The Implicit Relational Assessment Procedure is not very sensitive

to the attitudes and learning histories it is used to assess

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Abstract

Several recent articles have reached the same conclusion that effects on the Implicit Relational Assessment Procedure (IRAP) are biased in some way or demonstrate generic patterns of effect regardless of what domain is being assessed. Multiple accounts have been advanced to explain why this might be the case. However, no work has sought to either (a) precisely estimate this generic effect, (b) correctly label it as a method effect, or (c) considered the implications of this confound on the validity of analyses in past and future research. This study used a large open dataset (*N* = 709) of IRAPs capturing implicit evaluations in multiple domains (*k* = 10). Results demonstrated a specific generic pattern among IRAP effects that was common across domains. The majority of variance in IRAP effects is attributable to the generic pattern rather than the domain being assessed. The IRAP is therefore relatively insensitive to the attitudes or learning histories that it is intended to assess. Common analyses of IRAP data are confounded by this method effect, and these analyses are employed in 83% of the published IRAP literature. This undermines the validity of at least some of the claims in the majority of IRAP publications.

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Implicit measures have seen widespread use across many clinical and social domains over the past two decades and are now a mainstay of psychological measurement (Greenwald & Lai, 2020; Nosek et al., 2011). A wide variety of implicit measures have been created, with each procedure having unique features and benefits. In particular, the Implicit Relational Assessment Procedure (IRAP: Barnes-Holmes et al., 2010) is acknowledged to be one of few implicit measures that allows researchers to assess the relations between stimuli of interest (Gawronski & De Houwer, 2011). That is, the IRAP can assess not only how automatically concepts and attributes are associated (e.g., self and negative) but also the manner in which they are related. For example, the distinction between “I am bad” versus “I want to be bad” (Remue et al., 2013, 2014). IRAP researchers have offered this distinction as a potential benefit of the procedure compared to associative measures (Gawronski & De Houwer, 2011; Hughes et al., 2012).

## Generic patterns in IRAP data

However, the IRAP has also been subject to an important criticism: effects on the task appear to suffer from a ‘positive framing bias’. Stimulus categories are often evaluated positively on the IRAP even whenthe participant would be expected to hold neutral or negative attitudes towards that category (O’Shea et al., 2016). For example, normative participants apparently demonstrate positive evaluations of both death (Hussey, Daly, et al., 2015) and Hitler (previously unpublished data reported in this article).

O’Shea et al. (2016) argued that this effect occurs due to the valence of the IRAP response options: ‘True’ is more positively valenced than ‘False’, and valence congruence between the response option and the valence of the attribute stimuli generate positive IRAP effects. To put this another way, whereas the IRAP is intended to provide a measure of the automatic relating of the category and attribute stimuli (e.g., ‘Black people’ and ‘Pleasant’), the effect may instead be driven by the congruence between the valence of the attribute stimuli and response option (e.g., ‘Pleasant’ and ‘True’ vs. ‘False’). O’Shea et al. (2016) therefore advance two key ideas: (1) they argue that IRAP effects are driven in large part by some factor that is unrelated to the phenomenon that is of direct interest to researchers using the task, and (2) they advance a specific explanation of this, which will we refer to here as the ‘valence congruence account’.

Subsequent research has agreed with the idea that IRAP effects are influenced by factors other than category-attribute relations but has provided alternative explanations of why this phenomenon occurs. Finn et al. (2016) employed an IRAP which involved relating non-evaluative stimuli (i.e., colors and shapes). Despite including no evaluative stimuli, a comparable bias was demonstrated, whereby effects on some trial types were larger than others. This would seem to suggest that O’Shea et al.’s (2016) valence congruence account is insufficient. Finn et al. (2016) advanced an alternative account of this effect, which they continued to develop in a subsequent publication (Finn et al., 2018). The key point to be appreciated here is that while O’Shea et al. (2016) and Finn et al. (2016, 2018) disagree as to the *cause* of this bias in IRAP effects, the presence, replicability, and generalizability of these biases in IRAP effects is apparently uncontroversial.

## Goals of the current research

In contrast with previous work that has focused on explanations of these biases in IRAP effects, the current research seeks to (1) quantify this bias more precisely, and (2) consider its implications for the validity of the conclusions made in the published literature. We argue that these biases, which we hereafter refer to as the ‘generic pattern’ observed in IRAP effects, are evidence of a method effect. Scheweizer (2020) describes method effects as “systematic variation observed in measurement that originates from the method of measurement instead of from the attribute, which the scale or measurement procedure is expected to capture. Method effects are major sources of impairment of the quality of measurement. Because of a method effect a scale or measurement procedures does not or only partly measure what is expected to measure.” (p.337). Method effects represent important confounds to analyses employing that measure and claims based on those analyses. Recent research has highlighted potential methods effects in other implicit measures, such as the Affect Misattribution Procedure (Hughes et al., 2022; Hussey & Cummins, 2022). Put simply, the presence of a method effect in RIAP data would imply that any given IRAP is simultaneously measuring two things: attitudes or learning histories to a given attitude domain, but also a generic tendency to demonstrate a given pattern on the IRAP regardless of the attitude or stimulus domain. If present, analyses of IRAP data which do not systematically take account of such a method effect would be unable to attribute the effects they detected to participants attitudes or learning histories (i.e., to make substantive conclusions of interest to the researcher) or merely to the method effect. The validity of claims made based on such confounded analyses is undermined.

Previous debate about the nature of any generic pattern may have been driven by the fact that this pattern has not yet been well estimated, due to a combination of small sample sizes (Median = 41 according to a recent systematic review: Hussey, 2023) and a limited range of domains. In order to overcome this, this article used an unprecedently large sample (*N* = 709 after exclusions) addressing multiple attitude domains (*k* = 10). This was achieved by collating data from published and unpublished IRAP studies conducted across two labs that undertook multi-year IRAP research programs. This work aimed to (1) assess the evidence that IRAP effects tend to follow a generic pattern by estimating the generic pattern more precisely; (2) understand the severity of the generic pattern by quantifying the proportion of variance in IRAP effects that comes from undesirable sources (i.e., the generic pattern) versus desirable sources (i.e., sensitivity to the domain being assessed); and (3) make recommendations about which common analytic strategies give rise to valid versus invalid inferences as a result of this generic pattern.

# Method

All data and code for data processing and analysis code is available on the Open Science Framework ([osf.io/vhzsn](https://osf.io/vhzsn/)). We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study (Simmons et al., 2012). All analyses were done in R (R Core Team, 2022) using the packages ez (Lawrence, 2016), schoRsch (Pfister & Janczyk, 2019), and lme4 (Bates et al., 2015).

## **Data**

Data were taken from an existing, publicly available dataset of IRAP data (osf.io/v3twe; see Hussey & Drake, 2020). The current study therefore employs secondary analysis of existing data, with sample size being determined by data availability. Inclusion criteria were as follows: (1) The study must have included at least one standard IRAP (i.e., not variants such as the MT-IRAP or Training IRAP); (2) The IRAP must employ single-word valenced attribute category stimuli (e.g., positive vs. negative). This did not include other more specific categorizations (e.g., masculine/feminine) or more elaborate propositions (e.g., ‘I can approach’ vs. ‘I cannot tolerate it’). This served to limit the differences between IRAPs to the domain being evaluated while keeping other aspects of the procedure relatively consistent; (3) The IRAP must have used ‘True’ and ‘False’ as response options within the procedure; (4) When a study employed multiple IRAPs within participants, only the first IRAP that each participant completed was included. Data from 12 IRAPs across 10 domains using 12 stimuli sets and a total of 753 participants met inclusion criteria.

## Performance exclusions

Participants whose percentage accuracy or mean reaction time on the IRAP test blocks were more than 2.5 standard deviations from the mean were excluded as outliers. These exclusions were calculated separately for each domain to allow for differences in the distributions of mean reaction times. This method was adaptive to differential mean response latencies between domains, removed the need for an arbitrary cutoff, and is consistent with recommendations for the treatment of outliers in the wider reaction-time literature (Ratcliff, 1993; Whelan, 2008). A total of 44 participants (5.8%) were excluded on this basis leaving 709 participants in the analytic sample.

## Participants

Ethical approval for each original study was granted by the local institutional review board, and informed consent was obtained from all individuals prior to participation. Where demographics data were present, participants were typically female (193 women [62.5%], 159 men [37.2%], 1 identified as nonbinary [0.3%]) and young adults (*M*age = 20.1, *SD* = 4.7). Sample size by attitude domain ranged from 19 to 131 (*M* = 70.9, *SD* = 38.8).

## 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 presented 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 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. The correct responses alternated between blocks. For example, an IRAP study examining racial attitudes might include “White people” and “Black people” as category stimuli and positive and negative words as attribute stimuli. In this example, a participant must respond to “White people” and “Dangerous” with “True” on one block and “False” on the other block. Participants initially complete pairs of these blocks during a practice phase of the task; once responding meet predetermined criteria for a both of a pair of practice blocks (typically a median reaction time < 2000 ms and a percentage accuracy > 80%), participants proceed to the test block (typically three pairs). Only data from the test blocks were used in the analyses (Hussey, Thompson, et al., 2015). Details of each IRAP’s stimuli, task parameters, and responding rules can be found in the Supplementary Materials of the original dataset ([osf.io/v3twe](https://osf.io/v3twe/)).

## Data processing

IRAP studies typically use 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 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.

Four *D* scores were calculated for each IRAP, one for each of the four trial-types (e.g., ‘life – positive’, ‘life – negative’, ‘death – positive’, ‘death – negative’). Data for each study were scored so that positive *D* scores referred to faster responding on the blocks assumed to be consistent with participants’ learning histories, as in the original studies. For example, positive *D* scores on trial-type 1 on the body-shape IRAP referred to faster responding to the stimulus pair ‘thin – positive’ with ‘True’ relative to ‘False’; comparably, positive *D* scores on trial-type 1 on the Christian-Muslim IRAP referred to faster responding to ‘Christians – Safe’ with ‘True’ relative to ‘False’. Means, SDs, and correlations between D scores on each trial type are presented in Tables 1S and 2S in the supplementary materials.

# Results

## Evidence for the generic pattern

We hypothesized that if the IRAP is relatively sensitive to the domain being assessed then a greater proportion of variance will be attributable to the main effect for IRAP stimuli domain and/or the interaction between domain and trial type than for the main effect for trial type. However, if IRAP effects are mostly driven by the generic pattern then the main effect for trial type effect would be larger than the main effect for domain or their interaction effect. This latter scenario would imply that the IRAP is relatively insensitive to the stimulus domain being assessed.

A mixed within-between ANOVA was run using type III sum of squares method with IRAP *D* scores as the dependent variable, IRAP trial-type as the within subjects independent variable (i.e., Category 1 – Positive, Category 1 – Negative, Category 2 – Positive, Category 2 – Negative), and domain as the between subjects independent variable. Only data from the domains featuring known-words were used (i.e., all domains other than the non-words IRAP). Because our hypothesis involved comparing the proportion of variance attributable to the effects effect rather than assessing differences in means, only the ANOVA effect sizes are reported. Three different metrics of effect size are reported: , , and , where refers to the percentage of variance explained (akin to ); (‘partial’) refers to the percentage of variance explained after controlling for all other main and interaction effects; and (‘generalized’) also refers to percentage of variance after controlling for other effects, but is corrected to be interpretable across differential factorial designs (e.g., number of levels and within vs. between factors; see Lakens, 2013). All three metrics are reported here to maximize informativeness. Confidence intervals were bootstrapped using the case removal and percentile method with 2000 replications. Point estimates for effect sizes are computed as median bootstrapped estimates for robustness. Following standard practice, 90% confidence intervals are reported rather than 95% confidence intervals on the basis that squared effect sizes can only be positive numbers.

Given the mix of within and between-subject effects, was the most appropriate effect size to compare in order to make inferences. Indeed the generalized form of this effect size was created for exactly such purposes (Lakens, 2013). Results demonstrated that the majority of variance in IRAP effects was explained by the main effect for trial type ( = 0.70, 90% CI [0.66, 0.75], = 0.24, 90% CI [0.22, 0.27], = 0.17, 90% CI [0.15, 0.19]) with only a small fraction explained by either the main effect for domain ( = 0.10, 90% CI [0.07, 0.14], = 0.08, 90% CI [0.06, 0.10], = 0.03, 90% CI [0.02, 0.04]) or the interaction between domain and trial type ( = 0.09, 90% CI [0.07, 0.12], = 0.04, 90% CI [0.03, 0.05], = 0.03, 90% CI 90% CI [0.02, 0.04]). Hypothesis tests were conducted by converting the estimates of and their standard errors their difference scores and their standard errors (Cornell Statistical Consulting Unit, 2008), and then converting these to *z*-values and *p*-values (Lin, 1989). The proportion of the variance attributable to the main effect for trial type was larger than that attributable to the main effect for domain, *z* = 10.0, *p* < .00000000000000000000001, and interaction effect, *z* = 10.5, *p* < .0000000000000000000000001. Results are illustrated in Figure 1. Results therefore supported the conclusion that variation in the IRAP effects are mostly attributable to a generic pattern among the IRAP trial types. IRAP effects are therefore relatively insensitive to the attitude domain being assessed.

**Figure 1***.* Effect size estimates for the ANOVAs. Error bars represent 95% confidence intervals.



## Estimating the generic pattern

A meta-analytic model was then used to estimate the generic pattern. Specifically, a crossed random effects model with *D* scores as the dependent variable, trial type as independent variable, and both domain and participant as random effects (i.e., random intercepts). The Wilkinson notation of this model was as follows:

D ~ 1 + trialtype + (1 | domain) + (1 | participant)

The generic pattern was therefore estimated via the estimated means for each trial type. Estimated means were Category 1 – Positive: *M* = 0.34, 95% CI = [0.30, 0.38], *p* < .001; Category 1 – Negative: *M* = 0.14, 95% CI = [0.06, 0.21], *p* < .001; Category 2 – Positive: *M* = -0.20, 95% CI = [-0.28, -0.12], *p* < .001; Category 2 – Negative: *M* = 0.02, 95% CI = [-0.06, 0.09], *p* < .001. This pattern is illustrated in Figure 2 (upper panel), along with the estimated marginal means in each domain.

**Figure 2.** The generic pattern in IRAP effects*.* Upper panel illustrates mean IRAP effects for each attitude domain, with the meta-analyzed generic pattern in black. Lower panel compares effects on a non-words IRAP with the generic pattern. Error bars represent 95% CIs.



In order to facilitate the understanding of this generic pattern, the Category 2 trial types were inverted following standard guidelines for the interpretation of IRAP effects (Hussey, Thompson, et al., 2015). This provided a common interpretation across trial types: positive *D* scores represent more positive evaluations and negative *D* scores represent more negative evaluations (i.e., quicker responding to positive attribute stimuli with ‘True’, or negative attribute stimuli with ‘False’). These inverted *D* scores are illustrated in Figure 3. The generic pattern therefore implies that, regardless of what attitude domains served as Category 1 and Category 2, participants evaluative Category 1 more positively than Category 2, and affirm positively more than they reject negativity. As can be seen from the estimated means, the ordinal ranking among the trial types is Category 1 – Positive > Category 1 – Negative > Category 2 – Positive > Category 2 – Negative.

**Figure 3.** Meta-analyzed estimates of the generic pattern in IRAP effects. Scores for Category 2 have been inverted for interpretability. Error bars represent 95% CIs.



## Does the generic pattern represent a methods effect or genuine attitudes and learning histories?

The previous set of analyses provided evidence for the existence of the generic pattern, adding to that provided by previously published work (e.g., Finn et al., 2016, 2018; O’Shea et al., 2016). We now turn to the status of the generic pattern, as either a method effect or due to participants’ genuine attitudes and learning histories. Scheweizer (2020) describes method effects as systematic variation that originates from the method of measurement instead of that which the procedure is expected to capture.

We describe two sources of evidence that the generic pattern represents a method effect.

Existing source of evidence is the effect sizes presented already

First, we considered evidence from a negative control. If the IRAP is a measure of participants’ preexisting attitudes and learning histories (i.e., involves no method effect), then participants should demonstrate no IRAP effect around novel stimuli which they have no attitudes towards or learning histories around (i.e., evaluations of the non-words CUG and VEC). That is, this type of method effect can be tested via a negative control: a near zero score should be observed under conditions where near-zero scores are expected to be observed. This was already examined by O’Shea et al. (2016), who found that participants demonstrate IRAP effects towards these non-words that are significantly different from the neutral point (*D* score = 0). As such, published research already supports the idea that the generic pattern represents a method effect. Our dataset contained a replication of this finding: 19 participants complete a non-words IRAP. Following O’Shea et al.’s (2016) strategy, we combined the positive trial types (CUG positive and VEC positive) and the negative trial types (CUG negative and VEC negative). One-sample *t*-tests to test whether effects on each combined trial type were different from zero. We replicated the pattern reported in O’Shea et al. (2016): there was a significant IRAP effect the positive trial types, *t*(18) = 4.75, *p* = .0002, Mean *D* score = 0.27, but not the negative trial types, *t*(18) = 1.00, *p* = .33, Mean *D* score = 0.07. The presence of this IRAP effect (i.e., any evidence of an IRAP effect on any trial type) represents evidence of a method factor. Descriptively, the generic pattern is also visible when plotting the generic pattern observed across known attitude domains versus that on the non-words IRAP (see Figure 2, lower panel).

# Discussion

Results demonstrate that IRAPs assessing implicit evaluations demonstrate a generic pattern among trial types that is unrelated to the domain supposedly being assessed. Variation in IRAP effects is attributable in large part to this generic pattern, much more so than the category stimuli employed in the procedure. This suggests that the IRAP is relatively insensitive to the attitudes and learning histories that it was designed to measure.

Results from our meta-analytic model provided insight into the nature of the generic pattern (see Figures 2 and 3). This appears to take the form of a specific ordinal ranking in mean scores between the four trial types. Our results, based on a far larger sample size and range of domains, suggest that both existing accounts of the generic pattern are incorrect: the pattern is not either a ‘positivity bias’ (O’Shea et al., 2016) or a ‘single trial type dominance effect’ (Finn et al., 2018), but instead represents both differences between positive and negative attributes and also differences between the two categories. This pattern is difficult to attribute to genuine properties of the attitude domains themselves, and is more easily attributed to as-yet-unknown features of the IRAP task itself that elicit such behaviour within the task. The generic pattern among IRAP effects seems to be a replicable effect, but undermines the IRAP’s utility in assessing the implicit attitudes or learning histories that most researchers are interested in when using the task. Put another way, the analysis of mean scores on the IRAP trial types is therefore severely confounded by the generic pattern.

In behavioural terms, the category stimuli appear to exert relatively weak stimulus control over reaction times relative to other, likely less interesting, sources of control. However, it should be noted that these negative implications for the IRAP are agnostic to the level of analysis used by a researcher, whether represenationalist (e.g., that IRAP effects can be used to measures implicit attitudes or associations in memory, etc.) or functional analytic-abstractive (e.g., in terms of relational responding or other concepts developed within Relational Frame Theory; see Barnes-Holmes & Hussey, 2016; Hughes et al., 2011, 2012).

## Generalizability

An important limitation of the current study was that it limited itself to IRAPs that employed evaluative target stimuli. As discussed previously, this choice was necessary in order to make comparison between IRAPs that were relatively similar other than their concept category stimuli. This raises an important question: is there any reason to believe that non-evaluative IRAPs are also affected by a method effect? While the results presented here were limited to evaluative IRAPs, we argue that there is multiple reasons to be concerned about method effects in all IRAPs (i.e., that this is a general method effect). First, theoretical research by other researchers on the causes of generic patterns has already demonstrated effects in non-evaluative IRAPs, suggesting that there is already mutual agreement that such effects are not limited to evaluative IRAPs (e.g., Finn et al., 2018). Second, the types of analyses that are confounded by this method effect are all related to the assumptions that (a) the neutral or zero point (i.e., *D* score = 0) is non-arbitrary and (b) equivalently non-arbitrary between trial types. Conceptual critiques of these assumptions date back to before the first IRAP publication and were aimed at the use of the D score within the Implicit Association Test (Greenwald et al., 2003; for critique of arbitrary metrics see Blanton & Jaccard, 2006). Both of these assumptions have been shown to be violated, first by O’Shea et al.’s (2016) demonstration of effects on a non-word IRAP that participants should demonstrate near zero effects on, and again by our replication of their effect in the current research. Third, as a general point, we argue that a measure’s validity must be demonstrated rather than assumed. The generic pattern among evaluative IRAP effects highlights a method effect within the task. While the specific nature of generic patterns among other types of IRAP may take a different form (e.g., different mean scores between trial types), the underlying method effect is likely to remain (e.g., the aforementioned violation of assumptions remains to be the case). In the next section, we highlight types of analyses whose results are confounded by this method effect. Researchers who argue that it is valid to these analyses should provide evidence that these analyses and their assumptions (e.g., the non-arbitrariness of the zero point) are valid. In summary, we acknowledge that the current analyses were limited to evaluative IRAPs, but caution against the conclusion that the implications of the method effect therefore have limited generalizability.

## Implications for the validity of conclusions in the published literature

The presence of this generic pattern is problematic for most research using the IRAP. Generally speaking, when researchers use the IRAP in their research, they wish to use the task to help explain another phenomenon of interest (i.e., behaviour within the IRAP functions as the thing that explains: the *explanans*) rather than in order to investigate behaviour within the IRAP itself (i.e., where behaviour within the IRAP represents the thing to be explained: the *explanandum*), although exceptions do exist (Finn et al., 2016, 2018; O’Shea et al., 2016). In the IRAP’s modal use-case, the presence of a generic pattern is likely to represent a strong barrier to the task being useful to their goals. And, more worryingly, the generic pattern may cause researchers to make invalid inferences, by misattributing the presence of IRAP effects to attitudes or learning histories (i.e., driven by the relations among category stimului and evaluative stimuli) when they are instead merely instances of the generic pattern. For example, Hussey, Daly, et al. (2015) concluded that normative participants demonstrated counter-intuitive positive evaluations of death. In light of the generic pattern among IRAP effects, it would be more accurate – and less interesting – to characterize these results merely as ‘the generic pattern among IRAP effects was observed’, with no reference to what this might say about the original domain of interest.

The existence of a generic pattern has significant implications for how the results of past and future IRAP studies should be interpreted. Indeed, many of the conclusions made in the published literature may be undermined or invalidated due to the confound that the generic pattern represents. To understand why this is the case, consider that, by definition, the generic pattern means that non-zero IRAP effects are likely to be observed regardless of whether participants possess attitudes or learning histories that would previously be expected to be the source of such IRAP effects. As such, the presence of IRAP effects – that is, *D* scores that are significantly different from zero – cannot reasonably be equated with evidence for implicit attitudes (i.e., at the cognitive level of analysis) or learning histories involving the category stimuli (i.e., at the behavioural analytic-abstractive level). Analyses that treat *D* = zero as a reference point, even tacitly, will therefore produce false or invalid domain level inferences. Somewhat unfortunately, this concern about the validity of conclusions when a *D* score at or near zero is treated as meaningful have been around as long as the IRAP itself (Blanton & Jaccard, 2006). However, these have previously been conceptual arguments, where the current work is empirical.

*Table 1.* A description of commonly-used methods of analysis for IRAP data, as well as the validity of the inferences which are typically made from them.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| Comparison | Example research question | Analytic method | Common inference | Conclusions |
| Mean *D* scores from a single trial-type compared against 0 | Is the “White people – positive” *D* score significantly different from zero? | One-sample *t*-test | A “White people – positive” bias was observed. | Confounded |
| Mean *D* scores within-subject compared between-trial types | Does participant 1’s “White people – positive” *D* score differ from their “White people – negative” *D* score? | Within-subjects *t*-test/ANOVA | “White people – positive” biases were larger than “White people – negative” biases. | Confounded |
| Mean *D* scores from a given trial-type and participant compared between time points | Do *D* scores on the “white people – positive” differ between timepoints 1 and 2? | Within-subjects *t*-test/ANOVA | “White people – positive” bias changed between timepoints/after the intervention. | Unaffected |
| Mean *D* scores from a given trial type compared between-subjects | Do effects on the “White people – positive” *D* score differ between Black and White participants? | Between-subjects *t*-test/ANOVA | White people demonstrated a larger “White people – positive” bias than Black people. | Unaffected |
| *D* scores from a given trial type correlated with other trial-types | Are “White people – positive” *D* scores negatively associated with “White people – negative” *D* scores? | Correlation/regression | Positive evaluations of White people are negatively associated with negative evaluations of White people. | Unaffected |
| *D* scores from a given trial type correlated with external variables | Are “Black people – negative” *D* scores positively associated with self-reported racism? | Correlation/regression | Negative evaluations of Black people on the IRAP and in a self-reported racism scale are positively associated. | Unaffected |
| *Notes:* Conclusions refers to the validity of substantive domain-level conclusions in light of the existence of the generic pattern among IRAP effects. | | | | |

In order to explicate which specific types of analyses and conclusions are impacted by the presence of the generic pattern among IRAP effects, we discuss each of the most common forms of analysis of IRAP data in turn. Table 1 provides a summary of the most common comparisons, research questions, analytic methods, and inferences from IRAP data, and the validity of such conclusions in light of the generic pattern. Figure 4 uses results from a hypothetical between-groups IRAP study to illustrates some of these common comparisons and the validity of their domain level conclusions.

**Figure 4.** Statistical comparisons whose substantive conclusions are invalidated by the existence of the generic pattern among IRAP effects.



It is useful to unpack the example comparisons made in Figure 4 in detail in order to understand the validity of their domain level conclusions. In the case of the comparison labelled (a), a researcher might observe that the ‘White people – positive’ trial type showed an effect that was significantly greater than zero (e.g., using a one-sample *t* test). While it is indeed correct to describe the group as having demonstrated a non-zero IRAP effect here, it would be invalid to interpret this as evidence of a substantive domain-specific effect regarding evaluations of ‘White people’ more generally. For example, conclusions such as “the sample evaluated White people positively” would be invalid because this particular IRAP effect would likely be generated regardless of what category stimuli were used (i.e., it is confounded). That is, our understanding of the generic pattern implies that the effect in our hypothetical study likely has little to do with the stimulus category ‘White people’, and therefore no conclusions regarding participant evaluations of ‘White people’ should be made. In general, we therefore recommend that comparisons of IRAP *D* scores against the zero point (e.g., via one sample *t* tests) should be avoided when attempting to make substantive conclusions about the domain being assessed in an IRAP.

For the comparison labelled (b), a researcher might observe that the ‘Black people – negative’ trial type showed an effect that was significantly larger than the ‘Black people – positive’ trial type (e.g., using a paired samples *t* test). They might then make the domain level conclusion that ‘implicit negativity towards Black people is stronger than implicit positivity towards them’. Although initially less obvious, this inference still relies on a common interpretation of the zero point between the two trial types (i.e., that *D* = 0 has some shared domain-level meaning between trial types). However, our results demonstrated that this is not the case, as the generic pattern takes the form of IRAP effects of different magnitudes between trial-types (see Figure 2 and 3). As such, this substantive conclusion would also be invalid. In general, we therefore recommend that comparisons of IRAP *D* scores between trial types within a single IRAP (e.g., via paired-samples *t* tests) should be avoided when attempting to make domain level conclusions.

Finally, for the comparison labelled (c), a researcher might observe that mean effects on the ‘Black people – negative’ trial type were significantly different between control and intervention conditions. They might then conclude that their intervention ‘served to reduce implicit negativity towards Black people’. Because this comparison involves scores on only a single trial-type, with no direct or tacit reliance on interpretation of the zero point, this domain level conclusion would not be invalidated by the existence of the generic pattern among IRAP effects. Similarly, a comparison made within-subjects on the same trial-type (e.g., pre-post intervention) would also remain valid. Although not illustrated in Figure 4, domain-level conclusions of the results of correlations among trial types and between trial types and external variables (e.g., self-report or behavioural tasks) would also not be invalidated by the existence of the generic trial type effect (see Table 1 and Figure 4).

Given that we have argued that many common analyses of IRAP data are confounded by this method effect and therefore likely produce invalid conclusions, it seemed important to assess the prevalence of such analyses in the published literature. Hussey (2023) recently reported a systematic review of published research using the IRAP between 2006 and 2022. We made use of the list of 151 IRAP publications returned as part of that systematic review. We did not attempt to assess whether this affect publications central claims (or were related to primary versus secondary hypotheses, etc.), on the basis that recent meta-scientific research has highlighted that published research typically does not report with sufficient detail either (a) the mapping of the results of specific statistical tests onto specific verbal claims, or (b) report how verbal hypotheses and claims relate to one another or rank in importance (e.g., Scheel, 2022). We therefore limited ourselves to a more reliable and objective assessment: whether publications reported results for specific types of analyses that are likely to be confounded by the method effect (i.e., those list in Table 1 as confounded). The full text of each publication was searched for instances. A maximum of one instance was extracted from each publication: that is, we coded each article as TRUE/FALSE for containing at least one confounded analysis. All results, including the text extracted from each publication and the page number it was taken from, are reported in the supplementary materials. Results demonstrated that 126 of the 151 IRAP publications inspected reported at least one analysis that is likely to be confounded by the method effect, a prevalence of 83.4%. This assessment of the prevalence of confounded analyses doesn’t not imply that all claims made in those publications are invalid, but it does suggest that at least some of the results reported in a large proportion of IRAP publications are undermined to some degree. We readily admit that several articles we ourselves have written were found to contain such confounded analyses, and some of the claims of those articles may now be invalid.

## Conclusions

Evidence from a large dataset of published and unpublished IRAP studies showed that IRAPs examining very different domains – even those using non-words – demonstrate startlingly similar patterns of effects. This finding was in agreement with general conclusions of several recent articles that there is a generic pattern among IRAP effects. However, due to its large sample size relative to the existing IRAP literature, this study was the first to quantify the generic pattern more precisely and to consider its implications for the valid interpretation of published and future IRAP studies. Results demonstrated that majority of variance in effects on evaluative IRAPs is attributable to this generic pattern rather than the domain it is intended to measure. The IRAP is therefore relatively insensitive to the attitudes and learning histories it is intended to assess. This represents a serious confound and has negative implications for the published literature: multiple common analyses of IRAP data are likely to produce invalid domain level conclusions. There is therefore a strong need to systematically evaluate the prevalence of these types of analyses and invalid inferences in the published IRAP literature.

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