Measure What You are Trying to Predict: Applying the Correspondence Principle to the Implicit

Association Test

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Abstract

The Implicit Association Test (IAT) is nearly synonymous with the implicit attitude construct. At the same time, correlations between the IAT and criterion measures are often remarkably low. Developed within research using explicit measures of attitudes, the *correspondence principle* posits that measures should better predict criteria when there is a match in terms of the level of generality or specificity at which both are conceptualized (Ajzen & Fishbein, 1977). As such, weak implicit-criterion correlations are to be expected when broad general implicit measures are used to predict highly specific criteria. Research using explicit measures of attitudes consistently supports the correspondence principle, but conceptual correspondence is rarely considered by researchers using implicit measures to predict behavior and other relevant criterion measures. In five experiments (total $N = 4650$), we provide the first direct evidence demonstrating the relevance of the correspondence principle to the predictive validity of the IAT and Single Target IAT. That said, it is not the case that the IAT *always* predicts criteria better when correspondence is high. Inconsistency across the pattern of results suggests there is much more that remains to be understood about the relevance of the correspondence principle to the implicit-criterion relationship. Taken together, however, our findings suggest that conceptual correspondence typically increases (and never decreases) the magnitude of implicit-behavior and implicit-explicit relationships. We provide a framework for future research necessary to establish when correspondence is more likely to increase the predictive validity of measures such as the IAT.

Keywords: Implicit Association Test, predictive validity, implicit attitudes, correspondence principle

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Attitudes are presumed to be a central determinant of human behavior (e.g., Ajzen, 2011; Allport, 1935). In other words, we expect attitudes to predict a wide range of relevant outcomes. Arguably, the predictive ability of attitudes would increase further if researchers devised methods to bypass measurement error resulting from reliance on self-report measures of attitudes (e.g., concerns with self-presentation and lack of introspective access). Beginning more than 30 years ago with the publication of a measure of affective priming (Fazio, Sanbonmatsu, Powell, & Kardes, 1986), a class of measurement procedures termed “implicit” measures have long promised to do just that. Indeed, there was some hope that the priming measure might act as a “bona fide pipeline” to attitudes (Fazio, Jackson, Dunton, & Williams, 1995). Optimism about the usefulness of implicit measures only intensified with the subsequent publication of the Implicit Association Test (IAT: Greenwald, McGhee, & Schwartz, 1998), a measure that has since dominated the literature on implicit attitudes due, at least in part, to its relatively strong psychometric properties (Bar-Anan & Nosek, 2014). Meta-analyses of the IAT’s predictive validity, however, have consistently found rather weak statistical relationships between implicit measures and criterion measures (Greenwald, Poehlman, Uhlmann, & Banaji, 2009; Kurdi et al., 2018; Oswald, Mitchell, Blanton, Jaccard, & Tetlock, 2013).

That said, we have been down this road before. Controversy about the predictive validity of attitude measures is not new and it is not unique to implicit measures. Half a century ago, Wicker (1969) sparked a crisis among attitude researchers with his comprehensive review of studies demonstrating inconsistent and weak relationships between attitudes and behaviors. Although some researchers were ready to declare the death of the attitude construct as a useful

predictor of behavior, others were motivated to better understand what factors explained the observed inconsistencies between attitudes and behaviors (Kelman, 1974). In the decades following Wicker's review, researchers identified several important methodological and conceptual moderators of the relationship between attitudes and behaviors (see Ajzen & Fishbein, 2005). One of the most important theoretical advances made during this time was the development of the *correspondence principle*, which posits that the magnitude of correlations between attitudes and behaviors depends on the extent to which they correspond in terms of their generality vs. specificity (Ajzen & Fishbein, 1977).

Ajzen and Fishbein noted that in many studies finding weak attitude-criterion relationships, researchers attempted to predict a single specific behavior (e.g., support for a tax on plastic bottles) from a broad and general attitude (e.g., attitudes toward the environment). Although general attitudes should be able to predict behaviors that are measured at a similarly general level, it is unreasonable to expect broad general attitudes toward a target to excel at predicting any single specific behavior related to the attitude object. They may do so, but they are unlikely to do so *well*. To predict a specific behavior well, attitudes must be measured at a similarly specific level. Indeed, whereas there tend to be weak correlations between general self-reported attitudes and specific behaviors, there are relatively large correlations between specific behaviors and highly correspondent explicit attitudes (Kraus, 1995).

In sum, although the relationship between attitudes and behaviors has historically been contentious, the correspondence principle allowed researchers to address many of the concerns and maximize their ability to use attitudes to predict behaviors and other relevant outcomes. Despite this, the importance of the correspondence principle continues to be under-appreciated by attitude researchers (Ajzen, 2011).

Conceptual Correspondence and Implicit Measures

In the context of implicit attitudes, a few prominent researchers have recently recognized the theoretical relevance of conceptual correspondence (Blanton, Burrows, & Jaccard, 2016; Gawronski, 2019; Gawronski & Brannon, 2017; Jaccard & Blanton, 2007; Jost, 2019; Kurdi et al., 2018). Nevertheless, the correspondence principle has been investigated almost exclusively within meta-analyses of implicit-criterion relationships. Further, meta-analytic findings are mixed, dependent on idiosyncratic methodological and analytical decisions, and reliant on existing studies that tend to be both underpowered and of variable quality (Greenwald et al., 2009; Hofmann, Gawronski, Gschwendner, Le, & Schmitt, 2005; Kurdi et al., 2018; Oswald et al., 2013). Clear agreement exists that the extant research examining implicit-criterion relationships failed to adequately consider the correspondence principle. For example, Oswald et al. (2013) noted that correspondence was so uniformly low in the existing literature that it could not be included as a meta-analytic moderator at all. Additionally, both Gawronski (2019) and Jost (2019) argue that the widespread use of non-correspondent implicit and criterion measures has likely led to systematic meta-analytic underestimations of the magnitude of implicit-criterion correlations.

Despite the consensus regarding its importance, direct empirical tests of the correspondence principle using implicit measures are almost non-existent (for exceptions see Blanton et al., 2016; Payne, Burkey, & Stokes, 2008). Complementing meta-analytic conclusions by systematically testing the influence of conceptual correspondence on the magnitude of implicit-criterion correlations is more than overdue. In three sets of experiments, we tested the following key predictions about the influence of target correspondence on implicit measures' abilities to predict relevant criterion measures:

H1: A specific criterion will be better predicted by a specific IAT than by a general IAT (between-subjects).

H2: A specific IAT will predict a specific criterion better than a general criterion (within-subjects).

H3: A general criterion will be better predicted by a general IAT than by a specific IAT (between-subjects).

H4: A general IAT will predict a general criterion better than a specific criterion (within-subjects).

Notably, H1 and H2 (and, likewise, H3 and H4), ask a related question about the correspondence principle in a slightly different way. Namely, H1 (and H3) ask the question of whether a single outcome is better predicted by one of two different IATs. In comparison, H2 (and H4) ask whether an individual IAT better predicts one of two different outcomes. Results from each of these four hypotheses hold value in terms of understanding how evaluations eventuate in behavior, in contextualizing and qualifying each set of results, and providing a more complete test of the correspondence principle as it applies to implicit-criterion relationships. Further, unexpected asymmetric findings between the parallel sets of hypotheses may have theoretical and methodological implications.

Peripheral Aims of the Current Experiments

We designed our experiments primarily to test a simple (or single) association pattern of predictions (see Perugini, Richetin, & Zogmaister, 2010) because our goal was to test the relationship between implicit measures and criterion measures. There are other considerations, however, such as how strongly implicit measures relate to explicit attitude measures, and whether they predict criterion above and beyond explicit measures (i.e., incremental validity).

Although these additional concerns were not our central focus, we did measure some form of explicit attitudes in most experiments. As such, we report exploratory analyses incorporating explicit attitudes to test 1) the influence of correspondence on implicit-explicit relationships and 2) issues related to incremental validity.

Insofar as it is possible with our data, we present exploratory results from two different applications of the correspondence principle to incremental validity. The first approach is to test whether the IAT predicts unique variance in criteria above and beyond one explicit measure that corresponds highly with the content of the IAT (e.g., Kurdi et al., 2018). The second approach is to test whether the IAT predicts unique variance above and beyond any available explicit attitude measures that correspond in any way with either the implicit or criterion measure (Blanton et al., 2017).

All supplemental analyses are available at the following anonymized (for peer-review) OSF page unless otherwise noted:

https://osf.io/xehfu/?view_only=6b131b49580340be99d0a92d043e2a40.

Analysis Plan

Although our hypotheses would most commonly be tested by comparing correlations or moderated regression, there are several reasons why these approaches are suboptimal for the current data. The problem involves the patterns of measurement error and variances across the general and specific IATs within each experiment. Making direct comparisons across two measures with different reliabilities attenuates regression coefficients for the less reliable measure (Little, Card, Bovaird, Preacher, & Crandall, 2007). This can produce a false interaction effect or hide a true interaction effect. Unequal reliabilities are especially problematic when the variances of the predictor variable (IAT scores) across the dichotomous moderator variable (IAT

type) are different (Kenny, 2015). Failure to accurately assess and account for implicit measures' reliabilities is a key issue that has remained largely unaddressed (Kurdi et al., 2018; LeBel & Paunonen, 2011). In this case, multiple groups SEM is one solution as it allows for the two regression slopes to be compared without measurement error and should thus minimize bias (e.g., Kline, 2012).

Factor loading invariance. Meaningfully comparing regression slopes in multiple groups SEM requires the factor loadings to be (reasonably) invariant across groups. In other words, each factor indicator should contribute to the latent construct similarly for both IATs. Constraining factor loadings puts both measures in the same units of measurement, thus allowing for cross-group regression coefficient comparisons (Chen, 2008; Guenole & Brown, 2014; Schmitt, Golubovich, & Leong, 2011). If factor loadings are not invariant, however, constraining them across groups may be problematic because the measures can not necessarily be compared on the same metric.

Establishing factor loading invariance requires that model fit does not worsen significantly after constraining factor loadings across groups (Putnick & Borstein, 2016). To assess invariance, we examined modification indices in a fully constrained model (i.e., one in which the factor loadings for two different IATs were constrained to be equal) and then used a backward approach to release potentially non-invariant loadings (Jung & Yoon, 2016). Although this approach is limited on its own, we report only this test in the manuscript as full reporting of additional tests of invariance is unwieldy. Similarly, we only report the test for IAT factor loadings despite applying constraints for the whole model because there is no strong reason to believe that criterion measures would operate differently for the different groups, especially with counterbalancing of implicit and self-report measures (i.e., only half the participants could have

their self-reports affected by the content of the randomly assigned general or specific IAT).

Output, MPlus syntax, and chi-square differences for other invariance tests (e.g., examining alternative fit indices) are available on this manuscript's OSF page.

Reference group scaling. Obtaining standardized regression coefficients separately for each group is uninformative in multiple groups SEM because they do not incorporate invariance constraints nor the different variances for the predictor variables at each level of the moderator. Yet, unstandardized coefficients are not easily interpretable. To address this, we used a model identification method that scaled the latent variables to be standardized in reference to one of the groups. For model identification, the means and variances for each latent variable were fixed at 0 and 1 in one reference group (general IATs) and freely estimated in the other focal groups (specific IATs). Identifying the model in this way also requires one factor loading and its intercept to be set equal within each latent variable across the groups. The result is that, within each experiment, the general IATs' coefficients are standardized and the specific IATs' units are scaled to the same metric (i.e., they are in the same units as the general IATs' standardized units). As such, the regression coefficients are directly comparable within each experiment and can be interpreted in the same way as more familiar standardized betas.

Estimation method. All analyses used maximum likelihood estimation with robust standard errors in MPlus (Muthén & Muthén, 2017). Correspondingly, all chi-square difference tests used Satorra-Bentler scaled chi-square values. This estimation method is relatively robust to even severe departures from multivariate non-normality (Kline, 2012).

Overview of Experiments

We tested the relevance of the correspondence principle to the relationship between implicit attitudes and criterion measures (e.g., explicit attitudes, self-reported behavior, policy

preferences) in a series of five experiments across two attitude domains. The first three experiments used either the IAT (Exp. 1a) or the ST-IAT (Exps. 1b and 1c) to predict criterion from implicit attitudes toward the general category of fruit (Exps. 1a, 1b, 1c) or the more specific categories of bananas (Exps. 1b and 1c) and cantaloupe (Exps. 1a, 1b, 1c). The last two experiments compared the effectiveness of an Immigrants IAT (general) and a Border Wall IAT (specific) to predict explicit attitudes (Exp. 2b) and policy preferences (Exps. 2a and 2b).

We report how we determined our sample size, all data exclusions, all manipulations, and all measures for all experiments in this manuscript.

Experiment 1a: Fruit/Cantaloupe IATs

To test the correspondence principle's predictions, we directly manipulated IAT specificity by creating a Fruit IAT (general) and a Cantaloupe IAT (specific). We then measured general and specific self-reported fruit and cantaloupe consumption behaviors and explicit attitudes. All participants completed *either* the Fruit IAT or the Cantaloupe IAT, but all participants completed *all* the fruit- and cantaloupe-related self-report measures. In other words, we manipulated general vs. specific IATs between-subjects and general vs. specific outcomes within-subjects. To support H1, the Cantaloupe IAT should be better than the Fruit IAT at predicting cantaloupe consumption. For H2, the Cantaloupe IAT should predict cantaloupe consumption better than it predicts fruit consumption. For H3, the Fruit IAT should be better than the Cantaloupe IAT at predicting fruit consumption. To support H4, the Fruit IAT should predict fruit consumption better than it predicts cantaloupe consumption.

Method

Participants and Procedures

Participants were volunteers from the Project Implicit website (<https://implicit.harvard.edu>). We pre-registered a total sample of 1000 participants based on a priori power analyses aimed at providing 80% power to detect a small interaction effect ($f^2 = .01$, suggested $N = 787$), but with oversampling to account for planned exclusions and the additional power needed to detect continuous by categorical interactions (Aguinis, Beaty, Boik, & Pearce, 2005; Kenny, 2018; McClelland & Judd, 1993).

After choosing to visit the Project Implicit website and participate in a study, participants were randomly assigned to complete one of several studies in the pool. Data collection was terminated automatically upon reaching the target number of completed studies.¹ Of the 997 participants in the completed data sets, 936 remained after planned method- and data-based exclusions. Participants were excluded if they had excessive errors on the IAT (40% errors in any critical block, 30% errors overall, or 10% trials faster than 300ms) or if they were missing data for more than one item within either the fruit or cantaloupe consumption scales. In the Cantaloupe IAT group, we excluded 27 participants for IAT errors and eight for excessive missing self-report data. In the Fruit IAT group, we excluded 21 for IAT errors and four for missing data. Of the remaining participants, 51% reported being female, 72% reported being White, and were 38.6 years old ($SD = 14.5$) on average. Participants were randomly assigned to complete either a Fruit IAT or a Cantaloupe IAT, but completed all self-report fruit and cantaloupe measures. Order of the IAT and self-report measures were counterbalanced, and the order of self-report measures and their items were randomized.

Measures

Implicit Association Tests. The Fruit and Cantaloupe IATs were identical except for their category labels ('Fruit' or 'Cantaloupe') and their stimuli – either six images of cantaloupe

or six images of different fruits. Both the Fruit and Cantaloupe IATs used ‘Other foods’ as the comparison category. The ‘Other foods’ stimuli included six images of non-fruit foods. Both IATs used ‘I Like’ and ‘I Don’t Like’ as labels for the attribute categories. Using these labels means we constructed what is generally referred to as a Personalized IAT (Olson & Fazio, 2004), although ours is a hybrid version as we also included error feedback (as in Han, Olson, & Fazio, 2006, Experiment 2; Smith, De Houwer, & Nosek, 2013, Study 2). Stimuli for the categories were 10 words synonymous with ‘Like’ and ‘Dislike’. Internal consistencies for the manifest IAT scores, calculated from the average of 600 split-half correlations, were .65 for the Fruit IAT and .77 for the Cantaloupe IAT (obtained from <https://iatmeta.shinyapps.io/relicalc/>).

Each IAT had seven blocks where participants sorted target and attribute stimuli into various combinations that appeared on their computer screen. The first two blocks each included 20 practice trials for the target categories (Block 1) and the attribute categories (Block 2). A subsequent practice block (Block 5) had 28 practice trials; trials were identical to Block 1, but with the position of the target categories reversed. There were 120 total critical trials across Blocks 3, 4, 6, and 7. To facilitate SEM analyses, we calculated three IAT scores with the D_2 algorithm (Greenwald, Nosek, & Banaji, 2003; Nosek & Smyth, 2007). The latent IAT indicators had 40 trials each. The first indicator had all 20 trials from both B3 and B6; the second and third indicators had the first 20 and last 20 trials from both B4 and B6, respectively. Trials with response times greater than 10,000ms or less than 400ms were dropped, but we kept latencies from trials in which participants made an error (i.e., initially sorted the stimulus into the wrong category before subsequent correction).

Self-report behaviors and intentions. Fruit and cantaloupe consumption were each measured with four items intended to capture subjective and objective eating behaviors. We

asked participants how often they ate fruit and cantaloupe in the last week, in general, and how much they expected to eat fruit in the next week and in the future, generally. Response options ranged from 1 to 8 (or 0 to 7 days). For the manifest cantaloupe consumption variable, internal consistencies were $\alpha = .86$ and $\alpha = .88$ for the Fruit IAT and Cantaloupe IAT groups, respectively; for fruit consumption, alphas were .93 in both groups.

Explicit attitudes. Explicit fruit and cantaloupe attitudes were measured using three items (e.g., “How much do you like eating cantaloupe [fruit]?”), “How much do you enjoy eating cantaloupe [fruit]?”, and “How positive are your attitudes toward eating cantaloupe [fruit]?”. Response options ranged from 1 to 7. The alphas for the manifest explicit cantaloupe attitude scales were .97 and .96 in the Fruit and Cantaloupe IAT groups, respectively; alphas for explicit fruit attitudes were .90 and .91.

Results

Tests of Invariance

We set up the models for each group with the IAT as the only exogenous (i.e., predictor) variable and the four behavior/attitude outcomes as endogenous (i.e., outcome) variables. The only difference between the models for each group was IAT type (Fruit vs. Cantaloupe). Each group’s separate chi-square contributions to the overall model are available in supplemental materials, but the separate models fit well and both groups contributed similar chi-square values to the overall model. See Table 1 for factor loadings prior to any between-group constraints other than the marker variables.

After freely estimating two residual covariances with extremely high modification indices within both group’s fruit and cantaloupe behavior factors², the configural model’s fit was excellent, $\chi^2(214) = 419.685, p < .001, CFI = .981, RMSEA = .045 [.039, .052]$.

Table 1

Factor loadings for Fruit IAT and ST-IAT Experiments (1a-1c)

	<i>Fruit</i>		<i>Cantaloupe</i>		<i>Banana</i>	
	<i>Std. (SE)</i>	<i>Unstd. (SE)</i>	<i>Std. (SE)</i>	<i>Unstd. (SE)</i>	<i>Std. (SE)</i>	<i>Unstd. (SE)</i>
<i>IAT</i>						
Trials 1-20 (B3/B6)	.55 (.05)	0.23 (.02)	.74 (.03)	0.37 (.03)		
Trials 21-40 (B4/B7)	.85 (.04)	0.42* (.02)	.86 (.02)	0.42* (.02)		
Trials 41-60 (B4/B7)	.75 (.03)	0.36 (.02)	.77 (.03)	0.38 (.03)		
<i>ST-IATs (Combined)</i>						
Trials 1-24 (B2/B4)	.50 (.06)	0.19 (.02)	.47 (.05)	0.16 (.03)	.56 (.05)	0.21 (.03)
Trials 25-48 (B2/B4)	.62 (.04)	0.23* (.02)	.64 (.05)	0.24* (.02)	.64 (.05)	0.23* (.02)
Trials 49-72 (B3/B5)	.65 (.05)	0.25 (.02)	.47 (.05)	0.17 (.03)	.55 (.05)	0.20 (.03)
Trials 73-96 (B3/B5)	.60 (.05)	0.22 (.02)	.52 (.05)	0.20 (.04)	.45 (.05)	0.17 (.03)

* indicates marker variable constrained between groups to set the scale for the factor variance

Factor loading invariance. Model fit was significantly worse after constraining the IAT factor loadings, $\Delta\chi^2(2) = 18.198, p < .001$. Modification indices suggested that the first IAT factor loading was potentially non-invariant. Model fit significantly improved after freely estimating that loading, $\Delta\chi^2(1) = 16.246, p < .001$. Ignoring this non-invariance shifts the Cantaloupe IAT coefficients slightly downward relative to the Fruit IAT compared to a model with the factor loadings unconstrained. Otherwise, there appears to be no substantive difference in results between invariance ignored and partial invariance models (see supplemental analyses). Constructs can reasonably be compared if invariant and non-invariant models converge on similar results (Chen, 2008; Guenole & Brown, 2014). Nevertheless, identifying this source of measurement non-invariance is informative because it provides insight into one way the two IATs' psychometric properties differ. We address this in the General Discussion.

Hypothesis Tests

We tested the substantive hypotheses by comparing regression coefficients between- and within-subjects. The between-subjects comparisons tested whether the general-level Fruit IAT

and the specific-level Cantaloupe IAT outperform one another when predicting correspondingly general (fruit) or specific (cantaloupe) outcomes; the within-subjects comparisons tested whether a single IAT (either the Fruit IAT or the Cantaloupe IAT) excels at predicting the general vs. specific measures.

To increase readability, we report p -values and other parameters only sparingly in the main text. Table 2 contains regression coefficients, standard errors, and 95% confidence intervals for all relevant implicit-criterion and implicit-explicit relationships. Table 3 provides detailed information about the chi-square difference tests for the between-subjects and within-subjects comparisons.

Factor means and variances. Factor means and variances are reported relative to the Fruit IAT ($M = 0$, $s^2 = 1$). People had relatively less favorable implicit cantaloupe attitudes ($M = -.952$, $s^2 = 1.677$). The Cantaloupe IAT scores varied significantly more than Fruit IAT scores, $\Delta\chi^2(1) = 22.145$, $p < .001$.

Predicting behavior. The Fruit IAT significantly predicted fruit consumption but not cantaloupe consumption. Conversely, the Cantaloupe IAT significantly predicted cantaloupe consumption but not fruit consumption.

Behavior: Between-subjects. The pattern of between-subjects differences (general vs. specific IATs predicting one outcome) was consistent with the correspondence principle. In support of H1, the Cantaloupe IAT predicted cantaloupe consumption significantly better than the Fruit IAT and, in support of H3, the Fruit IAT predicted fruit consumption significantly better than the Cantaloupe IAT.

Table 2

Regression Coefficients, Standard Errors, and 95% Confidence Intervals for Experiments 1a-1c

<i>Outcome</i> /Predictor	IAT (Exp. 1a)				ST-IATs (Exps. 1b/1c)			
	<i>B</i>	<i>S.E.</i>	95% CI	<i>p</i>	<i>B</i>	<i>S.E.</i>	95% CI	<i>p</i>
<i>Fruit Behavior</i>								
Fruit IAT	.14	.06	[.05, .24]	.012	.13	.05	[.03, .23]	.012
Cantaloupe IAT	-.03	.04	[-.09, .04]	.497	.01	.05	[-.09, .12]	.800
Banana IAT	--	--	--		.15	.05	[.04, .26]	.006
<i>Cantaloupe Behavior</i>								
Fruit IAT	.07	.05	[-.02, .16]	.200	.14	.05	[.03, .24]	.011
Cantaloupe IAT	.25	.04	[.18, .32]	.001	.19	.06	[.07, .31]	.001
Banana IAT	--	--	--		.08	.06	[-.03, .19]	.135
<i>Banana Behavior</i>								
Fruit IAT	--	--	--		.11	.05	[.01, .22]	.037
Cantaloupe IAT	--	--	--		-.01	.06	[-.12, .10]	.901
Banana IAT	--	--	--		.19	.05	[.08, .29]	.001
<i>Fruit Attitudes</i>								
Fruit IAT	.17	.05	[.08, .26]	.002	.09	.05	[-.01, .20]	.089
Cantaloupe IAT	-.02	.04	[-.09, .05]	.664	.03	.05	[-.07, .14]	.549
Banana IAT	--	--	--		-.01	.05	[-.10, .09]	.896
<i>Cantaloupe Attitudes</i>								
Fruit IAT	.09	.05	[.00, .17]	.071	.10	.05	[.00, .24]	.051
Cantaloupe IAT	.30	.04	[.23, .36]	.000	.28	.06	[.17, .40]	<.001
Banana IAT	--	--	--		.07	.05	[-.03, .19]	.169
<i>Banana Attitudes</i>								
Fruit IAT	--	--	--		.06	.05	[-.04, .16]	.238
Cantaloupe IAT	--	--	--		-.02	.05	[-.13, .08]	.677
Banana IAT	--	--	--		.15	.05	[.05, .25]	.003

Note. Fruit IAT and ST-IAT coefficients are in standardized units. Cantaloupe IAT coefficients are in units standardized to the Fruit IAT; Cantaloupe/Banana ST-IAT are standardized to the Fruit ST-IAT.

Table 3

Chi-Square Difference Tests for Experiments 1a, 1b, and 1c

	IAT (Exp. 1a)		ST-IAT (Exps. 1b/1c)	
	$\Delta\chi^2$	<i>p</i>	$\Delta\chi^2$	<i>p</i>
Between-subjects comparisons (Two IATs predicting one outcome measure)				
<i>Predicting specific behavior</i>				
Cantaloupe IAT > Fruit IAT	7.276	.007	0.496	.481
Banana IAT > Fruit IAT	--	--	0.975	.324
<i>Predicting general behavior</i>				
Fruit IAT > Cantaloupe IAT	6.149	.013	2.575	.108
Fruit IAT > Banana IAT	--	--	0.074	.785
<i>Predicting specific attitudes</i>				
Cantaloupe IAT > Fruit IAT	10.977	.001	5.973	.015
Banana IAT > Fruit IAT	--	--	1.625	.202
<i>Predicting general attitudes</i>				
Fruit IAT > Cantaloupe IAT	7.604	.006	1.541	.447
Fruit IAT > Banana IAT	--	--	1.757	.185
Within-subjects comparisons (One IAT predicting two outcomes measures)				
<i>Fruit IAT</i>				
Behavior: Fruit > Cantaloupe	1.257	.262	0.112	.738
Behavior: Fruit > Banana	--	--	0.132	.716*
Attitudes: Fruit > Cantaloupe	1.408	.235	0.041	.840
Attitudes: Fruit > Banana	--	--	0.422	.516
<i>Cantaloupe IAT</i>				
Behavior: Cantaloupe > Fruit	29.960	< .001	6.164	.013
Attitudes: Cantaloupe > Fruit	32.473	< .001	15.433	< .001
<i>Banana IAT</i>				
Behavior: Banana > Fruit	--	--	0.024	.877
Attitudes: Banana > Fruit	--	--	2.421	.120

* indicates effect went opposite from the predicted direction

Behavior: Within-subjects. The within-subjects comparisons (one IAT better predicting one of two dependent measures) were consistent with the correspondence principle for the specific IAT, but not for the general IAT. In support of H2, the Cantaloupe IAT predicted cantaloupe behavior significantly better than fruit behavior. Although the difference was in the predicted direction, H4 was not supported as the Fruit IAT only trended toward better predicting fruit behavior compared to cantaloupe behavior.

Predicting explicit attitudes. The same approach tested between- and within-subjects relationships when predicting explicit attitudes from implicit measures. The pattern of significance was identical to what was found with behaviors. Additionally, the regression coefficients were similar in size, but slightly higher overall for implicit-explicit compared to implicit-behavior relationships. See Tables 2 and 3 for regression coefficients and chi-square difference tests.

Incremental validity. Neither IAT predicted unique variance in any criterion measure after controlling for their directly correspondent explicit attitudes (i.e., Fruit IAT after controlling for fruit attitudes; Cantaloupe IAT controlling for cantaloupe attitudes). In contrast, in both cases, explicit attitudes uniquely predicted criterion measures; analyses are available in supplemental materials.

Discussion

Overall, results from Experiment 1a supported the correspondence principle. That is, a general-level Fruit IAT and a specific-level Cantaloupe IAT differed in their abilities to predict criterion measures conceptualized at correspondingly general or specific levels (i.e., fruit or cantaloupe behaviors). For predictions at the specific level, the Cantaloupe IAT outperformed the Fruit IAT in predicting cantaloupe consumption (H1), indicating that it is possible to improve on

the prediction of specific behavior by changing the focus of the IAT to be correspondingly specific. Additionally, the Cantaloupe IAT was better at predicting cantaloupe vs. fruit consumption (H2). In this case, the specific implicit measure exemplified the correspondence principle — the Cantaloupe IAT not only predicted cantaloupe behavior reasonably well, but also failed to predict fruit behavior.

For predictions at the general level, the Fruit IAT outperformed the Cantaloupe IAT when predicting overall fruit consumption (H3). There was not clear support for H4, as the Fruit IAT did not significantly differ in its ability to predict fruit versus cantaloupe consumption. This non-significant difference did, however, trend in the predicted direction. It is plausible that because general-level implicit attitudes tend to predict specific criterion (at least weakly), the difference between general implicit attitudes' ability to predict general and specific outcomes may tend to be small relative to the magnitude of differences for specific implicit attitudes. In this case, the Fruit IAT predicted even its correspondingly general criterion measure very poorly, thus making it especially unlikely that it could predict fruit behavior significantly better than any other criteria at all.

When predicting explicit attitudes, relationships followed a pattern identical to the implicit-criterion relationships (both in direction and significance of differences). Additionally, neither IAT demonstrated any incremental validity in predicting behavior after controlling for explicit attitudes. Both of these findings are relatively unsurprising given that explicit attitudes and their correspondent behaviors approached redundancy for both fruit and cantaloupe ($r = .72$ and $r = .77$).

Experiments 1b and 1c: Fruit, Banana, and Cantaloupe ST-IATs

In addition to Experiment 1a, we ran two other very similar studies³. The key difference from Experiment 1a is that we used ST-IATs and included bananas as a more highly representative fruit exemplar in addition to the less representative cantaloupe. Each study was underpowered on its own because we underestimated the sample sizes needed to adequately test for moderation. Both studies used practically identical measures and converge on approximately the same findings, so we merged their data for brevity and to increase power. Factor loadings, regression coefficients, and chi-square difference tests separated for the two ST-IAT experiments are available in Supplemental Tables S1-S3.

Method

Participants and Procedures

We pre-registered a target sample of 990 participants for both Exp 1b and Exp 1c, intended to provide enough power each experiment to detect a medium sized interaction effect ($f^2 = .05$). As in Experiment 1a, all participants were randomly assigned to complete only one ST-IAT (Fruit, Banana, *or* Cantaloupe), but all participants completed *all* self-report measures (Fruit, Banana, *and* Cantaloupe). Data collection terminated automatically upon reaching the target number of completions (with a small margin of error).

Experiment 1b. Of the 994 participants who completed the experiment, 952 remained after making method- and data-based exclusions. We excluded 24 participants for ST-IAT errors (5 fruit, 9 banana, 10 cantaloupe) and 18 additional participants for excessive missing data (9 fruit, 4 banana, 5 cantaloupe). Participants were 75.9% white, 68.4% female and had an average age of 35.08 years old ($SD = 15.50$).

Experiment 1c. Of the 993 participants who completed the experiment, 960 remained after method- and data-based exclusions. We excluded 17 participants for ST-IAT errors (6 fruit,

6 banana, and 5 cantaloupe) and 16 additional participants for excessive missing data (8 fruit, 7 banana, 1 cantaloupe). Participants were 68.9% white, 66.5% female, and 33.18 years old on average ($SD = 14.09$).

Measures

Single-Target IATs. In Experiment 1b, we used a Traditional ST-IAT with ‘Fruit’, ‘Cantaloupe’, or ‘Banana’ as the category label and ‘Good words’ and ‘Bad words’ as the attribute labels. In Experiment 1c, we used a Personalized ST-IAT with ‘Eating Fruit/Eating Cantaloupe/Eating Banana’ as category labels and ‘I Like’ and ‘I Don’t Like’ as the attribute labels. We made two changes in the hopes of increasing the uniformly low relationships between the ST-IAT and criterion measures. We added ‘eating’ to the category labels to increase conceptual correspondence between the implicit and criterion measures. We personalized the attribute labels because we reasoned that personally liking eating fruit was more relevant to whether a person would actually eat fruit than merely feeling that eating fruit was good. The Personalized ST-IATs trended toward having uniformly stronger implicit-explicit and implicit-criterion correlations, but the patterns of findings were otherwise nearly identical for the two different ST-IATs (see supplemental materials, Tables S1-S3).

Each ST-IAT had 20 practice trials where participants sorted one attribute label (e.g., ‘I Like’) on one key and the other (e.g., ‘I Don’t Like’) on the other (Block 1). After that, there were four critical blocks of 48 items each (Blocks 2-5). In each block, participants sorted target stimuli (e.g., fruit) and attribute stimuli (e.g., ‘I Like’) on one key, and only stimuli for the other attribute (e.g., ‘I Don’t Like’) on another key. Blocks 2/3 and Blocks 4/5 shared the same target/attribute configuration. We created four latent indicators from each paired set of 24 trials from B1/B3 and B2/B4. For example, the first indicator was an IAT score calculated from the

first 24 trials of B1 and the first 24 trials of B3, the second indicator used the last 24 trials of B1 and B3, etc. The ST-IATs had much lower internal consistencies than their IAT counterparts. For the Personalized ST-IATs, the internal consistencies for the Fruit, Banana, and Cantaloupe ST-IATs were .49, .44, and .37, respectively (<https://iatmeta.shinyapps.io/relicalc/>). For the Traditional ST-IATs, the internal consistencies for the Fruit, Banana, and Cantaloupe ST-IATs were .42, .45, and .43, respectively.

Criterion: Self-reported behavior and explicit measures. Response options for all explicit attitudes and behavior that were identical in Experiment 1a and 1c, but the response options and item wordings varied slightly for Experiment 1b. Specifically, the explicit attitude items did not include ‘eating’ in their stems. For example, “How much do you like cantaloupe?” (Exp. 1b) instead of “How much do you like *eating* cantaloupe?” (Exps. 1a and 1c). In both Experiments 1b and 1c, we collected one additional ‘behavior’ item in which we asked participants to imagine how likely they would be to eat fruit, cantaloupe, or bananas at that moment. We excluded this item from the analyses to make the behavior items identical across all three fruit experiments.

Results

Tests of Invariance

We simultaneously aligned the factor loadings for the Fruit, Banana, and Cantaloupe ST-IATs in a single multiple groups SEM model. This put all the regression coefficients in a common metric for the three ST-IATs (once factor loadings were constrained across groups). See Table 1 for ST-IAT factor loadings prior to imposing constraints between the Fruit, Banana, and Cantaloupe ST-IAT factor loadings. The configural model had excellent overall fit, $\chi^2(753) = 1833.697, p < .001, CFI = .970, RMSEA = .048 [.045, .050]$. Constraining factor loadings across

the three ST-IATs did not significantly worsen model fit, $\Delta\chi^2(6) = 9.413, p = .152$. Moreover, modification indices from the constrained model did not identify any potentially non-invariant loadings.

Hypothesis Tests

As in the previous experiment, all regression coefficients, standard errors, 95% confidence intervals, and p -values are in Table 2. Table 3 has detailed information about the chi-square difference tests for the between-subjects and within-subjects comparisons.

ST-IAT means and variances. Means and variances are standardized in reference to the Fruit ST-IAT ($M = 0, s^2 = 1$). Implicit cantaloupe attitudes ($M = -0.199, s^2 = .873$) and implicit banana attitudes ($M = -0.175, s^2 = .759$) were both slightly less favorable than implicit fruit attitudes, descriptively. Variances did not significantly differ across the ST-IATs, $\Delta\chi^2(2) = 4.138, p = .126$

Predicting behavior. The Fruit ST-IAT significantly predicted fruit, cantaloupe, and banana behaviors. The Cantaloupe ST-IAT significantly predicted cantaloupe behavior, but not fruit or banana behavior. The Banana ST-IAT significantly predicted banana and fruit behavior, but not cantaloupe behavior. See Table 2 for regression coefficients, standard errors, confidence intervals, and exact p -values.

Predicting behavior: Between-subjects. The between-subjects comparisons tested whether the general Fruit ST-IAT was better than the specific Banana and Cantaloupe ST-IATs at predicting fruit behavior (H3), as well as if the Banana and Cantaloupe ST-IATs were better than the Fruit ST-IAT at predicting banana and cantaloupe behavior, respectively (H1). With one exception, all comparisons trended in the predicted direction. Namely, the Fruit ST-IAT was trivially worse than the Banana ST-IAT at predicting fruit behavior (H3). Despite the consistent

trends, there were not any significant differences between the ST-IATs in predicting general vs. specific criteria. The only noteworthy comparison is the Fruit ST-IAT trended strongly toward being better than the Cantaloupe ST-IAT at predicting fruit behavior.

Predicting behavior: Within-subjects. The within-subjects comparisons tested whether any single IAT was better at predicting general vs. specific criteria. The Fruit ST-IAT was not at all better at predicting fruit behaviors than it was at predicting cantaloupe or banana behaviors (H4). Likewise, the Banana ST-IAT was not at all better than the Fruit ST-IAT at predicting banana behavior. In support of H2, the Cantaloupe ST-IAT was, however, significantly better at predicting cantaloupe vs. fruit behavior.

Predicting explicit attitudes. For the between-subjects comparisons with explicit attitudes as the dependent variable, only the Cantaloupe ST-IAT was significantly better than the Fruit ST-IAT at predicting explicit cantaloupe attitudes. All other comparisons went in the expected direction but were not significant. For the within-subjects comparisons, The Cantaloupe IAT was significantly better at predicting explicit cantaloupe vs. fruit attitudes and the Banana ST-IAT noticeably trended toward better predicting explicit banana vs. fruit attitudes. As with behaviors, however, the Fruit ST-IAT was not at all better at predicting explicit fruit attitudes vs. explicit banana or cantaloupe attitudes.

Incremental validity. After controlling for explicit fruit attitudes, the Fruit ST-IAT predicted a small but significant amount of unique variance in fruit behavior ($\beta = .09$, $S.E. = .05$, $p = .047$) and cantaloupe behavior ($\beta = .16$, $S.E. = .08$, $p = .040$) but not banana behavior ($\beta = .10$, $S.E. = .07$, $p = .146$). Its ability to predict fruit behavior was attenuated only slightly after additionally controlling for banana and cantaloupe attitudes ($\beta = .08$, $S.E. = .04$, $p = .056$). For

cantaloupe behaviors, the effect size attenuated noticeably after additionally controlling for cantaloupe attitudes ($\beta = .06$, $S.E. = .03$, $p = .052$).

After controlling for explicit banana attitudes, the Banana ST-IAT predicted a significant amount of unique variance in banana behavior ($\beta = .10$, $S.E. = .05$, $p = .04$) but not fruit behavior ($\beta = .12$, $S.E. = .06$, $p = .151$). It still predicted unique variance in banana behavior after additionally controlling for explicit fruit attitudes ($\beta = .12$, $S.E. = .05$, $p = .013$). In fact, its ability to predict unique variance increased slightly, probably because controlling for fruit attitudes coincidentally removes banana-irrelevant fruit variance from the dependent variable. The Cantaloupe ST-IAT did not predict any unique variance in cantaloupe behaviors ($\beta = -.02$, $S.E. = .05$, $p = .853$) after controlling for explicit cantaloupe attitudes.

Discussion

Some aspects of the ST-IAT results are promising for the correspondence principle, although it requires one to overlook the patterns of significance for the direct comparisons. Almost every effect is in the predicted direction across both studies despite the ST-IATs being psychometrically questionable (e.g., reliabilities for the observed variables range from .37 to .49). It is also worth noting that because of the challenges associated with detecting moderation, Judd and Kenny (2010) urge people to give at least some weight to non-significant moderation effects. The consistent trends should be considered as part of a preponderance of evidence across all the experiments. Nevertheless, the findings for some of the predictions appear to robustly *not* support the correspondence principle.

In isolation, the Cantaloupe ST-IAT is highly amenable to the correspondence principle. That is, the Cantaloupe ST-IAT not only predicted the correspondingly specific cantaloupe criteria reasonably well, but also entirely failed to predict the non-correspondent fruit criteria.

Indeed, the within-subjects comparisons for the implicit cantaloupe measures stand out across the fruit-related experiments in their clear patterns of correspondence with both criterion and explicit attitude measures (see Table 2).

We also expected the highly specific Cantaloupe ST-IAT to be especially good at predicting correspondingly specific criterion measures compared to the general Fruit ST-IAT. Indeed, that is exactly what happened between the Fruit and Cantaloupe IATs, but not with the ST-IATs. The difference in the ST-IAT experiments appears to be a function not only of the Cantaloupe ST-IAT predicting cantaloupe behavior slightly worse than the Cantaloupe IAT, but also the Fruit ST-IAT predicting cantaloupe behavior slightly better than the Fruit IAT.

The Fruit ST-IAT predicted fruit, banana, and cantaloupe criteria to a very similar degree. It is not surprising that the general fruit ST-IAT weakly predicted the specific criteria; general attitude measures ought to be able to predict all sorts of specific criteria, just not very well. What is somewhat surprising and inconsistent with the correspondence principle, however, is that the Fruit ST-IATs were not at all better at predicting general compared to specific criteria.

The Banana ST-IAT did trend toward better predicting banana vs. fruit criteria, but its pattern was otherwise nearly identical with the Fruit ST-IAT across criterion and explicit attitude measures. In other words, implicit banana and fruit measures each predicted both fruit and banana criteria to a similar degree. Although the evidence is very weak, these results suggest that specific implicit attitudes may be relatively better at predicting general criterion when they better represent the general category. That is, even though implicit cantaloupe and banana evaluations both predict correspondingly specific outcomes, only the more representative banana evaluations also predict outcomes relevant to the overarching fruit category.

Experiments 2a and 2b: Immigrant and Border Wall IATs

Fruit consumption is relevant for researchers interested in health behaviors, but criticism of the IAT often focuses on its perceived shortcomings in predicting outcomes relevant to social groups (e.g., Oswald et al., 2013). Therefore, we also assessed whether target correspondence moderates the relationship between implicit evaluations and criteria that affect members of a social group. Specifically, we tested whether an Immigrant IAT or a Border Wall IAT better predicted support for general immigration policies and immigration policies specific to funding and building a U.S./Mexico border wall. In this way, we were able to test whether our theoretical claims generalized to a domain that is quite conceptually distinct from that investigated in the previous experiments. Procedures for the two experiments were identical aside from the addition of explicit attitudes in the replication.

Method

Participants and Procedures

Participants were U.S. citizens recruited from the Project Implicit website. After agreeing to take part in the study, participants were randomly assigned to complete either an Immigrant IAT or a Border Wall IAT; everyone completed all self-report immigration and border wall measures. Order of the IAT and self-report measures was counterbalanced; order of self-report measures and their items was randomized within the block of self-report measures.

Experiment 2a. For this experiment, we pre-registered a sample size of 800 participants, intended to provide 80% power to detect small interaction effects ($f^2 = .01$). Data collection ended automatically upon reaching the target number of completed studies (within a small margin of error). Of the 802 participants in the final data set (388 for the Immigrant IAT group; 414 for the Border Wall IAT), 720 participants remained after making exclusions based on the same criteria described in Experiment 1. In the Immigrant IAT group, 22 participants were

excluded based on their IAT error rates and 12 for excessive missing self-report data. In the Border Wall IAT group, 43 participants were excluded based on their IAT error rates and 5 for excessive missing self-report data. In the final sample, participants were 63% female, 72% white, and averaged 33.7 years old ($SD = 14.5$). On a scale from 1 (Strongly conservative) to 7 (Strongly liberal), participants were slightly politically liberal, on average ($M = 4.81$, $SD = 1.63$).

Experiment 2b. We ran a nearly identical follow-up study to increase confidence in our findings because we 1) underestimated the original sample size, 2) had borderline p-values, 3) found SEM results that diverged noticeably from our originally pre-registered regression analysis, and 4) wanted to include explicit attitudes as a dependent variable. We also took the opportunity to pre-register our measurement invariance and multiple groups SEM analysis plan before collecting data, as these were not included in Experiment 2a's pre-registration.

Experiment 2b used an identical procedure to Experiment 2a except for the addition of six explicit attitude items and the increase in target sample size from 800 to 1150. The rationale for the larger sample is a continuation of our recognizing that moderation analyses require very large samples, especially given small interaction effects and other study-specific issues (e.g., border wall policy preferences were highly skewed in the original experiment). Of the 1146 people in the original sample, 1082 remained after data- and method-based exclusions. We excluded 61 people for IAT errors (15 immigrant and 46 border wall) and 13 people for excessive missing data (8 immigrant and 5 border wall). Participants were 70.6% female, 73.6% white, slightly politically liberal ($M = 4.85$, $SD = 1.69$), and averaged 39.08 years old ($SD = 13.76$).

Measures

Implicit Association Tests. The Immigrant IAT used ‘Immigrants’ and ‘American Citizens’ as category labels. Stimuli were ‘Immigrant’, ‘Foreigner’, and ‘Newcomer’ for the ‘Immigrants’ category and ‘American citizen’, ‘Citizen’, and ‘U.S. Citizen’ for the ‘American Citizens’ category. The Border Wall IAT used ‘Border Wall’ and ‘No Border Wall’ as category labels. Stimuli were images of portions of border wall that were already constructed or in the process of being constructed for the ‘Border Wall’ category and images of similar Southwestern United States landscapes without a wall for the ‘No Border Wall’ category. Both IATs used Good/Bad as labels and the same good words (e.g., nice, wonderful, pleasure) and bad words (e.g., horrible, evil, awful) as stimuli. In Experiment 2a, the averages of 600 split-half correlations were $\alpha = .65$ for the Immigrant IAT and $\alpha = .81$ for the Border Wall IAT; in Experiment 2b, the internal consistencies were $\alpha = .69$ for the Immigrant IAT and $\alpha = .81$ for the Border Wall IAT.

Immigration policy support. Participants self-reported their support for or opposition to three legal and three illegal immigration policies (Pérez, 2010) with response options ranging from 1 (Oppose strongly) to 7 (Favor strongly). The stem for all items was “Indicate the extent to which you would favor or oppose policies aimed at...” and items included “Making it more difficult for undocumented immigrants to become U.S. Citizens” (illegal immigration) and “Increasing the number of visas available to legal immigrants” (legal immigration). Although Pérez (2010) separated the items into two subscales, we formed them into a single scale representing overall support for immigration policies. In Experiment 2a, internal consistencies were $\alpha = .80$ and $\alpha = .78$ for the Immigrant IAT and Border Wall IAT groups, respectively; in Experiment 2b, both alphas were .81.

Border wall policy support. Participants also self-reported support or opposition to three border wall policy items (e.g., “How much do you favor or oppose building a border wall on the U.S. Border with Mexico?”) Response options ranged from 1 to 7 (e.g., Oppose strongly/Favor strongly). In Experiment 2a, Alphas were .96 and .97 in the Immigrant IAT and Border Wall IAT groups, respectively; in Experiment 2b, both alphas were .98.

Explicit attitudes. In Experiment 2b, we added three explicit border wall attitude items (e.g., “How much do you like or dislike the idea of building a border wall?”). Response options ranged from 1 to 7 (e.g., Dislike Strongly/Like Strongly). We also added three immigrant attitude items (“How positive are your attitudes toward immigrants relative to other people, in general?”) Response options ranged from 1 (Strongly negative/Strongly positive attitudes toward immigrants). Alphas for the explicit immigrant attitudes were .90 and .92 in the Immigrant and Border Wall IAT groups. Alphas for explicit border wall attitudes were .99 in both groups.

Results

Tests of Invariance

We took the same approach to measurement invariance as we did in Experiments 1a, 1b, and 1c. The configural models fit well in Experiment 2a, $\chi^2(102) = 219.588, p < .001$, CFI = .969, RMSEA = .056 [.046, .067], as well as in Experiment 2b, $\chi^2(250) = 571.492, p < .001$, CFI = .973, RMSEA = .049 [.043, .054]. See Table 4 for factor loadings from both experiments.

Factor loading invariance: Experiment 2a. In the original experiment, constraining the IAT factor loadings did not significantly worsen model fit in Experiment 2a, $\Delta\chi^2(2) = 3.205, p = .201$, or in Experiment 2b, $\Delta\chi^2(2) = 3.517, p = .172$. Moreover, modification indices in the constrained model did not identify any potentially non-invariant factor loadings between the two IATs in either experiment.

Factor means and variances. Factor means and variances are in reference to each experiment's Immigrant IAT. Implicit border wall attitudes were much more negative than implicit immigrant attitudes in Experiment 1a ($M = -2.346$, $s^2 = 2.090$) and Experiment 2b ($M = -2.482$, $s^2 = 1.810$). Implicit border wall attitude varied significantly more in both Experiment 1a, $\Delta\chi^2(1) = 30.838$, $p < .001$, and Experiment 1b, $\Delta\chi^2(1) = 31.417$, $p < .001$.

Table 4

Standardized and unstandardized factor loadings for Border Wall and Immigrant IATs

	<i>Immigrant IAT</i>		<i>Border Wall IAT</i>	
	<i>Std. (SE)</i>	<i>Unstd. (SE)</i>	<i>Std. (SE)</i>	<i>Unstd. (SE)</i>
Experiment 1a				
Trials 1-20 (B3/B6)	.68 (.05)	0.34 (.03)	.79 (.03)	0.32 (.03)
Trials 21-40 (B4/B7)	.75 (.04)	0.36* (.03)	.85 (.03)	0.36* (.03)
Trials 41-60 (B4/B7)	.61 (.04)	0.30 (.03)	.82 (.03)	0.35 (.03)
Experiment 2b				
Trials 1-20 (B3/B6)	.63 (.04)	0.32 (.02)	.82 (.02)	0.40 (.02)
Trials 21-40 (B4/B7)	.75 (.03)	0.40* (.02)	.86 (.02)	0.40* (.02)
Trials 41-60 (B4/B7)	.68 (.03)	0.36 (.02)	.78 (.03)	0.36 (.03)

* marker variable constrained between groups for model identification

Hypothesis Tests

Predicting policy preferences. In both experiments, the Immigrant IAT and Border Wall IAT significantly predicted border wall policy preferences and immigration policy preferences. See Table 5 for regression coefficients, standard errors, 95% confidence intervals, and p-values.

Policy preferences: Between-subjects. The between-subjects comparisons tested whether the Immigrant IAT outperformed the Border Wall IAT when predicting immigration policy preferences (H3) and if the Border Wall IAT outperformed the Immigrant IAT when predicting border wall policy preferences (H1). The findings supported the general prediction but not the specific prediction. In support of H3, the Immigrant IAT was marginally (Exp. 2a) or significantly (Exp. 2b) better than the Border Wall IAT at predicting immigration policy

preferences in the two experiments. H1 was not supported, however, as the Border Wall IAT was not better than the Immigrant IAT at predicting border wall policy preferences in either experiment. See Table 6 for details about chi-square difference tests for between- and within-subjects comparisons.

Table 5

Regression coefficients from Experiments 2a and 2b

<i>Outcome</i> /Predictor	Experiment 2a				Experiment 2b			
	<i>B</i>	<i>S.E.</i>	95% CI	<i>p</i>	<i>B</i>	<i>S.E.</i>	95% CI	<i>p</i>
<i>Immigration Policies</i>								
Border Wall IAT	.23	.05	[.13, .32]	< .001	.24	.05	[.15, .33]	< .001
Immigrant IAT	.38	.08	[.23, .52]	< .001	.49	.06	[.37, .62]	< .001
<i>Border Wall Policies</i>								
Border Wall IAT	.27	.04	[.18, .36]	< .001	.30	.05	[.21, .38]	< .001
Immigrant IAT	.22	.07	[.09, .36]	.001	.31	.05	[.21, .42]	< .001
<i>Immigrant Attitudes</i>								
Border Wall IAT	--	--	--		.14	.04	[.06, .22]	.001
Immigrant IAT	--	--	--		.22	.05	[.12, .33]	< .001
<i>Border Wall Attitudes</i>								
Border Wall IAT	--	--	--	--	.32	.04	[.23, .40]	< .001
Immigrant IAT	--	--	--	--	.33	.05	[.22, .43]	< .001

Note. Immigrant IAT coefficients are in standardized units. Border Wall IAT coefficients are scaled to the standardized units of their study's Immigrant IAT.

Policy preferences: Within-subjects. The within-subjects comparisons tested whether the Immigrant IAT was better at predicting immigration policy relative to border wall policy preferences (H4) and if the Border Wall IAT was better at predicting border wall policy relative to immigration policy preferences (H2). The pattern was similar to the between-subjects comparisons. There was clear support for H4 in that the Immigrant IAT predicted immigration policy preferences better than border wall policy preferences in both experiments. However, H2 was not fully supported as the data only trended in the expected direction in the two experiments.

Predicting explicit attitudes. Both the Immigrant IAT and Border Wall IAT significantly predicted explicit immigrant and border wall attitudes in both experiments. For the between-subjects comparisons, the Immigrant IAT trended toward being better than the Border Wall IAT at predicting explicit immigrant attitudes, but there was practically no difference between the Immigrant and Border Wall IATs when predicting border wall attitudes.

The within-subjects comparisons, however, did more clearly support the correspondence principle. The Immigrant IAT was marginally better at predicting explicit immigrant attitudes than explicit border wall attitudes, and the Border Wall IAT was significantly better at predicting explicit border wall attitudes than explicit immigrant attitudes.

Table 6

Chi-square Difference Tests for Experiments 2a and 2b

	Experiment 2a		Experiment 2b	
	$\Delta\chi^2$	<i>p</i>	$\Delta\chi^2$	<i>p</i>
Between-Subjects (Two IATs predict one criterion)				
<i>Immigration policy preferences</i>				
Immigrant IAT > Wall IAT	3.280	.070	14.480	<. 001
<i>Border wall policy preferences</i>				
Wall IAT > Immigrant IAT	0.416	.519	0.068	.794*
<i>Immigrant attitudes</i>				
Immigrant IAT > Wall IAT			1.672	.196
<i>Border wall attitudes</i>				
Wall IAT > Immigrant IAT			0.045	.832*
Within-subjects (One IAT predicts both criteria)				
<i>Immigrant IAT</i>				
Policies: Immigration > Border Wall	8.633	.003	14.480	<. 001
Attitudes Immigrants > Border Wall	--	--	2.829	.093
<i>Border Wall IAT</i>				
Policies: Border Wall > Immigration	1.496	.221	1.152	.283
Attitudes: Border Wall > Immigrants			13.882	< .001

* indicates effect went opposite from predicted direction

Incremental validity. Explicit border wall attitudes and policy preference measures turned out to be completely redundant ($r = .98$). Thus, it would be nearly impossible for the Border Wall IAT to uniquely predict border wall policy preferences above and beyond explicit border wall attitudes. Explicit border wall attitudes and immigration policy preferences were highly correlated, but not redundant ($r = .73$). Nevertheless, the Border Wall IAT also did not uniquely predict immigration policy preferences after controlling for explicit border wall attitudes.

The Immigrant IAT did not uniquely predict border wall policy preferences after controlling for explicit immigrant attitudes ($\beta = .09$, $S.E. = .095$, $p = .342$). The Immigrant IAT did, however, predict unique variance in immigration policy preferences ($\beta = .27$, $S.E. = .07$, $p < .001$). We additionally controlled for explicit border wall attitudes to provide a stricter test of the Immigrant IAT's incremental validity. Even though the effect was somewhat attenuated, the Immigrant IAT still predicted unique variance above both types of explicit attitudes ($\beta = .21$, $S.E. = .04$, $p < .001$).

Discussion

Researchers most frequently use the IAT to examine implicit attitudes toward social groups. Unsurprisingly, a major criticism of the IAT is its shortcomings in predicting outcomes relevant to minority social groups (Oswald et al., 2013). In two experiments, we tested whether a general social group IAT (Immigrants) and a specific IAT (Border Wall) differed in their abilities to predict outcomes that were correspondingly general and specific.

Overall, the findings supported the correspondence principle's predictions, although not entirely. The clearest pattern involved the general IAT's ability to excel at predicting correspondingly general criteria. Compared to a Border Wall IAT, an Immigrant IAT better

predicted preferences related to a set of general legal and illegal immigration policies. Moreover, the Immigrant IAT predicted general immigration policy preferences better than it predicted specific border wall policy preferences.

Contrary to expectations, however, the Border Wall IAT and Immigrant IAT did not differ at all when predicting specific border wall policy preferences. Additionally, the Border Wall IAT only trended toward predicting border wall policy preferences better than it predicted general immigration policy preferences. The original experiment and a higher-powered replication converged to indicate that the specific Border Wall IAT does not excel at predicting specific border wall policies. Importantly, these findings do not appear to be caused by the Border Wall IAT's overall inability to predict either of the outcomes. Rather, the Border Wall IAT did fairly well at predicting both the general and specific policy preferences.

Incremental validity. The Immigrant IAT predicted unique variance in immigration policies after controlling for explicit attitudes toward immigrants (relative to other people). If the standard is to predict unique variance beyond explicit measures that correspond directly with the content of the implicit measure itself, then the outlook is promising. Indeed, this is consistent with the standard in meta-analytic tests of the IAT's incremental validity (Greenwald et al., 2009; Kurdi et al., 2018). Nevertheless, our explicit attitude measure was clearly impoverished if implicit measures ought to uniquely predict variance in criteria above and beyond *any* conceptually relevant attitudes a person can self-report (e.g., Blanton et al., 2016). Relatedly, although it remains an empirical question, the Immigrant IAT's incremental validity could likely become attenuated by merely asking people a few additional questions about their attitudes toward legal immigrants, illegal immigrants, and immigrants from different cultural backgrounds. Moreover, fully assessing explicit immigrant attitudes only covers correspondence

with the Immigrant IAT itself. Perhaps the implicit measures need to also predict unique variance after controlling for explicit attitudes that correspond with the criterion measure. As we demonstrated, the Immigrant IAT explained less unique variance in immigration policy preferences after additionally controlling explicit border wall attitudes, attitudes that correspond only moderately with the criterion measure.

General Discussion

As Ajzen and Fishbein (1977) highlighted over 40 years ago, it is unreasonable to expect general-level attitude measures to strongly predict specific criteria. Failures to meet these expectations have arguably contributed to the controversies related to the IAT's fitness as a measurement procedure (Jost, 2019). Once the correspondence principle is considered, however, the ability of general-level social group IATs to consistently predict specific criteria with any level of success could be viewed as quite impressive. Although Ajzen (2011) has continued to stress the importance of conceptual correspondence for understanding implicit-criterion and explicit-criterion correlations, the correspondence principle is rarely given full (or any) consideration when developing implicit and criterion measures (Blanton et al., 2016; Gawronski & Brannon, 2017; Oswald et al., 2013; Kurdi et al., 2018). When correspondence's influence on implicit-criterion relationships has been examined, it is almost exclusively in the context of meta-analyses comprised of existing studies that tend to be both underpowered and have minimal implicit-criterion correspondence (Kurdi et al., 2018). Our work constitutes the first direct test of the correspondence principle in the context of implicit-criterion correlations. Across five experiments using two implicit measures covering two attitude domains, we tested predictions derived directly from the correspondence principle regarding the relationship between implicit measures and the criteria of self-reported behavior, explicit attitudes, and policy preferences. We

additionally tested whether implicit measures showed evidence of incremental validity in these contexts above and beyond the predictive ability of concurrently-measured explicit attitudes.

The Influence of Conceptual Correspondence on Implicit-Criterion Correlations

Across two conceptual domains (health behaviors and policy preferences) that commonly use implicit measures, we tested the two major predictions of the correspondence principle. The *specific prediction* posits that specific-level measures should excel at predicting specific-level criteria. The *general prediction* posits that general-level measures should excel at predicting general-level criteria. We tested these predictions two ways, both between- and within-subjects. Between-subjects comparisons tested whether, in comparison to each other, general vs. specific IATs excelled at predicting general and specific criteria, respectively. Within-subjects comparisons tested whether, in reference to themselves, each general and specific IAT was better at predicting general vs. specific criteria. Although there are noteworthy exceptions and some unconvincing patterns of statistical significance, the experimental findings supported the correspondence principle, overall.

Specific vs. general IATs: Predicting specific criteria. The impetus for Ajzen and Fishbein (1977) to explicate the correspondence principle was recognizing that researchers commonly used very general attitude measures to predict very specific behaviors. They demonstrated that relationships between attitudes and behaviors were stronger when correspondingly specific explicit attitude measures replaced broad general ones. Based on their precedent, we expected to obtain the strongest support for the between-subjects specific prediction: Specific IATs should be better than general IATs at predicting specific criteria (H1). Beyond the historical parallels, these are arguably the most critical hypotheses because they not only reflect what may be a primary cause of controversy about the IAT's predictive validity, but

also a potential solution to the underlying problem. In other words, implicit-criterion relationships should increase when general-level IATs are replaced with specific-level IATs that correspond more closely to the specific-level criterion measures.

Remarkably, this set of predictions found inconsistent support across experiments. In Experiment 1a, the Cantaloupe IAT was far superior to the Fruit IAT at predicting cantaloupe behavior. In all three experiments, the Cantaloupe IAT and ST-IATs outperformed their fruit counterparts when predicting explicit cantaloupe attitudes. Yet, no other comparisons clearly supported the correspondence principle. In fact, the pattern of experimental evidence suggests that conceptual correspondence robustly failed to influence implicit-criterion or implicit-explicit relationships for the other pairs of general/specific implicit measures (fruit/banana and immigrant/border wall).

Specific IATs: Predicting Specific vs. General Criteria. We also tested the correspondence principle's specific prediction by comparing whether specific IATs predict specific criteria better than general criteria. Although the patterns were generally more favorable for the correspondence principle, findings from this class of predictions were also inconsistent across attitude objects. In each comparison, the implicit cantaloupe measures again clearly excelled at predicting correspondingly specific cantaloupe criteria. The implicit banana measures, however, did not excel at predicting banana criteria better than fruit, and the Border Wall IAT only trended toward better predicting border wall vs. immigration policy preferences.

General vs. specific IATs: Predicting general criteria. The other half of our comparisons tested the correspondence principle's predictions with regard to general attitudes. This class of findings is useful for highlighting some considerations that should be made by researchers who aim to predict criterion measures from general IATs. The implicit measures of

fruit attitudes either reached significance or strongly trended toward being better than their cantaloupe counterparts at predicting fruit behavior. Moreover, the Immigrant IAT predicted support for general immigration policies significantly better than the Border Wall IAT did. These findings suggest that in addition to their ability to weakly predict a breadth of relevant criteria, general IATs are also preferable over specific IATs so long as the criterion measures are correspondingly general.

General IATs: Predicting general vs. specific criteria. Findings across the two conceptual domains were more inconsistent with respect to the implicit measures' abilities to predict general criteria better than specific criteria. Specifically, whereas implicit immigrant attitudes predicted support for immigration policies better than support for border wall policies, implicit fruit attitudes were never more effective at predicting fruit consumption relative to any specific criterion or explicit attitude measures.

Variable Relevance of the Correspondence Principle

Our findings support the idea that, so long as it is justifiable, researchers will not go wrong by increasing conceptual correspondence between implicit and criterion measures. Comparisons were never significantly opposite from the predictions derived from the correspondence principle; further, the pattern of implicit-criterion and implicit-explicit relationships went in the correspondence-predicted direction in all but a couple trivial cases. Our findings do not, however, indicate that increasing correspondence is equally beneficial in all cases. The variability in support for hypotheses across measurement type and attitude domain indicates the opportunity for theory to make organized predictions.

Category representativeness: Specific IATs. The correspondence principle indicated the most relevance for implicit measures related to cantaloupe and the least relevance for implicit

measures of bananas and the border wall. One way to reconcile the specific IATs' asymmetrical patterns is to consider the specific exemplars' category-representativeness, or how central a specific exemplar is to people's mental representations of the general category.

We posit that the correspondence principle will be especially relevant to a specific attitude object to the extent that it is a poor exemplar of its overarching general category. Feature similarity is central to the associative processes underlying implicit attitudes (Gawronski & Bodenhausen, 2011). Because of their similarity, general categories and highly representative specific exemplars should be more strongly associated than general categories and non-representative specific exemplars. Whereas cantaloupe is presumably not highly representative of fruit in general, building a border wall is one of the most salient specific immigration policies in the United States, at least at the time the experiments were conducted (Fall of 2018 and Summer of 2019).

In the current experiments, we assumed that bananas and cantaloupe are strong and weak representatives of the general fruit category, respectively, and that border walls are highly representative of a general immigration policy category. Although hypotheses derived from the correspondence principle were relatively well-supported for the weakly representative attitude object, they were less well-supported for the strongly representative attitude objects. This observation should form an important piece of theory predicting when the correspondence principle will be relevant for increasing implicit measures' predictive validity.

Principle of aggregation: General IATs. The implicit immigrant measures had good predictive correspondence. In other words, implicit immigrant attitudes excelled at predicting correspondent relative to non-correspondent criteria. The implicit fruit measures, however, demonstrated almost none. Based on our reasoning about category representativeness, the

Immigrant IAT should have been disadvantaged relative to the Fruit IAT and ST-IATs. That is, implicit immigrant attitudes still excelled at predicting the general criteria even relative to the specific but highly representative border wall criteria. Implicit fruit attitudes consistently failed to demonstrate specificity when predicting criteria related to both the more and less representative banana and cantaloupe exemplars. This may be the result of idiosyncratic features of attitude objects or conceptual domains. Yet, one explanation could involve the way we conceptualized the general-level criterion measures.

Although the correspondence principle posits that general measures should predict general behaviors, another related principle – the principle of aggregation – may also be relevant. In general terms, the principle of aggregation is simply that any set of measurements, when combined, will be a more reliable and representative estimate than any one of the measurements on its own. When applied to attitudes research (Fishbein & Ajzen, 1974), the principle of aggregation suggests that the general behavioral measures should be the composite of several specific behaviors that more fully capture the relevant behavioral domain (Fishbein & Ajzen, 1974). Instead of aggregating specific behaviors, we asked people only about their eating behaviors and only in very general terms. A more inclusive method may have been to measure consumption of many different fruits in different contexts as well as measuring fruit-related criteria beyond mere eating behaviors. Indeed, this is one reason why the Immigrant IAT may have predicted general immigration policies so well relative to border wall policies. Namely, although the items all refer to immigration policies in general, they provide coverage of different types of immigrants (i.e., legal or illegal) and general policies (e.g., visas, citizenship, welfare rights). Border wall policies (or any specific policy) do not take advantage of implicit immigrant attitudes' broad conceptual coverage.

Comparing Implicit Measures

The decision to turn to multiple groups SEM to analyze our data was originally predicated on recognizing that the IATs and ST-IATs within each experiment had either unequal or low reliabilities (or both). The approach also opens up opportunities to better understand the psychometric properties of implicit and criterion measures. Ensuring comparability is essential if the goal is to develop a sounder understanding of implicit measures and the theoretical constructs they are intended to measure. More broadly, Kurdi et al. (2018) have called for a greater focus on the development of implicit and criterion measures that are both valid and reliable. To be sure, issues of reliability and validity should be of perennial concern, but it does seem there has been a resurging interest in ensuring that our constructs are well-measured (e.g., Flake & Fried, 2019; Flake, Pek, & Hehman, 2017; Hussey & Hughes, 2018).

Differences in IAT reliabilities and factor loading patterns. The specific IATs (Cantaloupe and Border Wall) were more reliable than their general counterparts (Fruit and Immigrants). The differences between the Fruit and Cantaloupe IATs appeared to be driven by the first blocks of critical trials. For the Immigrant and Border Wall IATs, however, the different reliabilities cannot be linked to any specific indicator(s). The Immigrant and Border Wall IAT factor loadings follow the same pattern as each other, but are uniformly lower for the Immigrant IAT. In the multiple groups model, the factor loadings are invariant because each indicator loads proportionally on their respective factors.

Our best initial explanation for these differences is that the specific IATs had more homogenous stimuli sets. Whereas the Cantaloupe IAT used a highly homogenous set of cantaloupe images, the Fruit IAT stimuli depicted various types of fruit. The Fruit/Cantaloupe IAT factor loading pattern suggests that the Fruit IAT might have a steeper learning curve than

the Cantaloupe IAT. As such, the first block of critical trials has more construct-irrelevant variance for the Fruit IAT. By the end of the first block of each set of critical trials, however, the Fruit and Cantaloupe IAT difficulties are on even footing.

For the Immigrant/Border Wall IAT, the differences persist across the entire procedure. The Border Wall IAT stimuli should be relatively easy to categorize by distinguishing the presence of a border wall versus an open desert landscape, whereas the Immigrant IAT used words that may not as clearly or fully represent the broad immigrant category. Additionally, images operate at a low level of representation that facilitates cognitive processing, but words operate at a high level of representation that makes them more difficult to cognitively process (Feroni & Bel-Bahar, 2010). As such, the Border Wall IAT may be consistently easier to process than the Immigrant IAT across the entire task.

Incremental Validity and the Correspondence Principle

We have identified two distinct approaches to assessing implicit measures' incremental validity in the extant literature. The first approach is to intentionally limit tests for incremental validity to only directly correspondent implicit and explicit measures. One meta-analytic application of this approach led to modest incremental validity for implicit and explicit measures, with a slight edge in overall effect size for the implicit measures (Kurdi et al., 2018). The second approach is to only consider claims to implicit measures' incremental validity after controlling for more exhaustive sets of conceptually relevant explicit measures. For example, Blanton et al. (2016) argue that focusing in on a single global explicit attitude measure often results in false attributions of consequential incremental validity to IATs.

The key distinction is that the first approach applies the correspondence principle in reference to the implicit measures' content and the second approach additionally applies it in

reference to the criterion measures' content. We applied both approaches when they were possible and relevant. In some cases, the IATs and ST-IATs predicted unique variance in criterion measures above and beyond correspondent explicit attitude measures. Yet, the explicit measures were crude, ad hoc, and narrowly focused. Almost certainly, we could eliminate much of the IAT's uniquely predicted variance in criterion measures merely by asking additional questions about attitudes. We suspect that the correspondence principle's stricter application would attenuate the IAT's apparent incremental validity and sharply shift the balance in favor of explicit measures. Yet, from the stricter perspective, a proper test of incremental validity should probably include not only a full battery of explicit measures but also a correspondingly complete set of implicit measures. Notably, both additions have practical concerns in terms of participant fatigue and potential order effects.

Ultimately, the standard for incremental validity depends on the aims of the research. Indeed, both approaches just described seem to be well-reasoned given their specific research contexts. Our point is not to tout one approach over the other, but to echo suggestions that the correspondence principle needs to be explicitly considered whenever researchers observe implicit-explicit dissociations or find evidence for the IAT's incremental validity (e.g., Blanton et al., 2016; Gawronski, 2019). If either of these topics are a primary research aim, the correspondence principle should be front and center from the outset.

Importantly, the current work suggests that the correspondence principle does not neatly and consistently apply across implicit measures or attitude domains. Although consistency would be simpler, in our view this is an opportunity for further understanding. We return to this in the foregoing.

Limitations

There are several issues limiting both the extent to which we can generalize our findings as well as the quality of inferences that can be made. Although we recognize that there are many general issues with implicit measures and the IAT in particular (for reviews, see Blanton, Jaccard, Strauts, Mitchell, & Tetlock, 2015; Gawronski, 2019), we focus our attention on concerns that are most germane to the current studies.

Limited coverage of conceptual domains. We tested the correspondence principle in two distinct conceptual domains, but implicit measures are commonplace across many research areas. Even looking only at the two domains we chose, however, the pattern of results diverged for some predictions in our experiments. Thus, we far more conceptual ground needs to be covered to develop a meaningful theoretical framework. We have started to set out the parameters of the search, provided methodological and statistical guidance, and noted its complexity.

“Behaviors”. Although we loosely refer to our criterion measures as behaviors, whether they capture *actual* behaviors is dubious (Baumeister, Vohs, & Funder, 2007). The only clear measures of real-world behavior are the items asking people to report on how many days they consumed fruit in the last week. These objective behaviors are only one piece of the criterion measures that ended up being weighted more heavily by subjective judgments about general behavior patterns and expectations about future behavior. Moreover, the objective items are predicting behavior that already occurred, whereas predicting future behavior is probably what most people have in mind as the gold standard for predictive validity (e.g., Yarkoni & Westfall, 2017). Although we could argue that expressing a desire to vote for or support a policy are behaviors, expressing preferences is also not truly a “behavior”. We selected these criteria because they would be easy for participants to accurately self-report and would probably qualify

as criterion measures if they were considered for inclusion in a meta-analysis. Nevertheless, we recognize that the criteria might come up short when it comes to both “prediction” and “behavior” per se.

Narrow focus on the IAT. Our only unambiguous evidence supporting the correspondence principle came from experiments using the IAT. Even then, the patterns were mixed across conceptual domains. The ST-IAT findings were somewhat favorable for our predictions, but ultimately inconclusive (and could also be interpreted as highly problematic). In our view, it is sensible to begin this work with the IAT given its usage rate, its positive psychometric properties relative to other implicit measures of evaluations, and its superior predictive validity compared to most other implicit measures (Bar-Anan & Nosek, 2014; Kurdi et al., 2018). To reduce the influence of measure-specific error, generalizing findings to implicit attitudes as a construct necessitates comparing results across multiple different implicit measures of similar constructs (Gawronski, 2019). As such, in addition to recommending that future experiments increase the number of domains covered and the legitimacy of the behaviors included, a full accounting of the relevance of the correspondence principle to the relationship between implicit measures and behavior also requires the use of additional implicit measures.

Beyond Target Correspondence

The studies we have presented only directly addressed one aspect of conceptual correspondence – how well the targets of the implicit and criterion measures correspond in their level of abstraction. The correspondence principle, however, also encompasses elements of action, context, and time (Ajzen & Fishbein, 2005). In other words, attitude-criterion correlations should continue to increase as the measures incorporate the same action, in the same situation, at the same time. For example, imagine trying to predict whether or not someone would eat

cantaloupe during a research study at 1pm. Compared to attitudes toward fruit in general, this behavior should be better predicted as the attitudinal measures incorporate the same target (cantaloupe), action (eating), context (during a research study) and time (in the early afternoon). Incorporating all these elements results in measuring *attitudes toward the behavior* rather than more general attitudes (Ajzen & Fishbein, 2005).

Whereas adding multiple correspondence components is easily accomplished with explicit measures, it is more challenging with implicit measures. We did add action correspondence to the Personalized ST-IAT labels (e.g., “Eating Fruit”). Whether this had any significant benefit is unclear because we did not set our experiments up to directly compare the Traditional and Personalized ST-IATs. Descriptively, however, the modifications did coincide with a noticeable upward shift in regression coefficients for the Personalized ST-IAT (See Supplemental Table S2). Even so, it is not clear whether any differences are attributable to the increased action correspondence (‘eating’ fruit), personalizing the attribute labels (‘I Like’), or both.

Although adding action correspondence to IAT labels is very feasible, including more than one element may prove to be impractical. One or more elements could potentially be incorporated into the IAT stimuli (e.g., images of someone eating cantaloupe in a lab setting), but whether or not this would exert the intended effect depends on several factors such as the extent to which IAT performance is influenced by category labels relative to stimuli (e.g., Bluemke & Fries, 2006; De Houwer, 2001). Even if it proves unwieldy to incorporate action, context, and time correspondence into the IAT, other implicit measures might be more amenable.

Conclusion

In sum, we have provided some initial experimental evidence across two unrelated domains that conceptual correspondence influences the magnitude of implicit-criterion relationships. It is more than clear, however, that we uncovered a great deal of complexity in the correspondence principle's application to implicit measures. Our findings suggest that if the goal is to maximize implicit measures' ability to predict behavior and other relevant outcomes, general implicit attitudes should be used to predict similarly general criteria, whereas specific implicit attitudes should be used when the goal is to predict similarly specific criteria. Yet, our experiments demonstrate that the predictive superiority of correspondent implicit measures is only a rule of thumb and does not uniformly guarantee increases implicit measures' ability to predict relevant outcomes.

Open Science Practices

Materials, data, and pre-registrations for all experiments in this paper, and all preliminary and otherwise related studies not reported in the paper are available at the project's Open Science Framework page ([OSF page](#)).

- ¹ Our sample size determination was inexact across studies (including preliminary studies) as we continued to find additional information about considerations related to difficulties detecting continuous by categorical interactions. Nonetheless, our sample sizes are pre-registered and data collection termination rules were consistent.
- ² The same factor residuals for the objective consumption behaviors (e.g., on how many days participants ate fruit last week and expected to eat it next week) had very high modification indices for each latent consumption variable in the three fruit-related experiments. The residual variances are irrelevant to the regression slopes and only affect overall model fit.

- ³ Although they preceded the first experiment chronologically, we report these experiments afterwards because it is easier to contextualize the pattern of findings as well as concerns with the study designs and implicit measures' psychometric properties.

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