European Journal of Work and Organizational Psychology

ISSN: (Print) (Online) Journal homepage: www.tandfonline.com/journals/pewo20

Can Harman's single-factor test reliably distinguish between research designs? Not in published management studies

Matt C. Howard, Melanie Boudreaux & Matthew Oglesby

To cite this article: Matt C. Howard, Melanie Boudreaux & Matthew Oglesby (27 Aug 2024): Can Harman's single-factor test reliably distinguish between research designs? Not in published management studies, European Journal of Work and Organizational Psychology, DOI: 10.1080/1359432X.2024.2393462

To link to this article: https://doi.org/10.1080/1359432X.2024.2393462

EUROPEAN JOURNAL OF WORK AND ORGANIZATIONAL PSYCHOLOGY
https://doi.org/10.1080/1359432X.2024.2393462

Can Harman's single-factor test reliably distinguish between research designs? Not in published management studies

Matt C. Howard[a], Melanie Boudreaux[b] and Matthew Oglesby[c]

[a]Mitchell College of Business, University of South Alabama, Mobile, Alabama, USA; [b]Al Danos College of Business Administration, Nicholls State University, Thibodaux, Louisiana, USA; [c]Sanders College of Business and Technology, University of North Alabama, Florence, Alabama, USA

ABSTRACT

The goal of the current article is to conduct a widescale empirical investigation on the (in)efficacy of Harman's single-factor test by showing that the approach is insensitive to aspects of research design known to influence common method bias (CMB). Our systematic literature review of 1,619 sources demonstrates that the amount of variance explained by the first factor of Harman's single-factor test does not differ between cross-sectional and multi-wave, single-source and multi-source, or mono-method and multi-method studies. We instead find that extraneous aspects of studies influence the amount of variance explained, including the number of studied indicators and retained factors. These results therefore suggest that Harman's single-factor test is not a reliable assessment of CMB, and we hope these results prevent future researchers from applying the analysis. Our discussion concludes with alternative suggestions for identifying and addressing CMB, such as the application of sophisticated research designs and marker variable techniques.

ARTICLE HISTORY
Received 14 August 2023
Accepted 12 August 2024

KEYWORDS
Common method bias; harman's single-factor test; harman's one-factor test; statistics; systematic literature review

Common method bias (CMB) is systematic variance in observations that can be attributed to study designs rather than substantive relations between variables (Conway & Lance, 2010; MacKenzie & Podsakoff, 2012; Min et al., 2016), and it is particularly problematic for research. CMB can cause otherwise unrelated variables to appear related, and results that should not be statistically significant may become statistically significant in the presence of CMB. This can cause researchers to routinely interpret results as supportive of hypotheses, but they are instead committing inadvertent Type I errors (Chang et al., 2020; Podsakoff et al., 2003). Likewise, CMB can cause otherwise related variables to appear unrelated, and results that should be statistically significant can become non-significant. Researchers could interpret these results as failing to support hypotheses, leading to Type II errors (Baumgartner et al., 2021; Fuller et al., 2016).[1] Thus, CMB is a severe impediment to accurate interpretations and research progress.

Due to its considerable ramifications, researchers have developed several methods to assess the biasing influence of CMB on observations (Chang et al., 2020; Min et al., 2016; Podsakoff et al., 2012). Among the most popular of these methods is Harman's single-factor test. To perform Harman's single-factor test, all studied indicators are included in an unrotated exploratory factor analysis (EFA), and CMB is considered problematic if the first factor explains more than 50% of the variance in indicators (Conway & Lance, 2010; Podsakoff et al., 2003). Despite its straightforwardness, some variations can be seen in applications of the approach. Researchers differ on the factor extraction method; some researchers specify that CMB is concerning if only a single factor emerges (both in conjunction with and independent of the extracted variance cut-off); and a small portion of researchers utilize rotations (Banalieva et al., 2017; Delmas & Toffel, 2008; Fainshmidt et al., 2019; Luk et al., 2008; Song & Qu, 2018).

Although Harman's single-factor test is widely used, several recent articles cast doubt upon its validity. Two of these articles performed broad simulation studies, assessing the approach across a variety of conditions and variations. These studies showed that Harman's single-factor test routinely produces false positives and false negatives (the latter being more common), causing authors to state that the approach "cannot consistently produce an accurate conclusion" (Fuller et al., 2016, p. 3197) and "is not sufficient evidence to conclude that substantial common method bias may not be present" (Aguirre-Urreta & Hu, 2019, p. 56). Schwarz et al. (2017) assessed the performance of Harman's single-factor test in an experimental study. The authors showed that the approach could not detect CMB from several known sources (e.g., mood, scale format), causing the authors to conclude that, "Harman's one-factor test is unable to detect CMB" (p. 115). Lastly, Howard and Henderson (2023) conducted a review of EFA in tourism and hospitality research, and they showed that no authors had ever identified concerning levels of CMB in any application of Harman's single-factor test. The authors called for a "moratorium" on the analysis (p. 1). These findings and others suggest that Harman's single-factor test provides no utility for the assessment of CMB (Baumgartner et al., 2021).

Despite this convincing evidence, several authors have shown that Harman's single-factor test remains popular (Baumgartner & Weijters, 2021; Cooper et al., 2020; Howard & Henderson, 2023; Kock et al., 2021). Using our own systematic literature review database of prominent management journals

CONTACT Matt C. Howard mhoward@southalabama.edu

Supplemental data for this article can be accessed online at https://doi.org/10.1080/1359432X.2024.2393462

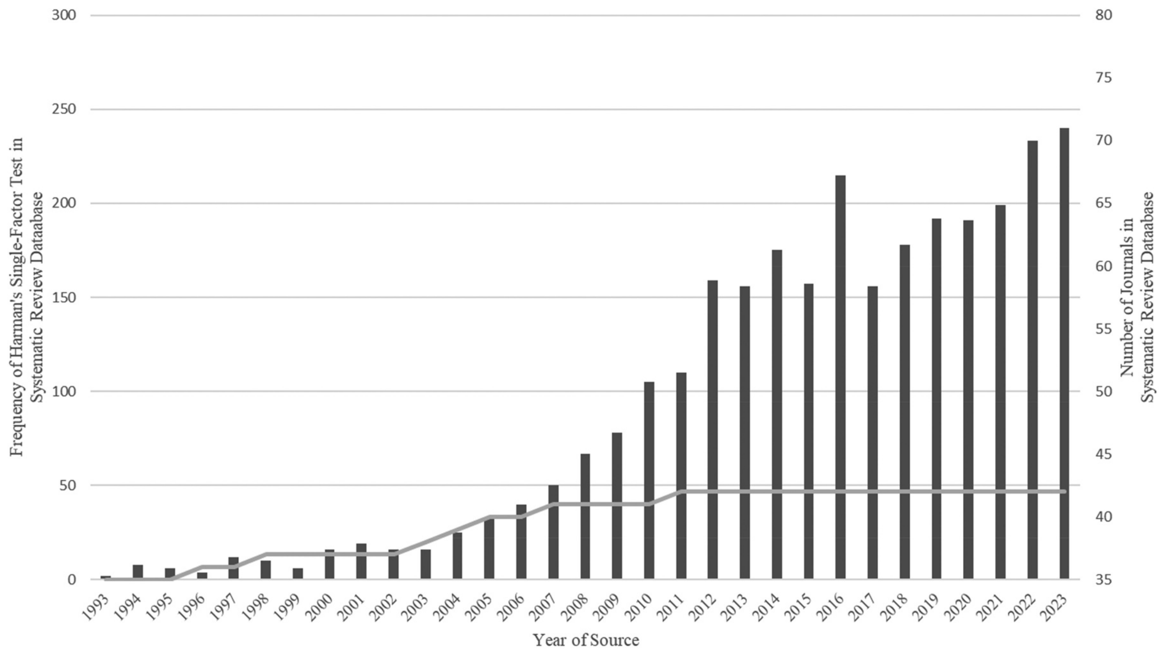

Figure 1. Frequency of Harman's single-factor tests by year in systematic literature review database.

(described below), Figure 1 demonstrates that the analysis has continued to steadily gain in popularity within the past 20 years, and its highest frequency of application occurred in 2023. This rise potentially corresponds with the greater attention paid to CMB sparked by the publication of influential works (e.g., Podsakoff et al., 2003), suggesting that researchers recognize that CMB is concerning but fall short of heeding the methodological and/or analytical recommendations of these prior works. Therefore, Harman's single-factor test is still continuously skewing authors' interpretations of results in modern research.

Given these considerations, the goal of the current article is to provide additional evidence regarding the (in)validity of Harman's single-factor test that may finally sway researchers' opinion of the analysis. Aguirre-Urreta and Hu (2019) noted that investigations of Harman's single-factor test have been either broad simulation studies or narrow empirical studies. While broad simulation studies test approaches across a variety of conditions, they suffer from their artificiality and reliance upon assumptions; while narrow empirical studies provide realistic assessments without relying on assumptions, they cannot test approaches across a variety of conditions. Thus, a significant gap in our understanding of Harman's single-factor test – and a possible cause for its continued proliferation – may be due to the dearth of widescale empirical investigation. We conduct this widescale empirical investigation in the current article.

While it is impossible to identify CMB in empirical studies with certainty, it is widely believed that more robust research designs produce less CMB (Chang et al., 2020; Min et al., 2016; Schwarz et al., 2017). Any assessment of CMB should discriminate between more and less robust research designs due to their differing amounts of produced CMB, and the current article tests whether Harman's single-factor test achieves this function in empirical studies. To accomplish this goal, we systematically review 1,699 applications of Harman's single-factor test in management outlets. We record the variance explained by the first factor, and we study attributes believed to impact CMB. These include the use of (1) multi-wave, (2) multi-source, and (3) multi-method designs. We also record aspects of studies that may affect the results of Harman's single-factor test but not CMB, which would cause the analysis to produce inaccurate results. These include (1) sample size, (2) number of factors, (3) number of indicators, (4) subfield of study, and (5) ranking tier of outlet. We test whether the research design aspects relate to the variance extracted by the first factor, which can indicate whether Harman's single-factor test differentiates between designs that produce differing amounts of CMB. We also determine whether these aspects still influence the extracted variance when accounting for the extraneous study aspects. If they do, Harman's single-factor test may accurately assess CMB. If they do not, our results would suggest that Harman's single-factor test is more reflective of extraneous aspects than CMB, and it therefore is not able to accurately assess CMB.

We conclude the current article with a discussion of methodological approaches to reduce CMB and alternative analyses to assess CMB. As Harman's single-factor test is still widely applied, authors have shown laudable interest in addressing CMB, but they may be presently unaware of more appropriate approaches. Thus, we hope that our systematic literature review will identify the validity of the analysis, and we likewise hope that our discussion will direct researchers towards more appropriate methodological and analytical treatments of CMB.

These efforts provide many implications for research and practice. First, the current article provides much-needed evidence regarding the (in)ability of Harman's single-factor test to assess CMB, perhaps swaying researchers who were unphased by prior studies. Second, if the current results are unsupportive of Harman's single-factor test, then researchers should abandon the analysis and allocate their focus to other assessments. An alternative analysis may provide accurate assessments of

CMB; however, it may be used less often because it is understudied, and the current article could promote its further investigation. Third, Harman's single-factor test has been repeatedly used to support the validity of research findings. If the analysis is shown to be ineffective, ample prior research would need to be reinvestigated using more robust analyses, opening avenues for future empirical research. Fourth, our discussion provides recommendations regarding how researchers can appropriately address CMB without using Harman's single-factor test, including both procedural remedies and measured variable techniques (Spector et al., 2019; Williams & McGonagle, 2016). By redirecting researchers to more fruitful approaches, the current article can promote more accurate research moving forward. Thus, the current article can significantly improve the methodological robustness of future studies across all fields of study.

Background

As mentioned, CMB is systematic variance in observations that can be attributed to research designs rather than substantive relations between variables (Chang et al., 2020; Conway & Lance, 2010; Fuller et al., 2016; MacKenzie & Podsakoff, 2012). CMB is believed to inflate or deflate the relations between variables, but it does not have an equal effect on all relations – even within the same study (Min et al., 2016; Podsakoff et al., 2003). While CMB may strongly inflate or deflate the relations of certain variables, it may have a relatively small effect on others. Due to the varied effects of CMB, it is particularly difficult to discern in observations.

Harman's single-factor test aims to assess CMB by identifying the amount of variance explained by the first factor of an EFA, which will always be the factor that explains the most variance in the indicators (Aguirre-Urreta & Hu, 2019; Fuller et al., 2016). If the first factor explains more than 50% of the variance in the indicators, then CMB is a concern by the standards of Harman's single-factor test. When performing an EFA, factors reproduce the common variance of indicators (Howard, 2016; Howard & Henderson, 2023).[2] In other words, factors represent the interrelations of indicators. When all indicators are strongly interrelated, they load strongly onto a common latent factor, and that latent factor explains more variance in these indicators. As the interrelations of the indicators decrease, they load less strongly onto a common latent factor, and that latent factor explains less variance in these indicators. The logic of Harman's single-factor test is, therefore, that CMB is concerning when it inflates indicators' covariances to an extent that their variances can be mostly explained by a single latent factor (Aguirre-Urreta & Hu, 2019; Baumgartner et al., 2021; Fuller et al., 2016).

Authors have investigated whether Harman's single-factor test is a suitable assessment of CMB in broad simulations (Aguirre-Urreta & Hu, 2019; Fuller et al., 2016) and narrow empirical studies (Schwarz et al., 2017), and both types of investigations have found the approach to be inaccurate; however, the continued proliferation of Harman's single-factor test (Figure 1) (Cooper et al., 2020; Howard & Henderson, 2023; Kock et al., 2021) implies that researchers consider its accuracy to still be uncertain. While justifications can only be presumed, researchers may believe that the assumptions applied to perform these broad simulation studies do not reflect the functioning of CMB in empirical studies, and they may also believe that the narrow empirical studies did not investigate the analysis in relevant contexts. It is also possible that researchers continue to apply Harman's single-factor test due to its ease of application, as the analysis can be applied with the click of a few buttons in many statistics programs, such as SPSS. If this is the case, researchers may believe that the relative ease of the analysis outweighs its inaccuracy.

For these reasons, researchers may continue to inappropriately disregard the valuable insights provided by prior simulation and empirical studies, as evidenced by their persistent application of Harman's single-factor test. In the current article, we make efforts to address this issue by performing a broad empirical study, which does not rely on assumptions (unlike prior simulation studies) and broadly investigates the approach across its many applied contexts in management (unlike prior narrow empirical studies). Specifically, the current article investigates three hypotheses via a systematic review of Harman's single-factor test in empirical research, wherein we determine whether the approach's results differ by specific study characteristics – including both characteristics known to reduce CMB and those that do not.

By definition, research designs that include methodological separation between variables reduce CMB (Podsakoff et al., 2003, 2012). If Harman's single-factor test can accurately assess CMB, then it should produce differing results based on the applied research design. In other words, Harman's single-factor test should be able to distinguish between research designs known to produce more or less CMB; the amount of variance explained by the first factor should be greater for weaker methodological designs, and it should be smaller for stronger methodological designs (Baumgartner et al., 2021; Doty & Glick, 1998). While research designs can be distinguished by many different manners, we presently focus on three: temporal separation, source separation, and measurement method separation.

Temporal separation refers to the measurement of variables at different points in time, such as obtaining self-reported job satisfaction and one week later obtaining self-reported job performance (Becker, 2021; Min et al., 2016). Research designs that utilize temporal separation are called multi-wave studies (stronger design), whereas research designs conducted at one-point in time are called cross-sectional studies (weaker design) (Brannick et al., 2010; Cooper et al., 2020). Temporal separation reduces CMB due to many reasons (Podsakoff et al., 2003, 2012). Transient mood states are known to influence response patterns, and obtaining responses at different points in time can help ensure that these mood states do not systematically paint responses to all indicators. Likewise, measuring all variables at the same time increases the likelihood that participants will activate the same mental schemas for all responses (particularly those associated with constructs measured first), whereas obtaining responses at different times can ensure that participants activate the most relevant mental schemas for each construct. These considerations are relevant whether responses are self- or other-reported, indicating that temporal separation can reduce CMB in results across various research designs (Becker, 2021; Brannick et al., 2010; Cooper et al., 2020; Min et al., 2016).

Source separation refers to the measurement of variables from different sources, such as obtaining self-reported job satisfaction and supervisor-reported job performance (Avolio et al., 1991; Conway & Lance, 2010). Research designs that utilize source separation are called multi-source studies (stronger design), whereas studies that obtain all variables from a common source are called single-source studies (weaker design) (Avolio et al., 1991; Bozionelos & Simmering, 2022). Source separation reduces CMB because psychological processes (e.g., consistency motif and social desirability) cause participants to respond in a consistent manner, even when their standing on the constructs call for inconsistency (Podsakoff et al., 2003, 2012). For instance, a person may feel a subconscious tendency to respond affirmatively regarding their self-efficacy after reporting high self-esteem, even if their self-efficacy is low, because they have a desire to appear rational. When providing responses regarding another person, psychological processes (e.g., leniency bias and acquiescence) can likewise prompt the participant to evaluate the other in a consistent manner (Podsakoff et al., 2003, 2012). By obtaining responses from multiple sources, the influence of these psychological processes can be reduced or even eliminated, as these processes are not involved in the measurement of all constructs. For this reason, researchers have touted multi-source studies as among the most effective approaches to reduce CMB (Avolio et al., 1991; Bozionelos & Simmering, 2022; Conway & Lance, 2010).

Measurement method separation refers to measurement of variables using different types of measures, such as obtaining self-reported job satisfaction and objective outputs of job performance (e.g., salesperson sales) (Campbell & Fiske, 1959; Conway, 1998; Viswanathan & Kayande, 2012). Research designs that utilize methodological separation are called multi-method studies (stronger design), whereas studies that use a single measurement method are called mono-method studies (weaker design). Measurement method separation also reduces CMB in several manners (Campbell & Fiske, 1959; Conway, 1998). Indicators with similar social desirability demands incur similar responses. With differing measurement approaches, researchers can ensure that their measures incur different social desirability demands, reducing the consistency of responses due to CMB. For instance, obtaining both self-reported survey responses and observed behavioural indicators would pose fewer concerns for CMB, as demand characteristics for each are notably different. Also, people are more likely to respond in a similar manner when survey items are formatted similarly, such as using common response formats (Dudycha & Carpenter, 1973; Podsakoff et al., 2003). By using different types of measures, researchers can ensure that common response formats are avoided and CMB is reduced.

Because these three research designs reduce CMB, the variance explained by the first factor of Harman's single-factor test should differ across these research designs. The variance explained should be less for data collected via multi-wave than cross-sectional, multi-source than single-source, and multi-method than mono-method research designs. If Harman's single-factor test does not show sensitivity across research designs, then the analysis cannot be considered a reliable approach to assess CMB. It should be noted that we do not test whether stronger designs always produce a first factor explaining less than 50% of variance or whether weaker designs always produce a first factor explaining more than 50% of variance. Instead, we only test whether the amount of variance explained differs across designs, which is a much less conservative assessment of Harman's single-factor test. Significant concerns should be expressed if Harman's single-factor test does not satisfy this relatively lenient expectation.

Hypothesis 1: The first factor obtained from Harman's single-factor test accounts for less variance with (a) multi-wave than cross-sectional, (b) multi-source than single-source, and (c) multi-method than mono-method research designs.

More than CMB can influence the common variance of indicators, and therefore more than CMB alone can influence the variance explained by the first factor of an EFA (Hair et al., 2019; Howard & Henderson, 2023). This may be a weakness of Harman's single-factor test. The results of the analysis may be more reflective of study aspects that do not influence CMB, and study results may pass (or fail) Harman's single-factor test due to these other aspects rather than their exclusion (or inclusion) of CMB. If this is the case, then Harman's single-factor test would serve little function as a diagnostic tool for CMB. We investigate five aspects independent of CMB that may influence the variance explained by the first factor of an EFA.

Howard and Henderson (2023) and Howard (2023) highlighted that the most appropriate sample size guidelines for EFA are based on the common variance of indicators (Gaskin & Happell, 2014; Rouquette & Falissard, 2011). The authors also noted that underpowered studies inappropriately overestimate common variance attributed to latter factors and underestimate common variance attributed to former factors. This causes the first factor to explain less variance in indicators regardless of its relation to CMB. Therefore, studies with smaller sample sizes are expected to produce first factors from Harman's single-factor test that explain less variance.

Further, variance among indicators is spread among retained factors, and the amount of explained variance cannot exceed 100%. Factors tend to explain more variance when extracted with fewer factors, as they do not compete with as many other factors to explain the limited amount of variance (Hair et al., 2019; Wu et al., 2014). On the other hand, factors tend to explain less variance when extracted with more factors, as they compete with more factors to explain the limited amount of variance. Thus, studies with more retained factors are expected to produce first factors from Harman's single-factor test that explain less variance in indicators.

Similarly, increasing numbers of indicators tends to produce both more emergent factors and less common variance that can be explained by a single factor (Hair et al., 2019; Howard & Henderson, 2023). Increasing numbers of indicators may gauge more constructs, and it may also introduce more variability across the indicators that reduces common variance (Hair et al., 2019; Howard & Henderson, 2023). Studies with more indicators are likewise expected to produce first factors from Harman's single-factor test that explain less variance in indicators.

Lastly, Cote and Buckley (1987) showed that the amount of CMB in results differs across fields of study. For instance, these authors found that CMB was more prevalent in studies of education than business. Studies investigating similar constructs produce more common variance, as indicators of these constructs are more strongly interrelated. In turn, this would cause the first factor from Harman's single-factor test to explain more variance. In the current article, we broadly investigate differences across subfields of management, as certain subfields may habitually study (dis)similar constructs that produce more (or less) common variance. We also test whether Harman's single-factor test explains more variance in outcomes based on the ranking tiers of outlets, as certain tiers may be more conservative regarding assessments of CMB. In exploring these differences, we do not make hypotheses regarding which subfield(s) or tiers produce Harman's single-factor test results with larger (or smaller) amounts of variance explained by the first factor; however, narrower subfields could be expected to study more similar indicators, and higher tier journals could be expected to be more conservative regarding the results of Harman's single-factor test. We instead test differences across subfields and tiers in an exploratory manner, akin to Cote and Buckley (1987), and we propose that subfield and tier differences occur for the variance explained by the first factor of Harman's single-factor test.

Hypothesis 2: The first factor obtained from Harman's single-factor test accounts for less variance with (a) larger sample sizes, (b) more retained factors, and (c) more indicators. It also demonstrates mean differences in magnitude across (d) subfields of management and (e) ranking tiers of journals.

Hypothesis 3: The research design aspects included in Hypothesis 1 still relate to the amount of variance explained by the first factor when accounting for the extraneous study aspects included in Hypothesis 2.

Method

Article retrieval and coding procedure

To create our systematic literature review database, we performed searches in December of 2024. We identified premier journals of management by using the Academic Journals Guide (AJG) (CABS, 2021). The AJG categorizes business-relevant journals and ranks them into five tiers: 4*, 4, 3, 2, and 1. We selected all journals in six AJG categories relevant to management that were ranked 4*, 4, or 3, and we chose these three tiers for two primary reasons. First, the first three tiers of the AJG list represent outlets most directly relevant to business research. By only including these three tiers, we can better ensure that our review solely focuses on management research. Second, higher-ranked journals are known to have a disproportionate influence on the field of management, and researchers are more likely to mimic approaches seen in higher-ranked outlets. Observing and correcting problematic approaches in these outlets could help ensure that the present manuscript provides broad contributions to management research.

This selection process resulted in the identification of 76 journals, listed in Supplemental Material A. While restricting searches to these journals, the authors separately searched for the terms, "Single-Factor Test" and "One-Factor Test", using Google Scholar, and all articles that performed Harman's single-factor test were retained. This process returned 2,911 sources. For each studied aspect of Harman's single-factor test, three coders independently coded subsets of 20 sources until sufficient interrater agreement was reached. We used a Cohen's κ cut-off of .80 for dichotomous coding categories (wave, source, and method) and an ICC cut-off of .80 for continuous coding categories (variance explained, sample size, retained factors, and indicators), which were chosen based on prior recommendations (Gisev et al., 2013; Koo & Li, 2016). Once these cut-offs were met, the coders then independently coded the remaining articles, conferring on any unclear coding decisions. The coders first coded the amount of variance explained by the first factor, as only articles reporting this statistic could be included in analyses. The number of Harman's single-factor tests with their first factor variance reported was 1,699 in 1,619 articles. These Harman's single-factor tests were then coded for each of the studied aspects described below. Our systematic literature review database is provided in Supplemental Material B.

Studied aspects

We coded the aspects of Harman's single-factor test based on the included indicators rather than the study itself, as this approach better reflects the impact of the studied aspects on the analysis results. For instance, some studies collected multi-source data from employees and their supervisors, and proceeded to conduct two entirely separate Harman's single-factor tests – one on the indicators collected from the employees and one on the indicators collected from the supervisors. In this case, we would have coded these as two separate Harman's single-factor tests performed on single-source data (rather than one analysis on multi-source data), as neither included indicators from multiple sources. Therefore, our systematic literature review database and analyses wholly reflect the Harman's single-factor tests rather than the broader study.

First factor variance explained. The coders recorded the amount of variance explained by the first factor of the Harman's single-factor test.

Occasion. Studies were recorded as either cross-sectional or multi-wave. Cross-sectional studies gave all measures in a single sitting for each participant, such as a paper survey given at one point in time. Multi-wave studies gave measures at different points in time, such as a paper survey given at one point in time followed by a second paper survey given a week later. For analyses, cross-sectional studies were coded as 0, and multi-wave studies were coded as 1.

Source. Studies were recorded as either single-source or multi-source. Single-source studies obtain all study data from one referent, such as a survey given to employees. Multi-source studies obtain study data from multiple referents, such as a self-report survey given to employees and an other-report survey given to supervisors to evaluate the employees. For analyses, single-source studies were coded as 0, and multi-wave studies were coded as 1.

Method. Studies were recorded as either mono-method or multi-method. Mono-method studies obtain study data from a single measurement approach, such as a survey alone. Multi-method studies obtain data from multiple measurement approaches, such as a survey and observations. For analyses, mono-method studies were coded as 0, and multi-method studies were coded as 1.

Sample size. The sample size of each study was recorded. When authors specified the sample size for the Harman's single-factor test, we coded this figure. Most authors, however, did not state the sample size for the analysis, and we coded the sample size of the overall study for most articles. When multiple occasions were involved, we coded the number of participants for the smallest wave included within the Harman's single-factor test, which was most often the final wave. For instance, if a study included three waves of data collection with sample sizes of 500 (Wave 1), 400 (Wave 2), and 300 (Wave 3), we coded the sample size as 300. This was because the analysis would calculate its estimates from the 300 participants for those indicators.

Retained factors. The coders recorded the number of retained factors from the EFA used to perform the Harman's single-factor test. Authors must have stated that the number of factors was specifically associated with the Harman's single-factor test for this aspect to be recorded, rather than a number of factors retained from a separate EFA, confirmatory factor analysis (CFA), or confirmatory composite analysis (CCA) (Hair et al., 2020).

Indicators. The coders recorded the number of indicators included in each Harman's single-factor test. If authors did not explicitly state this number, it was assumed that all non-categorical indicators used to measure the substantive variables of interest were included in the analysis.

Subfield of study. The subfield of management was recorded based on the journal in which the source article was published and its AJG categorization (Supplemental Material A). The six categories were: Entrepreneurship and Small Business Management; General Management, Ethics, Gender and Social Responsibility; International Business and Area Studies; Organizational Studies; Psychology (Organizational); and Strategy. While this aspect was coded as a categorical variable, it was analysed via dummy codes with the subfield of Entrepreneurship & Small Business Management serving as the referent for all dummy codes.

Tier of outlet. The tier of outlet was recorded based on the categorization of the representative journal (Supplemental Material A). While this aspect was coded as a categorical variable, it was analysed via dummy codes with the tier of 4* serving as the referent for all dummy codes.

Results

Results of primary analyses

We first assessed our systematic literature review database for outliers and influential cases. We considered outliers and influential cases to be those with a z-score above four, based on the recommendation of Hair et al. (2019).[3] Six single-factor tests had z-scores greater than four regarding their sample size, and ten had z-scores greater than four regarding their number of indicators. Eleven single-factor tests had z-scores greater than four for their number of factors. We rescaled these observations to the next largest value with a z-score below four. Further, the number of observations and variances of each condition were not equal, especially when inspecting combinations of study design aspects (e.g., multi-wave, multi-source, and multi-method studies). We addressed this concern in two manners. First, we did not test interaction between study design aspects. Our hypotheses did not involve interactions, and the variance explained by interactions is assumed to be notably smaller than any main effects (Hair et al., 2019). Authors have suggested that this is a viable approach to address unequal sample sizes and variances when interactions are not of theoretical importance (Chakraborty et al., 2009; Gunst & Mason, 2009). Second, we chose analyses recommended for our data characteristics, specifically those that can best account for unequal variances across groups. To compare means between two groups, we utilized Welch's t-test (Delacre et al., 2017). To compare means between multiple groups, we utilized Kruskal-Wallis one-way ANOVA (Odiase & Ogbonmwan, 2005). To analyse all our studied variables together, we utilized regression with multiple dummy-coded predictors (Alkharusi, 2012). The largest VIF value of any analysis was 1.47, below the widely recommended cut-off of 5 (Hair et al., 2019), indicating that multicollinearity was not an issue.

Table 1 provides frequencies of each coding category as well as means and standard deviations of continuous variables. It also provides the means and standard deviations for the variance explained separated by each level of the coding categories, and it includes correlations of the continuous variables with the variance explained. Supplemental Material C includes the frequency of study design aspects and their interactions.

We first assessed whether the research design aspects related to the variance explained by the first factor of Harman's single-factor test by conducting Welch's t-test. Our results were statistically significant for the number of measurement occasions (*Cohen's* $d = -.18$, $t = -2.68$, $p < .01$) but not sources (*Cohen's* $d = -.07$, $t = -1.00$, $p = .32$) or methods (*Cohen's* $d = .20$, $t = 1.78$, $p = .08$).[4] Based on Cohen's (1992) guidelines, each of these results would be within the small range (*Cohen's* $d \approx .20$) When inspecting these results, the mean difference in variance explained between cross-sectional ($\bar{x} = 26.93$, $\sigma = 9.24$) and multi-

Table 1. Frequencies, means and standard deviations, and correlations.

Dichotomous Variables	Frequency (Percentage)	Mean Variance Explained (Std. Dev)
1a.) Cross-Sectional	1,446 (85%)	26.93 (9.24)
1b.) Multi-Wave	253 (15%)	25.39 (8.31)
2a.) Single-Source	1,476 (87%)	26.79 (9.09)
2b.) Multi-Source	223 (13%)	26.12 (9.32)
3a.) Mono-Method	1,611 (95%)	26.80 (9.06)
3b.) Multi-Method	88 (5%)	24.88 (9.94)
4a.) ENT-SBM	285 (17%)	23.93 (9.01)
4b.) ETHICS-CSR-MAN	781 (46%)	28.04 (9.40)
4c.) IB&AREA	348 (20%)	26.28 (8.14)
4d.) ORGSTUD	36 (2%)	24.88 (11.31)
4e.) PSYCH-WOP-OB	154 (9%)	27.95 (7.84)
4f.) STRAT	95 (6%)	24.24 (9.12)
5a.) 4* AJG Tier	165 (10%)	23.53 (7.30)
5b.) 4 AJG Tier	307 (18%)	24.24 (8.34)
5c.) 3 AJG Tier	1,227 (72%)	27.74 (9.31)
Total	1,699 (100%)	26.70 (9.12)
Continuous Variables	Mean (Std. Dev.)	Correlation w/Variance Explained
5.) Sample Size	563 (1,779)	−.10**
6.) Retained Factors	6.48 (3.35)	−.36**
7.) Indicators	27.42 (14.70)	−.09**

Note. Initialisms and acronyms listed in 4a-4f are management subfield categories of the AJG list. In order, they are: entrepreneurship and small business management; general management, ethics, gender and social responsibility; international business and area studies; organizational studies; psychology (organizational); and strategy. The reported mean and standard deviation of sample size, retained factors, and indicators represents the sample after rescaling outliers.
*$p < .05$.
**$p < .01$.

wave ($\bar{x} = 25.39$, $\sigma = 8.31$), single-source ($\bar{x} = 26.79$, $\sigma = 9.09$) and multi-source ($\bar{x} = 26.12$, $\sigma = 9.32$), and mono-method ($\bar{x} = 26.80$, $\sigma = 9.06$) and multi-method ($\bar{x} = 24.88$, $\sigma = 9.94$) studies were modest compared to their standard deviations. This suggests that, while the first factor explains slightly more variance in weaker than stronger research designs, the magnitude of this difference is not large enough to meaningfully differentiate between the research designs. It should also be highlighted that, when all three of these study design aspects were included together in a regression analysis (Table 2), none had a statistically significant effect (all $p > .05$) and the total amount of variance explained in the first factor variance was negligible ($R^2 = .00$). Figures 2–4 provide a visual representation of differences (or lack thereof) in the variance explained by the first factor of Harman's single-factor test when separated by research design aspects.

Next, we tested whether the other study aspects related to the amount of variance explained by the first factor of Harman's single-factor test. We included all five extraneous aspects in a regression analysis (Table 2). The sample size ($B = -.00$, $\beta = -.07$, $t = -2.21$, $p = .03$), number of factors ($B = -1.09$, $\beta = -.44$, $t = -11.26$, $p < .01$), and number of indicators ($B = .06$, $\beta = .11$, $t = 2.87$, $p < .01$) each significantly related to the variance explained. The omnibus assessment of the subfield dummy codes was statistically significant, as the change

Table 2. Regression results.

	First Factor Variance			First Factor Variance			First Factor Variance		
	B	β	t	B	β	t	B	β	t
1.) Wave	1.35	.15	1.86				.22	.03	.23
2.) Source	−.40	−.04	−.49				−1.23	−.15	−1.17
3.) Method	−1.24	−.14	−.99				−1.42	−.17	−.93
4a.) ETHICS-CSR-MAN				2.28	.28	3.04**	2.27	.27	3.01**
4b.) IB&AREA				1.28	.15	1.56	1.21	.15	1.47
4c.) ORGSTUD				1.21	.15	.68	1.07	.13	.60
4d.) PSYCH-WOP-OB				4.09	.50	4.01**	4.04	.49	3.93**
4e.) STRAT				−.46	−.06	−.38	−.50	−.06	−.41
5a.) 4 AJG Tier				−.84	−.10	−.87	−.89	−.11	−.09
5b.) 3 AJG Tier				1.91	.23	2.29*	1.86	.23	2.21*
6.) Sample Size				−.00	−.07	−2.21*	−.00	−.07	−2.11*
7.) Retained Factors				−1.09	−.44	−11.26**	−1.07	−.43	−11.04**
8.) Indicators				.06	.11	2.87**	.06	.11	2.73**
R^2			.00			.20**			.20**

*$p < .05$.
**$p < .01$.

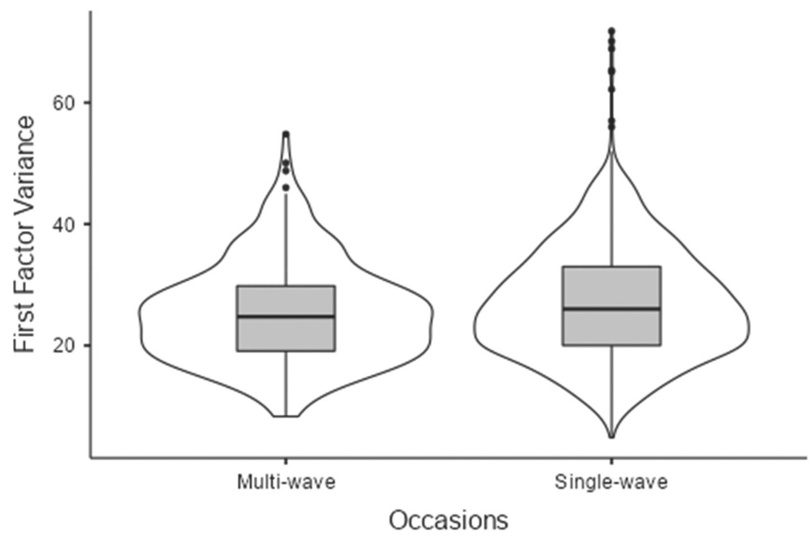

Figure 2. Violin plots of first factor variance separated by occasion.

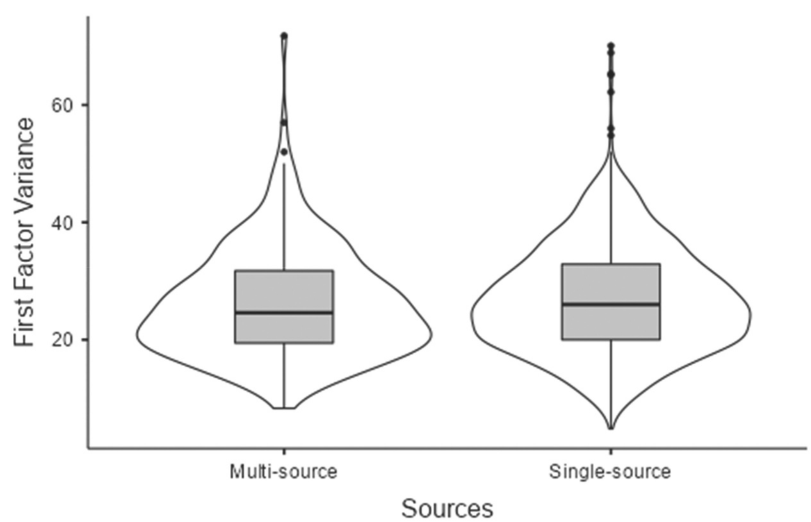

Figure 3. Violin plots of first factor variance separated by source.

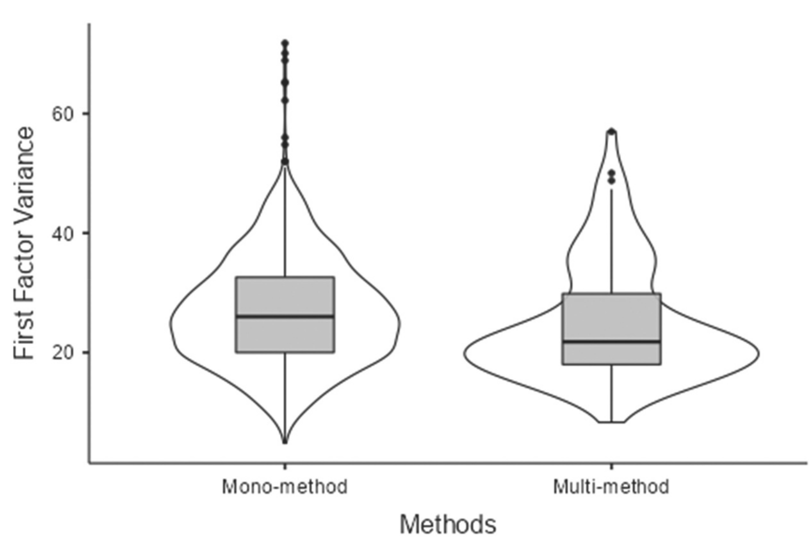

Figure 4. Violin plots of first factor variance separated by method.

in R^2 was statistically significant from their inclusion ($\Delta R^2 = .02$, $F = 4.67$, $p < .01$). The omnibus assessment of the tier dummy codes was similarly statistically significant, as the change in R^2 was statistically significant from their inclusion ($\Delta R^2 = .02$, $F = 10.03$, $p < .01$). Readers can refer to Tables 1–2 to observe specific subfield and tier differences. The total amount of variance explained in the first factor variance by these five predictors was sizable ($R^2 = .20$). These results suggest that Harman's single-factor test is indeed influenced by extraneous aspects.

We assessed whether the research design aspects related to the variance explained when accounting for the extraneous aspects. We included all aspects in a separate regression analysis (Table 2). The sample size ($B = -.00$, $\beta = -.07$, $t = -2.11$, $p = .04$), number of factors ($B = -1.07$, $\beta = -.43$, $t = -11.04$, $p < .01$), and number of indicators ($B = .06$, $\beta = .11$, $t = 2.73$, $p < .01$) significantly related to the variance explained. Multiple subfield comparisons were statistically significant, and their omnibus assessment was still statistically significant even when added after the research design aspects ($\Delta R^2 = .02$, $F = 4.53$, $p < .01$). Only one tier comparison was statistically significant, but the omnibus assessment was similarly statistically significant ($\Delta R^2 = .02$, $F = 9.85$, $p < .01$). The number of measurement occasions ($B = .22$, $\beta = .03$, $t = .23$, $p = .82$), sources ($B = -1.23$, $\beta = -.15$, $t = -1.17$, $p = .24$), and methods ($B = -1.42$, $\beta = -.17$, $t = -.93$, $p = .35$) did not significantly relate to the amount of variance explained. The total amount of variance explained in the first factor variance was essentially identical to the regression analysis with the extraneous aspects alone ($R^2 = .20$). These results suggest that Harman's single-factor test is unable to differentiate between research designs when accounting for extraneous aspects.

Results of sensitivity analyses

We performed a series of sensitivity analyses. In our primary analyses, we coded the number of measurement occasions as a dichotomous variable, but this variable could also be coded continuously. That is, it could instead be coded based on the amount of time between measurement occasions. For this reason, we conducted an alternative set of analyses, wherein this variable was coded as the number of days between measurement occasions in the study design. Cross-sectional studies were coded as 0.[5] The correlation between the number of days between measurement occasions and the variance explained by the first factor was $-.06$ ($p < .01$), and it was $-.17$ ($p = .02$) when only including multi-wave studies in analyses. When conducting a linear regression analysis with all predictors, the number of days between measurement occasions was not statistically significant ($B = -.00$, $\beta = -.02$, $t = -.42$, $p = .68$), and it still was not significant when only including multi-wave studies in analyses ($B = -.00$, $\beta = -.04$, $t = -.35$, $p = .72$). These results replicated our primary analyses, as the dichotomous treatment of multi-wave studies also produced a small but significant effect when studied in isolation and a non-significant result when studied with other predictors. Because our results were consistent between the two sets of analyses, these findings support the robustness of our interpretations.

Further, the only variables with more than one missing observation were the number of included items (85) and retained factors (810), which were not reported in some studies. Due to the significantly larger amount of missing data for the number of retained factors, we replicated our final regression analysis without this variable. The effect of the number of measurement occasions became statistically significant, but its effect was still modest ($B = 1.45$, $\beta = .16$, $t = 2.03$, $p = .04$). All other inferences were consistent between the two sets of analyses, and these findings again support the robustness of our interpretations.

Discussion

Broad simulation studies and narrow empirical studies have cast doubt on the validity of Harman's single-factor test (Aguirre-Urreta & Hu, 2019; Fuller et al., 2016; Schwarz et al., 2017), but researchers continue to use the approach to assess CMB (Baumgartner & Weijters, 2021; Howard & Henderson, 2023; Kock et al., 2021). To deter the use of Harman's single-factor test, the current article performed a broad empirical study via systematic literature review to determine whether the approach can differentiate research designs. Stronger research designs are believed to produce less CMB, whereas weaker research designs are believed to produce more CMB (MacKenzie & Podsakoff, 2012; Min et al., 2016). If accurate, Harman's single-factor should differentiate research designs that produce more or less CMB (Conway & Lance, 2010; Podsakoff et al., 2003). Therefore, the goals of the current article were to assess whether Harman's single-factor test was sensitive to research designs, insensitive to extraneous aspects of studies, and still sensitive to research designs when accounting for extraneous aspects.

Our results show that the variance explained by the first factor of Harman's single-factor test was significantly influenced by the number of measurement occasions when tested in isolation, but differences in variance explained between all research designs were small relative to their standard deviations. The largest difference in variance explained between research designs was mono-method vs. multi-method, which was only about one-fifth their pooled standard deviation. This indicates substantial overlap in the variance explained by strong and weak research designs, and this result alone casts doubt upon the validity of Harman's single-factor test. Furthermore, the variance explained was significantly influenced by the extraneous study aspects of sample size, number of retained factors, field of study, and tier of outlet. The amount of variance explained by extraneous aspects ($R^2 = .20$) was substantially larger than the amount of variance explained by the research design aspects ($R^2 = .00$). When accounting for these extraneous aspects in our final regression analysis, the research design aspects no longer significantly related to the variance explained. Therefore, Harman's single-factor test cannot differentiate between research designs that produce more or less CMB, and it is instead more reflective of extraneous study aspects than those known to influence CMB. Our results indicate that Harman's single-factor test is ineffective, and the approach cannot reliably assess CMB.

It should therefore be questioned why Harman's single-factor test was ineffective in the present study, despite its application by hundreds of researchers in our systematic literature review. The present results cannot provide a definitive answer, but suggestions can be provided. CMB can inflate or deflate relations between indicators, and it does not impact these relations equally (Conway & Lance, 2010; MacKenzie & Podsakoff, 2012; Min et al., 2016). Harman's single-factor test only assesses whether CMB strengthens relations among all measured indicators, which is represented by the variance explained by the first factor. Harman's single-factor test is ineffective when CMB only strengthens relations among a few measured indicators, and Harman's single-factor test is even less likely to identify CMB when it attenuates relations. Harman's single-factor test may have been ineffective at distinguishing between research designs because CMB may function differently than assumed by the rationale of the test.

Alternatively, the studied extraneous aspects had larger effects on the variance explained by the first factor than aspects of research design, suggesting that Harman's single-factor test is more influenced by "noise" than CMB. Harman's single-factor test may assess CMB to some relatively minor extent, but this influence is masked by the significantly larger effects of extraneous influences – including those not tested in the current article. CMB may function as assumed by Harman's single-factor test, but the approach does not effectively distinguish between variance in the first factor caused by CMB and variance caused by extraneous aspects.

It is also possible that stronger research designs are unable to reduce CMB, causing Harman's single-factor test to produce similar results between stronger and weaker research designs. This possibility is very unlikely, however, as decades of research on multitrait-multimethod matrices (Campbell & Fiske, 1959; Conway, 1998) has strongly supported the association of CMB with research design, and it is much more likely that Harman's single-factor test is unable to accurately assess CMB than CMB not existing at all. This possibility should not be considered as a reasonable explanation for our observed results, and instead Harman's single-factor test should be recognized as unable to effectively assess CMB.

Implications and future research directions

Our results provide several implications for research and practice. Our results align with prior investigations on the inability of Harman's single-factor test to accurately assess CMB (Aguirre-Urreta & Hu, 2019; Fuller et al., 2016; Schwarz et al., 2017), and we echo Howard and Henderson's (2023) moratorium on Harman's single-factor test. Researchers should no longer apply the approach until further investigations can identify appropriate modifications to produce accurate results. Aguirre-Urreta and Hu (2019) found that Harman's single-factor test is inaccurate whether factor analysis or principal component analysis is utilized, but researchers are continuously developing new factor analytic methods (Howard & Henderson, 2023; Scharf & Nestler, 2019; Zhang et al., 2019). It cannot be necessarily assumed that these new methods would likewise produce inaccurate results with Harman's single-factor test, and researchers should monitor the utility of these new methods for assessing CMB; however, other approaches discussed below have proven to be more effective at assessing CMB, and it may be more fruitful to investigate these other approaches than studying variations of Harman's single-factor test.

Hundreds of researchers have applied Harman's single-factor test to support their research designs, many of which are now known to have claimed support for designs that may have produced significant CMB. Researchers should reassess prior results that may have been particularly influenced by CMB, as these studies may have drawn inappropriate inferences. Supplemental Material B includes our systematic review database, which may aid this endeavour. Researchers can use our database to identify studies that applied cross-sectional, single-source, and mono-method designs, and they could replicate these findings using stronger designs. Many previously supported (unsupported) relations may be unsupported (supported) when utilizing stronger research designs that can account for CMB, and our understanding of associated theory can be significantly improved by reinvestigating these studies utilizing weak research design.

Additionally, procedural remedies are more effective than post hoc remedies of CMB (Conway, 1998; Cooper et al., 2020; Kock et al., 2021). Researchers should first and foremost ensure the application of appropriate research designs to minimize CMB. From our systematic literature review database, however, authors still frequently apply weak research designs. Reviewers and editors should hold authors to recommended standards of the social sciences, and researchers should strive to apply more robust methodological designs. Once studies have been conducted to assess the impact of specific methodological designs on CMB, researchers must then make appropriate tradeoffs between methodological rigour and resource availability.

Researchers should similarly conduct investigations to better understand the influence of these research designs on CMB. For decades, researchers have studied multitrait-multimethod matrices (Campbell & Fiske, 1959; Conway, 1998) to understand the relation of research design and CMB. While interest in these studies has declined over the years, some renewed interest can be seen for other types of studies investigating the effect of research design (Johnson et al., 2011; Min et al., 2016; Sharma et al., 2009). Min et al. (2016), for instance, found that a time lag of one day only weakly addresses CMB, and they call for researchers to assess the efficacy of longer timespans. We make a similar call for future research to investigate the effects of various research designs. While it is often assumed that multi-wave, multi-source, and multi-method research designs provide sizable benefits, it is indeed possible that benefits are only seen for certain types of these designs, such as those with longer temporal separation.

How to address common method bias?

Our results found that hundreds of authors have applied Harman's single-factor test in published articles, which suggests that these authors are sensitive to the detriments of CMB but still not utilizing ideal approaches to rectify these detriments. For this reason, we discuss how future researchers should address CMB in their studies, and we provide key references for additional reading. Our recommendations are summarized in Table 3.

Table 3. Recommended approaches for addressing common method bias.

Approach	Recommendation	Key References
Methodological Approaches		
Procedural Strategies	Aspects of study design are known to reduce common method bias, such as improving item clarity and ensuring that responses will be anonymous and confidential. Readers should refer to the key references to identify which procedural strategies are necessary.	Chang et al. (2020); Cooper et al. (2020); Jordan and Troth (2020); Kock et al. (2021)
Temporal Separation	Researchers should utilize theory to identify the timespan in which their studied effects unfold, and they should include time lags in their measurement occasions to properly assess these unfolding relations across time.	Hill et al. (2021); Johnson et al. (2011); Min et al. (2016)
Multiple Sources	Researchers should utilize theory to identify the appropriate source to report their variables of interest, and they should obtain measurements from these sources.	Ellingson and Tirol-Carmody (2022); Spector et al. (2019)
Multiple Methods	Researchers should utilize theory to identify the best measurement approach for their variables of interest, and they should use varied methods (when possible) to measure these variables of interest.	Eid et al. (2023); Geiser and Simmons (2021); Helm (2022)
Measured Approaches		
Measurement of Marker Variables	Measuring variables believed to be unrelated to the constructs of interest is presently considered an effective approach for identifying and reducing the influence of common method bias.	Miller and Simmering (2023); Steenkamp and Maydeu-Olivares (2021)
Measurement of Common-Method Bias Sources	Measuring variables believed to be the cause of common method bias is presently considered an effective approach for identifying and reducing its influence.	Baumgartner and Weijters (2021); Podsakoff et al. (2024)
Unmeasured Approaches		
Harman's Single-Factor Test	Do not apply until future researchers identify specific analytical approaches and cut-offs to provide accurate results, as the current article and others have shown Harman's single-factor test to be an inaccurate assessment of common method bias.	Current Article; Aguirre-Urreta and Hu (2019); Fuller et al. (2016); Howard (2023)
Unmeasured Latent Method Construct	This approach is presently not considered particularly accurate or robust, and it should only be applied in conjunction with methodological and measured approaches; however, researchers should monitor research developments, as authors have provided initial supportive evidence for certain unmeasured latent method construct approaches.	Baumgartner et al. (2021); Ding et al. (2023); Kam and Sun (2022)

The treatment of CMB follows the adage, "an ounce of prevention is worth a pound of the cure." Researchers should utilize research designs known to reduce CMB, which includes the three studied in the current article: multi-wave, multi-source, and multi-method designs (Eid et al., 2023; Ellingson & Tirol-Carmody, 2022; Hill et al., 2021). However, more subtle aspects of research design can also reduce CMB (Podsakoff et al., 2024). Perhaps the most often recommended include utilizing different scale points (e.g., 1 to 5 vs. 1 to 9), utilizing different response anchors (e.g., Disagree to Agree vs. Never to Always), randomizing item order, clarifying item wording, obscuring the study purpose, and ensuring participant anonymity and confidentiality (Chang et al., 2023; Cooper et al., 2020; Jordan & Troth, 2020; Kock et al., 2021). If measurements are obtained by raters or observers (rather than self-report), it is also necessary to provide clarification and/or training to avoid common biases (e.g., halo effect) (Podsakoff et al., 2024). Researchers should both include and report these aspects of study design, such that readers can be assured that CMB was minimized in the study's findings.

Approaches for assessing CMB can be categorized as unmeasured and measured procedures (Aguirre-Urreta & Hu, 2019; Kock et al., 2021; Min et al., 2016). Unmeasured procedures are analysis-focused approaches that are independent of research designs, and this category includes Harman's single-factor test. These procedures are appealing because they do not require considerations before collecting data, and researchers instead only perform the analyses afterwards. Research, however, has found mixed support for unmeasured procedures (Chin et al., 2012; Min et al., 2016; Richardson et al., 2009). Aside from Harman's single-factor test, the unmeasured latent method construct approach is likely the second most common unmeasured procedure, and authors have provided evidence that it is relatively poor at detecting CMB (Chin et al., 2012; Richardson et al., 2009). Despite discouraging evidence, unmeasured approaches remain popular due to their ease of application (Baumgartner & Weijters, 2021; Kock et al., 2021). Methodologists should strive to identify accurate unmeasured approaches due to the clear need in research, such as Ding et al. (2023) who provided initial evidence that their analysis can provide superior results than prior unmeasured approaches. Researchers more broadly should monitor these developments, as future methodologists may provide robust evidence that certain unmeasured approaches can indeed provide accurate results.

Alternatively, measured procedures involve both research design and analyses, as they require that certain variables are measured during data collection to be subsequently included in analyses. The two primary measured procedures include the measurement of marker variables and CMB sources (Richardson et al., 2009; Spector et al., 2019; Williams & McGonagle, 2016). The measurement of marker variables involves measuring variables unrelated to the constructs of interest, and then controlling for these variables in analyses. Because these variables should be unrelated to the constructs of interest, any shared variance is believed to be due to CMB, and CMB should be partialled out when controlling for these unrelated variables. On the other hand, the measurement of CMB sources involves measuring variables believed to be the cause of CMB, such as mood or social desirability, and again controlling for these measures in analyses. Because these sources are associated with CMB, any shared variance is again believed to be due to CMB, and CMB should be partialled out when controlling for these variables. Research has provided more support for measured procedures, particularly those that are able to model shared variance in more complex manners (e.g., via confirmatory factor analysis), and authors have particularly recommended the measurement of marker variables because it requires only one additional variable in analyses (rather than many variables representing different source effects) (Richardson et al., 2009; Spector et al., 2019; Williams & McGonagle, 2016). Researchers should apply both approaches, and methodologists should perform more analyses on these approaches; however, particular attention should presently be given to the measurement of marker variables due to their promise and popularity in extant investigations on CMB.

Limitations

Scope of searches

We believe that our broad search of management outlets benefited our analyses, as including multiple subfields enabled the current article to assess differences in the amount of variance explained by research domains. Perhaps even more important, including multiple subfields of study helped ensure that a wider range of methodological designs were included in our database, as subfields of studies have different beliefs regarding the importance of strong research designs. As seen in our results, our systematic literature review database was indeed able to include a large number of studies representing each research design aspect. Despite these benefits, different fields of study could have been analysed in the current article, and future researchers should replicate the current results using new systematic literature review databases.

Similarly, the current article included management outlets ranked 4*, 4 and 3 from the AJG list. We chose these tiers because they represent outlets most directly relevant to business research. Likewise, higher-ranked journals are known to have a disproportionate influence on the field of management, and observing and correcting problematic approaches in these outlets could help ensure that the present manuscript provides broad contributions to management research. It cannot be guaranteed, though, that our results generalize to lower-ranked outlets, namely those from tiers 2 and 1 of the AJG list. For this reason, researchers should consider replicating the current results in these alternative outlets to ensure that both practices are similar to the studied outlets and Harman's single-factor test similarly performs faultily.

Variations of Harman's single-factor test

Many variations of Harman's single-factor test can be applied, including differences in the factor extraction and rotation method. Too few authors specified these characteristics to code in our analyses, leaving the current article unable to test their impact on assessing CMB. We believe, however, that including these aspects would not significantly alter our interpretations. Prior simulations and statistical discussions have supported that Harman's single-factor test provides inaccurate results regardless of variations in the analysis (Aguirre-Urreta & Hu, 2019; Baumgartner & Weijters, 2021; Baumgartner et al., 2021), and variations in the approach would not be expected to produce significantly different results. Nevertheless, our results should be considered with this caveat in mind, and researchers should consider further investigations into whether variations of Harman's single-factor test can produce accurate results.

Likewise, the current article did not assess the validity of cut-offs for Harman's single-factor test. Our results showed, however, that the variance explained by the first factor is more reliant on extraneous aspects than study design aspects, and thereby the likelihood of any source passing or failing the cut-offs is also more determined by extraneous aspects than study design aspects. This immediately invalidates the use of Harman's single-factor test regardless of the applied cut-off. It should nevertheless be highlighted that the entire systematic literature review database only included thirteen Harman's single-factor tests that failed the 50% cut-off. In these thirteen cases, only one author considered CMB to be concerning. These findings are similar to two recent reviews of EFA in management (Howard, 2023) and tourism and hospitality (Howard & Henderson, 2023). Thus, although the current article did not directly discuss cut-offs, our results indicate that Harman's single-factor test is ineffective regardless of the applied cut-off.

Methodological and statistical considerations

Coding processes are never perfect. While most aspects could be directly recorded from articles, the coding of indicators was sometimes ambiguous. Authors rarely specified if they included control variables in their Harman's single-factor test, and they sometimes did not state how many items were retained in administering abbreviated versions of measures. Researchers also did not always state the amount of time between measurement occasions for our sensitivity analyses, such as by only specifying that months separated the administration of surveys. The coders made their best estimations when coding these aspects, but assumptions sometimes had to be made to include all relevant articles in our database. To alleviate this potential concern, we conducted sensitivity analyses for potentially impactful coding and analytical decisions, and the results from these sensitivity analyses replicated our primary results. In doing so, our sensitivity analyses provide significant support for the robustness of our interpretations, as they suggest that our observations arose from substantive effects rather than coding or analytical decisions.

We provided analyses that tested each studied aspect in both isolation and together. Readers interested in the frequency of Harman's single-factor test across study design aspects, subfields of management, and their interaction can refer to Supplemental Material C. Our analyses assessing the effects of each aspect together, namely our final regression analysis, provided the most accurate depiction of our results, as it accounts for dependencies across our studied aspects. Nevertheless, we recommend that future researchers should reanalyse our results with alternative analyses using Supplemental Material B. While our supplemental analyses (i.e., equivalence tests) did not produce differing inferences (supporting the robustness of our findings), we recognize that alternative analyses may produce novel and differing insights.

Alternative explanations

We recognize possible alternative explanations for our results. It could be argued that the amount of variance explained by the first factor of Harman's single-factor test did not differ between strong and weak research designs because published studies using weak designs may have been those that coincidentally produced smaller CMB. We believe that this possibility is relatively unlikely, as the first factor variance explained still related to the extraneous aspects. If CMB was minimized in published studies, it would be expected that the first factor variance (reflective of common variance) would produce attenuated relations with all predictors – not only the study design aspects. Further, the journal ranking tier explained modest variance in the Harman's single-factor test results, and the average results of the highest ranked outlets (AJG 4*) and the lowest ranked outlets (AJG 3) were similar, producing a difference of only about four percent in variance explained by the first factor. If journals solely published weak designs when they produced smaller CMB, it would be expected that higher-tier outlets would be noticeably more restrictive regarding this criterion. Because there was not enough difference between the tiers to alter interpretations of Harman's single factor test, our results suggest that this is not the case. Supplemental Material D also provides a reanalysis of our results when separated by the journal ranking tier. The results largely did not differ by tier, alleviating this potential concern.

To further address this possible alternative explanation, we reconducted our analyses with an alternative set of 100 Harman's single-factor tests from predatory outlets included on Beall's list (2024). Because these outlets have almost no peer review process, it would be expected that potential publication biases would not be evident in these outlets. If our primary results differed from these supplemental results, it would suggest that our primary results were indeed influenced by publication biases; however, if the two sets of results are consistent, then it would suggest that our primary results were not notably influenced by publication biases. The results of these analyses are provided in Supplemental Material F. Our primary results and these supplemental results were remarkably consistent, suggesting that our primary results did not arise solely due to influences of publication biases. The inability of Harman's single-factor test to distinguish research designs is due to the limitations of the analysis rather than biases with our investigation.

It could also be argued that published studies with subpar designs may have been those that produced smaller amounts of variance explained by the first factor, irrespective to any actual CMB. This possibility would also be problematic for Harman's single-factor test. That is, the analysis would have sizable enough variation in its results independent of CMB that researchers could conduct multiple studies with subpar methodological designs and choose to publish the study with the most appealing Harman's single-factor test results. This is possible given that the extraneous study aspects had stronger relations with variance explained by the first factor, and researchers could choose to manipulate these aspects in (re)conducting their subpar studies to obtain publishable results. Therefore, while alternative explanations are possible, we do not believe they encourage applications of Harman's single-factor test.

Conclusion

The current article demonstrated that Harman's single-factor test cannot differentiate between research designs that produce more or less CMB, and the approach more strongly relates to extraneous aspects of studies. These results strongly suggest that researchers should no longer apply Harman's single-factor test. Instead, we recommend that researchers devote greater attention to procedural remedies, and they should likewise apply measured variable techniques to address CMB. Via these changes, researchers can obtain clarity regarding theory that cannot be achieved via Harman's single-factor test. Therefore, future researchers should utilize approaches that are more accurate in assessing CMB rather than those that are only easy to apply.

Notes

1. The possibility of CMB attenuating relations is less often considered in management research and merits discussion. While many arguments could be provided as to why CMB may attenuate relations, we highlight three. First, researchers have argued that CMB does not produce reliable or consistent effects, and its effects can instead produce positive or negative influences on relations (Campbell & O'Connell, 1967, 1982; Podsakoff et al., 2024). Second, certain measurement approaches may negatively impact the validity and reliability of applied measures, which could cause resultant CMB to attenuate observed relations (Min et al., 2016). Third, studies have supported that CMB can deflate the magnitude of interaction and nonlinear effects even while inflating the magnitude of direct effects (Evans, 1985; Siemsen et al., 2010). While strengthening relations is the most common consideration regarding CMB in management research, it is nevertheless possible that CMB may also attenuate relations.
2. It is recognized that components in principal component analysis (PCA) represent the total variance of indicators, which is a combination of common and unique variance. For a single component to represent more than 50% of variance in all indicators, however, it must represent a substantial portion of common variance, as a single factor could not explain this much unique variance in all indicators. For this reason, our arguments still hold for PCA, but we only refer to EFA throughout the primary text for the conciseness of language.
3. Other authors have recommended different cut-offs for identifying outliers and influential cases. Supplemental Material E includes a renalaysis of our results when using z-score cut-offs of three and 2.24. All interpretations were consistent between these sensitivity analyses and our primary analyses, indicating that our results were not driven by our z-score cut-off. Therefore, these alternative results support the robustness of our findings.
4. We also performed equivalence analyses (Lakens et al., 2018). We chose liberal lower and upper bound cut-offs of – 5 and 5, respectively, indicating that our result would be statistically significant if the variance explained by the first factor differed between the two groups for each study design aspect (e.g., single- vs. multi-source) by more than a raw score of 5. For each study design aspect, the equivalence test produced statistically significant effects for both boundary cut-offs, indicating that the two groups for each study design aspect were found to be equivalent regarding the average variance explained by the first factor. This finding supports the robustness of our results.
5. The calculation of z-scores to identify outliers in the number of days between measurement occasions did not include these zeros, as doing so would cause most multi-wave studies to be considered outliers. Three studies produced z-scores above four regarding the number of days between measurement occasions, and they were therefore rescaled to the largest value that produced a z-score below four.

Disclosure statement

No potential conflict of interest was reported by the author(s).

Data availability statement

The authors confirm that the data supporting the findings of this study are available within the article supplementary materials.

References

Aguirre-Urreta, M. I., & Hu, J. (2019). Detecting common method bias: Performance of the Harman's single-factor test. *ACM SIGMIS Database: The DATABASE for Advances in Information Systems*, *50*(2), 45–70. https://doi.org/10.1145/3330472.3330477

Alkharusi, H. (2012). Categorical variables in regression analysis: A comparison of dummy and effect coding. *International Journal of Education*, *4*(2), 202. https://doi.org/10.5296/ije.v4i2.1962

Avolio, B. J., Yammarino, F. J., & Bass, B. M. (1991). Identifying common methods variance with data collected from a single source: An unresolved sticky issue. *Journal of Management*, *17*(3), 571–587. https://doi.org/10.1177/014920639101700303

Banalieva, E. R., Karam, C. M., Ralston, D. A., Elenkov, D., Naoumova, I., Dabic, M. . . . Wallace, A. (2017). Communist footprint and subordinate influence behavior in post-communist transition economies. *Journal of World Business*, *52*(2), 209–229. https://doi.org/10.1016/j.jwb.2016.12.002

Baumgartner, H., & Weijters, B. (2021). Dealing with common method variance in international marketing research. *Journal of International Marketing*, *29*(3), 7–22. https://doi.org/10.1177/1069031X21995871

Baumgartner, H., Weijters, B., & Pieters, R. (2021). The biasing effect of common method variance: Some clarifications. *Journal of the Academy of Marketing Science*, *49*(2), 221–235. https://doi.org/10.1007/s11747-020-00766-8

Beall's List. (2024). *Beall's list of potential predatory journals and publishers*. https://beallslist.net/

Becker, R. (2021). Have you ever seen the rain? The causal impact of the weather situation and the season on survey participation in a multi-wave panel study. *Survey Research Methods*, *15*(1), 27–41.

Bozionelos, N., & Simmering, M. J. (2022). Methodological threat or myth? Evaluating the current state of evidence on common method variance in human resource management research. *Human Resource Management Journal*, *32*(1), 194–215. https://doi.org/10.1111/1748-8583.12398

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*(3), 407–420. https://doi.org/10.1177/1094428109360993

CABS. (2021). *Academic journal guide 2018*. https://charteredabs.org/academic-journal-guide-2021/

Campbell, D. T., & Fiske, D. W. (1959). Convergent and discriminant validation by the multitrait-multimethod matrix. *Psychological Bulletin*, *56*(2), 81. https://doi.org/10.1037/h0046016

Campbell, D. T., & O'Connell, E. J. (1967). Methods factors in multitrait-multimethod matrices: Multiplicative rather than additive? *Multivariate Behavioral Research*, *2*(4), 409–426. https://doi.org/10.1207/s15327906mbr0204_1

Campbell, D. T., & O'Connell, E. J. (1982). *Methods as diluting trait relationships rather than adding irrelevant systematic variance*. New Directions for Methodology of Social & Behavioral Science.

Chakraborty, B., Collins, L. M., Strecher, V. J., & Murphy, S. A. (2009). Developing multicomponent interventions using fractional factorial designs. *Statistics in Medicine*, *28*(21), 2687–2708. https://doi.org/10.1002/sim.3643

Chang, S. J., Witteloostuijn, A. V., & Eden, L. (2020). Common method variance in international business research. In L. Eden, B. B. Nielsen & A. Verbeke (Eds.), *Research methods in international business* (pp. 385–398). Palgrave Macmillan.

Chin, W. W., Thatcher, J. B., & Wright, R. T. (2012). Assessing common method bias: Problems with the ULMC technique. *MIS Quarterly*, *36*(3), 1003–1019. https://doi.org/10.2307/41703491

Cohen, J. (1992). A power primer. *Psychological Bulletin*, *112*(1), 155–159. https://doi.org/10.1037/0033-2909.112.1.155

Conway, J. M. (1998). Understanding method variance in multitrait-multirater performance appraisal matrices: Examples using general impressions and interpersonal affect as measured method factors. *Human Performance*, *11*(1), 29–55. https://doi.org/10.1207/s15327043hup1101_2

Conway, J. M., & Lance, C. E. (2010). What reviewers should expect from authors regarding common method bias in organizational research. *Journal of Business & Psychology*, *25*(3), 325–334. https://doi.org/10.1007/s10869-010-9181-6

Cooper, B., Eva, N., Fazlelahi, F. Z., Newman, A., Lee, A., & Obschonka, M. (2020). Addressing common method variance and endogeneity in vocational behavior research: A review of the literature and suggestions for future research. *Journal of Vocational Behavior*, *121*, 103472. https://doi.org/10.1016/j.jvb.2020.103472

Cote, J. A., & Buckley, M. R. (1987). Estimating trait, method, and error variance: Generalizing across 70 construct validation studies. *Journal of Marketing Research*, *24*(3), 315–318. https://doi.org/10.1177/002224378702400308

Delacre, M., Lakens, D., & Leys, C. (2017). Why psychologists should by default use Welch's t-test instead of Student's t-test. *International Review of Social Psychology*, *30*(1), 92–101. https://doi.org/10.5334/irsp.82

Delmas, M. A., & Toffel, M. W. (2008). Organizational responses to environmental demands: Opening the black box. *Strategic Management Journal*, *29*(10), 1027–1055. https://doi.org/10.1002/smj.701

Ding, C. G., Chen, C. F., & Jane, T. D. (2023). Improving the performance of the unmeasured latent method construct technique in common method variance detection and correction. *Journal of Organizational Behavior*, *44*(3), 519–542. https://doi.org/10.1002/job.2673

Doty, D. H., & Glick, W. H. (1998). Common methods bias: Does common methods variance really bias results? *Organizational Research Methods*, *1*(4), 374–406. https://doi.org/10.1177/109442819814002

Dudycha, A. L., & Carpenter, J. B. (1973). Effects of item format on item discrimination and difficulty. *Journal of Applied Psychology*, *58*(1), 116. https://doi.org/10.1037/h0035197

Eid, M., Koch, T., & Geiser, C. (2023). Multitrait–multimethod models. In R. H. Hoyle (Ed.), *Handbook of Structural Equation Modeling* (2nd ed, pp. 349–266). Guilford Press.

Ellingson, J. E., & Tirol-Carmody, K. B. (2022). Unlocking the potential of Other-ratings for human resource management research. In M. R. Buckley, A. R. Wheeler, J. E. Baur & J. R. B. Halbesleben (Eds.), *Research in personnel and human resources management* (Vol. 40, pp. 1–41). Emerald Publishing Limited.

Evans, M. G. (1985). A monte carlo study of the effects of correlated method variance in moderated multiple regression analysis. *Organizational Behavior and Human Decision Processes*, *36*(3), 305–323. https://doi.org/10.1016/0749-5978(85)90002-0

Fainshmidt, S., Wenger, L., Pezeshkan, A., & Mallon, M. R. (2019). When do dynamic capabilities lead to competitive advantage? The importance of strategic fit. *Journal of Management Studies*, *56*(4), 758–787. https://doi.org/10.1111/joms.12415

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*(8), 3192–3198. https://doi.org/10.1016/j.jbusres.2015.12.008

Gaskin, C. J., & Happell, B. (2014). On exploratory factor analysis: A review of recent evidence, an assessment of current practice, and recommendations for future use. *International Journal of Nursing Studies*, *51*(3), 511–521. https://doi.org/10.1016/j.ijnurstu.2013.10.005

Geiser, C., & Simmons, T. G. (2021). Do method effects generalize across traits (and what if they don't)? *Journal of Personality*, *89*(3), 382–401. https://doi.org/10.1111/jopy.12625

Gisev, N., Bell, J. S., & Chen, T. F. (2013). Interrater agreement and interrater reliability: Key concepts, approaches, and applications. *Research in Social & Administrative Pharmacy*, *9*(3), 330–338. https://doi.org/10.1016/j.sapharm.2012.04.004

Gunst, R. F., & Mason, R. L. (2009). Fractional factorial design. *Wiley Interdisciplinary Reviews Computational Statistics*, *1*(2), 234–244. https://doi.org/10.1002/wics.27

Hair, J. F., Black, W. C., Babin, B. J., & Anderson, R. E. (2019). *Multivariate data analysis*. Hampshire. Cengage Learning EMEA.

Hair, J. F., Jr., Howard, M. C., & Nitzl, C. (2020). Assessing measurement model quality in PLS-SEM using confirmatory composite analysis. *Journal of Business Research*, *109*, 101–110. https://doi.org/10.1016/j.jbusres.2019.11.069

Helm, J. L. (Ed.). (2022). *Advanced multitrait-multimethod analyses for behavioral and social sciences*. Routledge.

Hill, A. D., Johnson, S. G., Greco, L. M., O'Boyle, E. H., & Walter, S. L. (2021). Endogeneity: A review and agenda for the methodology-practice divide affecting micro and macro research. *Journal of Management*, *47*(1), 105–143. https://doi.org/10.1177/0149206320960533

Howard, M. C. (2016). A review of exploratory factor analysis decisions and overview of current practices: What we are doing and how can we improve? *International Journal of Human-Computer Interaction*, *32*(1), 51–62. https://doi.org/10.1080/10447318.2015.1087664

Howard, M. C. (2023). A systematic literature review of exploratory factor analyses in management. *Journal of Business Research*, *164*, 113969. https://doi.org/10.1016/j.jbusres.2023.113969

Howard, M. C., & Henderson, J. (2023). A review of exploratory factor analysis in tourism and hospitality research: Identifying current practices and avenues for improvement. *Journal of Business Research*, *154*, 113328. https://doi.org/10.1016/j.jbusres.2022.113328

Johnson, R. E., Rosen, C. C., & Djurdjevic, E. (2011). Assessing the impact of common method variance on higher order multidimensional constructs. *Journal of Applied Psychology*, *96*(4), 744. https://doi.org/10.1037/a0021504

Jordan, P. J., & Troth, A. C. (2020). Common method bias in applied settings: The dilemma of researching in organizations. *Australian Journal of Management*, *45*(1), 3–14. https://doi.org/10.1177/0312896219871976

Kam, C. C. S., & Sun, S. (2022). Method factor due to the use of reverse-keyed items: Is it simply a response style artifact? *Current Psychology*, *41*(3), 1204–1212. https://doi.org/10.1007/s12144-020-00645-z

Kock, F., Berbekova, A., & Assaf, A. G. (2021). Understanding and managing the threat of common method bias: Detection, prevention and control. *Tourism Management*, *86*, 104330. https://doi.org/10.1016/j.tourman.2021.104330

Koo, T. K., & Li, M. Y. (2016). A guideline of selecting and reporting intraclass correlation coefficients for reliability research. *Journal of Chiropractic Medicine*, *15*(2), 155–163. https://doi.org/10.1016/j.jcm.2016.02.012

Lakens, D., Scheel, A. M., & Isager, P. M. (2018). Equivalence testing for psychological research: A tutorial. *Advances in Methods and Practices in Psychological Science*, *1*(2), 259–269. https://doi.org/10.1177/2515245918770963

Luk, C. L., Yau, O. H., Sin, L. Y., Tse, A. C., Chow, R. P., & Lee, J. S. (2008). The effects of social capital and organizational innovativeness in different institutional contexts. *Journal of International Business Studies*, *39*(4), 589–612. https://doi.org/10.1057/palgrave.jibs.8400373

MacKenzie, S. B., & Podsakoff, P. M. (2012). Common method bias in marketing: Causes, mechanisms, and procedural remedies. *Journal of Retailing*, *88*(4), 542–555. https://doi.org/10.1016/j.jretai.2012.08.001

Miller, B. K., & Simmering, M. J. (2023). Attitude toward the color blue: An ideal marker variable. *Organizational Research Methods*, *26*(3), 409–440. https://doi.org/10.1177/10944281221075361

Min, H., Park, J., & Kim, H. J. (2016). Common method bias in hospitality research: A critical review of literature and an empirical study. *International Journal of Hospitality Management*, *56*, 126–135. https://doi.org/10.1016/j.ijhm.2016.04.010

Odiase, J. I., & Ogbonmwan, S. M. (2005). JMASM20: Exact permutation critical values for the kruskal-wallis one-way ANOVA. *Journal of Modern Applied Statistical Methods*, *4*(2), 28. https://doi.org/10.22237/jmasm/1130804820

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*(5), 879. https://doi.org/10.1037/0021-9010.88.5.879

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*(1), 539–569. https://doi.org/10.1146/annurev-psych-120710-100452

Podsakoff, P. M., Podsakoff, N. P., Williams, L. J., Huang, C., & Yang, J. (2024). Common method bias: It's bad, It's complex, It's widespread, and It's not easy to fix. *Annual Review of Organizational Psychology & Organizational Behavior*, 11. https://doi.org/10.1146/annurev-orgpsych-110721-040030

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*(4), 762–800. https://doi.org/10.1177/1094428109332834

Rouquette, A., & Falissard, B. (2011). Sample size requirements for the internal validation of psychiatric scales. *International Journal of Methods in Psychiatric Research*, *20*(4), 235–249. https://doi.org/10.1002/mpr.352

Scharf, F., & Nestler, S. (2019). Should regularization replace simple structure rotation in exploratory factor analysis? *Structural Equation Modeling: A Multidisciplinary Journal*, *26*(4), 576–590. https://doi.org/10.1080/10705511.2018.1558060

Schwarz, A., Rizzuto, T., Carraher-Wolverton, C., Roldán, J. L., & Barrera-Barrera, R. (2017). Examining the impact and detection of the"urban legend" of common method bias. *ACM SIGMIS Database: The DATABASE for Advances in Information Systems*, *48*(1), 93–119. https://doi.org/10.1145/3051473.3051479

Sharma, R., Yetton, P., & Crawford, J. (2009). Estimating the effect of common method variance: The method—method pair technique with an illustration from TAM research. *MIS Quarterly*, *33*(3), 473–490. https://doi.org/10.2307/20650305

Siemsen, E., Roth, A., & Oliveira, P. (2010). Common method bias in regression models with linear, quadratic, and interaction effects. *Organizational Research Methods*, *13*(3), 456–476. https://doi.org/10.1177/1094428109351241

Song, J., & Qu, H. (2018). How does consumer regulatory focus impact perceived value and consumption emotions? *International Journal of Contemporary Hospitality Management*, *31*(1), 285–308. https://doi.org/10.1108/IJCHM-03-2017-0136

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*(3), 855–880. https://doi.org/10.1177/0149206316687295

Steenkamp, J. B. E., & Maydeu-Olivares, A. (2021). An updated paradigm for evaluating measurement invariance incorporating common method variance and its assessment. *Journal of the Academy of Marketing Science*, *49*(1), 5–29. https://doi.org/10.1007/s11747-020-00745-z

Viswanathan, M., & Kayande, U. (2012). Commentary on "common method bias in marketing: Causes, mechanisms, and procedural remedies". *Journal of Retailing*, *88*(4), 556–562. https://doi.org/10.1016/j.jretai.2012.10.002

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 & Psychology*, *31*(3), 339–359. https://doi.org/10.1007/s10869-015-9422-9

Wu, A. D., Zumbo, B. D., & Marshall, S. K. (2014). A method to aid in the interpretation of EFA results: An application of Pratt's measures. *International Journal of Behavioral Development*, *38*(1), 98–110. https://doi.org/10.1177/0165025413506143

Zhang, G., Hattori, M., Trichtinger, L. A., & Wang, X. (2019). Target rotation with both factor loadings and factor correlations. *Psychological Methods*, *24*(3), 390. https://doi.org/10.1037/met0000198