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# Accounting for Common Method Variance in Cross-Sectional Research Designs

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Cross-sectional studies of attitude–behavior relationships are vulnerable to the inflation of correlations by common method variance (CMV). Here, a model is presented that allows partial correlation analysis to adjust the observed correlations for CMV contamination and determine if conclusions about the statistical and practical significance of a predictor have been influenced by the presence of CMV. This method also suggests procedures for designing questionnaires to increase the precision of this adjustment.

Many theories used in applied psychology are based on the premise that behavior is a function of individuals' attitudes, beliefs, or perceptions of the situations in which they find themselves. For practical reasons, tests of theories such as Vroom's (1964) valence–instrumentality–expectancy theory and Fishbein and Ajzen's (1975) theory of reasoned action often have cross-sectional rather than longitudinal designs. This means that individuals' reports of their internal states are collected at the same time as their reports of their past behavior related to those internal states. Consequently, the possibility arises that method variance (MV) has inflated the observed correlations between these two types of variables artifactually. Indeed, Feldman and Lynch (1988) have argued that the behavioral self-reports could be significantly correlated with job dimensions that are completely meaningless to the respondents if they are asked to report their behavior and then provide ratings of job characteristics related to that behavior. More recent studies suggest that the MV problem is not this severe but does require further study (Crampton & Wagner, 1994; Harrison, McLaughlin, & Coalter, 1996).

Many researchers have contended that MV problems can be solved by using multiple methods of measurement, and some have advocated analysis of multitrait–multimethod (MTMM) matrices by means of confirmatory factor analysis (Millsap, 1990; Mitchell, 1985; Podsakoff & Organ, 1986; Williams, Cote, & Buckley, 1989). However, MTMM matrix designs require at least twice as many measures as a conventional design. This forces the researcher either to limit the scope of a study (i.e., the number of constructs to be measured) or to accept a reduced response rate when some members of a sample refuse to complete a lengthy questionnaire.

To avoid these unappealing alternatives, some researchers have sought procedures that allow the effect of a method factor to be partialed out. Podsakoff and Todor (1985) argued that a pervasive method factor would manifest itself in a factor analysis and that partialing out the first factor would remove MV. They recognized that this would be a conservative procedure because some construct variance would be partialed out along with MV, but Kemery and Dunlap (1986) showed that the negative bias in this procedure is so large that partialing out the first factor would produce virtually meaningless results. Podsakoff and Organ (1986) later agreed with Kemery and Dunlap (1986), stating that "under these circumstances, interpretation of the results becomes difficult, if not impossible" (p. 537) and recommending that the variance accounted for by the first factor be reported only as a last resort.

## A Method Variance Model

A clearer understanding of the MV problem can be gained by using the factor analysis model to illustrate the differences among the independent error (IE), common method variance (CMV), and unrestricted method variance (UMV) models. Specifically, the factor analysis model asserts that $R = R^* + \Theta$, where $R$ is the observed matrix of correlations among the variables, $R^*$ $(=\Lambda\Phi\Lambda')$ is the matrix of correlations among the variables reproduced from the factor loadings ($\Lambda$) and the intercorrelation among the factors ($\Phi$), and $\Theta$ is a matrix of error variances (Hayduk, 1988). Under the IE model, $\Theta$ is assumed to be diagonal (i.e., all off-diagonal elements are fixed at zero). By contrast, the UMV model asserts only that $\Theta$ is not diagonal in form. This model can be considered the most general model because it allows the error terms to be correlated but does not fix or constrain the intercorrelations among those error terms.

The CMV model is less restrictive than the IE model but more restrictive than the UMV model because it asserts that the observed variables are contaminated by a single unmeasured factor that has an equal effect on all of them. The CMV model is illustrated graphically in Figure 1, which shows two underlying constructs, $X_1^*$ and $X_2^*$, each of which has a single indicator, $X_1$ and

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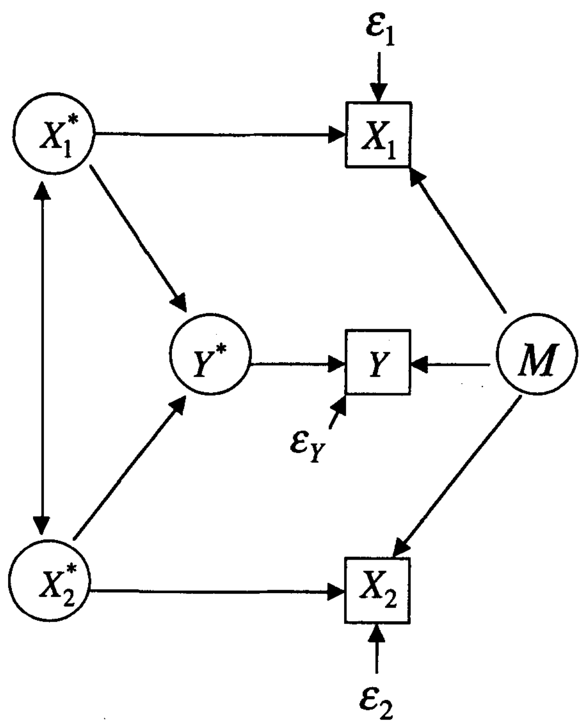


*Figure 1.* Path model of causal variables measured with error confounded with a method variable.

$X_2$, respectively. Both $X_1^*$ and $X_2^*$ cause $Y^*$, which also has a single indicator, $Y$. The CMV model assumes that $M$ has a constant correlation, $r$ (which may turn out to be zero but is not assumed a priori to be so), with all of the manifest variables. Thus, all the off-diagonal elements in $\Theta$ are constrained to be equal to $r$ (cf. Kemery & Dunlap, 1986). The variable $M$ in the CMV model can be interpreted as an unmeasured relevant cause (L. R. James, Mulaik, & Brett, 1982) that is common to all of the observed variables in the model. If $M$ is removed from Figure 1, the CMV model reduces to the IE model. If each of the observed variables has a distinct method variable, $M_i$, and all of the $M_i$s are allowed to be intercorrelated with each other, then the CMV model generalizes to the UMV model (cf. Williams & Brown, 1994).

If the manifest variables are measured without error and the rules of path analysis (Heise, 1975) are applied to Figure 1, one obtains three equations of the form,

$$r_{ij} = r_{ij}^* + r_{iM}r_{jM}, \tag{1}$$

where $r_{ij}$ is an observed correlation between the manifest variables $X_i$ and $X_j$, $r_{ij}^*$ is the corresponding correlation between the underlying constructs $X_i^*$ and $X_j^*$, and $r_{iM}$ and $r_{jM}$ are correlations between the method variable and the manifest variables. Thus, if the manifest variables are measured without error, $r_{1Y} = r_{1Y}^* + r_{1M}r_{YM}$, $r_{2Y} = r_{2Y}^* + r_{2M}r_{YM}$, and $r_{12} = r_{12}^* + r_{1M}r_{2M}$. Moreover, if $M$ is common to $X_1$, $X_2$, and $Y$, then $r_{1M} = r_{2M} = r_{YM}$. Thus, if the $r_{ij}^*$ = 0, then $r_{ij} = r_{iM}^2 = s_M^2$. That is, an upper bound for CMV, $s_M^2$, can be estimated by any of the values of $r_{ij}$. Unfortunately, it is not always possible to determine if any of the $r_{ij}^* = 0$ and, if so, which one.

There are two ways to address this problem. The best way is for the researcher to include a scale that is theoretically unrelated to at least one other scale in the questionnaire, so there is an a priori justification for predicting a zero correlation. Alternatively, the zero correlation can be identified ad hoc. If the $r_{ij}$s differ from each other—as they almost certainly will—it is necessary to determine which one of them provides the best estimate. Lindell and Brandt (2000) have recently argued that the smallest correlation among the manifest variables provides a reasonable proxy for CMV, but they have not provided a rigorous justification for this claim. Their conjecture can be supported by rearranging the terms in Equation 1 so that $r_{ij}^* = r_{ij} - r_{iM}r_{jM}$ and letting the term on the right-hand side of the minus sign be represented by $r_S$. If $r_S = \min(r_{ij})$, then the smallest value of $r_{ij}^*$ will be zero and the other values of $r_{ij}^*$ will be positive. Conversely, if $r_S$ takes any value larger than $\min(r_{ij})$, then some values of $r_{ij}^*$ will be negative. This latter condition is very important because negative correlations make no sense if one assumes that the method variable is independent of the constructs and that the variables can be reflected so that the intercorrelations among the variables are all positive. Of course, the true correlation between two variables can be negative when there is a linear constraint, such as when the sum of two or more variables is a constant. However, such situations appear to be rare, so $r_S = \min(r_{ij})$ generally will provide the best estimate of CMV.

This analysis indicates that CMV can be identified if at least one correlation between constructs equals zero. This condition will exist if researchers design their questionnaires to include a single variable that is theoretically unrelated to at least one other variable in the model. This theoretically unrelated variable provides discriminant validity to the design, albeit not to the extent that is present in the factorial manipulation characteristic of a classical MTMM design. In keeping with Podsakoff and Organ (1986) and Kemery and Dunlap (1986), Equation 1 shows that each of the observed correlations will be inflated by the square of the common method correlation, but it is only the smallest observed correlation ($r_S$) that estimates CMV. Consequently, it is the smallest correlation—not the first factor—that must be partialed out of the remaining correlations. Notwithstanding the fact that $r_S$ must be smaller than the first factor (which is equivalent to the average of the correlations), it also is likely to overestimate $r_{iM}$ because Equation 1 shows that $r_S$ is likely to contain construct variance as well as MV, just as the first factor does. However, the upward bias in $r_S$ can be expected to be much smaller than the corresponding bias in the first factor reported by Kemery and Dunlap.

### *Effects of Sampling Error*

It also is important to recognize that $r_S$ is subject to sampling error when, as ordinarily is the case, it is calculated from sample rather than population data. Consequently, $r_S$ could be spuriously low in a given set of data because of sampling fluctuations. Specifically, ad hoc selection of the smallest correlation provides an opportunity for capitalization on chance that is the mirror image of the problem with ad hoc selection of the largest correlation in stepwise regression analysis. Downward bias in $r_S$ will tend to increase with the number of scales from which the ad hoc selection takes place and decrease with the size of the sample from which the correlations are calculated. The downward bias due to these factors will tend to offset the upward bias in $r_S$ that is caused by inclusion of (true score) variance in $r_S$. Unfortunately, it is unclear to what degree the downward bias and upward bias offset each other and, thus, which of these two effects will predominate in any given situation.

Uncertainty about the magnitude of $r_S$ can be determined by conventional procedures for calculating a confidence interval around a correlation coefficient. The upper and lower bounds of a $p$th percentile confidence interval are given by

$$z_{r,p} = z_r \pm (z_{1-\alpha/2})/\sqrt{N-3}, \quad (2)$$

Using Fisher's $r$-to-$z$ transformation to compute $z_r$ and where $z_{1-(\alpha/2)}$ is the corresponding percentile in the unit normal distribution and $N$ is the sample size (Glass & Stanley, 1970). Thus, a plausible upper bound for $s_M^2$ can be estimated from the upper limit of the confidence interval for $r_S$.

Typically, researchers are less interested in estimating $s_M^2$ than in estimating the correlation of a predictor variable with a criterion variable after the estimated effect of CMV has been controlled. Accordingly, one would want to test whether partialing CMV out of the observed correlations reduces those correlations to statistical nonsignificance and, thus, changes the conclusions that can be drawn from the data. A partial correlation, $r_{Yi \cdot M}$, which shows the relationship between $Y$ and $X_i$, controlling for $M$, can be expressed as

$$r_{Yi \cdot M} = \frac{r_{Yi} - r_{iM}r_{YM}}{\sqrt{1-r_{iM}^2}\sqrt{1-r_{YM}^2}}, \quad (3)$$

where $r_{Yi}$ is the correlation coefficient suspected of being contaminated by CMV, $r_{iM}$ is the correlation of the corresponding predictor variable with the method factor, and $r_{YM}$ is the correlation of the criterion variable with the method factor. Equation 3 can be simplified as

$$r_{Yi \cdot M} = \frac{r_{Yi} - r_S}{\sqrt{1-r_S}\sqrt{1-r_S}}, \text{ or } r_{Yi \cdot M} = \frac{r_{Yi} - r_S}{1-r_S}. \quad (4)$$

This partial correlation also is subject to sampling error, and its confidence interval can be computed using the test statistic,

$$t_{\alpha/2, N-3} = \frac{r_{Yi \cdot M}}{\sqrt{(1-r_{Yi \cdot M}^2)/(N-3)}}. \quad (5)$$

Equation 4 indicates that when all of the variables are measured without error, there must be an adjustment for CMV in both the numerator and denominator. If $r_S = r_{Yi}$, then $r_{Yi \cdot M} = 0$, but as $r_S \to 0$, $r_{Yi \cdot M} \to r_{Yi}$. An important implication of this result is that a researcher should design a cross-sectional study to include at least one scale that is expected on the basis of prior theory to be unrelated to the criterion variable (i.e., $r_{Yi} = 0$). This will assure that $r_S$ is as close as possible to zero. Moreover, a context effect is most likely to be shared between a predictor and criterion if it is located adjacent to the criterion variable in the questionnaire (Harrison et al., 1996). The inclusion of a theoretically unrelated, proximally located MV marker variable is likely to provide a satisfactory proxy for $M$ by addressing both CMV and serial-position effects.

### *Effects of Measurement Error*

Of course, it is possible that $r_S$ is low, not because of a construct's irrelevance but because of the unreliability with which it has been measured. If this is the case, one would expect the values of $r_{ij}$ to be biased downward (Lord & Novick, 1968), leading to an underestimation of $s_M^2$ and an overestimate of $r_{Yi \cdot M}$. This suggests that adjustments for CMV should be made using disattenuated correlations such as

$$\hat{r}_{ij} = \frac{r_{ij}}{\sqrt{r_{ii}}\sqrt{r_{jj}}},$$

where $\hat{r}_{ij}$ is the disattenuated correlation between $X_i$ and $X_j$, $r_{ij}$ is the corresponding zero-order correlation, and $r_{ii}$ and $r_{jj}$ are the reliabilities of $X_i$ and $X_j$, respectively. Disattenuated correlations, $\hat{r}_{ij}$, can be substituted into Equation 4 in place of $r_{Yi}$ and $r_S$, yielding a disattenuated partial correlation,

$$\hat{r}_{Yi \cdot M} = \frac{\left(\frac{r_{Yi}}{\sqrt{r_{ii}}\sqrt{r_{YY}}}\right) - \left(\frac{r_S}{\sqrt{r_{MM}}\sqrt{r_{YY}}}\right)}{1 - \left(\frac{r_S}{\sqrt{r_{MM}}\sqrt{r_{YY}}}\right)},$$

where $r_{MM}$ is the reliability of the MV marker variable. In turn, this equation can be reexpressed as

$$\hat{r}_{Yi \cdot M} = \frac{\left(\frac{r_{Yi}\sqrt{r_{MM}} - r_S\sqrt{r_{ii}}}{\sqrt{r_{ii}}\sqrt{r_{YY}}\sqrt{r_{MM}}}\right)}{1 - \left(\frac{r_S\sqrt{r_{ii}}}{\sqrt{r_{ii}}\sqrt{r_{YY}}\sqrt{r_{MM}}}\right)} \text{ or}$$

$$\hat{r}_{Yi \cdot M} = \frac{r_{Yi}\sqrt{r_{MM}} - r_S\sqrt{r_{ii}}}{\sqrt{r_{ii}}\sqrt{r_{MM}}\sqrt{r_{YY}} - r_S\sqrt{r_{ii}}}. \quad (6)$$

This is not an easy equation to interpret, but it is informative to examine the case of equal reliabilities among the variables (i.e., $r_{ii} = r_{MM} = r_{YY}$), so that Equation 6 reduces to

$$\hat{r}_{Yi \cdot M} = \frac{(r_{Yi} - r_S)}{(r_{ii} - r_S)}. \quad (7)$$

Comparison of Equations 7 and 4 indicates that when the variables are not all perfectly reliable, an observed correlation coefficient, $r_{Yi}$, must be reduced by exactly the same estimate of CMV ($r_S$) as is used in the numerator of Equation 4. However, the disattenuated correlation in Equation 7 is adjusted in the denominator by subtracting the estimate of CMV from the (common) reliability of the variables instead of from unity (which is the reliability of the variables when they are measured without error). The denominator of Equation 7 almost always will be smaller than the denominator of Equation 4, so $\hat{r}_{Yi \cdot M} \geq r_{Yi \cdot M}$. That is, adjusting for unreliability always decreases the estimated impact of CMV. Of course, Equation 6 reduces to Equation 4 when the variables are all perfectly reliable (i.e., $r_{ii} = 1$).

## An Empirical Basis for the Common Method Variance Model

On the face of it, the equal-weights assumption in the CMV model appears to be quite restrictive because it seems unlikely that the method factor $M$ has exactly the same impact on all of the observed variables. However, this limitation may be more apparent than real if the equal-weighted model provides little loss in predictive power in an estimation sample and better predictive power on cross-validation than differential weights. That is, the real question is whether the equal-weights assumption provides a reasonable approximation to the data.

This criterion of reasonable approximation is a common one in applied psychological research. For example, researchers routinely analyze their data as if they are of interval quality and have multivariate normal distributions with homoscedastic variances. Moreover, exploratory factor analyses commonly rely on orthogonal rotations even when the scales are expected to be intercorrelated and the items within scales are equally weighted even though their factor loadings vary. In all of these cases, the argument is made that the assumptions provide a reasonable approximation to the data. The question is not whether the assumptions are correct; it is understood that they are not. Rather, the question is whether the departures from the assumptions would change the ultimate conclusions.

Specific support for the argument that an assumption of equal weights in the CMV model will not distort the results significantly enough to alter conclusions can be found in the literature on equal versus differential weights in regression analysis (Dawes & Corrigan, 1974; Ree, Carretta, & Earles, 1998). To understand the relevance of this literature to the CMV model, it first is necessary to examine the nature of the method variable, *M*. Figure 1 represents *M* as a single unmeasured common cause, but this is only an approximation. As Table 1 illustrates, each scale score ($X_1$) is the sum of the items in that scale. In turn, each item ($x_{ij}$) is the sum of the true score ($x^*_{ij}$), random error ($\varepsilon_{ij}$), and context factors ($c_{ijk}$). The random errors are minor, independent, and unstable (L. R. James et al., 1982), whereas the context effects are stable and have nonminor direct influences.

The net effect of these context factors on each item is the sum of their individual effects, and the sum of the context factors on each scale is the sum of their effects on the items in that scale. Thus, *M* represents a sum of sums. When each scale has a large number of items with weakly to moderately positive correlations and similar weights and the items are influenced by context effects that meet similar conditions, Wilks's (1938) theorem will apply, and equal weights are likely to provide a reasonable approximation.

## Applying the Common Method Variance Model

Practical application of the CMV model should be conducted in two stages. The first stage involves designing a questionnaire to ensure that the proposed marker variable and the other scales come as close as possible to meeting the assumptions of the CMV model. The second stage is defined by the eight steps required to conduct the data analyses.

### *Questionnaire Design for the Common Method Variance Model*

Researchers' major concern about MV effects has been to eliminate them in all variables because this will yield more accurate estimates of zero-order correlation coefficients. However, the CMV model suggests that it also can be helpful to equate the variables with respect to MV susceptibility because MV-marker variable analysis can remove these effects to the degree that they are common to all variables. Thus, a secondary goal in questionnaire design is to construct the items so that the CMV assumptions are true, just as experiments are designed to make analysis of variance assumptions true.

Accordingly, the plausibility of the CMV model can be reinforced by devising items (and thus scales) that are similar in their susceptibility to such MV contaminants as transient mood states, response styles, acquiescence, illusory correlations, social desirability, similarity of semantic content, and proximity to the criterion variable. For example, items can be equated for familiarity (and face validity) by basing items on concepts that are derived from interviews with a pretest sample of potential respondents. This procedure has been advocated as a method of ensuring that respondents' salient beliefs are assessed (Fishbein & Ajzen, 1975) and minimizing the probability of creating pseudoattitudes that are dominated by context effects (Schuman & Kalton, 1985).

Moreover, acquiescence can be reduced by reverse scoring some of the items, and scales can be equated for acquiescence by making the same proportion of items in each scale reverse scored. Scales can be equated for susceptibility to contiguity effects by including the same number of items in each scale. Further, scales can be equated for proximity to the criterion if items from different scales

Table 1
*Hypothetical Scale Scores ($X_i$), Each Represented as the Sum of Items ($x_{ij}$) That in Turn Are the Sum of a True Score ($x^*_{ij}$), Random Error ($\varepsilon_{ij}$), and Common Context Factors ($c_{ijk}$)*

| Item | True score | Random error | Context factor 1 | Context factor 2 | Context factor 3 | Context factor 4 |
|---|---|---|---|---|---|---|
| $X_1$ | | | | | | |
| $x_{11}$ | $x^*_{11}$ | $\varepsilon_{11}$ | $c_{111}$ | $c_{112}$ | $c_{113}$ | $c_{114}$ |
| $x_{12}$ | $x^*_{12}$ | $\varepsilon_{12}$ | $c_{121}$ | $c_{122}$ | $c_{123}$ | $c_{124}$ |
| $x_{13}$ | $x^*_{13}$ | $\varepsilon_{13}$ | $c_{131}$ | $c_{132}$ | $c_{133}$ | $c_{134}$ |
| $X_2$ | | | | | | |
| $x_{21}$ | $x^*_{21}$ | $\varepsilon_{21}$ | $c_{211}$ | $c_{212}$ | $c_{213}$ | $c_{214}$ |
| $x_{22}$ | $x^*_{22}$ | $\varepsilon_{22}$ | $c_{221}$ | $c_{222}$ | $c_{223}$ | $c_{224}$ |
| $x_{23}$ | $x^*_{23}$ | $\varepsilon_{23}$ | $c_{231}$ | $c_{232}$ | $c_{233}$ | $c_{234}$ |
| $X_3$ | | | | | | |
| $x_{31}$ | $x^*_{31}$ | $\varepsilon_{31}$ | $c_{311}$ | $c_{312}$ | $c_{313}$ | $c_{314}$ |
| $x_{32}$ | $x^*_{32}$ | $\varepsilon_{32}$ | $c_{321}$ | $c_{322}$ | $c_{323}$ | $c_{324}$ |
| $x_{33}$ | $x^*_{33}$ | $\varepsilon_{33}$ | $c_{331}$ | $c_{332}$ | $c_{333}$ | $c_{334}$ |

are intermixed, but this procedure probably is unnecessary. An adequate level of equivalence in terms of proximity to the criterion can be achieved by interposing scales that are irrelevant to the hypotheses (i.e., "filler" scales). For this reason, it would be ideal to locate the MV-marker variable scale immediately after the theoretically relevant predictors and before the dependent variable.

It also is important to have the questionnaire be short enough that it avoids transient mood states such as boredom and fatigue. If respondents perceive a questionnaire as excessively long, irrelevant, or repetitive, they are likely to reduce their cognitive effort and shift from response accuracy to response speed as their goal for cognitive processing as they progress through the questionnaire. This would make the last items most susceptible to response styles, peripheral cues, acquiescence, distortion in the direction of consistency with previous responses, and stereotypic responding (such as all midrange responses or all extreme responses). However, this is likely to have little impact on items that require relatively little cognitive processing, such as behavioral self-reports and demographic items. Thus, these items should be placed at or near the end of the questionnaire.

Researchers should include one or more multiple marker variables that are designed to estimate the effect of CMV by being more similar to the criterion in terms of semantic content, close proximity, small number of items, novelty of content, and narrowness of definition (Harrison et al., 1996). For example, self-report of a theoretically irrelevant behavior would be expected to be very sensitive to the impact of similar semantic content and proximity to the true criterion variable if the latter also was a behavioral self-report. On the other hand, a theoretically irrelevant predictor could be designed to have fewer items and to be more novel in content and more narrowly defined than the other predictor variables.

### Data Analysis for the Common Method Variance Model

Once the questionnaire has been designed and distributed and data have been collected, the researcher should take the following steps to adjust estimated predictor–criterion correlations for CMV.

1. Eliminate artifactual negative correlations by reflecting (reverse scoring) any variables that have a preponderance of negative correlations with other variables. At the end of this step, the remaining negative correlations generally will be statistically nonsignificant and small. If there are many of these nonsignificant negative correlations, it is likely that the net effect of the context variables is zero. A large proportion of nonsignificant positive and negative correlations suggests that a variable has a true correlation of zero (i.e., there is error variance but neither true score nor MV).

2. Inspect any variables that have statistically significant negative correlations to determine if the negative correlation is due to a linear constraint. For example, negative correlations will appear if all values of a nominal variable have been dummy coded and included in the correlation matrix. Delete any one of these variables to eliminate the negative correlation.

3. Inspect each predictor variable's zero-order correlation for statistical significance. If it is not significant at this step, it will not be statistically significant after the adjustment for CMV.

4. Pick the smallest positive value of $r_{ij}$ or $r_{Yi}$ as the estimate of $r_S$. Using the smallest positive value of $r_{Yi}$ is more conservative because there almost always are fewer correlations between the predictors and the criterion than among the predictors, thus affording less opportunity for capitalization on chance in selection of the smallest correlation. This conservatism often would seem to be appropriate when a marker variable is an ad hoc estimate. Researchers may be able to justify using one of the values of $r_{ij}$ to estimate $r_S$ when the number of predictors is small, the sample size is large, and there is a compelling argument that the two variables from which $r_{ij}$ is calculated are in fact theoretically unrelated.

5. Use the selected value of $r_S$ in the partial-correlation adjustment described by Equation 4 and test its significance using Equation 5. If any zero-order correlations that were statistically significant in Step 3 remain significant, this suggests that the results cannot be accounted for by CMV.

6. Conduct a sensitivity analysis by computing the *p*th percentile points (e.g., 75th, 95th, 99th) of the confidence interval for $r_S$ and using these larger values in the partial-correlation adjustment. If any partial correlations that were statistically significant in Step 5 remain statistically significant, this provides added support for the conclusion that the obtained statistical significance cannot be accounted for by MV. Alternatively, one could use the second, third, or *k*th smallest positive value of $r_{ij}$ or $r_{Yi}$ as the estimate of $r_S$ and use this to assess the sensitivity of the conclusions to the particular value selected for $r_S$. However, estimating $r_S$ from the *k*th smallest correlation might not work well unless the number of variables in the analysis is large (e.g., 10 or more). In either case, the more stringent the test criterion that is passed successfully, the greater is the confidence in rejecting CMV as a plausible rival hypothesis.

7. Use Equation 6 to estimate the correlation corrected for unreliability and CMV, $\hat{r}_{Yi \cdot M}$. However, Equation 5 should not be used to test its statistical significance because the sampling distribution of $\hat{r}_{Yi \cdot M}$ is not the same as that of $r_{Yi \cdot M}$. Either jackknife or bootstrap methods (Efron, 1982) can be used to test the statistical significance of $\hat{r}_{Yi \cdot M}$.

8. As is the case with any other partial-correlation coefficient, the researcher should follow up the test of statistical significance by judging the practical significance of the obtained values of $r_{Yi \cdot M}$ and $\hat{r}_{Yi \cdot M}$. Accordingly, one should consider whether a predictor accounts for a meaningful proportion of the variance in the criterion variable.

## Example

Suppose that a researcher is conducting a study of the effects of organizational climate on member participation (effort, absenteeism, and turnover intentions) in a cross-sectional study. Four predictors, leader characteristics ($X_1$), role characteristics ($X_2$), team characteristics ($X_3$), and job characteristics ($X_4$) were identified a priori as being theoretically relevant to the criterion variable (L. A. James & James, 1989; Lindell & Brandt, 2000). Suppose further that a fifth predictor—marital satisfaction—was identified a priori as being theoretically unrelated to the criterion variable and, therefore, was placed in the questionnaire directly between the criterion variable and the other predictors to serve as the MV-marker variable.

Table 2 contains hypothetical correlations among four climate scales ($X_1$–$X_4$), the marital satisfaction scale ($X_5$), and a behavioral self-report of member participation ($Y$). The data support the researcher's model by confirming that three of the four theoreti-

Table 2
*Hypothetical Correlations Among Leader Characteristics, ($X_1$) Role Characteristics ($X_2$), Team Characteristics ($X_3$), Job Characteristics ($X_4$), Marital Satisfaction ($X_5$), and Self-Reported Member Participation (Y)*

| Scale | $X_1$ | $X_2$ | $X_3$ | $X_4$ | $X_5$ | $Y$ |
|---|---|---|---|---|---|---|
| $X_1$ | .93 | | | | | |
| $X_2$ | .22** | .89 | | | | |
| $X_3$ | .28** | .12 | .83 | | | |
| $X_4$ | .15* | .15* | .40** | .87 | | |
| $X_5$ | .14 | .09 | .15* | .12 | .84 | |
| $Y$ | .19** | .14 | .42** | .34** | .08 | .82 |
| $r_{Yi \cdot M}$ | .12 | .07 | .37** | .28** | .00 | |
| $\hat{r}_{Yi \cdot M}$ | .13 | .07 | .45 | .33 | .00 | |

*Note.* $N = 87$. Values on the diagonal are estimates of scale reliability.
* $p < .05$. ** $p < .01$.

cally relevant predictors have statistically significant correlations with the criterion variable, whereas the theoretically irrelevant predictor has a nonsignificant correlation with the criterion. Moreover, the correlations of the MV-marker variable with the other predictor variables are low. These low correlations with the other predictor variables further support the discriminant validity of the MV-marker variable.

Table 2 shows that the correlation for Variable 2 (role characteristics) is not significant even before the CMV adjustment is applied. Application of Equations 4 and 5, using $r_{Y5} = .08$ as the estimate of $r_S$, shows that controlling for CMV reduces the significant correlation for Variable 1 (leader characteristics) to statistical nonsignificance. However, the correlations of Variables 3 (team characteristics) and 4 (job characteristics) with the criterion remain statistically significant even when CMV is controlled. Moreover, these correlations have practical significance because they account for theoretically meaningful amounts of variance explained. Finally, as expected from the analysis in the previous section, application of Equation 6 shows that the disattenuated partial correlations of all four variables with the criterion are slightly higher than the corresponding first-order partial correlations.

Table 3 shows the results of the sensitivity analysis for different values of $r_S$. The stub column shows three alternative upper percentiles for the sampling distribution of $r_S$ (i.e., Type I error, $\alpha = 1 - p = .25$, .05, and .01). The first column displays the values of the unit normal distribution, $z_\alpha$, corresponding to each of the levels of $\alpha$. The second column contains values of $r'_S$ corresponding to the respective levels of $z_\alpha$. The values of $r'_S$ were computed using Equation 2. The last four columns contain the values of $r_{Yi \cdot M}$ for each of the theoretically relevant predictors at each of the three levels of $\alpha$.

These data show that the correlations for $X_3$ (team characteristics) and $X_4$ (job characteristics) remain greater than zero at all levels of $r'_S$. Moreover, the correlation for $X_4$ remains statistically significant beyond $p < .05$ (i.e., at $z_\alpha = 1.96$), and the correlation for $X_3$ remains statistically significant beyond $p < .01$ (i.e., at $z_\alpha = 2.57$). Thus, one can conclude that the correlations of $X_3$ and $X_4$ with $Y$ cannot reasonably be accounted for by CMV and that these two variables still retain their practical significance in terms of a meaningful amount of variance explained.

## Conclusion

Marker-variable analysis provides a useful extension of previous research on the MV problem. It is a significant improvement over partialing out the first factor, which severely overstates the impact of MV. It also is superior to the heuristic arguments about the magnitude of MV effects summarized by Podsakoff and Organ (1986), which are susceptible to being used to assume away MV effects. Finally, it is far superior to overlooking MV effects altogether, which seems to be a very common way of addressing this problem.

MV-marker-variable analysis can be used in conjunction with the results of Crampton and Wagner's (1994) work. Although these researchers concluded that MV effects in survey research seem to have been overstated, they noted that the seriousness of the problem appears to differ from one set of constructs to another in ways that cannot be attributed unequivocally to item similarity, construct abstractness, or an interaction between these two attributes. These results suggest that MV-marker-variable analysis should be conducted whenever researchers assess correlations that have been identified as being most vulnerable to CMV (e.g., self-reports of job satisfaction with ability and performance). For correlations with low vulnerability (e.g., self-reports of performance with role characteristics, job scope, and leader traits), conventional correlation and regression analyses may provide satisfactory results.

In addition to providing a simple computational method for adjusting cross-sectional correlations for contamination by CMV, MV analysis reinforces a very important principle for the design of cross-sectional surveys. Researchers should design their questionnaires to support a test of discriminant validity by deliberately including at least one MV-marker variable that meets three conditions. First, it must have documented evidence of high reliability. Thus, the variable should be measured by a multi-item scale, and the reliability of this scale (e.g., as measured by coefficient $\alpha$) should be reported. Second, the MV-marker variable must be theoretically unrelated to at least one of the other variables. Researchers should recognize that *theoretically unrelated* means something quite different from *theoretically distinct.* The latter means only that the two constructs do not measure exactly the same thing (i.e., the correlations between the true scores are $r^*_{ij} < 1$), whereas the former means that the two constructs are statistically independent of each other (i.e., the correlations between the true scores is exactly $r^*_{ij} = 0$). When the statistical independence of a candidate marker variable is in question, a researcher might design the questionnaire to include multiple marker variables. Conservatism can be added in the analysis stage by using an upper percentile value of $r_S$ (e.g., the 75th, 95th, or 99th percentile value) rather than $r_S$ itself.

Table 3
*Sensitivity Analysis on Estimated Values of $r_{Yi \cdot M}$ for $\alpha = .25$, .05, and .01*

| $\alpha$ | $z_\alpha$ | $r'_S$ | $r_{Y1 \cdot M}$ | $r_{Y2 \cdot M}$ | $r_{Y3 \cdot M}$ | $r_{Y4 \cdot M}$ |
|---|---|---|---|---|---|---|
| .25 | 1.52 | .19 | .00 | −.06 | .28 | .19 |
| .05 | 1.96 | .22 | −.04 | −.10 | .26 | .15 |
| .01 | 2.57 | .27 | −.11 | −.18 | .21 | .10 |

An adjustment for CMV will be most compelling if the questionnaire has been designed a priori to include an MV-marker variable. However, an analysis of CMV effects can be conducted even if a questionnaire has not been so designed. Inclusion of an MV-marker variable increases the likelihood of obtaining a small correlation, but the absence of one does not necessarily imply that the correlations will all be large. Thus, an analysis of CMV effects can be conducted on the correlation tables reported in previously published studies, to determine if CMV effects could plausibly have accounted for any statistically significant correlations reported there. Elimination of CMV artifacts would provide an important complement to the adjustments for sample size, unreliability, and variance restriction now commonly reported in meta-analyses (Hunter, Schmidt, & Jackson, 1982).

One limitation of the analytic method proposed here is that the correctness of the calculations in Equations 4 and 6 depends on the validity of the CMV model. If the UMV model is correct, all parameter estimates must be made using a structural equations model. Structural equations modeling is more flexible than marker-variable analysis because it is capable of testing MV models that are less restrictive than the CMV model (although the models that can be tested are subject to the limits of identifiability constraints). However, marker-variable analysis has the advantage of being a significant improvement over the IE model, which is the typical assumption. In addition, it is readily understood by anyone trained in statistical significance testing, requires only very simple computations, and can be done very quickly. The latter advantage is likely to prove to be particularly significant when it must be done many times, as when conducting a meta-analysis. It is important to understand the role of the assumption of CMV, that is, that the method variable has equal weight on all of the manifest variables. It is quite unlikely that the method variable will have exactly equal weight on all of the manifest variables; however, a variety of analysts have concluded that equally weighting predictor variables often does not cause a significant loss in predictive power in an estimation sample and usually has superior cross-validation in a holdout sample because both sampling and psychometric errors promote overfitting of the data (Dawes & Corrigan, 1974; Ree et al., 1998). This research suggests that the critical question is not whether the assumption of equal weights is violated at all (which is a virtual certainty), but rather if violations are so large that they alter the conclusions about the statistical and practical significance of the predictor–criterion correlations.

This reasoning suggests that the CMV model needs only to provide a reasonable approximation to the data. Equal weighting is more likely to be plausible when the dependent variable is a self-reported behavior than when it is a global evaluation and also is more likely when global evaluations are preceded by both positive and negative perceptions rather than just one or the other. These effects are most likely when all of the predictor variables are equally similar to the criterion in terms of semantic content, number of items, novelty of content, and scope of definition (cf. Harrison et al., 1996). Of course, it is impossible for all of the predictor variables to have equal proximity to the criterion variable. However, as noted earlier, locating the marker variable between the predictor and criterion variables will tend to produce a greater MV effect for the marker variable than for the substantive predictors, thus producing a conservative estimate.

The literature on equal weighting suggests that the greater plausibility of the UMV model does not come without a price; estimating all available parameters is likely to substantially overfit the data. Thus, the relative merits of the UMV and CMV models must be determined empirically. Future research should examine whether the CMV model does, in fact, provide a reasonable approximation to the data in estimation samples and better performance than the UMV model on cross-validation.

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