus, we used GEE to conduct a simulated ANOVA, as described in footnote 3, in which dummies for all predictors were entered in the following order: year, industry, firm, and CEO. This analysis generates results identical to actual ANOVA, and it reports individual CEO coefficients.

Table 4. Contextual conditions and fixed-effects coefficients for two IBM CEOs (ROA models)

Executive	Inherited conditions ^a			Industry ROA (excluding IBM)	Firm ROA	Average CEO ROA	CEO's fixed-effect coefficient (percentile in parentheses)		
	Profitability (%)	Company health	Year				ANOVA	MLM	New CiC method
Lou Gerstner	-4.4	0.36	1993	6.2%	-10.0%	5.2%	-3.7 (15.7)	-1.1 (20.5)	6.0 (92.2)
			1994	5.7	3.7				
			1995	5.1	5.2				
			1996	0.8	6.7				
			1997	0.1	7.5				
			1998	-2.7	7.3				
			1999	-6.1	8.8				
			2000	-11.2	9.2				
			2001	-11.2	8.7				
			2002	-8.9	3.7				
Sam Palmisano	9.0	4.16	2002	-8.9	3.7	9.4%	2.2 (83.2)	3.0 (92.0)	2.8 (78.8)
			2003	-1.5	7.3				
			2004	3.8	7.7				
			2005	7.7	7.5				
			2006	6.8	9.2				
			2007	6.5	8.7				
			2008	5.0	11.3				
			2009	8.2	12.3				
			2010	8.8	13.1				
			2011	7.5	13.6				

^a Inherited profitability is company's average ROA for two years prior to succession. Inherited company health is ratio of company's market-to-book (MTB) relative to industry median MTB at end of presuccession year.

company. Gerstner has been widely credited with saving IBM and leaving it exceptionally strong upon his retirement (Ante and Sager, 2002). The raw data validate Gerstner's acclaim. With inherited profitability of -4.4 percent and inherited company health of just 0.36 (i.e., IBM's MTB ratio was only about one-third the average MTB for its industry), Gerstner went on to deliver industry-beating performance in seven out of his nine years. Even when the technology industry faced a crisis in 2000 and 2001, IBM's strong performance continued. Despite this record, when looking at Gerstner's coefficients from the ANOVA and MLM models (-3.7 and -1.1, respectively), it would appear that Gerstner was a *poor* CEO; in fact, these coefficients (as obtained from the two customary analyses) place him in the bottom quartile of all CEOs in our sample. By comparison, our new CiC method yields a coefficient more in keeping with his actual contribution: 6.0, which is in the top decile of all CEOs in our sample.

Reviewing the data more closely, one can understand why these differences arise. Recall that all

prior studies of CEO effects, including those using ANOVA and MLM, account for industry and firm effects by calculating grand means from the entire data panel. As such, a CEO's own performance, as well as performance subsequent to the focal CEO, are all used in calculating the company (and industry) means. In this case, Gerstner inherited a poorly performing firm, delivered great performance, and left Palmisano a firm in excellent condition. Palmisano, the successor, built on this success. As a result, Gerstner's average performance was actually somewhat less than Palmisano's (mean ROA of 5.2% for Gerstner vs. 9.4% for Palmisano), less than IBM's overall panel mean (7.4%) and just slightly better than the full-panel industry mean ROA (4.4%). With ANOVA and MLM models, Palmisano's fixed-effect coefficients were greater than Gerstner's; but these models ignore the fact that Palmisano inherited a company performing better than its industry peers and with a market-to-book of more than four times the average of the industry. Under our CiC method, Palmisano has a coefficient of 2.8,

reflecting that, while he inherited a very healthy firm, he continued to improve it. In short, with our analytic changes, both Gerstner and Palmisano are seen in appropriately positive terms. The other methods, which calculate the grand mean across all years for the firm, overemphasize the positive impact of Palmisano and greatly diminish Gerstner's contribution.

Simulation

To further demonstrate the differences between the methods, we conducted a simulation by adjusting the performance of selected CEOs to observe how each model would react. Examining our full dataset, we randomly selected 30 CEOs who had positive fixed-effect coefficients (under all techniques) and artificially increased their ROAs by five points for every year of their tenures. To avoid any censoring, and to make the adjustments unequivocal, we only selected CEOs whose predecessors and successors were present in our data panel; as such, we adjusted the ROAs of these 30 selected CEOs upward by five points for their entire tenures, making them 'superstars.'

Logic would lead one to expect that the individual coefficients of these 30 CEOs, after the simulation adjustments, would correspondingly increase by five points. Under our method, that is exactly what happened—each increased by precisely five percentage points. However, under the other methods, the coefficients of these CEOs increased by far less than five points (for instance, the average increase with MLM was just 2.8 points), because some of the increased performance of these CEOs was assigned to industry and firm effects.

As a further experiment, we additionally adjusted the annual ROAs of the superstars' successors upward by 2.5 points (allowing the successors to enjoy some enduring benefits from the increased contributions of the superstars (à la Sam Palmisano)). With this adjustment, the coefficients under the conventional methods increased *even less* for the superstars (e.g., by only 2.3 points, on average, using MLM). This is because the enduringly increased performance of the superstars inflated the firm and industry averages used by ANOVA and MLM, thus further diminishing the superstars' apparent contributions. In contrast, our new technique continued to show full five-point increments for the superstars.

To reinforce that we are interested in accurately identifying both positive *and* negative CEO effects, we repeated the simulation by selecting 30 poorly performing CEOs and reducing their annual ROAs by five points. The results were symmetrical to what we described for the superstar simulation. Under our new CiC technique, the coefficients for the 30 new 'superduds' dropped by exactly five points. Under the other approaches, however, the drop was not nearly as great; and the more that we allowed the superduds' poor performance to persist into their successors' tenures, the less badly the superduds' coefficients looked. Said differently, if a CEO harms a firm so fundamentally as to impair its performance for years beyond his or her tenure, customary analytic techniques treat this as a sign that the company was an *inherently* poor performer, and the culprit CEO is scored rather forgivingly.

In sum, our new CiC technique generates substantially different individual coefficients for many CEOs than do prior analyses. With more accurate contextualization, our CiC especially yields larger coefficients—both positive and negative—for those CEOs (1) whose performance differs substantially from what their inherited conditions would predict and (2) whose contributions—for good or for ill—endure beyond their own tenures. Not only do conventional analyses underestimate the aggregate CEO effect, but they especially underestimate the effects of the most impactful individual CEOs.

DISCUSSION

We have introduced substantial analytic refinements for assessing the impact of business leaders on the performance of their companies. Although we follow prior researchers in using variance partitioning methodology to distinguish between contextual and executive influences on performance, we depart from others in how we gauge contextual factors. Instead of using full-panel dummy variables—or sample grand averages—to capture industry and firm factors, an approach that causes considerable blurring of executive and contextual influences, we use (1) an annual measure of industry performance that includes all firms in an industry (rather than just sample firms) but that excludes the focal firm's own performance and (2) two measures of the firm's condition and prospects

at the outset of each CEO's tenure. With these changes, we find evidence of a much greater overall CEO effect on performance from those obtained through typical methods, and we find that the indicators of many individual CEOs' contributions are very different from those obtained with customary approaches. We further show that this new technique more closely adheres to theoretically expected outcomes when applied to subsamples of differing levels of managerial discretion.

The overall CEO effect

As a way to adjudicate the long-standing debate about executive potency—between strategy and leadership scholars on the one hand and population ecology and neoinstitutional theorists on the other—researchers have conducted a series of studies aimed at answering the question ‘Just how much do CEOs matter?’ The answer, with limited exceptions, has been that CEOs account for about 10–20 percent of variance in profitability (Crossland and Hambrick, 2007, 2011; Lieberman and O'Connor, 1972; Mackey, 2008). Because of differences in sampling procedures, panel lengths, and other factors, comparisons *across* studies cannot be reliably drawn. However, when we use prior techniques *on our sample*, we also find a CEO effect in the 10–20 percent range: 16.3 percent with ANOVA and 20.4 percent with MLM. When Lieberman and O'Connor first reported such statistics, they equivocated about whether it signaled that CEOs matter a little or a lot. At one point, they said, ‘These results suggest that in emphasizing the effect of leadership, we may be overlooking far more powerful environmental influences (1972: 129).’ At another point, they wrote, ‘Thus, the leadership effect on company performance does matter (1972: 123).’

Since Lieberman and O'Connor's study, authors have invoked the ‘10–20 percent’ statistic to support whichever view they wished to emphasize. Some writers have used it as evidence that CEOs do not matter very much. For instance, in describing Lieberman and O'Connor's findings, Pfeffer and Salancik (1978: 10) said, ‘the magnitude of the administrative effect was dwarfed by the impact of the organization's industry and the stable characteristics of a given organization.’ Similarly, and much more recently, authors have invoked this research stream to assert that ‘little variance in organizational performance can be

attributed to differences in individual (CEOs)' (Podolny, Khurana, and Hill-Popper, 2005: 2). And iconic organizational theorist James March asserted that top executives of major corporations are ‘virtually interchangeable’ (Collingswood, 2009: 5). In contrast, Hambrick (2007: 341) portrayed the 10–20 percent figure as a glass half-full: ‘...evidence indicates that top executives have considerable influence over the form and fate of their companies.’

Results from our new CiC technique leave less room for ambiguity: at 38.5 percent for ROA (and 35.5 and 46.4 for ROS and MTB, respectively), the overall CEO effect on company performance is appreciably greater than obtained from customary VPM techniques (which, again, were generally in the 10–20 percent range for our sample). CEOs can—and often do—generate performance that is substantially different from what would be predicted by the conditions they inherited and what their peers deliver. Although contextual conditions (and unexplained factors) play major roles in influencing organizational outcomes, it seems that CEOs are not as overpowered by inertia, isomorphic pressures, and timidity as much as is often portrayed. Through a combination of strategic decisions, organizational (re)design, and daily leadership actions, some CEOs substantially alter the trajectories of their firms—sometimes favorably, sometimes unfavorably.

Our new CiC technique introduces changes in the measurement of two sets of contextual conditions: industry and firm factors. Moving away from the use of dummy variables for industries (which capture full-panel industry means), we instead use a measure of industry performance that is contemporaneous with each CEO's years in office *and* that excludes the focal firm's own performance.

Similarly moving away from the use of dummy variables for firms (which capture full-panel company means), we instead use indicators of each CEO's ‘inherited conditions’—prestart profitability and market valuation. Thus, we avoid the problem of treating the CEO's own performance as part of his or her context. Such a change inevitably brings about a substantial reduction in the firm effect (compared to other techniques), as each CEO's performance is no longer included in the firm-level control (for predicting the CEO's performance in a given year). However, it is not the case that our technique *automatically* shifts

some of the firm effect to the CEO effect. With our model, there can only be a CEO effect to the extent that CEOs are associated with performance that is *different* from what they inherited (and from peer firms). If CEOs do not matter very much, our technique would still show a smaller firm effect but *also* a small CEO effect. Our sizeable CEO effect indicates that some CEOs are able to alter the trajectories of their firms—perhaps appreciably more than previously thought.

Individual CEO effects

Our new technique also yields substantially different insights about the effects of individual CEOs on their firms. As our example of IBM CEOs Lou Gerstner and Sam Palmisano vividly illustrates, conventional methods place little weight on the condition of the firm prior to a CEO's start; in fact, for the first CEO in a data panel, customary methods make *no* acknowledgement of inherited conditions. Further, customary techniques embed the CEO's own performance into calculations of industry and firm contextual controls, thus suppressing his or her apparent effect; and they treat the company's performance *after* the CEO as part of his or her context, further diminishing the apparent effects of any CEO whose influence endures beyond his or her tenure. It seems, then, that customary methods especially mask the effects of the most distinctive CEOs—those who make large and enduring marks on their firms, either positive or negative.

Bertrand and Schoar (2003) pioneered the use of individual CEOs' fixed-effects coefficients as indicators of executive ability. We agree that such coefficients could be used to answer a wide array of research questions about CEO effectiveness (as we discuss momentarily); however, it is essential that such coefficients be as accurate and valid as possible, providing meaningful gauges of the actual contributions of individual CEOs, net of appropriate contextual conditions. Our method provides an enhanced path for such research to proceed.

Future research

Our method could be used to answer a host of important research questions. Perhaps most obviously, researchers could employ our type of analysis to determine how the aggregate CEO effect depends on the setting. Building on our results that

show different CEO effects depending on industry discretion, researchers may wish to revisit, for instance, Crossland and Hambrick's (2011) study of CEO effects in different countries. Researchers could also assess comparative conditions at the firm level, by exploring, for example, how the CEO effect differs in small vs. large firms, young vs. old firms, or firms with fragmented owners vs. concentrated owners. In short, our method allows increased precision in answering fundamental questions about when and where CEOs matter most (and least).

Researchers also can employ our refined method to study the influence and effectiveness of individual CEOs. Indeed, we see this as a particularly promising research avenue. Using CEOs' individual-level coefficients as a dependent variable, one could ask, for instance, What types of CEOs have the biggest effects—regardless of sign—on their companies? those with MBAs or those without MBAs? long-term insiders or outsiders? industry veterans or those from other industries?

Some researchers, of course, might be very interested in the signs, not just the absolute magnitudes, of individual CEOs' coefficients. For instance, researchers with an interest in governance might explore how various board attributes are associated with CEO effectiveness: What are the characteristics of boards that select better-performing versus worse-performing CEOs? Or, relatedly, how do CEOs' initial pay packages relate to subsequent CEO effectiveness? Our technique is suitable for any investigation in which an accurate context-controlled gauge of CEO effectiveness is important.

Our technique might also be highly relevant for strategy researchers who are interested in partitioning year, industry, and corporate (or parent) effects on the performance of business units (McGahan and Porter, 1997; Misangyi *et al.*, 2006; Rumelt, 1991). Customary VPM techniques, employing dummies or full-panel grand average indicators (summarized in Bowman and Helfat, 2001), allow researchers to assess any persistent influences from the predictive factors. But if they were interested, instead, in the degree to which business unit performance can *change*, or break from its *prior* path, investigators might benefit from using each business unit's prior performance (as opposed to panel-average performance) for explaining focal year business unit performance.

Researchers might also find ways to improve upon our technique. We experimented with alternative controls for each CEO's inherited conditions, including measures of presuccession ROA momentum and stock price momentum, with no appreciable differences from what we have reported; but other analysts might be able to identify more predictive *ex ante* firm conditions. Similarly, we experimented with various approaches for measuring industry conditions, with none yielding stronger results than what we have reported; but, here too, other researchers might identify additional refinements. It might be especially fruitful to incorporate lags into our analysis. Although we urge caution in assigning the first year or two of a CEO's performance to his or her predecessor's record (as Lieberman and O'Connor, 1972 experimented with), perhaps a suitable compromise would be to omit simply the first year of each CEO's record, treating it as an ambiguous and possibly anomalous hybrid of the old and the new leader.

SUMMARY

Do CEOs matter very much? We have developed a refined technique for addressing this centrally important question. By more completely disentangling contextual and CEO influences on company performance, we have shown that the CEO effect is appreciably greater than is obtained with customary techniques; with our new CEO in Context, or CiC, technique, about 38 percent of variance in ROA can be attributed to CEOs. Our technique also generates appreciably different insights about the influence and efficacy of many individual CEOs, perhaps most notably those whose performance differs dramatically from what they inherited *and* whose influence endures beyond their tenures—in short, the most impactful CEOs.

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Appendix: GEE models explaining firm ROA with percentage of variance explained by category

	(1)	(2)	(3)	(4)
	ROA	ROA	ROA	Residual ROA from Model 3
Calendar-year dummies	Included	Included	Included	—
Weighted mean industry ROA,excluding focal firm	—	0.47*** (0.03)	0.45*** (0.03)	—
Presuccession firm ROA (two-year mean)	—	—	0.26*** (0.02)	—
Presuccession market-to-book(relative to industry mean)	—	—	0.01*** (0.00)	—
CEO dummies	—	—	—	Included
Constant	0.04*** (0.01)	0.02*** (0.01)	0.00 (0.01)	0.00 (0.02)
Observations	4,866	4,866	4,866	4,866
Firms	315	315	315	315
Wald chi-square	153.0	392.8	654.0	4,866
R-squared (%)	2.5	9.4	21.5	49.1
Unexplained variance (%)	—	—	78.5	—
	Year	Industry	Firm	CEO^a
Incremental variance explained by category (%)	2.5	6.9	12.1	38.5

^a The variance explained by CEOs is calculated by multiplying the unexplained variance from Model 3 (79.8%) by the marginal R-squared from Model 4 (48.7%).