You Gotta Keep Em' Separated: The Efficacy of Proximal Remedies for Method Variance

ABSTRACT

Scholars have argued that method factors, such as common rater effects, bias estimates of covariation from same-source and single time-point investigations. In response, researchers have proposed procedural remedies. For such studies, recommended remedies include (1) presenting participants with a cover story to disguise the purpose of the survey (which addresses respondents' ability to produce data consistent with researchers' hypotheses), (2) randomizing item and scale presentation around filler scales (which addresses item and scale context effects), and (3) introducing a brief temporal separation (which addresses respondents' momentary mood). Though researchers have relied upon these proximal method variance remedies, there are no studies examining whether they nullify method variance. Here, we present the findings from two experiments utilizing the same measurement model and demonstrate that such remedies do, indeed, reduce (and in some instances, eliminate) the presence of method variance attributable to (1) consistency motifs, (2) context effects, and (3) mood. However, these sources of method variance did not substantially bias our findings. Rather, other sources of method variance (i.e., positive affectivity and negative item wording) consistently biased estimates. We conclude with recommendations for researchers wishing to addresses method bias in their same-source investigations.

Keywords:

method variance, proactive personality, job performance

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Few methodological problems have been discussed more than the presence and impact of method variance (Spector, Rosen, Richardson, Williams, & Johnson, in press). Method variance is variation in observations attributable to methodological sources rather than to substantive or theoretical constructs of interest (Campbell & Fiske, 1959). Though long debated (e.g., Lance & Simonovsky, 2015; Richardson, Simmering, & Sturman, 2009; Spector, 2006), method variance is often viewed as problematic in management research, where researchers may rely upon observations made using sources believed to share common method variance (CMV), which is variation in observations caused by use of a common method (Podsakoff, MacKenzie, Lee, & Podsakoff, 2003). CMV can threaten the substantive knowledge base, and procedural solutions have been proposed to address this problem (Podsakoff, MacKenzie, & Podsakoff, 2012). The proposed solutions are often simple to apply, such as using a cover story to disguise the purpose of a study, counterbalancing item presentation, and separating measures by an interval of time.

Unfortunately, procedural remedies have not received close examination, particularly using truly experimental designs comparing observations obtained with remedies to those without. Prior tests (e.g., Johnson, Rosen, & Djurdjevic, 2011) did not experimentally examine how or to what extent these remedies reduce the presence of method variance. Yet, such experiments are needed because they can demonstrate the extent to which a remedy addresses a proposed cause of method variance (e.g., participants' use of consistency motif, respondent mood), which may result in reduced method variance and less biased results.

Thus, in two experiments, we examined the efficacy of procedural remedies for different proximal causes of CMV and also examined the role played by other proximal causes of method variance. In both studies, we randomly assigned individuals to one of two conditions where remedies were either applied or not applied. This allows us to test the role of purported mechanisms of method variance and also to test the extent to which biased estimates are produced. As a contribution, we provide an experimental test of the efficacy of procedural remedies. Further, drawing on a recent distinction between common and uncommon method variance (see Spector et al., in press), we demonstrate the extent to which these sources of method variance contaminate scales and bias estimates of effect sizes.

THEORETICAL OVERVIEW OF METHOD VARIANCE

Differing perspectives exist regarding the terms 'method,' 'method variance,' and 'method bias.' Podsakoff et al. (2012) argue that ‘method’ encompasses several abstract elements (e.g., taking a paper-and-pencil instrument, responding using Likert scales), and that when these elements are shared across measures in the same investigation, this convergence results in bias. By contrast, Lance, Dawson, Birkelbach, and Hoffman (2010) defined method as alternative means for enumerating observations (e.g., self- vs. other-report, explicit vs. implicit assessment) to indicate standings on latent traits, a position that has been criticized as omitting common rater sources of method variance (e.g., consistency motifs). Method variance refers to the impact of a methodological aspect of study on a particular observation(s) (e.g., mood affecting observations obtained with a single-source single time-point design), which differs from the extent to which method variance is shared across methods, inflating parameter estimates (e.g., Williams & Anderson, 1994). This distinction is important because method variance can be unique to a specific set of observations within a dataset, such as when halo effects contaminate supervisor appraisals of employee performance but not subordinate measures. Indeed, such uncommon method variance, which introduces variation into a specific set of measures, attenuates observed covariances (Spector et al., in press). While both inflation and attenuation are forms of method bias, accounting for both common and uncommon method variance will result in more precise parameter estimates and stronger tests of theory (Spector et al., in press).

This manuscript examines the role of proximal causes of method variance (common rater effects, and item and measurement context effects) that are believed to corrupt the item response process, contaminate observations, and ultimately bias estimates of covariation (Podsakoff et al, 2003). We focus on single-source research designs as they are prevalent in organizational science due to lower cost and reduced administrative burden, and information regarding the efficacy of the proposed remedies can help researchers make the most of such designs. Furthermore, single-source designs should be ripe for method variance because the cognitive processes of retrieval (i.e., remembering responses to previously answered items rather than the whole of one's experience), judgment (i.e., falsely judging that one has effectively retrieved relevant memories), and in some instances reporting (i.e., modifying one's responses to be consistent) may not be addressed unless a remedy is applied (see Podsakoff et al., 2003, 2012).

Common Rater Effects

Common rater effects are any artifactual covariation introduced by gathering observations on or from the same rater (Podsakoff et al., 2003). Of the many common rater effects, there are four that are particularly relevant for same-source single time-point investigations: consistency motifs, implicit theories, mood states, and trait affectivity factors. Consistency motifs refer to the propensity for respondents to maintain consistency in their responses to questions, in which the desire to appear rational should affect response reporting (Podsakoff et al., 2003). Research indicates that consistency motifs contaminate measures and may even bias results (Harrison, McLaughlin, & Coalter, 1996). Similarly, implicit theories refer to respondents’ beliefs about the covariation among particular traits, behaviors, and or outcomes that may not accurately reflect reality (Sternberg, Conway, Ketron, & Bernstein, 1981). These biases are believed to affect judgments of response appropriateness (Podsakoff et al., 2003). In research settings, if participants are aware of the study purpose, it could equip them with an understanding of the hypotheses, inclining them to respond in a manner that is consistent with these hypotheses.

Different from consistency motifs and implicit theories are the interrelated notions of mood states and trait affectivity, both of which may be a common source of method variance. Mood states refer to respondents' momentary or brief mood state, which can influence the contents of what is recalled from memory by priming similarly valenced material stored in memory (cf. Blaney, 1986; Parrott & Sabini, 1990). Sometimes this has a desirable effect, such as when mood-congruency facilitates recall (Bower, 1981; Isen & Baron, 1991). Mood state should also be related to general affectivity, which is a respondent's propensity to view themselves or their environments in positive or negative ways (Podsakoff et al., 2003). Similar to mood, trait affectivity should exert comparable effects on the item response process and may explain the effect of momentary mood state on the item response process. Brief, Burke, George, Robinson, and Webster (1988) observed that negative affectivity contaminates observations of stress, job and life satisfaction, and depression, resulting in biased estimates of covariation (see also Chen & Spector, 1991, Jex & Spector, 1996; Williams & Anderson, 1994).

To address consistency motifs and implicit theories, researchers have used cover stories (e.g., Harrison et al., 1996), which are designed to both disguise the hypotheses under investigation and dispel implicit theories held by the participants (Podsakoff et al., 2003). To address the role of mood states, researchers recommend separating measures temporally to increase the amount of time between when observations of predictor and criterion variables are made (Podsakoff et al., 2003; 2012). Though it involves separation by time, this remedy addresses a proximal cause of method variance (i.e., momentary mood state). Despite wide use of these procedural remedies, there is no experimental evidence that they are effective.

Relevant Context Effects

Context effects refer to any artifactual covariation among observations caused by an item or scale's location or relationship to other items or scales in a survey (e.g., item/scale priming and embeddedness effects). Podsakoff et al. (2003) proposed that item context can affect retrieval and judgments of response appropriateness. Tourangeau, Rasinski, and D'Andrade (1991) demonstrated that participants respond more quickly to similar items placed closer together rather than further apart. Tourangeau et al.'s (1991) findings suggest that item intercorrelations may be a function of the consistent ordering of items within a particular instrument (i.e., serial-order effects). Essentially, if responses made to previous items or scales are maintained in short-term memory, these responses may influence responses to future items or scales. Steinberg (1994) found that one item in a 20-item scale became slightly more discriminating when it was presented later in the scale rather than as the first item, which suggests that a systematic item context effect can artificially increase the systematic variance explained by a latent trait. Furthermore, when negative scales were presented first, estimated inter-scale correlations were much higher, suggesting the presence of scale embeddedness effects. To address item priming and item embeddedness effects, researchers have proposed counterbalancing or randomizing the order of items or scales within a survey, particularly around one or more filler scales (Podsakoff et al., 2003). Randomizing items within a scale is assumed to eliminate item-serial-order effects; randomizing scale presentation should eliminate scale-serial-order effects; and introducing filler scales makes it more difficult to recall responses to prior scales, eliminating recall effects. No research has examined this remedy.

In line with Spector et al.'s (in press) call for controlling both common and uncommon method variance, we built in measures of method variance causes that were shared across our measures and unique to specific measures. In both studies, we used the same measurement model and tested the same underlying theory; namely, that proactive personality relates positively to voice, taking charge, task performance (i.e., in-role behavior), and contextual performance (i.e., OCB). We chose this model because prior research (Fuller and Marler, 2009) suggests that these relationships may be a function of CMV; that is, same-source meta-analytically derived correlations among variables were between 129-308% higher than those from multi-source data.

STUDY 1

In study 1, we examined the role of consistency motifs, implicit theories, and context effects. As described previously, cover stories cover story and randomizing items and scales around a filler scale should reduce the presence of method variance. Thus, we hypothesize that:

> Hypothesis 1: Proximal remedies of (a) use of a cover story and (b) randomizing items and scales will reduce method variance in same-source data.

In this study, the presentation of items within scales, as well as the entire scales, were randomized around filler scales, which were scales that measured other sources of method variance unaddressed by our chosen procedural remedies (e.g., positive and negative affectivity, and momentary mood). Yet, these common rater effects – specifically mood, positive affectivity, and negative affectivity—are likely to produce method effects. Therefore, we predict:

> Hypothesis 2: Common rater effects (mood, positive affectivity, and negative affectivity) will produce method variance in same-source data.

METHOD - STUDY 1

Sample and Procedure

Amazon's Mechanical Turk workers were recruited for \$1.30 for completing a survey, to which 621 responded and 556 agreed to participate after reading the informed consent. These 556 participants were then randomly assigned to one of two conditions: non-remedied (n = 274) or remedied (n = 282). Approximately 75% (n = 204) in the non-remedied and approximately 50% (n = 143) in the remedied condition correctly responded to the manipulation check of proper interpretation of the study purpose and were allowed to continue. Respondents' data were eliminated due to incorrectly or not responding to an attention check item (n=42), or not reporting demographics, abandoning the survey, or not completing an entire measure (n = 6). This final sample consisted of 299 individuals (control n = 182; experimental n = 117).

Those in the non-remedied condition were presented with a transparent study description: "The purpose of this study is to test for relationships between proactive personality and workplace behaviors (including taking charge at work, having a voice in the workplace, organizational citizenship behavior at work, and job performance)." Those in the remedied condition were presented a cover story to disguise the purpose of the study: "In this study, you will be asked to respond to statements about yourself and how you behave at work. Separate researchers built the content of this survey for separate purposes, and so the questions may or may not relate to one another. As such, there are no right or wrong answers, so please provide honest responses." Participants then responded to this item indicating their understanding of the study purpose: "Before you take our survey, please tell us which of the following correctly describes the purpose of this study." Response options were: (a) "The purpose of this study is to test for relationships between personality and workplace behaviors. As such, there is a clear purpose to the study." (b) "Separate researchers built the content of this survey for separate purposes, and so the questions may or may not relate to one another. As such, there is no single clear purpose to this study." Individuals who chose the appropriate response corresponding to the condition they were in were allowed to participate.

Following the instructions, participants in the non-remedied condition were given a survey with all items and scales presented in the same order (followed by demographics). For the remedied condition, two blocks/webpages of scales were created, one for proactive personality and another for the criteria scales, which were intermixed (see Podsakoff et al., 2003). We separated proactive personality from the criteria scales to minimize the CMV attributable to context effects. These two blocks were then randomized around five attitudinal filler scales (included as part of a separate investigation) that were intended to tax short-term memory, making it more difficult to recall responses to the substantive measure that was randomly administered at the beginning of the survey, thus minimizing the influence of recall effects (Podsakoff et al., 2003). Items within all scales were also randomized to minimize the role of serial-order effects. This survey concluded with demographic items.

Measures

Proactive personality. Bateman and Crant's (1993) 10-item proactive personality scale was used to capture proactive personality ($\alpha_{\text{non-remedied}} = .87$; $\alpha_{\text{remedied}} = .90$). This, and all scales included in this study, utilized a five-point Likert rating scale with anchors of 1 (strongly disagree) and 5 (strongly agree).

Voice. Van Dyne and LePine's (1998) 6-item scale, based on a modification of a previously published scale, was used to measure voice ($\alpha_{\text{non-remedied}} = .89$; $\alpha_{\text{remedied}} = .84$).

Taking charge. Morrison and Phelp's (1999) 10-item measure of taking charge was used for this study ($\alpha_{\text{non-remedied}}$ = .91; α_{remedied} = .93).

In-role and organizational citizenship behavior. Williams and Anderson's (1991) In-Role Performance Behavior (IRB) and Organizational Citizenship Behavior (OCB) scales, the latter of which includes OCBI (OCB directed at the individual) and OCBO (OCB directed at the organization), were used as dependent variables (IRB; $\alpha_{\text{non-remedied}}$ = .84; α_{remedied} = .78; OCBI; $\alpha_{\text{non-remedied}}$ = .83; α_{remedied} = .82; OCBO; $\alpha_{\text{non-remedied}}$ = .75; α_{remedied} = .66).

Consistency motif. To capture the presence of a consistency motif, we employed four sets of psychometric synonyms and antonyms on a 5-point agreement scale (Goldberg & Kilkowski, 1985). These items were interspersed throughout the substantive measures in the survey, capturing the consistency motif that might be contaminating responses to substantive items. Four scores (one per synonym/antonym pair) were calculated and then summed to capture the extent to which responses were inconsistent across item pairs, with higher scores corresponding to higher levels of inconsistent responding.

Positive and negative affectivity. To assess positive (PA) and negative affectivity (NA), we used the positive and negative affect schedule (Watson, Clark, & Tellegen, 1988), and participants were asked to read each item and then select the responses that indicate the extent to which they generally feel, on average. Coefficient alpha reliabilities for these scales were .93 (non-remedied) and .91 (remedied) for PA and .91 (non-remedied) and .93 (remedied) for NA.

Momentary mood state. Momentary mood state was measured with a single item ("My mood today can best be described as…") using a seven-point scale (1 = *unplesant*; 7 = *pleasant*). Across both conditions, momentary mood correlated positively with PA ($r_{non\text{-}rem.}$ = .56, p < .001;

$r_{rem.}$ = .47, $p < .001$) and negatively with NA ($r_{non\text{-}rem.}$ = -.52, $p < .001$; $r_{rem.}$ = -.55, $p < .001$), indicating construct validity.

Analytical Approach

We tested for the presence of method variance using latent variable modeling strategies (e.g., Williams & McGonagle, 2015). We applied the same latent variable model to the remedied and non-remedied observations separately, following Johnson et al.'s (2011) approach. A series of different models varying in their assumptions regarding the role of method factors were fit to the data and compared using chi-square difference tests, which were carried out using 'lavaan' in R. To explicate, method variance attributable to a specific cause could present equivalent effects on all measures (tested using constrained/fixed effects, or 'method-C' models) or vary in its extent and impact across all or only a few measures (tested using unconstrained effects, or 'method-U' models) (e.g., Williams & McGonagle, 2015). Both assumptions were tested for each source of method variance. Items with statistically significant (p<.05) method factor loadings were included in our method effects models. The resulting models were then compared, using chi-square difference tests, to determine the best relative fit. With the exception of affective factors (in which trait factors caused momentary mood), all method factors were modeled as independent of one another (Conway & Lance, 2010). We then conducted a variance decomposition analysis, which allowed assessment of extent to which different sources of method variance influenced each measure.

Method bias was tested by comparing the best-fitting method effect model to a model with biased coefficients (i.e., method-R; Williams & McGonagle, 2015). The method effect model would be the best-fitting model from a comparison of method-C and method-U models, which contained all statistically significant method effects. A statistically significant chi-square difference between method-U and method-R models suggests that modeled sources of method variance have biased estimates of these substantive regression effects (Williams & McGonagle, 2015). This test for bias allows us to conclude if effect size estimates are biased by measured causes, which for our study include consistency motifs, mood, trait affectivity, negative item wording, and item/scale intermixing. Lastly, to estimate the amount of method bias introduced by a source of method variance, we examined the difference in standardized regression effect estimates when a specific source of method variance was and was not accounted. For instance, if mood effects are detected, then standardized regression effect estimates from a model containing all method effects would be compared to a model that ignored the effects of mood, and the difference between these estimates could indicate bias (see Spector et al., in press).

Due to negatively worded items in our model, we created a negatively-worded method factor that explained variance in these items only (see Dalal & Carter, 2015). This method factor was theoretically independent from the other latent factors given the assumption that methods be uncorrelated with traits (Conway & Lance, 2010). Also, we modeled an unmeasured latent method construct in the remedied condition to control for method variance shared by our criteria measures attributable to those items being on the same webpage. Item parceling was employed following guidance by Hall, Snell, and Foust (1999). We used exploratory factor analysis to inform the construction of three to five parcels per trait. Parcels were designed to combine unmodeled yet secondary influences shared amongst a set of items into a parcel, increasing the accuracy of our parameter estimates (i.e., factor loadings and latent construct correlations).

RESULTS AND DISCUSSION - STUDY 1

We first conducted a manipulation check to determine if the cover study affected consistency motifs. Results of an independent samples *t* test were statistically significant (t =

2.015, df = 297, *p* = .045) and revealed a small to moderate effect (*d* = 0.24, 95% CI [.01, .72]), suggesting that consistency motifs were stronger in the non-remedied condition (*M* = 1.62, *SD* = 1.46) and weaker in the remedied condition (*M* = 1.98, *SD* = 1.60), where higher scores indicate more inconsistent responding. While scores in both conditions suggest that participants were generally consistent, there is evidence that our cover story weakened consistency motifs.

To test Hypothesis 1a, we examined the method effects present in the remedied and non-remedied group. Results (in Table 1) reveal the presence of unequal (i.e., congeneric) method variance (see method-U) in both conditions. A test for method bias revealed that effect size estimates were biased, to varying degrees, by these method effects. Table 2, which depicts the pattern of method variance loadings for both conditions, reveals two consistency motif effects in the non-remedied condition; specifically, consistency motifs contaminated two parcels: one reflecting OCBI (λ_{method} = .14) and another reflecting OCBO (λ_{method} = .13). The pattern suggests that higher inconsistency scores resulted in slightly more positive responses to items in these parcels. No consistency motif effects were observed in the remedied condition. Table 3 contains the results of a variance decomposition of each scale, which allows for relative comparisons of the contaminating effects played by consistency motifs and other method factors. In the non-remedied condition, consistency motifs comprised 12% of the method variance contaminating the OCBI measure (or 3% of the systematic variance) and 7% of method variance contaminating the OCBO measure (or 5% of the systematic variance). Consistency motif effects were not observed in our remedied condition, providing partial support for Hypothesis 1a.

As a test of Hypothesis 1b (the remedy of randomizing items/scales), we compared substantive reliability estimates across the two conditions for each measured variable. As expected, the substantive reliability estimates were higher in the non-remedied condition compared to the remedied condition due to context effects. For instance, the substantive variance estimate for proactive personality was 74% in the non-remedied condition and 66% in the remedied condition. Randomizing items and scales reduced the systematic impact of these context effects by between 9% and 33%, providing support for Hypothesis 1b.

As a follow-up test to Hypothesis 1, we compared effect size estimates from models where a specific source of method variance was and was not accounted (i.e., method factor loadings were modeled or not) to determine the extent to which method factors biased estimates of our standardized regression effects. The difference between the estimates from these models was then taken, which allowed for an estimate of bias introduced to a predictor-criterion estimate as a function of a specific source of method variance. For consistency motifs, we compared the standardized coefficients from our non-remedied condition. Our comparisons suggested that consistency motifs did not bias substantive effect size estimates (i.e., all differences in standardized coefficients were less than .005). Rather, other method factors played a stronger role. Thus, while the consistency motif was present, it was not biasing.

Hypothesis 2 anticipated that method variance would be present in our data due to the common rater effects of mood, positive affectivity, and negative affectivity. Table 2 indicates this is true across all substantive measures in both conditions, with an average $\lambda_{method} = .32$ across all variables (PA corresponding to more positive responses to scales). In the remedied condition (controlling for all sources of method variance), PA substantially inflated standardized regression estimates linking proactive personality to voice (by .03), taking charge (by .07), IRB (by .06), OCBI (by .12), and OCBO (by .20). Negative affectivity affected one parcel reflecting voice in the remedied condition, where higher NA corresponds to slightly less positive responses to items, though this method effect was not potent enough to cause a substantial amount of bias.

Momentary mood played a more prominent role than NA. In the non-remedied condition, mood affected responses to parcels that reflected taking charge (λ_{method} = -.15), IRB (λ_{method} = .10), OCBI (λ_{method} = .22), and OCBO (λs_{method} = .24 and .20). In the remedied condition, momentary mood only affected one parcel that reflected IRB (λ_{method} = .16), though this was the same parcel that was affected in the non-remedied condition. Given that momentary mood effects were observed more frequently in the non-remedied condition, we compared effect sizes from this condition that did and did not account for momentary mood. Mood effects were potent enough to attenuate, by .01, the proactive personality–OCBO standardized regression estimate, and mood comprised of 24% of the method variance and 16% of the systematic variance for the OCBO measure (an example of uncommon method bias). All other predictor-criterion relationships were unaffected by momentary mood.

Another source of bias was negative item wording, which consistently contaminated the IRB measure (non-remedied λ_{method} = .41, comprising 39% and 11% of the method and systematic variance, respectively; remedied = λ_{method} .64, comprising 33% and 12% of the method and systematic variance, respectively). It also consistently contaminated the OCBO measure (non-remedied λ_{method} = .43, comprising 35% and 21% of the method and systematic variance, respectively; remedied λ_{method} = .87, comprising 41% and 30% of the method and systematic variance, respectively). Negative item wording reflected a stronger source of uncommon method bias, as estimates from the remedied condition revealed that negative item wording attenuated standardized regression effect size estimates linking proactive personality to IRB (by .02), OCBI (by .02), and OCBO (by .08). As our criteria were allowed to covary and OCBI was strongly correlated with IRB and OCBO, the method bias estimate for the proactive personality–OCBI link was also affected. Lastly, item/scale intermixing exerted a constant method effect across the measurement model (λ_{method} = .24), comprising between 33% (OCBO) and 54% (IRB) of the method variance that contaminated our criterion measures. However, this was a relatively weak source of bias, as it minimally inflated (by .01) standardized estimates linking proactive personality to IRB and OCBI. Item/Scale intermixing also minimally attenuated (by .01) the standardized effect size estimate linking proactive personality with voice.

STUDY 2 - TEMPORAL SEPARATION OF MEASURES

A second popular procedural remedy for common method variance—temporal separation of measures – was investigated in Study 2. As temporal separation is expected to reduce common rater effects, we tested whether using a one-week temporal separation of measurement reduced the presence and impact of mood on both predictor and criterion measures. Thus, we predict:

Hypothesis 3: Temporal separation of measures will reduce the biasing role of method variance introduced by momentary mood.

METHOD - STUDY 2

Sample, Measures, Procedure, and Analysis

Participants (paid $5 to complete this survey) were recruited using SurveyMonkey and randomly assigned to conditions where they either received all measures at the same time (i.e., non-remedied) or a temporal separation of one week was used to divide the predictor and criterion measures. In the remedied condition, proactive personality and momentary mood were administered in a survey, and one week later voice, taking charge, and the in-role and organizational citizenship behavior measures were administered along with the method factor measures used previously (i.e., affectivity), a demographics questionnaire, and another measure of momentary mood. Participants' data was used if there were no more than two missing items on the independent and dependent variables. For the non-remedied data, 150 of 190 respondents

(78.9%) provided usable data. The temporally separated condition resulted in high attrition; only 183 (59.6%) of the 307 initial respondents provided usable data in the second wave.

RESULTS AND DISCUSSION - STUDY 2

Before testing Hypothesis 3, we sought to determine whether this study captured momentary mood and whether it was distinct from general affectivity. The test-retest correlation for the mood item in the remedied condition (one-week separation) was $r = .44$ ($p < .001$), which indicates some instability in momentary mood. To address construct validity, across both groups, the daily mood measure correlated positively with PA (rem.: $r = .27$, $p = .008$; non-rem.: $r = .37$, $p = .002$). For NA, there was a consistent negative relationship across both conditions, though the relationship was only statistically significant in the remedied condition (rem.: $r = -.24$, $p = .018$; non-rem.: $r = -.12$, $p = .313$). Additionally, there was a statistically significant negative correlation between momentary mood and NA in the remedied condition ($r = -.29$, $p < .001$; non-rem. $r = -.17$, $p = .068$). This suggests that we captured the presence of a momentary state of pleasantness or unpleasantness, but not trait affectivity. A confirmatory factor analysis of mood, PA, and NA indicated that these were unique, but related, constructs.

To test Hypothesis 3 and determine whether the one-week separation of measurement reduced the presence and biasing role of method variance by momentary mood, we split the data into separate groups and specified the same causal model as used in Study 1 (proactive personality predicting voice, taking charge, in-role behavior, and organizational citizenship behavior). Method effects present in each group are presented in Table 1. As in Study 1, unequal method variance was present in both conditions, attributable to captured causes of method variance. Tests revealed that effect size estimates were biased to varying degrees by method effects. Table 2 reveals a single momentary mood effect (.13) on a parcel reflecting OCBI in the non-remedied condition. No mood effects were observed in the remedied conditions. Table 3 indicates relative comparisons of the contaminating effects played by mood and other method factors. In the non-remedied condition, mood comprised 12% and 2% of the method and systematic variance, respectively, contaminating the OCBI measure. As no mood effects were observed in the remedied condition, we provide evidence that separating measures by one week can eliminate the presence of momentary mood effects.

To test the degree to which mood effects might bias standardized regression estimates, we compared estimates from models where the method factor was and was not accounted for. The difference between estimates from these models was used to estimate the bias associated with a specific cause of method variance. Our comparisons revealed no substantial biasing role of momentary mood (i.e., all differences were less than .004), supporting Hypothesis 3. Thus, a one-week separation of measurement, though weakening the role of method variance attributable to mood, was not needed for this measurement model because mood played such a weak role. So, while temporal separation may reduce bias due to mood state, the bias caused may not be large enough alone to warrant separation, because other method factors played a stronger role.

Although we did not hypothesize the role of common rater effects beyond mood in Study 2, we conducted post hoc analyses of affectivity. As was seen in Study 1, PA played a strong role, relating to all measures across both conditions, with λ_{method} averaging .37 (higher PA corresponded to more positive responses to measures). Whether this is contaminating method variance or meaningful substantive variance, however, is unknown. NA also played a prominent role, though weaker and less consistent than PA. Further, for the non-remedied condition, negative affectivity, when contaminating measures, exhibited negative loadings onto these positive measures (average $\lambda_{method} = -.24$), which suggests that higher NA scores corresponded to more negative responses to items. This pattern is consistent with research on affect as a cause of method variance (e.g., Williams & Anderson, 1994). However, NA played a relatively stronger and more complex contaminating role in our remedied condition. For example, negative loadings linked negative affectivity to both IRB and OCBO (average $\lambda_{method} = -.37$). However, positive loadings (average $\lambda_{method} = .16$) linked NA to some parcels that reflected proactive personality, voice, taking charge, and OCBI.

The extent to which these method effects biased effect size estimates was calculated. In a pattern similar to that found in Study 1, PA upwardly biased effect sizes to varying degrees. Inflation was strongest (by .14) with the standardized regression effect linking proactive personality and OCBO. Additionally, negative item wording, a source of method variance unique to IRB and OCBO measures, strongly attenuated estimates of covariation with proactive personality, with the strongest attenuation effect (-.47) in the proactive personality- OCBO relationship. Given that these effects act in opposing ways, but that negative item wording is stronger overall, the overall estimate of this standardized effect size would be somewhat attenuated by negative item wording unless this factor was modeled. Nevertheless, accounting for both sources of bias is preferred to accounting for only one or the other.

GENERAL DISCUSSION

The results of our investigation revealed that proximal remedies do, indeed, reduce the presence of method variance targeted by such remedies. Study 1 showed that context effects can be reduced by randomizing items and scales around filler scales. More specifically, randomizing items and scales reduced the inflating effects that occur when items and scales were presented in a consistent order. We also found that consistency motifs can be weakened by a cover story, as consistency motif effects were only observed in the non-remedied condition. In Study 2, mood effects were nullified by separating measurements of predictors and criterion variables by one week. Specifically, momentary mood effects were observed only in our non-remedied condition.

While these findings support the viability of proximal remedies for causes of method variance, they also revealed that, with the possible exception of context effects, these remedies address relatively minor sources of method bias that were unique and inconsistent in their effects. For instance, small biasing effects were observed for consistency motifs and mood. Yet, our findings suggest that other biases which are unaffected by these remedies are more important (i.e., PA, which consistently inflated effect size estimates across both studies). As PA is an important and stable trait in organizational behavior research, researchers should statistically account for this likely source of method variance. Additionally, negative item wording emerged as another consistently biasing method factor that exerted an attenuating effect on relationships when negative items were present in a measure. Thus, researchers may want to avoid using negatively worded items. Indeed, echoing Podsakoff et al. (2003) our results suggest that using both procedural and statistical remedies is important to reducing less-biased effect size estimates.

Despite addressing relatively minor and inconsistent causes of bias, these proximal remedies may be useful when researchers believe that other sources of method variance might play a complementary role. This might occur when independent method effects (e.g., positive affectivity and negative item wording) are common contaminants for a measurement model. This is because minor sources of method variance may have relatively independent and incremental effects that could be insignificant in isolation but potent in combination. Researchers may also wish to use these remedies to address uncommon method variance.

Comparing our findings to effects reported by Fuller and Marler (2009) suggests that more research is needed into the role that method variance plays into these substantive relationships. Fuller and Marler's (2009) estimate for the proactive personality–voice relationship is much smaller (ρ = .31) than the estimate provided by our two studies (ρ = .65-.76). Yet, it is not clear why this disparity exists. The original meta-analysis may not have accounted for unique sources of method variance, which would systematically attenuate estimates. Or, this disparity in findings could be social desirability, which is likely correlated with proactive personality and PA, but was not measured in this research. Finally, we conducted several post hoc tests to determine if other sources of CMV could have created these effects; yet, our subsequent tests of common scale format and anchor effects provided no evidence of this. A test of evaluation consistency bias (i.e., halo factor) on these positively valenced items was conducted using the unmeasured latent method factor modeling technique, yet it failed to run. Interestingly, after accounting for method factors, we found more conservative estimates of the relations between proactive personality to task performance (ρ = .39) and to contextual performance (ρ = .56) than did Fuller and Marler (2009). In our data, the proactive personality–IRB relationship (ρ = .15-.33) and the proactive personality–OCBI/O relationships (ρ = .14-.49) after accounting for method factors, resulted in statistically non-significant relationships. Additionally, prior estimates were likely inflated by PA, as we provide evidence that method variance dominated the OCBO measurement model (74% of reliable variance in Study 1 and 63% in Study 2). We call for future research into the role of method variance in the assessment of OCB. Based on our findings, we recommend that researchers relying on same-source research designs may be wise to use separation remedies to reduce the presence and impact of method effects, even though these procedural remedies may not address important sources of method variance. Additionally, as suggested by others (i.e., Spector et al., in press), there may be specific sources of method variance for which to account in order to produce less biased estimates.

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TABLE 1
Chi-square, goodness-of-fit values, model comparison tests (consistency motifs and context effects remedied), and standardized regression effects.

		Study 1 (consistency motifs and context effects remedied)									
		Model–Data Fit Statistics					Standardized Regression Effects				
Model	Condition	χ^2	*df*	CFI	RMSEA [90%CI]	SRMR	PP–VC	PP–TC	PP–IRB	PP–OCBI	PP–OCBO
Step 1. Initial CFA											
	Non-remedied	652.68	404	0.92	0.058 [.050, .066]	0.07	0.79*	0.79*	0.43*	0.62*	0.46*
	Remedied	642.75	405[a]	0.89	0.071 [.060, .081]	0.09	0.81*	0.74*	0.24*	0.46*	0.52*
Step 2. Baseline Models											
	Non-remedied	775.50	443	0.89	0.064 [.057, .072]	0.15	0.77*	0.77*	0.38*	0.61*	0.41*
	Remedied	732.05	443	0.86	0.075 [.065, .084]	0.18	0.81*	0.74*	0.23*	0.45*	0.45*
Step 3. Method Effects											
Method-C	Non-remedied	763.51	439	0.89	0.064 [.056, .071]	0.13	0.75*	0.75*	0.32*	0.57*	0.33*
	Remedied	711.30	439	0.87	0.073 [.063, .082]	0.15	0.79*	0.72*	0.18	0.39*	0.33
Method-U	Non-remedied	674.82	418	0.91	0.058 [.050, .066]	0.08	0.69*	0.68*	0.22	0.49*	0.20
	Remedied	642.00	427	0.90	0.066 [.055, .076]	0.13	0.77*	0.67*	0.15	0.33	0.24
Step 4. Tests for Method Bias											
Method-R	Non-remedied	681.81	423	0.91	0.058 [.050, .066]	0.09					
	Remedied	950.53	432	0.83	0.081 [.074, .088]	0.13					
χ^2 **Model Comparisons**		$\Delta\chi^2$	Δdf	**χ^2 Critical Value: .05**							
1. Baseline vs. Method-U	Non-remedied	100.69*	25	37.65							
	Remedied	90.05*	16	26.30							
2. Method-U vs. Method-R	Non-remedied	6.99	5	11.07							
	Remedied	308.53*									
(Study 2 below)		Model–Data Fit Statistics					Standardized Regression Effects				

Model	Condition	χ^2	*df*	CFI	RMSEA [90%CI]	SRMR	PP–VC	PP–TC	PP–IRB	PP–OCBI	PP–OCBO
Step 1. Initial CFA											
	Non-remedied	650.98	408	0.93	0.058 [.049, .066]	0.06	0.78*	0.77*	0.40*	0.41*	0.44*
	Remedied	703.69	408	0.91	0.07 [.061, .079]	0.09	0.79*	0.68*	0.36*	0.53*	0.48*
Step 2. Baseline Model											
	Non-remedied	789.01	437	0.90	0.067 [.059, .074]	0.16	0.77*	0.76*	0.38*	0.39*	0.40*
	Remedied	809.75	437	0.89	0.076 [.068, .084]	0.16	0.79*	0.66*	0.36*	0.52*	0.46*
Step 3. Method Effects											
Method-C	Non-remedied	747.46	434	0.91	0.063 [.056, .071]	0.14	0.72*	0.71*	0.31*	0.31*	0.29*
	Remedied	794.58	434	0.89	0.075 [.067, .083]	0.15	0.78*	0.63*	0.35*	0.49*	0.43*
Method-U	Non-remedied	628.59	413	0.94	0.054 [.045, .062]	0.08	0.65*	0.62*	0.25*	0.18	0.14
	Remedied	630.19	405	0.93	0.062 [.052, .071]	0.07	0.76*	0.56*	0.33	0.42*	0.39
Step 4. Tests for Method Bias											
Method-R	Non-remedied	642.38	418	0.94	0.055 [.046, .063]	0.09					
	Remedied	797.76	410	0.89	.072 [.065, .080]	0.14					
χ^2 Model Comparison Tests		**$\Delta\chi^2$**	**Δ*df***	**χ^2 Critical Value: .05**							
1. Baseline vs. Method-U	Non-remedied	160.43*	24	40.11							
	Remedied	181.32*	32	26.30							
2. Method-U vs. Method-R	Non-remedied	13.79*									
	Remedied	167.57*	5	11.07							

Note: Method effects models reported are those that best fit their respective datasets. The focus was on the results of predictor–criterion relationships that might be typical of a management investigation. Method-C model includes method effects that are equal to like causes (e.g., method effects attributable to positive affect are equivalent but distinct in magnitude from negative affect and other method causes). Method-U includes unequal method effects. Fit indices: '*df*' = degrees of freedom; 'CFI' = comparative fit index; 'TLI' = Tucker-Lewis index; 'RMSEA' = root mean square error approximation; 'SRMR' = standardized root mean square residual. Correlations: 'PP–VC' = proactive personality–voice; 'PP–TC' = proactive personality–taking charge; 'PP–IRB' = proactive personality–in-role behavior; 'PP–OCBI' = proactive personality–coworker-directed organizational citizenship behavior; 'PP–OCBO' = proactive personality–organization-directed organizational citizenship behavior.

[a] - An additional degree of freedom is granted due to the use of a common blocking factor modeled via unmeasured latent method construct technique.

* $p < .05$

TABLE 2
Method-U Factor Loadings (Completely Standardized Solutions) – Study 1 and 2

Study 1	Substantive Factors						Method Factors					
Item	PP	VC	TC	IRB	OCBI	OCBO	PA	NA	Mood	CM	NIW	I/S
PPp1	.54/.61						.31/.35					
PPp2	.64/.68						.44/.50					
PPp3	.78/.79						.46/.44					
PPp4	.61/.47						.30/.55					
VCp1		.80/.83					.45/.29					/.24
VCp2		.64/.60					.32/					/.24
VCp3		.70/.53					.38/.24	/-.25				/.24
TCp1			.79/.83				.47/.39					/.24
TCp2			.73/.89				.32/.19					/.24
TCp3			.53/.74				.42/.30		-.15/.16			/.24
IRBp1				.82/.98			.25/		.10/			/.24
IRBp2				.62/.55			.30/				.41/.43	/.24
IRBp3				.23/.22								/.24
OCBIp1					.66/.88		.24/.21		.22/			/.24
OCBIp2					.68/.59		.24/.18					/.24
OCBIp3					.50/.32		.23/.27			.14/		/.24
OCBOp1						.47/.40	.19/.20		.24/			/.24
OCBOp2						.46/.39	.20/.36		.20/	.13/		/.24
OCBOp3						.46/.49	.22/				.64/.87	/.24
PAp1							.86/.81					
PAp2							.74/.65					
PAp3							.72/.64					
PAp4							.90/.90					
PAp5							.75/.69					
NAp1								.76/.90				
NAp2								.89/.85				
NAp3								.72/.89				
NAp4								.73/.77				
NAp5								.78/.75				
Mood									1/1			
CMI										1/1		

Study 2	Substantive Factors						Method Factors			
Item	PP	VC	TC	IRB	OCBI	OCBO	PA	NA	Mood	NIW
PPp1	.51/.72						.49/.34	-.18/		
PPp2	.68/.70						.55/.32	-.09/.13		
PPp3	.61/.72						.55/.37	/.21		
PPp4	.64/.74						.51/.34	/.13		
PPp5	.52/.56						.38/.37	/.13		
VCp1		.70/.78					.46/.24			
VCp2		.62/.75					.41/.20	/.11		
VCp3		.84/.89					.46/.30			
TCp1			.83/.71				.45/.38	/.22		
TCp2			.72/.65				.51/.40	/.18		
TCp3			.61/.84				.47/.32			
TCp4			.59/.62				.49/.38			
TCp5			.66/.75				.48/.25	/.12		
IRBp1				.86/.88			.20/27	/-.20		
IRBp2				.52/.14				-.28/-.51		.50/.74
IRBp3				.36/.39						
OCBIp1					.90/.90		.33/.33	/.18		
OCBIp2					.71/.69		.32/.35			
OCBIp3					.64/.66		.23/.37		/.13	
OCBOp1						.60/.63	.26/.33			
OCBOp2						.71/.66	.31/.36			
OCBOp3						.32/.06		-.39/.-.51		.71/.77
PAp1							.98/.92			
PAp2							.77/.76			
PAp3							.79/.85			
NAp1								.78/.80		
NAp2								.85/.91		
NAp3								.75/.86		
NAp4								.76/.84		
NAp5								.83/.91		
Mood									1/1	

Note: Numbers before the / are from the remedied condition, and numbers that appear after the / are from the non-remedied condition. All paths statistically significant ($p < .05$) except IRBp2 and OCBOp3. PP = 'proactive personality'; VC = 'voice'; TC = 'taking charge'; IRB = 'in-role behavior'; OCBI = 'organizational citizenship behavior - individual'; OCBO = 'organizational citizenship behavior - organization'; PA = 'positive affectivity'; NA = 'negative affectivity'; CM = 'consistency motif'; NIW = 'negative item wording'; I/S = item/scale intermixing.

TABLE 3
Reliability decomposition from Model-U (Study 1 and 2)

Study 1				Specific Method Reliability (%)					
Latent Factor	Cond.	Substantive Rel.(%)	Overall Method Rel. (%)	PA	NA	Mood	CM	NIW	I/S
	NR	0.62 (74%)	0.22 (26%)	.22 (100%)					
PP	R	0.57 (66%)	0.3 (34%)	.30 (100%)					
	NR	0.66 (78%)	0.19 (22%)	.19 (100%)					
Voice	R	0.51 (63%)	0.30 (37%)	.10 (36%)	.05 (17%)				.14 (47%)
	NR	0.66 (76%)	0.21 (24%)	.26 (100%)					
TC	R	0.65 (70%)	0.27 (30%)	.15 (55%)					.12 (45%)
	NR	0.51 (71%)	0.21 (29%)	.55 (52%)		.02 (10%)		.08 (39%)	
IRB	R	0.49 (64%)	0.27 (36%)			.03 (13%)		.09 (33%)	.15 (54%)
	NR	0.55 (75%)	0.19 (25%)	.13 (67%)		.04 (21%)	.02 (12%)		
OCBI	R	0.48 (63%)	0.28 (37%)	.13 (48%)					.15 (52%)
	NR	0.28 (37%)	0.48 (63%)	.16 (34%)		.12 (24%)	.03 (7%)	.16 (35%)	
OCBO	R	0.21 (26%)	0.59 (74%)	.15 (26%)				.24 (41%)	.19 (33%)
Study 2				Specific Method Reliability (%)					
Latent Factor	Cond.	Substantive Rel. (%)	Overall Method Rel. (%)	PA	NA	Mood	NIW		
	NR	0.48 (71%)	0.20 (29%)	.18 (90%)	.02 (10%)				
PP	R	0.62 (81%)	0.14 (19%)	.11 (75%)	.03 (25%)				
	NR	0.64 (78%)	0.18 (22%)	.18 (100%)					
Voice	R	0.86 (87%)	0.13 (13%)	.11 (86%)	.02 (14%)				
	NR	0.61 (67%)	0.31 (33%)	.31 (100%)					
TC	R	0.66 (72%)	0.26 (28%)	.20 (77%)	.06 (23%)				
	NR	0.53 (76%)	0.17 (24%)	.03 (20%)	.05 (29%)		.09 (51%)		
IRB	R	0.30 (40%)	0.45 (60%)	.07 (16%)	.18 (41%)		.19 (43%)		
	NR	0.71 (83%)	0.14 (17%)	.12 (88%)		.02 (12%)			
OCBI	R	0.68 (77%)	0.20 (23%)	.17 (86%)	.04 (24%)				
	NR	0.42 (52%)	0.39 (48%)	.13 (34%)	.09 (23%)		.16 (42%)		
OCBO	R	0.22 (37%)	0.36 (63%)	.11 (31%)	.12 (34%)		.13 (35%)		

Note: PP = proactive personality; TC = taking charge, PA = positive affectivity, NA = negative affectivity, CM = consistency motif, NIW = negative item wording, I/S = item/scale intermixing, NR = non-remedied condition, R = remedied condition. For substantive and overall method reliability percentages, the percent figures represent the amount of systematic (i.e., non-random) variance partitioned according to method vs. substantive sources. For the specific method sources, the percentages reflect the amount of method variance attributed to a specific cause of method variance.