Methodological Threat or Myth?

Evaluating the Current State of Evidence on Common Method Variance in HRM Research

Accepted Version *Human Resource Management Journal*.

Cite as:
Bozionelos, N., & Simmering, M. (2022). Methodological threat or myth? Evaluating the current state of evidence on common method variance in HRM research. Human Resource Management Journal, 32(1), 194-215. DOI: 10.1111/1748-8583.12398

Methodological Threat or Myth?

Evaluating the Current State of Evidence on Common Method Variance in HRM Research

Abstract

New key evidence on Common Method Variance (CMV) has been generated in the last decade (including quantitative and qualitative reviews, and simulations) to estimate its real validity threat, and evaluate the *post hoc* techniques to detect and correct for its effects. This work looks at the new evidence, and reviews all HRM-related empirical articles published in the last 10 years in six major journals. The following primary conclusions are drawn. First, adoption of new knowledge about CMV by the empirical literature has been uneven. Second, published research in these journals indicates few incidences of meaningful distortion of estimates due to CMV, even when *post hoc* tests are used to detect it. Third, these findings in the empirical literature mirror the conclusions of reviews and simulations of the last 10 years, which indicate that the probability of significant distortion of estimates because of CMV is very limited.

Keywords: Common method variance; common method bias; *post hoc* methods; evidence; simulations; reviews; coefficients; distortion; estimates

Methodological Threat or Myth?

Evaluating the Current State of Evidence on Common Method Variance in HRM Research

Few methodological topics can match the attention that common method variance (CMV) has received. CMV is believed to exist when shared error variance is created by use of a method that influences substantive relationships between variables. This systematic variance is typically of concern when the same respondents provide data for all variables, particularly when survey research is conducted (Podsakoff, MacKenzie, Lee, & Podsakoff, 2003).

Human resource management (HRM) research, as a primarily micro social science, is particularly susceptible to CMV-related concerns and thus deserves attention on this issue. For example, data regarding HRM practices and employee attitudes is regularly collected via survey and often from employees who are directly affected by HR programs and policies. Theses respondents are likely to be the best source of data, yet if the same set of respondents provide all data for analyses, this could contribute to CMV. As with other micro fields, as HRM research has become more sophisticated over time, concerns about CMV have become pervasive, influencing researchers' choice of measures and target journals, reviewers' comments and recommendations, and editorial guidelines and decisions (e.g., Chan, 2009; Chang, Van Witteloostuijn & Eden, 2010; Jordan & Troth, 2020; Rodriguez-Andura & Meseguer-Artola, 2020). Researchers who believe same-respondent data to be the best choice for their research questions must address concerns that CMV may artificially inflate relationships among substantive variables.

This paper assesses the evidence for potential dangers of CMV in HRM-related research using knowledge accumulated from the last 10 years as a compass. Over this time, findings from investigations with a methodology focus and from empirical studies provide information about how CMV can be estimated and how prevalent it may be. This evaluation of the current state of science on CMV thus allows a well-grounded conclusion that can inform future research. The current manuscript approaches this goal in the following ways. First, it provides an overview of CMV that distinguishes differences between CMV and common method bias (CMB). Second, as there has been a proliferation of new research regarding common method variance in the last decade, a summary of review articles and *post hoc* detection techniques is presented. Third, information on CMV from articles in the 1980s and 1990s is examined through the lens of the current state of knowledge on CMV. Fourth, to determine the treatment of CMV in the field of HRM, we review articles from the last 10 years in six major journals that feature HRM research. This assessment captures the use of procedural attempts to allay CMV concerns, the use of *post hoc* detection techniques, and the results of *post hoc* tests. Finally, conclusions and recommendations based on these findings are presented.

The steps outlined above make a number of important contributions. First, critically and comprehensively summarizing all major review and simulation studies conducted in the past 10 years it (a) allows re-interpretation of the early influential quantitative review studies on CMV to determine the accuracy of their claims, and (b) shows that this accumulated evidence suggests that the real danger of CMV to the validity of findings is minimal. Second, by reviewing evidence that appeared in the same period, it highlights the fact that despite efforts and resources in the development of sophisticated *post hoc* tests, these tests do not regularly indicate CMV or CMB in published data. Third, by reviewing all HRM-related empirical studies with perceptual data (a total of 1,710 studies) that appeared in six established journals over the past 10 years reveals that (a) though the empirical literature has become more aware of CMV, it has unevenly absorbed developments about its prevention and detection; and crucially (b) the results of *post hoc* tests in these studies show that CMB in published data is rare. Fourth, in light of these additions to knowledge, it provides recommendations for researchers about how to approach the choice of measures and the execution of *post hoc* testing for CMV.

CMV: What it is, its Causes and the Suspect Methodology

CMV is co-variation between measures of distinct constructs caused by identical ("common") measurement methods rather than the constructs themselves (e.g., Brannick, Chan, Conway, Lance & Spector, 2010). There is probability for error in every measure, and error is traditionally distinguished into non-systematic and systematic. Variance due to the method is a case of systematic error, and unlike non-systematic (random) error, it is not distributed evenly but unidirectionally instead, which may distort results (Carmines & Zeller, 1979). The concern with CMV is that when two or more different constructs are measured with the same method (e.g., questionnaires, same apparatus), the bias encouraged by that particular measurement method may be responsible for part of the observed covariance (e.g., Cote & Buckley, 1987; Doty & Glick, 1998), thus the term Common Method Variance. With CMV, what we observe as a relation (e.g., correlation, regression, or path coefficient) between two constructs may not fully reflect their true relationship, but may also reflect the error caused by the bias introduced by their common method of measurement.

A host of factors have been proposed as causes of systematic variance attached to the method of measurement (see Podsakoff et al., 2003 for an exhaustive list). Prominent such factors include social desirability, the consistency motif (individuals' desire to demonstrate consistency in their responses), implicit theories (individuals' tendency to believe that particular traits, behaviours and situations go together), demand characteristics (participants may try to "guess" what the researcher wants to find and adjust their responses accordingly), positive and negative affectivity (trait-like characteristics that dispose individuals to experience, and hence report, situations and events in a consistently positive or negative way), and response style (individuals' tendency to choose options towards high scores – acquiescent – low scores – disacquiescent – or the extremes). In addition, any characteristics of the measures or the measurement are seen as potential causes of CMV. These include the proportions and positions of positively and negatively (reversed) worded items, the similarity in response formats and anchors across measures, the placement of predictors and criteria variables within the survey, ambiguity in the wording of items, or even the occasion (time, location) where completion takes place (Brannick et al., 2010; Podsakoff et al., 2003).

Because of the potential to distort relationship estimates, research that relies on perceptual measures is judged by both reviewers and editors as possibly having results contaminated by CMV (Conway & Lance, 2010; Chang et al., 2010; Pace, 2010; Rodriguez-Andura & Meseguer-Artola, 2020; Williams & McGonagle, 2016). The survey is the measurement method seen as the primary culprit for systematic bias, particularly when two or more constructs are measured on one survey completed by the same individuals (Chan, 2009; Conway & Lance, 2010; Crampton & Wagner, 1994; Podsakoff & Organ, 1986; Malhotra, Schaller & Patil, 2017; Spector, 2006). Though this is commonly referred to as "self-reports," the term is inexact. "Other-reports", and in particular when two or more key variables are assessed by others (e.g., line managers, peers, customers), are equally likely to invite CMV (Conway & Lance, 2010). For this reason, here we adopt the term "same-respondent" (see also Chang et al., 2010), which is inclusive of all variants. Consequently, studies that rely on same-respondent methodologies tend to be explicitly or implicitly viewed as "inferior" and receive greater scrutiny (e.g., Chan, 2009; Legood, van der Werff, Lee & Den Hartog, 2020; Reio, 2010; Malhotra et al., 2017).

CMV is particularly pertinent to research in HRM and its related fields. Many constructs that are of interest in HRM are perceptual and/or contain high level of abstraction (e.g., perceptions of justice, organizational citizenship behaviours, leader-subordinate exchange, perceived organizational support, job satisfaction), which makes them vulnerable to factors that allegedly introduce bias in their measurement (e.g., Chan, 2009; Crampton & Wagner, 1994; Doty & Glick, 1998; Podsakoff & Organ, 1986). Thus, HRM researchers benefit from understanding recent advances in knowledge and practice regarding CMV.

Early Quantitative Reviews

Descriptions of CMV appeared as early as the late 1950s (Campbell & Fiske, 1959), but influential quantitative reviews from the late 1980s served to establish CMV as a central issue of concern (i.e., Cote & Buckley, 1987; Doty & Glick, 1988; Williams, Cote & Buckley, 1989). With analyses of multiple correlation matrices from existing empirical studies, authors of these reviews concluded that the variance believed to reflect CMV was sizeable and therefore a serious threat to validity. In particular, Cote and Buckley (1987) and Williams et al. (1989) concluded that on average, CMV accounted for 26.2% and 27%, respectively, of the co-variation between measures of constructs, while Doty and Glick (1988) reported a somewhat lower amount, 16%. In a later quantitative review Doty and Glick (1998) reported that an average of 32% of the observed co-variation was attributable to CMV. These percentages indeed appear high and the conclusion about the CVM threat seems justifiable.

Notably, other quantitative reviews that came out at the same time were much more moderate in their conclusions (Bagozzi & Yi, 1990; Spector, 1987). Furthermore, decades later, the debate over the danger CMV represents was not settled (e.g., see, for example, Richardson, Simmering & Sturman, 2009; Spector, 2006; Williams & McGonagle, 2016; Doty & Astakhova, 2020; Witteloostuijn, Eden & Chang, 2020). Nevertheless, the impact of these early quantitative reviews along with theoretical argumentation behind the dangers of same-respondent data (e.g., Campbell, 1982; Podsakoff & Organ, 1986) was strong enough to establish CMV as a major methodological concern in academic conscience (e.g., see Podsakoff et al., 2003; Podsakoff, MacKenzie & Podsakoff, 2012; Chang et al., 2010).[1] In tandem with the establishment of CMV as a major concern, there was a proliferation of *post hoc* techniques and strategies to detect and correct for its effects, as presented below.

Clarifying the Concepts: Common Method Variance vs. Common Method Bias

There is an important distinction between CMV and common method bias (CMB) that was often ignored in early quantitative reviews. Indeed, these terms are still used interchangeably in many more recent published works. CMV is systematic error variance that the common method of measurement may introduce, which may distort estimates of relationships. CMB means that the error variance in measurement is sufficiently large to lead to erroneous conclusions about the nature of the relationship (Doty & Glick, 1998; Podsakoff et al., 2012; Spector, 1987). In other words, CMV is a necessary, but not sufficient condition for CMB; for CMB to occur, CMV must be large enough so that the observed relationship deviates from the true relationship to such a degree that it elicits erroneous conclusions (e.g., to conclude that a systematic association between two constructs exists when in fact it does not) (Doty & Glick, 1998; Fuller, Simmering, Atinc, Atinc & Babin, 2016).

One confusing element of the CMV/CMB distinction is that CMB can be viewed under a dual prism: CMB-potential, which is the process by which the common method may introduce bias in the measurement to produce systematic error variance or CMV; and CMB-outcome, when that variance is large enough to meaningfully distort relationship estimates. However, we follow

[1] Notably, considerably less attention was devoted to the finding that different methods may also share systematic variance (Lance, Dawson, Birkelbach & Hoffman, 2010). This conclusion first emerged in Williams et al.'s (1989) study, in that the amount of covariance produced by multi-source data was at least equal to that attributed to same-respondent data (Williams et al., 1989, p. 446). However, the variance shared by different methods did not receive attention, maybe because there was no corresponding theoretical framework.

naming conventions adopted by many prior authors (e.g., Fuller et al., 2016; Doty & Glick, 1998; Spector, 1987) and therefore only refer to CMB when there is CMB-outcome.

Originally, CMV was described as strictly inflationary, influencing estimates only upwards and resulting in overestimation of the real relationship (Type I error) (Bagozzi & Yi, 1991; Cote & Buckley, 1987; Doty & Glick, 1998; Organ & Ryan, 1995). Because Type I error leads to the erroneous belief that there is a systematic relationship when in fact there is not, this gave rise to early warnings of the dangers of CMV (Campbell, 1982). However, evidence now indicates that CMV does not necessarily distort relationship estimates, and when it does, the effect can be either upwards or downwards (e.g., Crampton &Wagner, 1994; Chan, 2009; Lance et al., 2010). Indeed, a more detailed approach to this concept is described by Spector, Rosen, Richardson, Williams and Johnson (2019), who delineate the conceptual differences between inflating CMV and deflating CMV (termed Uncommon Method Variance, or UMV).

Recent research indicates that the mechanisms of how CMV manifests as CMB are multiple and complex (e.g., Chan, 2009; Doty & Astakhova, 2020), and thus, one cannot conclude that all CMV becomes biasing. Factors believed to cause CMV are likely to operate simultaneously and have differential effects across variables or respondents. For example, some respondents are high and some are low in negative affectivity or social desirability, and people hold different implicit theories, which invites the serious possibility that their effects cancel each other out (see also Chan, 2009; Spector, 2006; Spector et al., 2019), leaving relationship estimates intact. Furthermore, and critically important, recent evidence indicates how CMV may operate in conjunction with other types of error to influence relationship estimates. Notably, reliabilities of scales play a powerful role, as unreliability in measures is deflationary and, hence, often acts as a counterbalancing factor to suppress inflationary effects of CMV (e.g., Conway & Lance, 2010; Fuller et al., 2016; Lance et al., 2010). Moreover, error variance in the measurement of a single construct that is not shared by the rest of the substantive variables is likely to deflate relationship estimates (Spector et al., 2019). Finally, even if the conditions for CMV effects on estimates are favourable, whether it will manifest itself depends on the data-analytic strategy. Simulation studies indicate that coefficients for interactions or quadratic effects cannot be produced by CMB (Evans, 1985; Siemsen, Roth & Oliveira, 2010), nor regression coefficients or structural equation models that contain multiple predictors (Sturman, Ukhov, Richardson & Simmering, 2018). In other words, in complex statistical models, although CMV may be present, CMB is unlikely to produce statistically significant relationships when they do not truly exist.

Given that the threat to validity is not CMV but CMB, it is critical issue to establish if the point at which CMV turns into CMB can be identified (Doty & Glick, 1998). Arguably, a valid way to determine this is through simulation, because actual data pose challenges in partitioning true covariance from error, as error has various sources that researchers are normally neither aware of nor able to fully control (Harrison, McLaughlin & Coalter, 1996; Pace, 2010; Richardson et al., 2009). Furthermore, there is a risk in attaching the label of CMV to variance, when it could instead be comprised of both CMV and true covariance shared by substantive constructs (Conway in Brannick et al., 2010; Lance, 2008; Lance, Hoffman, Gentry, Baranik, 2008; Spector, 2006). Simulations allow the researcher to set true and observed scores along with the amount of CMV in the data, and hence have evidence about when CMV is present and when it causes CMB.

Fuller et al. (2016) conducted a simulation study that identified the point at which CMV became CMB under a variety of conditions. They found that for same-respondent measures with typical scale reliabilities, the probability that CMV produces inflationary CMB (that leads to Type I error) is remote. Just as Lance et al. (2010) found, Fuller et al. (2016) determined that imperfect scale reliabilities tended to deflate observed correlations and offset most inflationary CMV. To illustrate, for measures with what Fuller et al. (2016) labelled “typical” reliabilities (Cronbach α between .87 - .90), inflationary CMB appears only once the amount of total variance accounted for by CMV exceeds 60%. For lower reliabilities (Cronbach α between .77 - .80), the required amount of CMV is even greater because low reliabilities tend to attenuate effect size. Yet, the expectation that the amount of CMV in data would even approach 60% is highly unlikely. For example, the highest amount of CMV identified in quantitative reviews so far was .54 (Doty & Glick, 1998), although we caution against reliance on such estimates, due to the ambiguity involved in identifying CMV in that article.

Given that measurement reliability can create deflation that offsets inflationary CMB, it is meaningful to know the typical scale reliability in management research. Greco, O’Boyle, Cockburn and Yuan (2018) recently found, in their quantitative review that the range of mean Cronbach alphas for constructs commonly used in management research was .71 - .92, the majority of mean alphas were in the range of .80 - .90, and for no construct did the upper confidence interval of α value exceed .94 (Greco et al., 2018). Looking at these reliabilities in conjunction with Fuller et al.’s (2016) findings, and considering that simulations tend to be conservative (Harrison et al., 1996), it seems that inflationary CMB is less likely than previously believed (Fuller et al., 2016 p. 3195, labelled the probability of inflationary CMB as “nil”). Yet, CMV has traditionally been named as a culprit for the inflation of coefficients (e.g., Crampton & Wagner, 1994) – and this is what most researchers are still concerned about (e.g., Greene, Cowan & McAdams, 2020; Zeijen, Peeters & Hakanen, 2018).

Finally, the results of Fuller et al.’s (2016) simulation also suggest that the larger risk of CMV is deflationary CMB; that is, obtaining coefficients that understate the true relationship between the constructs (Type II error or false negative). Though Type II error is also problematic, in much HRM research, it not as undesirable as an over-estimation (inflation) of effect size and indeed may serve to provide a conservative estimate. For an under-estimation of effect size to occur, however, the following simultaneous combination of conditions is required (Fuller et al., p. 3195, Table 2): low CMV, low scale reliabilities (below .80), large confidence intervals, and a true relationship of considerable strength (correlation coefficient well above .30). This combination has arguably low probability to occur, as evidenced by a recent comprehensive review that indicated that the median correlation in published studies in HRM and related fields is .16 (Bosco, Aguinis, Singh, Field & Pierce, 2015).

***Post Hoc* Techniques**

As CMV has received more attention, considerable effort has been invested in the development of quantitative *post hoc* techniques that purport to detect CMV, or even correct for it. *Post hoc* techniques have become increasingly sophisticated, imposing elevated demands on researchers in terms of skill, time and resources. The most prominent such techniques are briefly presented below.

Harman's One-Factor Test. While Harman's one-factor (or single factor) test has long been criticized as ineffective, its popularity requires its inclusion in this review (see Fuller et al., 2016). The one-factor test can be conducted with exploratory factor analysis (EFA) to which all items in a same-respondent survey are subjected, and the unrotated solution is examined. If all items load on a single factor, or the first factor contains more than 50% of the variance extracted, the data is believed be biased by CMV. The test has also been conducted with confirmatory factor analysis (CFA), in which the fit of a one-factor model is compared to the fit of the measurement model. If the one-factor model fits best, this test purports to have identified CMV.

The unmeasured latent method construct (ULMC). The ULMC is based on the idea that if CMV is present, then an unmeasured latent factor representing CMV should be linked with the items (indicators) of the substantive constructs in the study (e.g., Organ & Greene, 1981). If the Structural Equation Model (SEM) that includes the ULMC (on which items load along with their loadings on their own substantive constructs) demonstrates improved data fit over the model with only the substantive constructs, this is believed to constitute evidence for CMV. If the estimates of relationships between the substantive constructs are significantly different in this ULMC model, this is seen as evidence for CMB. Because the ULMC makes use of latent factors, it allows for separating various presumed sources of error and also enables modelling CMV at the level of construct or scale item. Thus, it has been used under assumptions of both equal (noncongeneric) and non-equal (congeneric) effects of CMV (Podsakoff et al., 2003; 2012; Richardson et al., 2009).

Marker variable techniques. These techniques (developed by Lindell & Whitney, 2001, and Williams et al., 2003) are based on the idea that CMV should manifest in the relationships of substantive variables with other variables that bear no theoretical relevance to them. Consequently, researchers must identify *a priori* a construct that, on theoretical grounds, does not relate to one or more of the substantive constructs in the study. This is the marker variable, which is included in the survey. Ideally, markers should be chosen and measured *a priori* (Richardson et al., 2009). However, Lindell and Whitney (2001) suggest that the marker can also be chosen *post hoc* ("non-ideal" marker, Richardson et al., 2009). There are two main variants of the marker technique.

The correlational marker technique. The original conceptualization of the marker variable technique was developed by Lindell and Whitney (2001), who recommended identifying the smallest correlation between the marker variable and any substantive variable, and then using that value to estimate partial correlation coefficients. If these new "corrected" correlations become non-significant or significant at a lower level than observed correlations, then CMB or

CMV, respectively, is inferred to have been removed. The researcher can then use the corrected covariance estimates in the analysis. The technique assumes that CMB can only be inflationary (Lindell & Whitney, 2001) and that the effects of CMV are equal across substantive variables, which are not accurate assumptions.

The Confirmatory Factor Analysis (CFA) marker technique. The CFA variant (Williams et al., 2003; Williams, Hartman, & Cavazotte, 2010) purports to enable testing and controlling for CMV at the item level while it also allows for unequal effects of CMV on the substantive constructs and their indicators (Richardson et al., 2009; Schaller et al., 2015). However, unlike the correlational variant, the CFA marker technique requires that the marker is theoretically unrelated to all substantive constructs, which renders its selection more demanding in skills and resources (Simmering et al., 2015; Williams et al., 2010). The marker variable is included in a series of structural equations models which are then compared to determine the degree to which CMV is present. If the model that allows non-zero loadings from the marker construct to substantive indicators fits the data significantly better than the equivalent zero-loadings model, this suggests CMV. And if the model where substantive-substantive relationship estimates have been replaced with estimates from a model that assumes no marker-substantives relationships (baseline model) fits the data even better than the previous model, this is evidence for CMB (Williams et al., 2010).

*The choice of marker variables***.** The choice of marker variables is challenging, and theory and prescriptive advice have been built on their selection and utilization (see Simmering et al., 2015; Williams et al., 2010). Not only must the marker be theoretically unrelated, but also it must tap into those sources of bias that are likely to appear within the measurement context of the substantive constructs (Schaller et al., 2015; Richardson et al., 2009).

The measured CMV-cause approach. If presumed individual-level causes of CMV such as social desirability or response styles can be identified within the context of a particular study, they can be measured (Podsakoff et al., 2003). Then, using either conventional least squares or latent factor methods, a researcher can detect and correct for CMV/CMB. To illustrate, Weijters, Geuens and Schillewaert (2010) recommend including a number of “buffer” items in a survey, evaluating these for response styles (e.g., acquiescent responding), then correcting for their CMV/CVB effects using latent factor techniques.

What *post hoc* techniques suggest about CMV and CMB

To date, there are few studies that provide informed conclusions on the value of advanced *post hoc* techniques. Richardson et al. (2009) conducted a comprehensive simulation to evaluate the ULMC technique and the two marker techniques. Overall, their findings suggested that there is still rather a long way to go for *post hoc* tests to become trustworthy (Richardson et al., p. 796). To illustrate, there was a nearly 50% probability for the techniques to indicate CMB even in the absence of CMV. Furthermore, their accuracy in detecting presence of CMV was 41%, 69%, and 73% for the ULMC, the correlational marker, and the CFA marker technique, respectively. Nevertheless, a definite pattern emerged regarding the efficacy of the methods: the two marker techniques were superior to the ULMC in nearly all criteria. In particular, the two marker techniques were considerably more accurate at detecting CMV and also CMB when CMV was present. The marker techniques, and especially the CFA variant, performed best under the condition of using an ideal instead of a non-ideal marker. In fact, the CFA variant with an ideal marker had 84% accuracy across conditions in identifying the presence or absence of CMV, which appears reasonably precise (that of the correlational marker was 72%). The CFA variant had another fundamental advantage over the correlational (and also the ULMC): with an ideal marker the probability of falsely indicating CMV was very low or, in the words of

Richardson et al. (2009), it was “highly unlikely to detect CMV when it truly was not present” (p. 793). The conclusion of Richardson et al. (2009) was that the only *post hoc* test in which we can have reasonable confidence about the presence of CMV is the CFA marker technique with an ideal marker. Though in many criteria the performance of the correlational marker technique matched that of the CFA variant, Richardson et al. (2009) considered its high probability to indicate CMV when CMV was not actually present as unacceptable for recommending it.

Regarding accuracy of correction, on the other hand, Richardson et al. (2009) concluded that no technique produced accurate estimates of true relationships any better than no correction (“doing nothing”). Most significantly, the situation with erroneous corrections was especially pronounced when the true relationship was different from zero (Richardson et al., 2009), which likely represents the majority of real cases, given that hypotheses are normally constructed using theory and carefully contemplated reasoning. In light of these findings, Richardson et al. (2009) advised against using any of these *post hoc* techniques for correction.

Chin, Thatcher and Wright (2012) conducted a simulation to determine the efficacy of the ULMC in partial least squares (PLS) analyses. Under a wide variety of conditions, these authors found that the ULMC was even less accurate than what Richardson et al. (2009) had concluded. Their conclusion was that “the ULMC procedure does not accurately detect, or control, for CMB.” (p. 1017). This complemented the evidence for the lack of efficacy of the ULMC technique.

Simmering et al. (2015) conducted an exhaustive review of empirical studies that employed the marker technique for detection. Their conclusion was that CMB presence was a very rare phenomenon and made it “tempting to deduce that CMV simply is not a problem in most data and across disciplines” (p. 485). These authors, however, also wanted to eliminate the possibility that extant studies had made frequent use of non-ideal markers (such as demographic characteristics) that would have compromised the detection capacity of the technique (as per Richardson et al., 2009). Thus, they conducted two independent studies with actual data where, applying clearly defined procedures, they tested the CFA marker and the measured CMV-cause technique using six ideal markers and six CMV-cause variables, respectively. Their results showed that none of the markers nor any of the CMV-cause variables by themselves indicated CMB in real data that was highly susceptible to it (e.g., same-respondent, perceptual scales measured in the same way).

Schaller et al. (2015) conducted a quantitative review of all empirical studies published between 1990 – 2008 using the Theory of Planned Behaviour (TPB) (Ajzen, 1991), a highly influential social psychological theory with extensive applications in organization studies, including HRM (e.g., Ramsey, Punnett & Greenidge, 2008). TPB’s constructs of attitudes, personal beliefs, and perceptions are assumed amongst the most susceptible to CMV influence (e.g., Crampton & Wagner, 1994). Schaller et al. (2015) filtered the studies to include only those (174 in total) that used strictly same-respondent methodologies and had employed no CMV preventive techniques (for example, only cross-sectional studies with all data collected within a single administration). Part of the authors’ intent was to determine whether the correlational marker technique would produce changes to the significance of relationship estimates. Because ideal marker variables were not available, the authors used the smallest observed substantive-substantive correlation in each study. This represents an over-estimation of CMV because such a marker also contains true co-variance between substantive constructs. Application of the correction procedure using that highly “aggressive” (Schaller et al., 2015) marker caused no significant alterations to approximately 95% of the original correlations (and the 5% of correlations that changed to non-significant is within the limits of chance). The correlational marker technique assumes that CMV can only be inflationary and, hence, correction forces estimates downwards. Given that CMV can also be neutral or deflationary, it is likely that even the very small percentage of coefficients that became non-significant were in fact originally true coefficients. Schaller et al. (2015) also tested the effect of the marker correction on path estimates for those studies that included either path analysis or SEM, due to the popularity of these statistical techniques. Approximately 97% of the significant path coefficients remained significant, with the remaining 3% well within the limits of chance. Notably, with such an “aggressive” marker, this procedure in most cases probably deflated the true relationship examined, which could be responsible for those coefficients — which anyway only represented a very small proportion — that turned non-significant.

Harman’s one-factor test (Podsakoff & Organ, 1986) was the earliest test for CMV available and is diagnostic only. It has been criticized for conceptual reasons, such as its alleged low sensitivity, which increases the risk of Type I error (Podsakoff et al., 2003). Recent guides either omit it (Podsakoff et al., 2012) or recommend against relying on it alone (Chang et al., 2010). Nevertheless, the test is still widely utilized, likely because it does not require *a priori* inclusion of any survey variables or because of its simplicity to execute. However, Fuller et al. (2016) concluded that Harman’s one-factor test routinely produced Type II error (i.e., a false alarm for CMB). Thus, in contrast to conceptual criticisms, the test may be overly sensitive to CMB, which means it is over-protecting us. If true, it follows that we can probably trust all negative verdicts given in the thousands of studies that have utilized the Harman test so far and conclude that CMB is unlikely. Interestingly, Fuller et al.’s findings also indicated that for reliabilities in the range of .70 to .90 (the range that covers nearly all actual studies, Greco et al., 2018) the test stood a reasonable chance of correctly detecting CMB when present.

The safest verdict to draw from this body of research is that most *post hoc* methods available do not reliably help with CMV, with the exception of the CFA marker technique with ideal markers. However, while that test performs the best, it is still imperfect. Although several of the articles above used *post hoc* tests that are imperfect, their quantitative and qualitative reviews show that *post hoc* tests consistently yield an absence of CMB. Notably, more sophisticated *post hoc* techniques do not seem to fare much better than Harman's one-factor test. Therefore, it seems that complicated and laborious *post hoc* methods have not appreciably improved our chances of detecting CMB. If judged on utility (i.e., benefit divided by resource expenditure), they may have deteriorated our position. There are two potential explanations: either (a) existing *post hoc* methods are imperfect and further work in this direction is needed; or (b) *post hoc* methods in fact work well and they simply do not detect something that does not exist in the first place. If we consider the findings on the evaluation of *post hoc* methods along with the verdicts of review and simulation studies that looked specifically at the probability of CMB, the latter appears a plausible option.

Revisiting Early Reviews

Early influential review articles regarding the nature and likelihood of CMV and CMB can then be revisited through the lens of knowledge that has accumulated in recent years. Notwithstanding their rigor for the time they were conducted, judged retrospectively these studies had notable limitations. First, they operated under the assumption that all differences between relationship estimates from same-respondent (monomethod) and multi-source (heteromethod) measurement represent common method variance. In other words, they assumed that multi-source correlation always shows the true relationship while same-respondent always shows the wrong estimate because of CMV (Lance, Baranik, Lau & Scharlau, 2009), a claim that is highly disputable (Chan, 2009; Lance, et al., 2009, 2010; Schaller, et al., 2015). Indeed, a true relationship is likely unknown and very much depends on the perspective one wishes to take (e.g., see Chan, 2009; Lance et al., 2008). To illustrate, according to the principle "if it ain't trait it must be method" (Lance et al., 2009), measuring HR practices by asking HR managers and job satisfaction by asking employees themselves will give the true relationship, hence, it is more valid than measuring both by asking employees. However, there is evidence that what matters with HR systems is not only what systems are formally in place (and can be reported by HR managers or found in formal documents of HR policies) but in fact whether employees are aware of and how they experience these systems (e.g., Kehoe & Wright, 2013; Van De Voorde & Beijer, 2015).

Second, quantitative reviews of the late 1980s (Cote & Buckley, 1987; Doty & Glick, 1988; Williams et al., 1989) assumed that CMV, regardless of the amount, always causes meaningful distortion to relationship estimates (i.e., they did not differentiate between CMV and CMB). Considered in light of Fuller et al.'s (2016) simulation and other later work, the amounts of CMV identified in these early summative studies are far below the amount required to create concerns over CMB. Furthermore, the amount of CMV assumed could be over-estimates based on the knowledge we have accumulated since (for example, Lance et al.'s, 2010, quantitative review that took into account more recent knowledge when estimating the amount of CMV yielded a CMV estimate of just 18%). Both Bagozzi and Yi (1990) and Spector (1987) performed testing on whether the distortion CMV caused was meaningful (i.e., in essence, distinguishing CMV from CMB) and reached different conclusions. In fact, Williams et al. (1989), Spector (1987), and Bagozzi and Yi (1990) utilized exactly the same datasets from existing studies, yet Williams et al. concluded that CMV was clearly a danger to validity, Spector concluded the opposite, while Bagozzi and Yi's verdict was mid-way between the two positions. In retrospect, the different conclusions are attributable to different assumptions about the effects of CMV that led to different data analytic strategies. All of them apportioned CMV according to the principle "if it ain't trait it must be method". However, Williams et al. (1989) assumed that if the addition of a CMV factor improves the data fitness of the measurement model, this automatically constitutes evidence that CMV meaningfully distorts estimates. Bagozzi and Yi (1990) went further, testing whether the loadings of the CMV factor on measure items were significant. What they did not test, however, was whether the presumed "true" (i.e., heteromethod) relationship estimates were significantly different from the "CMV infected" (monomethod) relationship estimates. Spector (1987) did exactly that to conclude "the problem may in fact be mythical" (p. 442). We note, however, that although Doty and Glick's (1998) work is sometimes perceived to be amongst those that established CMV as a concern, it was in fact the first to explicitly distinguish the concepts of CMV and CMB in their analysis, on the basis of which they concluded that "common methods bias of 20% to 40% ... is probably not sufficiently large enough to invalidate many of our theoretical interpretations and research conclusions" (p. 400).

From CMV to CMB: Recent Reviews

The past decade has seen reviews, quantitative and qualitative, that assessed anew the evidence for CMB in more recent studies, using accumulated knowledge and more advanced analytic techniques. Lance et al. (2010) conducted a meta-analysis where they took into account the effect of measures' reliabilities on relationship estimates. Their findings suggested against the presence of CMB — a finding very similar to what was later produced in Fuller et al.'s (2016) simulation. In fact, Lance et al. (2010) found that correlation estimates obtained with same-respondent data were slightly lower than the presumed true correlations (i.e., those obtained with multi-source data) and did not significantly differ from the latter.

In line with the above conclusions are also the findings of Simmering et al. (2015), who conducted a review of all doctoral dissertations available in the ProQuest Digital Dissertations database that had implemented the marker variable technique. Their results concurred with those of Lance et al. (2010). In particular, 21 out of the 22 dissertations that had performed the CFA marker variable technique had concluded that either CMV was not present or when present, it did not bias the results (no CMB). The same conclusion was drawn in 35 out of the 36 dissertations that had employed the marker technique in any variant. Though these represent small proportions of dissertations (those that implemented the marker technique, which restricted sample sizes) the findings are useful because no information on CMV and CMB detection in non-published work, such as doctoral dissertations, had become available earlier. In conclusion, therefore, recent simulations and reviews suggest that the threat to validity imposed by CMV is minimal, and practically limited to deflationary CMB (Type II error).

Review of HRM Articles

This paper summarizes current knowledge on the nature and likelihood of CMV and CMB, particularly in light of new research. Yet, there is no information as to the degree to which these advances have been adopted in published HRM research. Knowing whether authors make use of new understandings in their research practices is useful for improving the quality of social science. Indeed, researchers are guided by other published work in their research design choices, often defaulting to norms and mimicry rather than adopting best practices (Atinc, Simmering, & Kroll, 2012). In addition, summarizing the results of *post hoc* tests for CMV and CMB in published empirical studies, and the extent to which these results concur with the conclusions of recent quantitative reviews and large-scale simulations, can help us develop a comprehensive picture of the presence and effects of CMV in HRM-related empirical research. Thus, in this section, HRM articles published in the last 10 years in six journals are evaluated by their approach to CMV and use of *post hoc* tests.

Method

Articles were drawn from six prominent peer-reviewed journals—*Human Resource Management Journal (HRMJ), Human Resource Management (HRM), International Journal of Human Resource Management (IJHRM), Personnel Psychology (PP)*, *Academy of Management Journal (AMJ),* and *Journal of Management (JOM)*. The first four have a specific focus on HRM, while *AMJ* and *JOM* are highly impactful management journals that regularly publish HRM-related research. All journals enjoy strong ratings in journal rankings (e.g., Chartered Association of Business Schools, 2018; five currently on the FT-list and one, *IJHRM*, in the past); hence, their articles should provide an accurate idea of CMV trends and findings of *post hoc* tests in high quality HRM-related literature.

Articles spanning 10 years (2010 – 2019) collected from the journals listed above were coded. That included all articles in *HRMJ*, *HRM*, *IJHRM*, and *PP* and those articles in *AMJ* and *JOM* that studied HRM topics. The authors reviewed a random sample of 40 articles from *AMJ* and *JOM* and independently determined whether HRM topics were covered or not. This resulted in smaller numbers of articles from these journals. The initial level of agreement on this assessment was 84%. Discrepancies were resolved via discussion, and following this, the second author evaluated all *AMJ* and *JOM* articles as to whether they addressed HRM.

Once articles were collected, they were reviewed and coded as described below. Based on the literature review presented above, the two authors established coding categories through discussion. Thirty randomly drawn articles were then coded by both authors. The percentage agreement on this initial coding was 96.25%. Coding discrepancies were discussed, and codes were refined. The authors then split the articles to code independently. For articles with multiple studies, each study was coded as a separate row.

The following coding categories and codes were used for each article. *Empirical* articles (coded 1) were distinguished from those that were not empirical (coded 0). Articles that made use of only *primary data* were assigned a code of 1; those with secondary (archival data) were assigned a code of 0; and a code of 2 was assigned if an article used both primary and secondary data. Articles with *quantitative data* were assigned a code of 1 for this category, with a code of 0 for qualitative data. Because data that is neither perceptual nor survey is unlikely to be affected by CMV, we coded those that did not have any as 0, and coded those with perceptual/survey data as 1 in this column.

CMV explicitly considered was coded 1 if the article had specific text related to CMV or CMB and coded 0 if this was absent. To determine whether CMV was specifically addressed or considered in the article, we searched for the terms “common method”, “common source”, “same source”, “single source”, “percept percept” along with their combinations with “variance” and bias”, such as “common method variance,” “common source bias”, etc. Additionally, we searched for references to authors commonly cited regarding CMV (e.g., “Podsakoff”, “Spector”). Moreover, all articles were skimmed through to cover for the possibility that unconventional terminology was utilized for CMV or CMB.

Regardless of whether an article explicitly considered CMV or not, we coded three procedural approaches believed to reduce or avoid CMV (Podsakoff et al., 2003). Although not reviewed in the current manuscript in regard to their ability to prevent CMV, these *a priori* approaches are typically seen as a means to allay concerns about the presence of CMV (Podsakoff et al., 2003). The *use of multi-source data* (1 if present; 0 if not) included matched surveys, the use of archival sources (e.g., company records) and surveys together, aggregation of group data, and the use of objective organizational data (e.g., sales data) along with survey data. The use of a *time lag between surveys or longitudinal data collection* was coded 1 when present (including articles that used within-person designs over time) and 0 if surveys were given at only one time. The use of an *experimental manipulation* was coded 1 when present and 0 if absent. An experimental manipulation existed if a treatment or stimulus was used in the study.

The *use of a post hoc CMV test* was coded as 1 if one was conducted (0 if not), and a code was then assigned to each of the following tests: *Harman's One-Factor Test using EFA*, *Harman's One-Factor Test using CFA*, the *Correlational Marker Technique,* the *CFA Marker Technique*, the *ULMC technique, controlling for a presumed cause of CMV, examining correlations to detect CMV* (i.e., noting that some correlations are low or nonsignificant, thus indicating a lack of inflationary bias), and *other test* (details captured from the article). For each *post hoc* test used in an article, we further coded the *result* (0 = no CMV found, 1 = CMV found; 0 = no CMB found, 1: CMB found) and whether a *post hoc correction* was made (1 = yes, 0 = no) before conducting analyses.

Results

For the results of our analysis, we considered only those articles that were empirical, had quantitative data, and included survey or perceptual data. Thus, out of a total of 2,959 articles reviewed, 1,710 were analysed further.

Table 1

The degree to which CMV was explicitly considered over the years is illustrated in Table 1. Over time, there was an upwards trend of explicitly considering CMV, with fluctuations between the years and an anomalous dip in 2015. The steepness in increase is quite sizeable, the overall proportion rising from 52% in 2010 to 77.85% in 2019. Two other key trends in this data are the use of *post hoc* tests and CMV-preventive research designs, both of which showed a general increase. The proportion of studies that employed *post hoc* tests nearly doubled, from 20% in 2010 to 37.58% in 2019; and similarly, though less steep, *post hoc* testing for those studies that explicitly mentioned CMV increased from 38.46% in 2010 to 48.28% in 2019. As a proportion of studies that explicit considered CMV, those that implemented at least one preventive method rose from 38.46% at the beginning of the decade to 53.45% in 2019, having reached a peak of over 65% in 2017. In 2019, nearly 9 out of 10 studies (87.06%) that explicitly discussed CMV utilized either *post hoc* testing or CMV-preventive design or both. The overall impression from this part of the analysis is that awareness of CMV, including adoption of *post hoc* testing, has increased over the past 10 years, though at the beginning of the decade the literature cannot be considered unaware by any means.

Table 2

Table 2 presents yearly data on *post hoc* tests and the results of the testing. The use of Harman's one-factor test has been dominant, either in its original EFA form or the CFA variant, encompassing nearly two out of three (63.86%) *post hoc* tests conducted in the past decade in this sample. Furthermore, its prominent use declined only minimally over time, with a gradual shift in favour of using a CFA over an EFA (not surprising, given that latent variable analysis has become more accessible and common). The ULMC technique was the second most popular, representing about one in six (16.50%) *post hoc* tests in the 10-year period. However, after reaching its peak in 2017, when it accounted for one in four tests, the ULMC technique shows decline. While it may be premature to judge, it is possible that this decline reflects absorption of simulation findings on the ULMC's untrustworthiness (e.g., Richardson et al., 2009; Chin et al., 2012). There seems to be an overall increase in the frequency of use of the marker technique.

However, a trend is hard to discern, and use of the technique still accounts for less than one in 10 *post hoc* detection tests. Furthermore, there is no clearly discernible trend over time in favour of the CFA variant (the correlational variant has dominated usage in all but two years). Nevertheless, ideal markers are much more frequently utilized than non-ideal markers (24 vs. seven, respectively, one study utilized both, and eight manuscripts did not specify or did not provide sufficient information to infer the type of marker), which is in line with recommendations. Finally, there is an appreciable increase in the utilization of multiple *post hoc* tests over time (averaging one in four studies in the past two years), which may be due to editorial or expert suggestions. Overall, there appears to be unevenness in the patterns of deployment of *post hoc* tests, and this could be due to emerging information about them over the course of the decade.

A more important element, however, is the outcomes of *post hoc* testing. As seen in Table 2, CMB was concluded in two of the 606 *post hoc* testing occasions, which represents a 0.33% probability. The probability of detecting the presence of CMV only – CMV but not CMB – was higher and represented 7.10% (43 out of 606) *post hoc* testing occasions. However, these findings must be interpreted with caution, as the test that detected CMV was almost exclusively (93.33% of the time) the ULMC technique. As this test was empirically found to be the least efficacious in rigorous quantitative assessment (Richardson et al., 2009), it is possible that some of these conclusions are false positives. The CFA marker technique, which seems to most accurate of the techniques available, suggested CMV (but not CMB) in one of the 10 cases it was deployed, that was with an ideal marker. Finally, there was no difference in the probability of detecting CMV presence via *post hoc* tests between those studies that utilized *a priori* procedural approaches to minimize CMV (151 in number, 14 detections of CMV) and those that did not (315 in number, 31 detections of CMV) [$\chi^2(1) = .04$, *ns*]. Of the two studies that concluded

CMB, one utilized longitudinal data (Bardoel & Drago, 2016). Hence, according to the findings, CMV detection (or CMB) is no more likely in studies with no procedural precautions against it.

The probability of concluding CMB was found very close to zero (0.33% or one in 300 *post hoc* tests). However, we find it worthwhile to visit in some detail the two studies (Bardoel & Drago, 2016; Wang, Yi, Lawler & Zhang, 2011) that concluded CMB. Bardoel and Drago (2016) utilized the measured CMV-cause technique, using the Big Five of Personality as presumed cause. Theory and subsequent guidelines on the measured CMV-cause suggest that presumed causes are specific narrow concepts, such as social desirability (e.g., Podsakoff et al., 2003). Personality, which the Big Five comprehensively covers, on the other hand, is a wide construct that reaches into most aspects of human thinking, affect and conduct, and explains considerable variance in the majority of attitudes and behaviours (e.g., Ozer & Benet-Martinez, 2006). Hence, it would not be surprising if personality related to a wide variety of substantive variables. Even so, the correction for the effect of the Big Five did not alter the significance level of the coefficient between the two substantive variables in Bardoel and Drago's (2016) study (it remained at .001 level). Nevertheless, because the corrected coefficient was lower than the uncorrected one at the third decimal (.072 compared to .073) the authors concluded that "that difference is consistent with expectations around common methods bias, so results presented below are restricted to the corrected measure" (p. 2613). It is reasonable to argue that if a specific CMV-cause, as proposed in the literature, had been employed – or if two instead of three decimals were utilized in line with guidelines (e.g., American Psychological Association, 2010) – there would have been no conclusion that CMB was present in the data.

Wang et al. (2011) deployed the ULMC technique, and the model with the added ULMC factor displayed significantly lower χ^2 fit statistic and χ^2/df, and its GFI and CFI values were .014 and .016 greater than the non-ULMC model. On the other hand, the non-ULMC model had a better – lower – RMSEA value (both models had acceptable fit statistics). On that basis, Wang et al. (2011) decided to use the ULMC-model for their analysis, which implies they considered CMB presence. However, there are some caveats behind that treatment: (a) the χ^2 and the χ^2/df heavily depend on sample size (which in Wang et al.'s study was arguably large, N = 633), hence, it is rarely non-significant and does not constitute reliable evidence of model fit or difference in fit (e.g., Schermelleh-Engel & Moosbrugger, 2003). The same applies to GFI about which most experts have long advised against reliance on it (Sharma, Mukherjee, Kumar, & Dillon, 2005); (b) more important, the differences in the GFI and the TLI indices were well below the minimum recommended cut-off point of .05 to suspect CMB on the basis of the ULMC method (Bagozzi & Yi, 1990); (c) the RMSEA, which is extensively utilized and viewed as a reliable fit index, was in favour of the non-ULMC model; (d) the variance accounted for by the ULMC factor was not inspected (for CMV to lead to CMB that amount must be extremely high and other conditions must also be met according to Fuller et al., 2016); (e) there was no examination for differences in factor loadings or in path coefficients between the model without and with the ULMC factor, which would provide decisive information about whether CMV had significant effect on estimates. On the basis of the above, it is not unreasonable to argue that this was a case of CMV-only presence at most.

The findings of this review of a decade of publications warrant some conclusions. First, the past 10 years have shown substantial growth in awareness about CMV in the HRM-related empirical literature. This is reflected in an appreciable increase in the utilization of both preventive methods and *post hoc* detection methods. Second, this increase in discussion of CMV has not been accompanied by a commensurate growth in awareness of evidence on the validity of *post hoc* techniques that appeared during the decade. Although the primary article that discussed concerns about Harman's one-factor test was not published until 2016 (i.e., Fuller et al, 2016), many prior reviews criticized this test and warned against its use (e.g., Podsakoff et al., 2003). However, there was no real decline in its utilization over the 10-year period in our review. The ULMC, which was identified in simulation as highly error-prone in 2009 (in Richardson et al.) has increased in use in the last decade. *Post hoc* techniques that are most promising, like the CFA marker technique, remain only marginally used. The only aspect by which HRM-related empirical work seems to be moving in line with recommendations from the literature is the deployment of multiple *post hoc* tests in a single study. Third, *post hoc* testing suggests CMV only sporadically, and hardly ever indicates CMB. This means that even on the limited occasions it was present, CMV did not bias the results. Fourth, use of *a priori* procedures does not reduce the already small probability that *post hoc* tests will indicate the presence of CMV.

Discussion

This work aimed to review evidence on CMV using knowledge that has accumulated over the past decade. Over this time, there have been noticeable additions to our understanding about the nature and effects of CMV and about the capacity of available *post hoc* methods to detect and correct for effects of CMV. Overall, contemporary evidence coming from both reviews and large-scale simulations regarding *post hoc* CMV detection techniques suggests that the danger to validity that CMV imposes in same-respondent research is low. This is particularly true for Type I error—which is most feared—yet, it appears safe to conclude that the danger CMV imposes is not high. These conclusions were corroborated by our review of more than 1,700 HRM-related studies published in six major journals over the past 10 years, which suggested a virtually nil probability of CMB detection.

These findings, however, invite three counterarguments, which should be seen as limitations of our study.

First, the low level of CMV detected in published research may not be because it does not exist but because research that indicates CMV or CMB is not likely to be submitted or published (leading to a “file drawer problem”). In light of journal editorials that discourage authors from submitting research in which same-respondent surveys are used without some mitigation of design or testing for CMV (e.g., Ashkanasy, 2008), there may be a number of manuscripts that were not submitted, were desk rejected, or rejected after review due to problems with detected or anticipated CMV/CMB. And, this could be more pronounced in journals of strong reputation, such as those we included in our review. Simmering et al. (2015) reviewed a small set of unpublished doctoral dissertations and concluded that the rate of CMV and CMB identified in them was very similar to that of published studies. Yet, the limitation remains because (a) Simmering et al. (2015) reviewed a very small sample and was not explicitly aimed at understanding the issue, and (b), and most important, doctoral work that reports presence of CMV and especially CMB in the data may not reach the point of submission or successful defence.

Second, because *post hoc* tests—albeit ones that lack efficacy (e.g., Harman’s one-factor test and the ULMC)—can be conducted with no additional survey items authors are likely to conduct these tests in the final stages of data analysis before submission or in the revision process. And, because the ULMC provides a means for controlling for CMV (again, in a way that is highly prone to both Type I and Type II errors), one would expect more articles to use this approach.

Third, *post hoc* detection tests may have yet to reach the level of fidelity that enables detection of CMV and CMB when present; and this may be the reason for the overwhelmingly negative diagnoses. Therefore, and notwithstanding that considerable effort has already been expended in the development of *post hoc* techniques, more work and resources towards additional assessment of existing *post hoc* methods or towards the development of new more efficacious such methods may be in order.

Because our study cannot address the above issues, there cannot be absolute confidence over its conclusions about the presence of CMV and CMB in actual data. More work is needed to determine the degree to which the file drawer problem applies, along with further examining the validity of existing *post hoc* techniques or developing more accurate and reliable such techniques.

Recommendations.

Based on the review and systematic codification of 10 years' worth of research, what advice can we give researchers, reviewers, and editors in HRM regarding CMV? First, researchers should familiarize themselves with the latest knowledge on CMV, including the most efficacious *post hoc* tests. Importantly, those tests that have empirically been shown to lack accuracy have also been criticized for their conceptual limitations, and scholars should be aware of these as well. Relatedly, researchers should choose measures exclusively on what is best suited to tap a particular construct within the context of a study. Because CMV may be less prevalent than previously believed, trading off measures with lower validity to appease CMV concerns is ill-advised.

Researchers may also benefit from conducting a first screening for the possibility of method effects in their study, particularly by inspecting the correlation matrix and comparing the inter-correlations between key variables reported in the literature, ideally, from meta-analytic findings. If the sizes are noticeably different this may be an indication of common method effects, and a reason for inspecting the matter further. We should note, however, that (a) serious discrepancies do not necessarily mean common method effects and may reflect effects of the setting or of the sample and; (b) it is not only same-respondent surveys that generate variance but also multi-respondent (distinct-source) ones. Hence, this advice is also applicable to cases of multi-respondent study designs.

When *post hoc* tests are utilized, we urge researchers to use those tests that have fared well in empirical comparisons—the marker variable (and particularly the CFA variant with an ideal marker) and the presumed-cause techniques. These also allow substantial discretion in the choice of variables. Clear guidelines exist about the conditions to be met for the marker, and there is abundant literature about what the presumed causes can be. Haphazard use of these (e.g., by including a poorly chosen marker or a too broad presumed-cause that is not indicated by theory) may render misleading conclusions that may force the researcher to impose unnecessary correction that can bias estimates too far downward.

In cases where researchers conclude that CMV or CMB is present, they should be very cautious in their use of *post hoc* corrections. As seen in review and simulation studies, many *post hoc* methods for correction are inconsistent and thus untrustworthy, as they may bias estimates. Indeed, any researcher who implements a *post hoc* correction should present both uncorrected and adjusted estimates.

We urge reviewers and editors to also stay abreast of new findings regarding CMV, as we suspect the heavy use of techniques such as Harman's one-factor test could be driven by reviewer requests. Yet, as many of these tests are ineffective, and as there is mounting evidence that CMV concerns are overblown, we discourage reviewers and editors from assuming that same-respondent research necessarily suffers from CMB. Just as authors should compare their data to benchmarks in the literature, so should reviewers. Also, reviewers should recognize that lower scale reliabilities are likely to offset inflation due to CMV (Fuller et al., 2016; Lance et al., 2010) and that sophisticated models reduce the chances of CMB (Siemsen et al., 2010). Finally, if a *post hoc* test is warranted, reviewers should not recommend Harman's one-factor test or the

ULMC, as results are suspect, but should consider other more efficacious techniques. In particular, we echo Richardson et al.'s (2009) and Simmering et al.'s (2015) recommendations that the CFA Marker Technique with an ideal marker and the presumed cause technique can be useful for identifying CMV. Finally, as noted in Vandenberg (2006), misunderstanding and misuse of methodological approaches is often perpetuated by doctoral education. For those who teach or advise doctoral students, keeping up to date on new evidence, such as is presented here, can help advance social science.

In conclusion, much knowledge has been gained regarding CMV and CMB in the past 10 years, whose conclusions about the dangers of CMV are essentially mirrored in empirical research that has appeared at the same time. Authors are encouraged to use this evidence to make informed decisions about their research.

References

Ajzen, I. (1991). The theory of planned behavior. *Organizational Behavior and Human Decision Processes, 50*, 179-211.

American Psychological Association (2010). *Publication manual of the American Psychological Association* (6th ed.). Washington, DC: American Psychological Association

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

Atinc, G. M., Simmering, M. J., & Kroll, M. J. (2012). Control variable use and reporting in macro and micro management research. *Organizational Research Methods, 15*, 57-74.

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

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

Bardoel, E. A., & Drago, R. (2016). Does the quality of information technology support affect work–life balance? A study of Australian physicians. *International Journal of Human Resource Management, 27*, 2604–2620.

Bosco, F. A., Aguinis, H., Singh, K., Field, J. G., & Pierce, C. A. (2015). Correlational effect size benchmarks. *Journal of Applied Psychology, 100*, 431-449.

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

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

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

Carmines, E. G., & Zeller, R. A. (1979). *Reliability and validity assessment*. Beverly Hills, CA: Sage.

Chan, D. (2009). So why ask me? Are self-report data really that bad? In C. E. Lance & R. J. Vandenberg (Eds.), *Statistical and methodological myths and urban legends: Doctrine, verity and fable in the organizational and social sciences* (pp. 311–338). New York: Routledge.

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

Chartered Association of Business Schools (2018). *Academic journal guide 2018*. London: Chartered Association of Business Schools.

Chin, W. W., Thatcher, J. B., & Wright, R. T. (2012). Assessing common method bias: Problems with the ULMC technique. *MIS quarterly*, 36, 1003-1019.

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

Cote, J. A., & Buckley R. (1987). Estimating trait, method, and error variance: generalizing across 70 construct validation studies. *Journal of Marketing Research*, *24*, 315–18.

Cote, J. A., & Buckley, R. (1988). Measurement error and theory testing in consumer research: An illustration of the importance of construct validation. *Journal of Consumer Research, 14*, 579–82.

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

Doty, D. H., & Astakhova, M. (2020). Common method variance in international business research: A commentary. In L. Eden, Nielsen, B. B., & Verbeke, A. (Eds.), *Research methods in international business* (pp. 399-408). Cham, Switzerland: Palgrave MacMillan.

Doty, D. H., & Glick, W. H. (1988, April). Method variance in I/O research: Major effect or mythical beast? Paper presented at the Annual Meeting of the Society of Industrial and Organizational Psychology, Dallas, Texas, USA.

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

Evans, M.G. (1985). A Monte Carlo study of the effects of correlated method variance in moderated multiple regression analysis. *Organizational Behaviour and Human Decision Processes, 36*, 305-323.

Fuller, C. M., Simmering, M. J., Atinc, G., Atinc, Y., & Babin, B. J. (2016). Common methods variance detection in business research. *Journal of Business Research, 69*, 3192-3198.

Greco, L. M., O'Boyle, E. H., Cockburn, B. S., & Yuan, Z. (2018). Meta-Analysis of Coefficient Alpha: A Reliability Generalization Study. *Journal of Management Studies*, *55*(4), 583-618.

Greene, R. E., Cowan, H. R., & McAdams, D. P. (2020). Personality and coping in life challenge narratives. *Journal of Research in Personality, 86*, 103960.

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

Jordan, P. J., & Troth, A. C. (2020). Common method bias in applied settings: The dilemma of researching in organizations. *Australian Journal of Management*, *45*, 3-14.

Kehoe, R. R., & Wright, P. M. (2013). The impact of high-performance human resource practices on employees' attitudes and behaviors. *Journal of Management, 39*, 366-391.

Lance, C. E. (2008). Why assessment centers do not work the way they are supposed to. *Industrial and Organizational Psychology, 1*, 84-97.

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

Lance, C. E., Hoffman, B. J., Gentry, W. A., & Baranik, L. E. (2008). Rater source factors represent important subcomponents of the criterion construct space, not rater bias. *Human Resource Management Review, 18*, 223-232.

Lance, C. E., Baranik, L. E., Lau, A. R., & Scharlau, E. A. (2009). If it ain't trait it must be method: (Mis)application of the multitrait–multimethod design in organizational research. In C. E. Lance & R. J. Vandenberg (Eds.), *Statistical and methodological myths and urban legends: Received doctrine, verity, and fable in the organizational and social sciences* (pp. 337-360). New York: Routledge.

Legood, A., van der Werff, L., Lee, A., & Den Hartog, D. (2020). A meta-analysis of the role of trust in the leadership- performance relationship. *European Journal of Work and Organizational Psychology*. doi: 10.1080/1359432X.2020.1819241

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

Malhotra, N. K., Schaller, T. K., & Patil, A. (2017). Common method variance in advertising research: When to be concerned and how to control for it. *Journal of Advertising*, *46*, 193–212.

Organ, D. W., & Greene, C. N. (1981). The effects of formalization on professional involvement: A compensatory process approach. *Administrative Science Quarterly, 26*, 237–252.

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

Ozer, D. J., & Benet-Martinez, V. (2006). Personality and the prediction of consequential outcomes. *Annual Review of Psychology*, *57*, 401-421.

Pace, V. L. (2010). Method variance from the perspectives of reviewers: Poorly understood problem or overemphasized complaint? *Organizational Research Methods*, *13*, 421-434.

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

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

Podsakoff, P. M., & Organ, D. W. (1986). Self-reports in organizational research: Problems and prospects. *Journal of Management, 12,* 69–82.

Reio, T. G. Jr. (2010). The treat of common method variance bias to theory building. *Human Resource Development Review, 9*, 405-411.

Ramsey, J., Punnett, B. J., & Greenidge, D. (2008). A social psychological account of absenteeism in Barbados. *Human Resource Management Journal*, *18*, 97-117.

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

Rodriguez-Andura, I., & Meseguer-Artola, A. (2020). Editorial: How to prevent, detect and control common method variance in electronic commerce research. *Journal of Theoretical and Applied Electronic Commerce Research*, *15*, 1-5.

Schaller, T. K., Patil, A., & Malhotra, N. K. (2015). Alternative techniques for assessing common method variance: An analysis of the theory of planned behavior research. *Organizational Research Methods*, *18*, 177-206.

Schermelleh-Engel, K., & Moosbrugger, H. (2003). Evaluating the fit of structural equation models: tests of significance and descriptive goodness-of-fit measures. *Methods of Psychological Research Online*, *8*(2), 23-74.

Siemsen, E., Roth, A., & Oliveira, P. (2010). Common method bias in regression models with linear, quadratic, and interaction effects. *Organizational Research Methods*, *13*, 456–76.

Sharma, S., Mukherjee, S., Kumar, A., & Dillon, W.R. (2005). A simulation study to investigate the use of cutoff values for assessing model fit in covariance structure models. *Journal of Business Research, 58*, 935-43.

Simmering, M. J., Fuller, C. M., Richardson, H. A., Ocal, Y., & Atinc, G. M. (2015). Marker variable choice, reporting, and interpretation in the detection of common method variance: A review and demonstration. *Organizational Research Methods*, *18*, 473-511.

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

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

Spector, P. E., Rosen, C. C., Richardson, H. A., Williams, L. J., & Johnson, R. E. (2019). A new perspective on method variance: A measure-centric approach. *Journal of Management, 45*, 855-880.

Sturman, M., Ukhov, A., Richardson, H., & Simmering, M. (2018, August 10-14). Mitigating effect of additional variables on common method variance in structural equations models. Paper presented at the Annual Meeting of the Academy of Management, Chicago, USA. doi: 10.5465/AMBPP.2018.14939abstract

Van De Voorde, K., & Beijer, S. (2015). The role of employee HR attributions in the relationship between high-performance work systems and employee outcomes. *Human Resource Management Journal*, *25*, 62-78.

Wang, S., Yi, X., Lawler, J., & Zhang, M. (2011). Efficacy of high-performance work practices in Chinese companies. *International Journal of Human Resource Management, 22*, 2419-2441.

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

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

Williams, L. J., Edwards, J. R., & Vandenberg, R. J. (2003). Recent advances in causal modeling methods for organizational and management research. *Journal of Management*, *29*, 903-936.

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

Williams, L. J., & McGonagle, A. K. (2016). Four research designs and a comprehensive analysis strategy for investigating common method variance with self-report measures using latent variables. *Journal of Business and Psychology*, *31*, 339-359.

Witteloostuijn, A., Eden, L., & Chang, S-J. (2020). Common method variance in international business research: A reflection. In L. Eden, Nielsen, B. B., & Verbeke, A. (Eds.), *Research Methods in international business* (pp. 409-413). Cham, Switzerland: Palgrave MacMillan.

Zeijen, M. E. L., Peeters, M. C. W., & Hakanen, J. J. (2018). Workaholism versus work engagement and job crafting: What is the role of self-management strategies? *Human Resource Management Journal, 28*, 357-373.

Table 1.

Explicit consideration of CMV, utilization of CMV preventative methodologies, and *post hoc* detection per year and by Journal (raw scores and as percentages of articles for the year).

Year and Journal	Number of articles	CMV Explicitly considered	CMV Preventative: Multisource	CMV Preventative: Time Lag/Long.	CMV Preventative: Experiment/Intervention	CMV Post Hoc Detection
2010	150	78 (52.00%)	42 (28.00%)	13 (8.67%)	6 (4.00%)	30 (20.00%)
HRMJ	11	5 (45.45%)	2 (18.18%)	2 (18.18%)	0 (0.00%)	3 (27.27%)
HRM	30	18 (60.00%)	11 (36.67%)	1 (3.33%)	1 (3.33%)	12 (40.00%)
PP	26	15 (57.69%)	15 (57.69%)	6 (23.08%)	4 (15.38%)	0 (0.00%)
IJRHM	80	40 (49.38%)	13 (16.25%)	5 (6.25%)	0 (0.00%)	15 (18.75%)
AMJ	3	0 (0.00%)	2 (66.67%)	0 (0.00%)	1 (33.33%)	0 (0.00%)
JOM	0	0	0	0	0	0
2011	181	98 (54.14%)	53 (29.28%)	16 (8.84%)	11 (6.08%)	36 (19.89%)
HRMJ	8	3 (37.50%)	3 (37.50%)	0 (0.00%)	0 (0.00%)	0 (0.00%)
HRM	19	10 (52.63%)	6 (31.58%)	2 (10.53%)	4 (21.05%)	6 (17.65%)
PP	17	12 (70.58%)	8 (47.06%)	6 (35.29%)	2 (11.76%)	3 (5.88%)
IJRHM	134	71 (52.99%)	34 (25.37%)	7 (5.22%)	5 (3.73%)	26 (19.40%)
AMJ	1	0 (0.00%)	1 (100.00%)	1 (100.00%)	0 (0.00%)	0 (0.00%)
JOM	2	2 (100.00%)	2 (100.00%)	0 (0.00%)	0 (0.00%)	1 (50.00%)
2012	200	111 (55.00%)	62 (31.00%)	26 (13.00%)	9 (4.50%)	51 (25.50%)
HRMJ	13	12 (92.31%)	3 (23.08%)	0 (0.00%)	0 (0.00%)	8 (61.54%)
HRM	28	16 (57.14%)	12 (42.86%)	3 (10.71%)	6 (21.43%)	6 (21.43%)
PP	18	11 (61.11%)	7 (38.89%)	10 (55.56%)	0 (0.00%)	2 (11.11%)
IJRHM	139	71 (51.08%)	39 (28.06%)	13 (9.35%)	2 (1.44%)	34 (24.46%)
AMJ	2	1 (50.00%)	1 (50.00%)	0 (0.00%)	1 (50.00%)	1 (50.00%)
JOM	0	0	0	0	0	0
2013	205	116 (56.59%)	55 (26.83%)	26 (12.68%)	11 (5.37%)	56 (27.32%)
HRMJ	10	5 (50.00%)	0 (0.00%)	1 (10.00%)	0 (0.00%)	2 (20.00%)
HRM	29	22 (75.86%)	11 (37.93%)	7 (24.14%)	4 (13.79%)	13 (44.83%)
PP	20	11 (55.00%)	11 (55.00%)	8 (40.00%)	4 (20.00%)	3 (15.00%)
IJRHM	138	74 (53.62%)	28 (20.29%)	8 (5.80%)	2 (1.45%)	37 (26.81%)
AMJ	4	1 (25.00%)	1 (25.00%)	2 (50.00%)	1 (25.00%)	1 (25.00%)
JOM	4	3 (75.00%)	4 (100.00%)	0 (0.00%)	0 (0.00%)	0 (0.00%)
2014	158	100 (63.29%)	51 (32.28%)	31 (19.62%)	11 (6.96%)	52 (32.91%)
HRMJ	11	8 (72.73%)	4 (36.36%)	3 (27.27%)	0 (0.00%)	3 (27.27%)
HRM	20	17 (85.00%)	9 (45.00%)	5 (25.00%)	1 (5.00%)	10 (50.00%)
PP	29	20 (68.97%)	12 (41.38%)	12 (41.38%)	2 (6.90%)	6 (20.69%)
IJRHM	88	50 (56.82%)	20 (22.72%)	7 (7.95%)	2 (2.27%)	29 (32.95%)
AMJ	7	4 (57.14%)	3 (42.86%)	2 (28.57%)	5 (71.43%)	4 (57.14%)
JOM	3	1 (33.33%)	3 (100.00%)	2 (66.67%)	1 (33.33%)	0 (0.00%)

Table 1 (continued)

Year and Journal	Number of articles	CMV Explicitly considered	CMV Preventative: Multisource	CMV Preventative: Time Lag/Long.	CMV Preventative: Experiment/ Intervention	CMV *Post Hoc* Detection
2015	178	85 (47.75%)	51 (28.65%)	25 (14.04%)	26 (14.61%)	37 (20.79%)
HRMJ	22	8 (36.36%)	7 (31.82%)	1 (4.55%)	1 (4.55%)	4 (18.18%)
HRM	27	18 (66.67%)	6 (22.22%)	3 (11.11%)	4 (14.81%)	8 (29.63%)
PP	37	13 (35.14%)	11 (29.73%)	10 (27.03%)	13 (35.14%)	0 (0.00%)
IJRHM	81	43 (60.56%)	24 (29.63%)	10 (12.35%)	3 (3.70%)	25 (30.86%)
AMJ	4	0 (0.00%)	0 (0.00%)	0 (0.00%)	4 (100.00%)	0 (0.00%)
JOM	7	3 (42.86%)	3 (42.86%)	1 (14.29%)	1 (14.29%)	0 (0.00%)
2016	152	100 (65.79%)	43 (28.29%)	22 (14.47%)	12 (7.89%)	52 (34.21%)
HRMJ	16	12 (75.00%)	5 (31.25%)	2 (12.50%)	0 (0.00%)	10 (62.50%)
HRM	44	19 (43.18)	12 (27.27%)	5 (11.36%)	8 (18.18%)	11 (25.00%)
PP	18	11 (61.11%)	10 (55.56%)	7 (38.89%)	1 (5.56%)	2 (11.11%)
IJRHM	67	53 (79.10%)	11 (16.42%)	7 (10.45%)	3 (4.48%)	29 (4.28%)
AMJ	1	0 (0.00%)	1 (100.00%)	1 (100.00%)	0 (0.00%)	0 (0.00%)
JOM	6	5 (83.33%)	4 (66.67%)	0 (0.00%)	0 (0.00%)	0 (0.00%)
2017	146	92 (63.01%)	66 (45.21%)	42 (28.77%)	8 (5.48%)	46 (31.51%)
HRMJ	20	10 (50.00%)	12 (60.00%)	5 (25.00%)	1 (5.00%)	3 (15.00%)
HRM	42	23 (54.76%)	22 (52.38%)	12 (28.57%)	4 (9.52%)	13 (30.95%)
PP	24	17 (70.83%)	11 (45.83%)	12 (50.00%)	0 (0.00%)	3 (12.50%)
IJRHM	58	42 (72.41%)	20 (34.48%)	11 (18.97%)	3 (5.17%)	27 (46.55%)
AMJ	1	0 (0.00%)	1 (100.00%)	1 (100.00%)	0 (0.00%)	0 (0.00%)
JOM	1	0 (0.00%)	0 (0.00%)	1 (100.00%)	0 (0.00%)	0 (0.00%)
2018	191	115 (60.21%)	90 (47.12%)	47 (24.61%)	18 (9.42%)	55 (28.80%)
HRMJ	27	17 (62.96%)	10 (37.04%)	8 (29.63%)	0 (0.00%)	10 (37.04%)
HRM	63	36 (58.06%)	31 (49.21%)	12 (19.05%)	12 (19.05%)	14 (22.22%)
PP	22	11 (50.00%)	14 (63.64%)	14 (63.64%)	1 (4.55%)	4 (18.18%)
IJRHM	77	49 (63.63%)	33 (42.86%)	13 (16.88%)	5 (6.49%)	26 (33.77%)
AMJ	0	0	0	0	0	0
JOM	2	2 (100.00%)	2 (100.00%)	0 (0.00%)	0 (0.00%)	1 (50.00%)
2019	149	116 (77.85%)	61 (40.94%)	37 (24.83%)	7 (4.70%)	56 (37.58%)
HRMJ	19	14 (73.68%)	10 (52.63%)	2 (10.53%)	1 (5.26%)	9 (47.37%)
HRM	26	23 (88.46%)	17 (65.38%)	10 (38.46%)	1 (3.85%)	10 (38.46%)
PP	21	14 (66.67%)	10 (47.62%)	13 (61.90%)	2 (9.52%)	1 (4.76%)
IJRHM	77	63 (81.82%)	19 (24.68%)	8 (10.39%)	3 (3.90%)	36 (46.75%)
AMJ	1	0 (0.00%)	1 (100.00%)	1 (10.00%)	0 (0.00%)	0 (0.00%)
JOM	5	2 (40.00%)	4 (80.00%)	3 (60.00%)	0 (0.00%)	0 (0.00%)
2010-2019	1710	1011 (59.12%)	574 (33.57%)	284 (16.61%)	119 (6.96%)	471 (27.54%)
HRMJ	157	94 (59.87%)	56 (35.77%)	24 (15.29%)	3 (1.91%)	52 (33.12%)
HRM	328	202 (61.59%)	137 (41.77%)	60 (18.29%)	45 (13.72%)	102 (31.10%)
PP	232	135 (58.19%)	109 (46.98%)	98 (42.24%)	29 (12.50%)	18 (7.76%)
IJRHM	939	556 (59.21%)	241 (25.67%)	89 (9.48%)	28 (2.98%)	282 (30.03%)
AMJ	24	6 (25.00%)	11 (45.83%)	8 (33.33%)	12 (50.00%)	6 (25.00%)
JOM	30	18 (60.00%)	22 (73.33%)	7 (23.33%)	2 (6.67%)	2 (6.67%)

Table 2.

Utilization of available *post hoc* detection methods per year (percentages are on total *post hoc* tests for the year).

Year	Harman	Harman with CFA	ULMC	Presumed Cause	Marker Correlational	Marker with CFA	Inspected Correlations Matrix	Unknown Test	Combination of / Multiple Tests	Presence of CMV	Presence of CMB
2010	19 (55.88%)	6 (17.65%)	1 (2.94%)	1 (2.94%)	1 (2.94%)	0 (0.00%)	1 (2.94%)	5 (14.71%)	4 (11.76%)	1 (2.94%)	0 (0.00%)
2011	20 (48.78%	5 (12.20%)	5 (12.20%)	1 (2.44%)	3 (7.32%)	1 (2.44%)	3 (7.32%)	3 (7.32%)	5 (12.20%)	3 (7.32%)	1 (2.44%)
2012	24 (38.71%	20 (32.26%)	9 (14.52%)	3 (4.84%)	3 (4.84%)	0 (0.00%)	2 (3.23%)	1 (1.61%)	10 (16.13%)	3 (4.84%)	0 (0.00%)
2013	25 (37.88%	18 (27.27%)	9 (13.64%)	3 (4.55%)	2 (3.03%)	0 (0.00%)	3 (4.55%)	6 (9.09%)	7 (10.61%)	5 (7.58%)	0 (0.00%)
2014	16 (24.62%)	20 (29.23%)	12 (18.46%)	3 (4.62%)	0 (0.00%)	2 (3.08%)	5 (9.23%)	8 (10.77%)	10 (15.38%)	4 (6.15%)	0 (0.00%)
2015	17 (37.78%)	19 (42.22%)	5 (11.11%)	0 (0.00%)	3 (6.67%)	0 (0.00%)	1 (2.22%)	0 (0.00%)	7 (15.56%)	1 (2.22%)	1 (2.22%)
2016	26 (36.62%)	20 (28.17%)	10 (14.08%)	2 (2.82%)	6 (8.45%)	1 (1.41%)	1 (1.41%)	5 (7.04%)	18 (25.35%)	5 (7.04%)	0 (0.00%)
2017	18 (29.51%)	16 (26.23%)	15 (24.59%)	1 (1.64%)	5 (8.20%)	1 (1.64%)	3 (4.92%)	2 (3.28%)	14 (22.95%)	4 (6.56%)	0 (0.00%)
2018	27 (30.68%)	21 (23.86%)	21 (23.86%)	1 (1.14%)	6 (6.82%)	2 (2.27%)	2 (2.27%)	8 (9.09%)	26 (29.55%)	10 (11.36%)	0 (0.00%)
2019	21 (29.17%)	29 (40.28%)	13 (18.06%)	1 (1.39%)	1 (1.39%)	3 (4.17%)	0 (0.00%)	4 (5.56%)	16 (22.22%)	9 (12.50%)	0 (0.00%)
Total (%)	213 (35.21%)	174 (28.71%)	100 (16.53%)	16 (2.64%)	30 (4.96%)	10 (1.65%	21 (3.47%)	42 (6.93%)	117 (19.34%	45 (7.43%)	2 (0.33%)