# The IRAP has poor internal consistency and test-retest reliability:

# A file-drawer meta-analysis

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# Abstract

Vahey et al.’s (2015) meta-analysis argued that the Implicit Relational Assessment Procedure (IRAP) has good criterion validity and potential for clinical assessment. However, two other meta-analyses have shown the IRAP to have very low reliability. Here, we extend this evidence based through meta-analyses of file drawer data. Individual participant data from all of our published and unpublished studies was used to estimate the IRAP’s internal consistency and test-retest reliability across a large number of domains (*k* = 13) and participants (*N*s = 890 & 340 respectively). Results suggest that internal consistency is poor (α = .53, 95% CI [.46, .59]) and test-retest reliability is very poor (ICC = .18, 95% CI [.05, .30]). This severely limits the IRAP’s ability to function as a useful measure, for clinical use or otherwise, especially at the individual level. We conclude that researchers should be very cautious about choosing to employ the IRAP or when interpreting its results.

# The IRAP demonstrates very poor internal consistency and test-retest reliability:

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The Implicit Relational Assessment Procedure (IRAP: Barnes-Holmes et al., 2010) is a computer based reaction-time task designed to capture automatic relational responding. It has been used extensively as a measure of implicit attitudes (Gawronski & De Houwer, 2011) and within Contextual Behavioral Science due to its historic ties to Relational Frame Theory (Hussey, Barnes-Holmes, et al., 2015). In their meta-analysis of criterion validity of clinically relevant IRAP studies, Vahey et al. (2015) argued that the IRAP has potential as a tool for clinical assessment. This aspiration that the IRAP might be used in applied contexts is long-standing. However, concerns have been expressed about the IRAP’s low reliability (meta-analyses by Golijani-Moghaddam et al., 2013; Greenwald & Lai, 2020) and poor individual-level estimation (Hussey, 2020). Together, these suggest that Vahey et al.’s (2015) conclusions are overly optimistic, and that the IRAP’s utility in research or applied settings is likely to be limited.

The importance of precise measurement has received renewed attention within psychology in recent years. For example, multiple authors have recently noted that poor reliability can result in experimental effects that are highly replicable that nonetheless lead to false or invalid conclusions (Devezer et al., 2020; Hussey & Hughes, 2020). There is mutual consensus that measurement is a cornerstone of the scientific method, even in fields of research that have at times been skeptical of the utility of psychometric methods (e.g., behaviorism). For example, even an animal-behaviorist working with rats in Skinner boxes is concerned with whether the levers function well as a measure of the animal’s emitted behaviour: if the lever is too heavy or too stiff, the acquisition curve recorded will not accurately reflect the animal’s lever-pressing behaviour, and will hinder the researcher’s analysis. As such, although the IRAP has often been employed by behaviorally orientated researchers interested who identify as engaging in function analytic-abstractive research (see Barnes-Holmes & Hussey, 2016; Hayes & Brownstein, 1986), issues of psychometric reliability cannot be ignored.

## Previous meta-analyses of the IRAP’s reliability

The IRAP’s reliability has been examined in two previous meta-analyses of published articles. Golijani-Moghaddam et al. (2013) extracted data from 7 published studies containing 9 independent samples, including 318 participants for the meta-analysis of internal consistency and one study of 23 participants assessing test-retest reliability. Meta estimates of internal consistency (i.e., split-half reliability via Pearson’s *r* correlations with Spearman-Brown correlations) were = .65, 95% CI [.54, .74]. Just one study was found that reported test-retest reliability: *r* = .49, 95% CI [.10, .75] (Note that confidence intervals were not reported in the original article, but were calculated here using the total sample size).

More recently, Greenwald & Lai (2020) conducted a large scale review and meta-analyses of multiple implicit measures including the IRAP. Thanks to making their data and code openly available, it was possible to computationally reproduce their meta-analyses of IRAP data (see supplementary materials for data and code: [osf.io/v3twe](https://osf.io/v3twe/)). They note in their data that many estimates were sourced from other meta-analyses – presumably Golijani-Moghaddam et al.’s (2013). Data was included from 13 published studies, a total of 1207 participants for the meta-analysis of internal consistency and two studies with a total of 124 participants assessing test-retest reliability. Using Greenwald & Lai’s (2020), computationally reproduced estimates of internal consistency data were Cronbach’s α = .56, 95% CI [.46, .65], 95% CR [.03, .85], and test-retest reliability was Pearson’s *r* = .45, 95% CI [.33, .55].

In one sense, the results of the two meta-analyses show a significant degree of variation, with Greenwald & Lai (2020) reporting a substantively lower estimate of internal consistency than Golijani-Moghaddam et al. (2013). However, both meta-analyses support the idea that the IRAP’s reliability is problematically low, both below typically accepted cut-offs for assessment measures in psychology (Post, 2016), and also lower than other implicit measures such as the Implicit Association Test (IAT: Greenwald et al., 1998; internal consistency α = 0.50, test-retest *r* = 0.50: Greenwald & Lai, 2020). This poses a significant threat to the task’s basic and applied utility, both in relation to other assessment methods more generally but also compared to alternative implicit measures more specifically.

## The current research

Two factors suggest that there is need for additional assessment of the IRAP’s reliability. First, meta-analyses of published literature are susceptible to publication bias. Given the relationship between internal consistency and statistical power (Parsons, 2018), it is quite possible that IRAP studies that demonstrated poor measurement properties were less likely to obtain significant results, and therefore were unfortunately less likely to be published.

Second, published articles have used a range of different metrics when reporting reliability, and have frequently not reported gold-standard metrics. For example, published studies on test-retest reliability have reported Pearson’s *r* correlations. However, Parsons et al. (2019) recently highlighted that Pearson’s *r* captures one specific aspect of stability (i.e., the preservation of rank among participants between time-points) but neglects others (e.g., the absolute change in scores between timepoints). This can be illustrated using a simple example: imagine if at time-point 2 all participants scored exactly 10 points higher on an IQ scale than they did at time-point 1. A Pearson’s *r* correlation would suggest that test-retest reliability was perfect (*r* = 1.0) because rank among participants was preserved, despite there being clear and large changes in responses between the timepoints. In order to capture both aspects (preservation of rank and lack of absolute change), a measure of ‘Absolute Agreement’ such as Intraclass Correlation Coefficients (ICC) should instead be reported (specifically ICC[2,1]: see Parsons et al., 2019).

To take another example, the calculation of internal consistency via split-half reliability involves a somewhat arbitrary decision regarding how the data is split. While most IRAP studies have split by odd versus even trials by order of presentation, other common implicit measures such as the IAT instead split by first versus second half of the task by order of presentation. Parsons et al. (2019) note that both choices are arbitrary, and that internal consistency should instead be estimated by a permutation resampling approach. This involves creating a large number of random splits of the data and calculating reliability for each, then taking this mean of this distribution of reliabilities. Importantly, this method approximates Cronbach’s alpha where others frequently do not. However, in order to calculate both ICCs and permutation-based estimates of internal consistency, access to trial-level data is needed.

Both of the above factors were addressed by conducting a file drawer meta-analysis. That is, where all studies – both published or unpublished – originating from an individual or group are used. We pooled data for all studies that we have been involved in.

# Method

## Data

All code and data needed to reproduce our analyses is available on the Open Science Framework, along with all word and image stimuli, instructions, responding rules, and task parameters used in each of the IRAPs ([osf.io/v3twe](https://osf.io/v3twe/)).

Internal consistency data came from 26 different IRAPs in 13 different domains (see Figure 2) with a total of 964 participants. Test-retest data came from IRAPs in 8 domains with a total of 365 participants. The studies employed one of two different follow-up periods: immediate (7 domains) and 1-week (1 domain; see Figure 2). Some of this data has been published for other purposes (Drake et al., 2015, 2016, 2018; Finn et al., 2016; Hussey, Daly, et al., 2015). However, the large majority of this data was not considered by either of the two published meta-analyses of the IRAP’s reliability, with the exception of a subset of the friend-enemy and Lincoln-Hitler IRAPs (Drake et al., 2016) which was used in Greenwald and Lai (2020).

## Participants

All participants provided informed consent prior to participation, and studies were approved by the local institutional review boards. Where demographics data was available, a majority of participants were women (62.9% female, 36.8% male, 0.2% non-binary; *M*age = 21.0, *SD* = 5.8).

**Measures**

The IRAP is a computer-based reaction time task. Its procedural parameters have been discussed in great detail in many other papers (Barnes-Holmes et al., 2010; Hussey, Thompson, et al., 2015), and so only a brief overview will be provided here (see Hussey, 2020). On each block of trials, participants are presents with images or words at the top of the screen and in the middle of the screen. Response options are presented on the bottom left and bottom right hand sides of the screen, and are mapped to the left and right response keys. In order to progress to the next trial, the correct response must be given. Incorrect responses result in a red X being presented on screen. Between blocks of trials, this correct response changes so that, for example, participants must respond to “white people” and “dangerous” with “True” on one block and “False” on the other block. Participants complete pairs of these blocks in two phases: practice and testing. In order to progress from practice to testing, the participant must respond quickly and accurately on both blocks within the pair. This was typically a median reaction time < 2000 ms and percentage accuracy > 80%, however this varied slightly between IRAPs. Should they fail to meet this criteria, the participant completes another pair of practice blocks. Should they meet the criteria, they progress to the testing phase where they complete three pairs of blocks in a row. Following standard practice, only reaction time data from the test blocks is used in the analyses (Hussey, Thompson, et al., 2015). As noted previously, all word and image stimuli, instructions, responding rules, and task parameters such as practice criteria can be found in the Supplementary Materials ([osf.io/v3twe](https://osf.io/v3twe/)).

## Data processing

IRAP studies typically using the *D* scoring method to convert each participant’s reaction times into analyzable values. The *D* score has some similarities to Cohen’s *d*, insofar as it is a trimmed and standardized difference in mean reaction time between the two block types. The specifics of the *D* score have been discussed in precise detail in other publications (Barnes-Holmes et al., 2010; Hussey, Thompson, et al., 2015) and therefore will only be summarized here. Its key points are that reaction times > 10,000 ms are trimmed, a mean reaction time is calculated for the trials in each block type, and a standard deviation is calculated for the pooled trials in both blocks. The difference between the means is then divided by the standard deviation, resulting in a *D* score. Participants were excluded if their mean reaction times in the IRAP test blocks were ± 2 Median Absolute Deviations from the median, in order to exclude implausibly fast or inappropriately slow responding. A total of 74 participants (7.8%) were excluded from the internal consistency dataset on this basis and 25 participants (6.8%) from the test-retest.

# Results

## Meta-analytic strategy

All data processing and analyses were done in R (R Core Team, 2020). Interclass Correlation Coefficients were calculated using the psych package (Revelle, 2016). Meta-analyses were conducted using the metafor package (Viechtbauer, 2010, version 2.4-0) and Restricted Maximum Likelihood (REML) estimation. Meta-analysis of internal consistency estimates involved Bartlett transformations prior to analysis and inverse Bartlett transformations of meta-estimates for reporting. Analyses of test-retest reliability involved Fisher’s *r*-to-*z* transformations and inverse transformations. Heterogeneity metrics refer to heterogeneity in the transformed estimates.

## Internal consistency

As noted in the introduction, the IRAP’s internal consistency can be estimated by split-half reliability; however, multiple ways of splitting the data exist. Three ways were computed and are reported here, based on their relevance to making comparisons with the output of common software implementations of the IRAP, comparisons with other implicit measures, and to provide the most accurate estimate of internal consistency.

**Split-half via odd vs. even trials.** The modal strategy used in the IRAP literature is to use an odd-even split-half, where separate *D* scores are calculated for odd- and even-numbered trials by order of presentation, Pearson’s *r* correlations between these two sets of *D* scores are calculated, and then the Spearman-Brown correction is applied to adjust for test shortening (i.e., ). Multiple software implementations of the IRAP report this form of split-half *D* scores in their output. This result may be most useful when attempting to directly compare against results collected using the most common software implementations of the IRAP, although it does not necessarily represent the best estimate of the IRAP’s true internal consistency. When internal consistency was calculated using this method for each IRAP, the meta-analytic estimate of internal consistency was found to be poor: = .57, 95% CI [.47, .65], 95% CR [.10, .79].

**Split-half via first vs. second half.** Other popular implicit measures typically employ a different splitting method: the IAT’s split-half reliability is usually calculated by dividing the trials into the first- versus second-half of trials by order of presentation. Again, Pearson’s *r* correlations were then calculated between these two sets of *D* scores, and a Spearman-Brown correction was applied. This method is useful to calculate in order to directly compare the IRAP’s internal consistency to the IAT’s. Using this method, the meta-analytic estimate internal consistency was found to be very poor: = .49, 95% CI [.42, .56], 95% CR [.42, .56]. In contrast, a recent meta-analysis reported that the IAT’s internal consistency, when calculated using this method, was substantively better (α = .80: Greenwald & Lai, 2020).

**Split-half via many permutations.** The large differences in the results found between these two methods (odd vs. even, first vs. second half) serves to highlight that the choice of splitting method is simultaneously arbitrary and yet has a significant impact on conclusions. Which method, if any, should researchers accept as providing more accurate results? Parsons et al. (2019) argued that no single decision need be made: instead of employing a single splitting method, a very large number of permutations of spits should be computed (e.g., 2000). In each permutation, the data is split into two randomly determined halves, *D* scores are calculated for each, Pearson’s *r* correlations are calculated from these two sets of *D* scores, and then a Spearman-Brown correlation is applied. A distribution of estimates is therefore obtained across permutations. This distribution is then parameterized: the mean value is used as the estimate, and the quantile method is used to find 95% Confidence Intervals. Parsons et al. (2019) noted that this method approximates Cronbach’s α, and remove assumptions associated with specific split strategies (e.g., regarding learning occurring with the task between the first vs. second half). The permutation method was therefore deemed to be the most appropriate strategy to accurately estimate the IRAP’s internal consistency, and its results were used for conclusions.

Using the permutation method, the meta-analytic estimate of internal consistency was found to be poor, α = .55, 95% CI [.47, .62], 95% CR [.28, .72]. A small degree of heterogeneity was found between estimates, *Q*(*df* = 25) = 35.14, *p* = .086, 𝜏2 = 0.05, *I*2 = 28.6%, *H*2 = 1.4. A GOSH plot (Olkin et al., 2012) was used to attempt to understand this heterogeneity by assessing whether the meta-estimate was unduly influenced by outliers. This analysis uses resampling to calculate the distribution of estimates of effect size and heterogeneity (i.e., *I*2) using a large number of subsets (5000) of possible combinations of the effect sizes. As illustrated in Figure 1, results indicated bimodality in both estimates of effect size and heterogeneity that was driven by data from a single effect size (the sexuality IRAP: α = .93, 95% CI [.82, .97]), suggesting that it represented an outlier that biased the results. When this effect size was excluded, the meta-analytic estimate of internal consistency was found to be poor, α = .53, 95% CI [.46, .59], 95% CR [.43, .61], but now with no heterogeneity, *Q*(*df* = 24) = 19.84, *p* = .706, 𝜏2 = 0.00, *I*2 = 3.6%, *H*2 = 1.0. See Figure 2 (upper panel) for Forest plot. Due to the combination of the permutation-based split-half method and the exclusions of outliers, this represents the most appropriate estimate of the IRAP’s internal consistency among those reported here.



**Figure 1.** GOSH plot for internal consistency.



**Figure 2.** Forest plots.

## Test-retest reliability

As noted in the introduction, Parson’s (2019) argues that test-retest reliability is better captured by the calculation of metrics of ‘Absolute Agreement’ (i.e., Interclass Correlation Coefficients) than simple correlations between timepoints, on the basis that correlations capture preservation of rank but not absolute changes in scores. Results suggested that test-retest reliability was very poor, ICC = .18, 95% CI [.05, .30], 95% CR [-.11, .44]. A substantial degree of heterogeneity was found between the two studies, *Q*(*df* = 7) = 18.5, *p* = .010, 𝜏2 = 0.02, *I*2 = 60.4%, *H*2 = 2.5. A GOSH plot revealed no evidence of multimodality and therefore no evidence of outliers (see Figure 3). As such, this heterogeneity may be attributable to other unmodeled factors, such as the domain, follow-up period, features of the stimulus set or task parameters, or others. Results can be found in Figure 2 (lower panel). The IRAP’s test-retest reliability therefore appears to be lower than the IAT’s (*r* = .50) according to the recent review by Greenwald and Lai (2020).



**Figure 3.** GOSH plot for test-retest reliability.

# Discussion

Results suggest that the IRAP’s internal consistency is poor and its test-retest reliability is unacceptably low. Compared to previous meta-analyses, this work has the dual benefits of (a) being based on our complete file drawer data, which is not subject to publication bias, (b) uses more optimal analytic methods, and (c) is fully computationally reproducible as we share our data and code.

Our estimate of internal consistency (α = .53, 95% CI [.46, .59]) was smaller than that reported by in one previously published meta-analysis ( = .65, 95% CI [.54, .74]: Golijani-Moghaddam et al., 2013) and similar to that reported in the other (α = .56, 95% CI [.46, .65]: Greenwald & Lai, 2020). Our estimate of test-retest reliability (ICC = .18, 95% CI [.05, .30]) was significantly lower than those reported by either previously published meta-analysis (*r* = .49, 95% CI [.10, .75]: Golijani-Moghaddam et al., 2013; *r* = .45, 95% CI [.33, .55]: Greenwald & Lai, 2020). Differences in results may be due one or more features of our work relative to previous research: our larger test-retest sample, the resilience of whole-lab file-drawer meta-analyses to publication bias, or our more advanced statistical methods (e.g., controlling for absolute change between timepoints, use of permutation-resampling to avoid arbitrary choices in split-half, or assessment of outliers). While our results differ from previous meta-analyses to some degree, the conclusions of all agree that the IRAP’s internal consistency and test-retest reliability is poor at best.

## Implications of low reliability for statistical power

An underappreciated fact is that a measure’s reliability has a direct relationship with its ability to detect true effects (i.e., statistical power), and therefore the sample sizes needed for a given analysis. Parsons (2018) provides a useful discussion of how reliability provides a ceiling for the associations among variables. The maximum observed estimate of the true correlation among two measures and (i.e., ) is a function of the true correlation () and also the reliability of both measures (i.e. their self-correlation and ):

We can imagine that one measure is the IRAP and the other is some external variable of interest, such as a disgust-related behavioral approach task (Nicholson & Barnes-Holmes, 2012). If we put aside the reliability of the external variable (e.g., imagine it is perfect with a reliability of 1.0), we can use our meta-analyzed estimates of the IRAP’s reliability to estimate the maximum correlations that could be observed between the two. No one form of reliability fully captures a measure global reliability, so it is useful to calculate estimates using estimates for both test-retest reliability (ICC = .18)and internal consistency (α = .53). Maximum correlations with the IRAP (i.e., where true correlation = 1) was estimated to be *r* = .42 and .73, respectively. Smaller true correlations would also be scaled downward to a comparable degree. For example, a medium true correlation ( = .5) would imply maximum observable correlations of *r* = .20 and .36, respectively. These large reductions in the actual observed correlation among variables must then be considered when choosing sample sizes – loosely speaking, in order to detect what is in reality a ‘medium’ effect size, the researcher may have to power the study to detect ‘small’ effect sizes. Tasks with low reliability, such as the IRAP, therefore place studies under increased data collection burdens or lower statistical power to detect true effects.

## Ways to improve reliability

It seems important to consider ways in which the IRAP’s reliability could be improved. One possible and commonly recommended way of improving a tasks’ reliability is to increase its length. In this case this would involve adding additional trials to the IRAP. The Spearman-Brown prediction formula can be rearranged to make a specific prediction about the relative change in task length that would be needed to obtain a given reliability estimate. Where refers to the goal reliability, refers to the current reliability, and refers to the multiple of current test length:

Using the meta-analytic estimate of the IRAP’s internal consistency (α = .53), in order to increase internal consistency to α = .70, the task would need to contain 2.1 times the number of trials it currently does. Using the meta-analytic estimate of test-retest reliability (ICC = .18), in order to increase internal consistency to ICC = .70, the task would need to contain 10.8 times the number of trials it currently does. In order to put these in context, the IRAP currently takes around 10 to 15 minutes to complete. These increases would therefore result in a task that would take between 20 minutes and 2.5 hours complete, depending on the type and level of reliability desired. While technically possible, this may either put an unreasonable burden on participants or lower the tasks utility relative to information that could be collected via alternative methodologies.

As such, other approaches to improving the IRAP’s reliability may be more effective. Lessons could be learned from existing literature using similar tasks. For example, some versions of the Brief IAT have discarded data from the first few trials in each block as they tend to be slower and noisier than subsequent trials (Nosek et al., 2013). Elsewhere, researchers have examined the possibility of more robust scoring methods as alternative to the *D* score (De Schryver et al., 2018). Other avenues of work would be to consider how to wield better stimulus control over responding within responding IRAP-like tasks, which may also serve to increase the reliability of such behavior. Research has already shown that many more task features serve as important sources of stimulus control over the IRAP effect than was initially thought. For example, the dimension along which the two category stimuli are related, even though the task never requires the participant to emit this relational response (Hussey et al., 2016); or the rules presented before each block that indicate how the participant should respond, even though they merely specify the responding rule within that block (Finn et al., 2016). While these and other sources of stimulus control have been demonstrated, no work has used these to increase the reliability of behaviour within the IRAP.

## Conclusions

Vahey et al.’s (2015) meta-analysis of criterion validity concluded that the IRAP shows promise as a clinical assessment measure. However, a degree of reliability is a prerequisite for validity (Loevinger, 1957). The results of this and two other meta-analyses of reliability suggest that the IRAP’s reliability is poor at best and unacceptably low at worst. This poor reliability has direct negative implications for statistical power in past and future studies. Elsewhere, recent research has also suggested that the IRAP demonstrates very poor individual level estimation (Hussey, 2020). As such, in its current form, the IRAP likely has limited use as an assessment tool in either research or applied settings. Researchers should be exceptionally cautious when choosing to use the IRAP in their research, or when interpreting the results of IRAP studies.

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