Frank Germann, Peter Ebbes, & Rajdeep Grewal

The Chief Marketing Officer Matters!

Marketing academics and practitioners alike remain unconvinced about the chief marketing officer's (CMO's) performance implications. Whereas some studies propose that firms benefit financially from having a CMO in the C-suite, other studies conclude that the CMO has little or no effect on firm performance. Accordingly, there have been strong calls for additional academic research regarding the CMO's performance implications. In response to these calls, the authors employ model specifications with varying identifying assumptions (i.e., rich data models, unobserved effects models, instrumental variable models, and panel internal instruments models) and use data from up to 155 publicly traded firms over a 12-year period (2000–2011) to find that firms can indeed expect to benefit financially from having a CMO at the strategy table. Specifically, their findings suggest that the performance (measured in terms of Tobin's q) of the sample firms that employ a CMO is, on average, approximately 15% greater than that of the sample firms that do not employ a CMO. This result is robust to the type of model specification used. Marketing academics and practitioners should find the results intriguing given the existing uncertainty surrounding the CMO's performance implications. The study also contributes to the methodology literature by collating diverse empirical model specifications that can be used to model causal effects with observational data into a coherent and comprehensive framework.

Keywords: chief marketing officer performance implications, marketing–finance interface, panel data, endogeneity, instrumental variable

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In 2008, Nath and Mahajan reported that "[Chief marketing officer] presence in the Top Management Team (TMT) has neither a positive nor a negative impact on firm performance" (p. 65). In the words of Barta (2011), this finding "has caused quite a stir [among marketers]" because "firms with a CMO on the management team are not more successful than firms without a CMO." Moreover, Frazier (2007, p. 3)[1] urged CMOs to "hide this publication" because they are being "rapped for having zero impact." In short, Nath and Mahajan's (2008; hereinafter N&M) (lack of) findings have made many marketers nervous (also see Shoebridge 2007; Simms 2008). Yet their work has raised an important question: Do CMOs matter?

There is some evidence that suggests that CMOs should positively influence firm performance. For example, using data from 1987–1991 (in contrast, N&M use 2000–2004 data), Weinzimmer et al. (2003) find a positive correlation between CMO presence and sales growth. It is also widely believed that CMO presence raises the importance of marketing in the C-suite, which in turn should bring the customer to the boardroom and thereby improve firm performance (e.g., Kerin 2005; McGovern et al. 2004). Moreover, using an event study of CMO announcements, Boyd, Chandy, and Cunha (2010) find that the impact of CMOs on firm performance is contingent on the managerial discretion available to them. In addition, using the same sample as in their 2008 study, Nath and Mahajan (2011) report that powerful CMOs have a positive impact on sales growth.

However, researchers still seem unconvinced about the CMO's performance implications, as evidenced by their strong calls for additional research. For example, Boyd, Chandy, and Cunha (2010, p. 1174) assert that "the CMO remains a rather enigmatic creature in academic literature" and "the scarcity of … research … is lamentable." Similarly, Nath and Mahajan (2011, p. 74) urge for additional research to "shed further light on the issue of CMO presence." Moreover, in summarizing the findings from a recent CMO survey, Moorman (2013) reports that demonstrating the value of marketing remains a general challenge for CMOs. Indeed, approximately 66% of the CMOs surveyed state that they are experiencing pressure from their chief executive officer (CEO) or board to prove the value of marketing. Moreover, due to their nonfindings regarding CMOs' performance implications, N&M is still oft-cited in the media (e.g., Whitler 2013).

Against this backdrop, we build on N&M's work and seek a more comprehensive examination than what is available in the literature to model the causal effect of CMO

[1]It seems that Frazier (2007) had access to the prepublication version of Nath and Mahajan (2008).

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presence on firm performance. Specifically, using N&M's research as a starting point, we extend their work in three important directions:

1. *Time horizon*: N&M included five years of data in their study. Considering that executives' performance effects take time to manifest (e.g., Hambrick and Fukutomi 1991; Miller and Shamsie 2001), we extend N&M's sample by seven years (i.e., for a total of 12 years [2000–2011] vs. five years [2000–2004]).
2. *Industries*. N&M included a wide range of industries in their data set but excluded certain others (e.g., Food and Kindred Products [e.g., General Mills]; Transportation Equipment [e.g., Ford Motor Co.]; Miscellaneous Manufacturing Industries [e.g., Mattel, Inc.]). We include these additional industries in our analysis.
3. *Model*. We consider several econometric extensions over N&M. First, N&M employed between-effects regression and, thus, did not exploit the panel structure of their data. For relatively short time series (five years for N&M), using a between-effects model might be appropriate; however, because we include considerably more years in some of our samples, we explicitly exploit the panel structure by including time and firm effects in our analyses, which corrects for common unobserved time shocks and unobserved time-invariant firm-specific factors, respectively. Second, on the basis of results from a residual test, N&M treated CMO presence as exogenous; however, CMO presence is likely the result of a strategic firm decision, and treating CMO presence as an exogenous regressor might result in inconsistent estimates. We address the potentially endogeneous CMO effect by (a) exploiting the panel structure of the data and (b) using instrumental variables (IVs).

Our results are promising and suggest that CMO presence causally influences firm performance. As Table 1 shows, we first replicated N&M as closely as possible using the between-effects model and data similar to N&M (Study 1a); as one would expect, doing so yielded the familiar (non)results. Building on N&M, in Study 1b, we estimated additional models other than the between-effects model; the results reveal a positive and significant CMO effect in IV models that correct for the endogeneity of CMO presence. We then added additional firms/industries (Study 2) and find a positive and significant CMO effect across all models except the between-effects model. Next, in Study 3, we added several years to our initial sample (i.e., 2000–2011 vs. 2000–2004), and the results again reveal a positive and significant CMO effect in all but the between-effects model. Finally, in Study 4, we added additional industries and years to our initial sample; the CMO effect was again positive and significant in all models but the between-effects model.

TABLE 1
Overview

	Study 1a	Study 1b	Study 2	Study 3	Study 4
Industries sampled	Cross section of industries	Same as Study 1a	Studies 1a and 1b + additional industries	Same as Studies 1a and 1b	Same as Study 2
Firms sampled	Firms that report both advertising and R&D with sales of at least $250 million in 2002	Firms that report both advertising and R&D with sales of at least $250 million in 2002	Firms that report both advertising and R&D with sales of at least $250 million in 2002	Firms that report both advertising and R&D with sales of at least $250 million in 2002	Firms that report both advertising and R&D with sales of at least $250 million in 2002
Firm observations	123 firms	123 firms	155 firms	123 firms	155 firms
Time frame	2000–2004	2000–2004	2000–2004	2000–2011	2000–2011
Firm–year observations	615 (Sample 1)	615 (Sample 1)	775 (Sample 2)	1258 (Sample 3)	1604 (Sample 4)
Models used	Between-effects regression	Between-effects OLS	Between-effects OLS	Between-effects OLS	Between-effects OLS
		Random-effects	Random-effects	Random-effects	Random-effects
		Fixed-effects	Fixed-effects	Fixed-effects	Fixed-effects
		OLS with lagged DV	OLS with lagged DV	OLS with lagged DV	OLS with lagged DV
		2SLSRE	2SLSRE	2SLSRE	2SLSRE
		Random effects with control functions	Random effects with control functions	Random effects with control functions	Random effects with control functions
		HT[a]	HT[a]	HT[a]	HT[a]
		BB[b]	BB[b]	BB[b]	BB[b]
CMO effect on Tobin's q	Not significant	Significant and positive in some models	Significant and positive in all but the between-effects model	Significant and positive in all but the between-effects model	Significant and positive in all but the between-effects model

[a]Hausman and Taylor (1981).
[b]Blundell and Bond (1998, 1999).
Notes: In Study 1a, we attempted to replicate N&M as closely as possible. R&D = research and development, OLS = ordinary least squares, DV = dependent variable, and 2SLSRE = two-stage least-squares random effects.

Thus, our findings suggest that firms can indeed expect to benefit financially from having a CMO at the strategy table. More specifically, our data and analyses suggest that firm performance (i.e., Tobin's q) for firms with a CMO is approximately 15% better than that of firms without a CMO. The positive and significant effect of CMO presence is confirmed when we use excess stock returns (i.e., Jensen's α) as an alternate performance metric.

Beyond the substantive insight that CMOs provide value, our article also makes methodological contributions. Recognizing the nature of our data (i.e., secondary data on a large number of firms over several years), we identify and describe in detail four observational data modeling approaches that could be adopted to study complex marketing strategy phenomena such as the CMO's performance implications: (1) rich data models, (2) unobserved effects models, (3) IV models, and (4) panel internal instruments models. We hope that our discussion will encourage readers to see themselves as regression engineers—that is, understand the meaning of the model identifying assumptions—as opposed to "regression mechanics"—that is, estimate a large number of models and report the best results (Angrist and Pischke 2009, p. 28).

In what follows, we first briefly revisit N&M. We then discuss our modeling framework, describe our data and samples, and present our findings. We conclude with a discussion of our findings and the limitations of our research.

Background

We begin by providing the relevant aspects of N&M's study and then outline how we build on their research. N&M explore two related research questions: (1) What organizational factors are associated with CMO presence in TMTs? (2) Does CMO presence affect firm performance? They find that several firm factors such as the level of innovativeness (i.e., research and development [R&D] to sales ratio) and differentiation (i.e., advertising to sales ratio) are associated with the likelihood of CMO presence in the TMT. They also find that CMO presence has no statistical effect on firm performance, neither by itself nor when analyzed jointly with the various firm factors (e.g., interactions between CMO presence and innovativeness). This nonfinding regarding the CMO's performance implications forms the crux of our research.

Over five years (2000–2004), N&M observed 167 firms from a cross section of industries. They included all firms from the Compustat database with sales of at least $250 million in 2002, the midpoint of their observation period. From this set, they retained only firms without missing data on variables such as advertising and R&D as well as their two dependent variables, Tobin's q and sales growth. An executive listed in the TMT with the term "marketing" in his or her title implied CMO presence. As we discuss in our "Samples and Measures" section, we carefully followed N&M's data collection approach. We note that, in addition to Tobin's q and sales growth, we assess the CMO's impact on excess stock return (i.e., Jensen's α) and firm systematic and idiosyncratic risk.

Model Specification and Identification

Our primary objective is to establish the causal link between CMO presence or absence (simply referred to as "CMO presence" hereinafter) and firm performance. One might think that a simple model, such as that shown in Equation 1, in which one regresses firm performance on the dummy variable indicating CMO presence, would estimate this effect:

$$FP_{it} = \beta_0 + \beta_1 CMO_{it} + \epsilon_{it}, \tag{1}$$

where FP_{it} indicates the firm performance of firm i in year t, $CMO_{it} = 1$ if firm i had a CMO during year t and 0 otherwise, and the error term ϵ_{it} represents unexplained variation in FP_{it}.

However, this simple model suffers from serious limitations. For example, performance is driven by many other variables, such as advertising spending and organizational culture, and these other variables might also influence CMO presence. Information on these potentially important variables is usually not available; it does not seem to be available in our data, in which, for example, we observe advertising spending but not organizational culture. Thus, an omitted variable bias exists that may result in issues of endogeneity involving the CMO presence variable (e.g., Wooldridge 2002). This omitted variable bias also emanates from the recognition that CMO presence is likely based on a (nonrandom) strategic decision of a firm to maximize its performance; that is, firms hire a CMO because they believe that the CMO will help improve performance. Thus, one would need information on all input variables that go into the strategic CMO presence decision to correct for potential biases introduced by the nonrandom nature of CMO presence. However, because information on all such input variables is usually not available—it is not in our case—the belief that CMO presence is based on a strategic firm decision again induces endogeneity concerns due to omitted variables. Thus, the causal effect of CMO presence on firm performance is not identified in the simple model shown in Equation 1. In the words of Rossi (2014, p. 16), we have a strong case for "first-order" endogeneity.

Recognizing the panel nature of our data, we consider four potential approaches to model observational data to establish the causal link between CMO presence and firm performance: (1) rich data models, (2) unobserved effects models, (3) IV models, and (4) panel internal instruments models.[2]

[2]To conceptually identify the causal effect of CMO presence on firm performance, it is useful to think of an ideal experiment (as discussed in Angrist and Pischke 2009, 2010) in which one randomly assigns the CMO position to half the firms (treatment group) included and then tracks both types of firms (i.e., treatment and control groups) for several years to assess the impact of CMO presence. In such an experiment, random assignment would likely equalize the two groups in terms of unobserved variables, and the assignment of CMO would be random and no longer strategic (within the bounds of randomization as noted by Leamer 1983). Such an experiment is, of course, not practical; thus, we need to investigate nonexperimental solutions that can be used with observational data.

We discuss these models next, and we provide a summary of the models, including their identification assumptions, in Table 2.

Rich Data Models

The first approach is to collect an extensive data set such that no conceivable control variable (or proxies of such a variable) that correlates with both CMO presence and firm performance is omitted; we refer to these models as rich data models. Such a data set would result in the model shown in the following equation:

$$(2)\qquad FP_{it} = \beta_0 + \beta_1 CMO_{it} + \beta_2 X_{it} + \epsilon_{it},$$

where the matrix X_{it} captures the time-invariant and time-variant control variables.[3]

If such an extensive set of control variables were available, ordinary least squares (OLS), which exploits both within and between dimensions of the data, could be used to estimate the model (Section 7.8; Wooldridge 2002). Although we cannot claim with high confidence that no important variable is missing in our data, we report OLS results (i.e., using Equation 2).

Moreover, if one repeatedly observes the same firms over time, as we do in our panel data set, it is unrealistic to assume that the error terms for the same firm from different time periods are uncorrelated. Therefore, a popular alternative to the model shown in Equation 2 is the random-effects panel data model that assumes a composite error: $\epsilon_{it} = \alpha_i + u_{it}$, where α_i is a firm-specific random error term that captures unobserved firm-level effects and u_{it} is the random component that varies across firms and over time. Random effects α_i are assumed to be i.i.d. (usually a normal distribution) and capture all correlation of the error terms over time. Due to the random nature of firm-specific intercepts, the random-effects model exploits within and between variance more efficiently than OLS.[4]

An alternative to the random-effects model is to only exploit differences between firms, thereby ignoring the time-series information. Such a model, referred to as the between-estimator model (e.g., Verbeek 2012, p. 382), requires obtaining OLS estimates with firm-level means; that is,

$$(3)\qquad \overline{FP}_i = \beta_0 + \beta_1 \overline{CMO}_i + \beta_2 \overline{X}_i + \overline{\epsilon}_i,$$

where $\overline{FP}_i$ denotes the average financial performance for firm i across the years of observation (and similarly for the other variables), and $\overline{\epsilon}_i = \alpha_i + \overline{u}_i$ is the firm-level averaged error term.

The identifying assumptions of the between-estimator are similar to those of the OLS and random-effects model: data are available on all important variables, and explanatory variables are uncorrelated with the composite error term (that contains the missing variables). Although this model can be used to estimate the CMO effect (i.e., β_1), it is generally preferable to use the random-effects estimator—particularly when the panel time-series dimension becomes longer—because it exploits between and within dimensions of the data. To be consistent with N&M (see their Table 3), we estimate the between-effects model as well.

Unobserved Effects Models

Unobserved effects models control for omitted variables by either using firm-specific fixed effects or a lagged dependent variable as a control variable. These models exploit the fact that panel data have multiple observations for every firm, which enables modeling firm-specific intercepts or state dependence through a lagged dependent variable.[5] Building on the model shown in Equation 2, the fixed-effects model includes the firm-specific intercept α_i; that is,

$$(4)\qquad FP_{it} = \alpha_i + \beta_1 CMO_{it} + \beta_2 X_{it} + u_{it},$$

where α_i are now fixed unknown constants and u_{it} is i.i.d. over firms and time.

Typically, the firm-specific intercepts are not estimated; instead, the fixed-effects approach usually estimates the model in deviations from firm-level means. Thus, the fixed-effects model does not require the assumption that CMO_{it} is uncorrelated with α_i. However, it does require the assumption that $E(CMO_{it} \times u_{is}) = 0$ for all s, t (and similarly for X_{it}) so that the regressors are strictly exogenous. Thus, the identifying assumptions underlying the fixed-effects model are that (1) the omitted variable(s) is (are) time invariant (i.e., the firm-specific intercept captures the omitted variable[s]) and (2) there is enough variance in the dependent variable as well as the focal endogenous variable within firms to allow estimation of its effect (i.e., the focal endogenous variable—in our case, CMO presence—is identified only through within-firm variation). We note that after including firm fixed effects, all time-invariant between-firm variation is removed. Typically, when the time dimension is short (e.g., as in Zhang and Liu 2012, in which they have seven daily observations for all units of analysis), the identifying assumptions of a fixed-effects model are easy to justify. However, in our case, the sample period ranges from 5 to 12 years; thus, the identifying assumptions for the fixed-effects model might not be easily defensible. Yet we believe it is worth exploring this model's results as one set of results among others. We note that we also include time fixed effects in the model to tease out any unique year-to-

[3]Here, it is necessary to assume that $E(CMO_{it} \times \epsilon_{it}) = 0$ and $E(X_{it} \times \epsilon_{it}) = 0$; (i.e., CMO_{it} and X_{it} are exogenous) and some other mild regularity conditions (e.g., Verbeek 2012, p. 373). In other words, we would need to assume that the set of control variables captures all unobserved variation that would otherwise be part of the error term correlating with CMO. All remaining unobserved variation in the error term ϵ_{it} only correlates with the DV (i.e., firm performance FP_{it}) but not systematically with CMO_{it} or X_{it}.

[4]As is common, we use feasible generalized least squares to estimate the random-effects model, where it is typically required that $E(CMO_{it} \times u_{is}) = 0$ and $E(X_{it} \times u_{is}) = 0$ for all s, t (i.e., strict exogeneity), and $E(\overline{CMO}_i \times \alpha_i) = 0$ and $E(\overline{X}_i \times \alpha_i) = 0$ (e.g., Verbeek 2012, p. 384).

[5]In theory, one can specify a model with both firm-specific time-invariant effects and a lagged DV, as we do subsequently in Equation 7. However, estimating such a model requires strong assumptions (e.g., Nickell 1981), as we discuss in the "Panel Internal Instruments Models" section.

TABLE 2
Summary of Model Specifications

Model Type	Estimator	Description	Data Requirements	Main Identifying Assumptions
1. Rich data models	OLS estimator	Treats data as coming from independent and repeated cross sections and exploits the within and between dimensions of the data.	Rich data capturing all potential omitted variables are needed such that all variables that covary with firm performance and CMO presence are present or proxied for.	Given the rich set of control variables, all correlation between CMO and the error term is controlled for. That is, all variables that are likely to influence CMO presence and firm performance are captured by the included control variables. We note that there are minor differences in assumptions between the three rich data estimators: the assumptions are slightly stronger for between effects and random effects than for OLS, because of strict versus contemporaneous exogeneity (for details, see Verbeek 2012, p. 384). The rich data models assume that all control variables are uncorrelated with the error term; that is, the control variables are exogenous.
	Between-effects estimator	Assumes that the averages of the variables over time are representative of the cross-sectional unit and exploits the between dimension of the data.		
	Random-effects estimator	Acknowledges that firms are repeatedly observed over time and dependencies over time are attributed to a firm-specific intercept; it exploits the within and between dimension of data efficiently.		
2. Unobserved effects models	Fixed-effects estimator	Firm-specific effects capture time-invariant firm-specific omitted variables and wipe out the between-firm variance, thus exploiting the within dimension of the data.	Few time-varying control variables but no instrumental variables are needed. The dependent variable and key control variables should exhibit sufficient within-firm variance.	The omitted variable and/or the process causing the endogeneity bias do not vary over time. In addition, all other correlations between CMO and the error term are controlled for by firm time-varying (exogenous) control variables.
	Lagged dependent variable estimator	The first lag of the dependent variable is included as an explanatory variable.	Few control variables (time varying or invariant) but no IVs required.	The firm time-varying omitted variable(s) is (are) adequately captured by the lagged dependent variable and the other included (exogenous) control variables. No serial correlation in the error term.

TABLE 2
Continued

Model Type	Estimator	Description	Data Requirements	Main Identifying Assumptions
3. IV models	IV estimator using two-stage least squares (2SLS)	Treats data as coming from a cross section, corrects for an endogeneity bias by using instrumental variables that correlate with the endogenous regressor but not with the error term, and exploits within and between dimensions of data.[a]	Data on IV and some control variables (e.g., advertising spending in our case) so that the reliance on IV can be reduced to the extent possible.	It is necessary to conceptually identify a valid instrument meeting the instrument relevance and exclusion restriction (and provide theoretical justification for instrument relevance and the exclusion restriction). Identification of the instrument requires deep institutional knowledge and information on processes that determine the endogenous regressor. The strength of the instrument also needs to be assessed empirically and communicated.
	Two-stage least-squares random effects (2SLSRE)	The panel nature of the data is explicitly recognized in addition to the IV(s); that is, 2SLSRE exploits within and between dimensions of data efficiently.		
4. Panel internal instrument models	Hausman and Taylor (1981) estimator	Based on similar idea as fixed-effects estimation (i.e., deviations from firm means can be used to control for time-invariant unobserved effects), uses both within and between dimensions of the data, and is potentially more efficient than fixed-effects estimators.	Few control variables (time-varying or invariant) but no IV(s) are needed.	Same assumptions as the fixed-effects estimator.
	Blundell and Bond (1998, 1999) generalized method of moments estimator	Relies on past values of the endogenous variables to construct (internal) IVs and can be used in the presence of persistence when other such estimators (e.g., Arellano and Bond 1991) are not useful.		Lagged measures of the endogenous variables (note that in addition to the [lagged] CMO variable, the lagged dependent variable is also endogenous here) are good predictors of the change in the endogenous variables but do not correlate with the change in the firm-specific error term; some assumptions on serial correlation are required.[b]

[a]Endogeneity can arise due to omitted variable(s), simultaneity, and/or measurement error (e.g., Wooldridge 2002). In our case, the explanatory variable is a strategic choice variable. Unless data on all important determinants of the explanatory variable that also affect the dependent variable are available, there is a potential endogeneity bias due to omitted variables. We note that standard 2SLS treats data as coming from a cross section. Thus, in the case of panel data, 2SLSRE is potentially more efficient. In addition, when the endogenous independent variable is discrete (0/1), 2SLSRE may still be applied; however, depending on the empirical context, a potentially more efficient approach is obtained by assuming a probit model for the first stage. In that case, various IV estimators can be applied (e.g., Wooldridge 2002, procedures 18.1 or 18.4). We adapt the control function approach outlined in procedure 18.4 of Wooldridge (2002) in Model 7.

[b]As should be evident, the intuition behind the IV that is paramount in the IV models is lost in panel internal instruments models; thus, from a conceptual standpoint, it is usually difficult to justify the instrument derived from lagged measures or changes in lagged measures (also see Roodman 2009).

Notes: The descriptions, identifying assumptions, and suggestions in this table should be viewed as rules of thumb and/or heuristics that we developed from our reading of the literature and experience with data.

TABLE 3
Correlations and Descriptive Statistics (Based on Sample 4)

Variables	Correlations															
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1. CMO presence	1.00															
2. Tobin's q	.14	1.00														
3. Innovation	.16	.16	1.00													
4. Differentiation	–.02	.08	–.24	1.00												
5. Corporate branding	.06	.03	.09	–.16	1.00											
6. Total diversification	–.01	–.17	–.14	–.04	–.13	1.00										
7. CEO tenure	.02	.12	.02	–.15	.18	–.04	1.00									
8. Outsider CEO	.03	–.05	.02	.16	–.03	.08	–.12	1.00								
9. Market concentration	.06	–.05	.17	–.02	–.16	.00	–.03	.01	1.00							
10. Log(number of employees)	.04	–.02	–.20	.08	–.07	.25	–.11	–.08	.07	1.00						
11. COO presence	–.03	.04	–.06	.06	–.02	–.07	.10	.01	–.01	–.05	1.00					
12. Return on assets	–.04	.25	–.45	.10	.00	–.03	.03	–.08	–.20	.14	–.02	1.00				
13. Sales growth	.03	.34	.06	–.04	–.03	–.07	.06	.01	–.01	–.11	.07	.14	1.00			
14. Tobin's q (t – 1)	.11	.50	.10	.05	.03	–.12	.03	–.01	–.05	–.07	.02	.13	.38	1.00		
15. Systematic risk	.03	.01	.14	–.10	.16	–.07	.00	.06	–.07	–.10	–.03	–.17	.05	.05	1.00	
16. Idiosyncratic risk	.05	.06	.13	–.04	.07	–.12	–.04	.15	–.02	–.38	.03	–.25	.11	.16	.25	1.00
Summary Statistics																
M	.36	.50	–.03	.02	.43	.49	86.7	.29	508	2.16	.29	.07	.02	.76	1.04	.02
Mdn	.00	.11	–.01	.00	.00	.00	60.0	.00	381	1.91	.00	.06	–.01	.14	1.03	.02
SD	.48	1.40	.10	.04	.50	.50	85.3	.45	453	1.53	.45	.16	.22	3.39	.39	.01
Max	1.00	12.9	.65	.26	1.00	1.00	492	1.00	4306	6.14	1.00	.77	2.42	78.5	2.73	.12
Min	.00	–2.08	–.39	–.04	.00	.00	1.00	.00	.04	–1.68	.00	–2.14	–.86	–2.16	–1.17	.01

Notes: Following N&M, Tobin's q, innovation, differentiation, return on assets, sales growth, and Tobin's q (t – 1) are the raw values less the median values at the two-digit Standard Industrial Classification (SIC) level.

year fluctuations (e.g., boom and bust cycles that uniformly influence all firms in our sample).[6]

Moreover, if one believes that current firm performance is influenced by the past (e.g., past firm decisions, carry-over effects), the introduction of a lagged dependent variable might control for otherwise omitted variables and effects. Specifically, time-varying unobserved effects (as opposed to time-invariant unobserved effects, as in the fixed-effects model) can be captured by including the lagged dependent variable (in our case, firm performance) as an additional control variable in the model. We can represent such a model as follows:

$$(5)\qquad FP_{it} = \beta_0 + \beta_1 CMO_{it} + \beta_2 X_{it} + \rho FP_{it-1} + \epsilon_{it},$$

where FP_{it-1} is the firm performance of the previous time period and is included in the set of control variables.

Including a lagged dependent variable as a control variable in the model typically reduces the autocorrelation in the model (but does not necessarily eliminate it). Considering the model shown in Equation 5, the identifying assumption for the CMO presence effect is that (1) the omitted variables are fully accounted for by the lagged dependent variable and (2) there is no serial correlation present in the error term ϵ_{it}. If these identifying assumptions are met, the model would correct for firm-specific, time-varying omitted variables. Such a model exploits both within- and between-firm variance and can be estimated with OLS, as we do subsequently.

IV Models

If we cannot claim on theoretical grounds that the CMO effect is uncorrelated with the error term ϵ_{it} in Equation 2 (e.g., due to omitted variables), an alternate approach is to find one or more IVs that correlate with the CMO variable but not with the unobserved determinants of firm performance (that form part of the error term); that is, we must find IVs that meet the instrument relevance criterion and the exclusion restriction (e.g., Angrist and Pischke 2009). The exclusion restriction can be thought of as introducing an additional equation to Equation 2 to explain/predict CMO presence:

$$(6)\qquad CMO_{it} = \gamma_0 + \gamma_1 Z_{it} + \gamma_2 X_{it} + v_{it},$$

where Z_{it} is the IV that is excluded from Equation 2. In addition γ_1 must be $\neq 0$ and, similar to X_{it}, Z_{it} must be exogenous; that is, the IV must not be correlated with the model error term in Equation 2 and $E(Z_{it} \times \epsilon_{it}) = 0$.

The use of a "good" IV enables one to "partition the variation [in CMO presence] into that which can be regarded as clean or as though generated via experimental methods, and that which is contaminated and could result in an endogeneity bias" (Rossi 2014, p. 655). Unfortunately, good IVs can be difficult to find. Thus, Angrist and Pischke (2009, p. 117) argue that finding IVs requires "a combination of institutional knowledge and ideas about processes determining the variable of interest." Likewise, Rossi (2014) notes that good IVs need to be justified using institutional knowledge because there is no true test for the quality of IVs (for a discussion on statistical tests to examine the quality of instruments, see Rossi 2014).[7]

We use CMO prevalence among the sample firms' peers as our primary IV. We define peer firms as those sample firms that operate in the same primary two-digit Standard Industrial Classification (SIC) code(s) as the focal firm and meet other criteria (e.g., sales greater than $250 million in 2002) for inclusion in our sample.[8] Specifically, for each j = 1, …, J SIC code (where J = 44 in our sample), if there are i = 1, 2, …, N_j firms in the code, the peer influence variable for firm i (considering that the firm only belongs to one primary SIC code j) would be the number of firms with a CMO in code j other than firm i divided by $N_j - 1$. Moreover, if a firm belongs to multiple primary two-digit SIC codes, we calculate a weighted average of CMO prevalence using the number of sample firms in each primary SIC code as the weight to calculate the IV. Most firms are listed in multiple primary two-digit SIC codes, and these codes tend to change over time within a firm; therefore, the value of the peer CMO prevalence IV generally differs across firms and over time.

To verify that CMO prevalence among peer firms is a good IV, we need to, on the one hand, demonstrate instrument relevance (i.e., that the IV predicts CMO presence) and, on the other hand, argue that the IV meets the exclusion restriction (i.e., establish that the IV does not correlate with the error term that contains the omitted variables). In terms of instrument relevance, we need to conceptually make the case that CMO prevalence among peer firms correlates with CMO presence of the focal firm. Our argument

[6]Time-invariant variables in X_{it} are not allowed in the fixed-effects approach and are thus eliminated from the fixed-effects model during estimation. If interest lies in such regressors, this is a high price to pay for choosing the fixed-effects model over, for example, the random-effects model.

[7]Two other methods are also often used that parallel the use of IV approaches: (1) control functions and (2) matching approaches. Similar to IV methods, the control function approach requires that the exclusion restriction is met and uses information from the excluded variable to introduce a term for unobserved variables in the regression specification; thus, a control variable is introduced for the unobserved variables. Matching on the basis of the propensity score relies on creating an appropriate control group to compare with the treatment group and, thus, eliminates the need for instrumental variable(s). If treatment and control groups can be obtained through matching, a difference-in-difference estimator can be used to obtain the causal effect of CMO presence. For a comparison of instrumental variables, control function, and matching procedures using propensity scores, see Heckman and Navarro-Lozano (2004).

[8]We used the Compustat Segments database to identify all primary two-digit SIC codes to which our sample firms belonged in each sample year. We note that while all firms have one primary two-digit SIC code assigned (referred to as principal two-digit SIC code hereinafter; see Tables WA1 and WA2 in the Web Appendix), many firms belong to more than just one primary SIC code, and we use this information to calculate the IV. For example, in 2000, Procter & Gamble belonged to three primary two-digit SIC codes: 20 (Food and Kindred Products), 26 (Paper and Allied Products), and 28 (Chemicals and Allied Products).

here rests on two primary premises. First, we argue that the focal firms face similar market conditions as the peer firms because the firms operate in the same industry(ies). Second, we assert that the expectations of the focal firm and the peer firms are similar because we limit our sample to large firms (more than $250 million in sales in 2002) that invest in both advertising and R&D, functions often related to the marketing department. Thus, similar market conditions and similarity of expectations should make the instrument relevant.

What remains to be argued is why our instrumental variable meets the exclusion restriction—that is, why it is uncorrelated with the omitted variables that affect the focal firm's financial performance. There are two types of omitted variables of concern. First, there are firm-level variables such as organizational culture (as discussed previously). Here, we argue that the peer firms collectively either cannot observe or measure the focal firm's omitted variable(s) or cannot act on those variable(s) strategically. For example, organizational processes and cultures that are difficult to measure and quantify are embedded in an organization's fabric and thus become difficult to imitate (e.g., Granovetter 1985; Grewal and Slotegraaf 2007). Indeed, such processes are often difficult for the firms themselves to imitate (e.g., for the case of Saturn, see Kochan and Rubinstein 2000). Consequently, many firms have open secrets that are a source of competitive advantage (Barney 1991). In addition, some sources of competitive advantage, such as patents, have legal protection and thus are difficult to imitate as well. Furthermore, because most of our sample firms belong to several primary two-digit SIC codes, a large number of firms is used to calculate a focal firm's CMO prevalence-based instrument.[9] Thus, it seems highly unlikely that peer firms will take collective action against a single competitor and then also form other alliances similar in spirit to act against other competitors (which are also part of the other alliances these firms form). We conclude that it is unlikely that our instrument would relate to a focal firm's omitted variables (e.g., organizational culture) because (1) such variables may be difficult to assess and (2) collective action is difficult to manage. Therefore, the instrument should be uncorrelated with the omitted variable and, thus, the error term that contains the omitted variable, thereby meeting the exclusion restriction.

The second type of omitted variables of concern are exogenous shocks that may systematically influence firm performance and CMO prevalence over time, thereby creating a correlation between CMO prevalence and the error term (which contains the exogenous shocks). Such shocks could include economy-wide boom and bust cycles, in which the health of the economy tends to dictate organizational marketing emphases (e.g., Srinivasan, Lilien, and Sridhar 2011). Time fixed effects should be able to proxy shocks that are common across industries; thus, we include these effects in our model specifications. However, such exogenous shocks could also be specific to an industry in a given time period; for example, as the price of crude oil drops, the profitability of allied industries is reduced. Consequently, allied industries might cut their marketing spending, and perhaps also the CMO position, as their focus shifts from managing demand to cost cutting (e.g., Graham, Harvey, and Rajgopal 2005). Therefore, we also examine a model specification in which we include time fixed effects interacted with industry-specific fixed effects as an additional analysis. In such a specification, the effect of CMO prevalence on firm performance is identified by cross-sectional variation in CMO prevalence and firm performance within industries (SIC codes).

For our research, we use two IV approaches: First, given the panel structure of our data, we use the two-stage least-squares random-effects estimator (2SLSRE). A potentially more efficient IV estimator may be obtained by appropriately accounting for the discreteness of the CMO variable (e.g., Wooldridge 2002). Therefore, as a second IV model, we implement an estimation approach that accounts for the discreteness of the CMO variable. That is, we use a probit regression to estimate the first-stage regression model in Equation 6 and then include the inverse Mills ratio (IMR) as a control function in a second-stage random-effects regression to estimate Equation 2 (see Wooldridge 2002, procedure 18.4, p. 631).[10, 11]

Panel Internal Instruments Models

Our fourth set of models relies on the panel nature of the data to obtain IVs (we use the term "internal instruments" for instruments that arise from transformations of the included control variables in the main Equation 2 and "external instruments" for instruments excluded from the main Equation 2; see the "IV Models" section). Panel internal instruments models typically use some transformation of the endogenous variable (in our case, CMO presence) and other included exogenous variables to obtain instruments. Thus, no external instruments are required to estimate these models. We discuss and use the Hausman and Taylor (1981) and the Blundell and Bond (1998, 1999) approaches here.

[9]In several instances, we only have a few sample firms forming a peer group (i.e., primary two-digit SIC code industry). As a robustness check, we dropped peer groups with fewer than seven firms from our sample and repeated the IV models; our main finding that the estimated CMO effect is positive and significant for Tobin's q remained unchanged.

[10]We include dummy variables for the primary two-digit SIC codes (most firms belong to multiple primary two-digit SIC codes) to which each focal firm belongs as control variables in the first-stage regression (i.e., Equation 6) to account for SIC-based variance in CMO presence. These industry dummy variables are exogenous because CMO presence is not the primary variable that determines the firms' presence in an industry (two-digit SIC code). We note that the CMO effect remains positive and significant when excluding these additional SIC-based control variables from Equation 6.

[11]Following Wooldridge (2002, procedure 18.4, p. 631), we include CMO × (pdf/cdf), where pdf/cdf is the IMR and (1 – CMO) × (pdf/(1 – cdf)) in the second-stage model, where CMO = 1 if the firm has a CMO and CMO = 0 otherwise. We note that this approach requires normality assumptions for the model error terms.

Hausman and Taylor (1981; hereinafter, HT) propose an alternative to the (firm-specific) fixed-effects model in the presence of endogenous variables. In particular, HT show that the panel structure of the data can be used to obtain IVs without requiring the traditional exclusion restrictions. The HT approach is built on the same idea as the fixed-effects model: it assumes that the omitted variable(s) is (are) time invariant. Moreover, HT show that the idea can be extended to construct mean-centered IVs that are computed from the set of regressors. The HT approach exploits both the within and between dimensions of the data and allows for the inclusion of firm-specific random effects (HT; Verbeek 2012).

Building on the HT framework, Arellano and Bover (1995) and Blundell and Bond (1998, 1999; hereinafter, BB), among others, propose a more general IV framework for panel data models based on generalized method of moments (GMM) estimation. These models are particularly useful when both firm-specific intercepts and lagged dependent variables are included as control variables. In the BB model, lagged measures and/or changes in lagged measures are used as instruments for the lagged dependent variable (in our case, firm performance) and other potentially endogenous variables. Considering the two unobserved heterogeneity models discussed previously (i.e., fixed-effects and lagged dependent variable models), an alternate specification to Equations 4 and 5 would be a model that includes both a firm-specific error term and a lagged dependent variable; that is,

$$FP_{it} = \beta_0 + \beta_1 CMO_{it} + \beta_2 X_{it} + \rho FP_{it-1} + \alpha_i + u_{it}, \tag{7}$$

where α_i and u_{it} are both random terms defined as previously.

The estimation challenge of this model is that FP_{it-1} depends on α_i (i.e., the two are positively correlated) and FP_{it-1} is "endogenous" (in addition to CMO_{it}). Moreover, the endogeneity of FP_{it-1} could bias the effect of the CMO variable,[12] and even a fixed-effects estimation, which would remove α_i, would not solve the problem because the within-transformed lagged dependent variable is correlated with the within-transformed error (Nickell 1981; Verbeek 2012, p. 397). Thus, an alternative estimation approach is to take the first difference (which also removes the firm-specific error term α_i), as shown in Equation 8:

$$\begin{aligned} FP_{it} - FP_{it-1} = {} & \beta_1(CMO_{it} - CMO_{it-1}) + \beta_2(X_{it} - X_{it-1}) \\ & + \beta_3(FP_{it-1} - FP_{it-2}) + (u_{it} - u_{it-1}). \end{aligned} \tag{8}$$

This model cannot be estimated using OLS because FP_{it-1} is correlated with u_{it-1}. Instead, the endogeneity of the differenced lagged dependent variable can be addressed by using lagged values of the dependent variable as IVs resulting in a GMM estimator (e.g., Arellano and Bond 1991). However, the resulting GMM estimator has often been found to be unreliable in empirical applications because the lagged instruments tend to be weakly correlated with subsequent first differences, especially when the endogenous variables (in our case, lagged firm performance and CMO presence) are highly persistent (i.e., when there is serial correlation in these variables; BB). Thus, we must rely on GMM estimators that break the persistence in the data. Considering the various existing GMM estimators, BB's seems to be the most popular (for a critique, see Roodman 2009). In brief, the BB estimator considers Equations 7 and 8 as a system of equations. That is, it uses the lagged differences of the dependent variable as instruments for the equation in levels (i.e., Equation 7) as well as lagged levels of the dependent variable as instruments for the equation in first differences (i.e., Equation 8; see also Arellano and Bover 1995) to control for the potential endogeneity of the lagged dependent variable. The resulting system is usually estimated with a GMM approach. The validity of the (lagged) difference instruments for the levels equations depends on the assumption that changes in the dependent variable are uncorrelated with α_i, which is the case when the series is in a more or less steady state (e.g., Blundell and Bond 1999, pp. 7–8; Verbeek 2012, p. 403).

Overall Proposed Modeling Approach

The model specifications discussed previously rely on varying identifying assumptions and data requirements, as summarized in Table 2. Recognizing that there are many potential models available, we suggest that researchers explore the meaning of the various models' identifying assumptions in light of their context and then determine the appropriate specifications (as opposed to mechanically estimating many potential models and then only reporting the model[s] that provide[s] the best results, thus becoming, in the words of Angrist and Pischke [2009, p. 28], "regression mechanics"). For example, if researchers believe that they have an endogeneity problem of the first order and they can provide strong theoretical arguments for a good IV, the IV approach should be preferred. Similarly, some assumptions may be too strong for the specific situation a researcher faces (e.g., in our case, considering that we have a 12-year panel of annual firm data, it might be difficult to defend the fixed-effects assumption—i.e., that the cause for endogeneity of the CMO effect is time invariant), and thus such models may be ruled out from a conceptual standpoint.

We also want to recognize that it is important to assess and discuss the robustness of the results to various identifying assumptions (e.g., using both internal and external instruments) and investigate the same phenomenon from different vantage points. Indeed, it is conceivable that different models do not produce the same results. If that is the case, a careful conceptual justification is required for the identifying assumptions of the effect based on institutional knowledge. Thus, researchers should view themselves as regression engineers as opposed to "regression mechanics" (Angrist and Pischke 2009, p. 28) and understand the meaning of the model identifying assumptions for their context. Upon developing such an understanding, regression engi-

[12]While the direction and magnitude of the potential bias in the CMO effect are difficult to predict beforehand, some evidence exists in the literature that suggests that the estimated CMO effect may be biased downward (i.e., we would underestimate the CMO effect; see Keele and Kelly 2006).

neers should estimate and discuss a subset of models whose identifying assumptions make the most sense given the data and problem context (e.g., the models we summarize in Table 2). If the findings are convergent and show robustness, they would strengthen belief in the conclusions drawn. However, if the findings are not convergent, careful analysis of the identifying assumptions is needed to assess which results, if any, to believe or whether a different approach (e.g., a longer time series or additional control variables) is necessary.

Samples and Measures

In a first step, we aimed to replicate N&M's sample as closely as possible. Thus, using the Compustat database and considering the five-year period 2000–2004, we identified firms with sales of at least $250 million in 2002. From this set, and in line with N&M, we retained only firms without missing data on the various factors (e.g., advertising, R&D). Moreover, in N&M's footnote 7, they provide a breakdown of their sample by principal two-digit SIC code. We only retained firms with principal two-digit SIC codes represented in N&M's sample. Using N&M's (p. 70) filters, and after repeated efforts, our first sample (referred to as Sample 1 subsequently) consisted of 123 firms for which we were able to collect complete data across the five sample years. In comparison, N&M's sample comprises 167 firms.

We note that N&M excluded several firms/SIC codes for which complete data were available for 2000–2004. For example, N&M excluded firms from the Food and Kindred Products industry (e.g., General Mills Inc., Hershey Co., Kellogg Co.), the Transportation Equipment industry (e.g., Ford Motor Co., General Motors Co., Oshkosh Corp.), and Miscellaneous Manufacturing Industries (e.g., Mattel Inc., Hasbro Inc., Callaway Golf Co.). Therefore, we created Sample 2, which included these additional firms and principal two-digit SIC codes. The total number of firms included in Sample 2 is 155. Tables WA1 and WA2 in the Web Appendix compare our samples with N&M's in terms of principal SIC codes.

Moreover, since N&M's study was conducted, additional firm years have become available beyond 2004. At the time our research was initiated, the most recent year with full secondary data available was 2011. We thus created Samples 3 and 4 by adding as many firm years as possible to Samples 1 and 2. We note that the overall number of firms is the same in Samples 1 and 3 (i.e., 123 firms) as well as Samples 2 and 4 (i.e., 155 firms).[13]

CMO Presence

Considering Sample 4, our most complete sample, the percentage of firms with a CMO did not change significantly between 2000 and 2011: specifically, 35% of the sample firms had a CMO in 2000, whereas 37% had one in 2011 (the percentage of firms with a CMO ranged from 32% to 40% throughout our 12 sample years). These percentages are comparable to those reported by N&M: their CMO percentages ranged between 39% and 44%. We note, however, that although overall CMO prevalence across all firms remained fairly stable during the 12 sample years, it varied significantly within our respective sample firms. For example, in Sample 4, the largest sample, 30% of the firms had no CMO during the entire 12 years, 7% of the firms had a CMO during the entire 12 years, and 63% of the firms had a CMO during some years but not during others. In addition, considering Sample 1, our smallest and the sample closest to N&M's sample, 41% of the firms had no CMO during the entire five years, 16% of the firms had a CMO during the entire five years, and 42% of the firms had a CMO during some years and not during others. These numbers compare reasonably well with N&M's, in which approximately 66% of the firms showed no variance in CMO presence. We also conducted a simulation study, described in detail in the Appendix, which suggests that our data exhibit sufficient within-firm CMO variation over time to estimate our models.

Measures

We closely followed N&M's data collection procedures, collecting all variables, including the control variables. To do so, we used various secondary data sources, including COMPUSTAT, company websites, firms' 10-Ks, and proxy statements retrieved via EDGAR, Bloomberg, and Capital IQ. We included the following control variables in all our models: innovation, differentiation, corporate branding, total diversification, CEO tenure, outsider CEO, market concentration, number of employees (log), chief operating officer (COO) presence, and return on assets. We also included sales growth as an additional control variable in all models except those in which sales growth is the performance measure. For detailed information on the data collection procedures and variables, see N&M (pp. 71–72).[14]

Performance Measures

The CMO is posited to have both short- and long-term performance implications (e.g., Boyd, Chandy, and Cunha 2010). Thus, to assess whether and how CMO presence affects firm performance, we require a performance measure that is forward looking and cumulative. Moreover, given the cross section of firms (i.e., organizational heterogeneity) included in our sample, the measure also must be generalizable and comparable across (1) firms in many different industries and (2) firms that pursue different performance outcome goals.

Historically, most of the academic (marketing) research investigating firm performance has employed accounting-

[13]We list the names of the firms included in the four samples in Tables WA3 and WA4 in the Web Appendix.

[14]We excluded the two variables TMT marketing experience and TMT general management experience from our models for two reasons. First, as in N&M's case, biographical data were available for only approximately 75% of the executives of the sample firms. Second, virtually all TMT members had general management experience, so the variable showed no variation (i.e., it was 1 for almost all firms). We note, however, that when we estimated our models including the TMT marketing experience variable (based on data for approximately 75% of the executives), the CMO effect remained positive and significant in all but the between-effects model (using Sample 4).

based measures such as sales growth or return on investments (e.g., Buzzell and Gale 1987; Jacobson 1990). However, in the words of Anderson, Fornell, and Mazvancheryl (2004, p. 174), "such measures … contain little or no information about the future value of a firm." Indeed, accounting-based measures assume that previous investments affect only current-period earnings. Yet, in reality, most types of firm investments (e.g., employing a CMO) affect future earnings as well (e.g., Geyskens, Gielens, and Dekimpe 2002). Similarly, firms in the same industry may differ in their short-term objectives (e.g., one may emphasize profitability and the other may stress growth), and thus, the goals of the CMOs should differ across firms. Moreover, because of industry and firm differences in accounting practices, a comparison of accounting-based measures (e.g., sales growth, return on investment) across industries and firms is problematic (Anderson, Fornell, and Mazvancheryl 2004).

Capital market–based measures overcome these challenges in several ways: (1) they capture both immediate and future firm performance; (2) they are organizational goal agnostic, permitting performance comparison across firms that pursue different performance goals (e.g., growth vs. profits); and (3) they are less affected by accounting conventions because they include the potential effect of accounting practice inconsistencies across industries when evaluating expected future revenue streams (e.g., Amit and Wernerfelt 1990). Thus, capital market–based measures (rather than accounting-based measures) seem appropriate for the purpose of the study.

Consistent with N&M, we employ Tobin's q as our focal performance outcome measure. Tobin's q, the ratio of a firm's market value to the current replacement cost of its assets (Tobin 1969), is a forward-looking, capital market–based measure of the value of a firm. It provides a measure of the premium (or discount) that the market is willing to pay above (below) the replacement costs of a firm's assets, thus capturing any above-normal returns expected from a firm's collection of assets (Amit and Wernerfelt 1990). Another appealing feature of Tobin's q is that it adjusts for expected market risk; in other words, because Tobin's q combines capital market data with accounting data, it implicitly uses the correct risk-adjusted discount rate and thus minimizes distortion (e.g., Amit and Wernerfelt 1990; Montgomery and Wernerfelt 1988).

In addition to Tobin's q, we also consider the CMO's impact on three other capital market–based performance measures: Jensen's α, systematic risk, and idiosyncratic risk. Jensen's α (e.g., Aksoy et al. 2008; Jensen 1968; Lyon, Barber, and Tsai 1999) is the intercept derived from a risk model, usually the Fama–French (1993) three-factor model augmented with the momentum factor (Carhart 1997). Jensen's α should be zero unless the firm has a value driver not captured by the independent variables of the risk model (e.g., Jacobson and Mizik 2009); in other words, it captures risk-adjusted returns in excess of or below those predicted by the risk model.

Conceptual and empirical evidence suggests that marketing-related investments serve to reduce firm risk (e.g., McAlister, Srinivasan, and Kim 2007; Srivastava, Shervani, and Fahey 1998; Tuli and Bharadwaj 2009). Therefore, it may be that CMO presence has risk-reducing properties. Because both Tobin's q and Jensen's α are risk-adjusted measures (e.g., Aksoy et al. 2008; Madden, Fehle, and Fournier 2006; Montgomery and Wernerfelt 1988) and, in that sense, mask the effect of CMO presence on firm risk, we also investigate the CMO's direct impact on a firm's systematic and idiosyncratic risk. Systematic risk represents the degree to which a firm's stock returns are a function of market returns and thus are undiversifiable. In contrast, idiosyncratic risk represents risk specific to a firm that can be eliminated from an investment portfolio (e.g., Lintner 1965). Although there is an ongoing debate about the usefulness of risk measures for predicting firm value (e.g., Fama and French 1992; McAlister, Srinivasan, and Kim 2007), both idiosyncratic and systematic risk are important aspects of shareholder value (for a detailed discussion, see Tuli and Bharadwaj 2009), and we therefore include them as two additional performance measures.

Finally, we report the impact of CMO presence on sales growth to be consistent with N&M, who used both Tobin's q and sales growth as their performance measures. Following N&M, we calculate sales growth as the increase in sales as a proportion of the sales in the preceding year. However, because it is an accounting- and not a capital market–based measure, we maintain that sales growth is not a suitable measure for the context of this study. Table 3 presents the descriptive statistics.

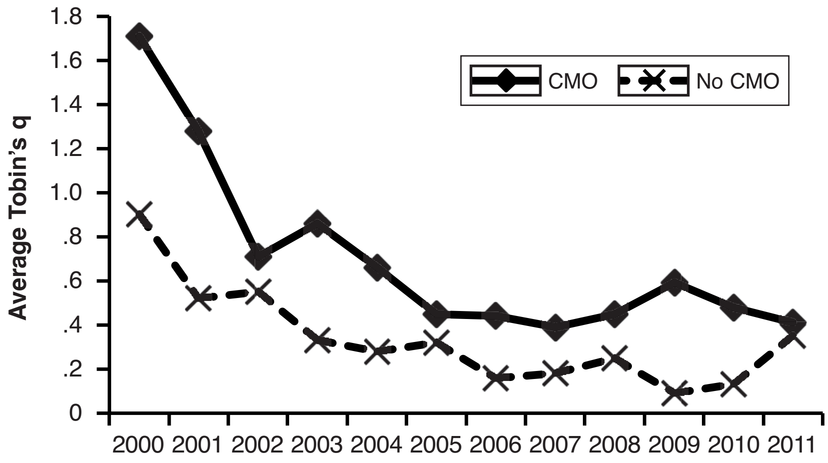
Results

The CMO and Firm Tobin's q

Model-free evidence. We first present model-free evidence regarding the relationship between CMO presence and Tobin's q. Considering Sample 4, our largest sample, and the average Tobin's q across all time periods (i.e., 2000–2011), firms with a CMO in a given year, on average, displayed a Tobin's q of .75, whereas firms without a CMO displayed a Tobin's q of .36. This difference is statistically significant ($t = 5.37$), suggesting that firms do benefit from having a CMO among the TMT (we note that we could have also estimated this effect using the model shown in Equation 1). We also plotted the average Tobin's q values by year and CMO presence to capture the CMO effect in each time period. Figure 1 and Table 4 show the average Tobin's q values of firms with and without a CMO across the 12 years. Although the difference in Tobin's q is not statistically significant in each time period (except in 2000, 2001, 2003, 2009, and 2010), Figure 1 again suggests that firms indeed benefit from having a CMO at the strategy table: the line depicting Tobin's q of CMO firms is always above the line depicting Tobin's q of non-CMO firms. Notably, the average Tobin's q of both types of firms (i.e., CMO and non-CMO firms) was significantly greater in 2000 than in subsequent years, likely the result of the Internet bubble.

Model-based evidence. We begin our model-based analysis by estimating a between-effects model using Sample 1 (Study 1a in Table 5) to replicate N&M's work as closely as

FIGURE 1
Average Tobin's q of Firms With and Without a CMO



Notes: Following N&M, the depicted Tobin's q averages are derived from the respective firms' raw Tobin's q values less the median industry values of each firm's principal two-digit SIC level.

TABLE 4
Average Tobin's q of Firms With and Without a CMO

Year	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Tobin's q (CMO)	1.71	1.28	.71	.86	.66	.45	.44	.39	.45	.59	.48	.41
Tobin's q (no CMO)	.9	.52	.55	.33	.28	.32	.16	.18	.25	.09	.13	.35
t-value	1.96	2.94	.87	2.14	1.61	.56	1.46	1.03	1.43	3.13	1.92	.30

possible. Like N&M, we find that the between-effects model indicates a nonsignificant CMO effect. Using the same sample (i.e., Sample 1), we then estimated the remaining models (Study 1b in Table 5). The CMO effect remains statistically nonsignificant in the OLS, random-effects, fixed-effects, OLS with lagged dependent variable and HT models. However, we obtain a significant and positive CMO effect in the 2SLSRE, random-effects with control function, and BB models.[15]

We then repeated our analyses using Sample 2 (Study 2 in Table 5). The CMO effect remains statistically nonsignificant when using the between-effects model; however, it becomes significant in all other model specifications. Subsequently, we estimated our models using Sample 3 (Study 3 in Table 5), and we obtain similar results to those obtained using Sample 2; that is, the CMO effect is positive and significant in all models except the between-effects model. Finally, using Sample 4, our largest sample, we repeated our analyses and again obtain a positive and significant CMO effect in all models but the between-effects model (Study 4 in Table 5). Thus, the CMO effect is fairly robust in terms of sign and statistical significance across various model identifying assumptions. Moreover, as the sample size increases from Sample 1 to Sample 4, the respective standard errors of the estimated CMO effect tend to decrease, lending higher confidence in the estimated CMO effect. We report the full model results for Sample 4, including control variables, in Table 6. For the full model results for Samples 1–3, see Tables WA5–WA7 in the Web Appendix.[16]

The effect of outliers. Using various outlier diagnostics (e.g., residual vs. fitted plots), we identified four firms with large lagged Tobin's q values that are potential outliers. Specifically, considering 2000, Yahoo Inc., RF Micro Devices Inc., Tibco Software Inc., and Foundry Networks Inc. each had lagged Tobin's q values of greater than 15 (i.e., large Tobin's q values in 1999, likely a result of the Internet bubble). Moreover, the company Move Inc. seems to have had an unusually large sales growth value, also in 2000, which is likely the result of a lawsuit that year (e.g., see Move Inc.'s 2000 10-K report). The unusually large lagged Tobin's q

[15]The instrument for CMO presence (i.e., CMO prevalence among peer firms) emerged as a significant predictor of CMO presence in the first-stage regression across all samples. For example, considering Sample 1, CMO prevalence's coefficient (z = 5.90) and the F-test of excluded instruments/Cragg–Donald Wald F-statistic (F = 4.77, $p < .0001$) are both strongly significant.

[16]We note that we also estimated our IV models using various other instrument and sample configurations. Our conclusions did not change: the CMO effect remained positive and significant. For example, using Sample 4, (1) we only considered the sample firms' principal SIC code (see Tables WA1 and WA2 in the Web Appendix) to identify the firms' respective sample peer group/calculate the CMO prevalence IV and (2) to ensure that CMO prevalence is calculated using a representative sample of peer firms, we only included sample firms that belong to principal SIC groups with 20 or more firms in them. Considering Model 6 (2SLSRE), the CMO effect was .76 (z = 3.14), and the F-test of excluded instruments/Cragg–Donald Wald F-statistic was 15.44 ($p < .0001$).

TABLE 5
Results—DV: Tobin's q

Model Type	Model Number	Models[a]	CMO Effect (SE) Studies 1a and 1b: 123 Firms, 5 Years, n = 615	Study 2: 155 Firms, 5 Years, n = 775	Study 3: 123 Firms, up to 12 Years, n = 1258	Study 4: 155 Firms, up to 12 Years, n = 1604
Rich data models	1	OLS[b]	.15 (.13)	.27** (.11)	.21*** (.07)	.26*** (.06)
	2	Between-effects[c]	.02 (.25)	.26 (.22)	.15 (.21)	.24 (.19)
	3	Random-effects	.21 (.13)	.30*** (.11)	.22*** (.08)	.24*** (.06)
Unobserved heterogeneity models	4	Fixed-effects	.17 (.15)	.23* (.13)	.18** (.08)	.18*** (.07)
	5	OLS with lagged DV	.08 (.12)	.18* (.10)	.14** (.07)	.17*** (.06)
IV models	6	2SLSRE	.79** (.32)	.81*** (.28)	.75*** (.21)	.77*** (.18)
	7	Random-effects with control function[d]	.83** (.38)	1.02*** (.35)	.57** (.25)	.55*** (.21)
Panel instruments models	8	HT	.21 (.14)	.27** (.12)	.21*** (.08)	.21*** (.07)
	9	BB[e]	.96*** (.30)	.60* (.33)	.48*** (.15)	.46** (.18)

*$p < .10$.
**$p < .05$.
***$p < .01$.
[a]Following N&M, we included the following control variables in the models: innovation, differentiation, corporate branding, total diversification, CEO tenure, outsider CEO, market concentration, number of employees (log), COO presence, return on assets, and sales growth. We also included year fixed effects in all models except the between-effects model.
[b]We also estimated this model including industry-specific year fixed effects to ensure that industry-specific exogenous shocks do not systematically influence firm performance and CMO prevalence over time. The CMO effect remained positive and significant (β_{CMO} = .22, t = 3.03; based on n = 1,604). Moreover, we also estimated the model (n = 1,604) using robust standard errors; the standard error changed from .062 to .065, and the CMO effect continued to be significant (t = 3.95 instead of t = 4.16).
[c]Model used by N&M. We note that N&M also included the lagged DV as a control variable in their model. We excluded the lagged DV here but note that the CMO effect remains nonsignificant when including the lagged DV in the model.
[d]We used the control function approach (e.g., Wooldridge 2002, procedure 18.4, p. 631): using a probit regression (DV = CMO presence; CMO prevalence in industry included as IV), we first calculated the IMR and then added the IMR × CMO variables as well as the (1 – CMO)[(pdf/(1 – cdf)] variable to Model 3 (i.e., the random-effects model).
[e]We included the second and third lag of the CMO and Tobin's q (t – 1) variables as instruments and note that the results are robust to other lag structures. In addition, the AR(2) test for autocorrelation of the residuals suggests that the differenced residuals do not exhibit significant AR(2) behavior.
Notes: DV = dependent variable.

observations from 1999 for the four aforementioned firms may have an impact on the CMO (point) estimate of Models 5 (i.e., OLS with lagged dependent variable) and 9 (i.e., BB) because we included the lagged Tobin's q variable in these two models. Moreover, the large sales growth value for the firm Move Inc. may affect all model results.

We examined robustness to these potential outliers in two ways. First, we dropped the five firms with potential outliers from our sample (all years) and then reestimated Models 5 and 9 using Sample 4. The estimated CMO effect remained positive and significant in both models (OLS with lagged dependent variable: β_{CMO} = .13, t = 2.79; BB: β_{CMO} = .38, z = 2.08), demonstrating that our estimation results in Table 5 for Models 5 and 9 are robust to these potential outliers. Second, again using Sample 4, we reestimated the remaining models after removing Move Inc. (all years) from our initial sample. Again, the CMO effect remained positive and significant in all but the between-effects model (OLS: β_{CMO} = .27, t = 4.44; between-effects: β_{CMO} = .25, t = 1.35; random-effects: β_{CMO} = .26, z = 4.02; fixed-effects: β_{CMO} = .19, t = 2.88; 2SLSRE: β_{CMO} = .78, z = 4.30; random-effects with control function: β_{CMO} = .58, z = 2.79; HT: β_{CMO} = .23, z = 3.49).

The CMO and Other Measures of Market-Based Performance

Jensen's α. To assess the CMO's impact on Jensen's α, we used the calendar–time portfolio approach and constructed a portfolio that buys (sells) stocks of our sample firms (i.e., Sample 4) with (without) a CMO (e.g., Aksoy et al. 2008; Jacobson and Mizik 2009; Madden, Fehle, and Fournier 2006). Considering the 12 years of observed data, we rebalanced this portfolio every month such that firms that had a CMO in month t but not in month t + 1 exited the portfolio

TABLE 6
Full Model Results for Sample 4—DV: Tobin's q

	Coeffiecient (SE)								
Independent Variables	**Model 1: OLS**	**Model 2: Between Effects**	**Model 3: Random Effects**	**Model 4: Fixed Effects**	**Model 5: OLS with Lagged DV**	**Model 6: 2SLSRE**	**Model 7: Random Effects with Control Function**	**Model 8: HT**	**Model 9: BB**
Constant	–.275**	–.128	.072	.745***	–.242*	–.121	–.119	.331	–.412*
	(.148)	(.214)	(.172)	(.201)	(.138)	(.189)	(.203)	(.218)	(.213)
Innovation	4.376***	6.426***	1.234***	–.710	3.677***	1.046**	.921*	–.021	.660
	(.341)	(.937)	(.446)	(.534)	(.321)	(.460)	(.515)	(.484)	(.651)
Differentiation	5.888***	6.498***	5.295***	5.742***	4.608***	5.291***	5.133***	5.344***	.975
	(.814)	(1.870)	(1.306)	(1.866)	(.762)	(1.343)	(1.351)	(1.568)	(1.558)
Corporate branding	.002	.053	.103	Omitted	–.022	.068	.093	.115	–.132
	(.063)	(.132)	(.129)		(.058)	(.134)	(.134)	(.218)	(.097)
Diversification	–.285***	–.150	–.172**	.010	–.221***	–.169**	–.228***	–.083	–.058
	(.063)	(.156)	(.078)	(.088)	(.059)	(.080)	(.084)	(.082)	(.098)
Outsider CEO	–.091	–.088	–.039	–.031	–.076	–.031	–.012	–.023	–.078
	(.067)	(.166)	(.081)	(.090)	(.062)	(.083)	(.086)	(.085)	(.082)
Market concentration	–.000	–.000	–.000	.000	–.000	–.000	–.000	.000	.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Firm size	.057***	.093**	–.087**	–.546***	.056***	–.089**	–.081**	–.260***	.033
	(.021)	(.047)	(.038)	(.072)	(.019)	(.039)	(.039)	(.051)	(.033)
CEO tenure	.002***	.001	.002***	.002***	.002***	.002***	.001***	.002***	.001**
	(.000)	(.001)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.001)
COO presence	.060	.034	.167***	.171***	.065	.166**	.182***	.174***	–.019
	(.066)	(.220)	(.063)	(.064)	(.062)	(.064)	(.067)	(.062)	(.061)
Prior performance					.142***				.520***
					(.009)				(.068)
Profitability (return on assets)	2.758***	4.692***	1.761***	1.320***	2.368***	1.721***	1.827***	1.512***	.665
	(.216)	(.795)	(.201)	(.203)	(.203)	(.206)	(.210)	(.196)	(.506)
Sales growth	1.569***	3.120***	1.214***	1.144***	.875***	1.167***	1.188***	1.152***	–.589
	(.137)	(.682)	(.117)	(.115)	(.135)	(.120)	(.123)	(.112)	(.445)
CMO presence	.259***	.241	.243***	.177***	.173***	.771***	.551***	.212***	.462**
	(.062)	(.187)	(.064)	(.066)	(.058)	(.183)	(.212)	(.065)	(.182)
Year fixed effects	Included	Not Included	Included	Included	Included	Included	Included	Included	Included

*$p < .10$.
**$p < .05$.
***$p < .01$.
Notes: DV = dependent variable.

at the beginning of month t + 1 and firms that did not have a CMO in month t but then hired a CMO in month t + 1 entered the portfolio at the beginning of month t + 1. We rebalanced monthly (as opposed to, e.g., yearly) because of differences in fiscal year ends of our sample firms (e.g., some firms' fiscal year ended in December, others in March). As before, we used the firms' 10-Ks and proxy statements to identify CMO presence. Using value-weighted and continuous monthly returns (e.g., Jacobson and Mizik 2009), we estimated the following Fama–French (1993) three-factor model augmented with the momentum factor (Carhart 1997) using stock-market data obtained from the Center for Research in Security Prices and Kenneth French's data library:

$$(9) \quad (R_{pt} - R_{ft}) = \propto_p + \beta_p(R_{mt} - R_{ft}) + \gamma_p(SMB)_t + \delta_p(HML)_t + \theta_p(MOM)_t + \epsilon_{pt},$$

where R_{pt} is the rate of return of our portfolio of CMO firms in month t, R_{ft} is the monthly risk-free return in month t, R_{mt} is the monthly return on a value-weighted market portfolio in month t, SMB_t is the Fama–French size portfolio in month t, HML_t is the Fama–French market-to-book ratio portfolio in month t, and MOM_t is the momentum factor in month t.

The intercept $\propto_p$ (i.e., Jensen's α) of the model provides an estimate of excess returns. Under the null hypothesis of no excess returns, $\propto_p$ will not be significantly different from zero (e.g., Jacobson and Mizik 2009; Jensen 1968). Consistent with our previous findings, the intercept of the model is positive and significant (α = .004, t = 2.61), providing strong corroborating evidence that firms benefit from having a CMO at the strategy table.

Systematic and idiosyncratic risk. We obtained the systematic and idiosyncratic risk measures by estimating the Fama–French model (see Equation 9) separately for each firm–year combination using daily stock-market data, which we again obtained from the Center for Research in Security Prices and Kenneth French's data library (e.g., Tuli and Bharadwaj 2009). We then used the two risk measures as the respective dependent variables in our nine models and report the results in Table 7. As with the previous analyses, we used Sample 4 for this analysis.

As the results indicate, CMO presence does not seem to have an impact on systematic risk of the firm across all

TABLE 7
Results—Other DVs; Based on Sample 4

Model Type	Model Number	Models[a]	CMO Effect (SE) DV: Tobin's q	DV: Systematic Risk	DV: Idiosyncratic Risk	DV: Sales Growth
Rich data models	1	OLS	.26** (.06)	.01 (.02)	.001 (.001)	–.00 (.01)
	2	Between-effects[b]	.24 (.19)	.00 (.06)	.0002 (.0002)	–.01 (.02)
	3	Random-effects	.24** (.06)	.02 (.02)	.001 (.001)	.00 (.01)
Unobserved heterogeneity models	4	Fixed-effects	.18** (.07)	.02 (.02)	.0004 (.001)	.02 (.01)
	5	OLS with lagged DV	.17** (.06)	.01 (.02)	.0003 (.0004)	–.01 (.01)
IV models	6	2SLSRE	.77** (.18)	.01 (.06)	.003* (.0016)	–.01 (.03)
	7	Random-effects with control function[c]	.55** (.21)	–.03 (.07)	.002 (.04)	.002 (.04)
Panel instruments models	8	HT	.21** (.07)	.02 (.04)	.001 (.001)	.00 (.01)
	9	BB[d]	.46* (.18)	.06 (.06)	.0003 (.001)	–.03 (.03)

*$p < .05$.
**$p < .01$.
[a]Following N&M, we included the following control variables in the models: innovation, differentiation, corporate branding, total diversification, CEO tenure, outsider CEO, market concentration, number of employees (log), COO presence, return on assets, and sales growth (except for the model in which DV is sales growth), and Tobin's q (except for the model in which DV is Tobin's q). We also included year fixed effects in all models except the between-effects model.
[b]Model used by N&M. We note that N&M also included the lagged DV as a control variable in their model. We excluded the lagged DV here but note that the CMO effect remains nonsignificant when including the lagged DV in the model.
[c]We used the control function approach (e.g., Wooldridge 2002, procedure 18.4, p. 631): using a probit regression (DV = CMO presence; CMO prevalence in industry included as IV), we first calculated the IMR and then added the IMR × CMO variables as well as the (1 – CMO)[(pdf/(1 – cdf)] variable to Model 3 (i.e., the random-effects model).
[d]We included the second and third lag of the CMO and financial performance (t – 1) variables as instruments and note that the results are robust to other lag structures. In addition, the AR(2) test for autocorrelation of the residuals suggests that the differenced residuals do not exhibit significant AR(2) behavior.
Notes: DV = dependent variable.

models. In contrast, the result from the 2SLSRE model (Model 6) suggests that CMO presence has a positive impact on idiosyncratic risk of the firm. Notably, however, none of the other models reveals a significant positive or negative CMO effect on idiosyncratic risk. Thus, we are faced with a situation in which we do not obtain convergent findings across models that are based on various identifying assumptions, as discussed previously. Instead of disregarding the nonsignificant (or significant) results, we must now carefully assess the identifying assumptions of the various models used and decide which result(s), if any, to believe. Previously, we argued that the CMO presence variable is likely endogenous, and we believe that we have made a strong case for our IV. Moreover, the Hausman test suggests that the 2SLSRE model is preferred over the rich data models, supporting the notion that CMO presence is endogenous. Thus, an IV model is likely to have the most reasonable identifying assumptions, and we therefore consider the finding obtained from the 2SLSRE model noteworthy. However, because the CMO effect is (positive but) not significant in the random-effects with control function model, our other IV model, we cannot conclude with high certainty that CMO presence indeed has a positive effect on idiosyncratic risk, a point we further address in the "Discussion" section.

The CMO and Firm Sales Growth

To be consistent with N&M, we also assessed the CMO's impact on sales growth. We again use Sample 4 as well as all nine models and report the results in Table 7. We find that CMO presence does not appear to have a (main) effect on a firm's sales growth, consistent with N&M's findings. We also estimated the model after removing the observations for Move Inc., because of the company's unusually large sales growth in 2000 (see previous discussion); the results did not change in any meaningful way. We also note that CMO presence does not appear to have a lagged effect (one or two years) on sales growth. As outlined previously, we maintain that sales growth is not a suitable measure to capture the performance implications of the CMO. Thus, we speculate that these nonfindings are at least partially a function of the sales growth measure (1) not being able to capture future performance implications of the CMO and (2) being an organizational goal agnostic measure.

Do Certain Firms Benefit More (or Less) from a CMO?

We next examined whether the effect of CMO presence is moderated by firm-specific characteristics—that is, whether some firms benefit more or less from having a CMO. We again used Sample 4 for this analysis. We first focus on Tobin's q as the outcome measure and subsequently examine the other outcome measures, that is, systematic and idiosyncratic risk, as well as sales growth. We present results from the 2SLSRE model here but note that the results were consistent across all models (results are available from the authors). In brief, Tobin's q seems to be improved by CMO presence in the TMT for (1) firms with relatively higher sales growth, (2) firms that are relatively smaller, and (3) firms whose CEO has relatively shorter tenure. We present the results in Table 8.

CMO × sales growth. The positive and significant interaction between CMO presence and sales growth seems consistent with extant strategy and marketing literature. For example, Day and Wensley (1988) propose that firms realize greater benefit from generating and acting on customer-oriented information in high-growth scenarios (also see Slater and Narver 1994). Considering that the CMO has long been viewed as the most direct steward of a firm's customers (e.g., Boyd, Chandy, and Cunha 2010) as well as the voice of the customer in the C-suite (e.g., McGovern et al. 2004), it can be expected that the CMO's performance impact is elevated in higher sales-growth firms.

CMO × number of employees. We find that the positive effect of CMO presence declines as the number of employees increases. Literature suggests that as the number of employees increases, the size of the TMT also increases (e.g., Haleblian and Finkelstein 1993). Moreover, the larger the TMT, the less power each individual TMT member tends to have (e.g., Finkelstein, Hambrick, and Canella 2009). Thus, as the number of employees increases, the CMO's influence on organizational strategic direction might decrease, providing support for the observed negative interaction effect.

CMO × CEO tenure. We find that the positive effect of CMO presence also decreases as the tenure of the CEO increases. This finding is consistent with extant literature that suggests that CEO power and influence increases with CEO tenure and, conversely, that other TMT members' power and influence is negatively related to CEO tenure (e.g., Hill and Phan 1991). Thus, the longer the CEO is in place, the less impact the CMO should have on the direction and strategy of the firm, in support of decreased performance implications of the CMO.

Other performance outcome measures. We next assessed moderating effects considering the other performance outcome measures, systematic and idiosyncratic risk, as well as sales growth. As Table 8 shows, a negative and marginally significant interaction between CMO presence and differentiation emerged when systematic risk was the dependent variable. Moreover, a positive and significant interaction between CMO presence and differentiation emerged in the model in which sales growth was the dependent variable. Although the interaction was not significant when considering Tobin's q as the outcome measure (see Table 8), the finding is consistent with N&M's prediction that firms pursuing relatively high levels of differentiation should benefit more from having a CMO in the C-suite than firms pursuing lower levels of differentiation (N&M did not find empirical support for this hypothesis). Quite correctly in our opinion, N&M argue that strategies of differentiation increase the need to identify market opportunities quickly. Moreover, they maintain that strategies of differentiation rely heavily on marketing capabilities and that having a CMO among the TMT should help develop and foster such capabilities.

TABLE 8
Results—Interaction Effects; Based on Sample 4 and Model 6 (2SLSRE)

	CMO Effect (SE)			
Variable[a]	DV: Tobin's q	DV: Systematic Risk	DV: Idiosyncratic Risk	DV: Sales Growth
CMO	.77*** (.16)	.04 (.05)	.003** (.001)	–.02 (.02)
CMO × Sales growth	.73*** (.24)	–.11 (.08)	.005** (.002)	Omitted
CMO × Differentiation	1.24 (2.04)	–1.12* (.67)	–.02 (.02)	.89** (.38)
CMO × Innovation	–1.18 (.80)	–.10 (.27)	–.006 (.007)	.15 (.15)
CMO × Return on assets	–.04 (.40)	.18 (.13)	.001 (.004)	.13 (.08)
CMO × Number of employees	–.16*** (.05)	–.01 (.02)	–.0005 (.0004)	–.001 (.01)
CMO × Market concentration	–.0002 (.0001)	–.0001 (.0001)	–.0000 (.0000)	–.00 (.00)
CMO × Diversification	.10 (.14)	.02 (.05)	–.0001 (.001)	–.01 (.03)
CMO × Corporate branding	–.07 (.14)	–.01 (.05)	–.0004 (.001)	–.03 (.03)
CMO × Outsider CEO	–.12 (.14)	–.02 (.05)	.002* (.001)	–.05* (.03)
CMO × CEO tenure	–.003*** (.001)	–.0004 (.0003)	–.0000 (.0000)	–.0002 (.00014)
CMO × COO	–.06 (.13)	–.06 (.04)	.001 (.001)	–.04 (.03)

*$p < .10$
**$p < .05$.
***$p < .01$.
[a]We excluded the CMO × Sales growth interaction from Model 4 (DV: Sales Growth) because sales growth is the DV of that model. We also note that we included the interactions between CMO and the respective year dummies in all the models. In addition, the variance inflation factors of the coefficients are less than 5 across all models, indicating that multicollinearity is not a concern.
Notes: DV = dependent variable.

Considering idiosyncratic risk as the performance outcome measure, we also find a positive and significant interaction between CMO presence and sales growth. Recall that we also find a positive interaction between CMO presence and sales growth when considering Tobin's q as the performance outcome measure. Thus, both Tobin's q and idiosyncratic risk seem to be increased by CMO presence in firms with relatively higher sales growth. We posit that the joint effect of CMO presence and sales growth on idiosyncratic risk can at least be partially explained by the notion that CMOs tend to be risk seeking (e.g., Lord 2014). Indeed, when a firm experiences high growth, its CMOs might be even more risk prone, in support of the observed positive interaction effect.

Double selection. We also took into consideration a potential double selection effect as we examined the interactions. That is, the selection of a CMO and the selection of a firm strategy (differentiation, innovation, diversification, and branding strategy) may be occurring at the same time, thus resulting in double selection effects. To address this potential issue, we reestimated the 2SLSRE models shown in Table 8 treating not only the CMO variable but also the four firm strategy variables as endogenous. We used peer firm variables (e.g., differentiation prevalence among peer firms) as instruments for the firm strategy variables following the same procedure and logic as for the CMO presence instrument. The results again suggest that Tobin's q seems to be improved by CMO presence in the TMT for (1) firms with relatively higher sales growth, (2) firms that are relatively smaller, and (3) firms whose CEO has relatively shorter tenure. Moreover, as before, systematic risk seems to be decreased (and sales growth increased) by CMO presence in the TMT for firms that are relatively more differentiated. Finally, as before, we find a positive and significant interaction effect between sales growth and CMO presence when idiosyncratic risk is the dependent variable.

Discussion

The CMO Is Not Dead!

Recent business press articles have suggested that CMOs are increasingly "powerless and peripheral" (e.g., Turpin

2012, titled "The CMO Is Dead"). Our findings, which seem to be quite robust, question such conclusions. In short, our findings suggest that firms benefit from having a CMO among the TMT. Considering our largest sample (Sample 4), the CMO's effect on Tobin's q ranges from .17 to approximately .77, depending on model specification. Thus, on the basis of the smallest CMO effect estimate (i.e., .17), our data and analyses suggest that Tobin's q of firms that employ a CMO is approximately 15% larger than that of firms that do not employ a CMO.[17] The CMO effect also seems to be moderated by firm-specific characteristics, including sales growth, number of employees, CEO tenure, and, to a lesser degree, differentiation strategies of the firm. Moreover, our results indicate that, in addition to Tobin's q, CMO presence has a positive impact on excess stock returns (i.e., Jensen's α). In contrast, CMO presence does not seem to have a direct impact on a firm's sales growth. We note that these findings reinforce the importance of the marketing–finance interface, in that it is only through market–based measures that the significance of the CMO is observed.

Beyond the substantive insights, we hope that our research also makes a methodological contribution to the broader marketing strategy literature. Modeling phenomena as complex as the CMO's performance implications, in our opinion, requires not only using a diverse set of models but also careful consideration of the identifying assumptions underlying these models. Moreover, if the results are sensitive to identifying assumptions (i.e., if different models give different results), researchers should discuss the identifying assumptions and speculate on why the findings might be sensitive to these assumptions.

Here, we find that the positive effect of CMO presence on Tobin's q is consistent across a diverse set of models, each with different identifying assumptions. Given these convergent findings, our confidence in the identified positive and significant CMO effect on Tobin's q is high. We also find that the positive effect of CMO presence on firm idiosyncratic risk only manifests in the 2SLSRE model. We believe that we have made a strong case for our choice of IV, namely, CMO prevalence among peer firms; in addition, the Hausman test suggests that CMO presence is endogenous. Thus, it is possible that the nonfindings for idiosyncratic risks using the other models suffer from an endogeneity bias, but the 2SLSRE model corrects for that bias. However, because the CMO effect is not significant in the random-effects with control function model, our other IV model, we cannot conclude with high certainty that CMO presence indeed has a positive effect on idiosyncratic risk. We hope that researchers will further explore the CMO's impact on firm risk. For example, a potentially fruitful study might entail reassessing the control variables we include in the risk model, followed by identifying additional control variables, and then reestimating the various models. Indeed, we used the same control variables in the risk model as we did in the Tobin's q and sales growth model, which we adopted from N&M. However, N&M developed their model with Tobin's q and sales growth as the dependent variables, and not risk. Therefore, it is possible that our risk model excludes important control variables.

Limitations

Although we believe that we have broken new ground with this work, there are clear limitations, some of which provide fruitful avenues for further research and discussion. First, simply adding a CMO to the TMT will most likely not improve the performance of a firm; rather, the positive effect that we observe in our study likely derives from the role of marketing in our sample firms. Webster, Malter, and Ganesan (2003), for example, suggest that CMO presence is a credible signal that the firm is likely to appreciate the marketing concept, and N&M propose that CMO presence is an indicator of marketing's influence in the firm. Thus, if firms employ a CMO, marketing is likely to play a more prominent role in the firm, and that could be the source of the performance effects we find. We note that the role of marketing in the firm should, of course, be heavily influenced by the CMO. Further research could examine whether this prediction holds and, for example, attempt to measure a firm's market orientation (by means of, e.g., content analyzing annual reports [e.g., Noble, Sinha, and Kumar 2002]) and then analyze whether market orientation mediates the CMO's positive impact on firm performance.

Second, as we mentioned previously, our insights regarding the CMO's effect on idiosyncratic risk are limited given the lack of convergent findings. We hope that future studies will further explore the CMO's impact on firm risk. Third, our results are based on larger firms (i.e., firms with more than $250 million in sales) that invest in both R&D and advertising. We do not find a significant correlation between firm size and CMO presence. Thus, we believe that our results should hold even for smaller firms, although extrapolation outside the data range on which the models were estimated should be done with caution. However, our results should not be generalized to firms that do not invest in advertising and R&D.

Conclusion

Many CMOs struggle to prove their worth to other members of their top management team. Perhaps accordingly, the average tenure of a CMO is estimated to be two years, compared with five years for CEOs (e.g., N&M). Against this backdrop, there have been strong calls among academics and practitioners alike to shed more light on the performance implications of CMO presence (e.g., Boyd, Chandy, and Cunha 2010). The key finding of our study is that firms seem to benefit from having a CMO among the TMT; we hope that this seemingly robust finding will help cement the presence of a CMO at the strategy table.

[17]To arrive at these performance gain estimates, we computed two predicted values of Tobin's q, one for firms with a CMO and one for those without a CMO, using the coefficients from Model 5 (e.g., β_{CMO} = .17) and setting the other independent variables at their sample mean. We then calculated the percentage difference between the two predicted values.

Appendix: Simulation Study: Within-Firm CMO Variability

We conducted a simple simulation study to investigate the impact of various degrees of within-firm CMO variability on four of the estimators that we use—namely, the OLS, between-effects, fixed-effects, and random-effects estimators. We consider the following data-generating model in our simulation:

$$y_{it} = \beta_{0i} + \beta_1 CMO_{it} + \beta_2 X_{it} + \epsilon_{it}, \quad \text{(A1)}$$

where y_{it} is the dependent variable (e.g., Tobin's q), CMO_{it} is 1 if firm i has a CMO in time period t and 0 otherwise, and X_{it} is an exogenous regressor that varies across firms and over time. We do not consider endogeneity here and treat both CMO_{it} and X_{it} as exogenous. The parameter β_{0i} is a random intercept that varies across i. We set the true parameters as follows: $\beta_1 = 1$, $\beta_2 = -1$, and the regressor X_{it} is independent and normally distributed across i and t with mean 0 and variance 1 and has a small correlation (multicollinearity) with CMO_{it} of .2. The error terms β_{0i} and ϵ_{it} are i.i.d. from a normal distribution, with $E(\beta_{0i}) = 10$ and $E(\epsilon_{it}) = 0$, and variances σ^2_β and σ^2_ϵ, respectively. The error terms represent the unobserved between-firm variation (β_{0i}) and unobserved within-firm variation (ϵ_{it}). We change the ratio of the two variances of the error terms to represent situations in which there is relatively more within ($\sigma^2_\epsilon > \sigma^2_\beta$) versus between ($\sigma^2_\beta > \sigma^2_\epsilon$) unobserved variation. We use the same sample size as in our empirical application (i.e., Sample 4: 155 firms and 12 time periods).

We generate the CMO variable as follows: With probability p, the firm has a CMO for all time periods, and with probability q, the firm switches its CMO status one time during the 12-year observation period (we note that several of our sample firms switch CMO status more than once during the 12 years; however, to be conservative, we assume that firms switch CMO status at most one time in this simulation study). We randomly decide (with equal probabilities) in which time period the firm changes its CMO status. For example, if a firm changes its CMO status during the fifth year, we could observe the following scenario: 000010000000 (i.e., a CMO is present in the fifth year but not in years 1–4 and years 6–12). We set p equal to the observed CMO prevalence in our empirical application (~.4) and vary q in 20 steps from .05 to 1 (i.e., in increments of .05). We did not include q = 0 because the fixed-effects estimator for CMO would not be identified in that case.

We consider the following three scenarios regarding between versus within unobserved variation:

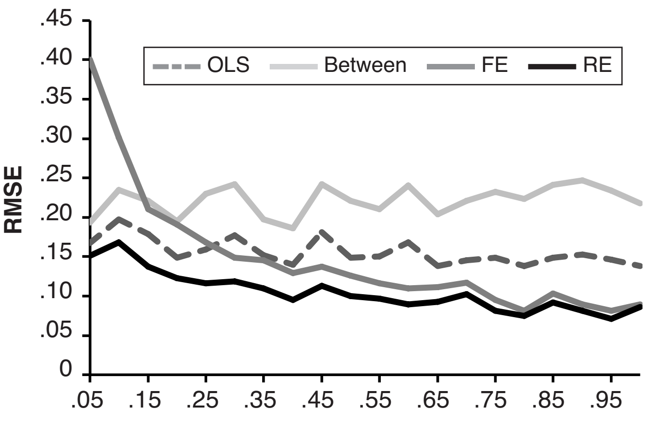
1. $\sigma^2_\epsilon = 1$ and $\sigma^2_\beta = 1$ (relatively equal within and between unobserved variation),
2. $\sigma^2_\epsilon = 1.5$ and $\sigma^2_\beta = .5$ (relatively more within unobserved variation), and
3. $\sigma^2_\epsilon = .5$ and $\sigma^2_\beta = 1.5$ (relatively more between unobserved variation).

Thus, we have a total of 20 × 3 = 60 experimental conditions. For each experimental condition, we generate 100 data sets using Model A1 and estimate β_1 with OLS, between-effects, fixed-effects, and random-effects. We analyzed the root mean square error (RMSE) and the bias of the estimated CMO effect for each estimator.

Figures A1 and A2 summarize our findings for Scenario 1 (we only report detailed results based on Scenario 1 here but note that the pattern of results based on Scenarios 2 and 3 were similar; detailed results are available from the authors).[18] As Figure A1 shows, the RMSE of the fixed-

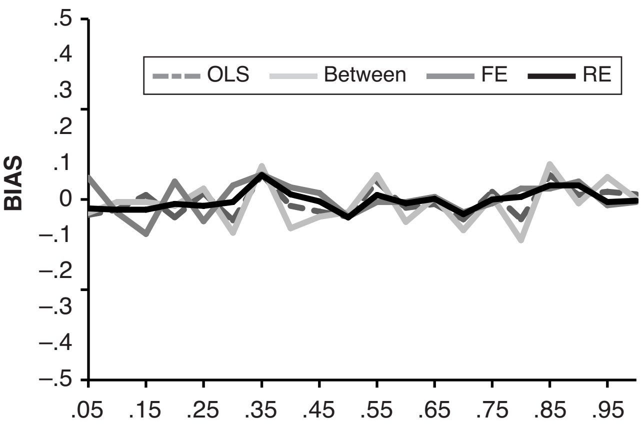
[18]For each experimental condition, we generate L = 100 simulated data sets. For each data set $\ell = 1, \ldots, L$, we compute K estimators for β_1 using OLS, between-effects, fixed-effects, and random-effects. Then, the RMSE for the kth estimator is $RMSE_k = (1/L)\Sigma^L_{\ell=1}(\hat{\beta}^k_{1\ell} - \beta_1)^2$, where $\hat{\beta}^k_{1\ell}$ is the estimate of the CMO effect of the kth estimator (e.g., the FE estimator) in the ℓth simulated data, and β_1 is the true value of the CMO effect used in the simulation study to generate the data. Similarly, $BIAS_k = [(1/L)\Sigma^L_{\ell=1}\hat{\beta}^k_{1\ell}] - \beta_1$. A good estimator has a bias of 0 and a low RMSE; the best estimator here has the lowest RMSE.

FIGURE A1
RMSE Based on Scenario 1: $\sigma^2_\epsilon = 1$ and $\sigma^2_\beta = 1$



Notes: Between = between-effects model; FE = fixed-effects model; RE = random-effects model.

FIGURE A2
BIAS Based on Scenario 1: $\sigma^2_\epsilon = 1$ and $\sigma^2_\beta = 1$



Notes: Between = between-effects model; FE = fixed-effects model; RE = random-effects model.

effects estimator is largest for small values of q; that is, when most firms (up to approximately q = 15%) do not change CMO status across the 12 time periods. However, when approximately 20% of the firms change CMO status once during the 12 time periods, the between-effects estimator has the largest RMSE. For Scenarios 2 and 3 (results not shown), we find similar patterns: the fixed-effects estimator has the largest RMSE for values of q up to 45% and 10%, respectively. Notably, the random-effects estimator always has the lowest RMSE, and OLS always has a lower RMSE than the between-effects estimator. Moreover, Figure A2 shows that, across all values of q, the CMO effect is estimated in an approximately unbiased way by all four estimators. That is, even when only 5% of the firms switch CMO status once, we still obtain an approximately unbiased estimate of the CMO effect (Scenarios 2 and 3 yielded almost identical results).

In our empirical application (considering Sample 4), 37% of the firms do not switch CMO status and 63% of the firms switch CMO status once or more. In our simulation study, the fixed-effects, random-effects, and (to a lesser degree) OLS estimators always behave well for levels of q > 60%. The between-effects estimator, however, has a fairly high RMSE for all levels of q. Whether fixed-effects, random-effects, and OLS estimators are more or less efficient than between-effects estimators in the end is an empirical question. However, our simulation results indicate that fixed-effects, random-effects, and OLS estimators indeed seem to be more efficient than the between-effects estimator given the context of our CMO study.

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