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

By

Aric Rindfleisch
University of Wisconsin-Madison

Alan J. Malter
University of Arizona

Shankar Ganesan
University of Arizona

Christine Moorman
Duke University

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Institute for the Study of Business Markets
The Pennsylvania State University
484 Business Building
University Park, PA 16802-3603
(814) 863-2782 or (814) 863-0413 Fax
www.isbm.org, isbm@psu.edu

CROSS-SECTIONAL VERSUS LONGITUDINAL SURVEY RESEARCH:

CONCEPTS, FINDINGS, AND GUIDELINES

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Aric Rindfleisch*
Alan J. Malter
Shankar Ganesan
Christine Moorman

*Aric Rindfleisch (aric@bus.wisc.edu) is Associate Professor of Marketing, School of Business, University of Wisconsin-Madison. Alan J. Malter (amalter@eller.arizona.edu) is Assistant Professor of Marketing and Shankar Ganesan (sganesan@eller.arizona.edu) is Associate Professor of Marketing and McCoy-Rogers Faculty Fellow, Eller College of Management, University of Arizona. Christine Moorman (moorman@duke.edu) is T. Austin Finch, Sr. Professor of Marketing, The Fuqua School of Business, Duke University. This research was supported by grants from the Institute for the Study of Business Markets, the Marketing Science Institute, and the Netherlands Organization for Scientific Research. This research also benefited from the helpful comments of Russ Winer, Ruth Bolton, Man-Wai Chow, Alka Citrin, Jan Heide, John Lynch, Scott MacKenzie, Jan-Benedict Steenkamp, Fred Webster, seminar participants at Erasmus University Rotterdam, INSEAD, Georgia Tech, University of Groningen, and anonymous reviewers.

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ABSTRACT

Cross-sectional surveys are frequently employed by marketing academics and business marketing practitioners. 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 paper, we conceptually examine the role of longitudinal surveys in addressing these validity concerns. We then provide an illustrative comparison of the validity of cross-sectional vs. longitudinal surveys using two datasets as well as a Monte Carlo simulation. Our conceptualization and findings suggest that under certain conditions, results from cross-sectional data exhibit validity comparable to results obtained from longitudinal data. We conclude by offering a set of guidelines to assist academic and business marketing researchers in deciding whether to employ a longitudinal survey approach.

In order to understand, explain, and predict marketplace behavior, marketing academics and practitioners ask questions. Although these questions take many forms, they often appear as items in surveys of managers or consumers. Of the 636 empirical articles published in the *Journal of Marketing* and *Journal of Marketing Research* between 1996 and 2005, 178 (nearly 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), non-response 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 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, and 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, as CMV bias 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 are 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) in particular, which is widely viewed as especially prone to CMV bias and incapable of causal insights. This rising concern about the validity of cross-sectional surveys is an important issue, as this method represents the most common form of empirical research in many areas, including marketing channels, salesforce management, and marketing strategy, and hence provides a critical foundation for much of our knowledge of these topics (Jap and Anderson 2004). In fact, of the 178 survey-based *JM* and *JMR* articles noted earlier, 94% are cross-sectional in nature.

In order to reduce the threat of CMV bias and enhance CI, survey researchers typically recommend three different data collection strategies: (1) employing multiple respondents, (2) obtaining multiple types of data, or (3) gathering data over multiple time 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, as few survey articles employ any of 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 of marketing. Consequently, the marketing literature provides little guidance in 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 time periods (i.e., longitudinal data). We focus on this strategy because we believe 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 data bases (e.g., firm level). However, many constructs of interest to marketing scholars are more subjective in nature (e.g., opportunism, relationship quality,

[1] For recent examples of these data collection strategies, see Atuahene-Gima (2005), Brown et al. (2002), and Im and Workman (2004) (multiple respondents), Reinartz et al. (2004), Voss et al (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 time periods).

trust, non-financial performance) or examine units of analysis (e.g., sub-unit or project-level) that are difficult to obtain from a source other than a survey. Second, while employing multiple informants by gathering predictors from one respondent and outcomes from another may be appropriate when surveying large firms, this approach is quite difficult when surveying small firms or consumers (e.g., Brown et al. 2002; Erdem et al. 2006; Voss et al. 2006). In contrast, longitudinal data can be obtained for any measure or subject employed in a cross-sectional survey.

Hence, collecting longitudinal data collection as a means of reducing CMV and enhancing CI appears 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 a number of potential problems such as confounds due to intervening events, and a reduction in sample size due to respondent attrition. Thus, while desirable, longitudinal data collection 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 efficacy of longitudinal data collection in terms of addressing CMV bias and CI. We then perform a comparative assessment of these two validity threats for cross-sectional vs. longitudinal data using two survey datasets focused on collaborative new product development. Recognizing the contextual limits of these datasets, 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. Based on 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 vs. longitudinal surveys in terms of reducing CMV and enhancing CI. We developed our ideas from a review of literature across marketing, management, economics, sociology, psychology, statistics, epidemiology, and philosophy. Our goal is to establish a set of conceptual criteria for evaluating the merits of cross-sectional vs. longitudinal survey research. Thus, we aim to complement prior research which has focused on reducing these concerns via enhanced measures or analytics (e.g., Bagozzi and Yi 1991; Podsakoff et al. 2003).[2]

Reducing Common Method Variance 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 et al. 2002). Moreover, in a few studies, the degree of method variance has been found to equal or exceed the amount of trait variance (Cote and Buckley 1987). While some degree of CMV is undoubtedly present in most survey-based studies, the degree to which CMV alters the relationship between a predictor and 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 et al. (2002) find that correlations between organizational climate and employee satisfaction are 32% lower when measured longitudinally rather than cross-sectionally.

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[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).

When considering various strategies for reducing CMV bias, it is important to recognize that this bias is a by-product of the research process as a whole, including the measurement procedures, the respondent, and the study context (Ostroff et al. 2002). As noted by Podsakoff et al. (2003), 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 et al. (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 7-point Likert scale) and common-scale anchors (e.g., "strongly disagree" vs. "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, hence, 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 via *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 (Baumgartner and Steenkamp 1998; Podsakoff and Organ 1986). This tendency can result in artificial covariation between a predictor and its outcome. In theory, a longitudinal approach should minimize these threats, as 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 (Baumgartner and Steenkamp 1998).

In addition to its limited role as a solution for certain types of response tendencies, a longitudinal approach can also create additional respondent-based biases. For example, longitudinal surveys often entail a considerable degree of respondent attrition, which introduces an added risk of non-response bias (Armstrong and Overton 1977). Also, as noted by Podsakoff et al. (2003), temporal separation may allow contaminating factors to intervene, and thus, “could mask a relationship that really exists” (p. 888). Hence, this solution may create some formidable side effects.

Survey Context. Finally, CMV bias also appears 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 fact that the constructs in social-psychological research (e.g., personality, affective states, and cognitive processes) are more abstract compared to many constructs in marketing (e.g., brand loyalty, service quality, and market orientation). Consequently, marketing studies that employ constructs drawn from social-psychological research may be particularly prone to CMV bias. Based on this logic, Crampton and Wagner (1994) suggest classifying constructs into three levels of increasing abstraction (and hence 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). Since contextual influences are inextricably linked to the research question that a survey is designed to answer, longitudinal data seems unlikely to reduce this particular source of CMV bias.

Enhancing Causal Inferences

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 noted by Mackie (1965, p. 262), "Causal assertions are embedded in both the results and the procedures of scientific investigation." Hence, causal inferences 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 empirically proven (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 (cf. 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* causal inferences 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 generally regard causes that are temporally distant from their effects as harder to establish than those that are proximate (Bradburn, Rips, and Shevell 1987; Einhorn and Hogarth 1986). For example, the effect of interorganizational trust upon information sharing is more likely if this trust is recent and ongoing (Moorman, Zaltman, and Deshpandé 1992; Narayandas and Rangan 2004). On the other hand, some causal relationships may be less contiguous in nature, and hence only appear after an extended period of time (Cook and Campbell 1979). For instance, 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 upon theory and context (Marini and Singer 1988; Mitchell and James 2001). Consequently, longitudinal data will exhibit superior CI only if it captures these boundaries. Unfortunately, most marketing studies do not explicitly theorize the time interval in which a hypothesized effect will be manifested.

In order to evaluate the value of longitudinal data in capturing temporal order, it may be useful to think of effects as having *start* and *end* dates which mark the earliest and latest points in time 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.[3] 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 causal inference include covariation and coherence (Einhorn and Hogarth 1986; Marini and Singer 1988). As detailed below, 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 noted by Holland (1986, pp. 950-951), "…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 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).[4] Because cross-sectional and longitudinal surveys employ observations rather than manipulation, both rely upon covariation as an important causal cue.[5]

Coherence is the degree to which predictor and outcome variables conform to theoretical expectations and display a logical pattern of nomological relationships with other relevant variables. As noted by Hill (1965, p. 298), "the cause-and-effect interpretations of our data should not conflict

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[3] For cross-sectional surveys of ongoing processes (e.g., relational norms, power, and dependence) it seems likely that the start dates for the outcome variables associated with these processes will precede data collection.
[4] Some scholars employ the term "congruity" to refer to the strength or degree of association between predictors and outcomes (e.g., Einhorn and Hogath 1986; Marini and Singer 1988).
[5] 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 et al. 1985; Cook and Campbell 1979; Goldthorpe 2001; Marini and Singer 1988).

with generally known facts." Thus, the degree to which predictor and outcome variables exhibit coherence is theory dependent. For example, consider a study which 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). Hence, research which shows that information sharing is lower among competitors than channel members could more confidently infer that trust may be 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 due to 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 suggests 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.

In the next section, we provide an illustrative empirical examination of these two 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 finish by discussing our results and offering a set of guidelines to assist survey researchers in determining whether or not to collect longitudinal data as a means of reducing CMV and enhancing CI.

EMPIRICAL ILLUSTRATION

To assess the relative merits of cross-sectional vs. longitudinal research in terms of resolving CMV bias and enhancing CI, we utilize two established survey datasets of firms engaged in collaborative new product development. This context is appropriate, as 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 et al. 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, etc.) 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 datasets as an illustrative assessment of the relative merits of cross-sectional vs. longitudinal surveys.

Data Description

Both of our datasets 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 sought to examine the factors that influence new product development success in collaborative contexts.

The first dataset, referred to as the *alliance dataset*, investigates firms involved in formal new product alliances across a variety of industries (Rindfleisch and Moorman 2001, 2003). The second dataset, referred to as the *optics dataset,* investigates firms involved in informal new product-related information sharing relationships in the optics industry (Ganesan et al. 2005). In order to ensure comparability, all of our analyses used data only from individuals that responded to both the Time 1 and Time 2 surveys. As summarized in Table 1, both studies satisfied standard criteria for key informant qualification and non-response bias (Armstrong and Overton 1977; Campbell 1955). Each dataset includes three predictor variables assessed at Time 1: (1) relational tie strength, (2) product knowledge acquisition, and (3) process knowledge acquisition. These datasets also includes four outcome variables assessed at both Time 1 and Time 2: (1) new product creativity, (2) new product development 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 outlined by Doty and Glick (1998) and Podsakoff et al. (2003). Specifically, we use confirmatory factor analysis (CFA) procedures to assess the degree of CMV in the cross-sectional vs. 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 datasets as well as 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 noted by Granger (1980, p. 329), "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 vs. longitudinal data. Our approach follows Einhorn and Hogarth (1986) by building a case for causal inferences via 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 Common Method Variance Level and Degree of Bias

Level of CMV. We employ a CFA nested modeling approach which compares the fit indices of a trait-factor model with those of a model that includes both trait and method factors. Figure 1 depicts this model. As shown in this figure, the Time 1 exogenous constructs (i.e., predictors) and the Time 1 endogenous constructs (i.e., outcomes) are linked to one method factor, while the Time 2 endogenous constructs are linked to a second method factor. In essence, this model is a multiple-trait, multiple-method (MTMM) model that allows the variance in each study to be partitioned into trait, method, and error components (see Bagozzi and Yi 1991). If the fit indices 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 noted by Podsakoff et al. (2003), MTMM 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 indices for the alliance dataset met or exceeded recommended standards, while the fit indices for the optics dataset were somewhat below recommended standards. However, our objective is to assess *comparative fit* rather than maximize the fit of any particular model.

As shown in Table 2, the chi-square difference between each of the hierarchically-nested trait-only models versus the corresponding method-and-trait models is significant at $p < .01$. Results indicate the trait-and-method models provide superior fit compared to the trait-only models, and thus, there appears to be a significant level of CMV present in both datasets. In order 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.[6]

As shown in Table 3, on average, traits account for the majority (65%) of the total variance across both datasets. 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%) vs. 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 vs. Time 2 across both datasets. 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.

[6] 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 assume that the remaining percentage of variance is due to error. The estimated percentages for each key construct were averaged across the various models that employed each construct.

The variable that displays the lowest level of CMV (approximately 5%) is new product development speed (see Table 3). We believe this is due to both method and context factors, as this variable was assessed using a semantic-differential scale (while 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 while the cross-sectional data contain common method variance, the level is moderate and the addition of longitudinal data does not significantly reduce it.[7]

Degree of CMV Bias. As noted by Doty and Glick (1998), even if CMV is low, it may still bias results. Thus, following their guidance, we assess the degree to which CMV biases the results of specific relationships. Thus, we assessed CMV *bias* by comparing the effects of the three predictors on the four outcomes (at Time 1 vs. Time 2) for the trait-only models vs. 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 shown in Table 4, the overall difference in the size of the path coefficients of the outcome variables between the trait-only vs. 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 datasets. More importantly, the average difference in coefficients between the trait-only vs. the trait-and-method models was nearly identical for Time 1 (.09) vs. Time 2 (.07). Moreover, with the exception of financial satisfaction, all of the outcome variables exhibited a similar degree of CMV bias at Time 1 vs. Time 2. In total, 67% (16 out 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 out of 24) fell within this confidence interval for Time 2 outcomes.

[7] In order to validate our CMV assessment, we also conducted an additional test recommended by Podsakoff et al. (2003) (i.e., correlated uniqueness model). In this model, each observed variable is caused by one trait factor and a measurement error term. The results of this test generally support the findings of our MTMM analysis and indicate that the Time 1 datasets exhibit a modest amount of CMV.

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

Assessment of Causal Inference

In order to examine our three markers of causality (i.e., temporal order, covariation, and coherence), we employ both quantitative analysis as well as qualitative evaluations. The goal of these tests is to assess the relative CI ability of the cross-sectional vs. longitudinal surveys in our two datasets. 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 a number of different tests provide corroborating evidence.

Temporal Order. At first glance, it may seem apparent that the Time 2 surveys in our two datasets are preferable to their Time 1 counterparts in terms of temporal order. However, in order to establish temporal order, a survey must satisfy three conditions: (1) there must be evidence the cause occurred prior to 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, while 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, as the outcome variables were collected 30 months (optics study) and 36 months (alliance study) following assessment of the predictors. Although less evident, the Time 1 surveys also appear to satisfy this criterion. When the initial surveys were administered, participants reported that they had a rich history of relations 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), like the first, seems 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 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 the Time 1 predictor and outcomes for alliances that were early (n = 35) vs. 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 compared to 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 vs. late) and each of the Time 1 predictor variables.[8] We regressed each of the four outcome variables on these interactions and associated main effects and found no significant interactions.[9] 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, as respondents reported on current or recently completed collaborations. The ability of the Time 2 surveys to meet this criterion is not as 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

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[8] Stage of development was assessed by asking informants to report their level of agreement (using a 7-point Likert scale) with this statement: "The project was in an early stage of development." Responses of 4 or less were classified as early, while responses of 5 or more were classified as late.

[9] Coefficients for stage of development*process knowledge are not significant for creativity (-.40, *ns*), speed (-.32, *ns*), financial satisfaction (.01, *ns*), or product satisfaction (-.51, *ns*). Stage of development*product knowledge is not significant for creativity (-.22, *ns*), speed (-.07, *ns*), financial satisfaction (.59, *ns*), or product satisfaction (-.05, *ns*). Stage of development*relational ties is not significant for creativity (-.22, *ns*), speed (-.07, *ns*), financial satisfaction (.59, *ns*), or product satisfaction (-.05, *ns*). Stage of development was not assessed in the optics study; hence, we cannot provide a parallel analysis for this study.

the predictor variables upon the NPD 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 vs. 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 dataset.[10] This suggests that the Time 2 survey did not surpass the end date of these four predictors. In aggregate, the three criteria suggest that the longitudinal data collection provided 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 recommended by Goldthorpe (2001), we assessed covariation using series of structural equation models (SEM) that estimated the associations between the predictor variables and the NPD outcome variables at Time 1 vs. Time 2. This analysis provides a comparative assessment of the effects of the predictors (collected at Time 1) upon 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 time periods suggests that a longitudinal approach would provide additional insights. Following our conceptualization, we examine both the presence and degree of covariation between these predictors and outcomes.

As shown in Table 5 for the alliance dataset, 10 of 12 predictors are significant at Time 1 and 8 of 12 are significant at Time 2. Likewise, for the optics dataset, 9 of 12 predictors are significant at Time 1 and 7 of 12 are significant at Time 2. In sum, of the 19 predictors significant at Time 1, 15

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

(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.

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

Coherence. Coherence concerns 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 relationship between our predictor and 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 shown in Table 5, the results of the Time 1 and Time 2 studies for both datasets display a similar degree of coherence with theoretical expectations with very few deviations.[11]

Second, we examined the degree to which the relationship between predictors and outcomes fits with other variables in our datasets, including communication frequency, alliance composition, and tacit knowledge.[12] Based on prior research, communication frequency should be more strongly associated with product and process knowledge acquisition than with satisfaction with financial

[11] For the alliance study, the only difference between the two surveys are that process and product knowledge are significantly related to speed at Time 1 but not Time 2, and relational ties has 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 Time 2.

[12] Collected in both the alliance and optics studies, communication frequency measures how often the respondent communicated with external information providers about the focal NPD project using face-to-face, telephone, fax, and email. Alliance structure was assessed as the percentage of alliance partners who were competitors to the focal firm, and was collected only in the alliance study. Tacit knowledge is a 3-item scale ($\alpha = .69$) measuring the degree to which the knowledge obtained from collaborators is in non-codified form and was assessed only in the optics study.

outcomes (Yli-Renko, Autio, and Sapienza 2001). As expected, communication frequency exhibits significant positive correlations with both process (Alliance: $\rho = .33$, $p < .01$; 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: $\rho = .05$, *ns*; Optics: $\rho = -.06$, *ns*) and Time 2 (Alliance: $\rho = .27$, $p < .10$; Optics: $\rho = -.06$, *ns*). 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 expected, alliances with more competitors are positively related to NPD 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 ($\rho = -.04$, *ns*) and Time 2 ($\rho = -.17$, *ns*) in the optics study. While these latter results run counter to expectations, they are consistent across both time 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 our findings are largely illustrative, as these two datasets focus on a fairly narrow domain (i.e., collaborative NPD) and limited 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 allows us to better identify the conditions under which survey researchers should invest in a longitudinal approach. Although our simulation is primarily designed to test the effects of a wide range of experimental parameters on CMV bias, the results also have important implications for CI, as method bias limits the ability to draw accurate causal inferences (Podsakoff et al. 2003).

Simulation Design

The two empirical datasets in our analysis exhibit low method variance, high trait variance, and sizable covariance between predictors and outcomes. Consequently, these datasets may not be representative of the broader body of survey research in marketing. This is an important concern, as 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 also impact CMV bias (Bagozzi and Yi 1991). Thus, our Monte Carlo simulation was designed to examine the effect of method variance, trait variance, and construct covariance on CMV bias and CI across a broader set of parameters. Following 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 endogenous (i.e., outcomes) constructs (ρ = .10, .30, .50, .70, .90), which represents a 5 x 2 x 5 between-subjects factorial design.[13]

The simulation was designed 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 earlier analysis, the exogenous construct at Time 1 was loaded on the trait factor, the exogenous and endogenous constructs at Time 1 were loaded on the first method factor, and the endogenous constructs at Time 2 were loaded on the second method factor. To achieve consistency with our earlier

[13] We chose the levels of method variance based on 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% as 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.

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 in order to ensure identification (MacKenzie et al. 2005). Our simulation was set to approximate a sample size of 500 respondents. According to MacKenzie et al. (2005), this represents a moderate sample for survey studies and also provides sufficient statistical power.

Simulation Results

The simulation generated a normally distributed dataset for each of these 50 population covariance matrices (i.e., 5 x 2 x 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., CFI = .99, RMSEA = .01, SRMR = .01).[14] Following prior Monte Carlo studies, we then analyzed the combined estimates associated with these 5,000 observations (50 matrices x 100 replications) using ANOVA (e.g., Jarvis et al. 2003; MacKenzie et al. 2005). This approach allowed us to examine the influence of each condition (i.e., method variance, trait variance, and factor correlations) on CMV bias (and CI indirectly).

The ANOVA results are reported 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 (DV1 and DV2). These differences represent the degree of CMV bias present in cross-sectional data. As shown in Table 6, 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 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: $F_{(1, 4950)} = .32$, $p < .58$;

[14] We decided not to include a trait-only model in our simulation as it would result in misspecification. A trait-only model would suggest a method variance of 0%, which is much lower than the minimum found in both Doty and Glick (1998) and Cote and Buckley (1987) and also below the method variance levels used in our experimental design.

DV2: $F_{(1, 4950)} = .65$, $p < .42$) to CMV bias. There are no significant interactions among the three manipulated factors.

In order 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. 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, while 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 rather small (i.e., maximum 6% difference in factor correlations) and is generally 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 $p < .05$.[15] The mean bias associated with these various levels range 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 $\rho = .90$. Interestingly, a correlation of $\rho = .50$ resulted in nearly no bias (0 for DV1 and -.01 for DV2), while 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., $\rho = .10$ to $.30$).[16] However, when this correlation is large (i.e., $\rho = .70$ to $.90$), the

[15] The only difference in correlations which are not significant is when the correlations are .1 and .3.
[16] As noted by an anonymous reviewer, 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.

deflating effect on parameter estimates is rather small. To substantiate this finding, we calculated the percent of relative CMV bias associated with each correlation level and found that the percentage of bias is significantly higher for small correlations (ρ = .10, bias = 76%; ρ = .30, bias = 67%) than for large correlations (ρ = .70, bias = 7%; ρ = .90, bias = 7%).[17] These results suggest that longitudinal data are more valuable when the expected correlations between predictors and outcomes are small.

One surprising finding is that trait variance does not appear to significantly influence CMV bias. These null results may be due to the restricted range of trait variance (36% and 49%) manipulated in our initial Monte Carlo study.[18] To assess whether trait variance impacts CMV bias beyond this range, we conducted a second Monte Carlo simulation that included two additional levels of trait variance (64% and 80%).[19] Results of this second simulation largely mirror those of the first.[20] As before, 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, once again, trait variance exhibits no significant effect on the degree of CMV bias. These results suggest that when researchers utilize measures that adequately capture the trait variance (in the range of 36% to 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 small.

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[17] Percent of relative CMV bias is calculated as 100 x the difference between the parameter estimate and its population value divided by the population value.

[18] As trait variance increases, *ceteris paribus*, CMV bias should decrease (because the signal-to-noise ratio increases). In fact, 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 the fact that method variance dominates the effect of trait variance in terms of their relative influence on CMV bias (Doty and Glick 1998).

[19] The second simulation employed a 4 x 2 x 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 to .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 conducted by Guadagnoli and Velicer (1988), which found that trait variances below this level are capable of provide stable population-level inferences only when sample sizes are large (i.e., n > 300).

[20] Additional details regarding the results of this analysis are available from the authors.

Recall our earlier analyses found low method variance and high trait variance in both the alliance and optics datasets. According to our Monte Carlo results, these conditions appear to produce minimal risk of CMV bias in cross-sectional data. Thus, the contribution of longitudinal data to reducing CMV bias and enhancing CI in these particular studies appears to be modest. Hence, the time and expense of conducting follow-up surveys could have been saved without compromising the validity of their cross-sectional findings. Our 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

Based on 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 datasets 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 suggest that a cross-sectional approach may be a viable (and less costly) means of reducing common method bias and enhancing causal inference under certain conditions.[21] The results from our Monte Carlo simulations suggest 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 causal inference. Moreover, our analyses revealed that the two cross-sectional datasets exhibit relatively little CMV bias. This lack of apparent bias can be attributed to the

[21] We do not wish to imply that longitudinal data are without merit. Indeed, as noted 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).

fact that these datasets focused on relatively concrete and externally-verifiable constructs (knowledge acquisition, new product development creativity, speed), as well as 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 recommended by Podsakoff et al. (2003), 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 via statistical techniques, such as structural equation modeling. While 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 common method variance 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 an initial and follow-up data collection may not necessarily enhance causal inference. For example, in both the alliance and optics datasets, relational ties appear to have already passed their start date at the time of the initial survey. Thus, this construct was measured 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 to other causal cues, such as covariation or coherence. For example, based on temporal separation alone, we would expect that the causal influence of the acquisition of product and process knowledge on NPD speed should be clearer at Time 2 than at Time 1. However, our tests of covariation suggest that, for our alliance dataset, the

Time 1 survey displays stronger causal cues than its Time 2 counterpart (see Table 5). As noted by Einhorn and Hogarth (1986, p. 6), 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 noted earlier, CMV and CI validity concerns can be managed via three distinct survey data collections strategies: (1) multiple respondents, (2) multiple data sources, and (3) multiple time 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 in order to illustrate the relative benefits of longitudinal data collection.

According to Podsakoff et al. (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, as rival method-based explanations for inferred causal relationships are rendered unlikely. While this approach is conceptually appealing, multi-respondent surveys are quite rare, and occur almost exclusively in studies of large firms (e.g., Anderson and Weitz 1992; Atuahene-Gima 2005; Im and Workman 2004; Ross et al. 1997). Indeed, locating multiple respondents may be quite difficult in small organizations where an owner-entrepreneur is in charge of most decisions (e.g., Ganesan et al. 2005). Multiple respondents may also be untenable for studies of consumers (e.g., Peck and Wiggins 2006) or employees (e.g., Donovan et al. 2004) or for confidential interfirm relationships (e.g., Carson 2007; Hunter and Perrault 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 where decision making is highly centralized and when activities (e.g., interorganizational alliances, key account management) are largely managed by a single individual.

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. This strategy is often recommended by editors and reviewers (Summers 2001; Zinkhan 2006), but seldom employed by survey researchers. A recent example of this approach is Zettelmeter et al. (2006), which investigates the effect of the Internet on automobile pricing by assessing consumer information usage via a cross-sectional survey and automobile prices via a commercial database of automobile transactions.[22] Perhaps one reason why this technique is not more widely employed is that many of the outcomes marketing scholars seek 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 (see Voss et al. 2006 for a recent exception).

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

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[22] For other recent examples of this approach, see Reinhart et al. (2004) and Voss et al. (2006).

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. In order to help survey researchers evaluate the merits of collecting data at a second point in time, we offer eight guidelines based on our conceptual foundation and empirical findings. The first three guidelines focus primarily on CMV, while the next five guidelines are directed towards CI. However, given the interrelated nature of these two validity threats, all of these guidelines hold some degree of relevance for both issues. We recommend that researchers employ these guidelines as a helpful checklist when considering the merits of longitudinal data collection (see Table 7).

1. *Nature of Key Constructs*. As noted earlier, constructs that are relatively concrete, externally-oriented, and verifiable are believed to display lower CMV bias compared to constructs that are abstract, internally-oriented, and non-verifiable (Crampton and Wagner 1994; Jap and Anderson 2004). Our analysis of the alliance and optics datasets supports this view. The two measures in these datasets 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 salesforce management often employ constructs that are concrete and externally-oriented. 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 upon both a survey's measures as well as its informants. For example, measures dealing with sensitive topics such as income (both household and corporate) are more susceptible to socially-desirable response bias compared to measures dealing with 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 from the majority population. For example, in the optics dataset, 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 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. *Measure 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 developing guidelines for how these items should be formatted or scaled. Authors, reviewers, and editors appear to share an informal consensus that a Likert format and a 5 to 7-point scale is the most appropriate means of assessment. In fact, 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 (see Fink 2003 for a detailed listing). 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 optics datasets 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 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, while others suggest that they may also exert long-term effects (Mela, Gupta, and Lehmann 1997). In short, while the start dates of many marketing phenomena may be static, end dates can be quite 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 clearly mark 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 causal inference 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 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 via analytical techniques. While this focus has provided important advances, analysis cannot substitute for theory. As noted by an early statistician, "calculations neglect a very important part of the knowledge which we often possess" (Wright 1921, p. 559).

A well-developed theoretical foundation enhances causal inference 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 et al. 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 as 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*. One potential drawback of longitudinal research is the possibility that the temporal gap between an initial and 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 CEO 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 follow-up surveys. Because top management support is a critical determinant of a firm's market orientation (Jaworski and Kohli 1993), CEO 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 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, salesforce compensation systems may be a relatively closed system and less open to outside influences (Cravens et al. 1993). Hence, longitudinal research should be more 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 causal inference by eliminating alternative explanations (Cook and Campbell 1979; Mill 1843; Popper 1959). In general, longitudinal data is 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.

While unable to incorporate fixed effects over time, the likelihood of alternative explanations can be reduced in cross-sectional surveys via appropriate data collection strategies. For example, many cross-sectional studies attempt to obviate competing explanations by assessing the indicants of these explanations via 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 due to the reporting burden placed on respondents. We encourage researchers to consider this approach as 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, etc.). Nevertheless, when the likelihood of alternative explanations is high and researchers are unable to fully account for them via 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 possessing differing levels of a predictor (e.g., how information usage differs among firms with high vs. low levels of trust). In other words, their conceptual argument

appears to be *between-subject* in 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 is *within-subject* in nature.[23] Of these two types of arguments, longitudinal data collection appears most valuable for the latter, as within-subject comparisons are typically obtained via multiple observations over time.[24] However, most survey-based marketing studies appear to focus on between-subject arguments. Hence, 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 between-subject in nature (e.g., consumer values), while others have more of a within-subject flavor (e.g., relationship life cycle) However, many of our popular theoretical platforms (e.g., trust and commitment, market orientation, transaction cost analysis, and 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 seek 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.[25] Moreover, researchers who are studying arguments that are inherently between-subject in nature should clearly state this fact 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.

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

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

[25] In situations in which longitudinal data collection is infeasible, researchers interested in assessing developmental phenomenon may be able to simulate these effects via 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. They then used 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 about relationship evolution over time.

CONCLUSION

This paper provides a conceptual and empirical examination of the relative merits of cross-sectional vs. longitudinal survey research in terms of reducing the threat of CMV bias and enhancing causal inference. Our conceptual arguments and empirical results suggest that, while 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 is 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 either descriptive in nature or strongly rooted in theory. In contrast, a longitudinal approach appears 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 via a cross-sectional approach. In order to maximize the validity of either approach, academic and business marketing researchers need to employ a combination of strong theory, careful survey design, and appropriate statistical tools.

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TABLE 1

DATA CHARACTERISTICS

	Alliance Study (Rindfleisch and Moorman 2001, 2003)	**Optics Study** (Ganesan, Malter and Rindfleisch 2005)
	Time 1 Survey	
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 VPs ▪ Highly knowledgeable (5.8 on a 7-point scale) ▪ Substantial experience within the firm (Mean = 15 years)	▪ 71% senior managers or above ▪ Highly knowledgeable (6.6 on a 7-point scale) ▪ Substantial experience within the firm (Mean = 10 years)
Non-response 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	
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%
Non-response Bias Assessment	No difference in key measures between Time 2 responders vs. Time 1 non-respondents, and last one-third of Time 2 responders vs. Time 1 respondents	Firms that responded to the Time 2 survey were statistically similar to non-respondent firms
Measure Reliability	α range: .87 to .96	α range: .88 to .92

TABLE 2

CONFIRMATORY FACTOR ANALYSIS FIT INDICES AND MODEL COMPARISON

	Trait-Only Model					**Trait-and-Method Model**					**Model Difference**	
Alliance Study	**χ^2 (df)**	**GFI**	**CFI**	**SRMR**	**RMSEA**	**χ^2 (df)**	**GFI**	**CFI**	**SRMR**	**RMSEA**	**$\Delta\chi^2$ (df)**	**Sig. level**
Relational ties (T1)→ Creativity (T1, T2) & Speed (T1, T2)	509 (289)	.74	.95	.07	.08	452 (262)	.77	.96	.07	.07	58 (27)	.01
Product knowledge (T1)→ Creativity (T1, T2) & Speed (T1, T2)	1194 (242)	.64	.75	.08	.14	1058 (217)	.68	.78	.13	.08	135 (25)	.01
Process knowledge (T1)→ Creativity (T1, T2) & 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) & 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) & 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) & Financial satisfaction (T1, T2)	446 (142)	.74	.91	.11	.12	200 (122)	.85	.98	.08	.06	246 (20)	.01
Optics Study												
Relational ties (T1)→ Creativity (T1, T2) & Speed (T1, T2)	1120 (289)	.68	.80	.10	.12	896 (262)	.73	.84	.08	.11	224 (27)	.01
Product knowledge (T1)→ Creativity (T1, T2) & Speed (T1, T2)	1122 (265)	.70	.78	.08	.12	926 (239)	.74	.83	.07	.11	196 (26)	.01
Process knowledge (T1)→ Creativity (T1, T2) & 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) & 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) & 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) & Financial satisfaction (T1, T2)	734 (142)	.72	.76	.11	.14	577 (124)	.78	.82	.10	.12	157 (18)	.01

Note: The trait model accounts for only the measures while the method model accounts for whether the measures were collected at Time 1 (T1) or Time 2 (T2)

TABLE 3

SUMMARY OF TRAIT, METHOD, AND ERROR VARIANCES ACROSS BOTH DATASETS

	Trait Variance	Method Variance	Error Variance
Dependent Variables			
Creativity (T1)	.71	.17	.12
Creativity (T2)	.74	.12	.14
Speed (T1)	.55	.04	.41
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**

Note: Results are based on combined analyses of all the trait and method models across both the alliance and optics datasets (see Table 2).

TABLE 4

ESTIMATES OF COMMON METHOD BIAS ACROSS BOTH DATASETS

Outcome Variable[1]	Correlation Coefficient (Trait-and-Method Model)[2]	Correlation Coefficient (Trait-only Model)[2]	Common Method Bias[3]
Creativity (T1)	.22	.28	.06
Creativity (T2)	.29	.35	.06
Speed (T1)	.21	.25	.04
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**

[1] The predictor variables in each model were: relational ties, product knowledge, and process knowledge.
[2] Reflects the average correlation coefficients across all predictor variables for this outcome variable.
[3] Difference in correlation coefficients between the trait-and-method model and trait-only model.

TABLE 5

CAUSAL INFERENCE COMPARISON FOR THE ALLIANCE AND OPTICS DATASETS

		Correlation between IV and DV					
Independent Variable (T1)	**Dependent Variable (T1 & T2)**	**Alliance Dataset**			**Optics Dataset**		
		Time 1	**Time 2**	**$\Delta\chi^2(1)$**	**Time 1**	**Time 2**	**$\Delta\chi^2(1)$**
Product Knowledge	Creativity	.37	.38	.01	.36	.44	.72
Process Knowledge	Creativity	.24	.20	.11	.27	.08	4.13*
Relational Ties	Creativity	.30	.75	25.91**	.17	.23	.43
Product Knowledge	Speed	.29	-.03	10.68**	.04	.14	.81
Process Knowledge	Speed	.35	-.01	13.98**	.21	.19	.03
Relational Ties	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

Notes: $^{**}p < 0.01$, $^{*}p < .05$. All correlations above .14 are significant at .05. The chi-square difference represents a model where the correlations between the IV and DVs are freely estimated compared to a model where the correlations are constrained equality.

TABLE 6

MONTE CARLO SIMULATION ANALYSIS OF VARIANCE RESULTS

Source	**Difference in Factor Correlations (Time 1 vs. Time 2) for DV1**			**Difference in Factor Correlations (Time 1 vs. Time 2) for DV2**		
	df	F	p-value	df	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

TABLE 7

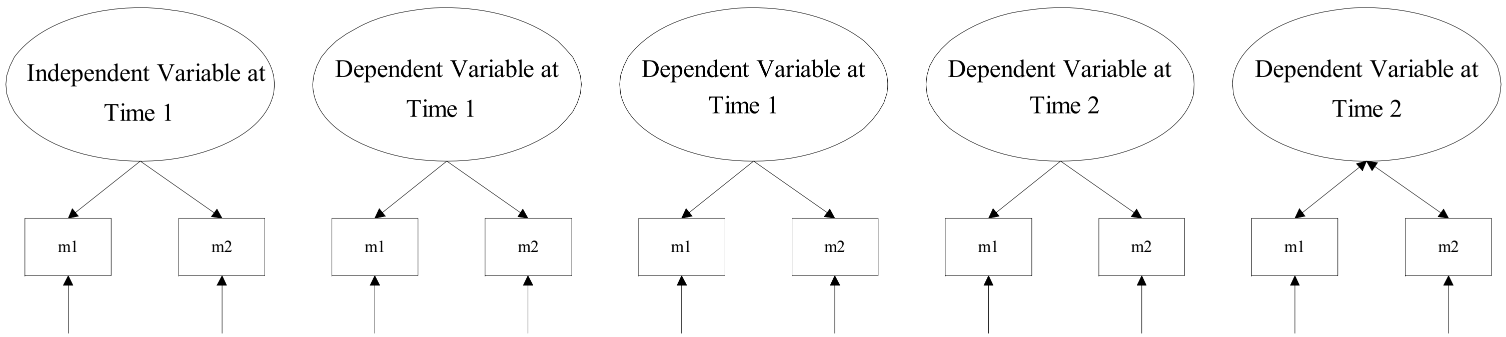
GUIDELINES FOR SELECTING A SURVEY RESEARCH APPROACH

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. Measure format and scales	Heterogeneous	Homogenous
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-subject	Within-subject

FIGURE 1

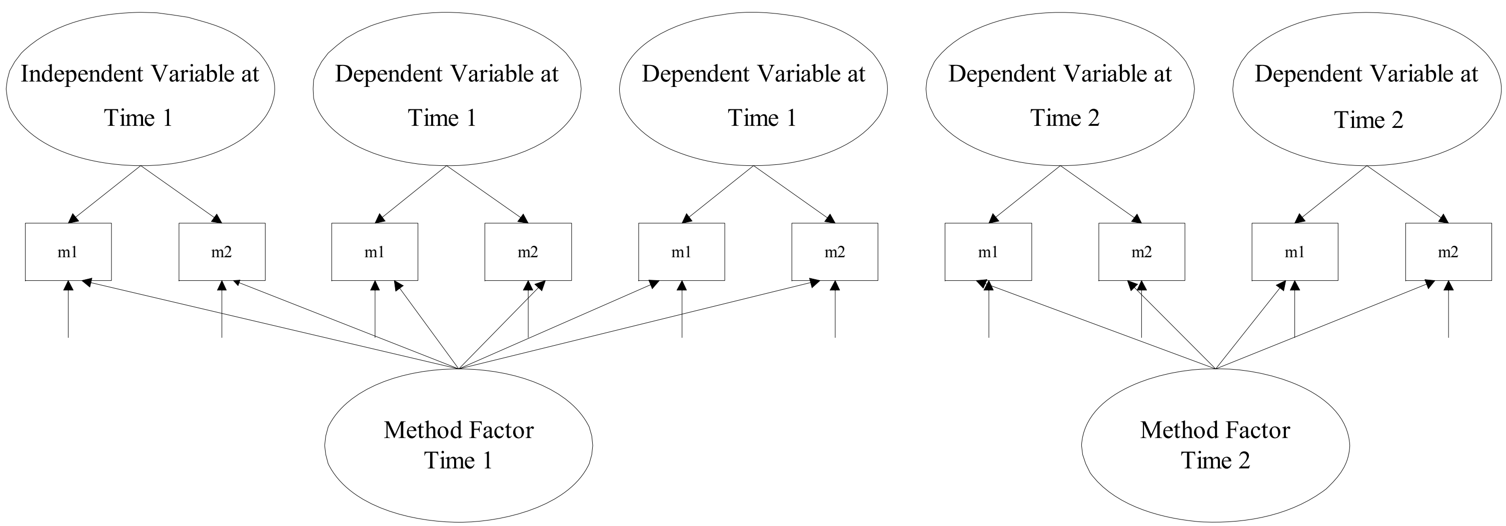
MODEL STRUCTURE FOR COMMON METHOD VARIANCE ASSESSMENT

Trait-Only Model



Note: All trait constructs are correlated. We show only two indicators for illustrative purposes; the actual number of indicators ranges from 3 to 7

Trait-and-Method Model



Note: 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.