

Verification Report:
A critical reanalysis of Vahey et al. (2015)
“A meta-analysis of criterion effects for the Implicit Relational
Assessment Procedure (IRAP) in the clinical domain”

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Vahey et al.’s (2015) meta-analysis concluded that the Implicit Relational Assessment Procedure (IRAP) has high clinical criterion validity (meta-analytic $\bar{r} = .45$) and therefore “the potential of the IRAP as a tool for clinical assessment” (p. 64). Vahey et al.’s (2015) power analyses are frequently cited for sample size determination, especially their heuristic of $N > 37$. This article attempts to verify those results. Results were found to have very poor reproducibility at almost every stage of their analytic strategy, with errors biased towards inflating the effect size. The reported meta-analysis results were found to be mathematically implausible, and could not be reproduced despite numerous attempts. Multiple internal discrepancies were found in the effect sizes, e.g., between the forest vs. funnel plot, and the forest plot vs. the supplementary data. 23 of the 56 (41.1%) originally included effect sizes were not criterion effects that were erroneously included. Inspection of the original articles revealed 360 additional effect sizes that met inclusion criteria that should have been included. A new meta-analysis was calculated to understand the compound impact of these errors. The effect size was half the size of the original ($\bar{r} = .22$), and the power analyses recommended sample sizes nearly 10 times larger than the original ($N > 346$), which no published original study using the IRAP has met. In aggregate, this seriously undermines the credibility and utility of the original article’s conclusions and recommendations. Vahey et al. (2015) appears to need substantial correction. In particular, researchers should not rely on its results for sample size justification. A list of suggestions for error detection in meta-analyses is provided. All code and data available at osf.io/jg8td.

There is now a growing literature on post-publication scientific error detection in meta-analyses. Much of this has focused on quantifying the prevalence of issues that undermine the validity of meta-analysis results, such as errors in effect size extraction (e.g., Gøtzsche et al., 2007; Lakens et al., 2017; Maassen et al., 2020), or indicators of reproducibility such as the reproducibility of the systematic search strategy, specification of the exact method to compute effect sizes, choice of weightings and estimator function, and sharing of data and code (López-Nicolás et al., 2022). More recently, this has been supplemented with work that is explicitly focused on error detection that has the goal of examining what features of a meta-analysis can be checked and how, and where meta-analyses tend to make errors (Kadlec et al., 2023). This article continues in this vein: following the logic of error detection tools for original research articles (e.g., Heathers et al., 2018),

it focuses on features of meta-analyses that are either informative but often overlooked or which repeat information. Both provide vectors for error detection. The intended meta-scientific utility of this manuscript is therefore to provide a relatively fine-grain description of what information was inspected for errors and how, in the hope that some of these methods of verification allow other meta-analyses to be more efficiently and effectively inspected for errors.

Vahey et al.’s (2015) meta-analysis of the Implicit Relational Assessment Procedure (IRAP: Barnes-Holmes et al., 2010) concluded that it possesses good criterion validity and that the IRAP “demonstrates the potential of the IRAP as a tool for clinical assessment” (p. 64). Based on a non-systematic review, the authors (a) provided an estimate of the association between IRAP effects and clinically relevant criterion variables, (b) reported that the IRAP compares favorably to other

popular implicit measures, including the Implicit Association Test (Greenwald et al., 1998), and (c) used their meta-analyzed estimate of effect size to conduct power analyses and make sample size recommendations for future research using the IRAP. While there has been a subsequent debate about the degree to which the IRAP is or is not an “implicit” measure (Barnes-Holmes & Harte, 2022a; Hussey, 2022), and indeed what the term even means (Corneille & Hütter, 2020), these debates are secondary to the fact that the IRAP, and tasks like it, are claimed to be valid measures of individual differences based on sources of evidence such as Vahey et al. (2015).

Rationale for verification

In addition to the meta-scientific utility of doing so discussed previously, there are at least four rationales for performing a verification of Vahey et al.’s (2015) results. First, there is good a priori reason to believe that meta-analyses in general often contain non-replicable results. Lakens et al. (2017) recently demonstrated that the results of the majority of a random sample of meta-analyses published in psychology cannot be computationally reproduced, often because of differences in individual effect sizes between those reported in meta-analyses and those reproduced from the original studies. Similarly, Maassen et al. (2020) found that almost half of the individual effect sizes reported in meta-analyses of psychology research could not be reproduced from the original articles. This was attributed to a variety of issues including errors in the extraction of effect sizes from original studies, insufficient details regarding data processing and transformation of effect sizes, and insufficient details of the specific meta-analytic approach employed. Comparable errors in meta-analyses have also been reported by others (e.g., Kadlec et al., 2023; Lakens et al., 2017).

Second, Vahey et al.’s (2015) article has been well-cited and used to guide subsequent work. At the time of writing in July 2024, it has been cited 143 times on Google Scholar, with roughly 20% of articles citing it to justify sample size decisions in lieu of a power analysis for that study (e.g., Bast & Barnes-Holmes, 2015; Farrell & McHugh, 2017; Leech et al., 2018; Maloney & Barnes-Holmes, 2016; Power et al., 2017; see supplementary materials for supporting quotes from each). Studies employing the IRAP have typically involved small sample sizes of around 40 participants. This is frequently argued to be acceptable because it is in line with Vahey et al.’s (2015) sample size recommendation: “a sample size of at least $N = 37$ would be required in order to achieve a statistical power of .80 when testing a continuous first-order correlation between a clinically-focused IRAP effect and a given

criterion variable” (p. 63). Kavanagh et al. (2022, p. 528) provided a particularly clear characterization of the ongoing importance of Vahey et al.’s (2015) results for practices in the broader IRAP literature: “The general strategy for recruiting numbers of participants was guided by the results of a recent meta-analysis of IRAP effects in the clinical domain, indicating that a minimum of 29 is required to achieve a power of 0.8 for first-order correlations (Vahey et al., 2015).” Given that research continues to rely on the conclusions of Vahey et al.’s (2015) meta-analysis, and that meta-analyses in general have been shown to have poor computational reproducibility, it is therefore useful to verify Vahey et al.’s (2015) results.

Third, Vahey et al. (2015) may have been modest when they reported that their results imply the IRAP compares “favorably” with other implicit measures. In fact, Vahey et al.’s (2015) reported meta-analytic effect size of $\bar{r} = .46$ would place it in $>90^{\text{th}}$ percentile of all meta-analytic effect sizes reported in psychology (Richard et al., 2003). Given that the IRAP is a reaction time based measure, which are inherently prone to noise and therefore poor reliability (as I discuss in the next section), the original result implies that the IRAP is a truly remarkable measure to be able to correlate so highly with a range of clinical criterion measures. Or, something is amiss with Vahey et al. (2015).

Fourth, in light of estimates of the IRAP’s low reliability (Hussey & Drake, 2020), there is good reason to believe that Vahey et al.’s (2015) meta-analytic estimate of $\bar{r} = .45$ is implausibly large. According to classical test theory, a measure’s reliability refers to the proportion of the variance that is caused by the construct rather than noise (Allen & Yen, 2002, p.73). As such, reliability places a limit on the mean observable associations between scores on any two measures: the less reliably the two variables are measured, the lower the observable correlation between the two variables. The observed correlation between two measures x and y (r_{xy}^{observed}) is a function of the true correlation between the variables (r_{xy}^{true}) and the reliability of both measures (their self-correlation r_{xx} and r_{yy}). This can be quantified via the Attenuation Formula derived from classical test theory (Revelle, 2009, equation 7.3):

$$r_{xy}^{\text{true}} = \frac{r_{xy}^{\text{observed}}}{\sqrt{r_{xx}r_{yy}}} \quad (1)$$

Table 1. Verifications of power analyses for 80% power.

Test	Tails	Estimated using*	Vahey et al. (2015)		Verified	New meta-analysis	
			\bar{r}	N	N	\bar{r}	N
Pearson's r	One	Point estimate	0.45	29	29	.22	126
Pearson's r	One	Lower bound of 95% CI	0.40	37	37	.15	273
Pearson's r	Two	Point estimate	0.45	36	36	.22	160
Pearson's r	Two	Lower bound of 95% CI	0.40	-	46	.15	346
Independent t -test (Cohen's d)**	One	Point estimate	1.01	26	26	.45	124
Independent t -test (Cohen's d)**	One	Lower bound of 95% CI	0.87	36	34***	.30	270
Dependent t -test (Cohen's d)**	One	Point estimate	1.01	8	8	.45	32
Dependent t -test (Cohen's d)**	One	Lower bound of 95% CI	0.87	10	10	.30	69

Notes:

* Researchers often use the point estimate of the meta-effect size. Perugini et al. (2014) recommended the lower bound of the 95% CI instead. Vahey et al. (2015) used both for power analyses.

** Necessary conversions from d to r were not reported in Vahey et al. (2015), but are recalculated here using the effectsize R package's 'r_to_d' function.

*** Discrepancy between the result reported by Vahey et al. (2015) and the recalculated result

Two of the variables in equation 1 already have empirical estimates. First, Vahey et al.'s (2015) estimate of the observed correlation between the IRAP and criterion variables was $r_{xy}^{true} = .45$. Second, estimates of the IRAP's reliability have been provided by a recent meta-analysis. At the trial-type level (i.e., the method of scoring IRAP as four scores that proponents of the task typically recommend), both internal consistency ($\alpha = .27$) and test-retest ($ICC_2 = .18$) are extremely low (Hussey & Drake, 2020).¹ This leaves two remaining variables, the IRAP's criterion validity after adjusting for measurement error (r_{xy}^{true}) and the criterion tasks' mean reliability (r_{yy}). Both of these variables share the same constraint: as correlations, their value cannot be below -1 or above 1. For the moment, if we assume that the criterion tasks' mean reliability is very good ($r_{yy} = 0.90$). This would imply that the lower limit of the IRAP's true criterion validity after adjusting for measurement error is somewhere between (a) implausibly high, $r_{xy}^{true} = .91$ (when using the estimate of internal consistency), and (b) mathematically impossible, $r_{xy}^{true} = 1.12$ (when using the estimate of test-retest reliability). Using lower and arguably more plausible values for the mean reliability of the criterion tasks produces even higher estimates for the true correlation, making both values impossible (i.e., when $r_{yy} = .70$, $r_{xy}^{true} = 1.04$ or 1.27 respectively).

Given that these estimates range between highly implausible and impossible, something appears to be amiss. Either Vahey et al.'s (2015) estimate of average criterion associations is somewhere between highly

implausible and mathematically impossible given the IRAP's reliability (i.e., assuming Hussey & Drake 2020 are right about the IRAP's reliability), or Hussey & Drake's (2020) estimates of the IRAP's average reliability is implausibly low given the IRAP's high criterion validity (i.e., assuming Vahey et al., 2015, are right about the IRAP's criterion validity). Ultimately it will be up to the research community to determine whether our analyses in Hussey & Drake (2020) are sound, and we provided open data and code to aid others in inspecting our work for errors. Because I am confident in our results reported there, I am instead motivated to inspect Vahey et al.'s (2015) data and analyses to determine if the issue lies there instead.

Method & Results

Vahey et al. (2015) reported the steps in their analyses in the conventional order: they identified effect sizes in the original article, applied inclusion and exclusion

criteria, extracted them, converted them to Pearson's r , averaged them when multiple effect sizes came from a given study, fit a meta-analysis model, and performed a power analysis on the meta-effect size to guide sample size determination in future studies. Attempts to verify these steps for this article were conducted and reported here in reverse order. Subsequently, I report a new meta-analysis and power analysis using the re-extracted individual effect sizes.

Transparency statement

All data, code, and formulae (e.g., to convert effect sizes) to reproduce the verification and extension analyses can be found in the supplementary materials (see osf.io/jg8td). In the process of conducting this

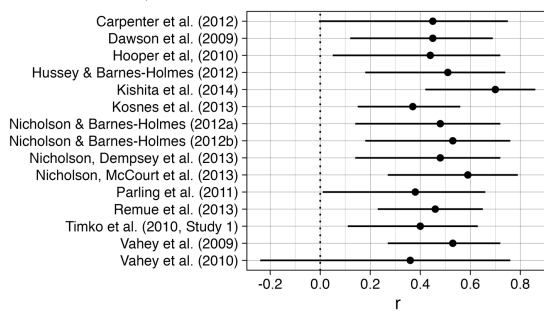
¹ Two other reviews of the IRAP's test-retest reliability have also been conducted (Golijani-Moghaddam et al., 2013: $\bar{r} = .49$; Greenwald & Lai, 2020: $\bar{r} = .45$). However, both estimated test-retest reliability from a very small number of studies ($k_s = 1$ and 2, respectively) with very small sample sizes ($N_s = 12$ and 25, respectively). Hussey & Drake's (2020) estimates, which were derived from a larger number of studies and participants ($k = 8$, $N = 318$) therefore represent the more precise estimates. In addition, Hussey & Drake (2020) employ both more appropriate methods to estimate reliability (i.e., permutation-based internal consistency rather than split-half reliability, and ICC_2 rather than Pearson's r . See Hussey & Drake (2020) for further discussion.

verification attempt, I contacted the corresponding author of Vahey et al. (2015) and requested that they share their code or further details of their analytic approach, who declined. In July 2019, I shared a copy of an earlier version of these verification attempts with the corresponding author, including code, data, and a set of slides outlining my concerns about the credibility of their findings. I have received no contact from the corresponding author since then. No corrections of Vahey et al. (2015) have been issued at the time of writing (July 2024), and to the best of my knowledge, the authors of Vahey et al. (2015) have made no public statements about these concerns about the credibility of their findings. Even five years after I initially raised these concerns, the senior author of Vahey et al. (2015) has continued to cite the article favorably as evidence for the IRAP's validity (e.g., Barnes-Holmes & Harte, 2022a, 2022b).

Power analyses

Details of the power analyses conducted by Vahey et al. (2015) were extracted. This included the meta-effect size used (i.e., using point estimate or lower bound Confidence Interval, following Perugini et al.'s recommendation, as adopted in Vahey et al. 2015), test (Pearson's r correlation, independent t -test, dependent t -test), the direction of hypothesis (one-sided vs. two-sided), and the recommended sample size (i.e., the result of the test). Verification tests were performed using the pwr R library (Champely, 2016). Table 1 contains the results of both the power analyses reported by Vahey et al. (2015) and those of the verification analyses. As can be seen in the table, Vahey et al.'s (2015) sample size recommendations were found to be computationally reproducible when their meta-analytic effect size was used, with one exception (difference $N = 36$ vs. 34).

Figure 1. Weighted-mean effect sizes and their 95% Confidence Intervals extracted from Vahey et al.'s (2015, Figure 1) forest plot.



Meta-analysis

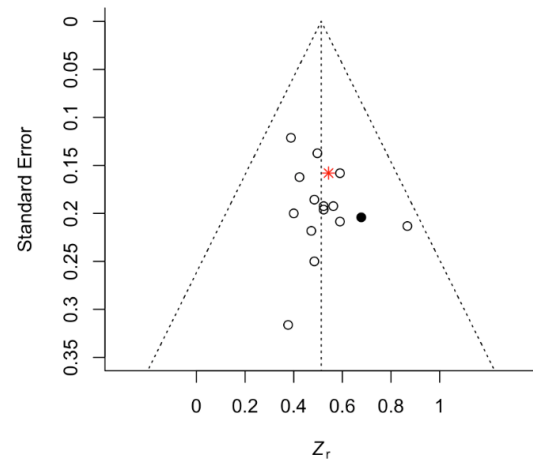
Issues with the meta-analysis results

Vahey et al. (2015) reported a meta-analytic effect size, 95% Confidence Intervals, and 95% Credibility Intervals. These were extracted from Vahey et al.'s (2015) forest plot in their Figure 1 (for \bar{r} and CR) and

the text on pages 62-63 (for the CI): $\bar{r} = .45$, 95% CI [.40, .54], 95% CR [.23, .67].

Prior to any attempt to reproduce these results, it is important to note that the point estimate and confidence intervals are not possible: the upper bound Confidence Interval is +.09 larger than the point estimate \bar{r} , whereas the lower bound Confidence Interval is -.05 smaller. While asymmetric intervals are indeed possible (e.g., when using a transformation such as Fisher's r -to- z), such transformations would create an asymmetry in the opposite direction (i.e., a smaller upper interval and larger lower interval). I know of no legitimate way to produce a meta-analytic effect size with asymmetric Confidence Intervals of the type that Vahey et al. (2015) report and they are, to the best of my knowledge, mathematically impossible. One plausible explanation is that one or more values are the result of typos. Another plausible explanation is that the Confidence Intervals (reported in text) and the \bar{r} and Credibility Intervals (reported in Figure 1) were obtained from different meta-analyses, employing different data and/or different modeling approaches.

Figure 2. Discrepancies between the data in Vahey et al.'s (2015) forest plot (their Figure 1) vs. funnel plot (their Figure 2).



Note: This plot was created from Vahey et al.'s (2015) results reported in their forest plot (their Figure 1) vs. their funnel plot (their Figure 2). The black dot refers to the location of one weighted mean effect size according to their forest plot. The red dot asterisk refers to its approximate location in their funnel plot (their Figure 2). Circles refer to data points that match between the two plots.

Issues with the original effect sizes' Confidence Intervals

Separately, it is also worth inspecting the weighted average effect sizes for each of the component studies reported in Vahey et al.'s (2015) Figure 1. These numerical values were extracted and are reproduced in Figure 1 of this manuscript, and serve as the data for the verification attempts below. The individual effect

sizes are labeled as representing weighted r values and its Confidence Intervals. As such, the confidence intervals should be symmetrical, but they are not. For example, the effect size reported for Vahey et al. (2010) is $r = .36$, 95% CI $[-.24, .76]$. This makes the upper bound $+.40$ larger than the point estimate whereas the lower bound is $-.60$ from the point estimate. The asymmetry is therefore in the opposite direction to that in the reported meta-analytic effect size's Confidence Interval and is in principle compatible with a transformation having been applied (e.g., Fisher's r -to- z). As I discuss later on in meta-analysis reproduction attempt 4, it is likely that the original figure mislabels what are weighted average Fisher's r -to- z transformed values as weighted average r values.

Data in the funnel plot does not match the forest plot

Vahey et al. (2015) also reported their weighted average effect size estimates in a funnel plot (see their Figure 2). However, when I created a funnel plot from the results reported in their forest plot, one of the data points did not match their funnel plot. See Figure 2 which illustrates this discrepancy. This suggests that the funnel plot and the forest plot were created from slightly different data sets. It is unclear which one represents the 'correct' data set (especially in light of the section on 'average effect sizes' that I discuss later), but this speaks to the broader pattern of non-reproducibility and internal inconsistencies in Vahey et al.'s (2015) results.

Implementation of the meta-analysis

Vahey et al. (2015) stated that they employed a Hunter & Schmidt style meta-analysis and cited Field & Gillett (2010). In personal correspondence with Vahey, he stated it should in principle be possible to reconstruct their analytic strategy and results from the code provided by Field & Gillett (2010), but he declined to share the actual code used in Vahey et al. (2015). In practice, it was surprisingly difficult to reconstruct what was done because of multiple discrepancies both (a) between Field & Gillett (2010) and Vahey et al. (2015) and (b) within Vahey et al. (2015) itself, as I will now discuss.

Field & Gillett (2010) describe and provide code for two different ways of conducting meta-analyses: a Hedges style "basic" meta-analysis and a Hunter and Schmidt style psychometric meta-analysis. Despite Vahey et al. (2015) stating that they applied the Hunter and Schmidt approach, multiple features of this approach are missing from their results (e.g., no correction for the reliability of the measures used to generate the correlation), and other features that are unique to the Hedges style meta-analysis were reported (e.g., calculation of Confidence Intervals, e.g., p. 63; apparent use of Fisher's r -to- z transformation, e.g., Figure 2).

In order to attempt to verify the original results, I downloaded the accompanying materials for Field & Gillett (2010) from Field's website. This contained two

different SPSS scripts, "Meta_Basic_r.sps" and "h_s syntax.sps". Confusingly, both scripts contain code to produce Hunter and Schmidt style meta-analyses, with the former also producing a Hedges style 'basic' meta-analysis.

The code in these scripts was examined in order to determine whether they (individually or jointly) were capable of calculating the various values that were reported in Vahey et al. (2015). That is, agnostic to the numerical results actually produced by running the scripts on the original weighted mean effect sizes, I first assessed whether these scripts were capable of producing (a) all of the variables that Vahey et al. (2015) reported and (b) producing them in an unambiguous way (i.e., only calculating a given variable via one method). Inspection of these two scripts showed that neither script was individually capable of computationally reproducing all four: of the forest plot, the meta-analytic point estimate, the Confidence Intervals, and the Credibility Intervals. This implies that the results reported in Vahey et al. (2015) were, at minimum, some combination of the results of the two different scripts. On the one hand, the "Meta_Basic_R.sps" script does not calculate Confidence Intervals for Hunter & Schmidt style meta-analyses. On the other hand, the "h_s syntax.sps" script does calculate Confidence Intervals on the meta-analytic effect size, but it also requires the user to provide reliability estimates for both variables in each correlation (i.e., the reliabilities r_x and r_y for the correlation r_{xy}) in order to correct the effect sizes for attenuation. Vahey et al. (2015) did not report extracting or using reliability estimates in this way in their article or supplementary materials. Finally, the script does not compute Confidence Intervals for the individual effect sizes, therefore the forest plot could not be reproduced from this script.

As such, in the absence of additional information, the two scripts provided by Field & Gillett (2010) were therefore not capable of fully and unambiguously reproducing the same variables as those reported in Vahey et al. (2015) without further alternation. In light of this, I therefore altered the implementations in multiple ways in order to attempt to reproduce Vahey et al.'s (2015) results.

Table 2. Verification attempts for the meta-analysis

Label	Implementation	Modifications from the original code	\bar{r}	95% CI		95% CR		95% PI	
				Lower	Upper	Lower	Upper	Lower	Upper
Vahey et al. (2015)	Vahey stated in personal correspondence that they employed Field & Gillett's code.	Unknown.	.45	.40	.54	.23	.67	-	-
Verification attempt 1	Hunter & Schmidt method using Field & Gillett's (2010) "h_s_syntax.sps"	All reliabilities were set to 0.	.47	.20	.74	.47	.47	-	-
Verification attempt 2	Hunter & Schmidt method using Field & Gillett's (2010) "Meta_Basic_r.sps"	Set variance in population correlations to zero if it is negative, as in "h_s_syntax.sps"	.46	-	-	.46	.46	-	-
Verification attempt 3	Hunter & Schmidt method using Viechtbauer's (2022) implementation in R and metafor.	Credibility intervals were implemented using Field & Gillett's (2010) equations 2 to 5.	.47	.40	.54	.47	.47	.40	.54
Verification attempt 4	A mix of Hunter & Schmidt and Hedges methods using Viechtbauer's (2022) implementation in R and metafor.	Credibility intervals were implemented using Field & Gillett's (2010) equations 2 to 5. Fisher's r -to- z transformations and z -to- r back transformations.	.47	.40	.54	.47	.47	.40	.54

Notes: CI = Confidence Interval. CR = Credibility Interval. PI = Prediction Interval. Although PIs were not reported in Vahey et al. (2015), where possible they were calculated in the verification attempts to see if they corresponded with the original CRs on the basis that the CRs could have been mislabelled. Cells shaded in grey match those reported in Vahey et al. (2015) within $\pm .01$.

Definitions of different types of intervals

Vahey et al. (2015) report both Confidence Intervals (CI) and Credibility Intervals (CR which attempt to estimate the generalizability of the meta-effect size (Field & Gillett, 2010; Hunter & Schmidt, 2004).

Vahey et al. (2015) state that such “Credibility Intervals are generally wider and thus more conservative than corresponding Confidence Intervals” (p.61), however, this is not the case: Confidence Intervals and Credibility Intervals have different estimands, and therefore the two have no correlation. Confidence Intervals quantify the precision of the estimate given sampling error (i.e., within-study variance, $\hat{\sigma}^2$), whereas Credibility Intervals are a function of between-study variance ($\hat{\tau}^2$: see Field & Gillett, 2010, equations 2, 3, 4, and 5). A third type of interval, Prediction Intervals (PI), take both into account and are often reported for meta-analyses (e.g., within the metafor R package). It is true that PIs are at least as wide as Confidence Intervals, however, this is not because they are more 'conservative' than Confidence Intervals but because they quantify a different property under different assumptions. It is unclear whether this discrepancy in Vahey et al. (2015) was due to (a) a misinterpretation of Credibility Intervals, or (b) whether they actually calculated PIs but mislabelled them as Credibility Intervals. In order to attempt to resolve this for the purpose of verification, it is useful to define all three to highlight the differences between them:

$$95\% \text{ CI} = \bar{r} \pm 1.96\sqrt{\hat{\sigma}^2} \quad (2)$$

$$95\% \text{ CR} = \bar{r} \pm 1.96\sqrt{\hat{\tau}^2} \quad (3)$$

$$95\% \text{ PI} = \bar{r} \pm 1.96\sqrt{\hat{\tau}^2 + \hat{\sigma}^2} \quad (4)$$

Where \bar{r} is the weighted average effect size, $\hat{\sigma}^2$ is the estimated within-study variance (i.e., the square of the Standard Error of \bar{r}), and $\hat{\tau}^2$ is the estimated between-study variance (heterogeneity).

An important point to appreciate regarding Credibility Intervals is that when between-study heterogeneity is zero ($\hat{\tau}^2 = 0$), the CR interval width will also be zero, as is the case in the results of the verification attempts reported later.

This also follows from Field & Gillett's (2010) equations 2, 3, 4, and 5 (note that they use slightly different notation), which define the variance in the estimate of population correlations as the variance of sample effect sizes (which Vahey et al. 2015 denote as s_r^2) minus the sampling error variance. As such, if the sampling error variance is found to be larger than the variance in the sample effect sizes, then $\hat{\sigma}_p^2$ will be negative, and Credibility Intervals cannot be

calculated, as the square root of a negative number is non-real. Although Field and Gillett (2010) do not discuss this possibility in their article, they cover this case in their code by setting negative values of $\hat{\sigma}_p^2$ to zero (see “h_s syntax.sps” script). In such cases, both the lower and upper bound of the Credibility Interval will equal the point estimate (i.e., $95\% \text{ CR} = \bar{r} \pm 1.96 \times 0 = [\bar{r}, \bar{r}]$). This would represent an important case in which Confidence Intervals are both wider than Credibility Intervals, contrary to Vahey et al.'s (2015) claim and indeed where the Credibility Intervals are implausibly narrow (i.e., 0).

Verification attempt 1

The first verification attempt employed Field's “h_s syntax.sps” script which implements the Hunter & Schmidt style meta-analysis that Vahey et al. (2015) cite and which Vahey stated in personal correspondence that they used. Copies of all original and altered scripts are available in the supplementary materials. The default 80% Credibility Interval widths were changed to 95%, as Vahey et al. (2015) reported using the latter width.

One other key assumption was made in order to allow the script to run. To take a step back, a Hunter & Schmidt style meta-analysis is sometimes referred to as a form of psychometric meta-analysis because it typically involves de-attenuating the effect sizes based on the reliability of the measures that produced them (Field & Gillett, 2010; Hunter & Schmidt, 2004). Although Hunter & Schmidt (2004) did describe what they referred to as a “bare-bones” meta-analysis that did not perform this deattenuation based on reliability, for Field's “h_s syntax.sps” script to run it requires the researcher to provide reliability values for both of the measures that produced each effect size. Vahey et al. (2015) do not report any extracting or estimating reliabilities or deattenuating the effect sizes based on them, and no reliability data is available in their manuscript or supplementary materials. In the absence of other information, I set the reliability for all variables to 1 in order to allow the script to run. Table 2 presents the meta-analysis effect size estimates reported by Vahey et al. (2015) as well as the results of all verification analyses conducted here.

This verification attempt did not reproduce the original results for any estimate (point estimate, Confidence Interval, or Credibility Interval). The point estimate was off by a small amount ($\bar{r} = 0.02$), but more than could be accounted for by rounding or other erroneous methods such as truncation. Confidence Intervals were nearly four times wider in the verification analysis than the original results. Credibility Intervals had zero width (i.e., because $\hat{\sigma}_p^2$ was negative, interval width was therefore set to zero), whereas they were wider than the Confidence Intervals in the original analysis. Note that setting the reliabilities to other values (e.g., the common reliability thresholds such as .70 or otherwise), would serve to inflate both the meta-

Figure 3. Forest plot for meta-analysis verification attempt 3.

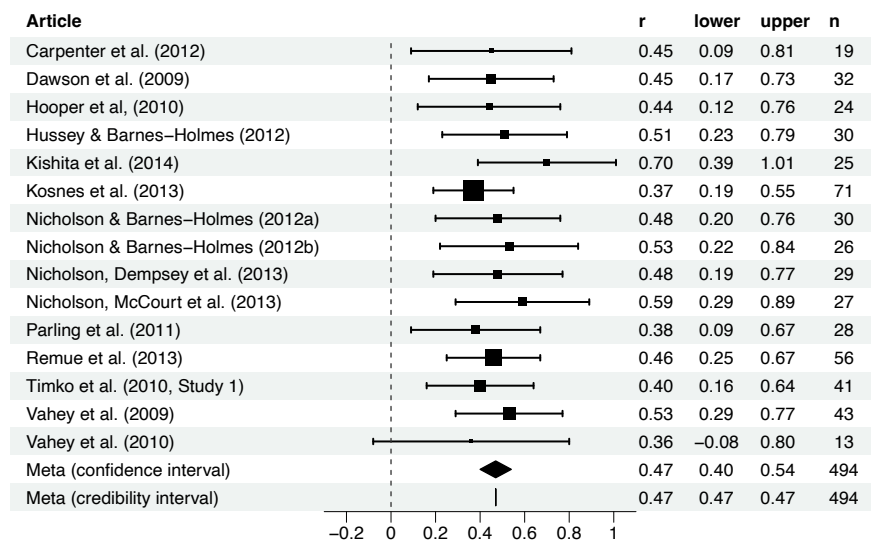
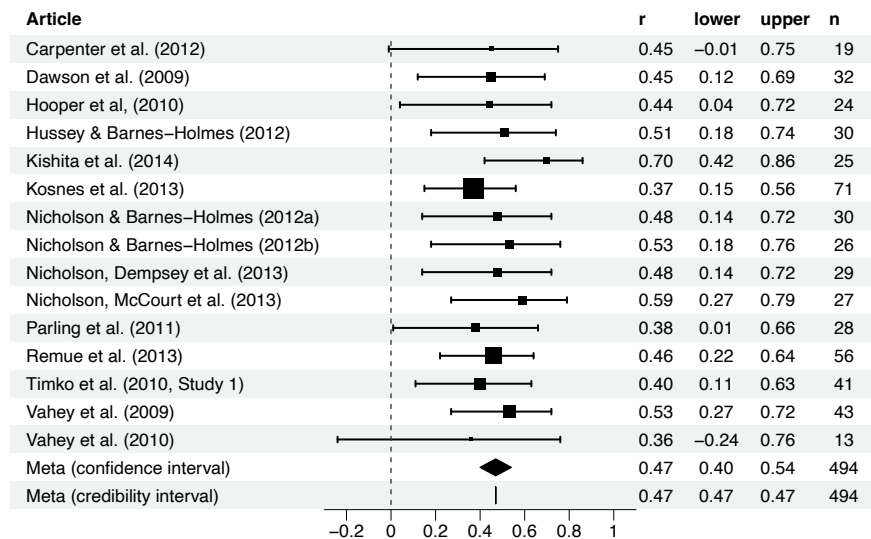


Figure 4. Forest plot for meta-analysis verification attempt 4.



analysis effect size and intervals. As such, additional verifications using alternative reliability values were not useful to explore.

Verification attempt 2

The second verification attempt employed Field's "Meta_Basic_r.sps" script, which implements a Hedges' style "basic" meta-analysis. Note that, unlike the "h_s syntax.sps" script, this script does not contain code to calculate Confidence Intervals. One necessary alteration was made to the code: if $\hat{\sigma}_p^2$ was negative it was set to zero to produce a Credibility Interval width of 0, as in the "h_s syntax.sps" script. Without this alternation, if $\hat{\sigma}_p^2$ was negative the script would fail to run.

This verification attempt also did not reproduce the original results. The point estimate was off by a small amount ($\bar{r} = 0.01$). This is more than could be

accounted for by common methods of rounding but could be obtained via (erroneous) truncation. Nonetheless, this value was the closest I could achieve across attempts and, in the grand scheme of things, is close to the reported value. However, the Confidence Intervals were nearly four times wider than the original results. In addition, the Credibility Intervals again had zero width (i.e., because $\hat{\sigma}_p^2$ was negative and interval width was therefore set to zero) and therefore greatly differed from the original results.

Verification attempt 3

In a third verification attempt, I switched from using Field and Gillett's SPSS code to Vichtbauer's (2022) implementation of a Hunter & Schmidt style meta-analysis that was written in R and the metafor package (Viechtbauer, 2010, 2024). This provided new avenues to attempt to reproduce the original results in a

programming language I was more familiar with, allowing me to try a variety of variations on a given attempt more efficiently. However, this also opened up the possibility that non-reproduction was due to implementational differences between languages. Field & Gillett's (2010) equations 2-5 were used to implement Credibility Intervals. In this attempt, the Confidence Intervals reported by Vahey et al. (2015) were reproduced. However, the point estimate and Credibility Intervals again did not reproduce the original results, and matched the results found in verification analyses 1 and 3.

This verification attempt also attempted to reproduce the original forest plot (Vahey et al., 2015, Figure 1), which was more feasible in R. It is useful to note that the original forest plot reported asymmetric Confidence Intervals around individual effect sizes. That is, the lower bounds are typically further from the point estimate than the upper bounds. This implies that some form of non-linear transformation was employed, such as a Fisher's r -to- z transformation. However, Vahey et al. (2015) do not report employing any transformations in their meta-analysis or forest plot. The forest plot associated with this verification attempt can be seen in Figure 3. Confidence Intervals around individual effect sizes were symmetric and therefore did not reproduce the original plot.

Verification attempt 4

In the fourth meta-analysis verification attempt, I applied Fisher's r -to- z transformations to the individual effect sizes prior to meta-analysis and back transformations prior to reporting and plotting. The analysis was otherwise identical to the previous attempt. All estimated values were identical to attempt 3, therefore the original meta-analysis results were not reproduced.

However, the forest plot associated with this attempt did reproduce the Confidence Intervals around the individual effect sizes from Vahey et al.'s (2015) original forest plot (see their Figure 1 and this manuscript's Figures 1 and 3), suggesting that Vahey et al. (2015) employed these transformations but did not report them. This under-reported data transformation also implies a second form of underreporting: Vahey et al. (2015) reported employing a Hunter & Schmidt style meta-analysis, but this implies that they diverged from this strategy by also applying Hedges style data transformations (in addition to not applying Hunter & Schmidt style corrections for reliability). While this reproduction of the original individual effect sizes and their Confidence Intervals gets us one step closer to understanding the original analytic strategy, it nonetheless does not reproduce the meta-analysis results.

Summary of attempts

A large number of small variations on the attempts that are reported here were also tried. For example, alternative values for reliability estimates in attempt 2, and not back-transforming the z values in attempt 4. I

also tried a number of purposeful mistakes, such as miscalculating Credibility Intervals based on plausible mathematical and coding errors. No attempt successfully reproduced the originally reported results.

Confidence Intervals around individual effect sizes in the original forest plot were only reproduced when Fisher's r -to- z transformations were applied (verification attempt 4) and not when they weren't (verification attempts 1-3). However, (a) Vahey et al. (2015) did not report applying this transformation in their meta-analysis or plot, and (b) Field & Gillett (2010) stated that this transformation is not part of the Hunter & Schmidt method of meta-analysis. This would therefore be an ad-hoc mix of the Hunter and Schmidt method and the Hedges and colleagues' method, as Field & Gillett (2010) describe them.

I noted previously in the "Issues with the meta-analysis results" section that the original meta-analysis point estimate is incompatible with the reported Confidence Intervals. Interestingly, if we assume that (a) the originally reported point estimate is incorrectly reported but the Confidence Intervals are correctly reported, and (b) that the Confidence Intervals are symmetrical, this would imply that a correct point estimate of .47 (i.e., at the halfway point between the intervals). A point estimate of .47 combined with Confidence Intervals of [.40, .54] was reproduced in verification attempts 3 and 4. However, this does not imply that the original results are merely the result of a typo in the point estimate, as (a) the Credibility Intervals in verification attempts 3 and 4 are very different from the original results, and (b) more confusingly, these results were produced only by Viechtbauer's (2022) implementation of the analysis in R and metafor, but not using the scripts that Vahey et al. (2015) report having used. Therefore, it remains unclear how Vahey et al. (2015) analyzed their data or obtained all their results.

Weighted average effect sizes

In order to attempt to retrace the steps involved in the original analysis, I then noted that Vahey et al. (2015) reported that the 15 weighted average effect sizes they used in their meta-analysis were calculated from 46 individual effect sizes and degrees of freedom taken from 15 studies. Vahey et al. (2015) reported the individual effect sizes and degrees of freedom in their supplementary materials. I therefore attempted to verify the weighted averages by recalculating them using Vahey et al.'s (2015) strategy of weighting by degrees of freedom. Results were not fully computationally reproducible: 2 of 15 (13%) recomputed weighted averages differed from those reported in Vahey et al.'s forest plot. On the one hand, the magnitudes of the differences were small ($\Delta\bar{r} = -.02$ and $.05$). On the other hand, given the simplicity of these calculations, the discrepancy is difficult to understand. Both instances with discrepancies came from articles whose first authors were co-authors of

Vahey et al. (2015), suggesting that they were not unfamiliar with the original studies.

Individual effect sizes

Next, I attempted to retrace the next step involved in the original analysis: the extraction of effect sizes from original articles. This involves (a) the correct application of inclusion criteria in terms of correct inclusions and the absence of incorrect omissions, and (b) the extraction and conversion of effect sizes.

Assessment of incorrect inclusions

Lakens et al. (2016) argued that “incorrect inclusion” is a common type of error in meta-analysis. That is, the inclusion of effect sizes that do not meet the inclusion criteria. Vahey et al. (2015) stated that the purpose of their meta-analysis was to “*quantify how much IRAP effects from clinically relevant responding co-vary with corresponding clinically relevant criterion variables*” (p.60). Their inclusion criterion was that “*the IRAP and criterion variables must have been deemed to target some aspect of a condition included in a major psychiatric diagnostic scheme such as the Diagnostic and Statistical Manual of Mental Disorders (DSM-5, 2013) ... The authors decided whether the responses measured by a given IRAP trial-type should co-vary with a specific criterion variable by consulting the relevant empirical literature.*” (p.60). Unfortunately, neither the original article nor its supplementary materials provided data for each extracted effect size regarding which specific clinical condition was targeted by the IRAP and the criterion variable, or the “specific empirical literature” that Vahey et al. (2015) used to justify the inclusion of each criterion.

Nonetheless, Vahey et al.’s (2015) own inclusion criterion required that effects referred to covariation between an IRAP and an external clinically relevant criterion variable, consistent with the APA Dictionary of Psychology definition of criterion validity (American Psychological Association, 2024). Using the descriptions in Vahey et al.’s (2015) supplementary materials, and with reference to the original papers, the individual effect sizes were re-evaluated against Vahey et al.’s inclusion criterion of covariance between an IRAP and a second external variable. While the clinical relevance of specific effects might be more subjective, the involvement of a criterion variable other than the IRAP can be assessed objectively. Worryingly, 23 of the 56 effect sizes (41%) employed by Vahey et al. (2015) were found to involve no external variable (i.e., they refer only to a reaction time differential between the IRAP block types, i.e. from a one-sample t -test), and were therefore not suitable to be included in a meta-analysis of the IRAP’s criterion validity. A large degree of incorrect inclusion error was therefore detected in Vahey et al.’s (2015) effect sizes.

Assessment of incorrect exclusions

In addition to incorrect inclusions, it is equally plausible that effect sizes that would have met inclusion criteria were erroneously not included. I therefore re-examined the same 15 articles as Vahey et al. (2015)

drew their effect sizes and searched for other effect sizes that met their inclusion criteria. Following Vahey et al.’s (2015) method, extractions were not limited to effect sizes reported in the articles, but also considered ones implied by the reported analyses (e.g., correlations where only the statistically significant estimates were reported). Where necessary, I contacted the authors of the individual articles to obtain additional estimates or data. For example, if non-significant correlations were reported as merely “other correlations were non-significant”, these effect sizes were obtained where possible. Two independent raters rated each effect for clinical relevance using Vahey et al.’s (2015) criteria. Agreement was found in 90% of cases (Cohen’s Kappa = 0.87, $p < .001$). As in Vahey et al. (2015), if either rater originally rated the effect as clinically relevant then it was included.

308 effect sizes were originally extracted. 53 were excluded as non-criterion effect sizes. 99 more were excluded as non-clinically relevant. This left 156 effect sizes for meta-analysis, compared to the 33 included by Vahey et al. (2015), after I excluded the 23 non-criterion effects (as discussed previously). This suggests that Vahey may have failed to include 85.3% of the effect sizes that met their inclusion criteria, representing a potentially serious source of incorrect non-inclusion error.

Note that these extractions are not exhaustive: some authors of original studies who were reported as having replied to Vahey et al.’s (2015) requests for additional information did not reply to my requests, perhaps due to the passage of time the ‘half-life’ of data.

These effect sizes were converted to Pearson’s r for use in a new meta-analysis that I discuss later. The specific methods of conversion are documented in the supplementary materials.

Assessment of erroneous calculation

Erroneous calculation refers to errors made in the transposition, conversion, or reporting of effect sizes. This can involve using the incorrect formula to convert effect sizes, treating Standard Errors if they are Standard Deviations, and other errors. Previous work has shown that such errors are unfortunately common in published meta-analyses (e.g., Göttsche et al., 2007; Maassen et al., 2020). In their supplementary materials, Vahey et al. (2015) provided explanations and references for how individual effect sizes were converted to Pearson’s r . However, inspection of those explanations revealed at least one error: 2 of the effect sizes were η_p^2 effect sizes taken from ANOVAs, which Vahey et al. (2015) stated that they “equated the relevant statistic [η_p^2] with r^2 therefore obtaining r using the square root function”. However, this conflates η_p^2 with η^2 : as a partialized effect size, η_p^2 cannot be converted to r , and therefore these conversions are erroneous.

A comprehensive assessment of the reproducibility of the conversions of the individual effect sizes to

Pearson's r was not performed on the basis that the above assessments had already determined these effect sizes to contain several errors (e.g. related to incorrect inclusion).

Issues in the publication bias analyses

Vahey et al. (2015) reported employing tests of funnel plot asymmetry, a sensitivity analysis based on selection models (Vevea & Woods, 2005), and Kendall's τ Rank Correlation Test (e.g., Egger et al., 1997). While I did not attempt to systematically verify the results of all of these, two points are worth highlighting here.

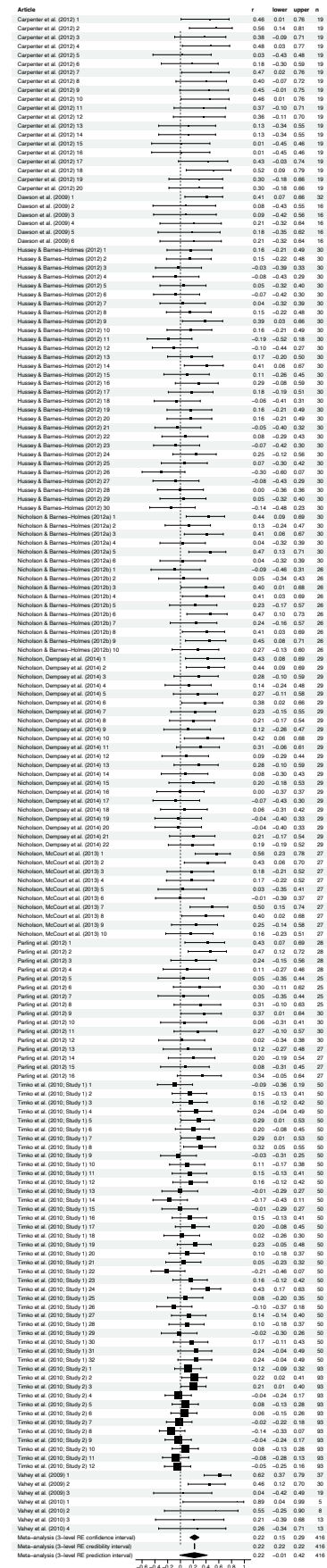
First, Vahey et al. (2015) conclude that the results of these tests suggest “that the current meta-analysis was not subject to publication bias” (p. 62). However, this falls into a statistical fallacy that is common in original research: non-significant p values should not be interpreted as evidence for the null hypothesis, only failure to reject the alternative hypothesis (Aczel et al., 2018; Greenland et al., 2016). Put differently, the absence of evidence is not the same as evidence of absence. This is especially important in the context of meta-analysis bias tests which frequently have very low power (Rücker et al., 2011; Sterne et al., 2000), as is the case here. The correct interpretation of such non-significant results is no evidence of bias was obtained rather than evidence of no bias. This difference in wording may seem subtle at first, but represents a fundamentally different and stronger claim. There are few areas of research where publication bias and p -hacking could reasonably be assumed to be completely absent. As such, direct evidence for this null effect would need to be strong to dismiss the presence of bias as a plausible default assumption.

Second, bias tests can allude to rigor or objectivity that might obscure other sources of information about whether bias is truly present. It is worth noting that 8 of the 11 (73%) articles used in the meta-analysis were co-authored by at least one author of Vahey et al. (2015). The authors of Vahey et al. (2015) therefore had direct knowledge of whether there was a file drawer of unpublished studies (or indeed any other source of bias), but they do not consider this in their estimation of bias. My own compilation of unpublished IRAP studies suggests that there are at least 6 unpublished PhD theses with clinically relevant IRAP studies, most of which came from Barnes-Holmes's research group (Hussey & Drake, 2022). Reporting quantitative tests of publication bias without also reporting *prima facie* evidence of publication bias from one's own research group ignores important evidence, and does so in a way that is biased towards enhancing the apparent criterion validity of the measure – a measure which was also created by the last author of Vahey et al. (2015).

Corrected meta-analysis and power analyses

In order to understand the compound impact of the various errors on the conclusions of the meta-analysis, I fitted a new meta-analysis to the 156 effect sizes re-extracted from the original articles. I then used the meta-analysis effect results to calculate new power

Figure 5. Forest plot for the new meta-analysis.



analyses. Whereas Vahey et al.’s (2015) method of dealing with the non-independence of multiple effect sizes taken from the same study was to average them, research suggests that it is more appropriate to model these dependencies using three-level meta-analyses (i.e., multi-level meta-analyses: Van den Noortgate et al. 2013).

A multi-level random effect meta-analysis with random intercepts for study was therefore employed. I employed the metafor packages’ default settings of a Restricted Maximum Likelihood estimator function and weighting by inverse variance (i.e., rather than N , given that inverse variance is a better estimate of precision and represents the contemporary standard).

Results demonstrated a meta effect size $\bar{r} = .22$, 95% CI [.15, .29], 95% CR [.22, .22], 95% PI [-.01, .42] (see Figure 4 for forest plot). Based on the non-overlap of their Confidence Intervals, this estimate is significantly smaller than the effect size reported by Vahey et al. (2015), i.e., $\bar{r} = .45$, 95% CI [.40, .54], 95% CR [.23, .67].

Table 1 contains the new power analyses based on this meta-effect size. As can be seen from the table, sample sizes are substantially larger than those recommended by Vahey et al. (2015). For example, whereas the original abstract includes the recommendation that IRAP studies should employ “ N of [at least] 29 to 37”, the update numbers for the same tests are N s of at least 126 to 273. Power analyses for the more common and less liberal two-sided test for Pearson’s r correlations would require N s of at least 160 to 346.

Discussion

The results of Vahey et al. (2015) could not be verified at multiple different stages in the analysis, and multiple errors and discrepancies were detected. The original article’s inclusion and exclusion criteria were not consistently applied: many effects that met Vahey et al.’s (2015) inclusion criteria were not included; conversely, many effects that were included did not meet inclusion criteria, e.g., 41.1% were not criterion effects as they did not involve an external variable. These inconsistencies in the application of the inclusion and exclusion strategy are biased towards including larger effect sizes and omitting smaller ones. The averaging of these effect sizes for each article was not computationally reproducible in 13% of cases. The results of the meta-analysis could not be reproduced despite numerous different attempts and approaches. The original power analyses were mostly reproducible, however, given the lack of reproducibility of the meta-analysis itself, the validity of those power analyses’ results based on that meta-analysis estimate was fundamentally undermined.

This lack of reproducibility is consistent with what has been found elsewhere for meta-analyses: errors in data extraction and conversion are common, results are frequently not reproducible, and this is hindered by the unavailability of data and code (Gøtzsche et al., 2007;

Kadlec et al., 2023; Lakens et al., 2016, 2017; Maassen et al., 2020).

After correcting the above issues, a new meta-analysis was conducted in order to convey the combined impact of these issues on the conclusions. Results suggested a meta-effect size of $\bar{r} = .22$, less than half that reported in the original article ($\bar{r} = .45$). Vahey et al. (2015) stated that, according to their results, the IRAP’s criterion validity compares “favorably” to the other popular implicit measures such as the Implicit Association Test ($\bar{r} = .22$ for addiction and $\bar{r} = .30$ for non-addiction psychopathologies: Greenwald et al., 2009) and evaluative priming methods (\bar{r} s = .18 to .28: Cameron et al. 2012; Herring et al., 2013; Rooke et al., 2008). Without endorsing the updated meta-analysis, by Vahey et al.’s (2015) line of argument, the current results suggest that the IRAP is therefore on par with other such measures rather than superior to them. This also brings the average criterion association observed for the IRAP closely in line with the average correlation observed across social and personality psychology (i.e., around $r = .2$: Hemphill, 2003; Hussey, 2023; Richard et al., 2003).

New power analyses mirroring the original ones were then conducted using this new meta-analytic effect size. These suggested that a much larger number of participants is required in future IRAP studies than recommended by Vahey et al. (2015). For example, although Vahey et al. (2015) make sample size recommendations for several different analyses and designs, it is most frequently cited for the specific recommendation of $N > 37$ to detect a first-order correlation ($\alpha = 0.5$, one-tailed, 80% power; e.g., Kavanagh et al. 2022). The sample size recommendation based on the updated meta-analytic effect size is $N > 273$ for a one-tailed correlation, and 346 for the much more commonly used and less liberal two-tailed correlation. It is worth noting that between 0% and 2.1% of published original research using the IRAP has included sample size meeting these criteria, according to a recent systematic review of IRAP research published between 2006 and 2022 (188 studies in 150 publications, median $N = 41$, range = 9 to 210: Hussey, 2023).

With that said, it is important to acknowledge that the primary reason to calculate a new meta-analysis was to illustrate the combined impact of the errors on the results rather than to endorse the results of this new meta-analysis or power analysis. In my opinion, Vahey et al.’s (2015) approach of taking different types of effects between the IRAP and other criterion tasks is fundamentally flawed as it combines apples with oranges on multiple different fronts. All the following effects were meta-analyzed together: effect sizes representing the magnitude of the compatibility effect on the IRAP itself, interaction effects between IRAP trial types and group allocations, and correlations between IRAP trial types and criterion tasks. In doing so, different types of IRAP data were combined as one:

data from single trial-types, overall effects for the whole task, and effects averaging the trial types in different ways. Lastly, effects treating the IRAP as the dependent variable, the independent variable, and purely associative effects were combined as one. It is exceptionally difficult to know what the resulting meta-analyzed effect size is an estimate of, i.e. what the estimand is, and whether it applies to the type of effect that a researcher may wish to observe in their own future work. For example, to what degree is the interaction effect between a depression IRAP’s trial types and high vs. low experiential-avoidance group informative to a separate study on the correlation between self-reported self-esteem and a self-esteem IRAP in a prisoner population? I argue that this apples-with-oranges approach is erroneous and leads to misleading conclusions, but the point of the verifications presented here is to highlight that Vahey et al.’s (2015) erroneous analytic approach was also erroneously implemented.

Limitations

It is possible that these verification analyses themselves contain errors. The purpose of a verification report is to attempt to independently verify the results presented in the original article and do not represent the last word on error detection. Equally, perhaps there is some way to reproduce the original results (e.g., of the meta-analysis) in a way that I have not considered, and my not being able to reproduce them does not necessarily mean they are non-reproducible in some absolute manner. Verification attempts are in general enhanced with access to the original code. Unfortunately, however, the first author of the original article declined to share their code.

Future research on error detection in meta-analyses

The recommendations of much of the previous meta-science research on errors in meta-analyses have been recommendations to the authors of meta-analyses themselves on how to prevent errors (e.g., Lakens et al., 2016; López-Nicolás et al., 2022). Relatively fewer recommendations, or indeed general strategies, have been made for researchers engaged in error detection. Kadlec et al. (2023) provide an excellent example of this, with both descriptions of their general error detection strategies and concrete recommendations such as regarding Standardized Mean Difference effect sizes (e.g., Cohen’s d , Hedges’ g) that are larger than 3 with great suspicion. The current research offers some additional suggestions and guiding principles for error detection in meta-analyses:

1. Check whether reported intervals are symmetrical around the point estimate, including both intervals around estimates from original studies and meta-analysis results. The asymmetry of intervals can provide a clue that something may be amiss, depending on their compatibility with the reported model and transformations.
2. Some plots repeat what should be the same information. This provides an error-checking

vector to validate that data which should be identical is indeed so. For example, forest plots often present effect sizes both graphically and numerically, and forest plots and funnel plots present both effect sizes and (repressions of) their precision (i.e., Confidence Intervals vs. Standard Errors, which can be calculated from one another: $SE = 95\% \text{ CI width} / [1.96 \times 2]$).

3. Data can be extracted from plots for error checking using free and Open Source tools such as WebPlotDigitizer (Marin et al., 2017).
4. Systemic checking of effect size extractions and conversion can be time-consuming. However, initial spot checks can easily be performed on the most extreme effect sizes, which are most likely to have involved an extraction or conversion error (e.g., confusing SE with SD: see Kadlec et al., 2023).
5. Spot checks on the accurate application of the inclusion criteria can also be performed. This can include checks for incorrect inclusions and incorrect omissions.
6. The normative plausibility and mathematical possibility of correlations can be assessed by deattenuating them for the reliability of the measures that produced them. This can be done using empirical estimates for those measures or plausible values (which can themselves be informed by data from the literature: Hussey et al., 2023). Correlations larger than the reliability of the component measures are implausible.
7. The normative plausibility of effect sizes, both original and meta-analytic, can also be compared to large-scale analyses of this in the literature (e.g., Hemphill, 2003; Plessen et al., 2023; Richard et al., 2003).
8. Bias assessments can also be scrutinized. This can include an assessment of over-claiming via incorrect interpretations of non-significant tests for bias. Additionally, it can be useful to assess the overlap in authorship between the meta-analysis and the original studies.

Conclusions

The results of Vahey et al. (2015) were found to have poor reproducibility at almost every stage of their analytic strategy. In aggregate, these seriously undermine the credibility and utility of the conclusions and recommendations of the original article. Recalculated results suggest that the meta-effect size was less than half the original result ($\bar{r} = .45$ vs. $.22$), and sample size recommendations were more than 15 times as large (minimum $N = 37$ vs. 346). Vahey et al. (2015) may therefore require substantial correction, and researchers should not use it for sample size planning.

Conflict of Interest

Prof Dermot Barnes-Holmes, one of the authors of Vahey et al. (2015), was my PhD supervisor between 2010 and 2015. I have not actively collaborated with Prof Barnes-Holmes since 2015. Articles lead by third parties of which we were both co-authors were

published up to 2018. The author declares no other conflict of interest associated with the publication of this manuscript.

Acknowledgements

I first presented elements of these analyses at the Association for Contextual Behavioral Science meeting in Dublin in 2019. That symposium was also the first time I met my now-wife, Sabrina. I would therefore like to acknowledge that although the IRAP literature has brought me much frustration over the years, it also brought me my greatest joy.

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