Summer 2014

Common method variance: An experimental manipulation

Alison Wall
Louisiana Tech University

COMMON METHOD VARIANCE: AN EXPERIMENTAL MANIPULATION

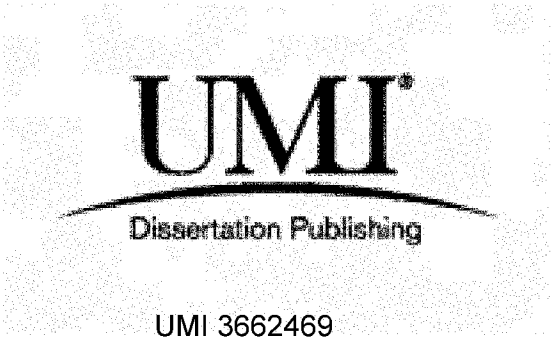
by

Alison Wall, B.B.A., M.B.A.

A Dissertation Presented in Partial Fulfillment
of the Requirements for the Degree of
Doctor of Business Administration

COLLEGE OF BUSINESS
LOUISIANA TECH UNIVERSITY

August 2014



ProQuest LLC
789 East Eisenhower Parkway
P.O. Box 1346
Ann Arbor, MI 48106-1346

LOUISIANA TECH UNIVERSITY

THE GRADUATE SCHOOL

June 27, 2014

Date

We hereby recommend that the dissertation prepared under our supervision by Alison Wall

entitled Common Method Variance: An Experimental Manipulation

be accepted in partial fulfillment of the requirements for the Degree of Doctor of Business Administration

Dr. Marcia Dickerson — Supervisor of Dissertation Research

Dr. Rebecca Bennett — Head of Department

Department of Management

Department

Recommendation concurred in:

Dr. Christie Fuller

Dr. Bryan Fuller

Advisory Committee

Approved:

Director of Graduate Studies Dr. Tim Bisping

Dean of the College Dr. Tim Bisping

Approved:

Dean of the Graduate School

GS Form 13a
(6/07)

ABSTRACT

Although common method variance has been a subject of research concern for over fifty years, its influence on study results is still not well understood. Common method variance concerns are frequently cited as an issue in the publication of self-report data; yet, there is no consensus as to when, or if, common method variance creates bias. This dissertation examines common method variance by approaching it from an experimental standpoint. If groups of respondents can be influenced to vary their answers to survey items based upon the presence or absence of procedural remedies, a better understanding of common method variance can be developed. The results of this study supported that common method variance can be manipulated through research design, but not to the same degree for all variables. Further, not all of the proposed remedies resulted in significant changes in the results. In addition, the CFA marker technique was used to determine the extent of common method variance in the data. The results indicated that, while common method variance existed in the data set, it did not do so at such levels as to bias results. Additionally, the results indicated support for the noncongeneric perspective of common method variance in that all items were affected to differential degrees. Taken as a whole, these findings show that while common method variance exists and can potentially cause variance in data, the bias produced is minimal. Further, the results indicate the remedies that are posited to reduce common method variance may be less effective than previous researchers believed.

APPROVAL FOR SCHOLARLY DISSEMINATION

The author grants to the Prescott Memorial Library of Louisiana Tech University the right to reproduce, by appropriate methods, upon request, any or all portions of this Dissertation. It is understood that "proper request" consists of the agreement, on the part of the requesting party, that said reproduction is for his personal use and that subsequent reproduction will not occur without written approval of the author of this Dissertation. Further, any portions of the Dissertation used in books, papers, and other works must be appropriately referenced to this Dissertation.

Finally, the author of this Dissertation reserves the right to publish freely, in the literature, at any time, any or all portions of this Dissertation.

Author Alison Wall

Date 7-21-2014

DEDICATION

This dissertation is dedicated to all of my friends and family who encouraged and supported me, to my parents who inspired in me a thirst for a deeper understanding of the world, and to my fiancé who stood by me and supported me throughout this process. Without you, the completion of this dissertation would not have been possible.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

ACKNOWLEDGEMENTS

This dissertation would not have been possible without the direction, support, and guidance of my committee. I would like to thank my chairperson, Dr. Marcia Dickerson for her time, support, encouragement, and generosity throughout this process. I greatly appreciate your investment in me, your faith in my potential, and your willingness to work with me. I would also like to thank my other committee members, Dr. Christie Fuller and Dr. Bryan Fuller, for your support, advice, and guidance in the completion of my studies. In addition, I would like to thank the other members of the Louisiana Tech faculty and staff who inspired, taught, supported, and guided me throughout my time as a doctoral student. I have learned so much throughout this process and I will be forever grateful for each of your roles in helping me reach this achievement.

I would like to thank my friends and family who offered support, advice, and encouragement. Specifically, I would like to thank my parents, Katharine and Jerry, for their love and support throughout my academic years. To my brothers, Christopher and Stephan, thank you for inspiring, supporting, and motivating me. To my future stepdaughters, Lauren and Lindsay, thank you for your support, encouragement, and patience while I completed my degree.

Finally, I would like to thank Jerome Walker, my fiancé, my partner, my best friend. You showed me unending patience and support and encouraged me when I needed it most.

CHAPTER ONE

INTRODUCTION

In social science research, researchers are reliant upon participants to provide the most accurate response possible. Few tools exist that offer the convenience and breadth of information as self-report questionnaires. In some cases, self-reports may be the only viable tool for data collection. However, as convenient or indispensable as any tool may be, there are always flaws or potential drawbacks to its use. With all measuring devices, the features of the device are intended to measure the variance of the trait or construct in question. There are also features of the device that are characteristic of the method itself, which are unrelated to the trait being measured. Systematic variance in the relationships can occur due to either group of features. Depending on both the nature and extent of the variance, there exists the potential that the scores rendered from the measurement method can become invalid and create biased or incorrect conclusions (Campbell & Fiske, 1959). This phenomenon is referred to as method variance or method bias (e.g. Podsakoff & Organ, 1986; Podsakoff, MacKenzie, Lee, & Podsakoff, 2003; Campbell & Fiske, 1959). Method variance is a topic of great interest and discussion in management research. The topic of method variance is not yet fully understood and little substantive theory has been developed to explain exactly how or why method variance occurs. Because there are multiple schools of thought and limited empirical studies regarding CMV effects, no consensus has been reached.

The concept of method variance is grounded in classical test theory, which proposes that every measured variable is comprised of three major components of variance (Pedhauzer & Schmelkin, 1991). The first component is trait variance, which is the construct the researcher is genuinely looking to measure. The second component is method variance or systematic error due to the measurement method. The third component is error variance, which is the random error of measurement and/or nonsystematic influences on measured variables. Classical test theory is also referred to as the true-score model and is based upon the idea that an observed score is comprised of two components: a true score and an error score. The true score is that which would exist in perfect conditions. Conceptually, the true score never changes and the differences found from one study to another are the result of error. Method variance is the component of the error score that can be attributed to the manner in which the score is obtained. For reliable and valid measurement, researchers want to maximize the measurement of trait variance and minimize the measurement of method and error variance. The potential for method variance can be exacerbated in situations in which the relationships in question involve comparing more than one measure collected through the same method. This is most notably of concern in studies based solely on self-reported data (Chan, 2009; Campbell & Fiske, 1959). The basic premise of the concern is that, in addition to the true relationships, there also exists a compounded spurious relationship that is relative to the measures or methods of measurement themselves or common method variance (Kline, Sulsky, & Rever-Moriyama, 2000; Podsakoff et al., 2003).

Common method variance (CMV) has been defined as systematic error variance due to rater response styles, item characteristics, and aspects of measurement that can

threaten the validity of study findings when measures are collected using the same or similar methods (Podsakoff et al., 2003; Podsakoff, MacKenzie, & Podsakoff, 2012). The primary concern of using the same or similar measurement methods for multiple constructs is that you may inadvertently introduce biases or systematic variances that are inherent to the measurement method itself, rather than the actual relationships being investigated (Podsakoff et al., 2012), thus potentially biasing or invalidating any observed relationships. In addition, because this method variance can either inflate or deflate observed relationships between constructs, it can lead to both Type I and Type II errors (Podsakoff et al., 2003). This makes it more difficult to recognize when and if it has caused a significant change in the results. These characteristics make understanding the nature of method variance that much more important, especially when one considers the popularity and, in some cases necessity, of using self-report data.

As such, CMV has been a topic of concern for management researchers for many years; however, debate continues about its nature and likelihood of occurrence. While there are many studies aimed at detecting the level of influence CMV has on both the measures used in the field and the relationships among those measures (Podsakoff et al., 2003), there has been little consensus on this level, and multiple competing perspectives have developed. Because of this, CMV has been referred to as everything from a myth or an urban legend (Spector, 2006) to a "specter that has the potential to haunt interpretations of observed relationships" (Johnson, Rosen, & Djurdjevic, 2011)to a "search for a black cat in a dark room" (Castille & Simmering, 2013). Some argue that CMV is pervasive in most same-source data and is one of the main sources of measurement error that threatens the validity of researcher conclusions (Podsakoff &

Organ, 1986; Podsakoff, Mackenzie et al., 2003; Podsakoff et al., 2012; Bagozzi & Yi, 1990; Cote & Buckley, 1987; Siemsen, Roth, & Oliveira, 2010). Others adopt a no-CMV perspective and contend that, if CMV does exist, it does so at such low levels that it does not bias results (Spector, 1987; Spector, 2006; Brannick, Chan, Conway, Lance, & Spector, 2010; Castille & Simmering, 2013; Richardson, Simmering, & Sturman, 2009).

CMV is a source of error that may be unknown even to the respondent (Kline et al., 2000; Moorman & Podsakoff, 1992), thus it cannot be directly seen or measured; therefore, its influence on research parameters must be inferred methodologically. Unfortunately, as methodological inferences are undoubtedly influenced by the researcher beliefs, it becomes all the more difficult to understand when CMV influences occur. In those studies that find evidence for it, or lack thereof, it is unclear whether the findings are actually linked to CMV or some other unmeasured substantive variable. A number of statistical methods to determine CMV's influence on substantive relationships have been proposed and some have been empirically supported (Richardson et al., 2009; Lindell & Whitney, 2001).

Though CMV cannot be directly or easily measured, researchers have identified several potential sources for its occurrence. Podsakoff et al. (2003) identified four categories of method effects: those produced by a common source or rater, those produced by item characteristics, those produced by item context, and those produced by measurement context (Podsakoff et al., 2003; Podsakoff et al., 2012). Common source or rater effects include consistency motif that arises from the desire of the researcher or biased questions. Implicit theories and illusory correlations are based upon assumptions of raters concerning the co-occurrence of rated items and their interrelatedness. Social

desirability is the propensity of respondents to select responses in line with what society values. Leniency biases exist when respondents rate those they know or like more highly than they rate others. Acquiescence bias is the tendency to agree with the attitude of the item, regardless of content. Positive/negative affect is the general feeling of the respondent's self-concept and how he/she views himself, either positively or negatively. Transient mood state involves the daily changes each person feels in response to the day's events. Method effects produced by item characteristics include item social desirability, item complexity/ambiguity, scale formats and scale anchors, and negatively worded or reverse coded items. Item social desirability is present when items are worded such that a socially desirable response can be inferred. Item complexity/ambiguity influences items that are too difficult, confusing, or open to interpretation. Scale formats and scale anchors effects occur through measuring different constructs with the same or similar scales. Negatively worded or reverse coded items effects create cognitive speed bumps to trigger controlled, rather than automatic, responses. Method effects produced by item context include item priming, item embeddedness, context-induced mood, scale length, and intermixing items of different constructs on the questionnaire. Item priming effects occur from asking questions an order that shapes responses. Item embeddedness effects result when surrounding questions frame item responses. Context-induced mood effects occur because question wording can induce certain moods or responses. Scale length effects occur because shorter scales increase the likelihood that respondents will consider previous answers in their responses. Intermixing items of different constructs on the questionnaire is intended to make it more difficult to distinguish between constructs, but increases cognitive speed bumps. Lastly, method effects produced by the

measurement context include time and location of the measurement and the use of a common medium to obtain measurements. The time and location of the measurement increase or decreases the likelihood that previous responses are salient. The use of a common medium to obtain measurements is potentially biasing because certain means of measure are more/less prone to certain biases.

In addition to the sources of influence for CMV, there is also the question of when CMV is likely to be a legitimate cause for concern (Chan, 2009; Lance, Baranik, Lau, & Scharlau, 2009). Most researchers agree that CMV is a potential issue for any variable that either has an attitudinal component or is susceptible to social desirability, impression management, personality, or affect (Kline et al., 2000; Johnson et al., 2011). Unfortunately, these types of variables may be best or only measurable through self-reports as others can only speculate or use proxies to evaluate another person's attitudes. Some common variables that are considered prone to CMV include job satisfaction, core self-evaluations, organizational commitment, leader-member exchange, proactive personality, deviance, and role perceptions (Richardson et al., 2009; Biderman, Nguyen, Cunningham, & Ghorbani, 2011; Williams & Anderson, 1994; Johnson et al., 2011). In fact, it may not be possible for some variables, such as core-self evaluations (Chang, Ferris, Johnson, Rosen, & Tan, 2012; Johnson et al., 2011), to be measured through other methods and procedural remedies, such temporal or proximal separations, may not be appropriate or feasible (Johnson et al., 2011). While most researchers agree that CMV is an issue that needs consideration in light of study design and the constructs being examined, there is disagreement as to how prevalent CMV is and how it influences research results.

When one assumes that CMV exists in all self-reported data to biasing extents, (s)he may take the perspective that studies that do not include alternate measures are invalid and/or not fit for publication (Campbell, 1982; Kline et al., 2000). This perspective may result in the elimination of potentially valid studies from publication when they may not be influenced by CMV. Alternatively, this perspective may lead to researchers concluding that differences in self/other reports are always attributed to method effects rather than addressing the potential that the differences could be due to measuring legitimately different constructs. Chan (2009) points out that if one automatically assumes the existence of CMV, they may refuse to publish or submit legitimate findings or they may make unnecessary apologies and/or corrections when CMV is not an issue. For example, if a researcher is collecting data on employee behaviors, the employee may be accurately representing what they do, whereas the other report may be reporting only behaviors that are salient and/or valued by the organization. Alternatively, you may assume that CMV is the cause for variation when in reality it is due to other issues, such as model misspecification, and unintentionally discount legitimate trait variance or fail to examine other theoretically related variables. Such is the case with social desirability and positive/negative affect, both of which may be considered CMV or they may be theoretically relevant to the construct in question. For example, when one is investigating personality, it is possible that positive/negative affect or social desirability will influence a person's worldview and interactions with others thus, skewing their actual perceptions and behaviors (Kline et al., 2000). In other words, it is possible that people who score highly on social desirability will more frequently engage in desired behaviors, such as organizational citizenship or voice, and/or people

who are high in positive affect may genuinely feel more satisfied with their jobs and/or their interactions with organizational leaders. To view either as purely causes of method variance could result in a misspecification error and prevent examination of the substantive relationships.

On the other hand, if a researcher assumes that CMV exists and needs to be addressed, then (s)he will adopt one of two competing perspectives: congeneric and non-congeneric. The congeneric perspective of CMV holds that CMV is likely to exist, but the effects are not equivalent across all measures and are expected to vary across raters, items, constructs, and contexts (Richardson et al., 2009). In other words, method constructs will interact differentially with substantive variables and constructs. Support for this perspective is evident in Biderman et al.'s (2011) attempt to further the understanding of method influences in personality literature. They examined the "ideal employee" factor and found that a model that included three separate method factors was a better fit than a model with only one method factor. Their results supported a single factor that affects all variables, a factor that affects only negatively worded items, and a factor that affects only positively worded items. The noncongeneric perspective of CMV holds that CMV is likely to exist and influence all items in the same way and to the same degree. In other words, a single method factor should emerge that can account for any variance and there is a constant correlation between the method factor and the variables (Richardson et al. 2009; Lindell & Whitney, 2001). Support for this perspective is found in Schmit & Ryan's (1993) work, which found an "ideal employee factor." In their study, a six-factor model fit the data better than the original five-factor model and the sixth factor, the "ideal employee" factor accounted for method variance.

An alternative third perspective is No CMV. When one assumes that CMV does not exist, or does not do so at a biasing level, it is believed that the method alone is insufficient to produce a significant bias and all constructs share the same biases when measured with the same methods (Richardson et al., 2009; Spector, 1987; Spector, 2006). Most researchers who adopt this view do so as a means for countering concerns that CMV may provide an alternate explanation for their findings. The general idea behind this perspective is that if CMV is prevalent, then all self-report data should be correlated. As that is not the case, the use of self-reported data should not itself indicate problems with CMV (Spector, 2006). The concern with this approach is some studies have estimated that anywhere from 18 – 32 % of variance may be due to method factors (Podsakoff et al., 2012). If one assumes that CMV is non-biasing or non-existent and does not test for it or consider it in study design, the question as to whether CMV can be an alternate explanation cannot be ruled out. This perspective is evident in some of the research on personality. Many researchers believed the Big Five personality factors (Costa & McCrae, 1988) to be faulty because they are highly correlated with each other and researchers have found they often converge into fewer factors, not taking the potential for method biases into account. When management researchers began using confirmatory factor analysis in the 1990s to evaluate and study method bias in student versus job applicant settings, they found that a sixth method factor, the "ideal employee factor," which consisted of a combination of respondent faking and socially desirable responding, emerged (Schmit & Ryan, 1993; Biderman et al., 2011). Had they assumed the no CMV perspective, researchers may have discontinued use of a potentially useful measure.

The three divergent perspectives of CMV highlight the need for further research and understanding of the phenomenon. The uncertainties as to the perspectives, nature, importance, and potential biasing influence of CMV are prevalent in recent published research. In spite of the conflicting viewpoints as to the validity of self-report data, a recent review found that a large amount of published literature in marketing, psychology, organizational behavior, education, management information systems, international business, personality, and individual differences involved only a single measurement method typically self-reports (e.g. Podsakoff et al., 2012; Rindfleisch, Malter, Ganesan, & Moorman, 2008; Chan, 2009; Chang, van Witteloostujin, & Eden, 2010; Teo, 2011). In addition, almost half of the studies in *Academy of Management Journal* and *Journal of Applied Psychology* in 2007 mentioned CMV, though it typically was mentioned as an avoided limitation rather than an active assessment as to its impact on findings (Richardson et al., 2009). If most researchers are simply citing journal articles that support the no CMV perspective rather than implementing procedural or statistical remedies, there exists minimal opportunity to evaluate the true nature and existence of CMV and when it is a problem. When authors find evidence for CMV, most do not provide discussion as to how or why the CMV occurred or was expected to occur, e.g. congeneric versus noncongeneric. Further, if study results are biased by CMV and those biases are not addressed, any subsequent meta-analyses or studies will be biased as well, thus exacerbating the potential problems. Authors who have directly addressed CMV in their studies through either statistical or procedural remedies have found that the influence of CMV varies based upon the construct in question. Some researchers have found no significant influence and others have found as much as a third of the variance

was due to CMV effects (Johnson et al., 2011). This finding provides additional support for why many researchers either disagree as to the prevalence and influence of CMV or are unsure as to when and how it may truly bias results.

Statement of the Problem

There has been much concern and discussion over the existence of CMV. Some researchers have found evidence that it exists and substantially influences results; while others have found no evidence of CMV or at least that, it has no substantial impact on results (e.g. Spector, 1987; Podsakoff et al., 2012; Cote & Buckley, 1987; Campbell & Fiske, 1959). This dissertation posits that if CMV exists, then its influence is something that a researcher can manipulate. While this theory is not wholly new, little research thus far has specifically set out to determine whether research results and CMV can be altered through the research setting. Authors have recommended procedural changes to reduce CMV effects (e.g. Podsakoff et al., 2003), but very little research has explicitly tested whether the precautions actually have the desired effect of reducing or eliminating CMV (Castille & Simmering, 2013; Rindfleisch et al., 2008; Malhotra, Kim, & Patil, 2006).

This study proposes to examine the nature of CMV by approaching it from an experimental standpoint. In research, variables can be measured or manipulated, and in this study, the manipulation of the presumed causes of CMV may elucidate the nature of this source of error. The empirical literature on faking in selection tests is used as a guide to improve understanding of how one might detect systematic error in responses. In faking research, a random set of respondents taking a personality survey are either motivated or asked to "fake good" or to present their best selves in responses, while a comparable random set of respondents is asked to respond normally (e.g. Bing,

Kluemper, Davison, Taylor, & Novicevic, 2011; Schmit & Ryan, 1993; Paulhus, Harms, Bruce, & Lysy, 2003; Steenkamp, De Jong, & Baumgartner, 2010; Robie, 2006; Smith & Robie, 2004; Chan, 2009). A comparison of the two randomly selected samples informs researchers as to the influence of faking.

In this dissertation, two presumed causes of CMV are studied: implicit theories and demand characteristics (Podsakoff et al., 2003). The notion of implicit theories indicates that respondents make logical connections among survey scales and therefore answer more consistently than two different respondents would, thus inflating substantive relationships (Baumgartner & Steenkamp, 2001; Meade, Watson, & Kroustalis, 2007; Salancik & Pfeffer, 1977; Salancik, 1984). Demand characteristics are present when respondents feel that they must answer items in a particular way due to researcher demand or the item wording. These responses are more likely to be overall positive and consistent with researcher expectations (e.g. Kline et al., 2000; Biderman et al., 2011; Schmit & Ryan, 1993; Robie, 2006; Podsakoff et al., 2003).

If groups of respondents can be influenced to answer normally, such that they use strong implicit theories, and other groups can be influenced against the use of implicit theories, then data from these groups can be compared to determine the level of correlation among substantive variables and the amount of CMV present. Similarly, respondents can be influenced by demand characteristics.

Finally, although CMV cannot be measured directly, empirical research indicates that a post hoc statistical technique called the Confirmatory Factor Analysis Marker Variable Technique, can be used to detect CMV accurately (Richardson et al., 2009; Williams et al., 2010). This technique makes use of a marker variable, which is a

carefully chosen proxy for CMV that is included with substantive variables on a same-source survey. Thus, in addition to examining differences in relations among variables between experimental and control groups, a marker variable will be included to estimate the amount of CMV present in the data.

Research Questions

1. Can CMV be manipulated (either magnified or attenuated) through the research setting (e.g. by certain instructions)?
    a. Psychological separation of scales/variables relatedness
    b. Proximal separation of scales/variables
    c. Presentation of survey instructions (video versus written)
2. To which measured causes of CMV are the scales most susceptible?
    a. Affect
    b. Social Desirability

Contributions of the Study

The contributions of this dissertation are three-fold. First, this study will examine the viability of recommended procedural efforts to reduce CMV in academic research. This will allow researchers to have a clearer idea of which efforts are the most likely to result in support for the validity of their data. Second, this study will show whether the research setting can create, magnify, or attenuate CMV effects through research settings and to what extent certain manipulations influence study results. This will allow researchers a better understanding of how and when CMV influences study results and whether research design can influence its occurrence. Finally, the use of a properly

chosen and analyzed marker variable to detect CMV in data will provide further evidence of both the efficacy of procedural efforts to reduce CMV and marker techniques to detect CMV. The use of marker variables as a proxy for detecting CMV aids researchers in that they allow for additional evidence as to the validity of results. It is anticipated that this dissertation will provide additional insight and practical implications into how CMV functions and the viability of procedural remedies aimed at reducing its occurrence.

Plan of Study

The remainder of this dissertation will be organized as follows. In Chapter Two, a review of CMV literature will be provided. Chapter Three will include a discussion of the proposed statistical methods and procedures to be used for the empirical study. A plan for the experimental manipulations will be provided. In Chapter Four, the findings of the empirical study will be presented. Finally, in Chapter Five, the practical implications and insights into areas where future research may be directed will be discussed.

CHAPTER TWO

LITERATURE REVIEW AND HYPOTHESES

This chapter reviews the literature on CMV, the effects of CMV on research outcomes, and proposed remedies for CMV. Although it has been over fifty years since the publication of the original Campbell & Fiske (1959) article that brought CMV concerns to light by suggesting that researchers implement multi-trait multi-method research designs to improve accuracy, a full empirical and theoretical understanding of CMV has yet to be developed. The resurgence of interest in CMV research stems mainly from an editorial by the 1982 outgoing editor of the *Journal of Applied Psychology*. In this editorial, he stated that he believed one of the remaining acceptable biases of reviewers and editors was that which is directed towards studies in which no variables are measured independent of a self-report questionnaire and any study that uses only self-report data should be rejected because it contributes little value (Campbell, 1982). While many researchers agree with his assessment, there are almost as many who disagree and feel that self-report studies are not fatally flawed and can potentially contribute to theoretical development (e.g. Brannick et al., 2010; Cote & Buckley, 1988; Castille & Simmering, 2013; Lance et al., 2009; Conway & Lance, 2010). Several schools of thought have emerged and procedural and statistical remedies have been developed, but there has yet to evolve a consensus and true understanding of CMV and its effects on research outcomes. Many researchers have examined the potential biasing

effects of measuring multiple constructs with the same measurement method. However, the vast majority of the research has focused on social desirability or the proposition of remedies with little empirical investigation as to their efficacy (e.g. Baltes, Bauer, Bajdo, & Parker, 2002; Meade et al., 2007; Kline et al., 2000; Podsakoff et al., 2003; Spector, 2006; Conway & Lance, 2010). The lack of empirical research has led to misconceptions and confusion as to what researchers should do and what reviewers and editors should expect (e.g. Conway & Lance, 2010; Ashkanasy, 2008; Chang et al., 2010).

Definition of CMV

To highlight the controversy with CMV, a logical starting point is with the definition itself. As previously mentioned, all measurements contain three types of variance: trait, method, and error. The general intent of research design is to maximize detection of trait variance and minimize both method and error variances. The first key to understanding what constitutes method variance is to understand what is meant by the term "method." Most researchers agree that the term "method" refers broadly to several aspects of the measurement process, including item structure or wording, location, survey format, response format (e.g. Podsakoff et al., 2003; Podsakoff et al., 2012; Baumgartner & Steenkamp, 2001; Campbell & Fiske, 1959). Some researchers disagree and believe that "method" should be defined more narrowly to only include measurement facets such as item content, structure, or format that may elicit similar responses and exclude such effects as response tendencies and both item and measurement contextual factors (e.g. Podsakoff et al., 2012; Lance et al., 2009; Bagozzi & Yi, 1990). Further, other researchers question whether rating sources constitute different methods for measuring the same traits (Conway & Lance, 2010). The only consensus is that measurement

method is composed of a combination of three main elements including the rater, the instrument, and the procedure (Burton-Jones, 2009). A rater is the individual who is providing a score or completing a survey. An instrument is the device used to obtain the score, such as a survey and the items and presentation of items on the survey. A procedure is the process through which the instrument is used to obtain a single score or multiple scores. For the purpose of this dissertation, the more broad definition in that any of the decisions relating to these three elements can potentially be considered "method" is used. The purpose of this is two-fold. First, the more broad definition allows for the consideration of more factors as potential biasing sources. Second, whether or not one believes contextual factors should be included as "method" factors, one must still account for the potential biases or errors such factors may produce (Podsakoff et al., 2012). It is important to acknowledge that what is considered method effects for one study may be valid theoretical trait variance for another (e.g. Conway & Lance, 2010; Lance et al., 2009; Chan, 2009; Chen & Spector, 1991).

Now that the term method is defined, attention can be given to the question of what constitutes method variance. The basic premise of method variance is that, in addition to the true trait variances observed, there also exists a spurious relationship that is relative to the measures or methods of measurement themselves (Kline et al., 2000; Podsakoff et al., 2003). Thus, method variance (MV) can been defined as systematic error variance that results from rater response styles, item characteristics, and aspects of measurement that can threaten the validity of study findings (Podsakoff et al., 2003; Podsakoff et al., 2012). In other words, MV is any variance that can be attributed to the method of measurement rather than the construct in interest (Bagozzi & Yi, 1991). It is

important to note, that while self-reports are typically targeted as the most rife with issues, they are not the only methods with potential for variance. Each measurement method has its own potential sources of variance, which may or may not be unique to that particular method and method variance is not unique to quantitative research (Conway & Lance, 2010; Burton-Jones, 2009). If all measures are collected using the same or similar methods or unique methods with the same or similar sources of variance, you may have CMV, which has the potential to influence or bias results (Spector & Brannick, 2009). CMV is defined as "systematic error variance shared among variables measured with and introduced as a function of the same method or source" (Richardson, Simmering, & Sturman, 2009). An average of variances found in studies using MTMM (multi-trait multi-method) matrices indicates that anywhere from 18% - 32% of variance was due to method factors (Podsakoff et al., 2012). One issue with the use of MTMM methods is that the trait and method factors can become confounded and may be the result of the emergence of a general factor across methods rather than a true relationship among the methods used (Lance et al., 2010).

Common Method Variance (CMV) versus Common Method Bias (CMB)

Researchers are concerned that method factors can significantly bias estimates of construct validity, reliability, and parameter estimates. The degree to which these biases occur is referred to as method bias (Podsakoff et al., 2012). When multiple measures are influenced by the same or similar method factors (i.e. CMV is present), any statistically significant relationships found may be the result of common method bias (CMB) rather than systematic trait variance. It is important to note that method bias and common method bias are two unique concepts. Method bias is that which exists in any individual

measurement method and is defined as the "difference between the measured score of a trait and the trait score that stems from the rater, instrument, and/or procedure used" (Burton-Jones, 2009). Common method bias (CMB) is the "difference between the trait score and measured score that is due to the use of a common method to take more than one measurement of the same or different traits" (Podsakoff et al., 2003). CMV is only an issue to the extent that it produces CMB; thus, the real question is not whether CMV is statistically significant, but whether CMB is large (Meade et al., 2007).

Two negative effects of CMB may result from the presence of CMV (Podsakoff et al., 2012). The first is that its presence may bias construct reliability and validity estimates that can lead a researcher to believe that a scale accurately reflects a latent measure when it does not. This may be evident in scales that elicit lower reliabilities when applied to different populations or subsequent studies in that method bias may have occurred in one of the studies that influenced the reliability of the scale. This type of error can also lead to error in any subsequent meta-analyses that are conducted. The second effect of CMB is that it can bias parameter and interval estimates between constructs (Cote & Buckley, 1987; Bagozzi & Yi, 1990; Baumgartner & Steenkamp, 2001) affecting hypotheses by leading to Type I or Type II errors and incorrect perceptions of how much variance is accounted for by a given model. The concern is that method variance may be inseparable from systematic trait variance and may lead to erroneous conclusions if the variance exists at biasing levels (Podsakoff et al., 2012).

If, when, and how CMV leads to CMB, and the corresponding improper or erroneous conclusions that may result is at the heart of the CMV controversy. Study results have provided evidence for multiple perspectives and theories. Researchers are

still attempting to understand when and how CMV leads to common method bias and to what degree it biases (Crampton & Wagner, 1994; Spector, 2006; Spector & Brannick, 2009). In other words, if CMV exists, then it may not do so at such levels that truly bias results. Therefore, the automatic rejection or discounting of studies that employ a common method could be erroneous and prevent sound theoretical and empirical conclusions from being published. Alternatively, the automatic assumption that multiple methods (e.g. self/other reports) and the lower correlations they tend to produce are the result of reduced CMV and CMB may also be erroneous. The score may actually measure two different constructs and each method or source has its own unique sources of variance, which can result in different conclusions.

Typically, CMV is thought to cause upward bias or inflation of relationships. Thus, measures with CMV are expected to exhibit a stronger observed relationship than the true relationship (Doty & Glick, 1998; Podsakoff & Todor, 1985; Brannick et al., 2010; Organ & Ryan, 1995). If this is the case, the presence of CMV and CMB is most likely to result in a Type I error in which one falsely accepts that a true relationship exists. However, this is not always the case as CMV is just as likely to cause downward bias or deflation of relationships (e.g. Kline et al., 2000; Lance et al., 2009; Conway & Lance, 2010; Lance, Dawson, Birkelback, & Hoffman, 2010; Meade et al., 2007). Conway and Lance (2010) used classical test theory (Lord & Novick, 1968) to provide a mathematical explanation as to how it is just as likely that results are attenuated because the unreliability of the single method simultaneously works against the mono-method inflation and produces results that may be inflated, attenuated, or equal. The same is true if the method effects of a multi-method study are correlated. They further demonstrate

that if the multiple methods selected have uncorrelated sources of method variance, the results will usually be downwardly biased to account for multiple method factors and the error variance that is inherent within each method. This type of bias is similar to a Type II error in which you falsely reject an alternative hypothesis in favor of the null or no relationship. It is also possible that CMV can have no effect because the method variance is attenuated by the error variance to such a degree that they cancel each other's influence. In addition to the interaction of method variance and error variance, another issue is whether the method factor(s) affect all variables in a measure or construct in the same way or differently.

Two main perspectives as to how CMV affects variables or measures exist: a congeneric perspective and a non-congeneric perspective (Richardson et al., 2009). The congeneric CMV perspective assumes that not all method effects are the same and that data can potentially be subject to more than one form of method variance. Researchers using this perspective believe that method effects will vary with the rater, item, construct, and/or context of measurement and that one or more method constructs may correlate differentially with individual items and constructs. When not explicitly stated, this perspective is implicit in all studies that consider one or more method effects, such as self/other reports, social desirability, item wording and positive/negative affect (e.g. Williams & Anderson, 1994; Cote & Buckley, 1987; Biderman et al., 2011). The non-congeneric CMV perspective assumes that all items are affected the same way by a single source or method factor. While most researchers do not explicitly state this perspective, it is evident in research that addresses a single general method factor, like the aforementioned "ideal employee factor" (Schmit & Ryan, 1993) or the "ideal leader

schema" (Judge & Cable, 2004). The third CMV perspective is that it does not exist or does not exist at biasing levels. Typically, this perspective is only evident in a paper's limitations section, when CMV is referred to as something that was overcome by study design. Most prevalently, the works of Spector are cited as a defense to the no CMV perspective (Spector, 1987; Spector, 2006); however, other researchers have also found support for this perspective (Lance et al., 2010; Crampton & Wagner, 1994). If researchers assume that CMV does not exist or make incorrect assumptions about its influence, they run the risk of rejecting good theories and supporting bad ones or basing conclusions on biased data (Burton-Jones, 2009).

CMV Research

Given the belief that CMV could potentially bias data and results, most authors will have to address it at some point regardless of their perspective (Conway & Lance, 2010). Several reviewers have stated that CMV concerns have either led them to reject a manuscript or led to their manuscript being rejected for publication (Pace, 2009). For example, in submissions to the *Journal of Organizational Behavior (JOB)* and *Journal of International Business Studies (JIBS)*, authors who use survey data are required to address potential CMV threats to validity for publication (Ashkanasy, 2008; Chang et al., 2010). Thus, CMV must be addressed at the research design stage and researchers must determine how and when their research needs to address it (Pace, 2009). There are three main questions and concerns that authors, reviewers, editors, and dissertation chairs and other "gatekeepers" have regarding the CMV/CMB debate and why it should be addressed (Conway & Lance, 2010). The first is whether false results are being published due to artificial inflation. The second is whether valid results are being

dismissed due to unwarranted CMB concerns. The third is that faulty statistical or procedural techniques are being applied which result in false results, false confidence in those results, or false beliefs concerning the existence and influence of CMV/CMB.

Pace (2009) conducted a survey of editorial board members of the *Journal of Applied Psychology* (*JAP*), the *Journal of Organizational Behavior* (*JOB*), and the *Journal of Management* (*JOM*) to gain a better understanding of reviewer perceptions of CMV. She found that 86.5% of reviewers believed that CMV meant that all variables measured with the same method would be inflated due to the method chosen; however, only 48.7% agreed that CMV created difficulty in drawing firm conclusions about results. Though the terminology was not used, most participants (82.9%) adopted the congeneric CMV perspective by agreeing that CMV affects some variables more than it affects others. She also found support for the idea that self-reports are particularly targeted with 56.7% of respondents stating that self-reports were more likely to have issues with CMV than other methods. However, research has failed to demonstrate that suspected sources of bias in self-report data, such as social desirability, affect, and acquiescence have either consistent or strong effects on results (e.g. Williams & Anderson, 1994; Kline et al., 2000; Castille & Simmering, 2013; Chan, 2009). With the possible exceptions of longitudinal data, complex modeling with a large number of variables, and non-linear modeling, research has not shown that other types of data collection are less prone to CMV (Conway & Lance, 2010; Podsakoff et al., 2012; Baltes et al., 2002; Siemsen et al., 2010; Rindfleisch et al., 2008). Overwhelmingly, participants believed that the problem of CMV was due mainly to research design and needed to be addressed at that stage. The general belief is that CMV can be a problem, some variables are affected more than others (congeneric

perspective) and a focus solely on the method of measurement is insufficient. The conclusion is that researchers need to move beyond the speculation of when and how CMV might exist and begin systematically testing and measuring its effects.

Potential Sources of CMV

In considering when CMV effects may be observed, researchers must consider all potential sources of CMV. As previously mentioned, there are four main categories of common method effects: rater, item characteristics, item context, and measurement context. Within each category, there are multiple potential sources of variance and bias (Podsakoff et al., 2003).

Method Effects Produced By a Common Source or Rater

CMV produced by a common source or rater is the most frequently cited effect. This type of method effect occurs from any covariance between the predictor and criterion that results from the same respondent providing both scores (Podsakoff et al., 2003). While there have been several meta-analytic studies that have found support for common rater bias, there is some debate as to whether rating source variations constitute CMV (Conway & Lance, 2010). The argument that common source or rater may not be a source of CMV/CMB is that for it to be CMV one must assume that different rating sources represent the same variable(s). Over several studies using confirmatory factor analysis (CFA), Hoffman, Lance, and colleagues (Conway & Lance, 2010) found that rater source factors represented alternative, complementary differences in job performance. In other words, these differences represented trait or substantive variance, rather than method variance. Some measures that are likely to have common source

biases include the following: leader behaviors and outcome variables such as job performance and leader efficacy, job performance and personality variables, attitudes, organizational citizenship behaviors (OCBs) organizational commitment, person-organization fit, participative decision making and work outcomes, and OCBs and performance evaluations (Podsakoff et al., 2003). Common rater effects emerge due to response styles of the respondent and the respondent's interactions with the study or measurement method.

The first type of common rater or source effects is consistency motif. Bias that stems from consistency motif is that which exists due to the desire of people to maintain consistency between their thoughts and their attitudes and is produced because people search for similarities in the questions and attempt to respond in the same or similar ways to multiple questions (Podsakoff et al., 2003; Salancik & Pfeffer, 1977; Podsakoff & Organ, 1986). Consistency motif is likely to be of the greatest concern in those studies that address attitudes, perceptions, or behaviors.

A related method effect to the consistency motif is implicit theories and illusory correlations. This effect occurs when respondents distort their scores based on either their co-occurrence in the study, i.e. illusory correlations, or their personal beliefs, i.e. implicit theories (Berman & Kenny, 1976; Podsakoff et al., 2003). In other words, respondents hold particular beliefs about the interrelatedness of particular traits, behaviors, and/or outcomes and adjust their responses according to those beliefs. Using implicit theories, respondents may respond based upon assumptions about themselves rather than their actual behaviors (Pace, 2009). This would be akin to respondents believing they are good employees and that good employees engage in OCB, therefore,

they report those behaviors whether they genuinely engage in them or not. Illusory correlations affect ratings by the rater imposing his/her beliefs of items or variables that they expect to covary onto their responses (Podsakoff et al., 2003; Pace, 2009). For example, if a measure of job satisfaction and a measure of organizational commitment are located nearby or in the same survey, the respondents may assume that the items are related and respond in a way to ensure the items are correlated. Alternatively, if survey respondents think items should not be related, they may respond in a way that deflates the correlation. Studies have found that implicit theories and illusory correlations may influence ratings of leader behavior (Eden & Leviatin, 1975; Lord, Binning, Rush, & Thomas, 1978; Phillips & Lord, 1986), attributions of the causes of group performance (Guzzo, Wagner, Maguire, Herr, & Hawley, 1986; Staw, 1975) and perceptions about the relationship between employee satisfaction and performance (Smither, Collins, & Buda, 1989).

Another form of rater effects is social desirability. Social desirability stems from the need for social approval and acceptance and the belief that it can be achieved if one exhibits traits that are consistent with those that society values (Crowne & Marlowe, 1964). Socially desirable responding (SDR) refers to the tendency of some people to respond to items in a manner that is consistent with societal expectations rather than their true feelings (Crowne & Marlowe, 1964; Paulhus, 1991; Paulhus, 2002). SDR is called "one of the most pervasive response biases" in survey data (Mick, 1996). Another way it can influence results is that people may want to please the researcher and tell them what they want to hear, so they modify their responses to be consistent with their perceptions of the researcher's expectations (Podsakoff et al., 2003). Social desirability can bias

answers and produce spurious relationships or mask the true relationships between variables. The concern with self-reported data is that people do not respond truthfully, but instead provide answers to make themselves look good (Paulhus, 2002; Crowne & Marlowe, 1964). One issue with SDR is that for some measures, it may be a legitimate theoretical construct rather than a source of method bias (e.g. Chan, 2009; Lance et al., 2009; Paulhus, Harms, Bruce, & Lysy, 2003; Kline et al., 2000).

Leniency biases are another form of rater effects. They are the propensity for respondents to attribute positive traits, attitudes, and/or behaviors to those they know or like or those with whom they are ego-involved (Podsakoff et al., 2003). Leniency biases may be evident or produce spurious correlations in both self and other report data in that respondents may rate differently those they like and those they do not regardless of actual scores. Effects of leniency biases could be measured by gathering data on ratings of likability or self-esteem and comparing those ratings to measures of performance, attitudes, or perceptions of others. Studies have shown that both self-esteem and feelings of affection towards or strong relationships with others increase the likelihood of leniency bias (Thornton, 1980; Farh & Dobbins, 1989). Classical projection theory also supports this notion in that people are more likely to project undesirable traits to those that they do not like or do not know well (Fisher, 1993). While leniency bias has been shown to produce spurious correlations between employee satisfaction and leader consideration behavior and perceptions of productivity, drive and cohesiveness (Schriesheim, Kinicki, & Schreisheim, 1979), it has also been linked to substantive predictions of both future performance and employee motivation (Bol, 2011). Again, this emphasizes the

consideration that what may be a source of method bias in one study is actually a substantive variable in another.

Another type of source related method effect is acquiescence biases or the propensity of respondents to agree/disagree with the item or attitude of the item regardless of content (Podsakoff et al., 2003; Baumgartner & Steenkamp, 2001; Winkler, Kanouse, & Ware, 1982). In other words, if the statement is positive sounding, then respondents agree, whereas, if it is a negative sounding statement, then respondents disagree. This can result in an increased correlation between all positively worded items and all negatively worded items, regardless of whether or not the constructs are conceptually related. One study on personality research has found support for this to be a substantial method effect by finding a general method factor as well as one method factor for all positively worded items and one for all negatively worded items (Biderman et al., 2011). It is important to note that the post hoc statistical technique applied in that study has been found to produce false positives for CMV detection (Richardson et al., 2009), so the study findings may not be replicable.

Positive or negative affect or emotionality is another type of source related method effect. It refers to the tendency of respondents to view themselves and the world around them either in generally negative or generally positive terms and respond accordingly (Podsakoff et al., 2003; Watson & Clark, 1984). High positive affect is a pervasive individual characteristic in which a person exhibits positive emotionality and self-concept. In other words, individuals are more likely to view their lives and interactions with others in a positive manner. Negative affect is a pervasive individual characteristic in which a person exhibits negative emotionality and self-concept. In other

words, respondents are likely to view their lives and their interactions with others in a negative manner. Authors have found mixed results for the influence of positive/negative affect on job related variables such as employee stress, job and life satisfaction, depression, and organizational commitment (Williams & Anderson, 1994; Brief, Burke, George, Robinson, & Webster, 1988; Chen & Spector, 1991). Again, it is important to note that while positive/negative affect may be a source of bias for some measures, it may be a relevant theoretical construct in others and partialing out its effects may lead to model specification errors (Chan, 2009; Lance et al., 2009).

While positive and negative affect are dispositional enduring traits unlikely to change, transient mood state is not. This method effect refers to the influence of recent mood-altering experiences that influence the manner in which respondents view themselves and their environment, which in turn influences their perceptions and responses (Podsakoff et al., 2003). Transient mood state may change on a daily basis and can stem from any number sources such as physical feelings or illness, interactions with customers or co-workers, work-family conflict, or having a good or bad day. Due to mood state, a survey respondent may answer items more or less positively because of how they are feeling at a given moment rather than how they generally feel.

Method Effects from Item Characteristics

Another source of method effects is generated by the items themselves. Item characteristics effects result from the properties or characteristics of the presentation of the item, either in form of wording or content (Podsakoff et al., 2003; Cronbach, 1946). The way in which the item is presented can trigger certain responses that would not occur if the item were worded differently or presented in a different form. Bäckström et al.

(2009) found that, by manipulating item wording in the International Personality Item Pool (IPIP) (Goldberg, 1999), they were able to change the item loading factors significantly. Examples of item characteristics this include positive or negative item wording, item social desirability, item complexity and/or ambiguity, and scale format and/or anchors.

Positive and negative item wording refers to the fact that the use of positively or negatively worded items may produce spurious relationships, e.g. creating a new factor of only positively or negatively worded items (Podsakoff et al., 2003). Negatively worded items are also referred to as reverse-coded items. The intent of including a mix of positively and negatively worded items is to create "cognitive speed bumps" and reduce response biases such as acquiescence. In theory, their inclusion should make the respondent pay more careful attention to the way in which they are responding by requiring a greater application of thoughtfulness. Harris & Bladen (1994) found that by varying stress versus comfort wording and comparing a model in which wording was controlled with a model in which wording was not controlled, they were able to elicit significant variation in correlations between role ambiguity, role conflict, role overload, job satisfaction, and job tension. Biderman et al. (2011) also found support for item wording effects with one factor loading on positively worded items and one loading on negatively worded items. Other researchers have supported these findings and found that respondents do not always recognize or respond properly to negatively worded items and that their presence frequently elicits a factor that loads only on negatively worded items (Schmitt & Stults, 1985; Podsakoff et al., 2003).

Another wording or item characteristic is item social desirability or item demand characteristics. In addition to the presence of socially desirable responding (SDR), there may be a level of social desirability evident in the question wording. Item social desirability effects occur when one or more items or constructs are written in ways that elicit more/less socially desirable attitudes, behaviors, and/or perceptions (Thomas & Kilmann, 1975). In addition, those items that elicit more social desirability are perceived as being more related to each other (Podsakoff et al., 2003). Item demand characteristics are similar in that these items may inadvertently convey hidden signals that elicit the expected or desired responses (Podsakoff et al., 2003). The concern is that participants may try to be "good" participants and respond in ways that the researcher wants so as not to "ruin" the research (Orne, 1962).

There may also be an effect present if the item is complex or ambiguously worded. Item complexity/ambiguity occurs when items are not clearly written and trigger respondents to respond in a manner that is random or more susceptible to their own heuristics or response styles (Podsakoff et al., 2003). While researchers are encouraged to focus on making their items clear and simple in the scale design process, some constructs may be more complex or require more complicated items (Spector, 1987). Items that may be considered more complex or ambiguous can include double-barreled questions or those that include words with multiple meanings, technical jargon or colloquialisms, or unfamiliar or infrequently used words (Podsakoff et al., 2003). If the item is ambiguous, respondents may misunderstand, apply incorrect meanings, or revert to random responding or other response tendencies.

In addition to the wording or content of the items, the form in which the items are presented may also be an issue (Torangeau, Rips, & Rasinski, 2000). Most researchers use similar scale formats or anchors when designing studies to reduce complexity and provide a standard format from which the respondent can respond (Podsakoff et al., 2003). Scale formats refer to the way in which respondent scores are provided and include Likert scales, semantic differential scales, and "faces" scales. Scale anchors are the values that the respondent may use to provide their scores and include options such as "strongly agree" – "strongly disagree", "always" – "never", "extremely – "not at all." Anchors also include the number of options from which the respondent may choose, e.g. five versus seven response options.

Method Effects from Item Context

Item context effects refer to any influence or interpretation that a respondent might deduce from an item based solely on its proximity to other items in the measurement instrument (Wainer & Kiely, 1987; Podsakoff et al., 2003). Weijters, et al. (2009) examined the effect of changing the proximity of items on a questionnaire and found that correlations increased even in unrelated items if they were next to each other. Item context has the potential to influence the interpretation of a question, the information retrieved and the evaluation of that information, and the response item selected (Harrison & McLaughlin, 1993). Types of item context effects include item priming, context-induced mood, item embeddedness, scale length, and intermixing items of different constructs on a questionnaire.

Item priming effects occur because the positioning of the predictor (or criterion) variable on the questionnaire make the variable more salient to the respondent who will

then imply causal or other relationships to the items and vary their responses accordingly (Salancik & Pfeffer, 1977; Salancik, 1984). By asking questions about particular features of the work environment, other questions become more prominent (Podsakoff et al., 2003). For example, asking a respondent about job characteristics, then asking about attitudes towards job characteristics may frame the attitudes to focus only on those job characteristics that were previously addressed.

Similar to the priming effect, items may also elicit certain moods or attitudes in the respondent based upon their location in the questionnaire. The idea that the first questions encountered set the tone for responses to the remainder of the questionnaire, regardless of the content of the items, is referred to as context-induced mood (Podsakoff et al., 2003). Context-induced mood is related to the aforementioned transient mood state in that it refers to changing attitudes rather than dispositional traits. For example, if respondents find a question too personal or offensive, they may change the manner in which they respond and revert to their chosen response style, refuse to provide answers, or answer the remainder of the survey in a negative mood state. If the scales are too long or difficult, people may get bored or refuse to engage in the effort required to legitimately answer questions or engage in satisficing or selecting the response that is "good enough" rather than trying to find the most accurate answer (Podsakoff et al., 2012).

Another item order effect is item embeddedness, which refers to the potential for either neutral items embedded in the context of positively or negatively worded items to take on those items properties (Podsakoff et al., 2003). The influence of item embeddedness is due to the presence of "cognitive carryover effects" that occur when the mental process(es) associated with one item carry over and provide an easily accessible

cognitive schema with which to answer subsequent questions (Harrison & McLaughlin, 1993). Both general and construct specific support has been found for this method effect. Marsh and Yeung (1999) found that when questions of self-esteem were embedded with other questions, the responses varied significantly from when the self-esteem measures were on their own. Harrison and McLaughlin (1993) found that neutral items embedded in blocks of either positively or negatively worded items were rated similarly. Harrison, McLaughlin, & Coalter (1996) manipulated question order to create either a positive or negative measurement context of outcome favorability and fairness perceptions and found that in a positive context outcome favorability and fairness were marginally related, but in a negative context the relationship increased significantly.

Another potential order effect is the intermixing (or grouping) of items or constructs on the questionnaire. Items from different constructs that are grouped together may decrease intraconstruct correlations and increase interconstruct correlations, thus reducing reliabilities and making it more difficult to distinguish between constructs (Podsakoff et al., 2003). In other words, respondents vary responses to make nearby items consistent regardless of topic. Interestingly, item intermixing has been proposed as a procedural technique aimed at reducing common method bias (Kline et al., 2000). The significance of the simultaneous decrease of correlations within constructs and increase of correlations across constructs is not well understood, but could lead to artifactual covariation among constructs.

Lastly, a non-order related method context effect might also be a source of bias. Scale length may influence the manner in which respondents respond. If scales have fewer items, responses to previous items are more likely to be accessible in short-term

memory and recalled when responding to other items while longer scales are likely to increase fatigue and careless responding (Podsakoff et al., 2012; Podsakoff et al., 2003). In other words, shorter scales may reduce some forms of bias, while simultaneously increasing other sources of bias (e.g., priming effects).

Method Effects from Measurement Context

The fourth source of method effects are those that arise from the measurement context itself. This follows the traditional logic that the act or style of being measured creates covariation (Podsakoff et al., 2003). In other words, simply by observing or being observed, behaviors and cognitions change. The main contextual elements that may influence study results include the time, location, and media chosen for the measurement.

When predictor and criterion variables or different constructs are measured at the same point in time or at the same location, it may produce artifactual covariance independent of the content of the constructs themselves. These measures may exhibit systematic covariance due to the ease of access of previous response cognitions and the likelihood that both measures coexist in the respondent's memory, providing contextual cues for memory retrieval or triggering the use of implicit theories of relatedness (Podsakoff et al., 2003). Use of the same time or location may result in biases such as self-deception, memory biases, or perceptual biases (Podsakoff et al., 2012). This is a common concern; however, it may not be possible for researchers to separate these measures as separation can only be used if the phenomenon is stable, attrition is not likely, and it is financially and logistically feasible (Podsakoff et al. 2012). Another option to increase separation is for researchers to use methodological separation, such as through the employment of different scales, anchors, or collection locations.

The final type of method effect to be discussed is that which results from the use of a common media or medium to obtain results. The use of the same or similar interviewers, expectations, or instructions may influence responses and be a potential source of bias (Podsakoff et al., 2003). Research has shown that some media, such as face-to-face interviews and pencil-paper are differentially prone to social desirability and response style biases (e.g. Martin & Nagao, 1989; Richman, Keisler, Weisband, & Drasgow, 1999; Podsakoff et al., 2003).

In summary, there are several different sources of method effects, which may result in CMV or CMB. Some measures or measurement designs may be differentially prone to certain effects and many sources of method effects can be addressed in the research design stage of development. Method effects may come into play at a different level of the response process including comprehension, retrieval, judgment, response selection or response reporting. Podsakoff et al. (2003) provides an overview of the stages and the potential method biases to which they are subject, which I will summarize here. At the comprehension stage, respondents are logically organizing the information presented and attempting to identify the information sought. At this stage, the most likely source effect is item complexity or ambiguity. If the item is ambiguous or complex, the respondent is likely to look for cues from surrounding questions or rely on implicit theories. The second response stage is the retrieval stage in which the respondent generates his/her retrieval strategy and memory cues, retrieves specific or generic memories, and fills in any missing details in recollection. The most likely method effects to occur at this stage are measurement context, item context, transient mood states, and item content. At the judgment stage of the response process, respondents assess the

completeness and accuracy of their memories and draw inferences from them to fill in any gaps in what is recalled. At this point, the most likely effects are consistency motif, implicit theories, priming effects, item demand characteristics, and item context-induced mood states. In the response selection stage, the respondent matches their judgment with a response category. The most likely method effects at this point are common scale anchors or formats and item context-induced anchoring effects. At the final stage of the response process, response reporting, respondents edit their response and make their selection. Consistency motif, leniency bias, acquiescence bias, demand characteristics, and social desirability are the most likely sources of method effects at this stage.

The Ongoing CMV Debate

As previously stated, there is much disagreement on the topic of CMV and researchers have come to different conclusions as to its importance, nature, and existence. Most researchers agree that more studies need to be done in order to understand when CMV is a source of bias (Brannick et al., 2010). This section contains a review of the conflicting findings regarding CMV. First, articles that found support for the existence of CMV are discussed. In the second section, articles that found a lack of support for the existence of CMV are discussed.

Evidence In Favor Of CMV

Perhaps the most heavily cited works in favor of the existence of CMV are by Podsakoff and colleagues. In a critical research review, they found that on average, the amount of variance accounted for in the presence of a common method was approximately 35% versus only 11% when a common method was not present (Podsakoff et al., 2003). Cote and Buckley (1987) in their examination across 70 multi-trait multi-

method matrices in over 114 journals from various fields found that CMV varied based on both the field of study and the measures in questions. Specifically CMV in measures was 41% for attitude measures, 25% for personality and aptitude/achievement measures, and approximately 23% for performance and satisfaction measures. The average variance attributed to CMV was 26.3% in their study. When several meta-analyses (Doty & Glick, 1998; Cote & Buckley, 1987; Lance et al., 2010; Williams et al., 1989) were combined, Podsakoff et al. (2003) found that the total variance attributable to method factors in items ranged from 18-32%. Other meta-analyses found the percent of inflation due to common method bias to range from 38%-92% (Podsakoff et al., 2012; Doty & Glick, 1998). Scherpenzeel and Saris (1997) found additional evidence that effects of CMV varied based upon the type of construct being measured, item social desirability, form and length of the response scale, method of data collection, position of the item in the questionnaire, and the type of information requested.

In their study, Williams and Anderson (1994) found support for the congeneric perspective of CMV in that the method effects associated with negative affect were not equal among items or within constructs. Baumgartner and Steenkamp (2001) found an average variance of 8%, with a range from 0% - 29%, was due to five specific response styles. Schmit and Ryan (1993) factor analyzed item composites of the NEO-FFI (Costa & McCrae, 1989) using applicant and non-applicant samples. They found that the five factor model of personality fit the non-applicants, but a sixth factor emerged for the applicants, which was comprised of several item composites from across four of the five subscales, that they called it the "ideal employee" factor and attributed it to CMB. Organ and Ryan (1995) found in their meta-analysis that studies using self-reported ratings of

OCB and dispositional and attitudinal variables resulted in spuriously higher relationships. Biderman et al. (2011) found support for the congeneric influence of both single and multiple method factors and that if researchers only controlled for one factor significant reductions in model fit resulted. Johnson et al. (2011) also found support for the congeneric perspective. Their study indicated that as much as one third of the variance in core self evaluations could be attributed to method effects and that controlling for a latent social desirability factor was the most effective at reducing CMV, but that not all measures were affected equally. In another study, it was found that regression coefficients could be both attenuated and inflated due to method variance, that the effect had an inverse relationship with the number of variables in the regression equation, and that method variance tended to reduce both interaction and quadratic effects (Siemsen et al., 2010). Gorrell et al. (2011) found that the use of more abstract items resulted in significant increases in CMV, supporting the method effects of item complexity/ambiguity. It is important to note here, that the procedure employed by several of these authors to detect CMV (e.g. Cote & Buckley, 1987; Biderman et al., 2011; Johnson, Rosen, & Djurdjevic, 2011; Gorrell et al., 2011; Schmit & Ryan, 1993) was unmeasured latent method construct (ULMC) confirmatory factor analysis. This method has been found to produce false positives in detecting the presence of CMV (Richardson et al., 2009; Crampton & Wagner, 1994). Another concern is that in the meta-analyses, it is neither possible to know all of the methodological considerations in a study nor what each article considered a "method" for all of the studies nor whether measurement or other specification errors were present in the original data (Cote & Buckley, 1987; Lance et al., 2009). Further, there is potential that the "method" factors

are actually unmeasured substantive variables such as social desirability, affect, or response style or legitimate perceptual differences in the constructs being measured (Chan, 2009; Lance et al., 2009).

Evidence against CMV

While many researchers have found evidence for the existence and biasing effects of CMV, there are also many who found either no or minimal effects. Perhaps the most heavily cited evidence against the biasing effects of CMV comes from the works of Spector and colleagues. Their results indicate that studies finding CMV may be exaggerated or oversimplified and that it is more likely that a combination of the method and trait variance is responsible for the findings (e.g. Chen & Spector, 1991; Spector, 1987; Spector & Brannick, 2009; Brannick et al., 2010). Spector (2006) posited that if CMV exists and affects relationships, then we should see "clear evidence of inflation whereby observed correlations are larger with monomethods than with multimethods" and that all self-report scales should be correlated if the self-report itself is a method of introducing bias. In other words, if a true CMV/CMB factor exists, there should be a baseline correlation among all variables. Because CMV is not a universal inflator of variables, it is likely more complex than simply an issue of the rater source or measure.

Crampton & Wagner (1994) conducted a series of analyses on self versus multi-method datasets and found that while inflation existed in some datasets, it was neither pervasive nor predictable in its direction and level of influence and neutral effects were just as likely to occur. Similarly, Malhotra et al. (2006) used multiple techniques and found no significant CMV bias. Further, they analyzed 19 papers (216 correlations) in issues of *Management Science, Information Systems Research,* and *MIS Quarterly*

between 1993 and 2003 and found that by applying the correlational marker technique, only 18-25% of the correlations became insignificant when the correction was applied. This supported the idea that the problem may not be as pervasive as some believe. In addition, Simmering, Fuller, Richardson, Ocal, & Atinc (under review) found evidence for CMV using some techniques, but not others.

Other researchers have found that variance that was once attributed to method effects or other contaminants are now legitimate theoretical constructs (Lance et al., 2009) and that variance due to the method may be the result of different perspectives or thought processes (Lance et al., 2010). Social desirability, negative affect, and acquiescence have all been found to, in some cases, create no significant effect (Spector, 1987). Paglis and Williams (1996) used CFA to show that CMV would have to create an influence of between 18-20% in order for it to create a plausible alternative to the actual relationships of interest. Lance et al. (2010) found that the unreliability of measures counteracts CMV and that the presence of shared variance may be a product of the measurement or it may be the result of an actual relationship. The potential exists that certain "method factors" may in fact exist because we believe they exist. This creates a "Pygmalion" or "Golem" type effect, in that people who have certain traits, like high (low) levels of social desirability, self-esteem, or positive affect, are genuinely more (less) satisfied or engage in more (less) exhibitions of certain types of behavior (Pace, 2009; Chan, 2009; McNatt, 2000). This brings forth the issue as to whether that type of effect can or should be controlled. Meade et al. (2007) found that in modeling trait correlations at the latent level, CMV existed but the bias was not significant. In other words, CMV was present, but CMB was small to non-existent and the presence of

common methods does not inherently indicate a problem in results. Baltes et al. (2002) found that CMV was not a factor in non-linear models of job characteristics and outcomes. Evans (1985) and Siemsen et al. (2010) found that when researchers use more complex models and interactions, the presence of CMV is reduced. By adding more variables to the model that are uncorrelated with the substantive variables, you can reduce and eventually eliminate any source of CMV (Siemsen et al., 2010).

All of these findings taken together indicate that different approaches and different research considerations may influence CMV findings, but when CMV is a cause for concern is not well understood. Further research is needed to isolate when and how CMV is likely to exist and cause issues. Additionally, it is possible that the contradictory or conflicting findings may be the result of appropriate and unavoidable ambiguities within and among domains rather than method effects (Crampton & Wagner, 1994).

Remedies for CMV

Whether one believes that CMV is a pervasive problem that affects and potentially invalidates all mono-method studies or not, it remains an issue that should be addressed. Most researchers agree that addressing CMV sources in the design stage of development is the best method for reducing potential CMB. While several post-hoc statistical techniques have been developed, none have received wide empirical support or validation. Further, each of remedies may introduce their own form of method bias.

Procedural Remedies for CMV

Procedural remedies for CMV are addressed a priori in the research design and are intended to eliminate potential causes of CMV. Procedural remedies include the following: obtaining the predictor and criterion from different sources; introducing

temporal, proximal, or psychological separation between the predictor and criterion; eliminating common scale properties for the substantive variables; improving scale items to eliminate ambiguity; reducing social desirability bias in item wording; and balancing the number of positive and negative items (Podsakoff et al., 2012). The following section will provide a review of each of the procedural remedies.

The first procedural remedy is to obtain the predictor and criterion from different sources. As previously mentioned, this may introduce its own biases in that each source is subject to its own method effects and self/other respondents may not be measures of the same criteria. Obtaining the predictor and criterion from different sources (e.g. data reported by two different people or obtained from an archival source) is posited to reduce/eliminate common rater response biases, idiosyncratic implicit theories, and response styles because the individual rater's mindset cannot bias both the predictor and the criterion (Podsakoff et al., 2012). Use of this procedure has been found to decrease correlations in several studies (e.g. Ostroff, Kinicki, & Clark, 2002; Lowe, Kroeck, & Sivasubramaniam, 1996; Lance et al., 2009). This procedure may not be appropriate for all studies. It does not work if both the predictor and criterion are assessing individual values, beliefs, attitudes, or perceptions because they may not translate into observable behaviors that others can use to infer information about the individual (Podsakoff et al., 2012; Brannick et al., 2010).

The second procedural remedy is temporal, proximal or psychological separation of the predictor and criterion (Podsakoff et al., 2012). This procedure creates a separation between the two measures with the intent of reducing the respondents' ability and/or motivation to use previous responses to fill in gaps, infer details, or answer

subsequent questions. A temporal separation is a time delay, typically either a few weeks or a few months. Temporal separation works by reducing ability of the respondent to recall information by allowing the information to leave short-term memory (Podsakoff et al., 2012). Temporal separation has been found to be a potentially effective procedure for reducing some method biases; however, it can increase the complexity and cost of the study design, may allow other factors to influence responses, and may increase attrition rates. This remedy also relies on the assumption that the relationship between the constructs is stable, but recent research indicates that method bias may not dissipate over time (Alessandri, Vecchione, Fagnani, & Barbaranelli, 2010; Weijters, Geuens, & Schillewaert, 2010). In other words, it is unclear whether temporal separation is detecting method variance or merely transient moods states. Rindfleisch et al. (2008) looked at cross-sectional versus longitudinal data and found that longitudinal data collection reduced CMV. Other studies have found contradictory results. Some studies have found that correlations between variables was reduced over time (Ostroff et al., 2002; Johnson et al., 2011), but it is unclear whether the reduced correlations can be attributed solely to temporal separation and not other extraneous factors, such as mood or legitimate changes in perceptions. Some researchers have found that there was no difference in the magnitude of correlations in studies in which a temporal separation was present versus absent; however, it is unknown whether the lack of difference was due to the separation not working as a remedy for CMV or whether CMV was not present in the first place (Castille & Simmering, 2013). One of the greatest potential drawbacks of using temporal separation is respondent attrition. It may also allow other non-

methodological triggers to create influence, such as transient mood states, and it may be difficult to determine what the appropriate delay between measures should be.

The second type of separation is proximal separation or separation based on physical distance. Proximal separation works by eliminating retrieval triggers. Empirical research indicates that separation by at least six items can diminish biases due to context and question order effects and that separation with measures of different constructs that use the same or different formats or intentional buffer questions are most effective (Podsakoff et al., 2012; Weijters et al., 2009; Torangeau et al., 2000). Some of the potential problems with proximal separation are that it can increase scale length issues (fatigue, lowered response rates, increased cost) and, if filler items are related, it may introduce new bias (Podsakoff et al., 2012). Further, in their study, Castille and Simmering (2013) found no difference in data that was proximally separated versus data that was not.

The third type of separation is psychological. Psychological separation works by creating a cover story to reduce the salience of linkages and reducing the relevance of previous information (Podsakoff et al., 2012). Very few studies have addressed the efficacy of this procedure. One study that addressed the idea of psychological separation discusses use of a "multiple studies" cover story in which the researcher states, "for reasons of convenience or efficiency several unrelated studies are being conducted at the same time" (Aronson et al., 1998). Psychological separation via cover story has been successfully used in priming experiments (Higgins, Rholes, & Jones, 1977) and attitudinal experiments (Rosenberg, 1965). Other studies have found no significant differences between groups who received a cover story and groups that did not (Castille

& Simmering, 2013). A disadvantage of the technique is the difficulty of creating a credible cover story, thus it is essential to thoroughly pretest the cover story to ensure efficacy. Another procedure for assessing the effects of psychological separation is to incorporate a direct measure, such as the perceived awareness of research hypothesis scale (PARH), which asks whether the respondents perceived that they knew what the survey was about or what the researcher was attempting to find (Rubin, Paolini, & Crisp, 2010). Other techniques for introducing psychological separation are to camouflage the criterion or predictor by embedding it in other questions to make it less psychologically prominent or disguise the reasons for obtaining the predictor or criterion measure by misleading participants as to its purpose in the study (Podsakoff et al., 2012). Psychological separation is unlikely to reduce memory biases or item context effects and may work best when combined with temporal or other separation.

Another procedural technique researchers may use is to eliminate common scale properties. This remedy functions through the belief that if question formats are similar, people will think the questions are associated with each other (Campbell & Fiske, 1959; Podsakoff et al., 2003; Podsakoff et al., 2012). It is important that researchers take care when eliminating common scale properties so that they do not to alter the conceptual meaning of the measures (Nunnally & Bernstein, 1994). Previous research has found support for the use of different scale formats or anchors in reducing the correlations of items (Podsakoff et al., 2012; Weijters et al., 2010). One concern is that this technique may introduce other biases or create confusion by giving too many response types.

Improving the scale to eliminate ambiguity is another procedural option for researchers. Ambiguous scales are those that leave room for respondent interpretation or

the assignment of idiosyncratic meanings that can cause respondents to be unsure of how to respond. Ambiguous scale items include those anchors that may be interpreted differently by each respondent or words with multiple meanings or multiple ideas in one sentence (Johnson, 2004) and can be remedied by a number of options (Podsakoff et al., 2012). The first option is to keep questions simple, specific, and concise at the scale design stage (Torangeau et al., 2000). Other options include defining terms, focusing question content, avoiding double-barreled questions, avoiding complicated syntax, and labeling every response point (Krosnick, 1991; Torangeau et al., 2000; Podsakoff et al., 2012). While no studies to date have explicitly examined the effects of item ambiguity on the estimates of relationships between two constructs (Podsakoff et al., 2012), research has shown that if items are more abstract, then CMV tends to be more of a problem (Gorrell et al., 2011).

Yet another procedural remedy is to reduce the social desirability bias in item wording. Item wording can undermine the accuracy of responses by causing the respondent to modify their response based on the perceived social desirability of the item (Podsakoff et al., 2012). Bäckström et al. (2009) found that through manipulations of the IPIP Big Five scale item wordings to make them less socially desirable they were able to change the loadings of the originally worded items significantly. One way of reducing item social desirability includes obtaining independent assessments of item level social desirability and revising the wording of highly rated items. Another is to calculate a correlation between responses to a recognized social desirability scale (Podsakoff et al., 2012; Paulhus, 2002). There is no direct empirical evidence of either procedure's efficacy at reducing bias in correlations between measures of different constructs.

Further, the procedures may be difficult to implement, as it may be complicated to revise the item without compromising its content validity. In addition, the relationship between an item and its perceived social desirability may be nonlinear (Kuncel & Tellegen, 2009; Podsakoff et al., 2012).

The final procedural remedy, balancing positive and negative items, has been studied by several researchers. By balancing positive and negative items, researchers attempts to reduce acquiescence or disacquiescence response style biases. Respondents who use acquiescence response styles are those who disproportionately use the positive end of the scale. Disacquiescence styles include those respondents who disproportionately use the negative end of the scale (Podsakoff et al., 2012). Balancing positive and negative items does not eliminate the occurrence of the behavior, but controls for its bias by balancing half of it upwards, half downwards, and vice versa (Baumgartner & Steenkamp, 2001). This remedy may cause confusion for respondents because it increases the cognitive load for processing information, Further, most scales are not already balanced so researchers may have to modify the items or number of items and could inadvertently change the meaning of the item (Podsakoff et al., 2012; Nunnally & Bernstein, 1994).

Implementation of the previously mentioned procedural remedies must be determined a priori. One of the potential drawbacks of any procedural technique is that it may not eliminate biases or reviewers may still have questions or concerns about the data. To address the common situation in which there is a concern about the presence of method bias after data has been collected, a number of post hoc statistical techniques have been developed.

Post Hoc Remedies for CMV

Post hoc statistical techniques are used in addition to procedural remedies in order to detect and/or reduce the effects of CMV/CMB. A significant advantage for researchers who failed to account for CMV concerns in the planning stage is that many post hoc techniques do not require a priori planning. While many of these techniques have theoretical soundness, they have not been broadly empirically tested and may not produce the desired results. Post hoc statistical techniques include the following: the Harman's one-factor test unmeasured latent method factor technique, the correlation-based marker variable technique, the CFA marker technique regression-based marker variable technique, the instrumental variable technique, the directly measured latent method factor technique, and the measured response style technique.

The most widely used technique is the Harman's single factor test (Podsakoff et al., 2003; Podsakoff & Organ, 1986). In this technique, researchers load their study variables into an exploratory factor analysis and examine the unrotated factor solution. If CMV is present, then a single factor will emerge or one factor will account for most of the variance. An alternative to this technique is to input all variables into a CFA model to see if a single factor emerges (Podsakoff et al., 2003; Malhotra et al., 2006). This technique has several drawbacks. The first is that it only detects and does not correct for CMV (Podsakoff et al., 2003). Further, it has been shown to be insensitive at detection because if a common factor emerges, then you may have CMV or you may have a construct that is lacking in discriminant validity or a causal relationship. In addition, the technique is not able to detect moderate or small levels of CMV. It is also unlikely that a single factor will emerge as there has been little support for the noncongeneric view of

CMV and the failure to find a single factor does not necessarily indicate that the data is CMV free. In addition, as the number of substantive latent factors increase, it becomes less likely that a single factor will be able to account for variance (Malhotra et al., 2006). Due to these limitations and in spite of its wide use, researchers do not recommend this technique (e.g. Lindell & Whitney, 2001; Podsakoff & Organ, 1986; Malhotra et al., 2006; Podsakoff et al., 2003).

Using the unmeasured latent method factor technique researchers add a first-order method factor into a structural equations model with the only purpose of indicating shared variance (Podsakoff et al., 2012). The advantages include the following: no specific factor is needed; the effect is modeled at the measurement level rather than latent level; and there is no requirement for the method effect to be equal or noncongeneric. Some of the disadvantages include the following: the unmeasured factor may reflect CMV as well as unhypothesized variances due to actual construct relationships (Richardson et al., 2009); it can cause identification problems if the ratio of indicators to substantive constructs is low; and it is based on the assumption that the method factor does not interact with the trait factors. Most of the literature that has found support for the biasing influence of CMV used this technique, thus bringing into question the validity of studies that have found support for CMB (e.g. Biderman et al., 2011; Cote & Buckley, 1987; Gorrell et al., 2011). Despite its use in the literature, recent empirical research has not supported the efficacy of this procedure, as it is likely to find CMV when it is not actually present (Meade et al., 2007; Richardson et al., 2009; Conway & Lance, 2010).

A third statistical technique is the correlation-based marker technique (Lindell & Whitney, 2001). In this approach, the researcher first identifies a theoretically unrelated

marker variable. Next, the researcher uses the smallest correlation between the marker variable and substantive variables as the estimate of method bias. Third, the researcher adjusts the zero-order correlations between substantive variables by the estimate and divides by the quantity of 1 minus the estimate. The researcher then examines whether the resulting partial correlation differs from zero. If the partial correlation is significant, then the relationship holds even after controlling for method bias. Next, the researcher partials out the smallest observed correlation to obtain a corrected measure. In other words, an estimate of CMV can be identified if at least one correlation between the constructs should be zero, which can be accomplished by including a single variable that is theoretically unrelated to the substantive variables.

Some authors have used the correlation based marker technique using a post hoc marker by selecting the lowest correlation from the existing substantive variables as described by Lindell and Whitney (2001). Researchers have used this technique of correction because of it is relatively easy to implement (Podsakoff et al., 2012; Lindell & Whitney, 2001). Yet, if the post hoc marker variable chosen does not share the same measurement characteristics as the substantive variables (e.g., measured on a Likert scale), then it cannot accurately measure CMB and instead functions as a measure of some other effect. The most significant disadvantages to this technique are that it assumes equal effects of the marker variable on every substantive variable and that method bias only causes inflation of relationships. This can be problematic because most of the studies that have found support for the existence of CMV/CMB have supported the congeneric perspective and method effects do not always cause inflation. Some other disadvantages of the technique include the following: it ignores measurement error that

could reduce the correlations between the marker variable and substantive variables; it controls for bias at a scale rather than item level; and it assumes the marker variable does not interact with the substantive variable (Podsakoff et al., 2012). Research is inconclusive as to whether most authors using this technique do not find evidence of CMV because it truly does not exist, because it is unlikely the research will be published if CMV is found, or because the technique is ineffective at detecting CMV (Richardson et al., 2009; Williams et al., 2003).

The next statistical technique was developed to remedy some of the concerns with the correlational marker technique. The CFA (confirmatory factor analysis) marker technique (Williams et al., 2010) requires the researcher to use a series of marker variables that share measurement characteristics with substantive variables and to run latent variable models comparing relative fit. Some of the advantages of this technique include the following: it models method bias at indicator level rather than construct level; it provides a statistical test of the method bias based on model comparisons; and it permits a test of whether the method biases affect all measures equally (Podsakoff et al., 2012; Williams et al., 2010). One significant disadvantage to this technique is that it does not identify nature of the method bias, thus the conceptual meaning of the method factor is ambiguous. Other disadvantages include the following: it places no constraints on the relationships of the marker variables to each other; its results may be susceptible to the specific variables used as indicators; and its the use of estimations from a specified model may not provide correct standard errors and goodness of fit statistics (Podsakoff et al., 2012). One critical aspect of this technique is the proper selection of the marker variable (Richardson et al., 2009). A review of published research using marker variables

indicates that authors choose a wide variety of markers, many of which are not suitable for CMV detection (Simmering et al., under review). This may be one of the reasons why, in spite of the CFA marker technique receiving support in the literature as the most accurate at properly detecting CMV, it has been the least frequently employed by researchers (Richardson et al., 2009; Simmering et al., under review; Malhotra et al., 2006).

A fifth statistical remedy is the regression based marker technique in which you use marker variables that are uncorrelated with the substantive variables and are susceptible to method bias (Siemsen et al., 2010). The regression based marker technique has the same advantages and disadvantages as the correlational marker variable technique in that it ignores measurement error, controls for bias at the scale rather than item level, and assumes the method factor does not interact with substantive variables. Further, this technique only controls for a single method factor and it is unclear that the additional marker variables are actually capturing method variance and not something else (Podsakoff et al., 2012; Siemsen et al., 2010).

A sixth statistical remedy is the instrumental variable technique. Using this technique, the presence of a method factor will cause a structural error term for the equation to be correlated with the predictor (Podsakoff et al., 2012). This technique developed by Antonakis et al. (2010) violates the assumption of several estimation techniques and results in bias because it uses an exogenous predictor as an endogenous predictor. In other words, a predictor that is presumed to be external to the hypothesized relationship is based on a predictor internal to the relationship. In this technique, instrumental variables are added to the model and then the effect of the predictor on the

criterion is estimated using two-stage least squares. This technique has the advantage of providing a solution when one cannot identify or directly measure a method factor, but requires the researcher to accurately identify independent variables that are related to the endogenous predictor but not the structural error term which may prove quite difficult for researchers to do (Podsakoff et al., 2012; Antonakis et al., 2010).

The directly measured latent method technique is a procedural remedy that may be used when the researcher knows the potential source of method bias and has a measure for it (Podsakoff et al., 2012). Examples of directly measured latent methods include positive/negative affect, perceived awareness of the research hypothesis, and social desirability. Potential advantages of this technique are that it unambiguously identifies the source of the method bias, controls for measurement error, models the effects of the biasing factor at the item rather than construct level, and does not constrain the effects of the method factor (Podsakoff et al., 2012; Williams & Anderson, 1994). The most significant disadvantage of this technique is that it requires researchers to anticipate sources of method bias a priori and include measures of those sources in their data collection. It also assumes that the method factor does not interact with the substantive construct (Podsakoff et al., 2012; Bagozzi & Yi, 1990). This approach is recommended whenever possible as it allows for a direct measure of the method effects (Podsakoff et al., 2012).

The final statistical technique for correction is the measured response style technique (Weijters et al., 2010). Using this technique, a researcher systematically measures common response styles and partials out their effects on responses. While the technique requires the researcher to take several steps and is more involved than some of

the other procedures, it has several advantages over them. First, it requires that a relevant item population that does not form a meaningful nomological network be identified and a random sample taken. This could be a random sample of items from the survey conducted. It is recommended that researchers include a minimum of three sets of five items each. Next, the random sample of items using the same scale format as other survey items are inserted as buffer items between the scales of substantive interest. This technique can provide a measure of the most common response styles, such as acquiescence, disacquiescence, extreme response style, and midpoint response style (Weijters et al., 2008). For each set of response styles, the author obtains measures and uses them as indicators of the latent response style construct. The items selected for response style bias must be independent of the substantive variables; that is, there should be no theoretical reason why the responses should relate. Researchers need to ensure that they assess a complete profile of response biases because it may not be possible to know a priori which response style will be most prevalent (Weijters et al., 2010) and ad hoc selection of variables has been found to be less than optimal (De Beuckelaer et al., 2010). This statistical technique is recommended by researchers (Podsakoff et al., 2012); however, the requirement to include so many additional items in a survey may not be feasible in some research surveys.

The intent of procedural and statistical remedies is that while there are many choices, researchers should fit the remedies to the specific research questions and recognize that there is no "one technique fits all" approach. Regardless of the technique(s) employed, the end goal is to ensure that the results are valid and that method biases cannot account for a rival explanation of the study results. The best approach may

be to identify one or more potential sources of method bias, include them as manipulations in the study design, and test whether the hypothesized causes of CMV hold across conditions (Podsakoff et al., 2012; Brannick et al., 2010).

Manipulation of CMV and Hypotheses

As detailed in the previous section, CMV is the result of method biases and there are several procedural and statistical techniques that may decrease its occurrence and biases. In this dissertation, two presumed causes of CMV will be studied: implicit theories and demand characteristics. Implicit theories indicate that respondents make logical connections among survey scales and respond more consistently than two different respondents would, thereby inflating substantive relationships. Demand characteristics indicate that respondents feel they must respond to items in a particular way due to researcher demand or the item wording. These responses are more likely to be overall positive and consistent with researcher expectations. This study proposes to examine these sources of CMV from an experimental standpoint by attempting to manipulate them using control and experimental conditions. CMV can be approached by experimental manipulation because you can randomly assign one group to which you apply a manipulation using a remedial technique and one to which no remedial technique is applied (Kerlinger & Lee, 2000). By comparing the groups, one can determine whether there is a significant difference, which would indicate that the remedy was either effective or ineffective. See Figure 2.1 for a graphical depiction of the experimental design.

In experimental design, there are two types of straightforward manipulation: stimulus and instructional (McBride, 2012). With stimulus manipulation, different

experimental conditions use different stimuli, such as positive versus negative item wording or the presence or absence of a particular variable. If the respondent encounters the different stimuli, then their behavior, interpretation, or response is posited to change accordingly. Different stimuli can include a different person providing direction, a different context of measurement, or a different format of testing. In other words, stimulus manipulation involves exposing the respondents to different environmental or contextual stimuli and gauging their reactions to those stimuli. With instructional manipulation, different experimental groups are given different instructions, such as how to respond to survey items. In this way, instructional manipulation is similar to the cover story technique previously mentioned. The cover story technique may include a false purpose or incentive to fake (e.g. Robie, 2006; Ziegler & Buehner, 2009), or obscure the purpose of the study (e.g. Aronson et al., 1998; Zeigler & Buehner, 2009; Birkeland, Manson, Kisamore, Brannick, & Smith, 2006; Castille & Simmering, 2013; Ziegler, 2011). Instructional manipulation is targeted more towards cognitive processes and biases, whereas stimulus manipulation is targeted more towards reactions. In other words, instructional manipulations address how a person approaches a situation, while stimulus manipulations address what they do or how they interact with particular contexts or stimuli.

In the first experimental condition, instructional manipulation via a cover story will be applied to determine whether this procedural remedy influences results (Aronson et al., 1998; Podsakoff et al., 2012). The control group will receive traditional instructions implying that all of the items may be related because they are in the same study. One experimental group will receive instructions that the items are a collection of

several researchers' items and the items are not related, i.e. CMV reducing instructions. Another experimental group will receive instructions that many of the items are highly related, i.e. CMV inducing instructions. In this way, a demand characteristic is introduced that should either increase or decrease the respondents reliance on implicit theories and increase or reduce psychological separation of the variables. If the respondents are relying on implicit theories in their answers in the control condition, then the introduction of a cover story should increase or decrease the presence of CMV. If the respondents are not relying on implicit theories in their answers in the control condition, then the procedural remedy of a cover story will have no effect.

H1: The CMV reducing experimental manipulation in comparison to control conditions will result in:

(a) Weaker effect sizes

(b) Weaker relationships to marker variables

H2: The CMV inducing experimental manipulation in comparison to control conditions will result in:

(a) Stronger effect sizes

(b) Stronger relationships to marker variables

In addition to the instructional manipulation, two different types of stimulus manipulation intended to influence CMV will be used, including: communication medium and proximal separation. One proposed remedy for CMV is to vary methods for data collection, such as different tests, testing scenarios, or supervision (Ziegler, 2011). For the first stimulus manipulation, an application of written versus verbal communication medium for the delivery of instructions will be employed. The inclusion

of the video delivery of instruction as an experimental manipulation is based upon media richness theory (Daft & Lengel, 1986; Daft & Lengel, 1984). According to this theory, information richness refers to the amount of information a medium is capable of conveying and the ability of the information to communicate the message (Daft & Lengel, 1986). Communication media vary in their ability to convey rich information (Daft & Lengel, 1984). In other words, some forms of communication have a greater ability to ensure the accurate communication of information than others do. In order of decreasing media richness, communication methods are as follows: (1) face-to-face; (2) telephone; (3) memos and letters; (4) impersonal written documents; and (5) letters. Face-to-face communication is considered the richest because it allows multiple modes of communication via body language, tone of voice, and message content, thus it has additional contextual cues to aid the recipient in the correct interpretation of the message. In this manner, video communication should be similar. Although it does not allow for the opportunity of two-way communication, it should still increase the presence of the researcher and the likelihood of the respondent to respond in a manner that is consistent with researcher expectations (Ziegler, 2011). Previous research indicates that the provision of video instructions versus traditional written instruction creates significant improvement in motivation in terms of attention and better recall of instructions (Choi & Johnson, 2005). In this manner, the communication medium of instructional delivery is a way of manipulating demand characteristics. It is a form of stimulus rather than instructional manipulation because the instructions given in the video are no different from the instructions given in writing, only the delivery medium (stimulus) changes. If communication medium stimulus can be introduced that either increases or decreases the

influence of demand characteristics or reliance upon implicit theories, then the groups can be compared and the levels of CMV can be assessed. If there is no difference in the experimental groups, then the introduction of this CMV manipulation had no effect. If there is a significant difference in the experimental groups, then the delivery stimuli introduced significant CMV.

H3: Video instructions versus written instructions will result in:

(a) Stronger effect sizes

(b) Stronger relationships with marker variables

Another method of stimulus manipulation that will be employed is the use of proximal separation. As detailed previously, proximal separation works by using either item randomization or filler scales to reduce the salience of item relatedness and recall of previous responses. For this manipulation, randomization of items will be employed in the experimental group and traditional item order will be used in the control group. This manipulation is purely to detect the use of implicit theories in item response as the demand characteristics and other experimental conditions to which the respondents are exposed will be the same. In other words, the respondents will receive the same instructions, either video or written that the items are/are not related, regardless of proximal separation treatment group. In theory, increasing the proximal separation of variables should reduce CMV.

H4: The use of proximal separation versus no proximal separation will result in:

(a) Weaker effect sizes

(b) Weaker relationships with marker variables

H5: Measures of presumed causes of CMV will have stronger effect sizes in CMV inducing conditions versus reducing conditions.

(a) Social desirability will have stronger effect sizes with the substantive variables in the CMV inducing conditions than in the CMV reducing conditions

(b) Negative affect will have stronger effect sizes with the substantive variables in the CMV inducing conditions than in the CMV reducing conditions.

(c) Positive affect will have stronger effect sizes with the substantive variables in the CMV inducing conditions than in the CMV reducing conditions.

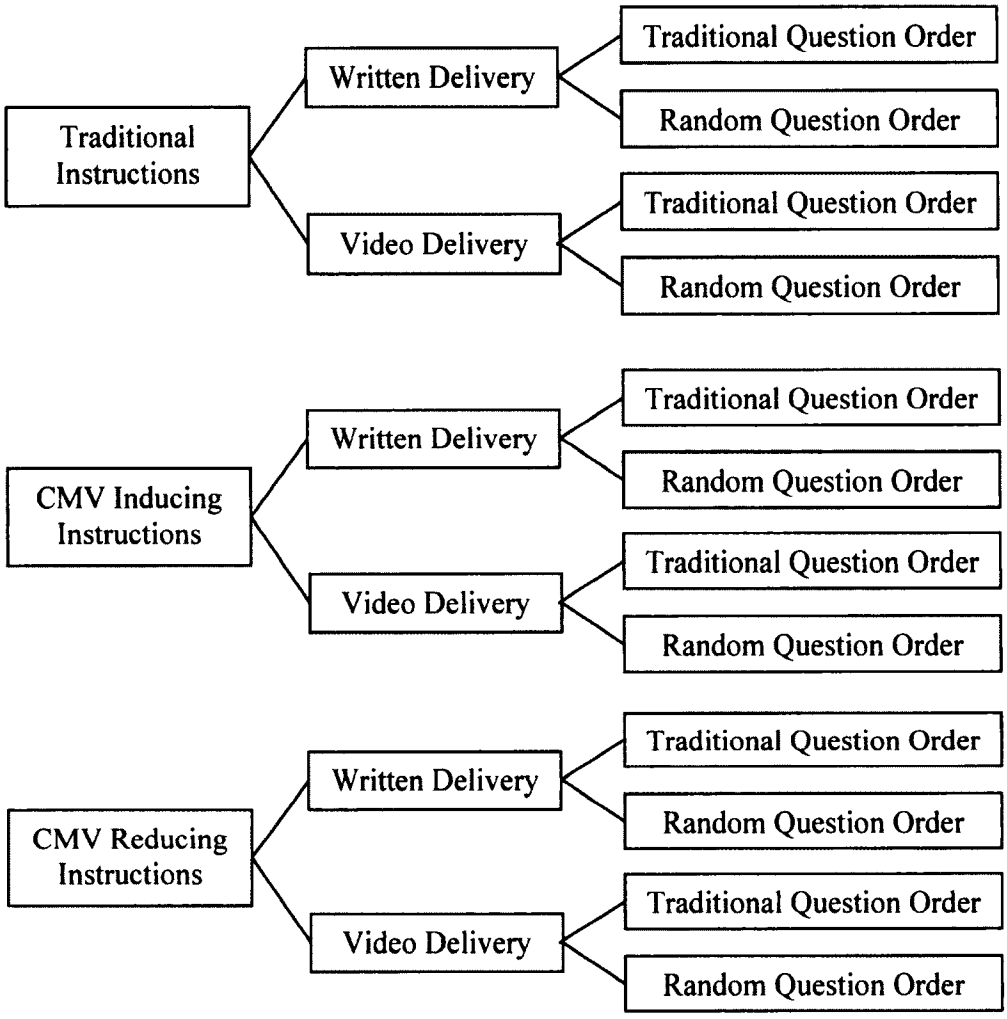


Figure 2.1 Experimental Model

CHAPTER THREE

METHODOLOGY

The objective of this chapter is to identify the methods that were used to experimentally manipulate and empirically test the hypotheses put forth in Chapter Two. The chapter is divided into three sections: (1) experimental design, (2) operationalization of the research variables, and (3) plan for statistical analyses.

Experimental Design

Experimental Objectives

The focus of this study was to determine the efficacy of remedial procedures to prevent CMV/CMB. As detailed in the previous chapter, there are a number of presumed design characteristics that influence CMV; however, most have received little empirical attention addressing their efficacy at influencing the biasing effects of CMV. The purpose of this experiment was to evaluate proposed a priori CMV remedies to assess their influence and usefulness. The experiment attempted to manipulate sources of CMV using control and treatment groups. By comparing the groups, one can determine whether there was a significant difference, which would indicate that the remedy was either effective or ineffective. There were twelve potential experimental groups including the control group. The experiment will employ a 3 x 2 x 2 design. For the analysis, the

experimental groups were collapsed based upon similarity of treatments and the research question being examined in order to isolate experimental effects more effectively.

Sources of Variation

This study was designed to include three treatment factors, namely "psychological separation," "instructional medium," and "proximal separation." The levels of these treatment factors were such that the respondents encountered one of twelve survey treatments based on a combination of the three factors. Traditional survey method employing traditional item order with written instructions was designed as the main control condition.

At the beginning of the survey, the respondents were randomly assigned to one of two treatments: traditional survey method or randomized items. In the traditional survey method, items and scales were presented in the standard format giving predictors and criterion in their causal order. In the proximal separation treatment, items for the survey were randomized. The use of randomization is similar to item counterbalancing as a procedural remedy and is aimed at reducing order, cognitive, and context effects (Podsakoff et al., 2003; Dillman, Smyth, & Christian, 2009). In surveys, early response options may be processed more deeply and may be more likely to be accurately because the respondent's mind becomes more cluttered or fatigued from information as the survey continues. Randomization functions, not to eliminate the order effects, but to average them out such that any effects due to the item's placement are cancelled out by the same item appearing at a different point to other respondents (Dillman et al., 2009).

After this assignment, participants were randomly assigned to receive their instructions under one of two conditions: either video instructions or written. The video

instructions were intended to simulate a richer media format in order to create a stronger likelihood of social desirability, motivation, and attempts to please the researcher (Choi & Johnson, 2005). Participants in the control group were told a traditional set of survey instructions "In this study you will be asked to respond to several statements about your work and behaviors. The purpose of this study is to examine the relationships among a variety of workplace behaviors." Participants in the treatment groups were told instructions intended to obscure the study purpose. Participants in the first experimental group were told the following set of instructions intended to increase CMV in response: "In this study you will be asked to respond to several statements about your work and behaviors. The purpose of this study is to verify the strong relationships that have been found by previous researchers." Participants in the second experimental group were told the following set of instructions: "In this study you will be asked to respond to several statements about your work and behaviors. Several researchers have contributed items to this survey for separate purposes, so the questions should not be related. There is no right or wrong answer, so please respond as accurately and honestly as possible." As a manipulation check, after encountering the instructions, the participants were asked: "Which of the following correctly describes the purpose of this study?" The respondents had three response options: (a) The purpose of this study is to examine the relationships among workplace behaviors. As such, there is a clear purpose to this study; (b) Separate researchers built the content of this survey for separate purposes and the questions may or may not be related. As such, there is no clear purpose for this study; or (c) The purpose of this study is to verify the strong relationships that exist among workplace behaviors. As such, there is a clear purpose to this study. To participate in the survey, respondents

must have correctly selected either (a) for the control condition or (b) for the first experimental condition or (c) for the second experimental condition.

Those participants who receive traditional instructions with no proximal separation were considered the control group for evaluating inducing versus reducing instructions. The group who received CMV inducing instructions and no proximal separation was considered the CMV inducing group. The group who received CMV reducing instructions with proximal separation was considered the CMV reducing group. The CMV reducing group is intended to mimic studies that employ multiple a priori techniques to reduce CMV. The CMV inducing group is intended to mimic studies that no a priori techniques aimed at reducing CMV are employed. In other words, the intent is to compare the two extremes. The groups were also compared based upon video versus written instructions, regardless of item order, and random versus traditional item order, regardless of video versus written instructions.

Operationalization of the Research Variables

The research variables that were selected for this survey are those that previous studies and or meta-analyses have found to be susceptible to CMV effects (e.g. Moorman & Podsakoff, 1992; Kline et al., 2000; Podsakoff et al., 2003; Lance et al., 2009; Williams et al., 2010). The measures included in this study were pro-active personality, organizational commitment, organizational citizenship behaviors, perceived organizational support, core self-evaluations, job satisfaction, leader member exchange, affective trust, supervisory procedural justice, interpersonal deviance, and taking charge. Two presumed causes of CMV that can be directly measured were included: positive/negative affect and social desirability. In addition, two marker variable scales

were used to assess CMV: attitudes toward the color blue and attitudes toward private label brands.

Proactive personality is a stable disposition of a person to take "personal initiative in a broad range of activities and situations" (Seibert, Kraimer, & Crant, 2001). A person with a proactive personality tends to be unconstrained by situational forces and attempts to create environmental changes; these people seek opportunities, show initiative, and try to bring meaningful positive change to their environments (Marler, 2008; Fuller & Marler, 2009). Proactive personality may be subject to social desirability bias and positive and negative affect. However, it is important to note that this variable may be theoretically related to those constructs as those who have positive affect or are prone to socially desirable responding may view making positive change as desirable and be more likely to have or indicate having a proactive personality. The proactive personality measure used included 10 items adapted from Bateman & Crant (1993). The measure included a 7-point Likert scale with 1=strongly disagree and 7=strongly agree. It has previously been assessed in only a few CMV studies (Fuller & Marler, 2009; Castille & Simmering, 2013). See Table 3.1.

Table 3.1 Items for Proactive Personality

1. I am constantly on the lookout for new ways to improve my life.
2. Wherever I have been, I have been a powerful force for constructive change.
3. Nothing is more exciting than seeing my ideas turn into reality.
4. If I see something I don't like, I fix it.
5. No matter what the odds, if I believe in something I will make it happen.
6. I love being a champion for my ideas, even against others' opposition.

Table 3.1 (Continued)

7. I excel at identifying opportunities.
8. I am always looking for better ways to do things.
9. If I believe in an idea, no obstacle will prevent me from making it happen.
10. I can spot a good opportunity long before others can.

Organizational commitment is the "relative strength of an individual's identification with and involvement in a particular organization" (Porter, Steers, Mowday, & Boulain, 1974, p. 604). It was measured with a shortened 9-item scale (Mowday, Steers, & Porter, 1979). The measure includes a 7-point Likert scale with 1=strongly disagree and 7=strongly agree. Organizational commitment may be related to positive or negative affect and socially desirable responding as individuals who are more positive about their interactions or are more likely to engage in socially desirable behaviors may indicate or experience higher levels of commitment. It has previously been assessed in several CMV studies (Kline et al., 2000; Moorman & Podsakoff, 1992; Biderman et al., 2011; Podsakoff et al., 2012). See Table 3.2 for scale items.

Table 3.2 Items for Organizational Commitment

1. I am willing to put in a great deal of effort beyond that normally expected in order to help this organization be successful.
2. I talk up this organization to my friends as a great organization to work for.
3. I would accept almost any types of job assignment in order to keep working for this organization.
4. I find that my values and the organization's values are very similar.

Table 3.2 (Continued)

5. I am proud to tell others that I am part of this organization.
6. This organization really inspires the very best in me in the way of job performance.
7. I am extremely glad that I chose this organization to work for over others I was considering at the time I joined.
8. I really care about the fate of this organization.
9. For me, this is the best of all possible organizations for which to work.

Organizational citizenship behaviors (OCB) are functional, prosocial behaviors that are directed at individuals, a group, or an organization that are not a stated part of the job description and do not result in direct reward or punishment (Schnake, 1991). It was measured using a 21-item 5-point Likert scale with 1=strongly disagree and 5=strongly agree (Williams & Anderson, 1991). It has been assessed in multiple CMV studies (Kline et al., 2000; Podsakoff et al., 2012). The scale includes measures of OCB directed towards the organization (OCBO), organizational citizenship behavior directed towards individuals (OCBI), and in-role behavior (IRB). Organizational citizenship behaviors may also be related to positive and negative affect and socially desirable responding as individuals who are more positive or engage in socially desirable behaviors may be more likely to engage in OCBs. See Table 3.3 for scale items.

Table 3.3 Items for Organizational Citizenship Behaviors

Items for OCBI:

1. Helps others who have been absent
2. Helps others who have heavy work loads

Table 3.3 (Continued)

3. Assists supervisor with his/her work (when not asked)
4. Takes time to listen to co-workers' problems and worries
5. Goes out of way to help new employees
6. Takes a personal interest in other employees
7. Passes along information to co-workers

Items for OCBO:

1. Attendance to work is above the norm
2. Gives advance notice when unable to come to work
3. Takes undeserved work breaks (R)
4. Great deal of time spent with personal phone conversations (R)
5. Complains about insignificant things at work (R)
6. Conserves and protects organizational property
7. Adheres to informal rules devised to maintain order

Items for IRB:

1. Adequately completes assigned duties
2. Fulfills responsibilities specified in job description
3. Performs tasks that are expected of him/her
4. Meets formal performance requirements of the job
5. Engages in activities that will directly affect his/her performance
6. Neglects aspects of the job he/she is obligated to perform (R)
7. Fails to perform essential duties (R)

Items denoted with (R) are reverse scored.

Perceived organizational support (POS) is an employee's belief as to the extent to which their employer "values their contributions and cares about their well-being" (Eisenberger, Huntington, Hutchison, & Sowa, 1986, p. 501). Perceived organizational support was measured with a shortened 9-item 7-point Likert with 1=strongly disagree and 7=strongly agree. It has previously been assessed for CMV (Kline et al., 2000). Positive and negative affect and social desirability may be related to perceived organizational support in that employees who are positive may be more likely to feel supported. It was expected to be less correlated to social desirability because the item is a reflection of a measure that is less personal to the respondent, thus there is less motivation to engage in socially desirable responding. See scale items in Table 3.4.

Table 3.4 Items for Perceived Organizational Support

1. The organization strongly considers my goals and values.
2. Help is available from the organization when I have a problem.
3. The organization really cares about my well-being.
4. The organization is willing to extend itself in order to help me perform my job to the best of my ability.
5. Even if I did the best job possible, the organization would fail to notice. (R)
6. The organization cares about my general satisfaction at work.
7. The organization shows very little concern for me. (R)
8. The organization cares about my opinion.
9. The organization takes pride in my accomplishments at work.

Items denoted with an (R) are reverse scored.

Core self-evaluations (CSE) are the fundamental appraisals that an individual holds about himself and is comprised of self-esteem, generalized self-efficacy, emotional stability, and locus of control. Core self-evaluations were measured using a 12-item, 5 point Likert scale with 1=strongly disagree and 7=strongly agree (Judge, Erez, Bono, & Thoresen, 2003). It has previously only been examined in two studies for CMV as a full construct (Johnson et al., 2011; Judge et al., 2003); however, the individual scale components have been examined multiple times (Moorman & Podsakoff, 1992; Podsakoff et al., 2012; Lance et al., 2009). Core self-evaluations are likely to be influenced by both positive and negative affect and social desirability in that positive individuals are more likely to have positive assessments of themselves and society expects people to view themselves in a positive way. See Table 3.5 for items.

Table 3.5 Items for Core Self-Evaluations

1. I am confident I get the success I deserve in life.
2. Sometimes I feel depressed. (R)
3. When I try, I generally succeed.
4. Sometimes when I fail, I feel worthless. (R)
5. I complete tasks successfully.
6. Sometimes, I do not feel in control of my work. (R)
7. Overall, I am satisfied with myself.
8. I am filled with doubts about my competence. (R)
9. I determine what will happen in my life.
10. I do not feel in control of my success in my career. (R)
11. I am capable of coping with most of my problems.

Table 3.5 (Continued)

12. There are times when things look pretty bleak and hopeless to me. (R)

Items with an (R) are reverse scored.

Job satisfaction is an individual's general feelings toward their job or aspects of their job (Cammann, Fichman, Jenkins, & Klesh, 1983). Job satisfaction was measured using a 3-item 7 point Likert scale where 1=strongly disagree and 7=strongly agree. It has been previously examined for CMV effects in several studies (Kline et al., 2000; Simmering et al., under review; Podsakoff et al., 2012; Moorman & Podsakoff, 1992; Johnson et al., 2011; Lance, Dawson, Birkelback, & Hoffman, 2010; Yang, Mossholder, & Peng, 2009). Job satisfaction should be more strongly related to positive and negative affect than social desirability because job satisfaction is a reflection of the individual's feelings about their job rather than a more personal internal perception. In addition, positive individuals are more likely to feel satisfied with their job than negative individuals are. See Table 3.6 for scale items.

Table 3.6 Items for Job Satisfaction

1. All in all, I am satisfied with my job.

2. In general, I don't like my job. (R)

3. In general, I like working here.

Items denoted with (R) are reverse scored.

Leader member exchange (LMX) focuses on the relationship between supervisors and subordinates, assuming that the quality of the relationship influences subordinate's experience and attitudes towards the organization, therefore, positive LMX improves

performance (Liden & Graen, 1980). LMX (α=.92) was captured using Liden and Graen's (1980) 7-item measure. Items are on a multiple 5-point Likert response scale (Graen & Uhl-Bien, 1995) LMX has been assessed for CMV in several studies (Baltes et al., 2002; Podsakoff et al., 2012; Williams et al., 2010; Simmering et al., under review). Positive and negative affect should be more strongly related to LMX than social desirability because it reflects a measure that is external to the employee's perceptions of self. There is less personal stake, but positive individuals are more likely to perceive their interactions with their supervisors positively. See Table 3.7 for scale items.

Table 3.7 Items for LMX

1. Do you know where you stand with your leader . . . do you usually know how satisfied your leader is with what you do? (Does your member usually know)
   (1) Rarely, (2) Occasionally, (3) Sometimes, (4) Fairly Often, (5)Very Often
2. How well does your leader understand your job problems and needs?
   (1) Not a Bit, (2) A Little, (3) A Fair Amount, (4) Quite a Bit, (5) A Great Deal
3. How well does your leader recognize your potential?
   (1) Not at All, (2) A Little, (3) Moderately, (4) Mostly, (5) Fully
4. Regardless of how much formal authority he/she has built into his/ her position, what are the chances that your leader would use his/ her power to help you solve problems in your work?
   (1) None, (2) Small, (3) Moderate, (4) High, (5) Very High
5. Again, regardless of the amount of formal authority your leader has, what are the chances that he/she would "bail you out," at his/her expense?
   (1) None, (2) Small, (3) Moderate, (4) High, (5) Very High

Table 3.7 (Continued)

6. I have enough confidence in my leader that I would defend and justify his/ her decision if he/she were not present to do so.

   (1) Strongly Disagree, (2) Disagree, (3) Neutral, (4) Agree, (5) Strongly Agree

7. How would you characterize your working relationship with your leader?

   (1) Extremely Ineffective, (2) Worse Than Average, (3) Average, (4) Better Than Average, (5) Extremely Effective

Trust in supervisor measures an "employees' willingness to be vulnerable based on expectations that the intentions, words, or actions of their supervisor can be relied upon" (Poon, Rahid, & Othman, 2006). It includes a cognitive and an affective component. It was measured using the 10 items developed by Yang and Mossholder (2006). Trust in supervisor has been assessed for CMV previously (Simmering et al., under review; Yang et al., 2009). It is expected to be related to positive and negative affect and social desirability because positive people should be more willing to trust and perceive positive results. Those who have lower levels of social desirability or higher levels of negative affect may be less likely to trust or perceive positive outcomes. See Table 3.8 for scale items.

Table 3.8 Items for Trust

Cognitive Trust

1. I can depend on my supervisor to meet his/her responsibilities.
2. I can rely on my supervisor to do what is best at work.
3. My supervisor follows through with commitments he/she makes.

Table 3.8 (Continued)

4. Given my supervisor's track record, I see no reason to doubt his/her competence.
5. I'm confident in my supervisor because (s)he approaches work with professionalism.

Affective Trust

1. I'm confident that my supervisor will always care about my personal needs to work.
2. If I shared my problems with my supervisor, I know (s)he would response with care.
3. I'm confident that I could share my work difficulties with my supervisor.
4. I'm sure I could openly communicate my feelings to my supervisor.
5. I feel secure with my supervisor because of his/her sincerity.

Supervisory procedural justice (SPJ) is the employee's perception of the supervisor's fairness in determining outcomes (Leventhal, 1980). It was measured using the four-item scale by Rupp and Cropanzano (2002). Items were measured with a 7-point Likert scale with 1=strongly disagree and 7=strongly agree. SPJ has been previously examined for CMV (Simmering et al., under review; Yang et al., 2009). Positive and negative affect should be more strongly related to SPJ than social desirability as it is a perception of external characteristics; therefore, the employee is less likely to feel motivated to modify their responses. See Table 3.9 for scale items.

Table 3.9 Items for Supervisory Procedural Justice

1. I can count on my supervisor to have fair policies.
2. Where I work, my supervisor's procedures and guidelines are very fair.
3. My supervisor doesn't have any fair policies. (R)

Table 3.9 (Continued)

4. The procedures my supervisor uses to make decisions are not fair. (R)

Items denoted by (R) are reverse scored.

Interpersonal deviance (ID) is a type of organizational misbehavior that is directed towards individuals within an organization rather than the organization or organizational property (Robinson & Bennett, 1995). This survey used a 7-item interpersonal deviance scale by Bennett and Robinson (2000). Items ask the respondent how many times within the last year they engaged in certain behaviors. Responses are on a 7-point Likert scale with 1 (never), 2 (once a year), 3 (twice a year), 4 (several times a year), 5 (monthly), 6 (weekly), 7 (daily). It was expected that ID would be more strongly related to socially desirable responding because deviance is considered a non-socially desirable behavior. It was also expected to be more strongly related to negative affect than positive because individuals who experience negative affect are more likely to experience the stressor precursors to deviant behaviors. See Table 3.10 for scale items.

Table 3.10 Items for Interpersonal Deviance

1. Made fun of someone at work
2. Said something hurtful to someone at work
3. Made an ethnic, religious, or racial remark at work
4. Cursed at someone at work
5. Played a mean prank on someone at work
6. Acted rudely toward someone at work
7. Publicly embarrassed someone at work

Taking charge behavior (TC) is a form of proactive behavior that involves an individual making voluntary efforts to "effect organizationally functional change with respect to how work is executed within the contexts of their jobs, work units, or organizations" (Morrison & Phelps, 1999, 403). It was measured using a 10-item 5-point Likert scale where 1=strongly disagree and 5=strongly agree. Taking charge behavior is likely to be positively related to positive affect and negatively related to both social desirability and negative affect. Individuals who are positive and confident in themselves are more likely to promote positive change in their environments. While the behavior may be socially desirable, individuals who engage in socially desirable responding may be less likely to engage in TC because it requires going against what is considered acceptable in order to effect change. The items for TC can be found in Table 3.11.

Table 3.11 Items for Taking Charge

1. I often try to adopt improved procedures for doing my job.
2. I often try to change how my job is executed in order to be more effective.
3. I often try to bring about improved procedures for the work unit or department.
4. I often try to institute new work methods that are more effective for the company.
5. I often try to change organizational rules or policies that are nonproductive or counterproductive.
6. I often make constructive suggestions for improving how things operate within the organization.
7. I often try to correct a faulty procedure or practice.
8. I often try to eliminate redundant or unnecessary procedures.
9. I often try to implement solutions to pressing organizational problems.

Table 3.11 (Continued)

10. I often try to introduce new structures, technologies, or approaches to improve efficiency.

Social desirability is a measure of the tendency to select responses that are socially approved (Moorman & Podsakoff, 1992; Nunnally & Bernstein, 1994). This survey used the 11-item Balanced Inventory of Desirable Responding (BIDR) scale (Paulhus et al, 2003). The BIDR scale intends to measure two aspects of social desirability, an intentional form, impression management, and an unintentional form, self-deceptive enhancement. Given the experimental manipulations, it would be expected that the impression management items will have a greater magnitude of change than the self-deceptive enhancement items. See Table 3.12 for scale items.

Table 3.12 Items for BIDR Social Desirability Scale

Impression Management:

1. I am always courteous, even to people who are disagreeable.
2. Once in a while, I laugh at a dirty joke.
3. I sometimes try to get even, rather than forgive and forget.
4. I always apologize to others for my mistakes.
5. Sometimes at elections, I vote for candidates I know little about.
6. I am sometimes irritated by people who ask favors of me.

Self-Deceptive Enhancement:

1. People often disappoint me.

Table 3.12 (Continued)

2. Life is a strain for me most of the time.
3. I worry quite a bit over possible misfortunes.
4. I have several times given up doing something because I thought too little of my ability.
5. In a group of people, I have trouble thinking of the right things to talk about.

The positive affect and negative affect (PANAS) scale designed by Watson et al. (1988) was used to measure positive and negative affect or the likelihood that one will have a positive or negative worldview. Items ask respondents how they generally feel in order to assess trait levels rather than state levels. Responses are on a 5-point scale: very slightly or not at all (1), a little (2), moderately (3), quite a lot (4), extremely (5). The ten positive items are averaged to create a scale of positive affect, and the 10 negative items are averaged to create a scale of negative affect. See Table 3.13 for scale items.

Table 3.13 Items for Positive and Negative Affect Scale (PANAS)

Positive Affect

1. Interested
2. Excited
3. Strong
4. Enthusiastic
5. Proud
6. Alert
7. Inspired
8. Determined

Table 3.13 (Continued)

9. Attentive

10. Active

Negative Affect

11. Distressed

12. Upset

13. Guilty

14. Scared

15. Hostile

16. Irritable

17. Ashamed

18. Nervous

19. Jittery

20. Afraid

The proper selection of marker variable scales a priori is essential because it serves as a proxy for method variance. In order to be a proper marker variable, the respondent must be triggered to use the same psychological influences as in forming their answers to substantive survey items (Lindell & Whitney, 2001). To be effective, the marker variable must be perceptual, similar in format to the substantive variables, and theoretically unrelated to the substantive variables. Therefore, any variance accounted for by the marker is not substantive variance, but can be attributable to method variance. If the marker variable is not perceptual in nature, it cannot capture the underlying response factors. Further, if the marker variable can be theoretically related to the study variables, then any variance shared could be true variance and not solely attributable to method

variance. Two commonly used marker measures were selected for this study. Attitudes towards the color blue were measured using the four positively worded items from the scale, which was designed explicitly for use as a marker variable to measure CMV (Miller & Chiodo, 2008). Attitudes towards private label brands (brand attitudes) was used as a second marker variable scale (Burton, Lichtenstein, Netemeyer, & Garretson, 1998). The measures included a 5 point Likert scale with 1=strongly disagree and 5=strongly agree. See Table 3.14 for the marker variable scale items.

Table 3.14 Items for Marker Variable Scales

Attitudes towards the Color Blue
1. I prefer blue to other colors.
2. I like the color blue.
3. I like blue clothes.
4. I hope my next car is blue
Attitudes towards Private Label Brands
1. Buying private label brands makes me feel good.
2. I love it when private label brands are available for the product categories I purchase.
3. For most product categories, the best buy is usually the private label brand.
4. Considering value for the money, I prefer private label brands to national brands.
5. When I buy a private label brand, I always feel that I am getting a good deal.

Control variables are often needed because they may aid in the explanation of variance of the outcome variables that are not related to the independent variables. By using control variables, the variance is controlled; as such, the relationships found can be

better understood. The demographic control variables included in this study were age, ethnicity, employment status, and education level all of which are frequently employed control variables that have been shown to have a variety of relationships with common organizational scales. While this study did not specifically address control variables or demographic differences, they were included as a non-hypothesized research interest to determine whether demographic variables created differences in CMV findings.

In addition, to identify low-quality data, attention check items were used to ensure the respondents remained attentive throughout the survey. Three attention check items were randomly interspersed among the substantive questions, asking the respondent to mark either "Agree," "Strongly Agree," and "Disagree" for the answer to that particular item. Using these attention checks helped to ensure the quality of the data in that it reduced the prevalence of random or careless responders in the final data set.

Number of Needed Observations

In order to be considered meaningful, the difference in the effect size needed was determined to be approximately .5, a medium effect size. With an α=.05, and a power=.70, 50 observations per treatment were needed (Cohen, 1988). With a statistical power=.80, 64 observations per treatment were needed. Therefore, the quota for the number of observations to collect was between 50 and 64 per treatment condition, for a minimum of 600 total respondents. Due to the random assignment of respondents to the groups, the final sample was uneven, with between 54 and 82 responses for each of the twelve experimental conditions.

Data Collection

The data used in this study was collected via Amazon.com's Mechanical Turk (MTurk) workers, with the survey as the "Human Intelligence Task" (HIT). MTurk has been used as a data source in a variety of applications (Castille & Simmering, 2013; Simmering et al., under review; Behrend, Sharek, & Meade, 2011; Buhrmester, Kwang, & Gosling, 2011 and has been found to be at least as reliable as more traditionally collected data (Buhrmester et al., 2011). MTurk data has been found to be equivalent to student samples in attentive responding characteristics, more diverse in personality, education, and ethnicity, and more susceptible to social desirability (Behrend et al., 2011; Buhrmester et al., 2011). The higher level of social desirability for the purposes of this study was actually desirable, as it should have only increased the amount of CMV in the data, providing ideal data for the application of remedies to reduce its bias. MTurk restricts workers to only those over 18 years of age who can provide a United States tax identification number. In addition, workers were restricted to those who were employed, at least part time, and lived in the U.S. Further restrictions included only those workers who had achieved a 97% work acceptance rate on MTurk and who had completed at least 100 HITs.

Statistical Methodology

Several statistical tools were used to analyze the data. T-tests and z-tests were used to examine differences in treatment groups and assess social desirability and positive and negative affect. In order to address a post hoc research question aimed at eliminating alternative explanations for the findings, the three-phase comprehensive CFA marker technique developed by Williams, et al (2010) was used to assess CMV.

CHAPTER FOUR

ANALYSIS AND RESULTS

The primary purpose of this study was to examine the effects of experimental manipulations on CMV response styles. This chapter will provide the results of this study. Chapter Five will provide a discussion of the findings, conclusions, and implications that can be drawn from these results.

Demographic Characteristics of Sample

As noted in Chapter Three, the participants in this study were recruited from Amazon.com's Mechanical Turk workers living in the United States. Workers were paid $1.00 to complete the survey and received a randomly generated code upon completion of the survey to receive compensation. The survey took an average of 14 minutes to complete. The initial number of respondents included 1,742 individuals. Once workers who failed to answer the manipulation check correctly were excluded, 846 respondents remained. Because of the nature of this manipulation check, more respondents than would be typical were excluded from continuing the survey. Based upon emails received by respondents who failed the initial manipulation check and those who completed the survey, it is likely that many MTurk workers respond to surveys based upon their own feelings without regard to survey instructions. In other words, the workers were not aware that they needed to pay careful attention to the instructions to continue. Although

the substantial reduction in respondents could be attributed to a bad manipulation, it is much more likely that the respondents who were able to respond to the manipulation check accurately were more susceptible to CMV bias as they recognized the differences in the instructions. Whether the recognition of instructional differences translated into differed responses is unknown since those who failed the manipulation check were not allowed to continue with the survey. After removing partially completed surveys and duplicate responses, 796 respondents remained. The final sample included 772 respondents after removing those who had failed more than one attention check.

Demographic questions asked respondents to identify their primary racial affiliation. White/Caucasian respondents made up 74.4% of the total respondents, Asian/Pacific Islanders made up 10.1% of the respondents, and Black/African Americans made up 8.5% of the respondents. Hispanics/Latin Americans made up 4.7% of respondents, and the remaining respondents classified themselves as other (2.1%) or preferred not to answer (0.3%). The average age of the respondent was 35 years (*s. d.* 11.08), with the youngest respondent being 18 and the oldest being 72. The education level of the respondents included 39.8% with a Bachelor's degree, 9.1% with a high school diploma or GED, 8.4% with an Associate's degree, and 16.6% with a Master's degree or higher. An item asking respondent gender was erroneously omitted from the survey.

Descriptive Statistics

SPSS was used to calculate the means, standard deviations, Cronbach's alpha reliabilities, and zero-order correlations for the sample scales. The results of the full data set are presented in Table 4.1. This table shows that most of the variables are

statistically significantly correlated, with the exception of the markers and demographic variables. The exception of the marker variables is important, as they were included as a measure of CMV and the reduced number of significant correlations provides an initial indication that CMV was not present in the data set as a whole (Simmering et al., unpublished manuscript). Scales are abbreviated in the tables as follows: proactive personality (PP), core-self evaluations (CSE), leader member exchange (LMX), supervisory procedural justice (SPJ), taking charge (TC), attitudes towards the color blue (ACB), organizational commitment (OC), perceived organizational support (POS), job satisfaction (JS), interpersonal deviance (ID), attitudes towards store brands (BA), organizational citizenship behaviors (OCB-I, OCB-O, and OCB-IRB), impression management (IM), social desirability (SD), positive affect (PA), negative affect (NA), employment (EMP), and education (ED).

Table 4.1 Descriptives and Correlation Table

Variable	Mean	s.d.	PP	CSE	LMX	Trust	SPJ	TC	ACB	OC	POS	JS	ID
PP	3.71	.62	(.89)										
CSE	3.48	.69	.49**	(.89)									
LMX	3.39	.84	.25**	.40**	(.91)								
Trust	3.65	.90	.25**	.38**	.85**	(.96)							
SPJ	4.84	.91	.16**	.35**	.72**	.79**	(.91)						
TC	3.64	.71	.57**	.31**	.24**	.21**	.15**	(.92)					
ACB	3.37	.74	.09*	.04	.03	0.05	-.02	.03	(.77)				
OC	3.37	.89	.42**	.42**	.54**	.58**	.44**	.42**	.10**	(.93)			
POS	3.79	.92	.32**	.43**	.67**	.70**	.59**	.28**	.06	.78**	(.94)		
JS	5.03	1.59	.29**	.43**	.61**	.63**	.54**	.25**	.05	.79**	.74**	(.94)	
ID	1.70	.99	.02	-.09*	.01	-.04	-.16**	0.06	.07	-.04	-.08*	-.04	(.87)
BA	3.37	.81	.11**	.07*	.07	.06	-.02	.12**	.10*	.14**	.04	.08*	.06
OCB-I	3.77	.70	.36**	.23**	.33**	.26**	.21**	.47**	.13**	.40**	.32**	.28**	-.03
OCB-O	4.15	.58	.18**	.30**	.22**	.18**	.34**	.17**	.03	.22**	.21**	.22**	-.75**

Table 4.1 (Continued)

OCB-IRB	4.37	.54	.18**	.25**	.21**	.15**	.29**	.18**	-.04	.10**	.10**	.13**	-.23**
IM	3.38	.46	.04	-.16**	-.04	-0.03	-.12**	.04	.03	.00	-.06	-.03	.22**
SD	2.73	.89	-.32	-.73**	-.32**	-.31**	-.29**	-.21**	.02	-.33**	-.36**	-.37**	.17**
PA	3.23	.90	.50**	.44**	.32**	.26**	.17**	.34**	.18**	.43**	.37**	.37**	.10**
NA	1.46	.70	-.18	-.51**	-.16**	-.20**	-.21**	-.08*	.09*	-.20**	-.23**	-.22**	.30**
EMP	n/a	n/a	.07	.18*	.10**	.09*	.04	.10**	.05	.10**	.08*	.10**	.09**
Age	34.79	11.08	-.14**	.05	.00	-.04	-.03	-.01	0.00	-.03	-.01	.01	-.18**
Race	n/a	n/a	.09*	-.05	.02	.01	-.07*	.06	.09*	.08*	.02	.01	.19**
ED	n/a	n/a	.01	.05	-.00	-.02	-.01	.09*	-.01	.07*	.03	.06	.02

Table 4.1 (Continued)

Variable	BA	OCB-I	OCB-O	OCB-IRB	IM	SD	PA	NA	EMP	Age	Race
BA	(.90)										
OCB-I	.16**	(.86)									
OCB-O	.01	.34**	(.77)								
OCB-IRB	.04	.30**	.61**	(.80)							
IM	.08*	.08*	-.14**	-0.04	(.57)						
SD	.02	-.19**	-.29**	-.21**	.27**	(.81)					
PA	.14**	.36**	.18**	.11**	.01	-.37**	(.94)				
NA	.03	-.06	-.26**	-.21**	.14**	.51**	-.14**	(.94)			
EMP	.02	.03	.03	.05	-.01	-.16**	.10**	-.05			
Age	.00	.09*	.18**	.13**	-.15**	-.15**	.06	-.14**	.09*		
Race	.04	.01	-.18**	-.19**	.06	.03	.17**	.10**	.04	-.17**	
ED	-.05	.02	-.03	-.01	.05	-.05	.04	.05	.11**	.07	.12**

*= p<.05; **=p<.0n

N=772

Alphas are reported on the diagonal

Independent Samples T-Tests and Z-tests

In order to assess the difference between the experimental treatment groups, independent samples t-tests, Fisher's z-tests, and ANOVA were used. The first step in testing the hypotheses was to calculate correlation tables for the different experimental groups to examine whether any of the correlations changed based upon the experimental conditions.

To gain a better understanding of which groups experienced significant changes based upon the experimental condition, Fisher's z-test scores were calculated using the program provided by VassarStats.net (Lowery, 2014) to assess the significance of the different correlations between each of the two comparative conditions. Z-tests are a type of discriminant analysis used for testing the hypotheses that the group means of two or more groups are equal (Hair, Black, Babin, & Anderson, 2010). Fisher's z-test provides a transformation of the correlation values to a normal distribution. The correlations were compared for each of the treatment conditions with either the CMV control group or the opposing treatment. For example, the correlations between proactive personality and each of the other variables in the CMV control condition were compared with the correlations between proactive personality and each of the other variables in the CMV reducing condition to determine whether the correlations were significantly different. The correlations across the conditions that showed a z-score value of $p<.05$ are bolded in the correlation tables 4.1-4.5 to indicate that there was a significant difference in the correlations between the conditions for that particular comparison. Only a handful of the measures indicated a significant difference across the conditions. The CMV reducing condition versus control condition showed the largest number of significant z-score

differences; however, of those, only a small subset changed in significance rather than just magnitude.

Table 4.2 shows the CMV control versus the CMV reducing condition. Based upon the correlations and z-tests, the scales that seemed the most susceptible to CMV manipulation were OCB-IRB, LMX, supervisory procedural justice, interpersonal deviance, and negative affect. Scales with no significant differences included taking charge, job satisfaction, brand attitudes, and OCB-I. Though not an explicitly stated research objective, this finding provided initial support for the congeneric perspective of CMV that all scales are not equally affected and most scales do not appear to be significantly affected in spite of the differences.

Table 4.3 shows the results of the CMV control versus the CMV inducing conditions. The findings were similar to those of the CMV reducing condition in that both LMX and OCB-IRB indicated the most significant differences. However, interpersonal deviance showed no significant differences across the treatment conditions. Other scales that showed no differences included taking charge, organizational commitment, job satisfaction, and OCB-O. The magnitudes of the majority of the relationships increased significantly, but most relationships did not change in significance. This finding also provides support for the congeneric perspective of CMV.

Table 4.2 Correlations between Control Group and CMV Reducing Treatment (Control Values on Bottom)

Variable	PP	CSE	LMX	Trust	SPJ	TC	ACB	OC	POS	JS	ID	BA	OCB-I
(PP		.62**	**.36****	**.35****	**.27****	.65**	.10	.48**	.44**	.31**	**-00**	.21*	.42**
CSE	.46**		**.51****	.46**	.40**	.32**	.08	.47**	.48**	.40**	-.04	.23**	.16
LMX	**.07**	**.26****		.88**	**.79****	.22*	.28**	**.71****	**.75****	.64**	-.11	.14	.34**
Trust	**.1**	.31**	.81**		.83**	.22*	.30**	**.69****	**.78****	.63**	-.04	.12	.25**
SPJ	**.01**	.33**	**.67****	.80**		.19*	.28**	.60**	**.74****	.61**	**-.08**	.03	.19*
TC	.62**	.33**	.15	.11	.08		.06	.42**	.29**	.22*	.17	.21*	.49**
ACB	.19*	.10	.16	.15	.04	.13		.19*	.26**	.18*	.08	.09	.11
OC	.46**	.36**	**.41****	**.48****	**.37****	.46**	.15		.80**	.80**	-.04	.19*	.36**
POS	.26**	.32**	**.51****	**.56****	**.49****	.23**	.13	.75**		.73**	.02	.10	.28**
JS	.36**	.43**	.45**	.55**	.48**	.28**	.12	.79**	.73**		.04	.07	.20*
ID	**.03**	-.23**	-.09	-.18*	**-.31****	-0.002	.05	-.11	-.17*	-.17*		.13	.06
BA	.14	.06	-.01	.02	-.03	.14	.29**	.23**	.04	.12	.03		.20*

Table 4.2 (Continued)

OCB-I	.39**	.31**	.39**	.3**	.23**	.50**	.26**	.44**	.37**	.38**	-.11	.23**	
OCB-O	.07	.26**	.18*	.16*	.35**	.20*	.03	.14	.20*	.23**	**-.52****	.03	.46**
OCB-IRB	.01	.18*	.13	.05	.24**	.17*	-.09	-.06	.00	.03	**-.40****	-.02	.28**
IM	.19*	-.13	-.02	.00	-.13	.15	.03	.06	-.07	.02	.17*	.09	.03
SD	-.35	-.75**	-.24**	-.34**	-.33**	-.23**	-.04	-.34**	-.36**	-.43**	.29**	.05	-.24**
PA	.58**	.51**	.29**	.28**	.13	.418**	.30**	.47**	.42**	.48**	.05	.16	.46**
NA	-.19*	-.54**	-.10	-.29**	**-.30****	-.13	.01	-.25**	-.21*	-.30**	**.40****	.03	-.08
EMP	.16	.20*	.04	.06	-.02	.21*	.12	.07	.03	.13	0.06	.10	.12
Age	-.16*	.04	-.05	-.06	.05	-.02	-.05	-.11	-.06	.04	-.16*	.10	.11
Race	.20*	.03	.05	.12	-.04	.07	**.26****	**.17***	.07	.10	**.20***	.15	.07
ED	.08	.09	.15	.08	.05	.09	.06	.19*	.19*	.19*	.01	-.07	.11

Table 4.2 (Continued)

Variable	OCB-O	OCB-IRB	IM	SD	PA	NA	EMP	Age	Race	ED
PP	.20*	.21*	-0.002	-.43**	.54**	-.32**	.01	-.14	.07	-.02
CSE	.24**	.19*	-.10	-.71**	.51**	-.47**	.08	.00	-.08	.00
LMX	.32**	.17	-.05	-.33**	.40**	-.16	.04	.08	-.03	.09
Trust	.24**	.07	-.03	-.26**	.32**	-.13	.02	-.01	-.02	.07
SPJ	.32**	.12	-.06	-.23*	.32**	**-.06**	.08	-.01	-.012	.09
TC	.06	.20*	-.03	-.24**	.34**	-.08	.01	-.04	.16	.17
ACB	.09	-.07	-.10	.10	.13	.04	.11	.03	**.04**	-.12
OC	.32**	.13	-.07	-.33**	.41**	-.29**	.02	.04	**-.06**	.06
POS	.19*	-.04	-.04	-.28**	.36**	-.22*	-.03	.01	-.04	-.03
JS	.23*	.02	-.04	-.25**	.28**	-.11	-.06	.04	-.10	.03
ID	**-.24****	**.05**	.16	.09	.06	**.06**	.07	-.21*	**0**	.05
BA	.13	.18*	.03	-.15	.08	-.14	-.03	-.09	.04	-.15
OCB-I	.29**	.32**	.09	-.26**	.43**	-.11	.06	.15	.07	.05
OCB-O		.47**	-.09	-.24**	.25**	-.17	0	.07	-.13	-.04

Table 4.2 (Continued)

OCB-IRB	.63**		-.03	-.18*	**.31****	0	.13	.10	**-.06**	**.28****
IM	-.13	-.01		.15	.05	.13	.012	**-.15**	.01	.15
SD	-.26**	-.16*	.31**		-.44**	**.41****	-.14	0	-.03	-.02
PA	.11	**.02**	.05	-.45**		-.16	.04	-.06	.13	.04
NA	-.23**	-.17*	.11	**.59****	-.26**		-.08	-.17	.12	.15
EMP	-.02	.07	-.03	-.15	.22**	-.09		-.08	.03	.25**
Age	.22**	.21*	**-.40****	-.18*	.13	-.08	.25**		-.17	.04
Race	-.28**	**-.30****	.11	0	.28**	-.04	11	-.19*		.01
ED	.04	**-.03**	.03	-.11	.11	.03	.09	0	.18*	

*= p<.05; **=p<.01
Control N=146; Reducing N=122
Correlations with significant z-score differences are bolded

Table 4.3 Correlations for CMV Control Group versus CMV Inducing Treatment (Control Values on Bottom)

Variable	PP	CSE	LMX	Trust	SPJ	TC	ACB	OC	POS	JS	ID	BA	OCB-I
PP		.49**	**.30****	.30**	.22**	.54**	.13	.34**	.23**	.22**	-.01	.13	.35**
CSE	.46**		**.46****	.50**	.46**	.32**	.00	.46**	.50**	.49**	-.16	.05	.25**
LMX	**.07**	**.26****		**.88****	.71**	.27**	-.10	.58**	**.75****	**.68****	.02	.11	.43**
Trust	.10	.31**	.81**		.83**	.19*	**-.21***	.61**	**.77****	**.70****	-.08	.10	.35**
SPJ	**-.01**	.33**	.67**	.80**		.23**	**-.22****	.49**	.64**	.60**	-.20*	.06	.35**
TC	.62**	.33**	.15	.11	.08		.05	.33**	.18*	.20*	-.05	.10	.50**
ACB	.19*	.10	.16	**.15**	**.05**	.13		-.00	-.07	-.17*	.13	**-.05**	.11
OC	.46**	.36**	.41**	.48**	.37**	.46**	.15		.75**	.76**	-.03	.11	.48**
POS	.26**	.32**	**.51****	**.56****	.50**	.23**	.13	.75**		.74**	-.06	.01	.41**
JS	.36**	.43**	**.45****	**.55****	.48**	.28**	.12	.79**	.73**		-.00	.09	.26**
ID	.03	-.23**	-.10	-.18*	-.31**	-.00	.05	-.11	-.17*	-.17*		.06	-.10
BA	.14	.06	-.01	.02	-.03	.14	**.29****	.23**	.03	.12	.03		.14

Table 4.3 (Continued)

OCB-I	.39**	.31**	.39**	.32**	.23**	.50**	.26**	.44**	.37**	.38**	-.11	.23**	
OCB-O	.07	.26**	.18*	.16*	.35**	.20*	.03	0.14	.20*	.23**	-.53**	.03	.46**
OCB-IRB	**.00**	.18*	.13	**.05**	**.24****	.17*	-.09	**-.07**	.00	.03	-.40**	-.02	.28**
IM	.20*	-.13	-.02	-.00	-.13	.15	.03	.06	-.07	.02	.17*	.09	.03
SD	-.36**	-.75**	-.24**	-.34**	-.33**	-.23**	-.04	-.34**	-.36**	-.43**	.29**	.05	-.23**
PA	.58**	.51**	.29**	.28**	.13	.42**	.30**	.47**	.42**	.48**	.05	.16	**.46****
NA	-.19*	-.54**	-.10	-.29**	-.30**	-.13	.01	-.25**	-.21*	-.30**	.40**	.03	-.08
EMP	.16	.20*	.04	.06	-.02	.21*	.12	.07	.04	.13	.06	.10	.12
Age	-.16*	.04	-.05	-.06	.05	-.02	-.05	-.11	-.06	.04	-.16*	.10	.11
Race	.20*	.03	.05	.12	-.04	.07	.26**	.17*	.07	.10	.20*	**.15**	.07
ED	.08	.09	**.15**	.08	.05	.09	.06	.19*	**.19***	**.20***	.01	-.07	.11

Table 4.3 (Continued)

Variable	OCB-O	OCB-IRB	IM	SD	PA	NA	EMP	Age	Race	ED
PP	.23**	**.26****	-.01	-.25**	.46**	-.17*	-.09	-.16	.01	-.08
CSE	.33**	.31**	-.23**	-.76**	.31**	-.53**	.12	.00	-.12	.07
LMX	.26**	.30**	-.05	-.43**	.31**	-.27**	.11	-.04	.02	**-.16**
Trust	.29**	**.29****	-.09	-.45**	.29**	-.37**	.09	-.05	-.03	-.11
SPJ	.44**	**.45****	-.17	-.41**	.12	-.39**	.04	-.06	-.16	-.14
TC	.30**	.34**	.05	-.13	.42**	-.16	.06	.01	-.07	-.03
ACB	-.06	-.14	.15	.04	.11	.05	.08	.00	.16	.10
OC	.33**	**.23****	-.02	-.41**	.42**	-.28**	.11	-.08	.02	-.03
POS	.29**	.22*	-.05	-.48**	.30**	-.32**	.10	-.02	**.06**	**-.06**
JS	.29**	.24**	-.08	-.48**	.31**	-.31**	.10	-.11	.04	-.00
ID	-.35**	-.34**	.14	.20*	.15	.31**	-.05	-.11	.27**	-.00
BA	.07	.12	-.02	-.00	.26**	.07	-.04	-.04	**-.08**	-.11
OCB-I	.35**	.41**	.11	-.18*	**.24****	-.11	.03	.06	-.09	-.07
OCB-O		.73**	-.10	-.19*	.12	-.34**	.05	.14	-.25**	-.07

Table 4.3 (Continued)

Variable	OCB-O	OCB-IRB	IM	SD	PA	NA	EMP	Age	Race	ED
OCB-IRB	.63**		-.04	-.21*	-.01	**-.40****	-.00	.16	-.30**	-.10
IM	-.13	-.01		.28**	.02	.05	.15	**.00**	.03	.07
SD	-.26**	-.16*	.31**		**-.24****	.47**	-.18*	-.08	.04	-.05
PA	.11	.02	.05	**-.45****		-.08	.07	-.03	.22*	.05
NA	-.23**	**-.17***	.11	.59**	-.26**		-.16	-.19*	**.19***	.04
EMP	-.02	.07	-.03	-.15	.22**	-.09		.27**	.01	.12
Age	.22**	.21*	**-.40****	-.18*	.13	-.08	.25**		-.17*	.12
Race	-.28**	-.30**	.11	.00	.28**	**-.04**	0.11	-.19*		.15
ED	.04	-.03	.03	-.11	.11	.03	.09	-.00	.18*	

*= p<.05; **=p<.01

Control N=146; Inducing=N=139

Correlations with significant *z*-scores are bolded

Table 4.4 depicts the comparisons between the CMV inducing condition and the CMV reducing condition. This comparison was done to assess the difference between a study that employed all of the standard remedies with one that employed none of the standard remedies. Similar to the two previous comparisons, OCB-IRB had a large number of significant differences. The rest of the scales exhibited only a small number of significant differences. Those relationships that existed were much stronger across these two experimental groups than across either with the control group. The CMV inducing condition showed much higher correlations on average than the CMV reducing condition.

Table 4.5 depicts the comparison between the experimental groups that received written instructions versus video instructions. Interestingly, the only two scales that showed many significant scores were proactive personality and race. Proactive personality showed relatively strong differences in the CMV control versus CMV reducing conditions, but not to the same level shown here. Most of the substantive variables showed significant decreases in correlations for those who received written instructions. The significant difference for race should be interpreted cautiously given the nature of it as a demographic/control variable. While the values are essentially meaningless, the differences indicate that certain racial populations may be more prone to change their responses based upon the richness of the media.

Table 4.6 shows the relationships between experimental groups that received randomly presented question blocks and those who received traditional item order. In line with previous findings, only OCB-IRB and negative affect showed several significant differences. Other scales showed only one or two significant differences.

Most of the relationships decreased in magnitude for those who received random item order, though the significance of the scale remained significant or did not change

To investigate the influences of the experimental conditions further, t-tests and ANOVAs were used. T-test results were similar to the z-test results and indicated that very few scales were different based upon treatment group. For the control group versus the inducing experimental condition, no significant differences were found between groups for any of the scales at $p<.05$. For the control group versus the reducing experimental condition, only the scales BIDR-SD ($t=2.08$) and Negative affect ($t=4.10$) were significantly different at $p<.05$. For the CMV inducing versus the CMV reducing group, only negative affect ($t=-2.67$) was significantly different at $p<.05$. For the experimental group based upon media richness theory, with one group receiving written instructions and one receiving video, only brand attitudes ($t=2.60$) and interpersonal deviance ($t=2.70$) significantly differed at $p<.05$. Proximal separation proved to be the most effective procedural remedy at creating significant differences between groups with LMX ($t=2.33$), trust ($t=2.77$), organizational commitment ($t=2.00$), positive affect ($t=1.98$), and negative affect ($t=3.19$) all indicating significant differences at $p<.05$. ANOVA results showed the same scales as being significantly different between groups. The results of the t-tests and ANOVA were not wholly consistent with those of the z-test as they found less significant differences across the experimental conditions and found no significant differences for the OCB scales.

Table 4.4 Correlations for CMV Inducing Treatment versus CMV Reducing Treatment (Reducing on Bottom)

Variable	PP	CSE	LMX	Trust	SPJ	TC	ACB	OC	POS	JS	ID	BA	OCB-I
PP		.49**	.30**	.30**	.22**	.54**	.13	.34**	.23**	.22**	-.01	.13	.35**
CSE	.62**		.46**	.50**	.46**	.32**	.00	.46**	.50**	.49**	-.16	.05	.25**
LMX	.36**	.51**		.88**	.71**	.27**	**-.13**	.58**	.75**	.68**	.02	.11	.43**
Trust	.35**	.46**	.88**		.83**	.19*	-.21*	.61**	.77**	.70**	-.08	.10	.35**
SPJ	.27**	.40**	.79**	.83**		.23**	-.22**	.49**	.64**	.60**	-.20*	.06	**.35****
TC	.65**	.32**	.22*	.22*	.19*		.05	.33**	.18*	.20*	-.05	.10	**.50****
ACB	.10	.08	**.28****	.30**	.28**	.06		-.00	**-.07**	-.17*	.13	-.05	.11
OC	.48**	.47**	.71**	.69**	.60**	.42**	.19*		.75**	.76**	-.03	.11	.48**
POS	.44**	.48**	.75**	.78**	.74**	.29**	**.26****	.80**		.74**	-.06	.01	.41**
JS	.31**	.40**	.64**	.63**	.61**	.22*	.18*	.80**	.73**		-.00	.09	.26**
ID	0	-.04	-.11	-.04	-.08	.17	.08	-.04	.02	.04		.06	-.10
BA	.21*	.23**	.14	.12	.03	.21*	.09	.19*	.10	.07	.13		.14
OCB-I	.42**	.16	.34**	.25**	**.19***	.49**	.11	.36**	.28**	.20*	.06	.20*	

Table 4.4 (Continued)

OCB-O	.20*	.24**	.32**	.24**	.32**	**.06**	.09	.32**	.19*	.23*	-.24**	.13	.29**
OCB-IRB	.21*	.19*	.17	.07	**.12**	.20*	-.07	.13	**-.04**	**.02**	**.05**	.18*	.32**
IM	0	-.10	-.05	-.03	-.06	-.03	-.10	-.07	-.04	-.04	0.16	.03	.09
SD	-.43**	-.71**	-.33**	-.26**	-.23*	-.24**	.10	-.33**	-.28**	**-.25****	.09	**-.15**	-.26**
PA	.54**	.51**	.40**	.32**	.32**	.34**	.13	.41**	.36**	.28**	.06	.08	.43**
NA	-.32**	-.47**	-.16	**-.13**	**-.06**	-.08	.04	-.29**	-.22*	-.11	**.06**	-.14	-.11
EMP	.01	.08	.04	.02	.08	.01	.11	.02	-.03	-.06	.07	-.03	.06
Age	-.14	0	.08	-.01	-.01	-.04	.03	.04	.01	.04	-.21*	-.09	.15
Race	.07	-.08	-.03	-.02	-.012	.16	.04	-.06	-.04	-.10	**0**	.04	.07
ED	-.02	0	.09	.07	.09	.17	-.12	.06	-.03	.03	.05	-.15	.05

Table 4.4 (Continued)

Variable	OCB-O	OCB-IRB	IM	SD	PA	NA	EMP	Age	Race	ED
PP	.23**	.26**	-.01	-.25**	.46**	-.17*	-.09	-.16	.01	-.08
CSE	.33**	.31**	-.23**	-.76**	.31**	-.53**	.12	.00	-.12	.07
LMX	.26**	.30**	-.05	-.43**	.31**	-.27**	.11	-.04	.02	-.16
Trust	.29**	.29**	-.09	-.45**	.29**	**-.37****	.09	-.05	-.03	-.11
SPJ	.44**	**.45****	-.17	-.41**	.12	**-.39****	.04	-.06	-.16	-.14
TC	.30**	.34**	.05	-.13	.42**	-.16	.06	.01	-.07	-.03
ACB	-.06	-.14	**.15**	.04	.11	.05	.08	.00	.16	.10
OC	.33**	.23**	-.02	-.41**	.42**	-.28**	.11	-.08	.02	-.03
POS	.29**	**.22***	-.05	-.48**	.30**	-.32**	.10	-.02	.06	-.06
JS	.29**	**.24****	-.08	**-.48****	.31**	-.31**	.10	-.11	.04	-.00
ID	-.35**	**-.34****	.14	.20*	.15	**.31****	-.05	-.11	**.27****	-.00
BA	.07	.12	-.02	**-.00**	.26**	.07	-.04	-.04	-.08	-.11
OCB-I	.35**	.41**	.11	-.18*	.24**	-.11	.03	.06	-.09	-.07

Table 4.4 (Continued)

OCB-O		**.73****	-.10	-.19*	.12	-.34**	.05	.14	-.25**	-.07
OCB-IRB	**.47****		-.04	-.21*	**-.01**	-.40**	-.00	.16	**-.30****	**-.10**
IM	-.09	-.03		.28**	.02	.05	.15	.00	.03	.07
SD	-.24**	-.18*	.15		-.24**	.47**	-.18*	-.08	.04	-.05
PA	.25**	**.31****	.05	-.44**		-.08	.07	-.03	.22*	.05
NA	-.17	0	.13	.41**	-.16		-.16	-.19*	.19*	.04
EMP	0	.13	.012	-.14	.04	-.08		.27**	.01	.12
Age	.07	.10	-.15	0	-.06	-.17	-.08		-.17*	.12
Race	-.13	**-.06**	.01	-.03	.13	.12	.03	-.17		.15
ED	-.04	**.28****	.15	-.02	.04	.15	.25**	.04	.01	

*= p<.05; **=p<.01

Inducing N=139; Reducing N=122

Correlations with significant z-scores are bolded

Table 4.5 Proximal Separation Correlations (No Separation/Traditional Order on Bottom)

Variable	PP	CSE	LMX	Trust	SPJ	TC	OC	POS	JS	OCB-I	OCB-O	OCB-IRB	ACB
PP		.52**	.29**	.28**	.20**	.57**	.43**	.38**	.29**	.36**	.21**	.21**	.03
CSE	.47**		.44**	.40**	.34**	.33**	.47**	**.49****	.46**	.25**	.28**	.23**	.02
LMX	.22**	.35**		.85**	.75**	.23**	.57**	.69**	.69**	.29**	.25**	.22**	.06
Trust	.21**	.36**	.85**		.77**	.23**	.59**	.69**	.63**	.22**	.18**	.16**	.13*
SPJ	.11*	.35**	.69**	.79**		.14**	.47**	.61**	.56**	.16**	.32**	.28**	.04
TC	.56**	.28**	.24**	.19**	.16**		.43**	.31**	.27**	.44**	.12*	.15**	-.04
OC	.41**	.36**	.51**	.56**	.41**	.40**		.79**	.79**	.36**	.28**	.14**	.10
POS	.27**	**.37****	.65**	.69**	.56**	.24**	.76**		.74**	.26**	.21**	.08	.08
JS	.281*	.41**	.58**	.63**	.52**	.24**	.79**	.74**		.25**	.22**	.12*	.11*
OCB-I	.36**	.21**	.37**	.29**	.25**	.49**	.43**	.37**	.31**		.34**	.29**	**.05**
OCB-O	.16**	.32**	.19**	.19**	.37**	.22**	.17**	.21**	.23**	.34**		**.53****	.09
OCB-IRB	.16**	.27**	.20**	.15**	.30**	.22**	.06	.11*	.13**	.30**	**.68****		.01
ACB	.14**	.05	.00	-.03	-.08	.10*	.09	.03	-.00	**.21****	-.02	-.08	

Table 4.5 (Continued)

BA	.13**	.03	.04	.03	-.02	.10*	.16**	.03	.11*	.20**	.02	.04	.14**
IM	.11*	-.19**	-.06	-.07	**-.17****	.10*	.03	-.08	-.02	.11*	-.14**	-.04	.05
SD	-.31**	-.74**	-.31**	-.35**	**-.36****	-.16**	-.32**	-.38**	-.40**	-.15**	-.29**	-.22**	.02
PA	.52**	.40**	.27**	.23**	.11*	.39**	.40**	.33**	.35**	.31**	.19*	**.03**	.23**
NA	-.20**	-.54**	-.18**	**-.29****	**-.31****	-.10*	-.23**	-.27**	-.26**	-.05	-.30**	**-.27****	.05
ID	.06	-.13*	-.01	-.09*	-.22**	.04	-.01	-.11*	-.05	-.04	-.38**	**-.29****	.12*
EMP	.09	.18**	.09	.09	.02	.12*	.09	0.09	.13**	.03	.04	.06	.03
Age	-.12*	.04	-.04	-.05	-.02	.02	-.09	-.03	-.02	.05	.18**	.19**	-.02
Race	.08	-.06	.03	.02	-.10*	-.01	.08	.04	.04	-.02	-.25**	**-.26****	**.20****
ED	-.03	.03	-.04	-.04	-.07	.02	.07	.02	.08	.00	-.06	-.08	.07

Table 4.5 (Continued)

Variable	BA	IM	SD	PA	NA	ID	EMP	Age	Race	ED
PP	.09	-.03	-.35**	.47**	-.18**	-.05	.04	-.16**	.10*	.06
CSE	.12*	-.09	-.74**	.48**	-.49**	-.04	.17**	.07	-.03	.07
LMX	.10	-.01	-.33**	.36**	-.16**	.03	.10	.04	-.01	.04
Trust	.09	.03	-.27**	.28**	**-.09**	.03	.08	-.02	-.01	.00
SPJ	-.02	**-.03**	**-.22****	.23**	**-.09**	-.09	.05	-.05	-.04	.06
TC	.14**	-.02	-.28**	.27**	-.06	.08	.08	-.05	.13*	.15**
OC	.11*	-.03	-.35**	.45**	-.17**	-.09	.09	.04	.06	.07
POS	.05	-.05	-.34**	.39**	-.22**	-.06	.07	.02	-.01	.04
JS	.05	-.04	-.34**	.39**	-.15**	-.03	.05	.05	-.03	.05
OCB-I	.09	.03	-.24**	.41**	-.08	-.01	.03	.14**	.06	.04
OCB-O	.01	-.13*	-.29**	.26**	-.19**	-.34**	.02	.17**	-.09	.01
OCB-IRB	.03	-.04	-.19**	**.21****	**-.12***	**-.12***	.03	.05	**-.09**	.08
ACB	.03	.01	.03	.12*	.14**	.00	.07	.03	**-.04**	-.08
BA		.07	-.07	.09	-.03	.05	.02	-.02	-.01	-.09

Table 4.5 (Continued)

IM	.08		.20**	-.01	.12*	.17**	-.02	-.09	.03	.03
SD	.09	.31**		-.41**	.47**	.09	-.16**	-.15**	-.01	-.11*
PA	.17**	.02	-.35**		-.10	.06	.06	.07	**.09**	.02
NA	.06	.15**	.55**	-.18**		**.19****	-.01	-.19**	.11*	.01
ID	.07	.26**	.22**	.12*	**.34****		.14**	-.24**	.13*	-.02
EMP	.02	.00	-.16**	.12*	-.09	.06		-.02	.04	.13*
Age	.02	-.20**	-.14**	.05	-.11*	-.14**	.19**		-.13*	.06
Race	.07	.09	.06	**.23****	.08	.23**	.03	-.19**		.09
ED	-.01	.06	.00	.07	.07	.04	.09	.07	.15**	

*= p<.05; **=p<.01

Random N=361; Traditional N=411

Correlations with significant z-scores are bolded

Table 4.6 Media Richness Correlations (Written Values on Top)

Variable	PP	CSE	LMX	Trust	SPJ	TC	OC	POS	JS	OCB-I	OCB-O	OCB-IRB	ACB
PP		.49**	**.19****	**.17****	**.09**	**.49****	.36**	**.24****	.22**	.33**	.15**	.19**	**.10***
CSE	.51**		.35**	.35**	.33**	.25**	.38**	.38**	.39**	.27**	.29**	.26**	.11*
LMX	**.33****	.45**		.87**	.74**	.22**	.54**	.68**	.59**	.32**	.19**	.21**	.06
Trust	**.34****	.43**	.83**		.79**	.19**	.55**	.71**	.59**	.24**	.14**	.15**	.05
SPJ	**.24****	.39**	.71**	.79**		.15**	.44**	.62**	.52**	.22**	.33**	.33**	.01
TC	**.65****	.36**	.26**	.24**	.16**		.39**	.26**	.23**	.49**	.11*	.17**	.04
OC	.49**	.47**	.57**	.62**	.47**	.44**		.78**	.81**	.39**	.20**	.11*	.08
POS	**.42****	.48**	.66**	.68**	.57**	.29**	.78**		.73**	.30**	.19**	.10*	.07
JS	.37**	.47**	.63**	.67**	.56**	.28**	.79**	.75**		.29**	.22**	.16**	.05
OCB-I	.39**	.19**	.37**	.29**	.23**	.45**	.40**	.34**	.28**		.34**	.26**	.10*
OCB-O	.22**	.32**	.26**	.25**	.37**	.24**	.24**	.22**	.23**	.36**		.64**	-.03
OCB-IRB	.19**	.23**	.22**	.17**	.26**	.21**	0.08	.09	.11*	.34**	.56**		-.09
ACB	**.06**	-.03	.01	.06	-.04	.02	0.09	.05	.06	.16**	.09	.02	
BA	.03	.05	.00	.04	-.00	.10*	0.08	-.01	.03	**.07**	.05	.03	.06

Table 4.6 (Continued)

IM	.08	-.10	-.06	.01	-.08	.01	-.03	-.07	-.05	.08	-.09	**.05**	.06
SD	-.33**	-.73**	**-.42****	-.38**	-.36**	-.23**	-.38**	**-.43****	-.42**	-.16**	-.27**	-.16**	-.00
PA	.48**	.44**	.36**	.28**	.20**	.35**	.43*	.40**	.37**	.32**	.25**	.17**	.13*
NA	-.21**	-.54**	-.22**	-.28**	-.23**	-.09	-.22**	-.29**	-.27**	-.02	-.22**	-.11*	.03
ID	-.03	-.08	.00	-.08	-.09	.02	-.12*	-.11*	-.09	-.09	-.31**	-.08	.02
EMP	.11*	.23**	.13**	.11*	.08	.15**	.09	.12*	.09	.05	.07	.12*	.05
AGE	-.05	.06	-.00	-.06	-.08	.11*	-.01	-.01	.01	.11*	.13**	.09	-.05
RACE	.02	-.05	.03	.03	-.05	.01	.03	.02	.01	.03	-.08	-.07	.05
EDUC	.09	.07	.02	-.02	.01	**.17****	.09	.06	.08	.09	.02	.04	-.13*

Table 4.6 (Continued)

Variable	BA	IM	SD	PA	NA	ID	EMP	Age	Race	ED
PP	.19**	.01	-.33**	.51**	-.19**	.05	.03	-.21**	.14**	-.06
CSE	.10	-.20**	-.74**	.45**	-.47**	-.09	.14**	.03	-.04	.02
LMX	.14**	-.02	**-.23****	.29**	-.11*	.01	.05	-.01	.00	-.01
Trust	.09	-.06	-.26**	.26**	-.13**	-.03	.05	-.01	-.01	-.01
SPJ	.00	-.13**	-.25**	.17**	-.19**	-.19**	-.01	.02	-.08	-.01
TC	.14**	.08	-.19**	.31**	-.07	.07	.07	-.14**	.09	**.01**
OC	.19**	.03	-.28**	.42**	-.19**	-.01	.11*	-.06	.09	.06
POS	.09	-.06	**-.28****	.33**	-.19**	-.07	.05	-.01	.02	.00
JS	.14**	-.01	-.32**	.38**	-.17**	-.02	.09	.01	.01	.05
OCB-I	**.23****	.07	-.21**	.38**	-.11*	-.08	.03	.07	-.01	-.07
OCB-O	.01	-.15**	-.29**	.12*	-.29**	-.39**	-.04	.20**	-.27**	-.06
OCB-IRB	.05	**-.09**	-.24**	.06	-.28**	-.32**	-.03	.15**	-.27**	-.06
ACB	.09	.01	.05	.23**	.12*	.09	.06	.05	.09	.11*

Table 4.6 (Continued)

Variable	BA	IM	SD	PA	NA	ID	EMP	Age	Race	ED
BA		.12*	.03	**.21****	.04	.04	.01	-.08	.06	-.07
IM	.02		.29**	-.01	**.16****	.23**	.05	-.14**	.09	.08
SD	.01	.23**		-.34**	.52**	.22**	-.10*	-.14**	.09	.02
PA	**.05**	.02	-.40**		-.14**	.13**	.13*	.03	**.23****	.02
NA	-.00	**.08**	.51**	-.18**		**.35****	.03	-.11*	**.16****	.08
ID	.04	.17**	.09	.01	**.17****		.10*	-.20**	**.25****	.05
EMP	.04	-.05	-.22**	.07	-.14**	.09		.12*	.08	.15**
Age	.09	-.15**	-.13*	.08	-.16**	-.16**	.07		**-.24****	.06
Race	-.02	.01	-.05	**.08**	**-.01**	**.06**	-.02	**-.1**		**.21****
ED	-.04	-.00	-.12*	.06	.01	-.04	.08	.07	**.03**	

*= p<.05; **=p<.01
Video N=373, Written
N=386
Correlations with significant z-scores are bolded

The results of the previous tests indicated that the use of CMV remedial techniques created very few significant differences in the results. There are two potential reasons for this. The first is that the remedial manipulations were not effective at either creating or reducing CMV bias. The second is that CMV bias was so pervasive that differences could not be detected regardless of the manipulations. In order to ensure that the data was not contaminated to a biasing degree and lend additional evidence to the lack of significant findings, a post hoc research question was developed. The post hoc research question was:

Post Hoc Question 1: Does CMV exist in the data set, and if so, does it exist at a biasing level.

In order to address the post hoc research question, a post hoc remedial technique was implemented. Previous research has indicated that the only post hoc technique that provides reliable evidence as to the biasing effects of CMV is the comprehensive CFA Marker Technique. Due to the a priori inclusion of marker variables, this study was able to use this method to evaluate the presence of CMV in the data. The results are provided in the following section.

Comprehensive CFA Marker Technique

To address the post hoc question and assess the influence of CMV using the marker variables, the comprehensive CFA marker technique proposed by Williams, et al (2010) was implemented. A subset of substantive variables that have demonstrated relationships in prior research and a marker variable were chosen (Fuller & Marler, 2009; Richardson et al, 2009). The chosen relationship to examine was that between exogenous variables of proactive personality and taking charge and endogenous variables of

organizational commitment, core self-evaluations, and job satisfaction. Attitudes towards the color blue was selected as the marker variable because it is a marker variable scale that was developed specifically for use as a marker variable (Miller & Chiodo, 2008). In addition, it serves as an ideal marker in that it is both theoretically unrelated to the substantive items, requires the same or similar cognitions as the substantive items, and uses the same or similar anchors as the substantive items. It has been assessed in previous research for efficacy (Miller & Chiodo, 2008; Richardson et al, 2009). This model was chosen due to consideration of the general rule of thumb for SEM to have 15 observations per indicator and the variables had relatively strong correlations in the full data set, as depicted in Table 4.1. This model has 47 indicators or observed variables and a sample size of 772.

For Phase I of the comprehensive CFA marker technique, models are compared to test for the existence and equality of method effects related to the marker latent variable. The first model examined was the CFA model, which allowed for the generation of a complete set of correlations among all of the substantive variables and the marker variable. This measure provided factor loading and measurement error variance estimates for the four marker variable indicators to use in the subsequent models. The second model, the Baseline Model, allows the substantive factors to be correlated, but the marker variable is considered orthogonal with its indicators having fixed factor loadings and error variances obtained from the CFA model. Specifically, the unstandardized factor loadings for ACB1, ACB2, ACB3, and ACB4 were .78, .64, .65, and .50, respectively. The unstandardized error estimates were .75, .15, .32, and.87. These fixed values in the Baseline and subsequent models enable the establishment of meaning for

the marker variables because subsequent models link the marker variable via secondary factor loadings and prevent its meaning from being compromised. For the method-C model, the parameter estimates for the marker variable were fixed and the marker variable assumed to be orthogonal. It differs from the baseline model in that it has additional factor loadings from the latent method factor to each of the substantive items that are fixed to be equal. The Method-U model is similar except that the estimates from the latent method factor to the substantive items are not forced to be equivalent, thus providing a test for the congeneric/noncongeneric perspectives. Based on examining the fit statistics and model comparisons, either the Method-C or the Method-U model should be chosen. The CFI results for the models are somewhat short of the .90-.95 often suggested. However, this can be attributed to the relatively large number of indicators (47) and the other fit indices do not indicate problems.

The results from the analysis, including the chi-square, degrees of freedom, and Comparative Fit Index (CFI) are shown in Table 4.7. The Baseline Model and Method-C Model comparison tested the null hypothesis that the method factor loadings associated with the marker variable were not related to the 47 substantive indicators when assumed equal. The chi-square difference test indicated support for rejecting the restriction of the 47 factor loadings to zero in the Baseline Model. Specifically, the Baseline Comparison as shown in Table 4.7 indicates a chi-square difference of 84.86 with 43 degrees of freedom, which exceeds the .05 chi-square critical value of 55.76.

Table 4.7 Chi-Square, Goodness-of-Fit Values, and Model Comparison Tests

Model	χ^2	*df*	CFI
1. CFA	3759.67	1019	.87
2. Baseline	3766.03	1027	.87
3. Method-C	3750.12	1026	.87
4. Method-U	3681.17	984	.88
5. Method-R	3682.21	994	.88

Chi-Square Model Comparison Tests

ΔModels	$\Delta\chi^2$	*Δdf*	Chi-Square Critical Value: .05
1. Baseline vs. Method-C	84.86**	43	55.76
2. Method-C vs. Method-U	68.95**	42	55.76
3. Method-U vs. Method-R	1.04	10	18.3

Next, the model comparison between the Method-U and Method-C Model was conducted. This comparison tested the null hypothesis that the method factor loadings are equal. The Method-C model provides a test for the non-congeneric CMV perspective, while the Method-U model provides a test for the congeneric CMV perspective. The difference test supported the rejection of the Method-C Model in favor of the Method-U model. Specifically, the comparison resulted in a chi-square difference of 68.95 with 42 degrees of freedom, which exceeds the .05 critical value of 55.76. Therefore, the Method-U Model represents the best model for addressing marker variance and the congeneric CMV perspective is supported. In other words, allowing the marker variables to have unequal loadings with each of the items provides a better fit for the data.

The last step in Phase I is to compare the selected model to a Method-R Model. The Method-R Model uses the substantive factor correlations from the Baseline Model in either the Method-C or Method-U Model, depending upon the previous step. The comparison provides a statistical test to indicate the level of bias due to the marker variable. The failure to find a significant difference in the Method-R Model from the Method-U Model indicates that CMV did not significantly contaminate the data.

Phase II, reliability decomposition, was conducted using AMOS outputs and Microsoft Excel. In addition to providing a means for testing the presence of method effects, reliability decomposition allows a way to quantify the amount of method variance associated with each of the latent variables. By decomposing the reliability estimates into substantive and method variance components, method effects can be better assessed for their influence. Phase II starts with using estimates of the factor loadings and error variances for each latent variable from the Baseline Model. The estimates are entered into the following formula to achieve a total reliability estimate (R_{tot}):

R_{tot} = (sum of factor loadings)^2/[(sum of factor loadings)^2 + sum of variances]

Next, both substantive and method factor loading estimates and error variances are entered into two additional equations. These estimates are obtained from either the Method-C or Method-U Model, whichever was selected in the previous phase. The completely standardized factor loadings for the Method-U Model can be found in Table 4.8. The values range from .44 to .94 and all substantive indicators significantly loaded on their respective constructs. For the method factor, 15 of the 47 factor loadings were statistically significant at $p<.05$. These equations are intended to partial out the overall systematic variance into substantive (R_{sub}) and method (R_{meth}) variance.

R_{sub} = (sum of substantive factor loadings)^2/[(sum substantive factor loadings)^2 + (sum of method factor loadings)^2 + sum error variances]

R_{meth} = (sum method factor loadings)^2/[(sum substantive factor loadings)^2 + (sum method factor loadings)^2 + sum error variances]

The values for R_{sub} and R_{meth} should equal the value for R_{tot}. Using this equation, the amount of inflation in the reliability estimates for the latent variable can be estimated. This method allows for a better understanding of CMV in the model. Table 4.8 reports the obtained factor loadings for the substantive constructs in the Method-U Model. As shown in the table, the method marker variable was only significantly related to a few of the items within each construct. For example, for proactive personality, the marker variable was significantly related to 5 of the 10 items. For taking charge, the marker variable was only related to 2 of the 10 items. One point of interest is that the marker variable did not significantly load on any of the reverse coded items, thus indicating it may be tapping in to a form of acquiescence bias. The majority of the significant loadings for all of the variables fell at .10 or less, indicating that the marker variable, even when significant, had minimal effects on the results.

Table 4.8 Method-U Model Factor Loadings: Completely Standardized Solution

Item	PP	TC	JS	OC	CSE	ACB
PP_1	.53*					.06
PP_2	.69*					.07
PP_3	.54*					.06
PP_4	.55*					.07
PP_5	.71*					.09*
PP_6	.68*					.11*
PP_7	.75*					.11*
PP_8	.66*					.09*
PP_9	.74*					.09*
PP_10	.69*					.05
TC_1		.66*				.06
TC_2		.65*				.00
TC_3		.81*				.05
TC_4		.81*				.03
TC_5		.68*				-.02
TC_6		.74*				.04
TC_7		.73*				.01
TC_8		.69*				-.01
TC_9		.79*				.09*
TC_10		.75*				.08*
JS1_1			.94*			.02
RJS1_2			.88*			.01
JS1_3			.91*			.09*
OC_1				.63*		.11*
OC_2				.83*		.06
OC_3				.65*		.10*
OC_4				.77*		.06
OC_5				.84*		.05
OC_6				.86*		.05

Table 4.8 (Continued)

OC_7	.83*		.09*
OC_8	.83*		.05
CSE_1		.60*	.05
RCSE_2		.70*	-.02
CSE_3		.57*	.10*
RCSE_4		.69*	.01
CSE_5		.44*	.14*
RCSE_6		.60*	0
CSE_7		.71*	.02
RCSE_8		.76*	.03
CSE_9		.54*	.11*
RCSE_10		.69*	-.03
CSE_11		.58*	.10*
RCSE_12		.74*	-.04
ACB_1			.66[a]
ACB_2			.85[a]
ACB_3			.76[a]
ACB_4			.47[a]

*=$p<.05$; [a]=factor loadings held constant through model comparisons

Table 4.9 presents the reliability decomposition results. As indicated in the table and consistent with the Method-U factor loadings, the marker variable had minimal influence on the substantive relationships. Most of the method effects fell at less than

.01, as represented by the .00 numbers in the table. This indicated that method bias, as detected by attitudes towards the color blue, did not strongly influence results. In fact, the total percentage of method bias was 2.53%. This indicates that method variance for this sample only accounted for 2.53% of the observed variance. This may be due to the inclusion of CMV control, CMV inducing, and CMV reducing conditions in the CFA marker evaluation. Taken as a whole, the findings indicate that there was no significant reliability bias indicated. The results further demonstrate that the data set as a whole was not significantly biased or that the bias cancelled itself out across the experimental conditions.

Table 4.9 Reliability Decomposition

	Reliability Baseline Model	Decomposed Reliability Method-U Model		
Latent Variable	Total Reliability	Substantive Reliability	Method Reliability	% Reliability Marker Variable
Proactive Personality	.92	.92	.01	1.10
Taking Charge	.92	.92	.00	.22
Job Satisfaction	.84	.84	.00	.17
Organizational Commitment	.92	.92	.01	.70
Core Self Evaluations	.89	.89	.00	.34

Phase III of the CFA marker technique includes a sensitivity analysis to assess the effects of sampling error on estimates of the method variance due to the marker variables. To conduct the sensitivity analysis, values that are higher than the actual estimates of method factor correlations, but within specified confidence intervals, are used. Using the unstandardized method factor loadings from either Method-C or Method-U allows for the determination of whether method factor loadings in the higher range of the confidence

intervals lead to different conclusions about the influence of the marker-based method variance. If there are no differences, then any concern about sampling error is reduced. In this study, the unstandardized method factors were obtained from the Method-U model and the comparisons are depicted as Method-S (.05) and Method-S (.01). In order to calculate the method factor values for the Method-S (.05) model, the probability value of 1.96 was multiplied by the respective standard errors and added to the original estimates. In order to calculate the method factor values for the Method-S (.01) model, the probability value of 2.57 was multiplied by the respective standard errors and added to the original estimates. Table 4.10 presents the factor correlations relating to each of the models examined. Factor loadings from the CFA and Baseline Model, which did not include the method factor loadings, are shown for comparison. The CFA results showed that proactive personality was significantly linked to the marker variable at $p<.05$, which is consistent with the other findings as 5 of the 10 items were significantly related to the marker variable. As can be seen in the table, very few of the factor loadings changed and those that did only changed by small amounts, less than .01.

Table 4.10 Method Models with Method Variance Sensitivity Analysis

Factor Correlations	CFA	Baseline	Method-U	Method-S(.05)	Method-S(.01)
Proactive personality – Taking charge	.63*	.63*	.63*	.63*	.63*
Proactive personality – core self-evaluations	.55*	.55*	.55*	.56*	.56*
Proactive personality – job satisfaction	.33*	.33*	.32*	.33*	.33*
Proactive personality – organizational commitment	.46*	.46*	.46*	.46*	.46*

Table 4.10 (Continued)

Taking charge – core self evaluations	.34*	.34*	.34*	.34*	.34*
Taking charge – job satisfaction	.29*	.29*	.28*	.29*	.29*
Taking charge – organizational commitment	.45*	.45*	.45*	.45*	.45*
Core self-evaluations – job satisfaction	.46*	.46*	.46*	.46*	.46*
Core self-evaluations-organizational commitment	.45*	.45*	.45*	.45*	.45*
Job satisfaction – organizational commitment	.86*	.86*	.86*	.86*	.86*
Proactive personality – attitudes towards blue	.09*	0	0	0	0
Taking charge – attitudes towards blue	.03	0	0	0	0
Organizational commitment – attitudes towards blue	.07	0	0	0	0
Job satisfaction – attitudes towards blue	.04	0	0	0	0
Core self evaluations – attitudes towards blue	.04	0	0	0	0

Results of Hypotheses Testing

The results previously discussed were used to test the hypotheses. Hypothesis 1 posited that the CMV reducing condition versus control would result in (a) weaker effect sizes and (b) weaker relationships to marker variables. This hypothesis was partially supported. The ANOVA and t-test results indicated that the only significant change at $p<.05$ was for CSE (t=-2.12), TC (t=-2.18); OCB-O (t=-2.19); OCB-IBR (t=-2.19). At

$p<.10$, interpersonal deviance and OCB-O were significantly different. All of the relationships between interpersonal deviance and the substantive variables were reduced and several were reduced significantly. The significant differences included relationships between interpersonal deviance and proactive personality, supervisory procedural justice, OCB-O, OCB-IRB, and negative affect. For OCB-O, the effects were more mixed, however, the only significant change was between OCB-O and interpersonal deviance. In the control condition the two scales were significantly related at -.52 ($p<.01$), but in the CMV reducing condition, the variables were significantly related at -.24 ($p<.01$). In addition, all of significant z-scores showed weaker effect sizes. Thus, H1a was supported for some of the variables. For H1b, ANOVA, t-tests, and z-tests showed significant differences for both negative affect ($t=3.89$) and the self-deceptive form of social desirability ($t=2.66$) at $p<.01$. Thus, H1b was partially supported, but not in the hypothesized direction.

Hypothesis 2 posited that the CMV inducing experimental manipulation in comparison to the control condition would result in (a) stronger effect sizes and (b) stronger relationships to the marker variables. For H2a, results of the ANOVA and t-tests indicated that none of the variables demonstrated a significant difference across conditions at $p<.05$. At $p<.10$, perceived organizational support showed a significant increase. Of the Z-test results that were significant, most increased in correlations with the CMV inducing manipulation, but only a few were significant. The most affected measures according to the z-scores were LMX and OCB-IRB; however, most of the differences, though significant, did not change the significance of the relationships. Thus, H2a was not supported.

Hypothesis 2b posited that the CMV inducing condition would result in the substantive variables having stronger relationships with the marker variables. Neither ANOVA nor t-tests showed any significant changes for either marker variable used in this study. Z-test scores provided some support for the influence of the attitudes towards the color blue marker variable scale, but very few were significant. The scores indicated significantly stronger relationships between the marker variable and the trust and supervisory procedural justice variables in the CMV inducing condition than in the control. The increased relationship with the marker variable provided some evidence of the efficacy of the experimental manipulation at increasing the relationships among the variables. Brand attitudes showed no significant differences with any of the substantive scales. Thus, H2b was partially supported.

Hypothesis 3 was based on media richness theory. It posited that video instructions would result in (a) stronger effect sizes and (b) stronger relationships with marker variables. ANOVA and t-test results indicated a significant difference ($p<.05$) across video and written conditions interpersonal deviance ($t=2.68$), but not in the hypothesized direction. Specifically, those who received written instructions demonstrated stronger relationships among the substantive variables than those who received video instructions. Although not significant according to t-tests or ANOVA, proactive personality responses showed several significant changes according to z-test results. The relationships between proactive personality and LMX, trust, supervisory procedural justice, taking charge, and perceived organizational support were much stronger in the video condition than in the written condition. Thus, H3a was supported for some scales. For H3b, the majority of the substantive variables relationships with

attitudes toward the color blue decreased for those who received written instructions, but not significantly so. For the brand attitudes marker, several relationships were significant for those who received written instructions, but not for those who received video instructions. T-tests and ANOVA supported the findings that the groups differed significantly in their responses for the brand attitudes marker variable (t=2.59) at p<.01, but not in the hypothesized direction. Those who received written instructions were more likely to exhibit positive feelings towards purchasing private label brands than those who received video instructions. Thus, H3b was supported for some scales with respect to brand attitudes, but not with attitudes towards the color blue.

Hypothesis 4 posited that the use of proximal separation versus no proximal separation would result in (a) weaker effect sizes and (b) weaker relationships with marker variables. T-tests and ANOVA supported H4a for LMX (t=2.33), trust (t=2.77), and organizational commitment (t=2.01). Z-tests supported the decrease in effect sizes across the conditions, with all but two significant relationships decreasing with the proximal separation treatment. The relationships between core self-evaluations and perceived organizational support and between positive affect and OCB-IRB were significantly stronger in the separation condition. The relationship between positive affect and OCB-IRB was near zero (.03) and non-significant in the no separation group and was .21 (p<.01) in the separation group. Thus, H4a was supported for some of the scales. For H4b, none of the tests indicated a difference between the groups for responses to the brand attitudes marker variable, but positive affect (t=1.98) and negative affect (t=3.13) were significant at p<.05. The z-test indicated one significant change for attitudes toward the color blue in relation to OCB-I. The relationship was near zero (.05)

and non-significant in the proximal separation group and .21 ($p<.01$) in the no separation group. The majority of the relationships with the marker variables decreased in the proximal separation group, but very few were significant. Thus, H4b was partially supported.

Hypothesis 5 posited that measures of presumed causes of CMV would have stronger effect sizes in the CMV magnifying condition versus the CMV reducing condition. Measures included (a) social desirability, (b) negative affect, and (c) positive affect. ANOVA and t-test results indicated that none of the relationships were significantly different across the treatment groups at $p<.05$. Thus, H5 was not supported.

CHAPTER FIVE

DISCUSSION

The purpose of this chapter is to provide a discussion of the results of the analysis presented in Chapter Four, discuss the implications and limitations of this study, and provide areas for future research.

Summary of Findings

This dissertation was aimed at exploring two research questions: (1) whether CMV could be manipulated through the research setting, and (2) to which measures of presumed causes of CMV are data most susceptible. The first question was addressed by comparing correlations among substantive variables in three conditions: CMV-inducing (i.e., instructions that indicate relations between variables and a traditional order of items), CMV-reducing (i.e., instructions indicating no relation between variables and a random order of items), and control (i.e., instructions that made no mention of relations between variables and a traditional order of items). The totality of findings indicates that CMV may be influenced through research condition, but not for all items. In fact, although the substantive relationships chosen for this dissertation were those that are either likely to exhibit CMV or have been previously shown to exhibit CMV, most correlations did not significantly change based upon experimental manipulation. Further, ANOVA and t-tests indicated very few differences across the conditions. A notable

exception included significant findings for negative affect in multiple comparisons. This finding is interesting given that negative affect is considered a relatively stable trait, yet differences between the groups were found in both the reducing conditions versus the control condition, the reducing condition and the inducing condition, and the item order conditions. This indicates that the sample populations differed in some fundamental, but unknown, way or that negative affect responses can be manipulated by study design. Although ANOVA and t-tests did not indicate significant differences in the groups, several correlations for LMX, interpersonal deviance, and OCB-IRB and other substantive scales were significantly different across treatment groups. All three of these variables were significantly related to the self-deceptive form of socially desirable responding. The manipulations were intended to induce/reduce biased responding according to researcher expectations. It is possible that because these scales were significantly related to social desirability, they were more likely to show differences across the manipulations. In other words, those who were more likely to engage in the self-deceptive form of social desirable responding were more likely to respond to changes in researcher expectations.

This research question was also addressed by examining the differences in correlations produced when the survey instructions were written form versus when they were presented in video form. The only scale that significantly changed according to ANOVA and t-tests was interpersonal deviance. In the written condition, responses were much more negatively related to the other substantive variables than in the video condition. This indicates that people may be less likely to admit to deviant behaviors when they feel a stronger presence of the researcher or a richer medium of delivery.

While not statistically significant according to t-tests and ANOVA, another interesting finding was that proactive personality demonstrated a much stronger relationship with the other substantive variables in the video condition versus the written condition. Proactive personality did not indicate many differences across the other experimental groups. Though the relationships did not change in significance or direction, the magnitude of the change was highly significant. This indicates that proactive personality may also be influenced by researcher presence. In addition, this was the only condition in which taking charge showed a significant difference across treatments. Given that each of these are variables that have a socially desirable/undesirable perspective, it is reasonable that the increased presence of the researcher would influence the likelihood of respondents to report more positive behaviors and less negative behaviors. The changes in socially desirable responding for this condition provide some support for this theory. Specifically, the social desirability scale was more strongly linked to the substantive variables in the video condition than in the written condition, but not significantly so. In addition, the marker variable of attitudes towards private label brands showed significant changes across these conditions. It was significantly related to several substantive relationships in the written condition, but only a few in the video condition. It is possible that the written manipulations were more effective at inducing/reducing bias than the video manipulations.

The second research question addressed the degree to which presumed causes of CMV—social desirability and affect—were more strongly related to some variables than others were. The pattern of results for these variables indicates that both social desirability and affect can be manipulated based upon research conditions. The strongest

differences for negative affect were found in the proximal separation/item order comparison. When negative affect was present next to positive affect in the traditional order, it had stronger relationships to the substantive variables than when it was presented randomly within the survey. The relationships between positive affect and negative affect to each other did not significantly change. This finding indicates that presenting the items together may result in method bias. The impression management form of social desirability remained relatively stable across the conditions, but the self-deceptive form showed several differences. In addition, negative affect showed a relatively strong positive relationship with self-deceptive enhancement in every condition and minimal to no relationship with impression management. This finding indicates that those with higher levels of negative affect are more susceptible to the experimental manipulations and are more likely to engage in self-deceptive enhancement. This finding makes sense in that those who are higher in negative affect may feel a greater pressure to meet researcher or societal demands than those who are more positive.

In addition to the research questions above, the efficacy of two different marker variables was examined. Both marker variables showed relatively weak but stable correlations with the substantive variables and neither was found to be statistically significantly different across any experimental condition comparisons with one exception. Attitudes towards private label brands was significantly different in the video versus the written condition, having significantly stronger relationships to the substantive variables in the written conditions. In general, the marker variable Attitudes towards the Color Blue identified no significant CMV in the data set. While not reported, Phase I of the CFA correlational method was conducted using the private label brand attitudes

marker variable and no significant differences were found between the two marker variable scales. While not explicitly addressed in the study design, there appears to be only minimal correlation between the marker variables and measured causes of CMV. All of the correlations are below .20, indicating that the patterns of marker variable responses cannot be accounted by other measured causes. However, in this study, the marker variables did not detect a significant level of bias, so that finding may not hold across samples.

Another contribution of this study was the examination of CMV in the data set through the addition of a post hoc research question using the comprehensive CFA marker technique. The technique allows the researcher to develop a better understanding of how marker variables detect CMV influences. Only some of the variables in this dissertation were modeled to assess the presence of CMV. The variables used included proactive personality, taking charge behaviors, organizational commitment, job satisfaction, and core self-evaluations. The marker variable used was attitudes towards the color blue. Using the CFA marker technique, no significant bias was found and the marker variable only accounted for 2.53% of the total variance across the models. It further indicated that of these variables, proactive personality was the most influenced by CMV as detected by the marker variable and was the only one with a significant relationship to the marker variable in the CFA model. This indicates that proactive personality is influenced by the method effect captured by the attitudes towards the color blue marker variable.

One benefit of the CFA marker technique is that it allows for the breakdown of method effects by item rather than by scale. This allows the researcher to identify which

particular items are most susceptible to its effects. An interesting finding was that the marker variable did not significantly load on any of the negatively worded items, only the positively worded items. Thus, it is possible that at least some of the method effect captured by this particular marker variable is a form of acquiescence bias.

Implications for Researchers

The findings presented above indicate several implications for researchers. First, the findings demonstrate that CMV may be present in some relationships measured on the same survey, but not in all of them and not to the same degree. This demonstration of congeneric CMV supports the use of a post hoc statistical correction technique like the Comprehensive Marker Technique because it can account for such variance and address CMV influences at the individual factor level. In addition, the comprehensive marker technique has not been applied in many studies, as such, the nature of method variance and its potentially biasing effects are not well understood. This study implemented the comprehensive marker technique to show that although method effects existed at differing levels across constructs, they did not do so at biasing levels. This provides support for researchers who are concerned about the validity of self-report data because this study attempted to create bias, but little was detected.

In addition, the results of this study indicate that the application of a priori procedural remedies may not significantly influence results. The procedural remedies used in this study included proximal separation, the use of cover stories, and the media with which the cover stories are delivered. The use of proximal separation proved to be the most effective at creating differences in responding, but even those differences were relatively minor. Very few differences were detected across the rest of the experimental

conditions and that which was detected was small. This indicates that either the manipulations were not effective or that CMV was simply not present at biasing levels, as supported by the CFA marker technique results. Another potential explanation is that CMV influenced all of the experimental conditions; however, the post hoc research question and application of the CFA marker technique indicated this to be untrue because it failed to detect biasing levels of CMV. Further, the research supported the congeneric perspective of CMV in that, when CMV exists, it does not influence all variables or items in the same manner or to the same degree. Even with proactive personality variable, which had 5 of the 10 items influenced by the marker variable, the bias was extremely small at .09. Thus, researchers who failed to apply a priori procedural remedies may still have viable results and the CFA marker technique can be used to demonstrate that the results and data set are viable.

Limitations

As in all research, this dissertation has some limitations. The first limitation involves the sample population. The experimental manipulations were applied to different samples within the population. In spite of random assignment, it cannot be guaranteed that the samples were truly equivalent. As such, there is a potential that the differences found were due to differences in the sample populations and not solely to the experimental manipulations. However, given the number of responses collected for each group and the random assignment, it is unlikely that the differences could be attributed solely to the different samples. In addition, the sample had an over representation of the Asian/Pacific Islander ethnicity in comparison to the U.S. population, which may influence the generalizability of the results.

Another potential limitation involves the selection of the marker variables. Only two marker variables were investigated and neither should have been theoretically related to workplace variables; however, they were very obviously non-related and several survey respondents noted the "random" questions in response to an open-ended question at the end of the survey. Because the marker variables were so obviously non-related to the substantive variables, they represented a cognitive speed bump that may have re-set the respondent's thought processes and tapped into a different set of cognitions than the substantive variables. However, the finding of some significant relationships among the marker variables indicates that method variance was accurately captured by the variables.

Future Research

As mentioned previously, there is an ongoing debate regarding the nature of CMV and how researchers can account for it in their same-source data. This dissertation informs the literature in several ways, but there are several research ideas that could be advanced based upon these findings. First, using the CFA marker technique to investigate differences in the other substantive scales could indicate greater differences for some constructs. In addition, there is the potential to break down the sample for evaluation at more specific experimental manipulations. In other words, examining all twelve potential experimental groups for differences may shed additional insight into the behavior of marker variables and the efficacy of procedural remedies.

Another potential area for future research includes the marker variables selected. Further examination of other marker variables or the development of a marker variable scale that, while still theoretically unrelated to the construct, is not obviously unrelated, may be another fruitful area for future research. In addition, the exploration of additional

remedies, such as temporal separation may allow for better understanding as to when and how CMV occurs at biasing levels.

Further examination of measured causes of CMV is another potential area for future research. A focus on variables that are theoretically related to social desirability or affect, such as proactive personality, taking charge behaviors, and organizational citizenship behaviors, may allow for a better understanding of when the measured causes lead to significant bias. Meta-analytic results indicated that social desirability is not significantly related to proactive personality (Fuller & Marler, 2009) and only weakly related to many other common constructs (Moorman & Podsakoff, 1992). Using the measures that separate the impression management and the self-deceptive enhancement forms of socially desirable responding may allow for a better understanding of when and how social desirability and affect contribute to CMV.

Conclusion

In spite of the limitations, this study offered several contributions and insights into CMV and its effects on data collection. First, the study found the application or non-application of certain procedural remedies had differing effects on substantive variables. Second, the study found that most of the differences indicated between the experimental groups were non-significant, thus supporting the validity of data that does not employ such remedies. Third, the study employed the comprehensive CFA marker technique to evaluate the ability of marker variables to detect CMV. These findings provide support for the use of self-report data in academic research, as there were no pervasive CMB effects.

APPENDIX A

HUMAN SUBJECT LETTER

LOUISIANA TECH
UNIVERSITY

MEMORANDUM

OFFICE OF UNIVERSITY RESEARCH

TO: Dr. Marcia Simmering-Dickerson and Ms. Alison Wall

FROM: Dr. Stan Napper, Vice President Research & Development

SUBJECT: HUMAN USE COMMITTEE REVIEW

DATE: May 21, 2014

In order to facilitate your project, an EXPEDITED REVIEW has been done for your proposed study entitled:

"Common Method Variance: An Experimental Manipulation"

HUC 1214

The proposed study's revised procedures were found to provide reasonable and adequate safeguards against possible risks involving human subjects. The information to be collected may be personal in nature or implication. Therefore, diligent care needs to be taken to protect the privacy of the participants and to assure that the data are kept confidential. Informed consent is a critical part of the research process. The subjects must be informed that their participation is voluntary. It is important that consent materials be presented in a language understandable to every participant. If you have participants in your study whose first language is not English, be sure that informed consent materials are adequately explained or translated. Since your reviewed project appears to do no damage to the participants, the Human Use Committee grants approval of the involvement of human subjects as outlined.

Projects should be renewed annually. ***This approval was finalized on May 21, 2014 and this project will need to receive a continuation review by the IRB if the project, including data analysis, continues beyond May 21, 2015.*** Any discrepancies in procedure or changes that have been made including approved changes should be noted in the review application. Projects involving NIH funds require annual education training to be documented. For more information regarding this, contact the Office of University Research.

You are requested to maintain written records of your procedures, data collected, and subjects involved. These records will need to be available upon request during the conduct of the study and retained by the university for three years after the conclusion of the study. If changes occur in recruiting of subjects, informed consent process or in your research protocol, or if unanticipated problems should arise it is the Researchers responsibility to notify the Office of Research or IRB in writing. The project should be discontinued until modifications can be reviewed and approved.

If you have any questions, please contact Dr. Mary Livingston at 257-2292 or 257-5066.

APPENDIX B

INFORMED CONSENT STATEMENT

INFORMED CONSENT STATEMENT

The project consists of a survey to determine the degree to which recommended a priori procedural modifications to surveys affect common method variance in data. You are being asked to complete an online survey. Your participation is voluntary.

Informed Consent Statement: The survey contains items that address your perceptions of your workplace. Your information will be kept confidential. There are no risks involved in this study. All responses are anonymous. No individual responses will be released; all data will be presented in aggregate. If you choose not to participate in the study, no negative consequences will follow. The participant understands that Louisiana Tech is not able to offer financial compensation nor to absorb the costs of medical treatment should you be injured as a result of participating in this research.

Please read the following statement and indicate yes or no that you are willing to participate in the survey.

I acknowledge that I have read and understood the description of the study: "Common Method Variance: An Experimental Manipulation", and its purposes and methods. I understand that my participation in this study will not affect my relationship with Louisiana Tech University or my employment. Further, I understand that I may withdraw at any time or refuse to answer any questions without penalty. Upon completion of the study, I understand that the aggregate results will be freely available to me upon request. I understand that the results of my survey will be confidential and accessible only to the principal investigators, myself, or a legally appointed representative. I have not been requested to waive nor do I waive any of my rights related to participating in this study.

- ☐ Yes
- ☐ No

REFERENCES

Alessandri, G., Vecchione, M., Fagnani, C. B., & Barbaranelli, C. (2010). Much more than model fitting? Evidence for the heritability of method effect associated with positively worded items of the life orientation test revised. *Structural Equation Modeling, 17*, 642-653.

Antonakis, J., Bendahan, S., Jacquart, P., & Lalive, R. (2010). On making causal claims: a review and recommendations. *Leadership Quarterly, 6*, 1086-1120.

Aronson, E., Wilson, T. D., & Brewer, M. (1998). Experimentation in social psychology. In D. T. Gilbert, S. T. Fiske, & G. Lindzey (Eds.), *The Handbook of Social Psychology* (Vol. 1, pp. 99-142). Boston, MA: McGraw-Hill.

Ashkanasy, N. (2008). Submitting your manuscript. *Journal of Organizational Behavior, 29*, 263-264.

Bacharach, S., Bamberger, P., & Conley, S. (1991). Work-home conflict among nurses and engineers: Mediating the impact of role stress on burnout and satisfaction at work. *Journal of Organizational Behavior, 12*(1), 39-53.

Bäckström, M., Björklund, F., & Larsson, M. (2009). Five-factor inventories have a major general factor related to social desirability which can be reduced by framing items neturally. *Journal of Research in Personality, 43*, 335-344.

Bagozzi, R. P., & Yi, Y. (1990). Assessing method variance in multitrait-multimethod matrices: The case of self-reported affect and perceptions at work. *Journal of Applied Psychology, 75*, 547-560.

Bagozzi, R., & Yi, Y. (1991). Multitrait-multimethod matrices in consumer research. *Journal of Consumer Research, 17*, 426-439.

Baltes, B. B., Bauer, C. C., Bajdo, L. M., & Parker, C. P. (2002). The use of multitrait-multimethod data for detecting nonlinear relationships: The case of psychological climate and job satisfaction. *Journal of Business and Psychology, 17*(1), 3-17.

Baumgartner, H., & Steenkamp, J. B. (2001). Response styles in marketing research: a cross national investigation. *Journal of Marketing Research, 38*, 143-56.

Behrend, T., Sharek, D., & Meade, A. (2011). The viability of crowdsourcing for survey research. *Behavioral Research, 43*, 800-813.

Bennett, R., & Robinson, S. (2000). The Development of a Measure of Workplace Deviance. *Journal of Applied Psychology, 85*, 349-360.

Berman, J., & Kenny, D. (1976). Correlational bias in observer ratings. *Journal of Personality and Social Psychology, 34*, 263-273.

Biderman, M. D., Nguyen, N. T., Cunningham, C. J., & Ghorbani, N. (2011). The ubiquity of common method variance: The case of the Big Five. *Journal Research in Personality*, 417-429.

Bing, M., Kluemper, D., Davison, H., Taylor, S., & Novicevic, M. (2011). Overclaiming as a measure of faking. *Organizational Behavior and Human Decision Processes, 116*, 148-162.

Birkeland, S., Manson, T., Kisamore, J., Brannick, M., & Smith, M. (2006). A meta-analytic investigation of job applicant faking on personality measures. *International Journal of Selection and Assessment, 14*(7), 317-335.

Bol, J. (2011). The determinants and performance effects of managers' performance evaluation biases. *Accounting Review, 86*(5), 1549-1575.

Brannick, M. T., Chan, D., Conway, J. M., Lance, C. E., & Spector, P. E. (2010). What is method variance and how can we cope with it? A panel discussion. *Organizational Research Methods, 13*, 407-420.

Brief, A., Burke, M., George, J., Robinson, B., & Webster, J. (1988). Should negative affectivity remain an unmeasured variable in the study of job stress? *Journal of Applied Psychology, 73*, 191-198.

Buhrmester, M., Kwang, T., & Gosling, S. (2011). Amazon's Mechanical Turk: A new source of inexpensive, yet high-quality, data? *Perspectives on Psychological Science, 6*, 3-5.

Burton, S., Lichtenstein, D., Netemeyer, R., & Garretson, J. (1998). A scale for measuring attitude toward private label products and an examination of its psychological and behavioral correlates. *Journal of the Academy of Marketing Science, 26*, 293-306.

Burton-Jones, A. (2009). Minimizing method bias through programmatic research. *MIS Quarterly, 33*(3), 445-471.

Cammann, C., Fichman, M., Jenkins, D., & Klesh, J. (1983). Assessing the attitudes and perceptions of organizational members. In S. Seashore, E. Lawler, P. Mirvis, & C. Camman (Eds.), *Assessing organizational change: A guide to methods, measures, and practices* (p. 84). New York: John Wiley.

Campbell, D., & Fiske, D. (1959). Convergent and discriminant validation by the multitrait-multimethod matrix. *Psychological Bulletin, 56*, 81-105.

Campbell, J. (1982). Editorial: Some remarks from the outgoing editor. *Journal of Applied Psychology, 67*, 691-700.

Castille, C. M., & Simmering, M. J. (2013). Searching for black cats in dark rooms: how viable are procedural remedies for CMV? *Annual Meeting of the Southern Management Association.* New Orleans, LA.

Chan, D. (2009). So why ask me? Are self-report data really that bad? In C. E. Lance, & R. Vandenberg (Eds.), *Statistical and methodological myths and urban legends: Doctrine, verity and fable in the organizational and social sciences* (pp. 309-336). New York, NY: Routledge/Taylor & Francis Group.

Chang, C.-H. (., Ferris, D. L., Johnson, R. E., Rosen, C. C., & Tan, J. A. (2012). Core self-evaluations: A review and evaluation of the literature. *Journal of Management, 38*, 81-128.

Chang, S.-J., van Witteloostujin, A., & Eden, L. (2010). From the Editors: Common method variance in international business research. *Journal of International Business Studies, 41*, 178-184.

Chen, P. Y., & Spector, P. E. (1991). Negative affectivity as the underlying cause of correlations between stressors and strains. *Journal of Applied Psychology, 76*, 398-407.

Choi, H., & Johnson, S. (2005). The effect of context-based video instruction on learning and motivation in online courses. *The American Journal of Distance Education, 19*(4), 215-227.

Cohen, J. (1988). *Statistical power analysis for the behavioral sciences.* Hillsdale, New Jersey: Erlbaum.

Conway, J., & Lance, C. (2010). What reviewers should expect from authors regarding common method bias in organizational research. *Journal of Business Psychology, 25*, 325-334.

Costa, P. J., & McCrae, R. (1988). From catalog to classification: Murray's needs and the five-factor model. *Journal of Personality and Social Psychology, 55*, 258-265.

Cote, J., & Buckley, R. (1987). Estimating trait, method, and error variance: generalizing across 70 construct validation studies. *Journal of Marketing Research, 24*, 315-318.

Cote, J., & Buckley, R. (1988). Measurement error and theory testing in consumer research: an illustration of the importance of construct validation. *Journal of Consumer Research, 14*, 579-582.

Crampton, S. M., & Wagner, J. A. (1994). Percept-percept inflation in microorganizational research: An investigvation of prevalence and effect. *Journal of Applied Psychology, 79*, 67-76.

Cronbach, L. J. (1946). Response sets and test validity. *Educational and Psychological Measurement, 6*, 475-494.

Crowne, R. S., & Marlowe, D. (1964). *The approval motive: Studies in evaluative dependence.* New York: Wiley.

Daft, R., & Lengel, R. (1984). Information Richness: A New Approach to Manager Information Processing and Organizational Design. In B. Staw, & L. Cummings (Eds.), *Research in Organizational Behavior.* Greenwich, Connecticut: JAI Press.

Daft, R., & Lengel, R. (1986). Organizational information requirements, media richness, and structural design. *Management Science, 32*(5), 554-571.

De Beuckelaer, A., Weijters, B., & Rutten, A. (2010). Using ad hoc measures for response styles: a cautionary note. *Quality and Quantity, 44*, 761-775.

Dean, A., & Voss, D. (1999). *Design and Analysis of Experiments.* New York, NY: Springer Schience+Business Media, Inc.

Dean, J., & Snell, S. (1991). Integrated manufacturing and job design: Moderating effects of organizational intertia. *Academy of Management Journal, 34*(4), 776-804.

Dillman, D., Smyth, J., & Christian, L. (2009). *Internet, mail, and mixed-mode surveys: The Tailored Design Method* (3 ed.). Hoboken, New Jersey: John Wiley & Sons, Inc.

Doty, D., & Glick, W. (1998). Common Methods bias: Does common methods variance really bias results? *Organizational Research Methods, 1*, 374-406.

Eden, D., & Leviatin, V. (1975). Implicit Leadership theory as a determinant of the factor structure underlying supervisory behavior scales. *Journal of Applied Psychology, 60*, 736-740.

Eisenberger, R., Huntington, R., Hutchinson, S., & Sowa, D. (1986). Perceived organizational support. *Journal of Applied Psychology, 71*, 500-507.

Farh, J.-L., & Dobbins, G. (1989). Effects of self-esteem on leniency bias in self-reports of performance: A structural equation model analysis. *Personnel Psychology, 42*(4), 835-850.

Fisher, R. (1993). Social desirability bias and the validity of indirect questioning. *The Journal of Consumer Research, 20*(2), 303-315.

Fuller, B. J., & Marler, L. E. (2009). Change driven by nature: A meta-analytic review of the proactive personality literature. *Journal of Vocational Behavior, 75*, 329-345.

Goldberg, L. (1999). A broad-bandwidth, public-domain, personality inventory measuring the lower-level facets of several five-factor models. In I. Mervielde, I. Deary, F. De Fruyt, & F. Ostendorf (Eds.), *Personality Psychology in Europe* (pp. 7-28). Tilburg, The Netherlands: Tillburg University Press.

Gorrell, G., Ford, N., Madden, A., Holdridge, P., & Eaglestone, B. (2011). Countering method bias in questionnaire-based user studies. *Journal of Documentation, 67*(3), 507-524.

Graen, G., & Uhl-Bien, M. (1995). Relationship-based approach to leadership: Development of leader-member exchange (LMX) theory of leadership over 25 years: Applying a multi-level multi-domain perspective. *The Leadership Quarterly, 6*, 219-247.

Grant-Vallone, E., & Donaldson, S. (2001). Consequences of work-family conflict on employee well-being over time. *Work & Stress, 15*(3), 214-226.

Guzzo, R. A., Wagner, D. B., Maguire, E., Herr, B., & Hawley, C. (1986). Implicit theories and the evaluation of group process and performance. *Organizational Behavior and Human Decision Processes, 37*, 279-295.

Hair, J. F., Black, W. C., Babin, B. J., & Anderson, R. E. (2010). *Multivariate Data Analysis.* Upper Saddle River, NJ: Prentice Hall.

Harris, M. M., & Bladen, A. (1994). Wording effects in the measurement of role conflict and role ambiguit: a multi-trait-multimethod analysis. *Journal of Management, 20*, 887-901.

Harrison, D. A., & McLaughlin, M. E. (1993). Cognitive processes in self-report responses: Test of item context effects in work attitude measures. *Journal of Applied Psychology, 78*, 129-140.

Harrison, D. A., McLaughlin, M. E., & Coalter, T. M. (1996). Context, cognition, and common method variance: psyychometric and verbal protocol evidence. *Organizational Behavior in Human Decision Processing, 68*, 246-261.

Higgins, E. T., Rholes, W. S., & Jones, C. R. (1977). Category accessibility and impression formation. *Journal of Experimental Social Psychology, 13*, 141-154.

Johnson, J. (2004). The impact of item characteristics on item and scale validity. *Multivariate Behavioral Research, 39*, 273-302.

Johnson, R., Rosen, C., & Djurdjevic, E. (2011). Assessing the impact of common method variance on higher-order multidimensional constructs. *Journal of Applied Psychology, 96*, 744-761.

Judge, T., & Cable, D. (2004). The effect of physical height on workplace success and income: Preliminary test of a theoretical model. *Journal of Applied Psychology, 89*, 428-441.

Judge, T., Erez, A., Bono, J., & Thoresen, C. (2003). The core self-evaluation scale (CSES): Development of a measure. *Personnel Psychology, 56*, 303-331.

Kerlinger, F. N., & Lee, H. B. (2000). *Foundations of Behavioral Research* (3 ed.). Thomson Learning.

Kline, T., Sulsky, L., & Rever-Moriyama, S. (2000). Common method variance and specification errors: A practical approach to detection. *The Journal of Psychology, 4*(134), 401-421.

Kohn, M., & Schooler, C. (1983). *Work and personality: An inquiry into the impact of social stratification.* Norwood, New Jersey: Ablex.

Krosnick, J. A. (1991). Response strategies for coping with the cognitive demands of attitude measures in surveys. *Applied Cognitive Psychology, 5*, 213-236.

Kuhn, T. S. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.

Kuncel, N. R., & Tellegen, A. (2009). A conceptual and empirical reexamination of the measurement of the social desirability of items: implications for detecting desirable response style and scale development. *Personnel Psychology, 62*, 201-228.

Lance, C. E., Baranik, L. E., Lau, A. R., & Scharlau, E. A. (2009). If it ain't trait it must be method: (Mis)application of the multitrait-multimethod methodology in organizational research. In C. E. Lance, & R. J. Vandenberg (Eds.), *Statistical and methodological myths and urban legends: Received doctrine, verity, and fable in organizational and social research* (pp. 339-362). New York: Routledge.

Lance, C., Dawson, B., Birkelback, D., & Hoffman, B. J. (2010). Method effects, measurement error, and substantive conclusions. *Organizational Research Methods, 13*, 435-455.

Leventhal, G. (1980). What should be done with equity theory? New approaches to the study of fairness in social relationships. In K. Gergen, M. Greenberg, & R. Willis (Eds.), *Social exchange: Advances in theory and research* (pp. 27-55). New York: Plenum.

Liden, R., & Graen, G. (1980). Generalizability of the vertical dyad lindage model of leadership. *Academy of Management Journal, 23*, 451-465.

Lindell, M. K., & Whitney, D. J. (2001). Accounting for common method variance in cross-sectional designs. *Journal of Applied Psychology, 86*, 114-121.

Lindell, M. K., & Whitney, D. J. (2001). Accounting for common method variance in cross-sectional designs. *Journal of Applied Psychology, 86*, 114-121.

Lord, F., & Novick, M. (1968). *Statistical theories of mental test scores.* Reading, MA: Addison-Wesley Publishing Company.

Lord, R. G., Binning, J. F., Rush, M. C., & Thomas, J. C. (1978). The effect of performance cues and leader behavior on questionnaire ratings of leadership behavior. *Organizational Behavior and Human Decision Processes, 21*, 27-39.

Lowe, K., Kroeck, K., & Sivasubramaniam, N. (1996). Effectiveness correlates of transformational and transactional leadership: a meta-analytic review of the MLQ literature. *Leadership Quarterly, 7*, 385-425.

Lowery, R. (2014, June 9). *Significance of the Difference Between Two Correlation Coefficients.* Retrieved from VassarStats: Website for Statistical Computation: http://vassarstats.net/rdiff.html?

Malhotra, N., Kim, S., & Patil, A. (2006). Common method variance in IS research: A comparison of alternative approaches and a reanalysis of past researc. *Management Science, 52*(12), 1865-1883.

Marler, L. E. (2008, May). Proactive Behavior: A selection perspective. *a dissertation.* Louisiana Tech University.

Marsh, H., & Yeung, A. (1999). The liability of psycholgical ratings: The chameleon effect in global self-esteem. *Personality and Social Psychology Bulletin, 25*, 49-64.

Martin, C., & Nagao, D. (1989). Some effects of computerized interviewing on job applicant responses. *Journal of Applied Psychology, 74*, 72-80.

McBride, D. (2012). *The process of research in psychology* (2 ed.). Thousand Oaks, California: Sage Publications, Inc.

McNatt, D. (2000). Ancient pygmalion joins contemporary management: A meta-analysis of the result. *Journal of Applied Psychology, 85*(2), 314-322.

Meade, A. W., Watson, A. M., & Kroustalis, C. M. (2007, April). Assessing common method bias in organizational research. *22nd Annual Meeting of the Society for Industrial and Organizational Psychology.* New York, New York.

Mick, D. G. (1996). Are studies of dark side variables confounded by socially desirable responding? The case of materialism. *Journal of Consumer Research, 23*, 106-119.

Miller, B., & Chiodo, B. (2008). Academic entitlement: Adapting the equity preference questionnaire for a university setting. *Paper presented at the Southern Management Association meeting in St. Pete Beach, FL.*

Moorman, R. H., & Podsakoff, P. M. (1992). A meta-analytic review and empirical test of the potential confounding effects of social desirability response sets in organizational behaviour research. *Journal of Occupational and Organizational Psychology, 65*, 131-149.

Morrison, E., & Phelps, C. (1999). Taking charge at work: Extrarole efforts to initiate workplace change. *Academy of Management Journal, 42*, 403-419.

Mowday, R., Steers, R., & Porter, L. (1979). The measurement of organizational commitment. *Journal of Vocational Behavior, 14*, 224-247.

Nunnally, J. C., & Bernstein, I. H. (1994). *Psychometric Theory* (3 ed.). New York: McGraw Hill.

Organ, D., & Ryan, K. (1995). A meta-analytic review of attitudinal and dispositional predictors of organizational citizenship behavior. *Personnel Psychology, 48*, 775-802.

Orne, M. (1962). On the social psychology of the psychology experiment: With particular reference to demand characteristics and their implications. *American Psychologist, 17*, 776-783.

Ostroff, C., Kinicki, A., & Clark, M. (2002). Substantive and operational issues of response bias across levels of analysis: An example of climate-satisfaction relationships. *Journal of Applied Psychology, 87*, 355-368.

Pace, V. L. (2009). Method variance from the perspective of reviewers: poorly understood problem or overemphasized complaint? *Organizational Research Methods, 13*, 1-14.

Paulhus, D. (1991). Measurement and control of response bias. In J. Robinson, P. Shaver, & L. Wrightsman, *Measures of psychology and social psychological attitudes* (pp. 17-59). San Diego, CA: Academic Press.

Paulhus, D. (2002). Socially desirable responding: The evolution of a construct. In H. I. Braun, D. N. Jackson, & D. E. Wiley (Eds.), *The Role of Constructs in Psychological and Educational Measurement* (pp. 49-69). Mahweh, New Jersey: Lawrence Erlbaum Associates.

Paulhus, D., Harms, P., Bruce, M., & Lysy, D. (2003). The over-claiming technique: Measuring self-enhancement independent of ability. *Journal of Personality and Social Psychology, 84*, 681-693.

Pedhauzer, E., & Schmelkin, L. (1991). *Measurement, Design, and Analysis.* Hillside, New Jersey: Lawrence Erlbaum Associates, Inc.

Phillips, J. S., & Lord, G. (1986). Notes on the theoretical and practical consequences of implicit leadership theories for the future of leadership measurement. *Journal of Management, 12*, 31-41.

Podsakoff, P., & Organ, D. (1986). Self-reports in organizational research - problems and prospects. *Journal of Mangement, 12*, 531-544.

Podsakoff, P., & Todor, W. (1985). Relationships between leader reward and punhsiment behavior and group processes and productivity. *Journal of Management, 11*, 55-73.

Podsakoff, P., MacKenzie, S., & Podsakoff, N. (2012). Sources of method bias in social science research and recommendations on how to control it. *Annual Review Psychology, 63*, 539-569.

Podsakoff, P., MacKenzie, S., Lee, J., & Podsakoff, N. (2003). Common method biases in behavioral research: A critical review of the literature and recommended remedies. *Journal of Applied Psychology, 88*, 879-903.

Poon, J., Rahid, M., & Othman, A. (2006). Trust-in-supervisor: antecedents and effect of affective organizational commitment. *Asian Academy of Management Journal, 11*(2), 35-50.

Richardson, H. A., Simmering, M. J., & Sturman, M. C. (2009). A tale of three perspectives: examining post hoc statistical techniques for detection and correction of common method variance. *Organizational Research Methods, 12*, 762-800.

Richman, W., Keisler, S., Weisband, S., & Drasgow, F. (1999). A meta-analytic study of social desirability distortion in computer-administered questionnaires, traditional questionnaires, and interviews. *Journal of Applied Psychology, 84*, 754-775.

Rindfleisch, A., Malter, A., Ganesan, S., & Moorman, C. (2008). Cross-sectional versus longitudinal survey research: Concepts, findings, and guidelines. *Journal of Marketing Research, 45*, 261-279.

Rizzo, J., House, R., & Lirtzman, S. (1970). Role conflict and role ambiguity in complex organizations. *Administrative Science Quarterly, 15*, 150-163.

Robie, C. (2006). Effects of perceived selection ratio on personality test faking. *Social Behavior and Personality, 34*(10), 1233-1244.

Robinson, S., & Bennett, R. (1995). A Typology of Deviant Workplace Behaviors: A Multidimensional Scaling Study. *Academy of Management Journal, 38*(2), 555-572.

Rosenberg, M. J. (1965). When dissonance fails: on eliminating evaluation apprehension from attitude measurement. *Journal of Personality and Social Psychology, 1*, 28-42.

Rubin, M., Paolini, S., & Crisp, R. J. (2010). A processing fluency explanation of bias against imigrants. *Journal of Experimental Social Psychology, 46*, 21-28.

Rupp, D., & Cropanzano, R. (2002). The mediating effects of social exchange relationships in predicting workplace outcomes from multifoci organizational justice. *Organizational Behavior and Human Decision Processes, 89*, 925-946.

Salancik, G. R. (1984). On priming, consistency, and order effects in job attitude assessment: With a note on current research. *Journal of Management, 10*, 250-254.

Salancik, G. R., & Pfeffer, J. (1977). An examination of the need-satisfaction models of job attitudes. *Adminsitrative Science Quarterly, 22*, 427-456.

Scherpenzeel, A., & Saris, W. (1997). The validity and reliability of survey questions: a meta-analysis of MTMM studies. *Social Methods Research, 25*, 341-383.

Schmit, M., & Ryan, A. (1993, December). The Big Five in personnel selection: Factor structure in applicant and nonapplicant populations. *Journal of Applied Psychology, 78*(6), 966-974.

Schmitt, N., & Stults, D. M. (1985). Factors defined by negatively keyed items: The result of careless respondents? *Applied Psychological Measurement, 9*(4), 367-373.

Schnake, M. (1991). Organizational citizenship: A review, proposed model, and research agenda. *Human Relations, 44*, 735-759.

Schriesheim, C., Kinicki, A., & Schreisheim, J. (1979). The effect of leniency on leader behavior descriptions. *Organizational Behavior and Human Performance, 23*, 1-29.

Seibert, S., Kraimer, M., & Crant, J. (2001). What do proactive people do? A longitudinal model linking proactive personality and career success. *Personnel Psychology, 54*, 845-874.

Siemsen, E., Roth, A., & Oliveira, P. (2010). Common method bias in regression models with linear, quadratic, and interaction effects. *Organizational Research Methods, 13*, 456-476.

Simmering, M., Fuller, C., Richardson, H., Ocal, Y., & Atinc, G. (under review). Marker variable choice, reporting, and interpretation in the detection of common method variance: A review and demonstration. *Organizational Research Methods*, 1-44.

Sims, H. J., Szilagyi, A., & Keller, R. (1976). The measurement of job characteristics. *Academy of Management Journal, 19*, 195-212.

Smith, D., & Robie, C. (2004). The implications of impression management for personality research in organizations. In B. Schneider, & D. Smith (Eds.), *Personality and organizations* (pp. 111-138). Mahwah, New Jersey: Lawrence Erlbaum Associates.

Smither, J. W., Collins, H., & Buda, R. (1989). When rate satisfaction influences performance evaluations: A case of illusory correlation. *Journal of Applied Psychology, 74*, 599-605.

Spector, P. E. (1987). Method variance as an artifact in self-reported affect and perceptions at work: myth or significant problem. *Journal of Applied Psychology, 72*, 438-443.

Spector, P. E. (2006). Method variance in organizational research: truth or urban legend? *Organizational Research Methods, 9*, 221-232.

Spector, P. E., & Brannick, M. T. (2009). Common method variance or measurement bias? The problem and possible solutions. In D. A. Buchanan, & A. Bryman (Eds.), *The Sage Handbook of Organizational Research Methods* (pp. 346-362). Los Angeles, CA: Sage.

Staw, B. M. (1975). Attribution of the "causes" of performance: A general alternative interpretation of cross-sectional research on organizations. *Organizational Behavior and Human Decision Processes, 13*, 414-432.

Steenkamp, J.-B. E., De Jong, M. G., & Baumgartner, H. (2010, April). Socially desirable response tendencies in survey research. *Journal of Marketing Research, 47*, 199-214.

Teo, T. (2011). Considering common method variance in educational technology research. *British Journal of Educational Technology, 42*(5), 94-96.

Thomas, K. W., & Kilmann, R. H. (1975). The social desirability variable in organizational research: An alternative explanation for reported findings. *Academy of Management Journal, 18*, 741-752.

Thornton, G. (1980). Psychometric properties of self-appraisals of job performance. *Personnel Psychology, 33*, 259-291.

Torangeau, R., Rips, L. J., & Rasinski, K. A. (2000). *The Psychology of Survey Response.* London: Cambridge University Press.

Viswesvaran, C., & Ones, D. (1999). Meta-analysis of fakability estimates: Implications for personality measurement. *Educational and Psychological Measurement, 59*, 197-210.

Wainer, H., & Kiely, G. L. (1987). Item clusters and computerized adaptive testing: a case for testlets. *Journal of Educational Measurement, 26*, 247-260.

Watson, D., & Clark, L. (1984). Negative affectivity: The disposition to experience negative aversive emotional states. *Psychological Bulletin, 96*, 465-490.

Watson, D., Clark, L., & Tellegen, A. (1988). Development and validation of brief measures of positive and negative affect: The PANAS scales. *Journal of Personality and Social Psychology, 54*(6), 1063-1070.

Weigters, B., Geuens, M., & Schillewaert, N. (2010). The individual consistency of acquiescence and extreme response style in self-report questionnaires. *Applied Psychological Measures, 34*, 105-21.

Weijters, B., Geuens, M., & Schillewaert, N. (2010). The stability of individual response styles. *Psychological Methods, 15*, 96-110.

Weijters, B., Geuns, M., & Schillewarert, N. (2009). The proximity effect: the role of inter-item distance on reverse-item bias. *International Journal of Research Marketing, 26*, 2-12.

Weijters, B., Schillewaert, N., & Geuns, M. (2008). Assessing response styles across modes of data collection. *Journal of Academy of Marketing Science, 36*, 409-422.

Williams, L. J., & Anderson, S. E. (1994). An alternative approach to method effects by using latent-variable models: applications in organizational behavior research. *Journal of Applied Psychology, 79*, 462-468.

Williams, L. J., Hartman, N., & Cavazotte, F. (2010). Method variance and marker varaibles: a review and comprehensive CFA marker technique. *Organizational Research Methods, 13*, 477-514.

Williams, L., & Anderson, S. (1991). Job satisfaction and organizational commitment as predictors of organizational citizenship and in-role behaviors. *Journal of Management, 17*(3), 601-617.

Williams, L., & Brown, B. (1994). Method variance in organizational behavior and human resources research: Effects on correlations, path coefficients and hypothesis testing. *Organizational Behavior and Human Decision Processes, 57*, 185-209.

Williams, L., Cote, J., & Buckley, M. (1989). Lack of method variance in self-reported affect and perceptions at work: reality or artifact? *Journal of Applied Psychology, 74*, 462-468.

Winkler, J., Kanouse, D., & Ware, J. (1982). Controlling for acquiescence response set in scale development. *Journal of Applied Psychology, 67*, 555-561.

Yang, J. M. (2006). Trust in organizations: A multi-bases, multi-foci investigation. *Paper presented at the 21st Annual Meeting of the Society for Industrial and Organizational Psychology.* Dallas, TX.

Yang, J., Mossholder, K., & Peng, T. (2009). Supervisory procedural justice effects: The mediating roles of cognitive and affective trust. *The Leadership Quarterly, 20,* 143-154.

Ziegler, M. (2011). Applicant faking: A look into the black box. *The Industrial-Organizational Psychologist,* 49, 29-38.

Ziegler, M., & Buehner, M. (2009). Modeling socially desirable responding and its effects. *Educational and Psychological Measurement, 69,* 548-567.