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Marketing academics and practitioners frequently employ cross-sectional surveys. In recent years, editors, reviewers, and authors have expressed increasing concern about the validity of this approach. These validity concerns center on reducing common method variance bias and enhancing causal inferences. Longitudinal data collection is commonly offered as a solution to these problems. In this article, the authors conceptually examine the role of longitudinal surveys in addressing these validity concerns. Then, they provide an illustrative comparison of the validity of cross-sectional versus longitudinal surveys using two data sets and a Monte Carlo simulation. The conceptualization and findings suggest that under certain conditions, the results from cross-sectional data exhibit validity comparable to the results obtained from longitudinal data. This article concludes by offering a set of guidelines to assist researchers in deciding whether to employ a longitudinal survey approach.

Keywords: survey methods, causality, cross-sectional surveys, longitudinal surveys, common method variance

Cross-Sectional Versus Longitudinal Survey Research: Concepts, Findings, and Guidelines

Marketing academics and practitioners ask questions to understand, explain, and predict marketplace behaviors. Although these questions take many forms, they often appear as items in surveys of managers or consumers. Of the 636 empirical articles published in *Journal of Marketing* and *Journal of Marketing Research* between 1996 and 2005, 178 (approximately 30%) used survey methods. Given this prevalence, scholars have devoted considerable attention to enhancing the validity of survey research, including item construction (Churchill 1979), reliability assessment (Peter 1979), response bias (Baumgartner and Steenkamp 2001), nonresponse bias (Armstrong and Overton 1977), informant qualification (John and Reve 1982), and construct validation (Gerbing and Anderson 1988).

In recent years, editors, reviewers, and authors of leading marketing journals have become increasingly concerned about the validity of survey research. Two issues dominate these concerns: (1) common method variance (CMV) (i.e., systematic method error due to the use of a single rater or single source) and (2) causal inference (CI) (i.e., the ability to infer causation from observed empirical relations). For example, Kamakura (2001, p. 1) cautions that "authors must be mindful of typical problems in survey research, such as halo effects, order effects, common-methods biases, and so forth." Likewise, Wittink (2004, p. 3) alerts survey researchers to "explicitly address the possibility of alternative explanations for their results" as a means of gaining "support for causal propositions that cannot be tested." These two issues are intricately related because CMV bias

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ISSN: 0022-2437 (print), 1547-7193 (electronic)

severely limits researchers' ability to draw CI and creates potential rival explanations (Lindell and Brandt 2000; Podsakoff et al. 2003). Combined, these issues present a serious threat to the validity of survey-based marketing studies. Thus, these concerns appear to be well placed.

Although the subject of these concerns is survey research in general, these issues are especially critical for cross-sectional research (i.e., surveys completed by a single respondent at a single point in time), which is widely viewed as being prone to CMV bias and incapable of causal insights. This rising concern about the validity of cross-sectional surveys is an important issue because this method represents the most common form of empirical research in many areas, including marketing channels, sales force management, and marketing strategy, and thus provides a critical foundation for much of the knowledge on these topics (Jap and Anderson 2004). Of the 178 survey-based *Journal of Marketing* and *Journal of Marketing Research* articles we noted previously, 94% are cross-sectional in nature.

To reduce the threat of CMV bias and enhance CI, survey researchers typically recommend three data collection strategies: (1) employing multiple respondents, (2) obtaining multiple types of data, or (3) gathering data over multiple periods (Jap and Anderson 2004; Ostroff, Kinicki, and Clark 2002; Podsakoff and Organ 1986; Podsakoff et al. 2003; Van Bruggen, Lilien, and Kacker 2002). All three strategies are capable of creating separation between the collection of independent and dependent variables, which in theory should reduce the hazard of CMV and increase CI as a result (Podsakoff et al. 2003). Unfortunately, this view is seldom tested because many survey articles fail to employ these data collection strategies.[1] Moreover, most CMV and CI research emphasizes analytical (rather than data-based) solutions to these validity threats, and the bulk of this literature has been published outside the marketing discipline. Consequently, the marketing literature provides little guidance with regard to the effectiveness of these data collection strategies in terms of reducing CMV or enhancing CI.

Our objective is to address this gap in the marketing literature by providing a conceptual and empirical assessment of the efficacy of collecting data over multiple periods (i.e., longitudinal data). We focus on this strategy because we believe that it is more generally applicable than obtaining multiple forms of data or employing multiple respondents. First, gathering multiple forms of data (e.g., a survey for predictors and a secondary database for outcomes) may be feasible for studies that employ constructs that have an objective referent (e.g., financial performance, customer retention) and units of analysis typically found in secondary databases (e.g., firm level). However, many constructs of interest to marketing scholars are more subjective in nature (e.g., opportunism, relationship quality, trust, nonfinancial performance) or examine units of analysis (e.g., subunit, project level) that are difficult to obtain from a source other than a survey. Second, although employing multiple informants by gathering predictors from one respondent and outcomes from another respondent may be appropriate when surveying large firms, this approach is difficult when surveying small firms or consumers (e.g., Brown et al. 2002; Erdem, Swait, and Valenzuela 2006; Voss, Montoya-Weiss, and Voss 2006). In contrast, longitudinal data can be obtained for any measure or subject employed in a cross-sectional survey.

Thus, collecting longitudinal data as a means of reducing CMV and enhancing CI would appear to be a worthy endeavor. However, longitudinal surveys demand additional expenditures in terms of time and money. These expenses are often prohibitive for academic researchers faced with limited budgets and marketing practitioners faced with limited time. Consequently, longitudinal survey research is easier to advocate than to implement. Moreover, longitudinal studies raise several potential problems, such as confounds due to intervening events and a reduction in sample size due to respondent attrition. Thus, although longitudinal data collection is desirable, it has its limitations.

Our goal is to examine the relative merits of longitudinal data collection. We begin this examination by providing a conceptual review of the value of longitudinal data collection in terms of addressing CMV bias and CI. We then perform a comparative assessment of these validity threats for cross-sectional versus longitudinal data using two survey data sets focused on collaborative new product development. Recognizing the contextual limits of these data sets, we view this assessment as largely illustrative in nature. Therefore, we further examine the boundaries of this illustration by conducting a Monte Carlo simulation that tests a wider range of parameters. Collectively, these analyses highlight the conditions under which longitudinal data collection is likely to be most valuable in terms of reducing CMV or enhancing CI. On the basis of these insights, we offer a set of guidelines to help marketing scholars and practitioners decide whether to invest in a longitudinal survey approach.

CONCERNS SURROUNDING CROSS-SECTIONAL SURVEY RESEARCH

This section conceptually examines the effectiveness of cross-sectional versus longitudinal surveys in terms of reducing CMV and enhancing CI. We developed our ideas from a review of the literature across marketing, management, economics, sociology, psychology, statistics, epidemiology, and philosophy. Our goal was to establish a set of conceptual criteria for evaluating the merits of cross-sectional versus longitudinal survey research. Thus, we complement prior research that has focused on reducing these concerns through enhanced measures or analytics (e.g., Bagozzi and Yi 1991; Podsakoff et al. 2003).[2]

Reducing CMV Bias

Several studies have found that CMV accounts for approximately 30% of the total variance in social science surveys (Cote and Buckley 1987; Doty and Glick 1998; Ostroff, Kinicki, and Clark 2002). Moreover, in a few stud-

[1]For recent examples of these data collection strategies, see Atuahene-Gima (2005), Brown and colleagues (2002), and Im and Workman (2004) (multiple respondents); Reinartz, Krafft, and Hoyer (2004), Voss, Montoya-Weiss, and Voss (2006), and Zettelmeyer, Morton, and Silva-Risso (2006) (multiple data types); and Bolton and Lemon (1999), Dahlstrom and Nygaard (1999), Jap (1999), and Maxham and Netemeyer (2002) (multiple periods).

[2]Because most longitudinal surveys entail a single follow-up study, we do not address issues related to repeated time-series data (e.g., Pauwels et al. 2004).

ies, the degree of method variance has been found to equal or exceed the amount of trait variance (Cote and Buckley 1987). Although some degree of CMV is undoubtedly present in most survey-based studies, the degree to which CMV alters the relationship between a predictor and an outcome is a topic of debate (Malholtra, Kim, and Patil 2005; Podsakoff et al. 2003).

Because most cross-sectional surveys are completed by a single respondent at a single point in time, this form of research is believed to be especially prone to potential CMV bias (Jap and Anderson 2004). Longitudinal surveys are often recommended as a solution because temporal separation reduces the cognitive accessibility of responses to predictors collected at an earlier time, which in turn reduces the likelihood that these earlier responses will influence subsequent responses to outcome variables (Hawk and Aldag 1990; Podsakoff and Organ 1986). In support of this assertion, Ostroff, Kinicki, and Clark (2002) find that correlations between organizational climate and employee satisfaction are 32% lower when measured longitudinally than when measured cross-sectionally.

When considering various strategies for reducing CMV bias, it is important to recognize that this bias is a byproduct of the research process as a whole, including measurement procedures, the choice of respondent, and the study context (Ostroff, Kinicki, and Clark 2002). As Podsakoff and colleagues (2003) note, the risk of these three influences can be reduced by various survey design strategies, many of which can be employed in a cross-sectional survey. In the remainder of this section, we review these sources of CMV bias and highlight the role of longitudinal data collection in reducing each.

Survey measurement procedures. Podsakoff and colleagues (2003) suggest that some measurement procedures are more likely to engender CMV bias than others. In particular, surveys that employ a single-scale format (e.g., a seven-point Likert scale) and common-scale anchors (e.g., "strongly disagree" versus "strongly agree") are believed to be especially prone to CMV bias. This belief is based on the notion that repeated contact with a single format and/or anchor will reduce cognitive processing and thus encourage straight-line responding that has little to do with actual item content. In theory, a longitudinal survey should minimize this danger because the outcome is separated from its predictor by time. However, if the follow-up survey also employs a common format and/or scale, a longitudinal approach may provide little value. Alternatively, the influence of measurement procedures can be reduced through measurement separation in a cross-sectional approach by employing different formats and scales for predictors versus outcomes (Crampton and Wagner 1994; Lindell and Whitney 2001).

Survey respondents. Common method variance bias may also result from respondent tendencies, including both transient states (e.g., moods) and enduring characteristics (e.g., response styles). For example, some respondents exhibit a psychological disposition to reply to survey items in a consistent manner (Podsakoff and Organ 1986; Steenkamp and Baumgartner 1998). This tendency can result in artificial covariation between a predictor and its outcome. In theory, a longitudinal approach should minimize these threats because temporal separation should break up the influence of transient moods and response styles. However, some respondent tendencies are less likely to be attenuated by temporal separation. For example, response bias, such as social desirability or acquiescence, appears to endure across multiple survey administrations (Steenkamp and Baumgartner 1998).

In addition to its limited role as a solution for certain types of response tendencies, a longitudinal approach may create additional respondent-based biases. For example, longitudinal surveys often entail a considerable degree of respondent attrition, which introduces an added risk of nonresponse bias (Armstrong and Overton 1977). Furthermore, as Podsakoff and colleagues (2003, p. 888) note, temporal separation may allow contaminating factors to intervene and thus "could mask a relationship that really exists." This solution may create some formidable side effects.

Survey context. Finally, CMV bias also appears to be at least partially attributable to a survey's context (Podsakoff et al. 2003; Williams, Cote, and Buckley 1989). For example, Cote and Buckley (1987) find that the percentage of method variance due to measurement is lower in marketing studies (16%) than in psychology or sociology (35%). This may be partly attributable to the constructs in social-psychological research (e.g., personality, affective states, cognitive processes) being more abstract than many constructs in marketing (e.g., brand loyalty, service quality, market orientation). Consequently, marketing studies that employ constructs drawn from social-psychological research may be particularly prone to CMV bias. On the basis of this logic, Crampton and Wagner (1994) suggest classifying constructs into three levels of increasing abstraction (and thus CMV proneness): (1) externally verifiable referents (e.g., new product development speed), (2) external manifestations of internal states (e.g., relationship stage), and (3) internal states and attitudes (e.g., new product satisfaction). Because contextual influences are inextricably linked to the research question a survey is designed to answer, longitudinal data seem unlikely to reduce this particular source of CMV bias.

Enhancing CIs

Marketing scholars and practitioners are typically interested in understanding how one or more marketing-related activities, processes, or structures explain various outcomes. Explanation rests on the fundamental assumption that outcomes have causes (Granger 1969). As Mackie notes (1965, p. 262), "Causal assertions are embedded in both the results and the procedures of scientific investigation." Thus, CIs lie at the heart of the type of inquiry common to most empirical marketing studies.

Philosophers of science widely agree that causal relationships are impossible to observe and cannot be proved empirically (Hume 1740; Mill 1843). Thus, causality must be inferred (Berk 1998). Over the past three centuries, philosophers and scientists have debated the principles and markers of inferred causality (Bunge 1979; Einhorn and Hogarth 1986). With a few notable exceptions (e.g., Marini and Singer 1988), most scholars suggest that temporal order is a key marker of causality (i.e., a cause must precede its effect). This principle is based on a simple but important observation of the physical world—the arrow of time flows in one direction, and the future cannot influence the past (Davis 1985; Granger 1980; Mackie 1965). As Davis notes (1985, p. 11), "after cannot cause before."

Because cross-sectional surveys collect data at a single point in time, longitudinal data are believed to possess superior CI ability (Biddle, Slavings, and Anderson 1985; Einhorn and Hogarth 1986). This belief is based on the assumption that longitudinal research captures temporal order by assessing the influence of a predictor at a time subsequent to its cause (Jap and Anderson 2004). This assumption appears to be widely held among marketing scholars. As a result, articles based on cross-sectional surveys often conclude by suggesting that longitudinal data would help untangle causal relationships (e.g., Griffith and Lusch 2007; Homburg and Fürst 2005; Ulaga and Eggert 2006; Zhou, Yim, and Tse 2005). However, research on causality questions this assumption by suggesting that (1) temporal order is not necessarily enhanced by the collection of longitudinal data and (2) temporal order is only one marker of causality.

Temporal order and longitudinal data. Several factors challenge the assumption that longitudinal data offer superior evidence of temporal order. For one, the time at which an event occurs often differs from the time at which it is recorded (Granger 1980; Marini and Singer 1988). For example, surveys of new product development often assess projects that have been under development for several months or years (e.g., Rindfleisch and Moorman 2001; Sethi, Smith, and Park 2001; Sivadas and Dwyer 2000). In these situations, there may be a natural temporal order between a cause (e.g., acquired knowledge) and its effect (e.g., product creativity) that can be captured by a cross-sectional design.

In such cases, longitudinal assessment may actually hamper CIs by weakening temporal contiguity (Marini and Singer 1988) and creating temporal erosion (Cook and Campbell 1979). Temporal erosion is a potentially severe problem, as philosophers of science typically regard causes that are temporally distant from their effects as more difficult to establish than those that are proximate (Bradburn, Rips, and Shevell 1987; Einhorn and Hogarth 1986). For example, the effect of interorganizational trust on information sharing is more likely if this trust is recent and ongoing (Moorman, Zaltman, and Deshpandé 1992; Narayandas and Rangan 2004). Conversely, some causal relationships may be less contiguous in nature and thus appear only after an extended period (Cook and Campbell 1979). For example, many diseases have a latent period between the time of exposure and the onset of illness (Rothman 1976). A similar type of latency may also occur for marketing phenomena, such as the adoption of radical innovations (Chandy and Tellis 1998). Thus, the establishment of appropriate temporal boundaries is highly dependent on theory and context (Marini and Singer 1988; Mitchell and James 2001). Consequently, longitudinal data will exhibit superior CI only if they capture these boundaries. Unfortunately, most marketing studies do not explicitly theorize the time interval in which a hypothesized effect will be manifested.

To evaluate the value of longitudinal data in capturing temporal order, it may be useful to view effects as having start and end dates that mark the earliest and latest points that the effect of a causal agent could be observed (Davis 1985; Ettlie 1977). Cross-sectional surveys face the challenge of assessing outcomes that have not yet hit their start date. Conversely, an improperly timed longitudinal survey risks temporal erosion and passing the outcome's end date (Mitchell and James 2001). In such cases, longitudinal data could result in inaccurate conclusions.

Other markers of causality. Although temporal order is a key marker of causality, it is merely one indicant. Other important cues for CI include covariation and coherence (Einhorn and Hogarth 1986; Marini and Singer 1988). These empirical cues may not necessarily be enhanced by longitudinal data.

"Covariation" is defined as correspondence in variation (i.e., correlation) between the value of a predictor and the value of an outcome and is widely regarded as a key marker of causality (Holland 1986; Marini and Singer 1988; Mill 1843). As Holland (1986, pp. 950–51) notes, "where there is correlational smoke there is likely to be causational fire." Historically referred to as "concomitant variation" (Mill 1843), this principle is based on the idea that effects are present when causes are present and that effects are absent when causes are absent. Thus, the principle of covariation originally focused simply on the presence of covariation between a predictor and an outcome. However, more recent scholarship recognizes the probabilistic nature of covariation in social science applications and suggests that the degree of covariation is also an important marker of causality (Einhorn and Hogarth 1986; Marini and Singer 1988). Because cross-sectional and longitudinal surveys employ observations rather than manipulation, both rely on covariation as an important causal cue.[3]

"Coherence" is the degree to which predictor and outcome variables conform to theoretical expectations and display a logical pattern of nomological relationships to other relevant variables. As Hill (1965, p. 298) notes, "the cause-and-effect interpretations of our data should not conflict with generally known facts." Thus, the degree to which predictor and outcome variables exhibit coherence is theory dependent. For example, consider a study that finds that trust covaries with information sharing. Prior research suggests that competitors are less trusting than channel members (Bucklin and Sengupta 1993; Park and Russo 1996). Thus, research that shows that information sharing is lower among competitors than among channel members could more confidently infer that trust is a causal agent. Given coherence's reliance on theory (rather than data collection), longitudinal data will not necessarily provide stronger evidence of coherence than cross-sectional data.

Summary and Next Steps

Cross-sectional surveys are widely believed to be biased because of CMV and limited in their degree of CI. Thus, longitudinal data collection is often recommended as a solution to these limitations. However, our review of the literature indicates that (1) this solution is incomplete and entails some potentially troubling side effects and, (2) in some cases, a well-designed cross-sectional survey may serve as an adequate substitute for longitudinal data collection.

[3]Although some scholars equate causality with experimental manipulation (e.g., Holland 1986; Rubin 1986), many social scientists regard this view as overly restrictive (Berk 1988; Biddle, Slavings, and Anderson 1985; Cook and Campbell 1979; Goldthorpe 2001; Marini and Singer 1988).

In the next section, we provide an illustrative empirical examination of these validity threats using two studies that contain both cross-sectional and longitudinal data. We then follow this empirical examination with a Monte Carlo simulation that examines these validity threats across a broader set of measurement parameters. We conclude by discussing our results and offering a set of guidelines to assist survey researchers in determining whether to collect longitudinal data.

EMPIRICAL ILLUSTRATION

To assess the relative merits of cross-sectional versus longitudinal research in terms of resolving CMV bias and enhancing CI, we use two established survey data sets of firms engaged in collaborative new product development. This context is appropriate because survey research has played a central role in prior investigations of this topic (e.g., Ganesan, Malter, and Rindfleisch 2005; Rindfleisch and Moorman 2001; Sethi 2000; Sethi, Smith, and Park 2001; Sivadas and Dwyer 2000). Moreover, research on collaborative new product development shares many of the same features (e.g., key-informant method, organizational-level constructs, modest sample sizes) common to the broader literature on interorganizational relationships. Despite this representativeness, we do not claim that our results are generalizable. Instead, our objective is to employ these two data sets as an illustrative assessment of the relative merits of cross-sectional versus longitudinal surveys.

Data Description

Both our data sets include an initial cross-sectional survey (i.e., Time 1) and a follow-up survey (i.e., Time 2) administered to the same key informant in each firm. Both studies examine the factors that influence new product development success in collaborative contexts.

The first data set, referred to as the "alliance data set," investigates firms involved in formal new product alliances across various industries (Rindfleisch and Moorman 2001, 2003). The second data set, referred to as the "optics data set," investigates firms involved in informal new product–related information-sharing relationships in the optics industry (Ganesan, Malter, and Rindfleisch 2005). To ensure comparability, all our analyses used data only from people who responded to both the Time 1 and the Time 2 surveys. As we summarize in Table 1, both studies satisfied standard criteria for key-informant qualification and nonresponse bias (Armstrong and Overton 1977; Campbell 1955). Each data set includes three predictor variables assessed at Time 1: (1) relational tie strength, (2) product knowledge acquisition, and (3) process knowledge acquisition. These data sets also include four outcome variables assessed at both Time 1 and Time 2: new product (1) cre-

Table 1

DATA CHARACTERISTICS

	Time 1 Survey	
	Alliance Study (Rindfleisch and Moorman 2001, 2003)	*Optics Study (Ganesan, Malter, and Rindfleisch 2005)*
Sampling frame	300 U.S. firms from multiple industries across 147 new product alliances	388 U.S. manufacturing firms in the optics industry
Sample size	106 firms	155 firms
Response rate	35%	44%
Key-informant criteria	66% presidents or vice presidents Highly knowledgeable (5.8 on a seven-point scale) Substantial experience within the firm (M = 15 years)	71% senior managers or above Highly knowledgeable (6.6 on a seven-point scale) Substantial experience within the firm (M = 10 years)
Nonresponse bias assessment	Early (first two-thirds) and late (last one-third) respondents were not different from one another on all key variables	Early (before reminder) and late (after reminder) respondents were not different from one another on all key variables
Measure reliability	α range: .76 to .96	α range: .81 to .91
	Time 2 Survey	
	Alliance Study (Rindfleisch and Moorman 2001, 2003)	*Optics Study (Ganesan, Malter, and Rindfleisch 2005)*
Timing of survey	36 months after Time 1 survey	30 months after Time 1 survey
Sample size	55 firms	73 firms
Response rate	70%	58%
Nonresponse bias assessment	No difference in key measures between Time 2 responders versus Time 1 nonrespondents and last one-third of Time 2 responders versus Time 1 respondents	Firms that responded to the Time 2 survey were statistically similar to nonrespondent firms
Measure reliability	α range: .87 to .96	α range: .88 to .92

ativity, (2) speed, (3) outcome satisfaction, and (4) financial satisfaction. Following the original studies from which these data are drawn, our objective is to assess differences between firms (i.e., between-subject variation) rather than changes in a firm over time (i.e., within-subject variation).

Overview of Analysis Procedures

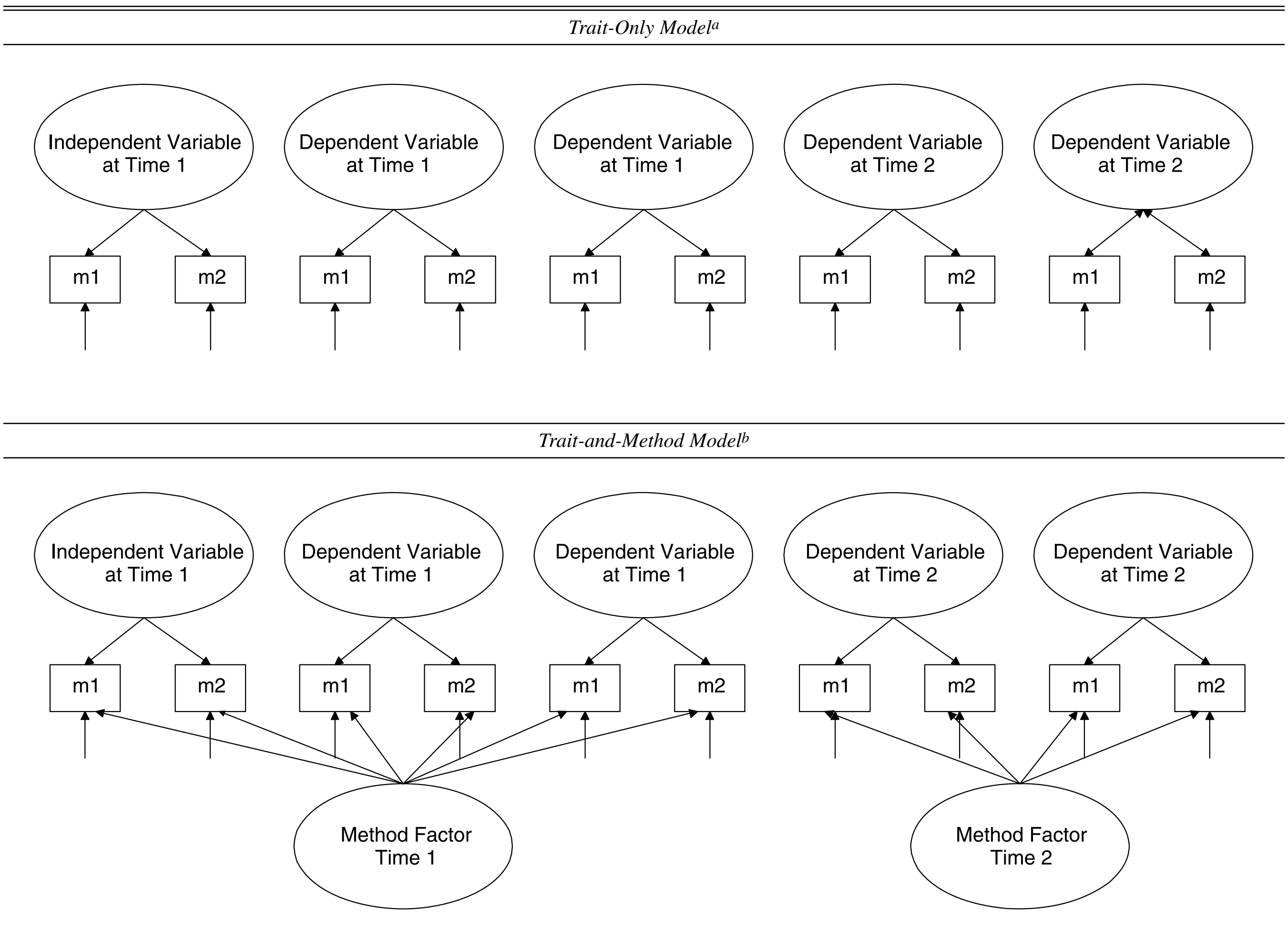
We organize our analysis of CMV bias and CI into two major sections. The first section focuses on CMV bias and follows procedures that Doty and Glick (1998) and Podsakoff and colleagues (2003) outline. Specifically, we use confirmatory factor analysis (CFA) procedures to assess the degree of CMV in the cross-sectional versus longitudinal data by separating each measure's variance into trait, method, and random components. We then examine the degree to which partitioning out method variance changes the relationship between predictors and outcomes. Thus, we examine both the level of CMV observed in our data sets and the degree to which it results in estimation bias.

The second section focuses on CI. In contrast to our assessment of CMV, our analysis of CI is considerably less clear-cut. As Granger (1980, p. 329) notes, "There is no generally accepted procedure for testing for causality." Thus, we employ a combination of quantitative and qualitative criteria to assess the relative CI ability of the cross-sectional versus longitudinal data. Our approach follows that of Einhorn and Hogarth (1986) by building a case for CI through an evaluation of multiple and distinct cues. This evaluation aligns with our conceptualization by examining cues associated with temporal order, coherence, and covariation.

Assessment of CMV Level and Degree of Bias

Level of CMV. We employ a CFA nested modeling approach that compares the fit indexes of a trait-factor model with those of a model that includes both trait and method factors. Figure 1 depicts this model. As this figure shows, the Time 1 exogenous constructs (i.e., predictors) and the Time 1 endogenous constructs (i.e., outcomes) are linked to one method factor, whereas the Time 2 endogenous constructs are linked to a second method factor. In essence, this model is a multiple-trait, multiple-method model that allows the variance in each study to be partitioned into trait, method, and error components (see

Figure 1
MODEL STRUCTURE FOR CMV ASSESSMENT



[a]All trait constructs are correlated. We show only two indicators for illustrative purposes; the actual number of indicators ranges from 3 to 7.
[b]All trait-and-method constructs are correlated. We show only two indicators for illustrative purposes; the actual number of indicators ranges from 3 to 7.

Bagozzi and Yi 1991). If the fit indexes for the trait-and-method model are superior to the trait-only model, CMV is believed to be present (Podsakoff et al. 2003).

The covariance matrices for each study served as the inputs, and all models were estimated using LISREL 8.71 (Jöreskog and Sörbom 1996). As Podsakoff and colleagues (2003) note, multiple-trait, multiple-method models often have difficulty achieving statistical convergence, especially when sample sizes are modest. We encountered this issue when all variables were employed in a single model. Thus, we tested a series of models that examined a reduced set of relationships (see Table 2). Overall, the fit indexes for the alliance data set met or exceeded recommended standards, whereas the fit indexes for the optics data set were somewhat below recommended standards. However, our objective was to assess comparative fit rather than maximize the fit of any particular model.

As Table 2 shows, the chi-square difference between each of the hierarchically nested trait-only models versus the corresponding trait-and-method models is significant at $p < .01$. The results indicate the trait-and-method models provide superior fit compared with the trait-only models, and thus there appears to be a significant level of CMV present in both data sets. To assess the level of CMV, we calculated the percentage of trait, method, and error variance for each of the CFAs displayed in Table 2.[4]

As Table 3 shows, on average, traits account for the majority (65%) of the total variance across both data sets. In contrast, method accounts for only 12% of the total variance. This percentage of method variance corresponds closely with the difference (14%) between the average variation explained between the trait-only (63%) and the trait-and-method (77%) models. To examine the incremental value of longitudinal data, we compared the change in the three variance components at Time 1 versus Time 2 across both data sets. On average, the outcome variables exhibited a 10% increase in trait variance, a 9% decrease in error variance, and only a 2% decrease in method variance at Time 2.

The variable that displays the lowest level of CMV (approximately 5%) is new product development speed (see Table 3). We believe that this is due to both method and context factors because this variable was assessed using a semantic-differential scale (whereas most of the other constructs employed Likert scales) and development speed is a concrete and externally verifiable phenomenon. In contrast, the variable that displays the highest level of CMV (approximately 20%) is new product financial satisfaction, which was assessed using a Likert scale and deals with an abstract and subjective phenomenon. These findings suggest that though the cross-sectional data contain CMV, the level is moderate, and the addition of longitudinal data does not significantly reduce it.

Degree of CMV bias. As Doty and Glick (1998) note, even if CMV is low, it may still bias the results. Thus, following their guidance, we assess the degree to which CMV biases the results of specific relationships. We assessed CMV bias by comparing the effects of the three predictors on the four outcomes (at Time 1 versus Time 2) for the trait-only models versus the trait-and-method models (Doty and Glick 1998). If CMV biases the results of the cross-sectional surveys, the path coefficients for these two models should differ.

As Table 4 shows, the overall difference in the size of the path coefficients of the outcome variables between the trait-only and the trait-and-method models was .08. This represents the difference in the size of the effects of the predictor variables on the outcome variables averaged across the two studies for both data sets. More important, the average difference in coefficients between the trait-only and the trait-and-method models was nearly identical for Time 1 (.09) and Time 2 (.07). Moreover, with the exception of financial satisfaction, all the outcome variables exhibited a similar degree of CMV bias at Time 1 and Time 2. In total, 67% (16 of 24) of the correlations in the trait-only models fell within a 95% confidence interval around the correlations in the trait-and-method models for Time 1 outcomes, and 75% (18 of 24) fell within this confidence interval for Time 2 outcomes.

In the aggregate, our results indicate that though CMV is present in both data sets, it has only a modest effect on the substantive interpretations at both Time 1 and Time 2 (see Doty and Glick 1998). Thus, these findings suggest that for these two data sets, the addition of longitudinal data provides little added value in terms of reducing the threat of CMV bias.

Assessment of CI

To examine our three markers of causality (i.e., temporal order, covariation, and coherence), we employ both quantitative analysis and qualitative evaluations. The goal of these tests is to assess the relative CI ability of the cross-sectional versus longitudinal surveys in our two data sets. Although empirical tests can provide only probabilistic cues to causality (Cook and Campbell 1979; Einhorn and Hogarth 1986; Goldthorpe 2001; Granger 1980), it is possible to infer causality with some degree of confidence if several different tests provide corroborating evidence.

Temporal order. At first glance, it may seem apparent that the Time 2 surveys in our two data sets are preferable to their Time 1 counterparts in terms of temporal order. However, to establish temporal order, a survey must satisfy three conditions: (1) There must be evidence that the cause occurred before the effect, (2) any latent period between the onset of the cause and the manifestation of the effect must have passed (i.e., the start date), and (3) the influence of the cause must still be ongoing at the time of survey measurement (i.e., the end date) (Granger 1969; Marini and Singer 1988; Rothman 1976). Although these conditions could apply to any survey, the first two appear most relevant for cross-sectional data, and the last seems most applicable to longitudinal data.

The first condition states that any variable hypothesized as a cause of another variable must precede this variable in time. Clearly, the Time 2 surveys meet this criterion because the outcome variables were collected 30 months (optics study) and 36 months (alliance study) after assessment of the predictors. Although less evident, the Time 1 surveys also appear to satisfy this criterion. When the initial

[4]Following Bagozzi and Yi (1991), we calculated the percentage of both trait and method variance for each latent construct by squaring the path coefficients in each trait-and-method model, and we assume that the remaining percentage of variance is due to error. We averaged the estimated percentages for each key construct across the various models that employed each construct.

Table 2

CFA FIT INDEXES AND MODEL COMPARISON

	Trait-Only Model					Trait-and-Method Model					Model Difference	
Alliance Study	χ^2 (*d.f.*)	*GFI*	*CFI*	*SRMR*	*RMSEA*	χ^2 (*d.f.*)	*GFI*	*CFI*	*SRMR*	*RMSEA*	$\Delta\chi^2$ (*d.f.*)	*Significance Level*
Relational ties (T1) → product creativity (T1, T2) and product speed (T1, T2)	509 (289)	.74	.95	.07	.08	452 (262)	.77	.96	.07	.07	58 (27)	.01
Product knowledge (T1) → product creativity (T1, T2) and product speed (T1, T2)	1194 (242)	.64	.75	.08	.14	1058 (217)	.68	.78	.13	.08	135 (25)	.01
Process knowledge (T1) → product creativity (T1, T2) and product speed (T1, T2)	502 (314)	.75	.95	.07	.07	408 (286)	.79	.97	.06	.05	95 (28)	.01
Relational ties (T1) → product satisfaction (T1, T2) and financial satisfaction (T1, T2)	400 (125)	.75	.92	.11	.12	189 (106)	.84	.97	.06	.08	210 (19)	.01
Product knowledge (T1) → product satisfaction (T1, T2) and financial satisfaction (T1, T2)	469 (142)	.71	.90	.11	.13	232 (122)	.81	.96	.06	.09	237 (20)	.01
Process knowledge (T1) → product satisfaction (T1, T2) and financial satisfaction (T1, T2)	446 (142)	.74	.91	.11	.12	200 (122)	.85	.98	.08	.06	246 (20)	.01

	Trait-Only Model					Trait-and-Method Model					Model Difference	
Optics Study	χ^2 (*d.f.*)	*GFI*	*CFI*	*SRMR*	*RMSEA*	χ^2 (*d.f.*)	*GFI*	*CFI*	*SRMR*	*RMSEA*	$\Delta\chi^2$ (*d.f.*)	*Significance Level*
Relational ties (T1) → product creativity (T1, T2) and product speed (T1, T2)	1120 (289)	.68	.80	.10	.12	896 (262)	.73	.84	.08	.11	224 (27)	.01
Product knowledge (T1) → product creativity (T1, T2) and product speed (T1, T2)	1122 (265)	.70	.78	.08	.12	926 (239)	.74	.83	.07	.11	196 (26)	.01
Process knowledge (T1) → product creativity (T1, T2) and product speed (T1, T2)	1043 (265)	.69	.81	.09	.12	845 (239)	.74	.85	.07	.11	199 (26)	.01
Relational ties (T1) → product satisfaction (T1, T2) and financial satisfaction (T1, T2)	892 (160)	.71	.70	.10	.14	735 (141)	.77	.76	.09	.10	157 (19)	.01
Product knowledge (T1) → product satisfaction (T1, T2) and financial satisfaction (T1, T2)	679 (142)	.74	.74	.09	.13	563 (124)	.78	.79	.09	.12	117 (18)	.01
Process knowledge (T1) → product satisfaction (T1, T2) and financial satisfaction (T1, T2)	734 (142)	.72	.76	.11	.14	577 (124)	.78	.82	.10	.12	157 (18)	.01

Notes: The trait-only model accounts for only the measures, whereas the trait-and-method model accounts for whether the measures were collected at Time 1 (T1) or Time 2 (T2). GFI = goodness-of-fit index, CFI = comparative fit index, SRMR = standardized root mean square residual, and RMSEA = root mean square error of approximation.

surveys were administered, participants reported that they had a rich history of relationships with their partner organization ($M_{Alliance}$ = 4.3 years; M_{Optics} = 6.4 years). This suggests that the measurement of these predictors at Time 1 reflect accumulated interactions that likely predate the new product development outcomes assessed at the time of initial survey administration.

The second condition (i.e., start date) also seems to be satisfied by the Time 2 surveys. Prior literature suggests that the outcomes of most new product development projects are apparent within two to three years (Chandy and Tellis 1998), which approximates the temporal gap between the initial and the follow-up surveys for both studies. To assess the influence of a temporal lag on the cross-sectional results of the alliance study, we examined the relationship between Time 1 predictors and outcomes for alliances that were early (n = 35) versus late (n = 71) in the development process. Presumably, projects at an early stage (e.g., conceptualization) should display a greater potential for a lag in new product outcomes than projects at a later stage (e.g., prototype development). To assess this, we created an interaction between each alliance's stage of development at Time 1 (early versus late) and each of the Time 1 predictor variables.[5] We regressed each of the four outcome variables on these interactions and associated main effects and found no significant interactions.[6] These findings suggest that the results of the Time 1 alliance survey are not compromised by a temporal lag.

The third condition, end date, suggests that the influence of a predictor variable diminishes at some point. As Granger (1969, p. 427) notes, if a measurement period is too far removed in time, "the details of causality cannot be picked out." The Time 1 surveys meet this criterion because respondents reported on current or recently completed collaborations. Whether Time 2 surveys meet this criterion is less clear. For example, at the time of the follow-up surveys, 64% (alliance study) and 33% (optics study) of the collaborations were not active. Thus, for these collaborations, the effect of the predictor variables on the new product development outcomes may have worn out. To test this assertion, we examined the degree to which the relationship between Time 1 predictors and Time 2 outcomes is influenced by relationship status (i.e., active versus defunct) at Time 2. We did this by examining the influence of the interaction between Time 2 relationship status and each of the Time 1 predictors on the four Time 2 outcomes. None of the interactions were significant in either data set.[7] This suggests that the Time 2 survey did not surpass the end date

Table 3

SUMMARY OF TRAIT, METHOD, AND ERROR VARIANCES ACROSS BOTH DATA SETS

	Trait Variance	*Method Variance*	*Error Variance*
Dependent Variables			
Product creativity (T1)	.71	.17	.12
Product creativity (T2)	.74	.12	.14
Product speed (T1)	.55	.04	.41
Product speed (T2)	.70	.05	.26
Financial satisfaction (T1)	.63	.19	.19
Financial satisfaction (T2)	.74	.21	.06
Product satisfaction (T1)	.68	.13	.20
Product satisfaction (T2)	.80	.08	.12
Independent Variables			
Relational ties (T1)	.51	.16	.34
Process knowledge (T1)	.66	.08	.26
Product knowledge (T1)	.51	.11	.38
Summary			
Dependent variable mean (T1)	.64	.13	.23
Dependent variable mean (T2)	.74	.11	.14
Independent variable mean (T1)	.56	.12	.33
Overall mean	.65	.12	.22

Notes: The results are based on combined analyses of all the trait-and-method models across both alliance and optics data sets (see Table 2).

Table 4

ESTIMATES OF COMMON METHOD BIAS ACROSS BOTH DATA SETS

Outcome Variable[a]	*Correlation Coefficient (Trait-and-Method Model)*[b]	*Correlation Coefficient (Trait-Only Model)*[b]	*Common Method Bias*[c]
Product creativity (T1)	.22	.28	.06
Product creativity (T2)	.29	.35	.06
Product speed (T1)	.21	.25	.04
Product speed (T2)	.10	.16	.06
Product satisfaction (T1)	.19	.28	.09
Product satisfaction (T2)	.15	.22	.07
Financial satisfaction (T1)	.10	.26	.16
Financial satisfaction (T2)	.07	.16	.09
Time 1 mean	.18	.27	.09
Time 2 mean	.15	.22	.07
Overall mean	.17	.25	.08

[a]The predictor variables in each model were relational ties, product knowledge, and process knowledge.

[b]Reflects the average correlation coefficients across all predictor variables for this outcome variable.

[c]Difference in correlation coefficients between the trait-and-method model and the trait-only model.

[5]We assessed stage of development by asking informants to report their level of agreement (using a seven-point Likert scale) with the following statement: "The project was in an early stage of development." We classified responses of 4 or less as early and responses of 5 or more as late.

[6]Coefficients for stage of development × process knowledge are not significant for creativity (–.40, n.s.), speed (–.32, n.s.), financial satisfaction (.01, n.s.), or product satisfaction (–.51, n.s.). Stage of development × product knowledge is not significant for creativity (–.22, n.s.), speed (–.07, n.s.), financial satisfaction (.59, n.s.), or product satisfaction (–.05, n.s.). Stage of development × relational ties is not significant for creativity (–.22, n.s.), speed (–.07, n.s.), financial satisfaction (.59, n.s.), or product satisfaction (–.05, n.s.). Stage of development was not measured in the optics study; thus, we cannot provide a parallel analysis for this study.

[7]For the alliance data set, coefficients for relationship status × process knowledge are not significant for creativity (–.47, n.s.), speed (–.37, n.s.), financial satisfaction (–.61, n.s.), or product satisfaction (–.21, n.s.). Likewise, relationship status × product knowledge is not significant for creativity (–.05, n.s.), speed (–.11, n.s.), financial satisfaction (.47, n.s.), or product satisfaction (.26, n.s.). Finally, relationship status × relational ties is not significant for creativity (.36, n.s.), speed (.08, n.s.), financial satisfaction (–.28, n.s.), or product satisfaction (.12, n.s.). Similarly, for the optics data set, relationship status × process knowledge is not significant for creativity (.13, n.s.), speed (.01, n.s.), financial satisfaction (–.16, n.s.), or product satisfaction (.50, n.s.). Likewise, relationship status × product knowledge is not significant for creativity (.01, n.s.), speed (.63, n.s.), or product satisfaction (.61, n.s.). Finally, relationship status × relational ties is not significant for creativity (.41, n.s.), speed (–.25, n.s.), financial satisfaction (–.22, n.s.), or product satisfaction (–.37, n.s.). The only significant effect was relationship status × product knowledge ($p < .10$) on financial satisfaction for the optics data set.

of these four predictors. In aggregate, the three criteria suggest that longitudinal data collection provides little incremental value in terms of temporal order.

Covariation. This marker of causality is typically assessed by examining the correlation between a hypothesized cause and its effect (Einhorn and Hogarth 1986). As Goldthorpe (2001) recommends, we assessed covariation using a series of structural equation models that estimated the associations between the predictor variables and the new product development outcome variables at Time 1 versus Time 2. This analysis provides a comparative assessment of the effects of the predictors (collected at Time 1) on the outcomes both cross-sectionally (i.e., Time 1) and longitudinally (i.e., Time 2). A significant difference in the coefficients of the predictors between these two periods suggests that a longitudinal approach would provide additional insights. Following our conceptualization, we examine both the presence and the degree of covariation between these predictors and the outcomes.

As Table 5 shows for the alliance data set, 10 of 12 predictors are significant at Time 1, and 8 of 12 are significant at Time 2. Likewise, for the optics data set, 9 of 12 predictors are significant at Time 1, and 7 of 12 are significant at Time 2. In summary, of the 19 predictors significant at Time 1, 15 (79%) are significant at Time 2. From a presence-of-covariation perspective, these results indicate that the longitudinal data in each study provide largely similar results as their cross-sectional counterparts.

To assess degree of covariation, we examined the size of the coefficients associated with each of our predictors for Time 1 versus Time 2 (see Table 5). Based on Cohen's (1988) criteria, the 24 correlations across both data sets exhibit a similar pattern of effect sizes across periods, with 5 large, 9 medium, and 10 small effects at Time 1 and 5 large, 3 medium, and 16 small effects at Time 2. There is no difference in the size of these coefficients across the two periods ($F_{(1, 47)}$ = .33, n.s.). Thus, the cross-sectional and longitudinal data appear to provide a similar degree of covariation.

Coherence. Coherence pertains to the extent to which the relationship between a predictor and an outcome conforms to expectations and is not subject to alternative explanations (Einhorn and Hogarth 1986; Granger 1980). We assess coherence using two approaches. First, we examined the degree to which the relationships between our predictor and the outcome variables fit within a broader nomological net. Prior theory suggests that relational ties, product knowledge, and process knowledge should have positive effects on new product creativity, speed, and outcome satisfaction. As Table 5 shows, the results of the Time 1 and Time 2 studies for both data sets display a similar degree of coherence with theoretical expectations with very few deviations.[8]

Second, we examined the degree to which the relationship between predictors and outcomes fits with other variables in our data sets, including communication frequency, alliance composition, and tacit knowledge.[9] Based on prior research, communication frequency should be more strongly associated with product and process knowledge acquisition than with satisfaction with financial outcomes (Yli-Renko, Autio, and Sapienza 2001). As we expected, communication frequency exhibits significant, positive correlations with both process (alliance: ρ = .33,

[8]For the alliance study, the only difference between the two surveys is that process and product knowledge are significantly related to speed at Time 1 but not at Time 2, and relational ties have a stronger association with creativity at Time 2 than at Time 1. For the optics study, the only empirical difference is that process knowledge is significantly related to creativity at Time 1 but not at Time 2.

[9]Collected in both the alliance and the optics studies, communication frequency measures how often the respondent communicated with external information providers about the focal new product development project using face-to-face, telephone, fax, and e-mail. We assessed alliance structure as the percentage of alliance partners who were competitors with the focal firm; we collected this only in the alliance study. Tacit knowledge is a three-item scale (α = .69) that measures the degree to which the knowledge obtained from collaborators is in noncodified form; we assessed this only in the optics study.

Table 5
CI COMPARISON FOR THE ALLIANCE AND OPTICS DATA SETS

		Correlation Between Independent Variable and Dependent Variable					
		Alliance Data Set			*Optics Data Set*		
Independent Variable (T1)	*Dependent Variable (T1 and T2)*	*Time 1*	*Time 2*	*$\Delta\chi^2(1)$*	*Time 1*	*Time 2*	*$\Delta\chi^2(1)$*
Product knowledge	Product creativity	.37	.38	.01	.36	.44	.72
Process knowledge	Product creativity	.24	.20	.11	.27	.08	4.13*
Relational ties	Product creativity	.30	.75	25.91**	.17	.23	.43
Product knowledge	Product speed	.29	–.03	10.68**	.04	.14	.81
Process knowledge	Product speed	.35	–.01	13.98**	.21	.19	.03
Relational ties	Product speed	.41	.32	.77	.19	.29	.71
Product knowledge	Product satisfaction	.14	.19	2.13	.15	.09	.23
Process knowledge	Product satisfaction	.14	.19	3.00	.24	.18	.24
Relational ties	Product satisfaction	.59	.61	.62	.32	.24	.56
Product knowledge	Financial satisfaction	.33	.13	3.44	.08	.03	.12
Process knowledge	Financial satisfaction	.25	.06	3.24	.09	.12	.10
Relational ties	Financial satisfaction	.46	.47	.01	.15	.20	.17

*p < .05.
**p < .01.
Notes: All correlations above .14 are significant at the .05 level. The chi-square difference represents a model in which the correlations between the independent variable and dependent variables are freely estimated compared with a model in which the correlations are constrained to equality.

$p < .01$; optics: $\rho = .27$, $p < .01$) and product (alliance: $\rho = .43$, $p < .01$; optics: $\rho = .26$, $p < .01$) knowledge acquisition at Time 1. In comparison, communication frequency is less strongly related to satisfaction with financial outcomes at Time 1 (alliance: $\rho = .05$, n.s.; optics: $\rho = -.06$, n.s.) and Time 2 (alliance: $\rho = .27$, $p < .10$; optics: $\rho = -.06$, n.s.). Previous research also indicates that alliances composed of competitors should exhibit lower creativity but faster development speed than alliances composed of channel members (Kotabe and Swan 1995). As we expected, alliances with more competitors are positively related to new product development speed at Time 1 ($\rho = .19$, $p < .05$) and Time 2 ($\rho = .20$, $p < .10$) and negatively related to new product creativity at Time 1 ($\rho = -.36$, $p < .01$) and Time 2 ($\rho = -.20$, $p < .10$) in the alliance study. Finally, the extant literature suggests that tacit knowledge should be positively associated with new product creativity but negatively associated with development speed (Hansen 1999). Our findings indicate that tacit knowledge is negatively related to development speed at Time 1 ($\rho = -.27$, $p < .01$) and Time 2 ($\rho = -.21$, $p < .10$) but unrelated to new product creativity at Time 1 ($\rho = -.04$, n.s.) and Time 2 ($\rho = -.17$, n.s.) in the optics study. Although these latter results run counter to our expectations, they are consistent across both periods. In combination, these findings suggest that the cross-sectional and longitudinal data display a similar degree of coherence.

MONTE CARLO SIMULATION

Thus far, our analysis suggests that longitudinal data provide little incremental value in terms of reducing CMV bias or enhancing CI. However, we recognize that our findings are largely illustrative because these two data sets focus on a fairly narrow domain (i.e., collaborative new product development) and assess a narrow range of measures. Thus, to test the validity of our findings across a broader set of conditions, we conducted a Monte Carlo simulation using EQS 6.1 (Jarvis, MacKenzie, and Podsakoff 2003; MacKenzie, Podsakoff, and Jarvis 2005). This technique enables us to better identify the conditions under which survey researchers should invest in a longitudinal approach. Although our simulation is designed primarily to test the effects of a wide range of experimental parameters on CMV bias, the results also have important implications for CI because method bias limits the ability to draw accurate CIs (Podsakoff et al. 2003).

Simulation Design

The two empirical data sets in our analysis exhibit low method variance, high trait variance, and sizable covariance between predictors and outcomes. Consequently, these data sets may not be representative of the broader body of survey research in marketing. This is an important concern because method variance is fundamental to CMV bias and can limit CI (Doty and Glick 1998). Likewise, both trait variance and construct covariance are foundational to CI and affect CMV bias (Bagozzi and Yi 1991). Thus, we designed our Monte Carlo simulation to examine the effect of method variance, trait variance, and construct covariance on CMV bias and CI across a broader set of parameters. In line with values found in many survey-based marketing studies, we specified five levels of method variance (10%, 20%, 30%, 40%, 50%), two levels of trait variance (36%, 49%), and five levels of covariance (i.e., observed correlation) between the exogenous (i.e., predictors) and the endogenous (i.e., outcomes) constructs ($\rho = .10, .30, .50, .70, .90$). Thus, our simulation employs a $5 \times 2 \times 5$ between-subjects factorial design.[10]

We designed the simulation to approximate the trait-and-method model depicted in Figure 1, which represents the structure of the CMV and CI analysis of our two data sets. Following our prior analysis, we loaded the exogenous construct at Time 1 on the trait factor, we loaded the exogenous and endogenous constructs at Time 1 on the first method factor, and we loaded the endogenous constructs at Time 2 on the second method factor. To achieve consistency with our prior analysis, we fixed the correlation between the endogenous constructs at Time 1 and Time 2 at $\rho = .20$ and the correlation of the two method factors at $\rho = .10$. All five constructs consisted of two reflective indicators to ensure identification (MacKenzie, Podsakoff, and Jarvis 2005). We set our simulation to approximate a sample size of 500 respondents. According to MacKenzie, Podsakoff, and Jarvis (2005), this represents a moderate sample for survey studies and also provides sufficient statistical power.

Simulation Results

The simulation generated a normally distributed data set for each of these 50 population covariance matrices (i.e., $5 \times 2 \times 5$), and each matrix contained 100 replications. Using this model, we calculated parameter estimates and fit statistics for all 100 replications across each of the 50 matrices. In general, the omnibus fit statistics met or surpassed recommended standards (e.g., comparative fit index = .99, root mean square error of approximation = .01, standardized root mean square residual = .01). Following prior Monte Carlo studies, we then analyzed the combined estimates associated with these 5000 observations (50 matrices × 100 replications) using an analysis of variance (ANOVA) (e.g., Jarvis, MacKenzie, and Podsakoff 2003; MacKenzie, Podsakoff, and Jarvis 2005). This approach enabled us to examine the influence of each condition (i.e., method variance, trait variance, and factor correlations) on CMV bias (and CI indirectly).

We report the ANOVA results in Table 6, which shows the effect of all three parameters and their interactions on the difference in factor correlations between Time 1 and Time 2 for each of the two endogenous constructs (Dependent Variable 1 [DV1] and Dependent Variable 2 [DV2]). These differences represent the degree of CMV bias present in cross-sectional data. As Table 6 shows, this bias is largely influenced by both the level of method variance (DV1: $F_{(4, 4950)} = 2.37$, $p < .05$; DV2: $F_{(4, 4950)} = 2.75$, $p < .03$) and the level of observed correlation between predictor

[10]We chose the levels of method variance on the basis of prior investigations, which suggest that survey research typically displays method variance levels from 10% to 50% (e.g., Cote and Buckley 1987; Doty and Glick 1998). Similarly, we chose to use a minimum trait variance of 36% because it represents a construct loading of .60. Conversely, a trait variance of .49 represents a loading of .70, which is a common level in survey-based marketing studies and is often viewed as a minimum threshold for establishing construct reliability and validity (Nunnally and Bernstein 1994). Finally, we chose a broad range of construct correlations from .10 to .90 to assess CMV for small, medium, and large effects.

Table 6
MONTE CARLO SIMULATION ANOVA RESULTS

	Difference in Factor Correlations (Time 1 Versus Time 2) for DV1			Difference in Factor Correlations (Time 1 Versus Time 2) for DV2		
Source	*d.f.*	*F*	p-*Value*	*d.f.*	*F*	p-*Value*
Method variance	4	2.37	.05	4	2.75	.03
Trait variance	1	.31	.58	1	.65	.42
Observed correlation	4	73.25	.01	4	81.41	.01
Method variance × trait variance	4	.07	.99	4	.36	.84
Method variance × observed correlation	16	.84	.64	16	.97	.49
Trait variance × observed correlation	4	.77	.55	4	.14	.97
Method variance × trait variance × observed correlation	16	.35	.99	16	.56	.92

and outcome variables (DV1: $F_{(4, 4950)} = 73.25$, $p < .01$; DV2: $F_{(4, 4950)} = 81.41$, $p < .01$). In contrast, trait variance appears unrelated (DV1: $F_{(1, 4950)} = .32$, $p < .58$; DV2: $F_{(1, 4950)} = .65$, $p < .42$) to CMV bias. There are no significant interactions among the three manipulated factors.

To calculate the degree to which method variance and correlation result in CMV bias, we conducted a series of Bonferonni pairwise comparisons of the differences in correlations across the five levels of method variance. The results indicate that only the 10% and 50% levels of method variance are statistically different at $p < .05$ for both DV1 and DV2. At the 10% level, the mean bias is –.01 for DV1 and –.03 for DV2, and at the 50% level, the mean bias is .05 for DV1 and .04 for DV2. Thus, the effect of method variance on CMV bias appears to be rather small (i.e., maximum 6% difference in factor correlations) and, in general, is limited to cases in which the method variance for a cross-sectional study is dramatically higher than its longitudinal counterpart.

We also conducted a series of Bonferonni pairwise comparisons of the differences in factor correlations across the five levels of correlations. This analysis shows that nine of the ten comparisons are significantly different at $p < .05$.[11] The mean bias associated with these various levels ranges from –.14 for DV1 and –.13 for DV2 when the observed correlation is $\rho = .20$ to a mean bias of .30 for both DV1 and DV2 when the correlation is $\rho = .90$. Notably, a correlation of $\rho = .50$ resulted in nearly no bias (0 for DV1 and –.01 for DV2), whereas a correlation of $\rho = .10$ resulted in moderate bias (–.06 for DV1 and –.11 for DV2). Similarly, a correlation of $\rho = .70$ resulted in moderate bias (.13 for DV1 and .10 for DV2).

These results suggest that CMV bias can be substantial across a wide range of observed correlations. Specifically, cross-sectional data appear to deflate structural parameter estimates, thus decreasing Type I and increasing Type II errors, when the correlation between a predictor and outcome is modest (i.e., $\rho = .10$–.30).[12] However, when this correlation is large (i.e., $\rho = .70$–.90), the deflating effect on parameter estimates is rather small. To substantiate this finding, we calculated the percentage of relative CMV bias associated with each correlation level and found that it is significantly higher for small correlations ($\rho = .10$, bias = 76%; $\rho = .30$, bias = 67%) than for large correlations ($\rho = .70$, bias = 7%; $\rho = .90$, bias = 7%).[13] These results suggest that longitudinal data are more valuable when the expected correlations between predictors and outcomes are small.

A surprising finding is that trait variance does not appear to influence CMV bias significantly. These null results may be due to the restricted range of trait variance (36% and 49%) manipulated in our initial Monte Carlo study.[14] To assess whether trait variance affects CMV bias beyond this range, we conducted a second Monte Carlo simulation that included two additional levels of trait variance (64% and 80%).[15] The results of this second simulation largely mirror those of the first. Again, both method variance and correlations have a significant influence ($p < .01$) on the degree of CMV bias between our hypothetical Time 1 and Time 2 settings. However, trait variance exhibits no significant effect on the degree of CMV bias. These results suggest that when researchers use measures that adequately capture the trait variance (in the range of 36%–80%) in an underlying construct, cross-sectional data are unlikely to produce substantial CMV bias.

Collectively, the Monte Carlo simulation results suggest that longitudinal data collection is most valuable when researchers are examining constructs, subjects, or contexts that display a substantial amount of method variance and when the correlations between predictors and outcomes are

[11]The only difference in correlations that are not significant is when the correlations are .1 and .3.

[12]As an anonymous reviewer noted, this type of deflation is the norm only in bivariate relationships. When multiple predictor variables are employed, the direction of the bias cannot be specified. Thus, this finding may not be replicated in a multivariate context.

[13]We calculated the percentatge of relative CMV bias as 100 times the difference between the parameter estimate and its population value divided by the population value.

[14]As trait variance increases, all else being equal, CMV bias should decrease because the signal-to-noise ratio increases. Our results support this conclusion. For example, with a factor correlation of .9 and method variance of 10%, CMV bias decreases from .116 when the trait loading is .6 (trait variance = 36%) to only .048 when the trait loading is .89 (trait variance = 80%). However, this trait variance–induced bias is not statistically significant in our ANOVA results. This lack of significance appears to be largely due to method variance dominating the effect of trait variance in terms of their relative influence on CMV bias (Doty and Glick 1998).

[15]The second simulation employed a $4 \times 2 \times 5$ design with four levels of trait variance (36%, 49%, 64%, 80%), five levels of correlations (10%, 30%, 50%, 70%, 90%), and two levels of method variance (10%, 20%). These trait variances indicate factor loadings of .6–.89, which represent the range of loadings often found in marketing surveys. Our selection of 36% as the minimum threshold for trait variance in the simulation was based on the result of a Monte Carlo simulation that Guadagnoli and Velicer (1988) conducted, which found that trait variances below this level are capable of providing stable population-level inferences only when sample sizes are large (i.e., n > 300).

small. Recall that our previous analyses found low method variance and high trait variance in both the alliance and the optics data sets. According to our Monte Carlo results, these conditions appear to produce minimal risk of CMV bias in cross-sectional data. The contribution of longitudinal data to reducing CMV bias and enhancing CI in these particular studies appears to be modest, and thus the time and expense of conducting follow-up surveys could have been saved. The next section discusses the broader implications of our research and offers a set of guidelines to help survey researchers assess the value of longitudinal data collection.

DISCUSSION

Summary and Conclusions

On the basis of a broad review of the literature on both CMV and CI, we offered a set of conceptual criteria for evaluating the value of longitudinal surveys in terms of addressing these two validity threats. We illustrated the application of these criteria in two data sets employing both cross-sectional and longitudinal data and then broadened this illustration by conducting Monte Carlo simulations across a wider set of empirical parameters.

Our thesis and findings indicate that a cross-sectional approach may be a viable (and less costly) means of reducing CMV bias and enhancing CI under certain conditions.[16] The results from our Monte Carlo simulations indicate that cross-sectional approaches may be sufficient when the relationships among constructs of interest are reasonably large in magnitude (e.g., trait correlations, $\rho > .50$). In contrast, when predictors and outcomes are weakly correlated, a longitudinal study may help researchers establish greater confidence in the stability of the relationships and thus enhance CI. Moreover, our analyses revealed that the two cross-sectional data sets exhibit relatively little CMV bias. This lack of apparent bias can be attributed to these data sets' focus being on relatively concrete and externally verifiable constructs (knowledge acquisition, new product development creativity, speed), and the measurement format used to assess some of the outcomes (e.g., semantic differential) differed from the format used for the key predictors (e.g., Likert). Thus, as Podsakoff and colleagues (2003) recommend, these cross-sectional studies reduce CMV bias by employing appropriate survey design techniques.

The notion that validity threats, such as CMV bias, may be minimized through survey design is an important message that is seldom voiced in the marketing community. Instead, marketing scholars have focused on eliminating these biases with statistical techniques, such as structural equation modeling. Although these techniques serve a useful function in terms of identifying the extent to which common method bias or other threats may confound interpretations of empirical relationships, there are other means of solving the CMV problem. Podsakoff and Organ (1986, p. 540) "strongly recommend the use of procedural or design remedies for dealing with the [CMV] problem as opposed to the use of statistical remedies or post-hoc patching up." Likewise, Goldthorpe (2001, p. 14) cautions that "causal explanation cannot be arrived at through statistical methodology alone." Our results are congruent with these recommendations and suggest that statistical techniques should be viewed as a supplement to, rather than a replacement for, careful survey design.

Our findings also reveal that creating temporal separation between initial and follow-up data collection may not necessarily enhance CI. For example, in both the alliance and the optics data sets, relational ties appear to have already passed their start date at the time of the initial survey. Thus, we measured this construct retrospectively at Time 1, and longitudinal data collection provided little added value in terms of temporal separation. Moreover, temporal separation is only one marker of causality and may lead to different conclusions when compared with other causal cues, such as covariation or coherence. For example, on the basis of temporal separation alone, we would expect that the causal influence of the acquisition of product and process knowledge on new product development speed should be stronger at Time 2 than at Time 1. However, our tests of covariation suggest that for our alliance data set, the Time 1 survey displays stronger causal cues than its Time 2 counterpart (see Table 5). As Einhorn and Hogarth (1986, p. 6) note, all cues to causality, including temporal ordering, are probabilistic and thus provide "only a fallible sign of a causal relation."

Guidelines for Selecting an Appropriate Data Collection Strategy

As we noted previously, CMV and CI validity concerns can be managed through three distinct survey data collection strategies: (1) multiple respondents, (2) multiple data sources, or (3) multiple periods. Our conceptualization and empirical assessment focused on the merits of the latter strategy (longitudinal data collection). In this section, we revisit the other two strategies to illustrate the relative benefits of longitudinal data collection.

According to Podsakoff and colleagues (2003), the most preferred data collection strategy for reducing CMV bias is to employ multiple respondents. This technique uses one set of respondents to assess predictors and another set to assess outcomes. For example, Im and Workman (2004) asked new product team leaders to evaluate market orientation and project managers to evaluate new product performance. This physical separation eliminates the risk of CMV bias and should also increase confidence in CI because rival method-based explanations for inferred causal relationships are rendered unlikely. Although this approach is conceptually appealing, multirespondent surveys are rare and occur almost exclusively in studies of large firms (e.g., Anderson and Weitz 1992; Atuahene-Gima 2005; Im and Workman 2004; Ross, Anderson, and Weitz 1997). Indeed, locating multiple respondents may be difficult in small organizations in which an owner/entrepreneur is in charge of most decisions (e.g., Ganesan, Malter, and Rindfleisch 2005). Multiple respondents may also be untenable for studies of consumers (e.g., Peck and Wiggins 2006) or employees (e.g., Donovan, Brown, and Mowen 2004) or for confidential interfirm relationships (e.g., Carson 2007; Hunter and Per-

[16]We do not want to imply that longitudinal data are without merit. Indeed, as we note in our following guidelines, longitudinal data may be effective in reducing CMV and enhancing CI in some cases. Moreover, longitudinal data may also serve to establish trends (e.g., Bolton and Drew 1991), assess test–retest reliability (Peter 1979), or validate new measures (Baumgartner and Steenkamp 2006).

rault 2007). Furthermore, researchers following a key-informant approach (Campbell 1955) are required to locate respondents who are intimately involved and highly knowledgeable about the topic under investigation. Locating multiple key informants may be especially difficult in firms in which decision making is highly centralized and when activities (e.g., interorganizational alliances, key account management) are largely managed by a single person.

If multiple respondents are infeasible, we suggest that survey researchers attempt to minimize CMV bias and enhance CI by obtaining multiple sources of data, such as employing a cross-sectional survey to collect a set of predictor variables and using secondary data sources for outcome variables. The use of two separate data sources nullifies the risk of CMV bias and also enhances CI ability by reducing the likelihood of rival method-based explanations. Editors and reviewers often recommend this strategy (Summers 2001; Zinkhan 2006), but survey researchers seldom employ it. A recent example of this approach is Zettelmeyer, Morton, and Silva-Risso's (2006) work, which investigates the effect of the Internet on automobile pricing by assessing consumer information usage through a cross-sectional survey and automobile prices through a commercial database of automobile transactions. Perhaps this technique is not more widely employed because many of the outcomes marketing scholars attempt to assess are perceptual in nature (e.g., opportunism, trust, relationship quality, perceived risk) or focus on units of analysis (e.g., project level) that may not be available through secondary data. Secondary data are also not typically available for confidential projects or for small or privately held organizations (cf. Voss, Montoya-Weiss, and Voss 2006).

In cases in which multiple respondents or multiple types of data are not feasible or desirable, survey researchers should consider employing a longitudinal approach. As we noted previously, in contrast to the more limited applicability of multiple respondents and multiple types of data, multiple periods can be used for both objective and subjective constructs and across a large array of consumer, firm, and interfirm contexts. Thus, multiple periods may be more appropriate than the other two approaches for minimizing CMV bias and enhancing CI. However, because of the cost (in both money and time) associated with this technique, it is infrequently applied.

Guidelines for Deciding Whether to Collect Longitudinal Data

Given the increased focus placed on CMV and CI concerns, survey researchers are more likely to consider longitudinal data collection in the years ahead. To help survey researchers evaluate the merits of collecting data a second time, we offer eight guidelines based on our conceptual foundation and empirical findings. The first three guidelines focus primarily on CMV, and the next five guidelines are directed toward CI. However, given the interrelated nature of these two validity threats, all these guidelines hold some degree of relevance for both issues. We recommend that researchers employ these guidelines, which are summarized in Table 7, as a helpful checklist when considering the merits of longitudinal data collection.

1. Nature of key constructs. Constructs that are relatively concrete, externally oriented, and verifiable are believed to display lower CMV bias than constructs that are abstract, internally oriented, and nonverifiable (Crampton and Wagner 1994; Jap and Anderson 2004). Our analysis of the alliance and optics data sets supports this view. The two measures in these data sets that were more concrete (creativity and speed) display lower levels of CMV bias than the two measures that were more abstract (product and financial satisfaction). In general, survey research in domains such as marketing strategy, marketing channels, relationship marketing, and sales force management often employs concrete and externally oriented constructs. In these cases, it appears possible to design a cross-sectional survey that minimizes CMV bias. In contrast, for research on more internally oriented and abstract topics, such as consumer or managerial attitudes, a longitudinal design may help reduce CMV bias.

2. Likelihood of response biases. By separating predictor and outcome variables over time, longitudinal surveys should minimize the dangers of some forms of response biases. However, the extent of these biases is highly dependent on both a survey's measures and its informants. For example, measures of sensitive topics, such as income (both household and corporate), are more susceptible to socially desirable response bias than measures of more innocuous topics, such as household or corporate size. Similarly, acquiescence bias appears to be most pronounced among informants who are young (i.e., children), low in educational attainment, or from minority populations (Benson and Hocevar 1985; Javeline 1999). However, survey studies in many domains, such as marketing strategy or interfirm relationships, tend to sample highly educated adults. For example, in the optics data set, 72% of the informants held advanced degrees. In these cases, the value of longitudinal data in minimizing response biases may be relatively low. Thus, a cross-sectional design seems reasonable when researchers expect low levels of response bias

Table 7

GUIDELINES FOR SELECTING A SURVEY RESEARCH APPROACH

Guideline	*Cross-Sectional Survey Design*	*Longitudinal Survey Design*
1. Nature of the key constructs	Concrete and externally oriented	Abstract and internally oriented
2. Likelihood of response biases	Low	High
3. Measurement format and scales	Heterogeneous	Homogeneous
4. Start and end dates	Unclear	Clear
5. Theoretical foundation	Well developed	Nascent
6. Likelihood of intervening events	High	Low
7. Likelihood of alternative explanations	Low	High
8. Nature of the argument	Between subjects	Within subjects

due to characteristics of their measures or informants. This recommendation is also consistent with the results of our Monte Carlo simulation, which suggest that CMV results in bias only when method variance approaches 50% of the total variance for a given measure.

3. Measurement format and scales. Over the past 25 years, the marketing literature has placed considerable attention on survey measurement (Churchill 1979; Diamantpoulous and Winklhofer 2001; Gerbing and Anderson 1988). However, this attention has focused primarily on procedures for constructing and refining scale items rather than on how these items should be formatted or scaled. Authors, reviewers, and editors appear to share an informal consensus that a Likert format and a five- to seven-point scale is the most appropriate means of assessment. Indeed, two-thirds (62 of 93) of the measures listed in the *Handbook of Marketing Scales* (Bearden and Netemeyer 1998) employ this format. However, most measures can be assessed using alternative formats, including semantic-differential, interrogative questions, and open-ended responses (for a detailed listing, see Fink 2003). The use of heterogeneous formats and scales is useful for disrupting consistency biases and increasing validity. For example, the low levels of CMV displayed in both the alliance and the optics data sets appear to be attributable to their mixed use of Likert (e.g., relational ties, knowledge acquisition) and semantic-differential (e.g., creativity, speed) formats. Thus, the use of a longitudinal approach as a means of disrupting response biases appears to be most valuable in cases in which separation in formats or scales is infeasible.

4. Start and end dates. Employing a longitudinal survey design requires researchers to have some knowledge about when the effect of a predictor variable begins and ends. For many marketing activities, such as a firm's launch of a new product or the purchase of this product by a consumer, start dates are clear. However, the date at which the effect of these activities stops is often subject to debate. For example, some researchers regard promotions as having short-term effects on consumer purchase behavior, whereas others suggest that they may also exert long-term effects (Mela, Gupta, and Lehmann 1997). In short, although the start dates of many marketing phenomena may be static, end dates can be dynamic and open-ended. In particular, predictors that involve ongoing interactions between firms, such as power or trust, are inherently dynamic and thus difficult to mark clearly with a defined end date (Jap and Anderson 2004). In these cases, the use of longitudinal data collection is challenging, as the date at which a follow-up survey should be conducted is difficult to determine.

5. Theoretical foundation. Although there is no prima facie test for determining causal relationships, philosophers of science assert that the strongest foundation for CI is the degree to which results conform to theory (Einhorn and Hogarth 1986; Goldthorpe 2001; Granger 1980; Marini and Singer 1988). The importance of theory-driven research as a means of establishing causal linkages received attention as marketers sought scientific status in the early 1980s (e.g., Bagozzi 1984; Deshpandé 1983; Hunt 1983; Peter and Olson 1983; Zaltman, LeMasters, and Heffring 1982). However, in recent years, the marketing community appears to be more focused on confirming causality through analytical techniques. Although this focus has provided important advances, analysis cannot substitute for theory. As an early statistician noted, "calculations neglect a very important part of the knowledge which we often possess" (Wright 1921, p. 559).

A well-developed theoretical foundation enhances CI by (1) providing guidelines for construct selection, (2) specifying a direction of causal flows, and (3) suggesting an array of moderators and mediators that are useful in eliminating competing theories (Zaltman, LeMasters, and Heffring 1982). Because theory development is a cumulative process, cross-sectional research in well-established domains, such as market orientation, interorganizational trust, and transaction cost theory, has a strong foundation for making causal assertions because they benefit from established measures with high trait variance. In contrast, research in developing theoretical domains, such as marketing metrics, customer relationship management, and brand communities, may have a weaker foundation (i.e., higher method variance, nascent measures) for making causal assertions. Thus, developing domains appear to have relatively more to gain from the use of longitudinal designs as a means of increasing confidence in their causal statements.

6. Likelihood of intervening events. A potential drawback of longitudinal research is that the temporal gap between an initial and a follow-up survey may allow intervening events to arise. These events are typically unanticipated and are likely to lie outside the researcher's purview. For example, imagine that a researcher assesses market orientation in one year and perceived firm performance one year later. Given that the average chief executive officer tenure is approximately five years (Allgood and Farrell 2003), a considerable number of firms in this researcher's sample are likely to experience a change in leadership between the initial and the follow-up surveys. Because top management support is a critical determinant of a firm's market orientation (Jaworski and Kohli 1993), chief executive officer turnover is an unanticipated intervening event that may alter the relationship between market orientation collected in an initial survey and performance collected at a later time.

By definition, unanticipated intervening events are extremely difficult to foresee. However, we suggest that these events are more likely to arise for marketing phenomena that are part of an open system (Scott 2004) and subject to turbulent environments (Davis, Morris, and Allen 1991). For example, prior research suggests that the value of organizational memory is heavily dependent on changes in the environment (Moorman and Miner 1997). In contrast, sales force compensation systems may be a relatively closed system and less open to outside influences (Cravens et al. 1993). Thus, longitudinal research should be better suited for the latter topic than for the former.

7. Likelihood of alternative explanations. In addition to assessing causal cues, such as temporal order, covariation, and coherence, researchers can also enhance CI by eliminating alternative explanations (Cook and Campbell 1979; Mill 1843; Popper 1959). In general, longitudinal data are viewed as superior to cross-sectional data in terms of reducing the risk of alternative explanations because this approach allows researchers to incorporate fixed effects into their design and analysis (Hsiao 2003). For example, by surveying the same firms at two points in time and analyzing this data using a within-subjects procedure, Rindfleisch and Moorman's (2003) study of the influence of competitor alliances on customer orientation reduces the likelihood of alternative explanations due to omitted variable bias.

Although unable to incorporate fixed effects over time, the likelihood of alternative explanations can be reduced in cross-sectional surveys through appropriate data collection strategies. For example, many cross-sectional studies attempt to obviate competing explanations by assessing the indicants of these explanations with a control variable approach. Unfortunately, length restrictions limit the number of control variables that can be assessed in a given survey. Alternatively, cross-sectional designs can incorporate fixed effects by asking a consumer or manager to rate multiple (rather than singular) instances of a phenomenon (e.g., relationships with three different suppliers) (e.g., Cotte and Wood 2004). However, this approach is rarely employed, perhaps because of the reporting burden placed on respondents. We encourage researchers to consider this approach because it increases their ability to rule out alternative explanations without creating the challenges typically associated with a longitudinal data collection (e.g., respondent attrition, unclear end dates). Nevertheless, when the likelihood of alternative explanations is high and researchers are unable to account for them fully through control variables or reporting multiple phenomena, a longitudinal data collection approach is well advised.

8. Nature of the argument. Many survey-based marketing studies appear to focus on how outcomes differ among entities that possess different levels of a predictor (e.g., how information usage differs among firms with high versus low levels of trust). In other words, their conceptual argument has a between-subjects nature. Conversely, researchers could focus on how outcomes are influenced by changes in a predictor within a set of entities (e.g., how an increase in trust affects information usage within a particular firm). In this case, the conceptual argument has a within-subjects nature.[17] Of these two types of arguments, longitudinal data collection appears to be most valuable for the latter because within-subjects comparisons are typically obtained through multiple observations over time.[18] However, most survey-based marketing studies appear to focus on between-subject arguments. Thus, for these types of studies, longitudinal data collection may not be necessary.

Unfortunately, most survey-based marketing studies do not clearly specify the nature of their argument. Some topics appear inherently to be of a between-subjects nature (e.g., consumer values), whereas others have more of a within-subject flavor (e.g., relationship life cycle) However, many popular theoretical platforms (e.g., trust and commitment, market orientation, transaction cost analysis, resource-dependence theory) can be viewed as occurring either between or within subjects. Thus, survey researchers should duly specify the nature of their conceptual argument and attempt to adopt a longitudinal data collection approach if they view the relationship between their predictors and outcomes as occurring mainly within subjects rather than between them.[19] Moreover, researchers who are studying arguments that are inherently of a between-subjects nature should clearly state this and note the relevance of cross-sectional data for their research objective. This explicit notation should lend additional confidence and validation for their use of a cross-sectional survey technique.

CONCLUSION

This article examines the relative merits of cross-sectional versus longitudinal survey research in terms of reducing the threat of CMV bias and enhancing CI. Our conceptual arguments and empirical results indicate that though longitudinal surveys may offer some advantages in terms of reducing these two validity threats, a cross-sectional approach may be adequate in many situations. Specifically, our research reveals that cross-sectional data are most appropriate for studies that examine concrete and externally oriented constructs, sample highly educated respondents, employ a diverse array of measurement formats and scales, and are strongly rooted in theory. In contrast, a longitudinal approach is most appropriate when the temporal nature of the phenomena is clear, when it is unlikely that intervening events could confound a follow-up study, or when alternative explanations are likely and cannot be controlled with a cross-sectional approach. To maximize the validity of either approach, researchers need to employ a combination of strong theory, careful survey design, and appropriate statistical tools.

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[17]We thank an anonymous reviewer for pointing out this important distinction.

[18]Cross-sectional data collection can also be used to examine within-subject arguments when an informant rates two or more instances of the phenomenon.

[19]In situations in which longitudinal data collection is infeasible, researchers interested in assessing developmental phenomenon may be able to simulate these effects with cross-sectional designs that assess the phenomenon at various temporal stages between firms. For example, Jap and Ganesan (2000) assess the effect of relationship life cycle using a cross-sectional approach that asked respondents to classify their relationship life cycle stage. Then, they use this classification to capture relationship dynamics across the life cycle. Thus, although the same firms were not followed over time, they were able to use a cross-sectional approach to capture between-firm variation that provided important insights into relationship evolution over time.

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